THREE ESSAYS ON THE ECONOMICS OF NUTRITION

ASSISTANCE AND FOOD SECURITY

by

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Dedicated to my family

THREE ESSAYS ON THE ECONOMICS OF NUTRITION ASSISTANCE AND FOOD SECURITY

by

XIA SI, BS

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ASSISTANCE AND FOOD SECURITY

Xia Si, PhD
The University of Texas at Dallas, 2018

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This dissertation focuses on the economics of nutrition assistance and food security. The first essay tests the substitution effect between public and private nutrition assistance programs in the United States. It is the first to address the causal relationship between shocks in the availability of public nutrition assistance and low-income households' private nutrition assistance utilization. In particular, we examined the way in which loss of WIC benefits when children aged-out of WIC eligibility impacted a household's utilization of private food assistance. Using a regression discontinuity analysis framework, I found that households significantly increased utilization of private nutrition assistance following a negative shock in the availability of public nutrition assistance. Estimates indicated that some households might have been able to compensate 50 – 80 percent of their loss in public WIC nutrition assistance by increasing the frequency of utilization of private nutrition assistance. The second essay exploited the expansion of Community Distribution Partners (CDPs) of Crossroads Community Service (CCS) to investigate if the reduction of travel costs improved low-income households' utilization of private nutrition assistance. I found that after a new CDP within 2 km from a client's address was opened, potentially shortening client's traveling distance, nearby clients' visiting frequency increased by 4.4% compared to clients living farther from this CDP site. The third essay investigated the impact of E-verify mandates, which make it more difficult for certain undocumented workers to find a new job, on the food security status of both citizens and non-citizens. Using a Difference in Difference approach and data from CPS's food security supplements, this study found that even through E-verify mandates had no significant effects on family income, they had unintended consequences on households' food security. E-verify mandates reduced the food security of both U.S. citizens and non-citizens residing in the U.S. The effect was consistent over different sub-types of food security measures.

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CHAPTER 1

AGING OUT OF WIC: AN INVESTIGATION OF THE COMPENSATION EFFECT OF PRIVATE NUTRITION ASSISTANCE PROGRAMS

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1.1 Introduction

Approximately 25% of US households participate in public food assistance programs. Among these, it is estimated that a third also participate in private food assistance programs operated by non-governmental organizations (Pruitt et al. 2016). The way in which households combine assistance from both types of sources may have important implications for understanding the impact of changes in public policy that influences the availability of public food assistance programs. Households may lose eligibility for food assistance programs suddenly—either because the eligibility rules changed or because the household's circumstances changed. In either case, we know very little about a household's strategy for making up for these lost resources. The purpose of this study is to understand how households use private nutrition assistance to compensate for lost public nutrition assistance benefits.

The compensation behavior of households, when faced with a loss of public nutrition benefits, is important for several reasons. First, a change in utilization of private nutrition assistance after a loss of public nutrition assistance provides a measure of the substitutability of these two programs. While both types of programs provide nutrition assistance, they do so in different ways and with different institutional structures that may affect the degree to which the two are perfect substitutes. Second, the results also provide some insight into the extent to which households rely upon public nutrition assistance programs. In order for any compensating substitution of private nutrition assistance to occur, loss of program eligibility must be significant enough to alter households' behavior.

Private nutrition assistance programs, such as those supported by food banks, are often viewed as an essential part of the community safety net. This safety-net is supposed to provide security for low-income households following income shocks associated with loss of employment and also in the case of an income shock such as the loss of public assistance benefits. Although it seems that the demand for private nutrition assistance has increased when federal welfare cuts have occurred (Loopstra, 2015), no research has yet established this causal relationship at the household level. We fill this gap by estimating the effect of an exogenous reduction in WIC (Women, Infants, and Children) availability on households' utilization of private nutrition assistance provided by Crossroad Community Services (CCS).

1.1.1 Private Nutrition Programs and CCS

Private nutrition assistance programs are food programs run by non-governmental organizations (NGOs) and supported primarily by donations and some federal grants/subsidies, such as food products sourced through the US Department of Agriculture subsidies programs. Before the 1980s, the majority of private nutrition assistance in the United States took the form of small soup kitchens in large metropolitan areas (Berner and O'Brien, 2004). Those private nutrition assistance programs were initially designed as an emergency response to short-term crises (Curtis & McClellan, 1995). However, demand for more nutritional assistance led to the establishment of food banks. Food banks increased organizational capacity of private food nutrition assistance programs and primarily respond to the needs of households with food insecurity and hunger (Tarasuk and Beaton, 1999; Teron and Tarasuk, 1999). With President Ronald Regan's welfare expenditure reductions, food banks developed rapidly after 1980. According to a survey completed in 2002, more than 90% of the food banks in the United States are established after 1981 (Poppendieck, 2009). In the late 1990s, the demand for food banks increased further with President Clinton's Personal Responsibility and Working Opportunity

Act. Food banks secure food donations from a variety of sources including food and grocery manufacturers, retailers, shippers, packers, growers and some government agencies. They partner with these agencies to identify excess food, and handle transportation and storage logistics (Tarasuk and JEakin, 2005). Instead of distributing foods to households directly, food banks rely on local food pantries to reach communities in need (Feeding America website). In our study, we utilize administrative data from one such food pantry, Crossroads Community Services (CCS).

Located in downtown Dallas, CCS is the most significant distributor of the North Texas food bank. CCS provided 2.7 million pounds of food to 15,055 households in 2015. CCS distributes food through both its in-house pantry and through a network of community distribution partners (CDPs), which are also located in Dallas County. Households that meet CCS's residential requirement are allowed to visit CCS or one of its CDP sites at most once a month. Based on the size of households, each household is provided a certain amount of vouchers in each category of foods, such as protein, grains, and vegetables. As long as availability allows, households can choose brands/flavors freely in each category. On average an 4-member household received around 100 pounds of foods in a single visit during the period of our study. Price-Waterhouse-Coopers estimated that the average value of the typical CCS food package for an 4-member household to be $200.

1.1.2 Public Nutritional Assistance and WIC

There is a variety of public nutrition assistance programs in U.S targeting different populations (Scholz and Levine, 2001). In 2016, federal expenditures on USDA's 15 nutrition assistance programs totaled $101.9 billion dollars (USDA website). The four largest public nutrition assistance programs are the Supplemental Nutrition Assistance Program (SNAP, formerly known as Food Stamps), the Special Supplemental Nutrition Program for Women, Infants and Children (WIC), The National School Lunch Program (NSLP) and the School Breakfast Program (SBP). Among them, the Supplemental Nutrition Assistance Program is the largest and covers around one in every seven people in the United States. It is also the most universal program, targeting no specific group beyond income criteria. SNAP provides vouchers to eligible households that can be used to purchase a wide variety of foods at a wide variety of authorized locations. Other programs specifically target the needs of children. NSLP and SBP provide free or low-cost meals to students from low-income households. WIC provides nutrition benefits for the youngest population including women who are pregnant, infants and children under age 5. Annually, WIC provides services to an estimated 8 million individuals in the US at a cost of approximately 6.2 billion dollars (2015). In this research, we focus on the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC).

The WIC program was first established as a pilot program in 1972 through an amendment to the Child Nutrition Act of 1966. Four requirements determine a household's eligibility for WIC assistance. First, the household must reside in the United States. Second, the applying household’s income must be lower than guidelines selected by each state. In Texas, the location of our study, household income must be below 185% of the federal poverty level. The third requirement is the nutrition risk requirement, which is usually checked at WIC clinics for free and in almost all cases is satisfied by households who have met the income criteria. Finally, WIC participants must fall into one of 4 eligible groups: pregnant women, postpartum or breastfeeding women, infants up to their first birthday and children up to their fifth birthday.

Once applied and deemed to be eligible, households receive General Electronic Benefit Transfer (EBT) cards which allow them to purchase select foods at authorized locations. WIC is not a direct cash transfer program, because the benefits are restricted to goods within a prescribed food package, which varies among the different eligible groups (Texas Health and Human Services). The WIC food package contains a fixed amount of food in each category and does not allow households to make substitutions across categories. However, households are allowed to choose any brands they want as long as the brands are WIC approved. For example, an eligible child from 2 years old to 5 years old in Texas receives 36 ounces of cereal, 2 pounds of whole grains, $8 worth of fruits an vegetable, 128 ounces of juice, 3 gallons of low-fat or fat-free milk, 1 quart of yogurt, 1 pound of cheese, 1 dozen eggs and a 16 ounce jar of peanut butter (USDA). The average value of the WIC food package is $61.24 per participant, per month (Center on Budget and Policy Priorities, 2016). All eligible participants in the same eligibility group receive the same level of WIC benefit regardless of income.

Overall, existing research provides evidence that WIC has a positive impact on health (Currie, 2003; Hoynes and Schanzenbach, 2015; Oliveira and Frazao, 2015). Among pregnant women, WIC participation has been associated with a decreased liklihood of low birthweight babies (Figlio, Hammersma, and Rotch, 2009; Hoynes, 2009; Hoynes, Page, and Stevens, 2011; Currie and Ranjani, 2014; Rossin-Slater, 2013), but has a negative association with breastfeeding (Jiang et al., 2010). WIC also has been associated with positive effects on nutrition intake among young children (Rose et al., 1998; Kreider et al., 2012).

Despite the significant evidence on the positive effects of WIC, we know little about the impact on households when WIC eligibility is lost at the time that the youngest child reaches their fifth birthday. Understanding the impact of losing WIC benefits on a child's nutrition intake and overall health is complicated because at the time of the 5^{th} birthday children are also transitioning into new school environments which are likely to confound any study focused on child health. However, we can observe whether households who utilize both private and public food assistance change their behavior in an attempt to make-up for the lost benefits, and we can measure the extent to which these households are capable of making up for lost WIC benefits with more frequent utilization of private nutrition assistance.

1.2 Methods

1.2.1 Identification Strategy

WIC eligibility is likely correlated with many unobserved characteristics that are also related to the frequency of utilization of private nutrition assistance (Jacknowitz and L.Tiehen, 2009, 2010; Hoynes and Schanzenbach, 2015). For example, lower income households are both more likely to be eligible for WIC and have a need to visit CCS more frequently. Further, even if we can perfectly control for household income, not all eligible households participate in WIC, and the selection into these programs is not random (Currie, 2003; Bitler, 2003; Bitler and Currie, 2005; Ludwig and Miller 2005). If program participants were more motivated to find assistance and more capable of finding and receiving these public benefits, then they likely were also more likely to be motivated and capable of utilizing private nutrition assistance. A naïve regression of CCS visiting probability on WIC participation would likely suffer from endogeneity and produce biased estimates.

To address this problem, we exploit the exogenous loss of WIC eligibility that occurs at a child's 5th birthday to understand the impact of the loss of WIC benefits on household's CCS visiting frequency. When the youngest child of a household reaches his / her fifth birthday, or becomes 60 months old, it suddenly becomes almost impossible for a household to be eligible for WIC. As shown in Figure 1.1, households had around 30 percent probability of actually receiving WIC when the youngest child was less than 60 months old. After the cutoff, the probability sharply dropped to less than 5 percent. If the change of the probability of receiving WIC is the only thing that happened exactly at the cutoff, which we will verify later, households around the 60-month threshold would be comparable in every way except for WIC eligibility and the associated probability of actually receiving WIC. Thus, by comparing visiting probability of households just to the left of the cutoff and households just to the right of the cutoff, we will be able to estimate the casual impact of receiving WIC on households' probability of visiting CCS.

1.2.2 Empirical Model

We adopted a fuzzy regression discontinuity design (Imbens and Lemieux, 2008; Lee and Lemieux, 2010) to identify the causal relationship between losing WIC and the probability of utilizing private nutritional assistance. Specifically, we estimated the system of equations as two-stage least square (2SLS), with equation (2) estimated as a probit regression:

$$(1) \qquad receivewic_{it} = \alpha_0 + \alpha_1 WICeli_{it} + f(agedistance_{it}) + X\gamma + \nu_{it}$$

$$(2) \qquad visit_{it} = \beta_0 + \beta_1 \widehat{receivewic}_{it} + f(agedistance_{it}) + X\delta + \mu_{it}$$

where $receivewic_{it}$ is an indicator variable that assumes the value of 1 if household *i* received WIC benefit, at month *t*. $WICeli_{it}$ is an indicator for households with at least one child of 60 month old or less at month *t* and $visit_{it}$ indicates households who utilized CCS nutrition assistance at month *t*. X is a vector of household socio-demographic characteristics. β_1, the primary coefficient of interest, captures the effect of WIC availability on the household's probability of utilizing CCS assistance at month *t*. We refer to this as the local average treatment effect (LATE).

The running variable, $agedistance_{it}$, is the monthly age of the youngest child centered at 60 months and is included as a polynomial function. Several different polynomial specifications were explored. In our primary specification, we used a quadratic polynomial that allowed the coefficients of the polynomial to differ at the left side and right side of eligibility cutoff:

(3) $$f(agedistance_{it}) = agedistance_{it} + agedistance_{it}^2 + WICeli_{it} * agedistance_{it} + WICeli_{it} * agedistance_{it}^2$$

We also estimated the Intent-to-Treat (ITT) effect, or the effect of WIC eligibility--independent of actual WIC participation--on households' probability of utilizing CCS nutrition assistance. Estimates for θ_1 in the following probit regression measure the ITT effect:

(4) $$visit_{it} = \theta_0 + \theta_1 WICeli_{it} + f(agedistance_{it}) + X\delta + \psi_{it}$$

Four criteria must be met to ensure the validity of the Fuzzy Regression Discontinuity design. First, a household being eligible for WIC must be strongly correlated with the household's actual probability of receiving WIC. Figure 1.1 plots households' probability of receiving WIC against the centered monthly age of their youngest children. It shows that after the eligibility cutoff, households' probability of receiving WIC sharply drops. Table 1.4 shows that the correlation between age eligibility and probability of receiving WIC is strong, significant, and consistent over different specifications. Given the evidence, this criterion holds. Second, being eligible for WIC must not make some households less likely actually to enroll in WIC. In the context of this study, this criterion is reasonable. Third, households must be unable to precisely manipulate the running variable, which means that no specific type of households will be clustered on one side of the eligibility cutoff, and households around the cutoff are comparable in everything but WIC eligibility. Finally, the age cutoff impacts households' visiting frequency exclusively through its impact on households' probability of receiving WIC, and other explanations for this change are unlikely.

Precise manipulation of the running variable is unlikely because households are required to provide documents at the time of WIC enrollment to verify the birthdate of their children. There is also no evidence that households first enroll their children, but change the birthdate near the threshold so that they can remain being eligible for WIC. To verify the imprecise manipulation of the running variable at the threshold, we also estimated a set of equations that described the behavior of control variables at the threshold as a system of Seemingly Unrelated Regressions wherein each equation, x_i is an element of X

(5) $$x_{it} = \mathrm{A}_0 + \mathrm{A}_1 WICeli_{it} + f(agedistance_{it}) + X\Gamma + \rho_{it}$$

A joint test of the coefficients A_1 fails to reject the null hypothesis that the coefficients are jointly equal to 0, which indicates that all other variables transitioned smoothly at the threshold Table 1.2 shows the estimated coefficients and test. This test provided evidence that there was no perfect manipulation of the running variable (Lee and Lemieux 2010), and the households just to the left of the cutoff and the households just to the right of the cutoff are comparable. Figure 1.3 plots a variety of household characteristics against the running variable and shows that the household characteristics transitioned smoothly through the threshold. We also tested if the density of observation changed significantly across the threshold with McCrary Density Test (McCrary, 2008). The result of this test (Figure 1.4) provides further evidence that there was no precise manipulation of the running variable.

Finally, we need to assume that passing the threshold of WIC eligibility influenced households' visiting decision solely through the change in their eligibility for WIC. This assumption may be challenged because children are more likely to go to school after they turn 5 and become eligible for two other public assistance programs, the NSLP and SBP. Children eligible for a school lunch program would improve the food security of households and decrease their need to visit CCS. However, this effect is unlikely to happen precisely at their fifth birthday. Bias introduced by these school-based public assistance programs, if it exists, is likely to be downward bias. To assess the potential for this bias, we also estimated a specification that limited the sample to summer visits only. Since young children are much less likely to receive school lunch during summer break, this specification should be able to eliminate potential bias from school lunch programs.

1.3 Data

Primary study data was obtained from CCS administrative database and was collected from 2012 to 2016. CCS requires clients to provide proof of residency and household membership by providing documents such as driver's license, birth certificates, and utility bills. This verification process is repeated every 6-months. CCS also records a variety of other self-reported data including clients' socio-demographic characteristics, and participation in public assistance programs such as SNAP and WIC.

As a product of this administrative process, we acquired a unique dataset that includes individual characteristics of each member of each household, the change of household members over time and whether a household visited CCS to utilize the assistance provided in each month. Our analysis sample includes all households with at least one child within certain months to the WIC eligibility cut-off during the study window (start date-end date). We included two specifications. In the standard specification, we only kept observations in which the youngest household member was between 48 and 72 months old, which is one year before and after the eligibility cutoff. In a "short bandwidth" specification, we only kept observations in which the youngest household member was aged 54 to 66 months old, which is a half year before and after the eligibility cutoff. We dropped households with very low visiting frequency (less than three times a year) because households that rarely visit CCS often report that they have other assistance sources. The inclusion of these households in our sample would bias results because utilization of private nutrition assistance is measured with error. We also excluded households with incomplete data for variables used in the empirical analysis.

Table 1.1 summarizes the characteristics of the 9,716 observations included in our analysis. On average, households had 4.9 members and conducted 82% of visits by cars. Households served by CCS were primarily African American (29%) or Hispanic (65%). 18% of households received some kinds of health care assistance such as Parkland Plus, a local health assistance program in Dallas County. Only 14% of household heads have a high school diploma or better.[1]

1.4 Results

1.4.1 Main Results

Table 1.3 presents the estimation results of equation (1) and equation (2) as a two-stage least squares system with our main specification. This specification used a quadratic polynomial and allowed the coefficients to differ across the threshold. The first and second state estimates are presented in Columns 1 and 2, respectively. First stage estimates (Column (1)) indicate that when a households' youngest children aged out of the eligibility requirement of the WIC program, they have an average 22.1 percentage point lower probability of receiving WIC. Estimates in Column (2) indicate that households' loss of WIC benefit led to a 19.9 percentage point increase in households' probability of visiting CCS in a month (LATE effect). Estimates in

[1] Household head is the person who registered this household at CCS. This is the only person in the household with complete information on education.

