Georgia State University

ScholarWorks @ Georgia State University

AYSPS Dissertations

Andrew Young School of Policy Studies

Spring 5-4-2020

Essays in Applied Microeconomics on Mexico

Francisco Beltran Silva

Follow this and additional works at: https://scholarworks.gsu.edu/aysps_dissertations

Recommended Citation

Beltran Silva, Francisco, "Essays in Applied Microeconomics on Mexico." Dissertation, Georgia State University, 2020.

doi: https://doi.org/10.57709/17139128

This Dissertation is brought to you for free and open access by the Andrew Young School of Policy Studies at ScholarWorks @ Georgia State University. It has been accepted for inclusion in AYSPS Dissertations by an authorized administrator of ScholarWorks @ Georgia State University. For more information, please contact scholarworks@gsu.edu.

ABSTRACT

ESSAYS IN APPLIED MICROECONOMICS ON MEXICO

BY

FRANCISCO BELTRAN-SILVA

MARCH 2020

Committee Chair: Dr. Alberto Chong

Major Department: Economics

This dissertation comprises three chapters on applied microeconomics that study different economic aspects around the intersection between street violence and nutrition in the context of Mexico. The first and pivot chapter, titled "Military Interventions and Obesity: Evidence from Mexico's Drug War", studies the relationship street violence and body weight. This chapter examines if exposure to street violence originated in the military interventions against the drug trafficking organizations (DTOs) that started in 2006 in Mexico had an impact on weight using longitudinal data from a household survey. My results indicate that military operations affect weight positively, increasing overweight although not to the extent of inducing obesity.

The second chapter, titled "Will Violent Crime Incentivize the Hiding of Small Firms?", explores the impact from street violence on a different outcome: tax compliance. This chapter examines the relationship between crime exposure and informality of businesses using a rotating

panel survey matched to municipal homicide rates. My hypothesis is that losses derived from crime may take away income that could otherwise be used to afford formality. Also, firms may prefer to stay underground to avoid disclosing their existence to criminals. I find that exposure to violent crime promotes informality. These results are further corroborated by using temperature as an instrumental variable.

The third chapter, “Fighting Against Hunger: A Country-Wide Intervention and its Impact on Birth Outcomes,” steps away from the crime scene to focus on nutrition again. This chapter studies the impact of *Sin Hambre* (SH), a food assistance program introduced in Mexico in 2013, on birth weight. I use a difference in difference approach exploiting timing and regional variations in exposure to evaluate the impact of the overall program on birthweight. Since municipalities were not randomly assigned, linear regression methodologies may lead to biased estimates. In order to address these concerns and obtain causal estimates, I employ a multiperiod difference-in-difference matching method. I find that exposure to SH leads to moderate impacts on birth weight at best.

ESSAYS IN APPLIED MICROECONOMICS ON MEXICO

BY

FRANCISCO BELTRAN-SILVA

A Dissertation Submitted in Partial Fulfillment

of the Requirements for the Degree

of

Doctor of Philosophy

in the

Department of Economics at the Andrew Young School of Policy Studies

of

Georgia State University

GEORGIA STATE UNIVERSITY

2020

Copyright by Francisco Beltran-Silva 2020

ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

Dissertation Chair:	Dr. Alberto Chong
Committee:	Dr. Daniel Kreisman
	Dr. Pierre Nguimkeu
	Dr. Charles Courtemanche

Electronic Version Approved:

Sally Wallace, Dean

Andrew Young School of Policy Studies

Georgia State University

May, 2020

DEDICATION

To my grandfather, whom I always admired for his kind heart and clever mind.

ACKNOWLEDGEMENTS

I am grateful to the many people who have inspired, guided, and supported me in the writing of this dissertation. I could not have made it this far without the support of my family. Despite the distance, the constant support from my mother and father lighted this long path. I am especially grateful to my sister, Ana Cristina, for always holding my hand.

My exclusive acknowledgments to Alberto Chong, my advisor and mentor, who fostered my creativity and gave validity to my dissertation. I want to thank my other mentors, Daniel Kreisman, Pierre Nguimkeu, Charles Courtemanche, John Gibson, and Tom Mroz, who never ceased to surprise me with their kindness and wisdom.