Column (3) indicate that losing WIC eligibility led to a 4.05 percentage point increase in households' probability of visiting CCS in a month (ITT effect).

Tables 1.4 and Table 1.5 present estimation results using a variety of specifications. The impact of WIC eligibility on households' probability of receiving WIC benefits is positive and statistically significant across all specifications (Table 1.4). Specification (1) is the main specification presented in Table 1.3. The estimates of Specification (2), which was similar to our primary specification but didn't allow the coefficients to differ across the threshold, showed that the LATE is smaller at 12.8 percentage point increase in visiting probability. Specification (3) is similar to the primary specification but included no control variables. The estimation of this specification showed that the effects were marginally larger. The estimated discontinuity labeled in Figure 1.1 and Figure 1.2 comes from this specification. Specification (4) limited the sample to only summer visits to test for the potential impact of school lunch programs. The estimated LATE is only marginally larger than LATE estimated in Specification (1). This result indicated that if there were potential bias introduced by the school lunch programs, estimated LATE was likely downward biased and the biases were small. Specification (5) used the same specification as Specification (1) but used a six months estimation bandwidth rather than 12 months in other specifications. Specification (6) used a linear polynomial function.

Specification (7) used season rather than month as the time unit to estimate the effect of WIC participation on a relatively longer term. Estimation results from Table 1.4 and Table 1.5 showed that having a child under 20 seasons old increased the household's probability of receiving WIC at least one month in that season by 14.2 percentage points and decrease a household's probability of utilizing CCS benefits at least once in that season by 5.6 percentage points. Calculated Local Average Treatment Effect from Specification (7) showed that losing WIC benefits will lead to a 35.3 percentage point increase in households' probability of visiting CCS at least one times in that season.

We also estimated specifications with different samples. Specification (8) used an infrequent visitor sample with households whose visiting frequency was less than 3 times a year. We have much fewer data on these households, and they reported higher probability of having other source of assistance. They were also more likely to have transportation difficulties accessing CCS's assistance. As expected, these households did not increase visiting probability after aging out of WIC eligibility. Specification (9) used a frequent visitor sample but dropped households which visited every month before aging out of WIC eligibility, and thus unable to further increase their visiting frequency. Results showed that by including only households that are potentially able to increase visiting frequency, the estimated increase in visiting frequency is five percentage points higher.

One can see from Table 1.4 that the first stage results are strong and consistent over different specifications. The second stage results in Table 1.5 are mostly consistent, but the significance level varies. The lower significance level in Specification (4) was likely due to smaller sample size. Gelman and Imbens (2017) suggested that specifications using high order polynomial functions can be misleading and produce inaccurate results, so we did not include any specification with higher order polynomials.

1.4.2 Comparison of WIC benefits and CCS Substitution

On average WIC provided $61.24 in food per participant per month in the fiscal year 2016 (Center on Budget and Policy Priorities estimation, 2016). Price-Waterhouse-Coopers estimated the value of CCS food packages to be $50 per household member per visit. Based on the average family size in the sample, on average households received food packages worth $244. A simple calculation based on the treatment effect of 19.9 percentage point increase in visiting probability, using Specification (1), showed that average households received $48.6 more from CCS after they lost WIC benefits. Thus, by increasing visits to CCS households compensated for 79% of their lost WIC benefits. Estimates from Specification (2), which are more conservative, showed that households compensated for 51% of their lost WIC benefits by increasing visits to CCS; and estimates including only households who could increase visiting frequency because they were not visiting monthly prior to loss of WIC eligibility (Specification 9) indicate that households compensate for 84% of their lost WIC benefits.

1.5 Discussion

In this paper, we identified the causal effect of lost WIC eligibility and a household's probability of utilizing private nutrition assistance. We found a significant compensation effect of private nutrition assistance programs. After the youngest child in a household aged out of WIC eligibility, the household became 19.9 percentage points more likely to utilize private nutrition assistance at CCS; and for the average household, the CCS benefits compensated for 51 percent to 84 percent of a household's lost WIC benefits. Our results suggest that private nutrition assistance plays an essential role as a part of the social safety net, especially when public assistance becomes unavailable.

Public and private nutrition assistance programs appear to be substitutable for many households. However, our results indicate that no households were fully able to compensate for the lost WIC benefits through more frequent use of CCS. Implicit in this assessment is the assumption that households were fully utilizing their entire WIC benefit prior to the loss of those benefits. If this assumption is not true, then compensation estimates should be considered a lower bound.

Additionally, substitutability of the non-food element of WIC and CCS is unclear. WIC provides participants with health screening, nutrition education and referral to other social service programs. CCS and food banks can provide some of these services, but the quantity and quality of services provided are likely to be different. CCS also provides households with donated clothes, which WIC does not provide. Another complication is the way households acquire assistance. WIC provides households with EBT cards which are widely accepted in grocery stores around the country. Food provided by food banks and CCS are comparatively limited in variety and flexibility. Based on these factors, although we have found private nutrition assistance programs can at least partially compensate part of households' loss in term of the "food gap," more research is need on other aspects of these assistance programs.

Because of CCS institutional policies, households are only allowed to visit one time per month. Therefore, there is an upward bound on the amount of additional food that households can obtain. This bound may prevent some households from fully compensating for the lost WIC benefits. Results suggest that if funding is available, private nutrition assistance programs that offered more generous nutrition support in the months following the loss of WIC might provide valuable assistance to households adjusting to the loss of WIC.

This study's results also provide evidence that loss of WIC assistance is a significant event for households. Because WIC benefits for older children are often not fully utilized, some might argue that loss of these benefits is relatively inconsequential. Our results suggest otherwise. Qualitative interviews with CCS clients provide evidence of numerous psychological and transportation-related costs associated with visiting CCS. Clients frequently state that they choose not to visit CCS unless all other means of providing food for the household are exhausted. In this context, the estimated 19.9% increase in visiting frequency is meaningful. Results indicate the loss of WIC caused a significant behavior change on the part of households. The welfare consequences of the lost benefits should be further explored in future studies.

1.6 Conclusion

When faced with a loss of public nutrition benefits, households increased their utilization of private nutrition assistance programs. In doing so, they were able to partially compensate for their lost benefits, suggesting that public and private nutrition assistance programs are at least partially substitutable. Results also, however, suggest that loss of public nutrition assistance is a significant event for households and that the short- and long-term consequences on child and household outcomes should be the subject of further study.

Appendix A

Tables and Figures for Chapter 1

Table 1.1. Summary Statistics

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
Dependent Variables					
Received WIC	9,327	0.173	0.379	0	1
Visit Probability	9,716	0.484	0.500	0	1
Control Variables					
Family Size	9,716	4.895	1.691	2	12
Visit by Car	9,716	0.823	0.382	0	1
African American	9,716	0.288	0.453	0	1
Hispanic	9,716	0.651	0.477	0	1
Health Assistance	9,716	0.178	0.383	0	1
High School or Better	9,716	0.139	0.345	0	1

Table 1.2. SUR Estimates and Joint Test

VARIABLES	Coefficient	Std. Error
Family Size	0.0007	0.0965
Visit by Car	0.0061	0.0235
African American	0.0136	0.0259
Hispanic	-0.0087	0.0257
Health Assistance	-0.0084	0.0234
High School or Better	-0.0075	0.0206
Observations =9716	$\chi^2(6) = 0.61$	P = 0.9962

Table 1.3. Estimation Results for Main Specification (N=9,327)

VARIABLES	(1) Received WIC	(2) Visit Probability	(3) Visit Probability
Age Eligible	0.221***		-0.0405***
	(0.0147)		(0.0127)
Received WIC		-0.199***	
		(0.0562)	
Month to Cutoff	-0.0110***	-0.000535	3.48e-05
	(0.00208)	(0.00440)	(0.00456)
Month to Cutoff * Age Eligible	0.000583***	0.000308	0.000336
	(0.000153)	(0.000321)	(0.000340)
(Month to Cutoff)^2	0.0203***	0.000741	-0.00107
	(0.00513)	(0.00538)	(0.00519)
(Month to Cutoff)^2 * Age Eligible	2.83e-05	-0.000250	-0.000407
	(0.000398)	(0.000347)	(0.000399)
Family Size	0.0322***	0.0174***	0.0110***
	(0.00529)	(0.00287)	(0.00236)
African American	0.000947	0.182***	0.181***
	(0.00520)	(0.0286)	(0.0286)
Hispanic	0.0903***	0.130***	0.112***
	(0.0241)	(0.0243)	(0.0242)
Health Assistance	-0.0170*	-0.0207**	-0.0173*
	(0.00854)	(0.0105)	(0.0105)
Visit by Car	0.108***	0.130***	0.109***
	(0.0243)	(0.0103)	(0.00838)

OLS coefficient estimates are provided in column 1; estimates in columns 2 and 3 are estimated average marginal effects with Logit Regression. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4. First Stage Estimation Results for Alternative Specifications

Regression Specification	Polynomial Function	Flexible Polynomia[1]	Controls	Time Unit	Age Eligible (First Stage Estimate)
(1)	Quadratic	Yes	Yes	Month	0.221***
(2)	Quadratic	No	Yes	Month	0.220***
(3) Graph	Quadratic	Yes	No	Month	0.223***
(4) Summer	Quadratic	Yes	Yes	Month	0.221***
(5) 6 month	Quadratic	Yes	Yes	Month	0.231***
(6)	Linear	Yes	Yes	Month	0.221***
(7)	Quadratic	Yes	No	Season	0.142***
(8)	Quadratic	Yes	Yes	Month	0.238***
(9)	Quadratic	Yes	Yes	Month	0.221***

Note: Number of observation is 9,327 for Specification (1), (2) and (3). Number of observation is 3,120 for specification (4); 4,747 for specification (5); 7,465 for specification (6); 47,772 for specification (8) and 8,239 for specification (9).

[1]"Flexible Polynomial" indicates that the coefficient of the running variable polynomial is allowed to differ at the left and right side of the eligibility cutoff.

[2]"Summer" indicates limiting sample to visiting in May, June, July and August.

[3]"6 month" indicates using 6 month bandwidth instead of 12 month in other specification.

[4] Used sample of infrequent visitors

[5] Dropped households that visit every month.

*** p<0.01, ** p<0.05, * p<0.1

Table 1.5. Second Stage Estimation Results and Estimated ITT Effect

Regression Specification	Polynomial Function	Flexible Polynomial	Controls	Time Unit	Intent-to-Treat Effect	Local Average Treatment Effect
(1)	Quadratic	Yes	Yes	Month	-0.0405***	-0.199***
(2)	Quadratic	No	Yes	Month	-0.0282***	-0.128***
(3) Graph 1	Quadratic	Yes	No	Month	-0.0422***	-0.205***
(4) Summer[2]	Quadratic	Yes	Yes	Month	-0.0476*	-0.223*
(5) 6 month[3]	Quadratic	Yes	Yes	Month	-0.0428***	-0.185***
(6)	Linear	Yes	Yes	Month	-0.0281***	-0.126***
(7)	Quadratic	Yes	No	Season	-0.0562**	-0.353**
(8)[4]	Quadratic	Yes	Yes	Month	0.0035	0.0217
(9)[5]	Quadratic	Yes	Yes	Month	-0.0481***	-0.241***

Note: Number of observation is 9,327 for Specification (1), (2) and (3). Number of observation is 3,120 for specification (4); 4,747 for specification (5); 7,465 for specification (6); 47,772 for specification (8) and 8,239 for specification (9).
[1]"Flexible Polynomial" indicates that the coefficient of the running variable polynomial is allowed to differ at the left and right side of the eligibility cutoff.
[2]"Summer" indicates limiting sample to visiting in May, June, July and August.
[3]"6 month" indicates using 6 month bandwidth instead of 12 month in other specification.
[4] Used sample of infrequent visitors
[5] Dropped households that visit every month.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 1.1. Eligibility Cutoff and WIC probability

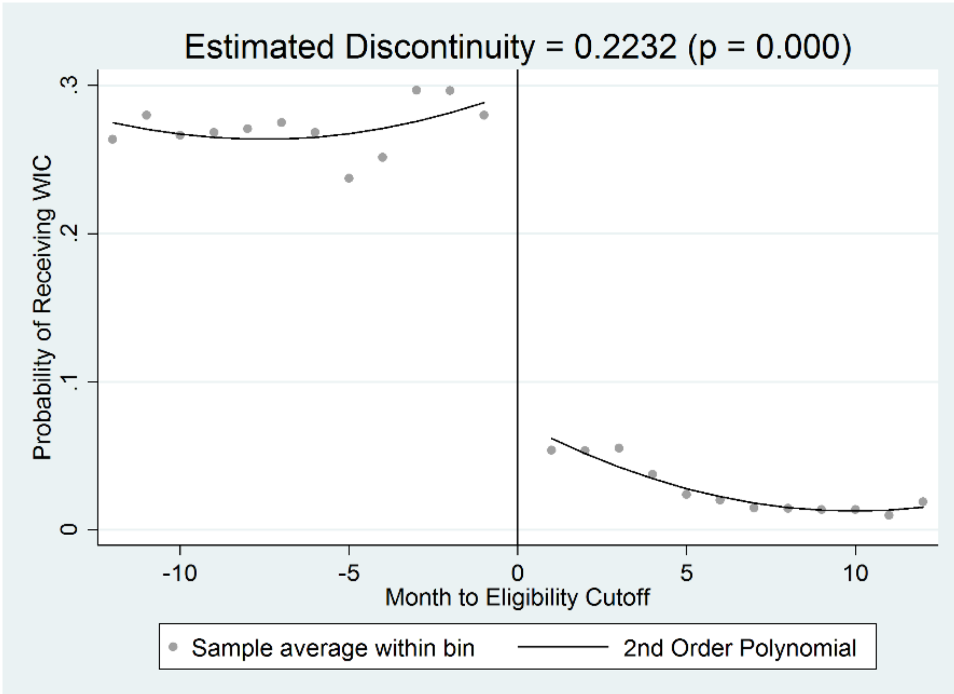


Figure 1.2. Eligibility Cutoff and Visiting Probability

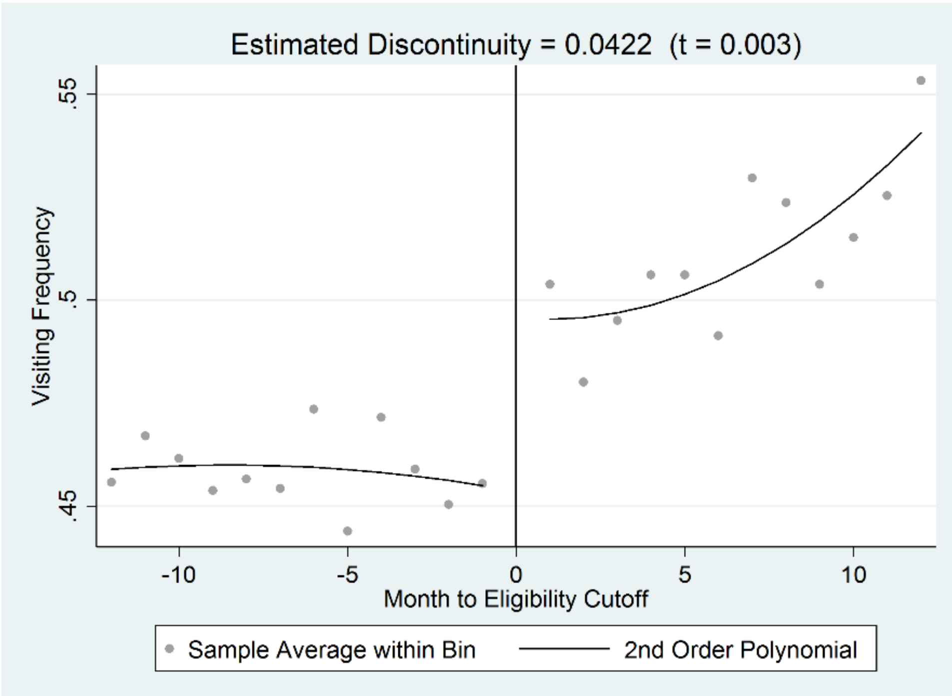
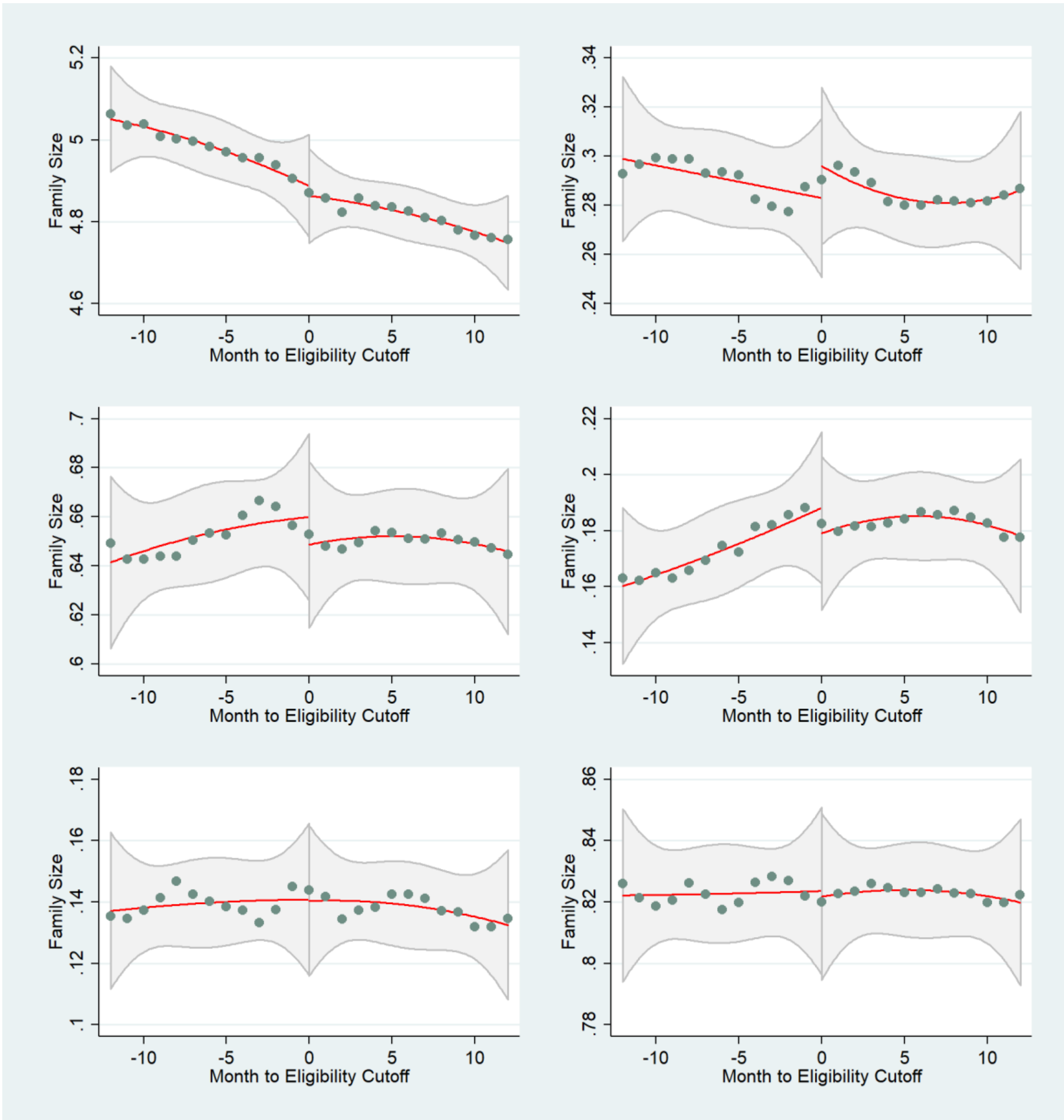
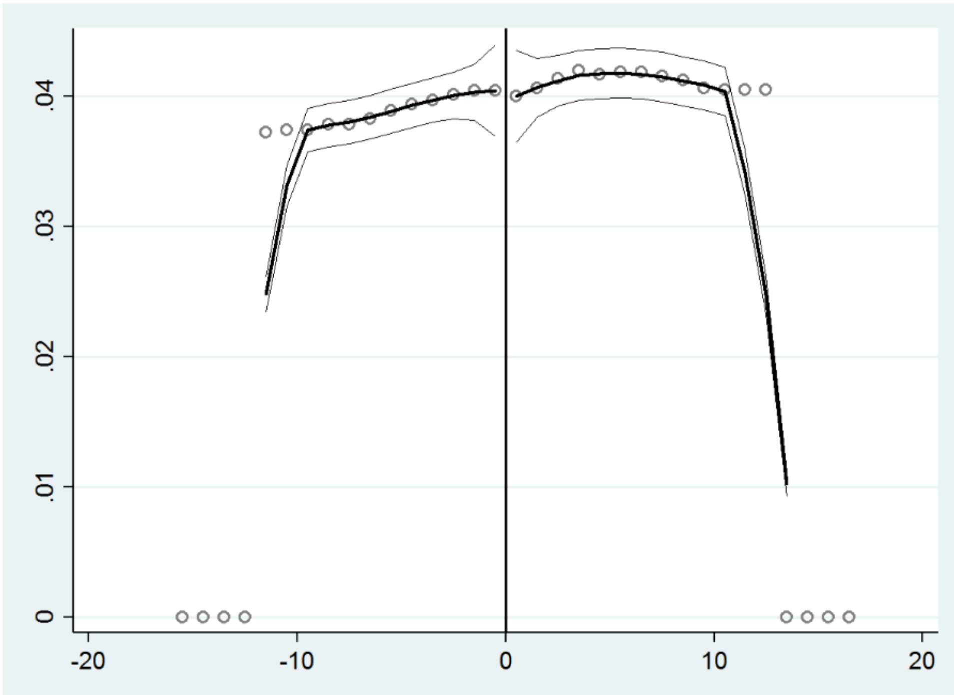


Figure 1.3. Control Variables through the Cutoff



Note: With 95% Confidence Interval and 2nd Order Polynomial

Figure 1.4. McCrary Test



CHAPTER 2

NETWORK EXPANSION AND UTILIZATION OF PRIVATE NUTRITION ASSISTANCE

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2.1 Introduction

In the U.S., 15.6 million households are food insecure despite layers of public and private food assistance programs that are available. Community-based non-governmental organizations (NGOs) exist across many communities to fill the gap for low-income food insecure households when public food assistance is either insufficient or unavailable to meet emergency food needs. However, these programs rarely solve the problem of food insecurity. According to a report by Feeding America, an NGO managing a series of food banks across the United States, 83 percent of its clients are food insecure (Feeding America, 2014). One way the efficiency of these programs can be improved is through increasing the frequency with which households utilize services. However, the determinants of client visiting frequency have received very little attention. Instead, NGOs' operations are designed with the idea that being open longer hours, at convenient locations, and with fewer administrative hurdles is consistent with better client service.

While these rules-of-thumb seem reasonable, operational decisions often involve trade-offs. For example, the most convenient locations may be difficult to staff during the hours most convenient to clients because they are located in neighborhoods with limited volunteer capacity. In other cases, services may be expanded, but the funding for expansion requires additional enrollment paperwork. In the present study, we examined a natural experiment whereby an NGO was able to expand its food distribution network; and we estimated the causal impact of this expansion on frequency of household utilization. The expansion involved a classic trade-off faced by NGO's: geographic expansion, but a reduction in service hours. Further, the expansion necessitated a programmatic change that required clients to pre-enroll for services. We are unable to dis-entangle the pre-enrollment impacts from the geographic expansion, but the potential implications of each are discussed.

2.1.1 The nutrition assistance programs in the United States

In the United States, there are a variety of federally funded nutrition assistance programs targeting different populations. In 2016, federal spending on USDA's 15 nutrition assistance programs totaled 101.0 billion dollars (USDA website). Those programs include the Supplemental Nutrition Assistance Program (SNAP, formerly known as Food Stamps), the Special Supplemental Nutrition Program for Women, Infants and Children (WIC), The National School Lunch Program (NSLP) and the School Breakfast Program (SBP). According to USDA, the goal of those programs is to reduce food insecurity and hunger by increasing food access, nutrition, and nutrition education for low-income Americans (National Research Council, 2013). However, food insecurity in the United States remains high. In 2016, 12.3 percent of U.S households, or 15.6 million people, were food insecure. Among them, 6.1 million households had very low food security, which means that "normal eating patterns of one or more household members were disrupted and food intake was reduced at times during the year because they had insufficient money or other resources for food." (USDA Economic Research Service, 2017)

Another important part of the social safety-net is private nutrition assistance programs run by NGOs. Mostly supported by donations, those programs began with small soup kitchens in large metropolitan areas (Berner and O'Brien, 2004). They were initially designed as an emergency response to short-term crises but quickly expanded as the need arose (Curtis & McClellan, 1995). Facing large demand, those non-profit organizations eventually evolved into food banks, which developed rapidly after 1980 with President Reagan's welfare cut. Feeding

America, the nation's largest food bank organization, runs a network of more than 200 food banks. Those food banks distributed three billion pounds of food in 2016 to 37 million individuals through their network of local distributors. (Feeding America, 2017). In the Dallas area, the largest distributor is Crossroads Community Services (CCS), the organization whose clients comprise our study population.

2.1.2 Crossroads Community Service and its Community Distribution Partners.

CCS distributed 2.7 million pounds of foods to 15,055 households in 2015. CCS's main food distribution site is a walk-in food pantry that is open Monday-Thursday from 8am-12pm. Beginning in 2012, CCS also began to expand food distribution through a network of Community Distribution Partners (CDPs). CDPs are organized by a variety of organizations including churches, public housing communities, and community centers. CDPs provide volunteers who travel to the main CCS site once a month to obtain food that is then redistributed to individual households that live in the community surrounding the CDP. CCS began its CDP program in 2012 and has continuously added new CDP sites to its network. In 2015, CCS had a total of 62 CDPs. From 2012 to 2015, CCS enrolled 29 new CDP sites to its distribution network. CDP sites are organized by community organizations that serve low-income families and usually begin with only a few households who receive food. However, they grow over time as nearby households learn of the service. We will examine the impact of this exogenous change in the local food distribution network in neighborhoods where CDPs locate on household's frequency of utilizing services.

For all clients, CCS verifies client's household eligibility to receive assistance based on household income and residence location. Households who are eligible for assistance next provide CCS with detailed household information including age, gender and educational attainment of all household members. Each household may receive food up to once per month and receive a food package that provides enough food for 21 meals for each household member. The food package for an average four-member household is usually around 100 pounds and households are responsible for providing their own means of transporting the food to their home.

CDPs apply the same rules, collect the same data and provide a similar amount of food as the main CCS site, though food selection at CDP sites is more limited. There are three primary differences between how clients access food at CCS's main site and CDP sites. First, clients have many more options for when they visit CCS's main site as compared to CDP sites. The main CCS site is open four days a week for 3-4 hours a day (48-64 hours per month), while CDP sites are typically open only 2 hours per month. Second, clients must pre-enroll to receive food at CDP sites. In most cases, clients must demonstrate eligibility and complete enrollment paperwork 1-2 weeks before they can begin receiving food at CDP sites, while clients may come to the main CCS site anytime that it is open, demonstrate eligibility and receive food that same day. Lastly, the travel distance between clients' homes and the food distribution site is much less for CDP sites. On average, the Euclidean distance between CCS clients' residency and CCS's main site is 10.7 kilometers, and the distance between CCS client's residency and the nearest CDP site is 1.63 km.

2.1.3 The puzzle of insufficient utilization of assistance, and research question

Our previous research (Si and Leonard, 2018) found that private nutrition programs were effective parts of the social safety-net. Using data from CCS, we found that when facing negative income shocks, some low-income households increased utilization of private nutrition assistance to partially compensate for the loss. However, our previous research also raised some questions. Not all households actively utilized private nutrition assistance following a negative income shock.

One possible explanation for sub-optimal utilization of private nutrition assistance is that transportation costs present a significant barrier to participation. Indeed, transportation is frequently noted as a crucial factor limiting poor households' ability to secure needed resources (Bouchard, 2015). Glaeser and Kahn (2008) found that the lack of access to alternative transportation might be one important reason that the poor are more likely to live in the city center, where land is more expensive but public transportation is accessible. Private nutrition assistance, unlike public assistance, usually necessitates that households travel to a distribution site to acquire food. Households that frequently utilize private nutrition assistance are more likely to visit by car compared to households that use private assistance infrequently. Households that do not own any vehicles may be forced to carpool with their friends or neighbors, which requires additional coordination effort. Although it is possible for many households to access assistance by using public transportation, in practice it is expensive both in terms of time and money; and transporting large quantities of food using public transportation is, in many cases, impractical—particularly for seniors and people with disabilities.

The CDP network has the potential to significantly reduce households travel costs and remove the transportation barrier to food access. However, the CDP model presents a trade-off between travel costs and convenience. Accessing food at a CDP site is less time consuming, but it must be done during a short 2-hour window that is determined by the CDP site and is not customized to meet clients scheduling needs. In addition, CDP utilization requires pre-enrollment. This may be a deterrent to some clients; however, it may also serve as a pre-commitment device that increases the odds that clients will come to the CDP site monthly to receive food. Numerous behavioral economic studies have shown that pre-commitment devices are effective at helping low-income individuals follow-through on behavior that they want to do, but often postpone as other competing interests promise more immediate benefits (Pesendorfer, 2006; Milkman et al., 2008).

In the present study, we will test these hypotheses. In particular, we test whether a new CDP site is associated with increased client visiting frequency.   If so, then we assert this increase is caused by the decreased travel distance and/or the pre-commitment benefits of the client enrollment process. Further, these effects are strong enough to overcome any potential loss in convenience associated with limited visiting hours available at CDP sites.

2.2 Data and the Identification of Treatment Groups

This study used CCS's administrative data from 2012 to 2015. CCS administrative data is recorded opportunistically when low-income households in Dallas County, TX visit CCS's main site or a CDP site to receive food. As a part of CCS's administrative process, address and children's age is verified from documents such as driver's license, birth certificates, and bills. Residential addresses were used to calculate the length of the shortest curve between each household' residency and CCS's main site and each CDP location using Vincenty's (1975) equations. Figure 2.1 is a map of the 29 CDPs that opened from 2012 to 2015 and households' home addresses.

A difference in difference approach was used to estimate the causal effect of the opening of a new CDP on client visiting frequency. Treatment assignment is determined by the household's distance to the nearest CDP site. Households near to a CDP site are assigned to the treatment group and more distant households comprise the control group. In this study, we used the "donut" model similar to Currie and Walker (2011), and Lin and Walsh (2014). In the baseline model, treated households are located within 2 km from a new CDP site and control households are located between 2.4 km and 4 km from a new CDP site.

Selection of an appropriate geographic boundary for treatment groups and control groups is essential for this study to estimate the impact of opening CDPs accurately. Unlike Currie and Walker (2011), which justify the choice of treatment and control group size based on estimated air pollution dissipation, we do not know the size of the true area of the treatment, and a model for likely treatment size does not exist. If the boundary defining the treatment group is too small, it means that there will be households that are treated (i.e. who are impacted by the new CDP site) but assigned to the control group. As a result, the estimated treatment effect will be smaller than the true treatment effect. If the radius defining the treatment boundary is too large, then there will be households that are not treated but assigned to the treatment group. As a result, the estimated treatment effect will also be smaller.

Conceptually, if a clear boundary between treatment group and control group actually exist, we can simply try different radius for treatment/control groups to uncover this boundary. As the boundary for the treatment group increases, there will be fewer and fewer treated households in the control group; and the estimated treatment effect should increase. After the true treatment group size is reached, further increases in the treatment boundary will result in untreated households being assigned to the treatment group; and the estimated treatment effect will begin to decrease. Thus, the radius with the highest estimated treatment effect will be the "true" treatment radius.

However, the boundary between the treatment and control groups is unlikely to be sharp. Due to the complex nature of urban traffic, and other boundaries (e.g., railroad tracks) there is likely to be an area where treated and untreated households are mixed. This potential mixed area is expected to cause an imprecise estimate of the real treatment effect. To partially solve this problem, we included a "buffer area" where households located in this area are dropped (i.e., the "donut" model). Doing so allowed us to have a cleaner cut between the treatment group and control group. In the appendix, we also provided estimates without the buffer area. The difference between the two approaches is minimal. Another issue is the existence of intersections of treatment/control groups for different CDP sites. Sometimes a household within the distance of a CDP site's treatment/control group may be also within the distance of another CDP site's treatment/control group. In this case, we assign the household to the treatment/control group of the CDP site that the housheold is closer to. As a result, a household will only appear in the treatment/control group of one CDP site.

The baseline specification is illustrated in Figure 2.2. There were many households located around a CDP. In the baseline specification, households located within 2 km of a potential CDP were assigned to a treatment group centered on this CDP. Those households were considered treated after the CDP site they centered around opened. Households located within 2.4 km to 4 km to a CDP site were assigned to a control group. This study dropped households located within the 2km to 2.4 km "buffer area" to get a more definite distinction between treatment groups and control groups. This practice was then applied to all 29 CDP sites that opened from 2012 to 2015 to clients. In subsequent models, we included different specifications with different treatment/control boundaries. When the treatment group boundary was changed, the buffer and control group boundaries were also adjusted proportionally.

2.3 Methodology

After assigning treatment and control groups, we implemented the difference in difference model to estimate the causal impact of opening new CDPs. This method assumes that since the treatment groups and control groups were spatially close, they should be similar enough to be comparable, and should have similar trends before treatment. Table 2.1 provides summary statistics for the treatment and control group, based on the baseline model.[2] For our baseline model with 2 km radius of the treatment group boundary, the number of observation is 64,656 in the treatment groups and 14,832 in the control groups. Compared to those in the control groups, the households in treatment groups visit more frequently and have a smaller household size. They are more likely to have African American household members and less likely to have children or babies in their household. They are also less likely to visit CCS by cars or receive some kinds of health care assistance such as Parkland Plus, a local health assistance program in Dallas County. While treatment group and control group that are similar enhance the comparability between them, statistics in Table 2.1 clearly shows that they are not the same.

[2] Sample characteristics for other analysis samples are similar and available from the authors upon request.

With a t-test, we are not able to reject the null hypothesis that characteristics of the treatment group and control group are the same.

One concern for the DID model is the extent to which CDP locations were exogenously determined. CDP's are often organized and opened because there exists local philanthropic community that is interested in providing food (rather than organized by clients themselves), the location of new CDP's is determined by the location of community organizations rather than the location of motivated clients. Figure 2.3 shows how the correlation between households' distance to a new CDP and visiting frequency changed when a new neighborhood CDP opened. Interestingly, closer proximity to the CDP site was associated with increased visiting frequency only after the new CDP opened; prior to the CDP opening, the opposite relationship was observed. Before a CDP opened, the correlation between households' visiting probability and their distance to that potential CDP is small and insignificant. This is reasonable because prior to opening, a CDP should not have any impact on households' visiting behavior. After the CDP opened, households' visiting probability had a significant negative correlation with their distance to the CDP. This suggests that CDP location was likely not opened because households around it are having increasing needs, and thus the opening of new CDPs are likely exogenous and unlikely to be correlated with the visiting frequency of nearby households.

Figure 2.4 shows the visiting probability trend of treatment groups and control groups before and after the opening of CDP sites using the baseline specification. New CDP's begin the process of enrolling clients up to 1 month prior to opening; this period is indicated by the shaded band in the figure and may be viewed as a sort of "partial" treatment. Before the enrollment period begins, treatment groups and control groups followed similar trends. This provides some evidence that if the treatment did not happen, both groups would still be comparable and would continue to follow the same trend after the potential treatment time. After treatment, the previously common trend began to differ, and households in treatment groups visited more frequently than those in control groups. If the comparability assumption holds, this change can only be attributed to the opening of new CDPs. Thus, by comparing the difference in visiting frequency of these two groups before and after the enrollment of the CDP, we can estimate the treatment effect of the opening of new CDPs.