Over the course of almost five years I learned so many things that, I believe, I ended up unlearning a few others. The long hours thinking, reading, and writing allowed me an endless stream of short moments to reflect about life and its meaning. After all these years I realize the most important thing in life is to enjoy the present, celebrating ourselves as much as the people that keep us smiling, our friends and family. I am thankful I had this experience for it gave me the chance of meeting new amazing people that are now important part of my life, especially Will Davis and Yasin Civelek.

Table of Contents

List of Figures

List of Tables

1 Introduction

The present dissertation comprises three chapters on applied microeconomics that study different economic aspects around the intersection between street violence and nutrition in the context of Mexico.

The first chapter in my dissertation examines if exposure to military operations against the drug trafficking organizations in Mexico caused any changes in weight-related outcomes using the number of operations from military records and longitudinal data from the Mexican Family Life Survey (MXFLS). My hypothesis is that these policies and the subsequent violence triggered, affected weight-related outcomes by inducing stress as well as by affecting food consumption and physical activity behaviors. I estimate a generalized difference in difference model that exploits the presumably exogenous regional variations in the number of military operations while controlling for selective migration. My results indicate that military operations affect weight positively, increasing overweight although not to the extent of inducing obesity. Using a score designed to diagnose depression I provide evidence that emotional well-being affections may lead these effects. Exploring other potential mechanisms, I cannot rule out that food expenditures and reductions in physical activity are potential drivers, but estimates are mostly insignificant. Therefore, observed changes may originate in metabolism alterations due to stress.

The second chapter in my dissertation examines the relationship between exposure to violent crime and firm informality. I employ a rotating panel survey matched to municipal homicide rates and temperature as an instrument for homicide rates and find that exposure to violent crime causes existing firms to become informal. My hypothesis is that losses derived from crime may take away income that could otherwise be used to afford formality. Also, firms may prefer to stay underground to avoid disclosing their existence to criminals. I find that exposure to violent crime promotes informality. On average an additional homicide per 10,000 people each quarter increases the probability to be informal between 0.6 percentage points and 1.1 percentage points. These results are further corroborated when using temperature as an instrumental variable, which indicates that these estimates range between 13.7 percentage points and 17.3 percentage points.

The third chapter estimates the impact of *Sin Hambre* (SH), a food assistance policy implemented in Mexico in 2013 on birth weight and other birth related outcomes. This national policy is a broad targeting and coordination strategy involving a large set of programs from several ministries aiming to fight hunger. I use a difference in difference approach exploiting timing and regional variations in exposure to evaluate the impact of the overall program on birthweight. Since municipalities were not randomly assigned, linear regression methodologies may lead to biased estimates. In order to address these concerns and obtain causal estimates, I employ a multiperiod difference-in-difference matching method proposed by Imai, Kim and Wang (2020), which compares each treated unit to a control unit constructed to be similar to the treated observation in terms of outcome and covariate histories.[1] I find that exposure to SH leads to moderate impacts on birth weight at best. I observe an impact of at most 5 grams for the national sample. Focusing on income eligible women only, estimates indicate an impact of at most 9 grams. Estimates, however, are not robust across specifications and some estimates point into null effects. Results using other methodologies lead to similar conclusions where some beneficial impact is observed but not for all specifications.

[1] Imai, K., Kim, I.S. and Wang, E., 2020. Matching Methods for Causal Inference with Time-Series Cross-Sectional Data.

2 Military Interventions and Obesity: Evidence from Mexico's Drug War

2.1 Introduction

Economic literature indicates that increasing rates in obesity can be explained by several economic factors including technological change, income, food prices, urban sprawl, labor force participation, among others. [2] According to recent literature, exposure to street violence, measured through crime rates, could also be a relevant determinant of obesity (Yu et al., 2016). This research is mostly associational and the gap in the literature exists for two reasons. First, because of legal and ethical constraints, lab designs cannot induce realistic levels of stress like those ascribed to street violence. Second, quasi-experimental study designs are difficult given the rarity of exogenous sources of variation in street violence that could address endogeneity concerns.