Specifically, we estimated the following equation as both a linear probability model and a logistic model for different treatment and control sizes:

$$(1) \quad y_{ist} = \alpha + \beta_1 treated_{st} + \beta_2 TreatmentGroup_{is} + \beta_3 after_{st} + X_{ist}\gamma + site_s + month_t + site_s t + \varepsilon_{ist}$$

where y_{ist} is an indicator that assumes the value 1 if household i from the treatment group or the control group around CDP site s utilized CCS nutrition assistance at month t. $after_{st}$ is an indicator that assumes the value 1 if at month t, site s has been opened. $TreatmentGroup_{is}$ is an indicator that assumes the value 1 if household i was assigned to the treatment group around CDP site s. $treated_{ist}$ is the interaction of $TreatmentGroup_{is}$ and $after_{st}$. We included month fixed effect $month_t$in order to control for the variation in households' overall change in visiting probability over time. A CDP site fixed effect, $site_s$, is included to control for site level variation of visiting probability. Finally, we included a vector of control variables X_{ist} which contains each of the variables listed in Table 2.1. All standard errors were clustered at site level

s. If the treatment group size was correctly defined and the comparability assumption of DID holds, the β_1 coefficient should capture the effect of opening a new CDP site on visiting frequency.

2.4 Results

Table 2.2 shows the estimated average marginal effect from a logistic regression model applied to equation (1). Each column represents the estimate with different treatment group boundary. As one can see in the table, a household containing pregnant women, babies, or household members in minority group is positively correlated to their probability of visiting CCS. Receiving health assistance is negatively correlated with households' probability of visiting CCS. The sign of the estimated average marginal effect is consistent over different radius of treatment group boundary, with the exception of visiting by cars. The estimated average marginal effect of the *Treated* variable shows the treatment effect of opening new CDP sites. In column (1), the estimated treatment effect is small and insignificant when the treatment group boundary radius is defined as 0.5 km. Column (2) shows the estimated treatment effect with a treatment group boundary radius of 1 km, our baseline specification; the estimated treatment effect is larger and more significant. This suggests that as we increase the radius, we are getting better separation of the treated households and the untreated households. The estimate shows that when a CDP site is enrolled, it caused households within 1 km of it to increase their visiting probability by 1.7 percentage points compared to households within 2 km but beyond 1.2 km from it. Column (3) shows that when the treatment group boundary radius increases to 1.5 km, the estimated treatment effect of the CDPs' enrollment is 3.1 percentage points. Column (4) shows that when we increase the treatment group boundary radius to 2 km, we get the highest estimated treatment effect. After the CDPs are enrolled, it caused the households in the treatment group to increase monthly visiting probability by 4.71 percentage points compared to households in the control group. Column (5) – Column (7) shows that, beyond the 2 km radius, the estimated treatment effect begins to decrease and eventually becomes insignificant. Figure 2.5 displays the estimated treatment effects and associated 95% confidence intervals. The estimated treatment reaches the peak at 2 km and decreases as the treatment group boundary radius increases further. Results indicate that the "true" radius of the treatment boundary is likely to be 2 km.

Table 2.3 and Figure 2.6 shows the results using the same specification as employed previously but using an infrequent visitor sample. The infrequent visitor sample only includes households that visit less than four times a year, which is consistent with the infrequent visitor sample in Si and Leonard (2018). The result shows that when the treatment group radius is 0.5 km, opening a new CDP results in a 2.56 percentage points increase in visiting frequency ($p<0.05$). This estimate is higher than the treatment effect estimated for the full sample. However, other than the 0.5 km radius, the pattern of estimated treatment effects is similar to the results from the full sample. The peak of the estimated treatment effect appears at the 2.5 km treatment group radius, suggesting that the "true" treatment size for the infrequent visitor sample is 2.5 km. However, one can see that for both the full sample and infrequent sample, the difference between the 2 km result and 2.5 km result are minimal.

To verify the robustness of our result, we also include results from other specifications. Table 2.4 shows estimation results based on a linear probability model. Results are consistent with findings from our standard specification in Table 2.2. Table 2.5 and Figure 2.7 shows result from the specification with logistic regression but without the buffer area. This means that for each CDP, the treatment groups are immediately adjacent to the control groups (i.e. no “donut” buffer area). The results from this specification are consistent with our standard specification.

Finally, Table 2.6 and Figure 2.8 show the results of a placebo test. In this test, we randomly assigned 29 CDP sites on the map and set a random enrollment date for them. If our results from the standard specification are valid, we should not observe similar results in the placebo test. The estimated treatment effect with a 0.5 km treatment radius in the placebo test have a significance level of 0.05, but it is negative. For other treatment radius, the estimated treatment effect is small and insignificant.

2.5 Discussion

In this paper, we identified the causal effect of the opening of a new CDP site on the visiting probability of nearby households. In doing so, we also identified the average impact radius of new food distribution sites. We found that when a new CDP site opened, the average impact radius was approximately 2 km. The enrolling of new CDP sites caused households within 2 km of the site to increase their visiting probability by 4.4 percentage points, compared to households farther away.

This result is consistent across the full sample and the infrequent visitor sample. Although infrequent visitors were found not to respond to a negative income shock by increased visiting to CCS (Si and Leonard, 2018), they do respond to the opening of new CDP sites. The fact that these low-income households did not respond to the increased need for assistance (e.g., income shock) but did respond to decreased utilization cost of aid (e.g., closer proximity to the food distribution site and/or reduction in psychosocial barriers provided by the enrollment “precommitment”) provides evidence that these costs likely constrained their capability to utilize private nutrition assistance when facing an income shock. This result has several implications.

First, the accessibility of assistance programs is essential. Assistance programs that are available but not sufficiently accessible for households constrained by transportation provide more benefit to low-income households that are comparably better off. Households that are most in need, on the other hand, receive fewer benefits. Second, availability of transportation is essential for households’ ability to utilize assistance. If some households are so constrained by transportation that they cannot sufficiently utilize CCS’s assistance, it is reasonable to believe they would also have difficulty in accessing affordable grocery, healthcare, and potential job opportunities. Thus, providing transportation to them may be very helpful. Enhancing public transit is one potential solution. In addition, new innovations in transportation such as peer ride sharing programs for mental health patients and mechanisms to donate uber trips are being explored and offer the potential to increase access to places that were not previously possible via public transport or walking. This paper also provides evidence to suggest that the CDPs’ impact on households starts to decrease at distances beyond 2km. We cannot suggest the optimal allocation of distribution sites without a cost/benefit analysis, but our results can serve as a basis for future work.

Second, the client enrollment process created as part of the CDP model may play an important roll in increasing utilization of food assistance. In qualitative work, CCS clients stated that they chose to visit CCS only when all other forms of assistance were depleted. They reported a general concern that private food assistance should be reserved only for households who were most in need, and so lack of urgent, severe need caused them to visit less frequently

(Swales et al., 2017). However, clients who visit a CDP site may have fewer psychosocial barriers. Because CDPs require clients to pre-enroll, each month food is specifically allocated to a particular household, and these households have made a commitment to pick-up the food.

Increasing households utilization of food assistance services is important for increasing the benefits of food assistance. While admirable, the phenomenon that many low-income households believe that food assistance should be accessed only as a last effort to provide food for the household is inefficient. This mentality produces suboptimal impacts on numerous household outcomes as it results in frequent cycling between states of extremely scarce resources (Gooptu et al., 2014; Kaestner et al., 2017; Gross & Notowidigdo, 2011; Bertrand et al., 2004; Bertrand et al., 2006).

One concern of our work is the external validity of our results. Our data is limited to the Dallas area; more work needs to be done to determine whether our findings apply to other metropolitan areas. Metropolitan areas in the United States are very different in structure, distribution of communities and availability of public transportations. For a metro area that has a similar distribution of communities, level of friendliness to pedestrian and availability of public transport, our result may be applicable. For metropolitan areas that are vastly different, the external validity of our findings should be examined with caution.

2.6 Conclusion

Expansion of distribution networks of private nutrition assistance via new sites located within the community and pre-enrollment processes can help low-income households to better access that aid despite the inconvenience of very limited hours during with the service can be accessed. This appears to be especially true for those who previously had difficulties accessing that assistance. We estimated that when new distribution sites were opened, it increased low-income households' probability of utilizing aid by 4.4 percentage points. The estimated impact radius of new distribution sites was around 2 km.

Our results are particularly important in light of the continuing high food insecurity rate in the United States. Although the U.S. government spends enormous amounts of money on several assistance programs, there are still 15.6 million U.S households that are food insecure. Alleviating transportation and psychosocial barriers to accessibility should be an element in designing and operating assistance programs to low-income households, and the CDP model employed by CCS has achieved promising results.

Appendix B

Tables and Figures for Chapter 2

Figure 2.1. CDP Sites Enrolled from 2012 to 2015 and CCS clients

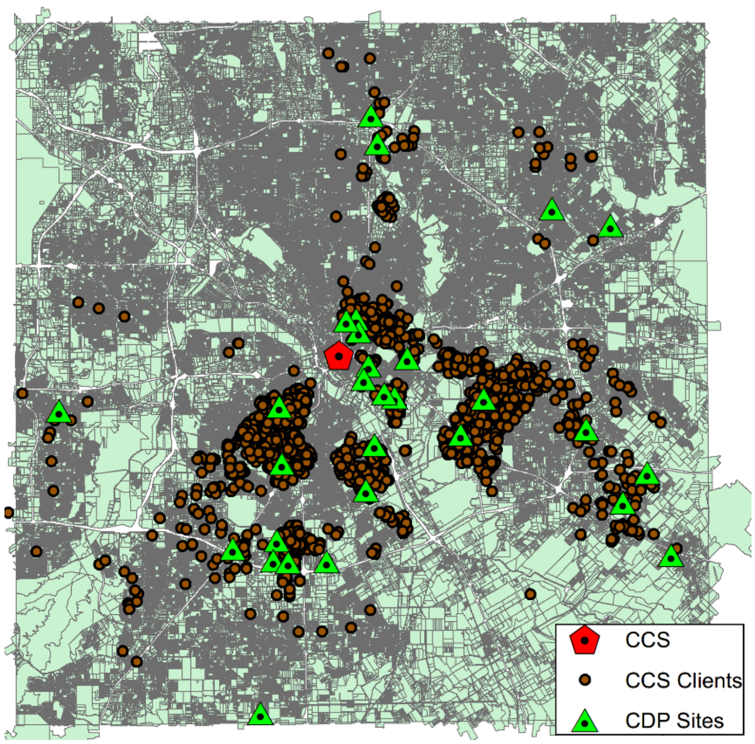
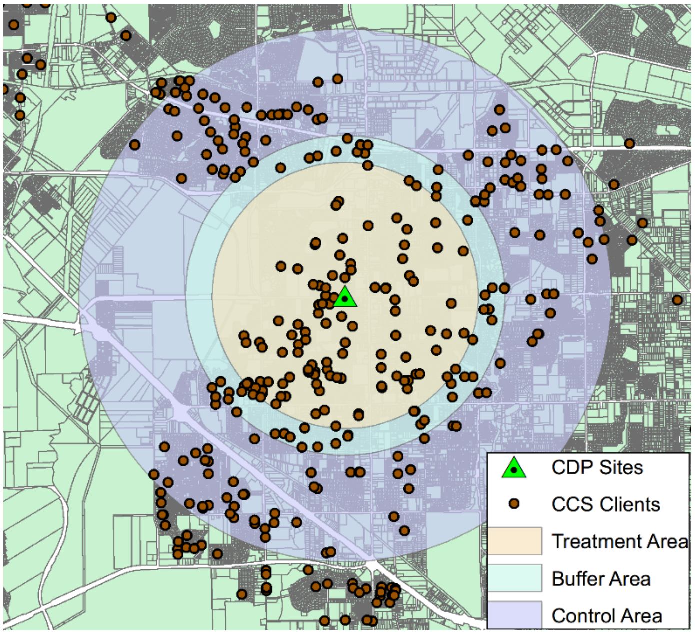


Figure 2.2. Example of a Treatment and a Control Group, for Baseline Specification



* In the baseline specification the treatment radius is 2 km and the control group are households located between 2.4km and 4 km from the new CDP. There is a 2 km – 2.4 km buffer area

Figure 2.3. Travel Distance and Visiting Probability, before and After Treatment

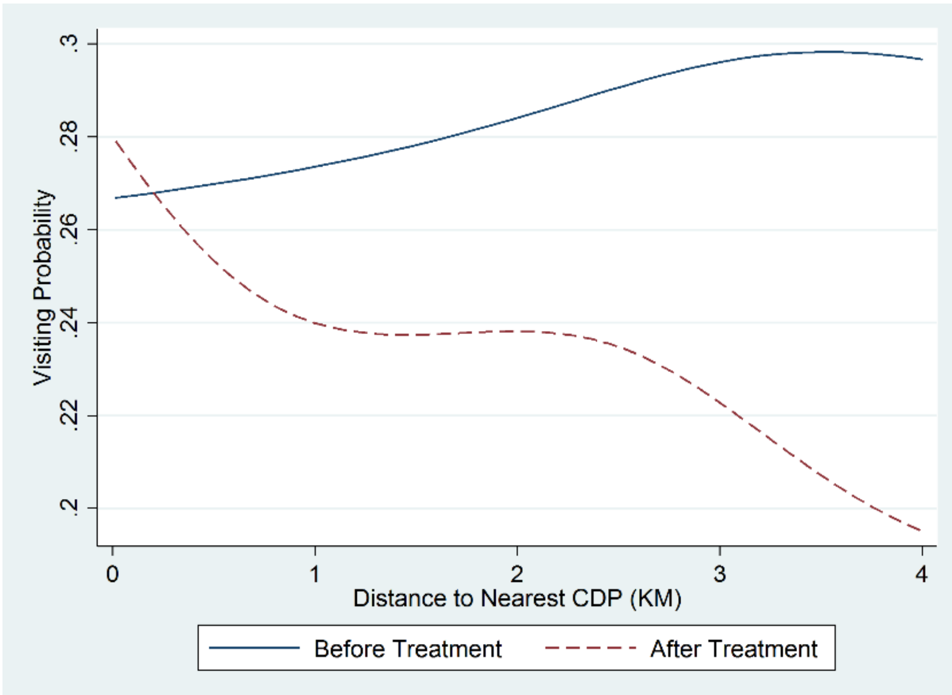
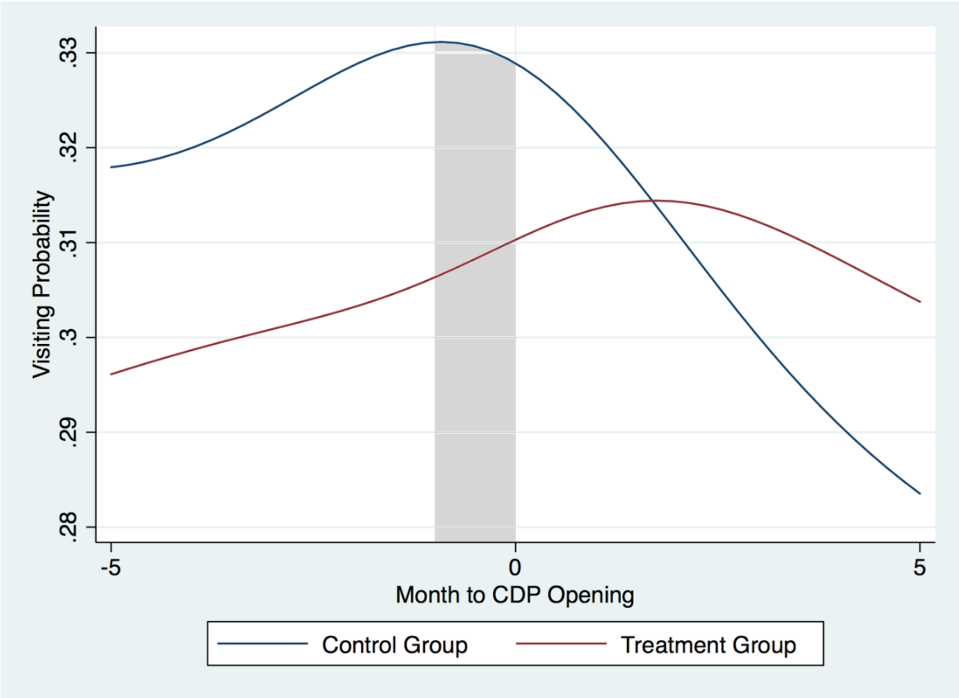


Figure 2.4. Month to Treatment and Visiting Probability, By Treatment and Control Group with Baseline Specification*



*In the baseline specification the treatment radius is 2 km and the control group are households located between 2.4km and 4 km from the new CDP. There is a 2 km – 2.4 km buffer area.

Figure 2.5. Average Marginal Effect of a new CDP opening on Visiting Frequency Estimated for Different Sized Treatment Boundaries, with Buffer

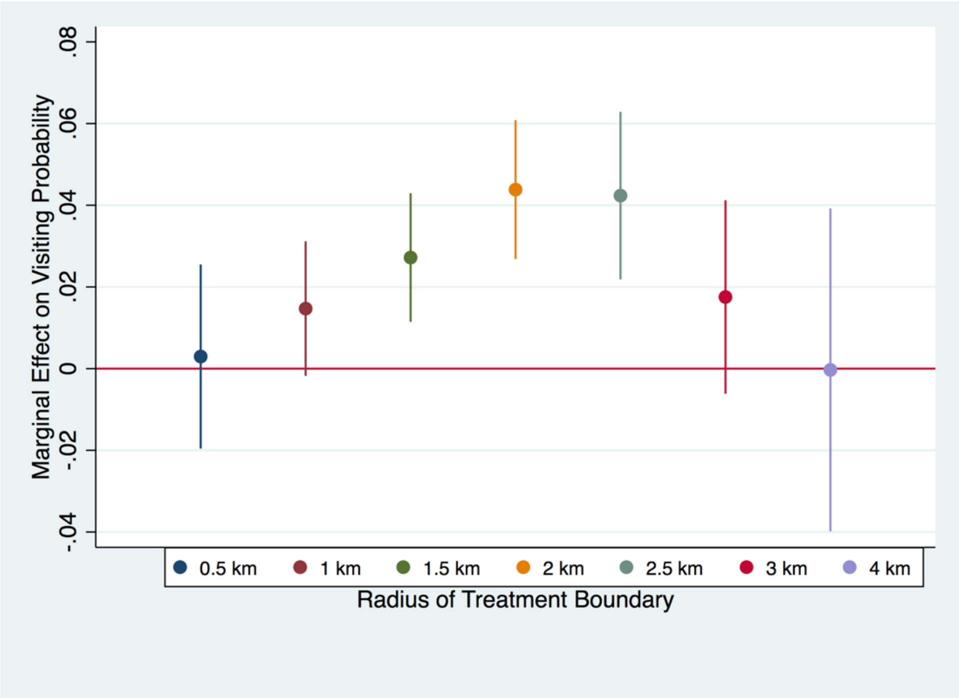


Figure 2.6. Average Marginal Effect of a new CDP opening on Visiting Frequency Estimated for Different Sized Treatment Boundaries, with Buffer and Infrequent Visitors

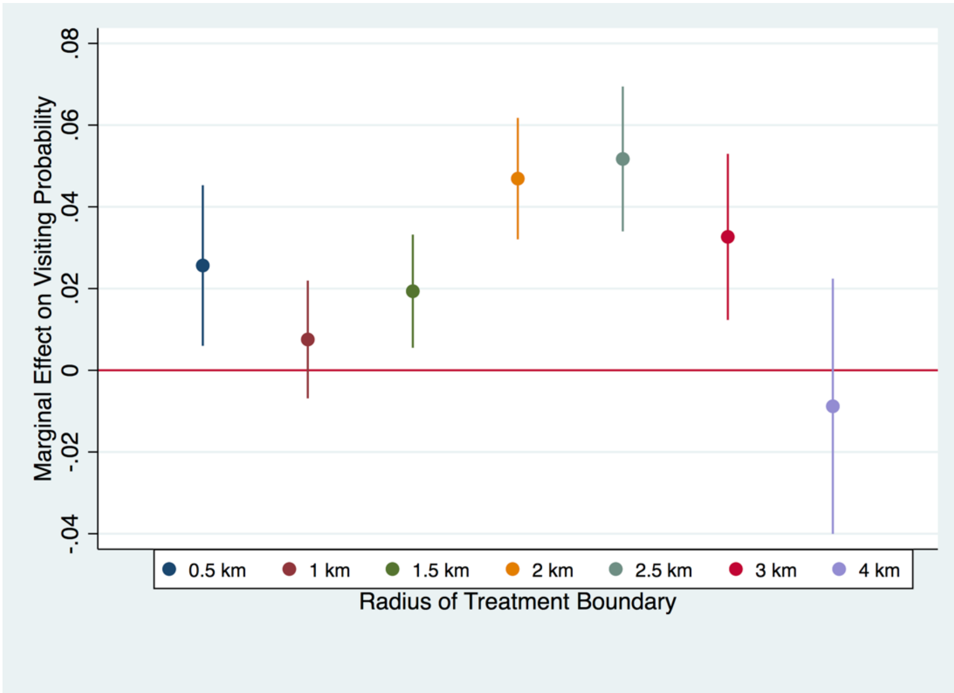


Figure 2.7. Average Marginal Effect of Treatment from Different Treatment Size, without Buffer

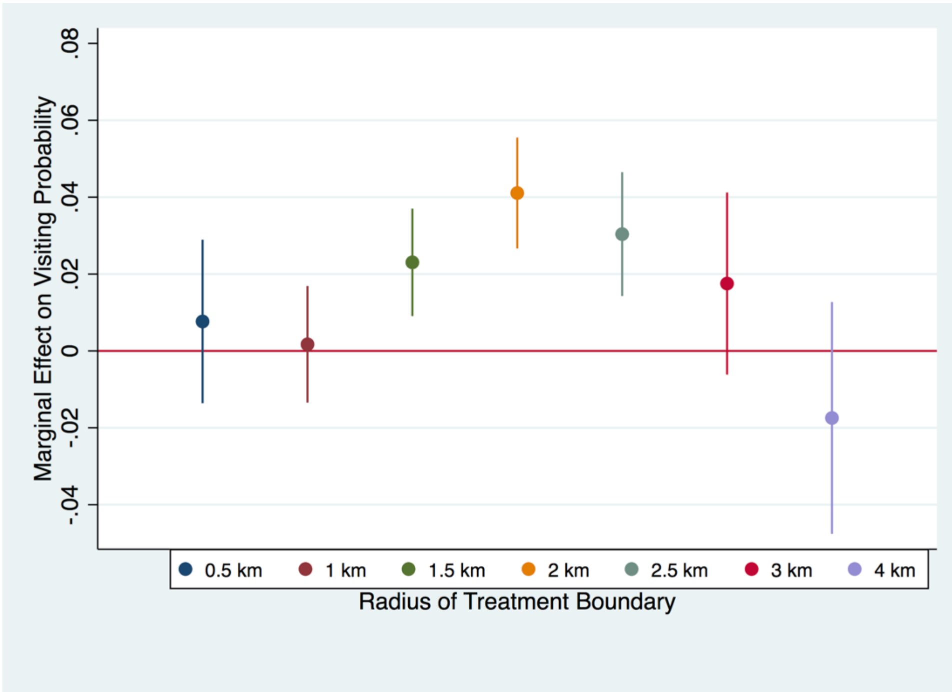


Figure 2.8. Average Marginal Effect of Treatment from Different Treatment Size, Placebo Test

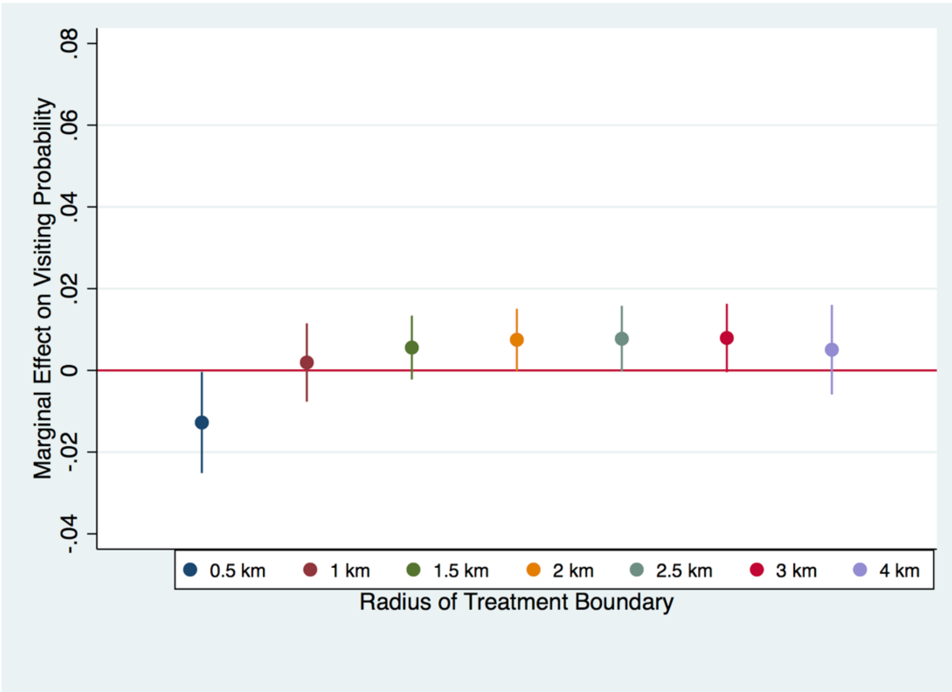


Table 2.1. Summary Statistics for Baseline Specification*

	Treatment Group			Control Group		
VARIABLES	N	mean	sd	N	mean	sd
Dependent Variable						
Visiting Proability	64,656	0.263	0.440	14,832	0.250	0.433
Control Variables						
Household Size	64,656	3.042	2.100	14,832	3.859	2.192
Pregnancy	64,656	0.0111	0.105	14,832	0.0110	0.104
Have Babies	64,656	0.0277	0.164	14,832	0.0374	0.190
Have Children	64,656	0.198	0.399	14,832	0.262	0.440
Visit by Cars	64,656	0.787	0.410	14,832	0.896	0.306
African American	64,656	0.394	0.489	14,832	0.313	0.464
Hispanic	64,656	0.457	0.498	14,832	0.629	0.483
Health Assistance	64,656	0.191	0.393	14,832	0.235	0.424

*In the baseline specification the treatment radius is 2 km and the control group are households located between 2.4km and 4 km from the new CDP.

Table 2.2. Logistic Model Estimates of Average Marginal Effect on Visiting Frequency, with Buffer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radius of Treatment Boundary	0.5 km	1 km	1.5 km	2 km	2.5 km	3 km	4 km
Treated	0.00296	0.0147*	0.0272***	0.0438***	0.0424***	0.0175	-0.000306
	(0.0115)	(0.00841)	(0.00803)	(0.00867)	(0.0105)	(0.0121)	(0.0202)
Treatment Group	0.00272	-0.0170**	-0.0253***	-0.0325***	-0.00358	0.00526	0.0479***
	(0.00953)	(0.00665)	(0.00634)	(0.00685)	(0.00834)	(0.0101)	(0.0173)
After	-0.00843	-0.0320***	-0.0490***	-0.0653***	-0.0651***	-0.0471***	-0.0309
	(0.0116)	(0.00902)	(0.00870)	(0.00928)	(0.0108)	(0.0124)	(0.0205)
Pregnancy	0.184***	0.129***	0.102***	0.108***	0.0855***	0.0890***	0.0974***
	(0.0223)	(0.0160)	(0.0145)	(0.0134)	(0.0131)	(0.0127)	(0.0128)
Have Baby	0.0886***	0.0718***	0.0652***	0.0568***	0.0522***	0.0566***	0.0547***
	(0.0163)	(0.0115)	(0.00980)	(0.00917)	(0.00893)	(0.00861)	(0.00867)
Have Child	-0.0292***	-0.0245***	-0.0189***	-0.0164***	-0.0113***	-0.0192***	-0.0174***
	(0.00722)	(0.00535)	(0.00463)	(0.00438)	(0.00416)	(0.00409)	(0.00408)
Have Car	-0.0184***	-0.00668	0.00627	0.000337	-0.00200	-0.000165	0.00413
	(0.00604)	(0.00495)	(0.00442)	(0.00432)	(0.00401)	(0.00398)	(0.00400)
African American	0.0763***	0.0446***	0.0235***	0.0316***	0.0361***	0.0278***	0.0287***
	(0.00897)	(0.00703)	(0.00609)	(0.00600)	(0.00556)	(0.00547)	(0.00551)
Hispanic	0.0837***	0.0387***	0.0216***	0.0270***	0.0264***	0.0178***	0.0180***
	(0.00993)	(0.00743)	(0.00633)	(0.00617)	(0.00577)	(0.00566)	(0.00568)
Health Assistance	-0.0213***	-0.0431***	-0.0442***	-0.0421***	-0.0439***	-0.0476***	-0.0476***
	(0.00630)	(0.00485)	(0.00429)	(0.00406)	(0.00384)	(0.00381)	(0.00381)
Observations	32,220	55,980	73,080	79,488	85,716	89,496	90,648
Treatment Group Observations	16,380	34,884	53,316	64,656	74,124	80,208	87,516

Robust Standard errors clustered at CDP sites level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.3. Logistic Model Estimates of Average Marginal Effect on Visiting Frequency, with Buffer and Only Infrequent Visitor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radius of Treatment Boundary	0.5 km	1 km	1.5 km	2 km	2.5 km	3 km	4 km
Treated	0.0256**	0.00754	0.0194***	0.0469***	0.0517***	0.0326***	-0.00881
	(0.0100)	(0.00736)	(0.00707)	(0.00758)	(0.00904)	(0.0104)	(0.0159)
Treatment Group	-0.00394	0.00138	-0.00514	-0.0216***	-0.00945	0.00107	0.0327***
	(0.00764)	(0.00549)	(0.00507)	(0.00533)	(0.00644)	(0.00787)	(0.0124)
After	-0.0298***	-0.0287***	-0.0446***	-0.0674***	-0.0720***	-0.0606***	-0.0213
	(0.0104)	(0.00798)	(0.00778)	(0.00817)	(0.00930)	(0.0105)	(0.0162)
Pregnancy	0.0472**	0.0302**	0.0252*	0.0216*	0.0215*	0.0145	0.0187*
	(0.0208)	(0.0142)	(0.0129)	(0.0118)	(0.0114)	(0.0112)	(0.0113)
Have Baby	0.0493***	0.0468***	0.0422***	0.0335***	0.0355***	0.0363***	0.0337***
	(0.0133)	(0.00926)	(0.00793)	(0.00736)	(0.00708)	(0.00685)	(0.00688)
Have Child	-0.0118*	-0.00518	-0.0135***	-0.00262	-0.00102	-0.00595*	-0.00397
	(0.00636)	(0.00467)	(0.00405)	(0.00378)	(0.00354)	(0.00351)	(0.00350)
Have Car	-0.0315***	-0.0257***	-0.0218***	-0.0188***	-0.0191***	-0.0198***	-0.0196***
	(0.00522)	(0.00423)	(0.00374)	(0.00363)	(0.00339)	(0.00335)	(0.00338)
African American	0.0210***	0.00472	-0.00543	-0.00648	-0.00568	-0.00905*	-0.00951**
	(0.00735)	(0.00596)	(0.00524)	(0.00508)	(0.00472)	(0.00464)	(0.00469)
Hispanic	0.0364***	0.0161***	0.0135**	0.00606	0.0103**	0.00754	0.00435
	(0.00810)	(0.00626)	(0.00542)	(0.00521)	(0.00488)	(0.00477)	(0.00481)
Health Assistance	-0.000603	-0.0146***	-0.0129***	-0.0123***	-0.0142***	-0.0160***	-0.0149***
	(0.00540)	(0.00419)	(0.00367)	(0.00345)	(0.00325)	(0.00322)	(0.00322)
Observations	25,020	43,128	55,908	61,092	66,600	69,012	69,768
Treatment Group Observations	16,380	34,884	53,316	64,656	74,124	80,208	87,516

Robust Standard errors clustered at CDP sites level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4. Linear Probability Model Estimates on Visiting Frequency, with Buffer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radius of Treatment Boundary	0.5 km	1 km	1.5 km	2 km	2.5 km	3 km	4 km
Treated	0.00439	0.0177**	0.0310***	0.0471***	0.0360***	0.0139	0.000769
	(0.0119)	(0.00879)	(0.00845)	(0.00909)	(0.00985)	(0.0108)	(0.0156)
Treatment Group	0.00199	-0.0196***	-0.0285***	-0.0364***	-0.00320	0.00560	0.0398***
	(0.0101)	(0.00719)	(0.00697)	(0.00763)	(0.00858)	(0.00940)	(0.0132)
After	-0.00769	-0.0338***	-0.0523***	-0.0693***	-0.0596***	-0.0441***	-0.0336**
	(0.0119)	(0.00942)	(0.00917)	(0.00967)	(0.00999)	(0.0109)	(0.0160)
Pregnancy	0.221***	0.149***	0.115***	0.124***	0.0987***	0.101***	0.111***
	(0.0299)	(0.0202)	(0.0177)	(0.0166)	(0.0163)	(0.0156)	(0.0159)
Have Baby	0.0979***	0.0767***	0.0696***	0.0606***	0.0568***	0.0608***	0.0587***
	(0.0193)	(0.0132)	(0.0111)	(0.0103)	(0.0103)	(0.00981)	(0.00981)
Have Child	-0.0295***	-0.0246***	-0.0189***	-0.0164***	-0.0113***	-0.0190***	-0.0173***
	(0.00718)	(0.00520)	(0.00452)	(0.00428)	(0.00407)	(0.00397)	(0.00396)
Have Car	-0.0175***	-0.00680	0.00573	-2.97e-05	-0.00189	-0.000156	0.00368
	(0.00576)	(0.00477)	(0.00430)	(0.00417)	(0.00391)	(0.00388)	(0.00390)
African American	0.0623***	0.0399***	0.0216***	0.0292***	0.0335***	0.0261***	0.0270***
	(0.00698)	(0.00611)	(0.00558)	(0.00546)	(0.00506)	(0.00501)	(0.00512)
Hispanic	0.0697***	0.0342***	0.0196***	0.0246***	0.0240***	0.0162***	0.0162***
	(0.00816)	(0.00665)	(0.00591)	(0.00571)	(0.00533)	(0.00526)	(0.00535)
Health Assistance	-0.0214***	-0.0414***	-0.0419***	-0.0400***	-0.0413***	-0.0447***	-0.0448***
	(0.00604)	(0.00449)	(0.00395)	(0.00374)	(0.00350)	(0.00346)	(0.00347)
Constant	0.181***	0.175***	0.199***	0.201***	0.186***	0.183***	0.143***
	(0.0226)	(0.0179)	(0.0164)	(0.0161)	(0.0157)	(0.0161)	(0.0188)
Observations	32,220	55,980	73,080	79,488	85,716	89,496	90,648
Treatment Group Observations	16,380	34,884	53,316	64,656	74,124	80,208	87,516

Robust Standard errors clustered at CDP sites level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5. Logistic Model Estimates of Average Marginal Effect on Visiting Frequency, without Buffer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radius of Treatment Boundary	0.5 km	1 km	1.5 km	2 km	2.5 km	3 km	4 km
Treated	0.00767	0.00172	0.0230***	0.0411***	0.0304***	0.0175	-0.0174
	(0.0109)	(0.00774)	(0.00713)	(0.00737)	(0.00822)	(0.0121)	(0.0154)
Treatment Group	-0.00305	-0.0128**	-0.0242***	-0.0343***	-0.0184***	0.00526	0.0624***
	(0.00902)	(0.00617)	(0.00559)	(0.00581)	(0.00652)	(0.0101)	(0.0128)
After	-0.0189*	-0.0280***	-0.0442***	-0.0597***	-0.0542***	-0.0471***	-0.0143
	(0.0107)	(0.00815)	(0.00772)	(0.00795)	(0.00868)	(0.0124)	(0.0156)
Pregnancy	0.180***	0.108***	0.0931***	0.0980***	0.0851***	0.0890***	0.0864***
	(0.0213)	(0.0149)	(0.0135)	(0.0130)	(0.0125)	(0.0127)	(0.0125)
Have Baby	0.102***	0.0643***	0.0584***	0.0541***	0.0526***	0.0566***	0.0523***
	(0.0153)	(0.0104)	(0.00923)	(0.00882)	(0.00847)	(0.00861)	(0.00844)
Have Child	-0.0284***	-0.0182***	-0.0188***	-0.0142***	-0.0152***	-0.0192***	-0.0147***
	(0.00691)	(0.00502)	(0.00442)	(0.00417)	(0.00400)	(0.00409)	(0.00397)
Have Car	-0.0211***	-0.00430	0.00332	0.00172	0.000680	-0.000165	0.00179
	(0.00581)	(0.00467)	(0.00426)	(0.00408)	(0.00395)	(0.00398)	(0.00392)
African American	0.0606***	0.0396***	0.0331***	0.0309***	0.0327***	0.0278***	0.0288***
	(0.00829)	(0.00649)	(0.00592)	(0.00567)	(0.00545)	(0.00547)	(0.00539)
Hispanic	0.0683***	0.0393***	0.0295***	0.0209***	0.0238***	0.0178***	0.0188***
	(0.00919)	(0.00679)	(0.00611)	(0.00584)	(0.00561)	(0.00566)	(0.00555)
Health Assistance	-0.0229***	-0.0354***	-0.0499***	-0.0487***	-0.0462***	-0.0476***	-0.0457***
	(0.00599)	(0.00456)	(0.00409)	(0.00388)	(0.00372)	(0.00381)	(0.00371)
Observations	34,884	64,656	80,208	87,516	91,800	92,112	93,312
Treatment Group Observations	16,380	34,884	53,316	64,656	74,124	80,208	87,516