The military interventions against the drug-trafficking-organizations (DTOs) that started in Mexico in 2006 provide an opportunity to study the effect of street violence on obesity. The so called "war on drugs" turned Mexico into one of the most violent nations. Including civilians not directly involved in the conflict, the country observed a death toll of roughly 234,000 homicides from 2006 to 2017 (Hernández Borbolla, 2017). Anecdotal evidence documented by newspapers reports people responded by taking shelter indoors to avoid the risks of an ongoing outdoors violence ("Violencia en Ciudad Juárez," 2009; "Tierra sin ley," 2011; "Golpea la violencia al turismo," 2012; Solera et al., 2012). These context raises questions on the health-related consequences associated with the conflict. Previous literature already documents evidence in this

[2] For further discussion see Hill et al. (1999), Cutler et al. (2003), Chou et al. (2004), Lakdawalla et al. (2005) and Courtemanche et al. (2016).

direction as it shows drug-related violence, measured through homicide rates, is associated with deleterious impacts on low birthweight and depression, yet, other measures of health have been ignored (Brown 2016, Balmori et al. 2015).[3] My hypothesis is that the military conflict could have affected weight related outcomes by inducing stress as well as by affecting food consumption and physical activity behaviors. Aside from the violent ground, Mexico is ideal to study this question as it has become one of the countries with the highest rates of obesity after the United States.[4] Since obesity is notoriously costly in terms of medical care spending, chronic conditions incidence, and death rates exploring this subject is of particular interest to Mexico (Flegal et al., 2005; Cawley et al., 2012; Sturm, 2012).[5] Lastly, the fact that medical infrastructure, namely clinics and hospitals, was not destroyed along the conflict makes this military conflict especially appealing.

The military interventions had their way triggering violence through several pathways. First, they provoked a crossfire between the DTOs and official authorities, that is military, navy and police forces. Second, by taking down former heads of historically large DTOs they led to the creation of multiple smaller DTOs which created further violence (Castillo et al., 2013). Lastly, not only the DTOs, but also the military committed human rights violations which affected civilians (Jiménez-Cáliz, 2017). Military operations records are a novel and arguably better measure of the distress caused by the conflict than homicide rates, the norm across related literature. First, military convoys are visible through highways and main roads, as opposed to

[3] Similarly, such literature has generally found drug-related violence, measured through homicide rates, adversely affects outcomes like migration (Basu et al., 2013, Atuesta et al., 2016), educational attainment (Márquez-Padilla et al., 2015, Jarillo et al., 2016, Brown et al., 2017), risk aversion (Brown et al., 2017), perception of insecurity (Gutiérrez-Romero 2016), economic activity (Robles et al., 2013, Enamorado et al., 2014) and household expenditures (Velásquez 2010), among others.
[4] According to the World Health Organization (2017) as of 2015 the rate of obesity in Mexico was 32.4% while that in the US was 38.2%.
[5] For instance, Cawley et al. (2012) estimate that the annual cost of treating obesity in the US is equivalent to 16.5% of all national medical care spending.

homicides, many of which do not occur in public venues and so are not noticeable. Second, military operations are a direct measure of the intensity of policy to be analyzed. Homicide rates, for instance, may not capture the impacts from military assaults not resulting in deaths.[6] Third, given that homicides are not classified with enough detail and many are not reported, any index of drug-related violence is subject to measurement error. Fourth, given its unprecedented nature the exogeneity of military operations can be unambiguously evaluated through an event study.[7] Admittedly, an inherent disadvantage of military operations is that records are only available at year by state level which misses to capture the effect from independent variations at a finer level.

This paper is relevant for several reasons. First, to the best of my knowledge, I am the first to analyze the impact of exposure to military operations on weight. Second, given the unprecedented nature of military interventions my paper is the first to provide causal evidence on the link between street violence and weight. Third, the use of the number of military operations introduces an alternative and cleaner identification to study the impact of this type of conflicts on other outcomes as it overcomes endogeneity concerns inherent to homicide rates. Fourth, different from research that studies the effect of violence on weight only, I explore several potential mechanisms including depression symptoms, safety perceptions, consumption and physical activity in the same longitudinal setting.

The paper is structured in the following way. Section 2 presents a theoretical framework to illustrate how street violence could affect weight. Section 3 details the data to be used. Section 4 describes the empirical methodology. Section 5 presents results. Finally, Section 6 outlines conclusions.

[6] Figure 2.1 in the Appendix shows the evolution of both military operations and homicide rates.

[7] Information from the Ministry of Defense (SEDENA) also shows soldiers by state are constant until 2006, indicating there were no operations of this nature prior to December 2006, at least since 2000.