Robust Standard errors clustered at CDP sites level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6. Logistic Model Estimates of Average Marginal Effect on Visiting Frequency, Placebo Test

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Radius of Treatment Boundary	0.5 km	1 km	1.5 km	2 km	2.5 km	3 km	4 km
Treated	-0.0128**	0.00193	0.00558	0.00750*	0.00774*	0.00793*	0.00507
	(0.00631)	(0.00489)	(0.00399)	(0.00387)	(0.00410)	(0.00427)	(0.00559)
Treatment Group	0.00619	-0.0142***	0.0209***	0.0240***	0.0082***	0.0115***	0.0125***
	(0.00496)	(0.00382)	(0.00299)	(0.00287)	(0.00304)	(0.00314)	(0.00405)
After	0.000667	-0.00764**	-0.0109***	-0.0139***	-0.0146***	-0.0157***	-0.0138***
	(0.00444)	(0.00384)	(0.00313)	(0.00319)	(0.00358)	(0.00386)	(0.00532)
Pregnancy	0.0369*	0.0321**	0.00614	0.00945	0.0247***	0.0223***	0.0202**
	(0.0213)	(0.0132)	(0.0106)	(0.00947)	(0.00889)	(0.00856)	(0.00831)
Have Baby	0.0637***	0.0272***	0.0358***	0.0416***	0.0404***	0.0394***	0.0332***
	(0.0140)	(0.00924)	(0.00712)	(0.00642)	(0.00618)	(0.00580)	(0.00569)
Have Child	-0.0353***	-0.0325***	-0.0295***	-0.0251***	-0.0265***	-0.0240***	-0.0237***
	(0.00593)	(0.00409)	(0.00319)	(0.00296)	(0.00284)	(0.00266)	(0.00260)
Have Car	0.0451***	0.0369***	0.0303***	0.0455***	0.0445***	0.0394***	0.0518***
	(0.00383)	(0.00289)	(0.00235)	(0.00219)	(0.00213)	(0.00201)	(0.00198)
African American	0.0460***	0.0428***	0.0415***	0.0462***	0.0416***	0.0518***	0.0532***
	(0.00612)	(0.00430)	(0.00341)	(0.00306)	(0.00296)	(0.00287)	(0.00276)
Hispanic	0.0371***	0.0210***	0.0220***	0.0123***	0.000864	0.0114***	0.0079***
	(0.00679)	(0.00472)	(0.00369)	(0.00336)	(0.00323)	(0.00309)	(0.00301)
Health Assistance	-0.0144***	-0.0239***	-0.0251***	-0.0238***	-0.0274***	-0.0333***	-0.0329***
	(0.00467)	(0.00335)	(0.00270)	(0.00257)	(0.00248)	(0.00235)	(0.00232)
Observations	69,084	125,928	190,260	221,076	237,528	259,128	273,924

Robust Standard errors clustered at CDP sites level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

CHAPTER 3

THE FOOD SECURITY CONSEQUENCE OF MANDATED EMPLOYMENT ELIGIBILITY VERIFICATION

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3.1 Introduction

Authorized by the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (IIRIRA), E-Verify is an online system administered by U.S. Department of Homeland Security that allows employers to determine the work-eligibility of their workers. When an employer submits information taken from a new employee's I-9 form, the system compares that information against 455 million records in the Social Security Administration (SSA) database and 80 million records in the Department of Homeland Security's (DHS) immigration databases (NCLS). If an employee's information matches a record contained in one of the databases, that employee is then considered eligible to work in the United States. If there is a mismatch, E-Verify alerts the employer, and the employee must contact the government agencies to solve the mismatch within eight federal government working days. During this eight-day period, the employee may still be allowed to work. In theory, this system can detect any unauthorized workers attempting to work using fake documents.

The E-verify system has evolved both regarding the information contained in the system and regulations related to the use of the system. In August 2007, the E-verify system added facial image data to help prevent identity fraud. Between 2006 and 2016, 22 states passed laws or had executive orders that required employers to verify workers' employment eligibility by using the E-Verify system; hereafter, we refer to these laws as "E-Verify mandates." In many states, E-Verify mandates were introduced alongside legislation allowing for harsher punishment for employers who knowingly employ unauthorized workers. For example, in Arizona, violation of this rule may result in a 10-day temporary Arizona business license suspension upon the first offense followed by permanent Arizona business license suspension upon the second offense

(Arizona Fair and Legal Employment Act HB 2779). Other examples include immediate cancellation of government contracts, reversion of unspent funds and monetary penalties (Idaho Executive Order 2009-10) and disqualification of certain tax credits on state income tax (Indiana SB 590). As a result of the mandates, E-Verify is now widely used. In the fiscal year 2017 (October 2016 – September 2017), United States Citizenship and Immigration Services reported that E-Verify processed 34,853,666 cases, and among those cases, 329,620 returned "Not Found Work Authorized."

As many E-verify mandates laws have stated, E-verify mandates were intended to reduce further hiring of undocumented immigrants and thus protect authorized workers. However, previous literature has reported mixed results. Using the Current Population Survey (CPS) data, Catalina Amuedo-Dorantes and Cynthia Bansak (2012) reported that announcement of E-verify mandates significantly reduced the employment likelihood of likely unauthorized males and females, but its implementation had no effect on employment likelihood. The hourly wages of likely unauthorized women increased following the enactment of E-Verify mandates. Also, the mandate appeared to redistribute likely unauthorized workers towards industries that typically benefitted from special exclusion, such as food service and agriculture. Pia M. Orrenius and Madeline Zavodny (2015) reported that implementation of E-Verify mandates reduced the average real hourly wage among male likely unauthorized Mexican immigrants. They also indicated that implementation of the mandate appeared to increase likely unauthorized female Mexican immigrant's probability of staying in the labor force and of switching employers. The announcement of E-verify mandates (passing of laws, which is earlier than implementation) had no significant effect.

Although the goal of the E-Verify mandates was to limit employment of unauthorized workers, the extant literature suggests that there may have been more complicated unintended consequences. This study focused on the food security consequences of E-Verify mandates. Food security, especially since 2007, has become a significant issue among low-income households in the United States. According to USDA, food insecurity level before 2007 had been steadily around 11 percent. During the 2007 financial recession, food insecurity rates rose rapidly, and as of 2018 remain three percentage point higher than the pre-recession level.

The contribution of this study is two-fold. First, it is the first to address the unintended consequences of E-verify mandates on food security. Compared to monetary measures, food security directly represents the economic hardship and potential health effects on low-income households which are especially vulnerable to the unintended consequence of the policy change. Second, this study is the first to address the impact of E-verify mandates on families rather than individuals, and these household-level impacts are examined for both citizen and non-citizen households. Finally, this study revealed that the unintended consequences of this policy do not come through the reduction of income of the directly impacted population.

3.2 Hypothesis

E-verify mandates might impact food security in several ways. If implemented as intended, E-verify mandates should affect employment. The mandates may also affect the prices of goods and services which would indirectly affect food security. Finally, E-verify mandates might indirectly cause strain on the local community social safety net.

E-Verify mandates might decrease employment among unauthorized workers and increase employment among authorized workers. These joint effects would occur if E-Verify mandates caused employers to substitute authorized workers for unauthorized workers and will lead to heterogeneous impacts on food security for authorized and unauthorized worker populations. Among authorized workers, the income of previously underemployed or unemployed workers would increase, and their food security would also improve. On the other hand, the food security level of unauthorized workers would decrease, along with their income. However, the effect of E-verify on changes in employment among authorized and unauthorized workers depends on the extent to which E-Verify mandates were enforced, and the degree to which informal or "black market" labor markets developed to circumvent the effects of the E-verify mandates.

Second, E-Verify mandates might have increased the costs of producing, distributing, and selling food. This would have occurred if E-Verify mandates reduced the labor supply in the food/agriculture, transportation, and retail sectors. Previous work shows that these sectors employed a large number of unauthorized workers before the E-verify mandates (Edwards and Ortega, 2017; Pew, 2015). This effect might have been especially significant in some "food desert" communities, where local access to grocery stores is limited. Increased food costs would likely increase food insecurity rates for both authorized and unauthorized populations.

Finally, both employment shocks and food cost shocks potentially associated with the implementation of the E-Verify mandates might produce an additional negative indirect shock to the local social safety-net. Most social safety-net systems provide some form of food assistance to combat food insecurity. The level of food assistance may be insufficient if the number of food-insecure households increases, and/or the cost of food increases. Further, the organizational capacity of local safety-net systems may be compromised as portions of the community are forced out of the mainstream labor force. Evidence for this weakening of local community resources was documented in 2007 when restrictions on unauthorized workers increased in Arizona (Bohn, Lofstrom and Raphael, 2014). These factors might indirectly increase food insecurity for both authorized and unauthorized populations.

Given these mechanisms through which E-verify mandates might impact food security rates, we have several hypotheses regarding how E-verify might affect households. First, we hypothesize that E-verify mandates will decrease the income of unauthorized workers and decrease their households' food security level. Next, we hypothesize that E-verify mandates will increase the income of workers who compete with unauthorized workers. Finally, we hypothesize an overall increase of food insecurity following the implementation of E-verify mandates. This effect will be attenuated by the potential increase in employment among authorized workers, but even the group of workers for whom the policy is likely to benefit might be adversely affected by higher food costs and potential inadequacy of the social safety net to respond to increased needs among unauthorized workers following implementation of the E-verify mandate.

3.3 Data

This study combined data from the December Food Security Supplement of the Current Population Survey (CPS) from 2004 to 2016 with E-Verify implementation data from the National Conference of State Legislatures (NCSL). NCSL data showed that between 2006 and 2016, 22 states implemented laws or executive orders requiring certain employers to use E-Verify to verify the identity of new employees. Among these 22 states, there were 3 different types of implementation of E-verify mandates: 5 of the states required all employers to verify the identity of new employees (hereafter "all employer" mandates); 14 states only required public sector employers and some of their contractors to use E-Verify (hereafter “public sector only” mandates); 4 states initially imposed public sector only mandates and later switched to be all employer mandate states. Figure 3.1 and Table 3.11 list the detailed time and type of states which required the use of E-Verify. Different states implemented E-verify at different times.

The CPS is a monthly survey of U.S. households conducted by the United States Census Bureau. Beginning in 2001, CPS added the Food Security Supplement, which is a set of questions used to measure food security. The Food Security Supplement is asked during the December CPS survey administration. Food security is measured using an 18-item food security questionnaire developed by the US Department of Agriculture (USDA). These questions were designed to comprehensively evaluate if households surveyed had physical, social and economic access to sufficient safe and nutritious food that meets their dietary needs and food preferences for an active and healthy life. The answers to these questions were then converted to food security categories using standard USDA guidelines (Bickel, et al., 2017). Households are categorized as food secure, marginally food secure, food insecure without hunger, or food secure with hunger. Food secure or marginally food secure households reported little or no indications of food-access problems or limitations. Households that are food insecure without hunger reported reduced quality, variety, or desirability of diet. However, households in this category reported little or no indication of reduced food intake. Households that are food insecure with hunger reported multiple indications of disrupted eating patterns and reduced food intake. Households with children are also asked about the food security of children in the households, and responses were used to create separate food security measures for children and adults in a household. The food insecurity rate is calculated as the number of individuals categorized as being food insecure with or without hunger divided by the total number of individuals sampled. We also looked into different food security measures, which are calculated based on questions with different time periods, known as recall. Food security measures with a 12-months recall are based on questions asking interviewees about their food situation in the past 12 months. Food security measures with a 30-days recall are based on questions asking interviewees about their food situation in the past 30 days.

In our full sample, the number of observation is 541,528 recorded between 2004 and 2016. Among those observations, 427,363 records are in the citizen sample, and 114,165 are in the non-citizen sample. There are 156,652 records which have children and answered questions related to children's food security and thus are in our children's food security sample. Table 3.1 shows summary statistics. In our full sample, 21.8 percent are African American, and 26.5 percent are Hispanic. 9.3 percent of individuals have ever served in the military. 60.9 percent of individuals report that they have senior in their household and 85.4 percent of individuals report that they have children in their household. 39.1 percent of individuals have a college degree or better, 53.9 percent have education level equal to or beyond high school, but less than a college degree. The mean household income variable in our data is a categorical variable ranging from 0 to 16. Each category represents a range of family income. The median category is 11, which indicates a family income of $40,000 - $49,999. 37 percent individuals have family income less than $25,000, which covers the federal poverty line for an average household.

Table 3.2 shows the food security status before the implementation of E-Verify mandates. Based on individuals' answers to questions and USDA standard guidelines, 12.34 percent of individuals were food insecure in the 12 months before data collection. Among them, 7.7 percent were food insecure without hunger, and 4.63 percent were food insecure with hunger. This number decreased to 6.98 percent, 4.18 percent, and 2.8 percent, respectively when asking only about food security in the past 30 days. The food security status for adults and children followed different patterns. With a 12-month recall, the adult food insecurity rate was 11.55 percent while children food insecurity rate was 9 percent. However, children had a higher probability of "food insecure without hunger" status, and a much lower probability of "food insecure with hunger" status compare to adults. These numbers indicate a tendency to prioritize children's food needs within a household.

3.4 Methodology

The variation in E-verify implementation across states and years was used to estimate the impact of E-verify on food security rates, using an approach similar to Amuedo Dorantes and Bansak (2012). Specifically, we estimated:

(1) $y_{ist} = \alpha + \beta treated_{st} + X_{ist}\gamma + state_s + yeardummy_t + state_s t + \varepsilon_{ist}$

Where $y_{ist}$is the family income / food security category of household *i* in state *s* at year *t*, $treated_{st}$ is an indicator variable that assumes the value of 1 if state *s* at year t had implemented E-Verify mandates and assumes the value of 0 otherwise. $state_s$ is a series of state dummy variables, $yeardummy_t$ is a series of year dummy variables. We allow for state specific temporal trends in the outcome variable through the inclusion of $state_{st}$. X_{ist} is a vector of household level control variables, shown in Table 3.1. Specifically, we estimated:

(2) $y_{ist} = \alpha + \beta_1 treated\_all_{st} + \beta_2 treated\_public_{st} + X_{ist}\gamma + state_s + yeardummy_t + state_s t + \varepsilon_{ist}$

For states that switched from public sector only mandates to all employer mandates, their status changed from $treated\_all_{st}$= 0 and $treated\_public_{st}$= 1 to $treated\_all_{st}$= 1 and $treated\_public_{st}$= 0.

To make sure that we had a precise definition of treatment, we dropped observations in the year in which a state implemented E-Verify. For example, Minnesota implemented E-Verify mandate in July 2011. We define individuals in Minnesota before 2011 as untreated and individuals in Minnesota after 2011 as treated. We dropped Minnesota observation in 2011 since 2011 observations mixed treated months and untreated months, which makes some outcome variables, like overall food security in the past 12 months, inconsistent with the same variable in other years.

In our identification strategy, we assume that after controlling for individual characteristics, unobserved variables that vary across both states and years and can potentially affect food security are not correlated with whether a state decides to implement an E-Verify mandate. This assumption also means that a state does not make decision on whether to implement E-Verify based on the future change in food security status in this state. Since E-Verify mandate laws are immigration policy which target unauthorized workers, we believe it is very unlikely that food security is among the considerations when making such decisions. If this assumption holds, then β in equation (1) would capture the overall treatment effect of E-Verify mandate. We estimated all specifications as Ordered Probit Models since the food security category is an ordered variable, and higher categories indicate higher food insecurity levels. To check the robustness of the specification, we also included the effect of a "Placebo Treatment", where we randomly assigned treatment. We found that the estimated treatment effect is small and insignificant in the placebo test across different specification.

3.5 Results

Table 3.3 presents results from equation (1) estimated as a Probit model with an indicator variable which assume the value of 1 when family income being less than $25,000 as the dependent variable. We used Probit model and turned family income category variable into a binary variable in this case since there are 16 categories on family income, making it hard to show the marginal effect on each category in an Ordered Probit Model. We choose 25,000 as the cutoff point because in our data, the income category of $20,000-$24,999 covers the average federal poverty line for average families. Column (1) shows results that pooled both types of E-Verify mandate as a single treatment variable, whereas other columns show the results from specifications that separate the two kinds of E-Verify mandates. Column (3), (4) and (5) present results from stratified samples based on citizenship: Citizen, Non-Citizen (all race/ethnicity) and Non-Citizen Hispanic, respectively. Throughout the different specifications, the estimated treatment effect on the probability of having less than $25,000 family income was small and insignificant. The E-Verify mandates had no significant impact on both U.S. citizens and Non-citizens' family income. Even estimates for the non-citizen Hispanic sample showed no significant effect. We also included an OLS version of the estimation in Table 3.12 with family income category as the dependent variable. The result is consistent with results in Table 3.3. The result is Table 3.4 shows the estimated average marginal effect from equation (1) and equation (2) as Probit models with the "have job" dummy variable as the dependent variable. Similar to

Table 3.3, Table 3.4 shows that the E-Verify mandate has no significant effect on the probability of an individual having jobs across different samples.

Since food security status is a discrete variable and its three values are ranked in the severity of food insecurity, an Ordered Probit model required in this scenario. Table 3.5, Table 3.6 and Table 3.7 show our primary results, which is the estimated average marginal effects from equation (1) and equation (2) as Ordered Probit model with food security status category as the dependent variable. Table 3.5 shows result of our baseline specification, which used food security measures with 12-months recall and the full sample. It shows the average marginal effect of E-Verify mandate and other covariate, which is the average impact of those variables on individuals' probability to be in each food security category. Table 3.6 and Table 3.7 show estimated treatment effects from other specifications which use more sub-samples. The difference between results in Table 3.6 and Table 3.7 is that they use different measures of food security. Table 3.6, like Table 3.5, shows the treatment effect on food security status with a 12-month recall, which is calculated based on questions asking interviewees' food situation in the past 12 months. Table 3.7 shows the treatment effect on food security status with a 30-days recall, which is calculated based on questions asking interviewees' food situation in the past 30 days.

As shown in Table 3.5, states' implementation of E-Verify mandates had significant adverse effects on the overall food security of households in our full sample, which included both citizens and non-citizens. Based on food security measures with a 12-months recall, states implementation of E-Verify mandates decreased the likelihood of households being in the "Food Secure" category by 1.7 percentage point. Compare to the baseline "Food Secure" probability of

87.66%, which is shown in Table 3.2, this is a significant reduction. Meanwhile, states implementation of E-Verify mandates increased the likelihood of households being in the “Food Insecure without Hunger” category by 0.87 percentage point and increased the likelihood of households being in the “Food Insecure with Hunger” category by 0.83 percentage point. Those changes are also large when compared to the baseline probability shown in Table 3.2. We will further discuss the scale of the impact in the discussion section of this paper. If we break E-Verify mandates by their types, the implementation of E-Verify mandates on public sectors and contractors decreased households’ likelihood of being in the “Food Secure” category by 1.85 percentage point. It also increased the likelihood of households being in the “Food Insecure without Hunger” category by 0.95 percentage point and increased the likelihood of households being in the “Food Insecure with Hunger” category by 0.90 percentage point. The implementation E-Verify mandates on all employer decreased households’ likelihood of being in the “Food Secure” category and increased the likelihood of households being in the “Food Insecure without Hunger” category and “Food Insecure with Hunger” category. However, the impact is smaller, and the significance level is only at the 0.10 level.

Table 3.6 and Table 3.7 provide estimates for more specifications with separated E-Verify mandate types, different food security measures and different sub-samples. As one can see in Table 3.6, the results with the full sample and overall food security level are what we have shown in Table 3.5. Other rows and columns show results with different measures of food security and different subsamples. With adults-only food security measure, the impact of the public sector and contractor E-Verify mandates is very similar to results from the full sample, and the effect of all-employer E-Verify mandate becomes insignificant. For the children food security measure, results shows that the implementation E-Verify mandates on public sectors and contractors have decreased children's probability of being in the "Food Secure" category by 1.86 percentage point, increased children's probability of being in the "Food Insecure without Hunger" category by 1.56 percentage point and increased children's probability of being in the "Food Insecure with Hunger" category by 0.29 percentage point. The difference in the impact again suggests that when facing the shock of the E-verify mandates, households prioritized the food supply of children, which is consistent with what we observed in the summary statistics.

The implementation of the E-Verify mandates on the public sector and contractors also caused a similar decrease in food security level among citizens, which is surprising considering that the E-Verify mandates are designed to protect those people who are undoubtedly authorized to work. The estimated treatment effect for non-citizens is larger and more significant than the estimated treatment effect from the full sample and the citizen sample as shown in the "non-citizen" section of Table 3.6.

Results in Table 3.6 also suggest that the implementation of E-Verify mandate on all employers has negative effect on the overall food security status. However, the significance level is at 0.10 level, and the effect becomes nonsignificant once we divide the full sample into citizen sample and non-citizen sample. E-Verify mandate on all employers also seems to have a larger impact on children's food security than adults' food security.

Table 3.7 shows results with food security measure with a 30-days recall. In the full sample, the E-Verify mandates on the public sector and contractor caused households to be 1.07 percentage point less likely to be in the "Food Secure" category. It caused households to be 0.55 percent percentage point more likely to be in the "Food Insecure without Hunger" category and

0.52 percentage point more likely to be in the “Food Secure with Hunger” category. Also, with a 30-days food security recall, some estimates in the non-citizen sample drop in significance level. Overall, the estimated treatment effect is smaller but still significant and consistent with results with 12-month recall food security measure considering the difference in the original food security level.

We also estimated OLS model versus our Ordered Probit Models in Table 3.8 – Table 3.10 for robustness check. The results are consistent with the estimate with those of the Ordered Probit Model. We also conducted a “Placebo Test”, where we randomly assigned 22 states as treated. We found that the estimated treatment effect is small and insignificant in the placebo test across different specifications.

3.6 Discussion and Conclusion

Implementation of E-verify mandates was hypothesized to impact employment, income, and household food insecurity. We found no statistically significant effects on household employment and income. The results are not surprising since some previous studies have found that the E-Verify mandates only have impacts on real wages and the probability of switching jobs for some specific subgroups (Orrenius and Zavodny, 2015). Other studies (Amuedo-Dorantes and Bansak, 2012) have found that the implementation of the E-Verify mandates has no impact at all on the labor market. No previous studies have found a significant effect on the labor market outcome for the full sample. However, we did find that implementation of E-Verify mandates had substantial adverse impacts on both U.S. citizens and non-citizens’ food security level. This effect was consistent over different subsamples and measures of food security status. We also found that the mechanism of this effect is unlikely to be related to family income or employment status. In addition, the adverse effect of the E-Verify mandates on food security was consistent for both non-citizens and U.S. citizens. The latter was not supposed to be negatively impacted by the E-Verify mandates on the labor market. Thus, it is unlikely that E-Verify affects food security by adversely affecting household's income. Based on previous findings, it is likely that the negative impact comes from other sources including the negative shock to the food, transportation, and retail sectors as well as potential shocks to the demand for social safety-net services.

We also found that the E-Verify mandates on all employers only had a marginally significant adverse effect on the food security status for all sub-samples. This is surprising since the all-employor E-Verify mandate was expected to have a more substantial impact. One possible explanation is that, included in the all employer mandate treatment group, were both the four states that implemented the E-verify mandates on all employers at the beginning and the five states that switched from public sector only mandate to the all employer mandate. Pooling these two groups together may have caused the estimated treatment effect to be smaller and the estimated variance in the treatment effect to be larger. Another possible explanation is that states that were able to pass laws that require the implementation of all-employer E-Verify mandates were states where the mandate was least likely to have substantial consequences for food insecurity.

The negative impact of the E-Verify mandate on food security is substantial in scale. One comparison we can draw is with the recession beginning in 2007. From 2006 to 2011, food insecurity level in the United States increased from 10.94 percent to 14.94 percent. Our estimate shows that overall, the E-Verify mandate increased the food insecurity level by 1.7 percentage points. This indicates that the impact of the E-Verify mandate was around 40 percent as large as the impact of the great recession beginning 2007.

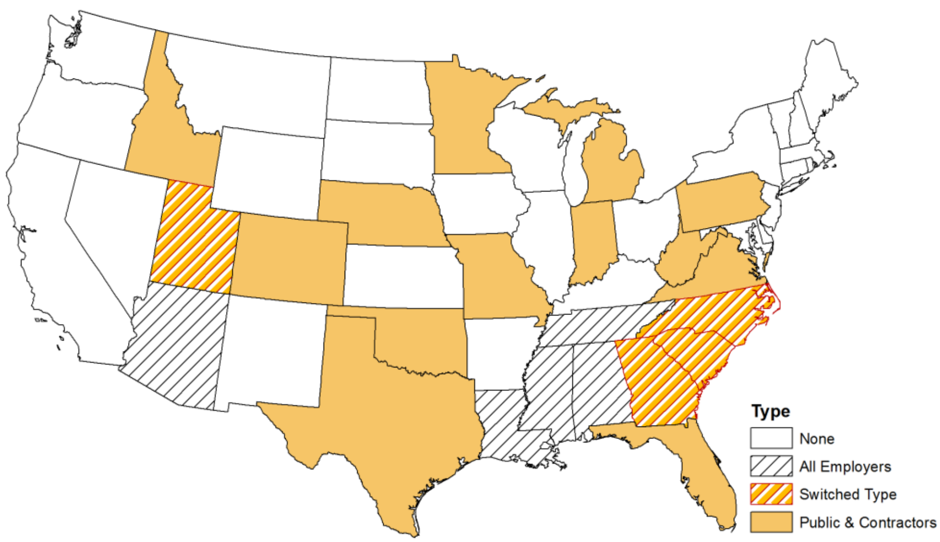
Our empirical study is not without limitations. First, while the CPS includes both citizen and non-citizen respondents, the representativeness of CPS data with regards to undocumented immigrants who are the primary target of E-verify mandates is unknown. To the extent that this population is not represented in our sample, our results likely provide a lower bound estimate of the impact of E-verify mandates on food insecurity. Additionally, we have no data on enforcement of E-verify mandates and the severity of penalties for violating E-verify mandates and/or the existence of informal labor markets that allow individuals to circumvent the mandates. There may be significant heterogeneity related to this across states, which we are unable to measure. Finally, more work is needed to understand the underlying mechanism of the impact of the E-Verify mandate on food security. While its effects on the food, transportation and retail sectors and demand for social safety-net services are possible candidates, we are not able to test hypotheses related to these potential mechanisms due to data limitations.

Nevertheless, the present study provides strong evidence for a significant unintended consequence of mandatory working eligibility verification that negatively impacts both U.S. citizens and non-citizens. This effect is robust to multiple measures of food insecurity and across different sub-populations.

Appendix C

Tables and Figures for Chapter 3

Figure 3.1. States Implemented E-verify Mandates between 2004 and 2016



Source: NCLS (2015)

Table 3.1. Summary Statistics

VARIABLES	(1) N	(2) Mean	(3) S.D	(4) Min	(5) Max
Treated	541,528	0.182	0.386	0	1
Treated (All Employer)	541,528	0.0590	0.236	0	1
Treated (Public Only)	541,528	0.123	0.329	0	1
African American	541,528	0.218	0.413	0	1
Hispanic	541,528	0.265	0.441	0	1
Military	541,528	0.0926	0.290	0	1
Senior[1]	541,528	0.609	0.488	0	1
Children[1]	541,528	0.854	0.353	0	1
College Degree	541,528	0.391	0.488	0	1
High School	541,528	0.539	0.498	0	1
Family Income[2]	541,528	9.633	4.749	0	16
Family Income Below $25,000[3]	541,528	0.3695	0.4827	0	1

[1]Senior and Children indicate whether individuals have seniors or children living in their household. [2]Family Income is a categorical variable, 9.6 is between category 9 (30,000-34,999) and category 10 (35,000-39,999). The median family income category is 11, which indicates family income between $40,000 and $49,999. [3]Family Income Below $25,000 is an indicator variable which assume the value of one if family income category is less than the $25,000-$29,999 category.

Table 3.2. Average Food Security Status Before E-verify Mandate

Food Security Measure	Food Secure	Food Insecure without Hunger	Food Insecure with Hunger	Observations
Overall Food Security, 12-Months Recall	87.66%	7.70%	4.63%	539,850
Adults Food Security, 12-Months Recall	88.44%	6.68%	4.87%	492,007
Children Food Security, 12-Months Recall	91.00%	8.10%	0.90%	156,652
Overall Food Security, 30-Days Recall	93.01%	4.18%	2.80%	491,996
Adults Food Security, 30-Days Recall	93.51%	3.60%	2.89%	491,993
Children Food Security, 30-Days Recall	94.83%	4.58%	0.59%	156,611

Table 3.3. Impact of Treatment on Probability of Family Income less than $25,000

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treated	-0.00470				
	(0.0192)				
Treated (All Employer)		0.00443	0.00244	0.0145	0.0119
		(0.0205)	(0.0196)	(0.0298)	(0.0250)
Treated (Public Only)		-0.00668	-0.0117	0.0288	0.0359
		(0.0193)	(0.0183)	(0.0279)	(0.0270)
Senior	0.0968***	0.0968***	0.102***	0.0652***	0.0674***
	(0.00511)	(0.00512)	(0.00498)	(0.00774)	(0.0116)
Children	0.0523***	0.0523***	0.0612***	0.0314***	0.00706
	(0.00402)	(0.00402)	(0.00359)	(0.00628)	(0.00582)
Military	-0.0309***	-0.0309***	-0.0295***	-0.0361***	-0.0344***
	(0.00330)	(0.00330)	(0.00313)	(0.00783)	(0.00937)
College Degree	-0.585***	-0.585***	-0.612***	-0.519***	-0.527***
	(0.00811)	(0.00810)	(0.00671)	(0.00878)	(0.0108)
High School	-0.326***	-0.326***	-0.349***	-0.273***	-0.271***
	(0.00600)	(0.00600)	(0.00486)	(0.00618)	(0.00682)
African American	0.0772***	0.0771***	0.0837***	0.0585***	0.0559***
	(0.00684)	(0.00680)	(0.00741)	(0.00653)	(0.00819)
Hispanic	0.0122*	0.0123*	-0.00424	0.0409***	
	(0.00627)	(0.00627)	(0.00719)	(0.00788)	
Observations	541,528	541,528	427,363	114,165	71,820
Sample	All	All	Citizen	Non-Citizen	Non-Citizen Hispanic

Column (1) shows Probit estimate of equation (1) with "Having family income less than $25,000" indicator variable as dependent variable. Column (2) – (5) shows Probit estimate of equation (2) with "Having family income less than $25,000" indicator variable as dependent variable.

Robust standard errors clustered at state level in parentheses

*** z<0.01, ** z<0.05, * z<0.1

Table 3.4. The Impact of Treatment on the Probability of Working

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treated	0.0135				
	(0.00973)				
Treated (All Employer)		0.00876	0.0100	-0.0108	-0.00744
		(0.0132)	(0.0131)	(0.0248)	(0.0252)
Treated (Public Only)		0.0146	0.0150	0.00194	0.00232
		(0.00938)	(0.00932)	(0.0136)	(0.0153)
Senior	-0.440***	-0.440***	-0.444***	-0.396***	-0.386***
	(0.00611)	(0.00611)	(0.00556)	(0.0119)	(0.0150)
Children	0.265***	0.265***	0.261***	0.284***	0.288***
	(0.00457)	(0.00456)	(0.00468)	(0.00935)	(0.0122)
Military	-0.0234***	-0.0234***	-0.0225***	-0.0239***	-0.0225***
	(0.00327)	(0.00328)	(0.00354)	(0.00753)	(0.00796)
College Degree	0.315***	0.315***	0.361***	0.233***	0.219***
	(0.0113)	(0.0113)	(0.00819)	(0.00967)	(0.00890)
High School	0.214***	0.214***	0.261***	0.133***	0.118***
	(0.00995)	(0.00995)	(0.00599)	(0.00656)	(0.00553)
African American	-0.00842*	-0.00833*	-0.0107**	-0.00525	-0.00672
	(0.00450)	(0.00447)	(0.00458)	(0.00588)	(0.00680)
Hispanic	0.0166***	0.0166***	0.0136***	-0.00755	
	(0.00442)	(0.00442)	(0.00445)	(0.00505)	
Observations	541,528	541,528	427,363	114,165	71,820
Sample	All	All	Citizen	Non-Citizen	Non-Citizen Hispanic

Column (1) shows Probit estimate of equation (1) with Working indicator as dependent variable.
Column (2) – (5) shows Probit estimate of equation (2) with Working indicator as dependent variable.
Robust standard errors clustered at state level in parentheses
*** z<0.01, ** z<0.05, * z<0.1

Table 3.5. Average Treatment Effect of E-Verify mandate on Overall Food Security Status

	All Treatment Types			Separate Treatment Types		
	FS = 1	FS = 2	FS = 3	FS = 1	FS = 2	FS = 3
Treated	-0.01698***	0.00871***	0.008273***			
	(0.005381)	(0.002752)	(0.002633)			
Treated (All Employer)				-0.01076*	0.005519*	0.005242*
				(0.006503)	(0.003336)	(0.003168)
Treated (Public Only)				-0.01848***	0.009477***	0.009002***
				(0.00529)	(0.002706)	(0.002588)
Senior	0.077526***	-0.03976***	-0.03777***	0.077565***	-0.03978***	-0.03779***
	(0.003607)	(0.001529)	(0.002117)	(0.003605)	(0.001529)	(0.002114)
Children	0.041229***	-0.02114***	-0.02008***	0.041263***	-0.02116***	-0.0201***
	(0.00182)	(0.000882)	(0.000989)	(0.001815)	(0.000879)	(0.000986)
Military	0.011373***	-0.00583***	-0.00554***	0.011385***	-0.00584***	-0.00555***
	(0.001793)	(0.000916)	(0.000881)	(0.001794)	(0.000916)	(0.000882)
College Degree	0.111049***	-0.05695***	-0.0541***	0.111067***	-0.05696***	-0.05411***
	(0.002521)	(0.00136)	(0.001445)	(0.002528)	(0.001364)	(0.001446)
High School	0.025222***	-0.01294***	-0.01229***	0.025244***	-0.01295***	-0.0123***
	(0.001877)	(0.000988)	(0.000909)	(0.001885)	(0.000992)	(0.000913)
African American	-0.04063***	0.020838***	0.019794***	-0.04078***	0.020913***	0.019865***
	(0.002278)	(0.001175)	(0.001147)	(0.002329)	(0.0012)	(0.001173)
Hispanic	-0.02739***	0.014047***	0.013343***	-0.02736***	0.014031***	0.013328***
	(0.002173)	(0.001183)	(0.001008)	(0.002182)	(0.001187)	(0.001013)
Family Income	0.013282***	-0.00681***	-0.00647***	0.013284***	-0.00681***	-0.00647***
	(0.00038)	(0.000192)	(0.000214)	(0.00038)	(0.000192)	(0.000214)
Observations	539,850	539,850	539,850	539,850	539,850	539,850
Sample	All	All	All	All	All	All

Estimated average marginal effect represent each variable' impact on the households' probability to be in each FS category.
FS = 1: Food Secure; FS = 2: Food Insecure without Hunger; FS = 3: Food Insecure with Hunger;
Estimation uses food security measures calculated by food security questions with 12-months recall
Estimation used full sample and Ordered Probit Model. Robust standard errors clustered at state level in parentheses
*** z<0.01, ** z<0.05, * z<0.1

Table 3.6. Detailed Average Treatment Effect of E-Verify Mandate on Food Security Status, with a 12-Month Recall

	Overall FS		Adults FS		Children FS	
	Treated (All)	Treated (Public)	Treated (All)	Treated (Public)	Treated (All)	Treated (Public)
All Sample						
FS = 1	-0.0107606*	-0.0184795***	-0. 0079491	-0.0172285***	-0.011309*	-0.018581***
	(0.0065026)	(0.0052904)	(0.0061232)	(0.0054117)	(0.0066697)	(0.0042677)
FS = 2	0.0055186*	0.0094773***	0.0037458	0.0081186***	0.0095695*	0.015723***
	(0.0033359)	(0.0027064)	(0.0028858)	(0.0025389)	(0.0056382)	(0.0036149)
FS = 3	0.005242*	0.0090023***	0.0042032	0.0091099***	0.0017395*	0.0028581***
	(0.0031678)	(0.002588)	(0.0032382)	(0.0028769)	(0.0010323)	(0.0006566)
Observation	539,850	539,850	492,007	492,007	156,652	156,652
Citizen						
FS = 1	-0.0076402	-0.0162974***	-0.004805	-0.0150199***	-0.0076093	-0.0169618***
	(0.0060601)	(0.0056914)	(0.0060185)	(0.0058569)	(0.0072311)	(0.0044783)
FS = 2	0.0038651	0.0082447***	0.0022209	0.0069423***	0.0065547	0.0146111***
	(0.0030643)	(0.0028714)	(0.00278)	(0.0026947)	(0.0062269)	(0.003858)
FS = 3	0.0037751	0.0080527***	0.0025841	0.0080776***	0.0010546	0.0023508***
	(0.0029962)	(0.002822)	(0.0032387)	(0.0031644)	(0.001005)	(0.0006265)
Observation	426,167	426,167	387,650	387,650	115,263	115,263
NonCitizen						
FS = 1	-0.0161723	-0.0235672**	-0.0163373	-0.00235264**	-0.0209517*	-0.0207565**
	(0.0171013)	(0.0148)	(0.0164539)	(0.0103691)	(0.0126541)	(0.01014)
FS = 2	0.0085827	0.0125073**	0.008108	0.0116759**	0.0170877*	0.0169285**
	(0.0090672)	(0.0078)	(0.0081527)	(0.0051393)	(0.0103365)	(0.0082832)
FS = 3	0.0075895	0.01106**	0.0082293	0.0118505**	0.003864*	0.003828**
	(0.0080353)	(0.0059)	(0.0083028)	(0.0052351)	(0.0023206)	(0.0018605)
Observation	113,683	113,683	104,357	104,357	41,389	41,389

Estimated average marginal effect represent treatment effect on the households' probability to be in each FS category.
FS = 1: Food Secure; FS = 2: Food Insecure without Hunger; FS = 3: Food Insecure with Hunger
Estimation uses food security measures calculated by food security questions with 12-months recall
Estimation used Ordered Probit Model. Robust standard errors clustered at state level in parentheses
*** z<0.01, ** z<0.05, * z<0.1

Table 3.7. Detailed Average Treatment Effect of E-Verify Mandate on Food Security Status, with a 30-Days Recall

	Overall FS		Adults FS		Children FS	
	Treated (All)	Treated (Public)	Treated (All)	Treated (Public)	Treated (All)	Treated (Public)
All Sample						
FS = 1	-0.0033833	-0.0107341***	-0.0026995	-0.0101852***	-0.0022346	-0.0073575***
	(0.0038485)	(0.0032313)	(0.0037072)	(0.0031246)	(0.0059774)	(0.0027887)
FS = 2	0.0017301	0.0054892***	0.001273	0.0048029***	0.0018733	0.0061678***
	(0.0019693)	(0.0016516)	(0.0017489)	(0.0014711)	(0.0050092)	(0.0023399)
FS = 3	0.0016531	0.0052449***	0.0014266	0.0053824***	0.0003613	0.0011897***
	(0.0018794)	(0.001582)	(0.0019584)	(0.0016557)	(0.0009683)	(0.0004509)
Observation	491,996	491,996	491,993	491,993	156,611	156,611
Citizen						
FS = 1	-0.001314	-0.0097378***	-0.000453	-0.0091163***	-0.0019031	-0.005814**
	(0.0039819)	(0.0038433)	(0.0039196)	(0.0037486)	(0.0053935)	(0.0030346)
FS = 2	0.0006551	0.0048545***	0.0002079	0.0041849***	0.0016285	0.0049751**
	(0.001985)	(0.0019157)	(0.0017992)	(0.0037486)	(0.0046153)	(0.0025997)
FS = 3	0.000659	0.0048833***	0.000245	0.0049314***	0.0002746	0.0008389**
	(0.0019969)	(0.0019285)	(0.0021204)	(0.0020324)	(0.0007783)	(0.0004368)
Observation	387,641	387,641	387,639	387,639	115,241	115,241
NonCitizen						
FS = 1	-0.0091439	-0.0120476*	-0.010389	-0.0121817**	-0.0146626	-0.0096701
	(0.0113352)	(0.0071391)	(0.0103419)	(0.0067034)	(0.0124412	(0.0084372)
FS = 2	0.0049916	0.0065767*	0.0052646	0.0061731**	0.0117957	0.0077794
	(0.0061813)	(0.003893)	(0.0052365)	(0.0033956)	(0.0100009)	(0.0067837)
FS = 3	0.0041523	0.0054709*	0.0051243	0.0060086**	0.0028669	0.0018908
	(0.0051545)	(0.0032478)	(0.0051064)	(0.0033099)	(0.0024433)	(0.0016554)
Observation	104,355	104,355	104,354	104,354	41,370	41,370

Estimated average marginal effect represent treatment effect on the households' probability to be in each FS category.
FS = 1: Food Secure; FS = 2: Food Insecure without Hunger; FS = 3: Food Insecure with Hunger
Estimation uses food security measures calculated by food security questions with 30-days recall
Estimation used Ordered Probit Model. Robust standard errors clustered at state level in parentheses
*** z<0.01, ** z<0.05, * z<0.1

Table 3.8. OLS Model, the Impact of Treatment on Overall Food Security Status

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treated	0.0293***				
	(0.00850)				
Treated (All Employer)		0.0190*	0.0139	0.0291	
		(0.0102)	(0.0102)	(0.0243)	
Treated (Public Only)		0.0315***	0.0281***	0.0393***	
		(0.00841)	(0.00951)	(0.0146)	
Placebo Treatment					0.00324
					(0.00465)
Senior	-0.118***	-0.118***	-0.125***	-0.0894***	-0.119***
	(0.00495)	(0.00496)	(0.00486)	(0.00561)	(0.00489)
Children	-0.0591***	-0.0592***	-0.0560***	-0.0750***	-0.0596***
	(0.00314)	(0.00313)	(0.00346)	(0.00509)	(0.00310)
Military	-0.0137***	-0.0137***	-0.0132***	-0.0126**	-0.0130***
	(0.00250)	(0.00250)	(0.00265)	(0.00608)	(0.00253)
College Degree	-0.175***	-0.175***	-0.166***	-0.194***	-0.174***
	(0.00535)	(0.00536)	(0.00763)	(0.00808)	(0.00523)
High School	-0.0792***	-0.0793***	-0.0736***	-0.0869***	-0.0789***
	(0.00502)	(0.00503)	(0.00688)	(0.00706)	(0.00482)
African American	0.0714***	0.0716***	0.0757***	0.0596***	0.0706***
	(0.00461)	(0.00467)	(0.00480)	(0.00748)	(0.00471)
Hispanic	0.0406***	0.0406***	0.0316***	0.0563***	0.0424***
	(0.00363)	(0.00365)	(0.00400)	(0.00546)	(0.00381)
Family Income	-0.0207***	-0.0207***	-0.0210***	-0.0199***	-0.0209***
	(0.000722)	(0.000723)	(0.000663)	(0.00120)	(0.000700)
Constant	1.477***	1.477***	1.472***	1.479***	1.476***
	(0.0119)	(0.0118)	(0.0124)	(0.0157)	(0.0123)
Observations	539,850	539,850	426,167	113,683	541,867
R-squared	0.086	0.086	0.085	0.088	0.086
Sample	All	All	Citizen	Non-Citizen	All

Column (1) & (5) shows OLS estimate of equation (1) with Overall Food Security as dependent variable.
Column (2) – (4) shows OLS estimate of equation (2) with Overall Food Security as dependent variable.
Robust standard errors clustered at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.9. OLS Model, the Impact of Treatment on Adults Food Security Status

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treated	0.0273***				
	(0.00859)				
Treated (All Employer)		0.0147	0.00989	0.0265	
		(0.00993)	(0.0105)	(0.0240)	
Treated (Public Only)		0.0299***	0.0268***	0.0381**	
		(0.00861)	(0.00986)	(0.0151)	
Placebo Treatment					0.00422
					(0.00469)
Senior	-0.113***	-0.113***	-0.121***	-0.0797***	-0.114***
	(0.00522)	(0.00523)	(0.00515)	(0.00591)	(0.00517)
Children	-0.0385***	-0.0386***	-0.0392***	-0.0431***	-0.0394***
	(0.00331)	(0.00330)	(0.00360)	(0.00635)	(0.00338)
Military	-0.0127***	-0.0127***	-0.0129***	-0.00888	-0.0119***
	(0.00269)	(0.00268)	(0.00299)	(0.00640)	(0.00270)
College Degree	-0.158***	-0.158***	-0.154***	-0.169***	-0.157***
	(0.00549)	(0.00551)	(0.00778)	(0.00789)	(0.00533)
High School	-0.0683***	-0.0684***	-0.0670***	-0.0699***	-0.0679***
	(0.00503)	(0.00504)	(0.00697)	(0.00692)	(0.00483)
African American	0.0692***	0.0695***	0.0729***	0.0594***	0.0685***
	(0.00469)	(0.00476)	(0.00501)	(0.00707)	(0.00479)
Hispanic	0.0354***	0.0354***	0.0291***	0.0511***	0.0373***
	(0.00365)	(0.00367)	(0.00426)	(0.00523)	(0.00383)
Family Income	-0.0212***	-0.0212***	-0.0216***	-0.0198***	-0.0213***
	(0.000701)	(0.000701)	(0.000664)	(0.00110)	(0.000679)
Constant	1.533***	1.533***	1.542***	1.495***	1.536***
	(0.0141)	(0.0140)	(0.0140)	(0.0168)	(0.0145)
Observations	492,007	492,007	387,650	104,357	494,024
R-squared	0.082	0.082	0.082	0.080	0.082
Sample	All	All	Citizen	Non-Citizen	All

Column (1) & (5) shows OLS estimate of equation (1) with Adults Food Security as dependent variable.
Column (2) – (4) shows OLS estimate of equation (2) with Adults Food Security as dependent variable.
Robust standard errors clustered at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.10. OLS Model, the Impact of Treatment on Children Food Security Status

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treated	0.0206***				
	(0.00477)				
Treated (All Employer)		0.0147*	0.0101	0.0259*	
		(0.00811)	(0.00926)	(0.0151)	
Treated (Public Only)		0.0218***	0.0200***	0.0237**	
		(0.00490)	(0.00575)	(0.0115)	
Placebo Treatment					0.00215
					(0.00334)
Senior	-0.00691	-0.00691	-0.00990*	0.00192	-0.00559
	(0.00498)	(0.00499)	(0.00582)	(0.00947)	(0.00504)
Children	0.00974**	0.00973**	0.00767*	0.0176**	0.00913**
	(0.00370)	(0.00371)	(0.00430)	(0.00831)	(0.00360)
Military	-0.00467	-0.00469	-0.000688	-0.0138*	-0.00379
	(0.00375)	(0.00376)	(0.00405)	(0.00747)	(0.00386)
College Degree	-0.123***	-0.123***	-0.119***	-0.125***	-0.121***
	(0.00658)	(0.00658)	(0.00882)	(0.00760)	(0.00659)
High School	-0.0688***	-0.0689***	-0.0679***	-0.0609***	-0.0683***
	(0.00686)	(0.00685)	(0.00862)	(0.00950)	(0.00662)
African American	0.0317***	0.0318***	0.0315***	0.0301***	0.0304***
	(0.00435)	(0.00441)	(0.00361)	(0.00823)	(0.00428)
Hispanic	0.0205***	0.0205***	0.0131***	0.0204***	0.0216***
	(0.00242)	(0.00243)	(0.00383)	(0.00435)	(0.00259)
Family Income	-0.0120***	-0.0120***	-0.0118***	-0.0127***	-0.0122***
	(0.000522)	(0.000522)	(0.000450)	(0.000909)	(0.000514)
Constant	1.263***	1.263***	1.265***	1.249***	1.263***
	(0.0110)	(0.0110)	(0.0117)	(0.0225)	(0.0113)
Observations	156,652	156,652	115,263	41,389	157,399
R-squared	0.064	0.064	0.061	0.061	0.064
Sample	All	All	Citizen	Non-Citizen	All

Column (1) & (5) shows OLS estimate of equation (1) with Children Food Security as dependent variable.
Column (2) – (4) shows OLS estimate of equation (2) with Children Food Security as dependent variable.
Robust standard errors clustered at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.11. OLS Model, the Impact of Treatment on Household Income

VARIABLES	(1)	(2)	(3)	(4)	(5)
Treated	0.121				
	(0.248)				
Treated (All Employer)		0.000972	0.0195	-0.0804	0.00624
		(0.250)	(0.237)	(0.354)	(0.284)
Treated (Public Only)		0.146	0.189	-0.169	-0.212
		(0.248)	(0.235)	(0.338)	(0.290)
Senior	-0.856***	-0.857***	-0.904***	-0.600***	-0.623***
	(0.0405)	(0.0405)	(0.0420)	(0.0647)	(0.0745)
Children	-0.598***	-0.599***	-0.657***	-0.458***	-0.157***
	(0.0343)	(0.0344)	(0.0322)	(0.0552)	(0.0487)
Military	0.300***	0.300***	0.285***	0.356***	0.346***
	(0.0352)	(0.0352)	(0.0342)	(0.0775)	(0.0813)
College Degree	6.222***	6.222***	6.460***	5.513***	5.312***
	(0.0712)	(0.0712)	(0.0711)	(0.0893)	(0.101)
High School	3.154***	3.154***	3.362***	2.608***	2.457***
	(0.0457)	(0.0459)	(0.0531)	(0.0451)	(0.0473)
African American	-0.870***	-0.868***	-0.943***	-0.659***	-0.598***
	(0.0756)	(0.0751)	(0.0819)	(0.0709)	(0.0794)
Hispanic	-0.120*	-0.120*	0.0535	-0.438***	
	(0.0681)	(0.0681)	(0.0824)	(0.0821)	
Constant	5.110***	5.108***	4.939***	5.750***	5.480***
	(0.151)	(0.150)	(0.154)	(0.183)	(0.167)
Observations	541,528	541,528	427,363	114,165	71,820
R-squared	0.211	0.211	0.217	0.191	0.174
Sample	All	All	Citizen	Non-Citizen	Non-Citizen Hispanic

Column (1) shows OLS estimate of equation (1) with Family Income as dependent variable.
Column (2) – (5) shows OLS estimate of equation (2) with Family Income as dependent variable.
Robust standard errors clustered at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.12. States E-Verify Mandate Implementation Year

STATE	PUBLIC & CONTRACTORS	ALL EMPLOYERS
ALABAMA	N/A	2012
ARIZONA	N/A	2008
COLORADO	2006	N/A
FLORIDA	2011	N/A
GEORGIA	2006	2012
IDAHO	2009	N/A
INDIANA	2011	N/A
LOUISIANA	N/A	2012
MICHIGAN	2012	N/A
MINNESOTA	2011	N/A
MISSISSIPPI	N/A	2008
MISSOURI	2009	N/A
NEBRASKA	2009	N/A
N. CAROLINA	2006	2012
OKLAHOMA	2007	N/A
PENNSYLVANIA	2013	N/A
S. CAROLINA	2008	2012
TENNESSEE	N/A	2012
TEXAS	2015	N/A
UTAH	2008	2010
VIRGINIA	2013	N/A
WEST VIRGINIA	2012	N/A

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BIOGRAPHICAL SKETCH

Xia Si was born July 31, 1989 in Shandong, China. He is the only son of Shitong Si, who is an excellent surgeon and Yasong Gao, who is an equally excellent business manager. After he graduated from Qingdao University, Xia traveled to the United States in 2011 to seek knowledge and global insight. He luckily met his love of life, Haozhe Wang, in Dallas. Her company made his life in a foreign country so much more beautiful.

Xia and Haozhe got married in 2016 in Plano, Texas.

CURRICULUM VITAE

EDUCATION

2018	**The University of Texas at Dallas**, Richardson, TX Ph.D. in Economics **Fields**: Econometrics and Labor Economics **Dissertation Title**: "Three Essays on the Economics of Nutrition Assistance and Food Security" **Committee**: Kurt J. Beron (Chair), Tammy Leonard, Daniel G. Arce M., and Rodney Andrews
2011	**Qingdao University**, Shandong, China B.S. in Economics

PUBLICATION

"Utilizing Behavioral Economics to Understand Adherence to Physical Activity Guidelines Among a Low-Income Urban Community." Shuval, K., **Si, X.**, Nguyen, B., & Leonard, T (2015). *Journal of Physical Activity and Health, 12(7), 947-953.*

WORKING PAPERS

"Aging out of WIC: An Investigation of the Compensation Effect of Private Nutrition Assistance Programs"

"Network Expansion and Utilization of Private Nutrition Assistance"

"The Food Security Consequence of Mandated Employment Verification"

"Does Public Transportation affect Private Nutrition Assistance Usage? Evidence from the Expansion of DART Stations". Zhang, Z., **Si, X.**, & Leonard, T.

AWARD / SCHOLARSHIP

2013 – Present	Graduate Fellowship, UT Dallas
2017	Charles C. McKinney Graduate Scholarship, UT Dallas

INVITED TALKS

2017	University of North Texas
2016	University of Texas Southwestern Medical Center
2016	University of Dallas

OTHER RESEARCH EXPERIENCE

2013 – 2015	Research Assistant for Dr. Tammy Leonard *Responsibilities include but not limited to: Communicating with partner organizations, designing and overseeing data collection process, identifying policy-relevant research ideas, conducting data analysis with a variety of software package, and writing research reports.*

TEACHING / WORKING EXPERIENCE

Fall 2017	Instructor for Principles of Macroeconomics
Summer 2017	Instructor for Principles of Macroeconomics
Spring 2017	Instructor for Principles of Macroeconomics
Fall 2016	Instructor for Principles of Macroeconomics *Focused on understanding, exploration, and application. Received excellent evaluations (4.46). Students wrote that they especially like the way this course introduces economic theory combined with real-world events.*
Spring 2016	Teaching Assistant for International Trade
Fall 2015	Teaching Assistant for Microeconomics Theory III *Responsibilities include evaluating students' performance and tutoring.*