2.2 Theoretical framework

Based on related literature I assume weight is related to caloric intake and energy expenditure through the following linear relationship:

$$W_i = k_i(F) - e_i(P, h) \tag{1}$$

where increases in caloric consumption, $k_i(F)$, increase weight though food consumption, F, and increases in energy expenditure $e_i(P, h)$ counteract these increases via physical activity, P, and physiological processes, h. The term i indicate functional differences across individuals due to lifestyle and genetics.

Stress can be defined as any situation that disturbs the equilibrium between a living organism and its environment like an injury, noise or personal problems (Ranabir et al., 2011). I argue that street violence is one such stressor. Evidence on the links between stress and weight suggest such relationship arises from behavioral and somatic responses that affect both F and P, where somatic responses refer to involuntary changes inherent to the body while behavioral responses include deliberate changes (De Vriendt et al., 2009).[8]

On somatic responses, literature points out that in general, stress promotes obesity by affecting appetite and altering physical activity.[9] About food consumption, literature indicates stress might be linked to neurotransmitters and hormones that control appetite causing what is known as stress-eating, (Björntorp, 2001; Torres et al., 2007; Epel et al., 2001; Oliver et al., 2000). Regarding physical activity, P, evidence documents there is a negative causal relationship between stress and physical activity mainly expressed through depression episodes where a positive relationship is found only among people already in the habit of exercise (Stults-

[8] Consensus is that in response to a stressor the sympathetic nervous system sends a signal that additional energy is required to face a threat which if left unused can lead to weight gain (Foss et al., 2011, Ranabir et al., 2011).
[9] The effect that stress, measured by cortisol levels, has on obesity is well documented in the medical literature (Rosmond et al., 1998, Björntorp et al., 2000, Björntorp, 2001, Bose et al., 2009, Ranabir et al., 2011).

Kolehmainen et al., 2014).[10] Recent findings also show that daily stressors and past depression can alter metabolism and promote obesity by decreasing post meal energy expenditures (Kiecolt-Glaser et al., 2015), the latter of which would affect the functional form of e_i.

In terms of behavioral responses, street violence could affect weight by altering physical activity and food consumption patterns. This is plausible if outdoors activities are costlier due to insecurity although individual differences on preferences and proximity to different goods will rule such relationship. Behavioral changes could also promote somatic changes if spending more time at home reinforces appetite distortions while further dissuading physical activity, (Santaliestra-Pasías et al., 2013; Aceves-Martins et al., 2016).[11] Insecurity could have the opposite effect if, for instance, it reduces visits to fast-food like restaurants and street-vendors stands. Furthermore, street violence could affect decisions related to working, commuting and entertainment alternatives which involve different bundles of food and physical activity. The former insights suggest that somatic responses to street violence may lead to increases in weight but that behavioral responses could go either way.

2.3 Data

The data I use for the analysis comes from three main sources. The first is the Mexican Family Life Survey (MxFLS), which I use to obtain information on weight related outcomes as well as depression symptoms, safety perceptions, food consumption and physical activity to explore potential mechanisms. MxFLS is a longitudinal survey conducted in three waves: the first,

[10] Since physical activity has beneficial effects on stress coping capacity, mental health, and weight regulation, reductions in physical activity following stressful events could have further adverse effects (De Vriendt et al., 2009; Stults-Kolehmainen et al., 2014).

[11] TV viewing has widely been identified to promote unhealthy food preferences through exposure to food advertisements, a higher energy intake by automatic eating, and overconsumption caused by distraction. Similarly, insecurity could motivate purchases of packaged versus fresh food. Packaged foods can be highly caloric and can contain food additives known to decrease circulating leptin, a hormone that helps inhibiting hunger (Ciardi et al., 2012; Mangge et al., 2013).

MxFLS-1, in 2002, the second, MxFLS-2, from 2005 to 2006, and the third, MxFLS-3, from 2009 to 2012.[12] The second source consists of records on military operations per state and year from the Mexican Secretariat of National Defense (SEDENA- Secretaría de la Defensa Nacional).[13] These records distinguish between two types of operations: eradications, which target drug fields, and interceptions, which target drug-related traffic through land vehicle checkpoints and surveillance stations that focus on air and water routes.[14] The third is administrative data on daily homicides collected by the National Institute of Statistics and Geography (INEGI- Instituto Nacional de Estadística y Geografía) to estimate state and municipality homicide rates by month and year as an alternative measure of the conflict intensity.

The main outcomes from MxFLS consist of Body Mass Index (BMI), as well as binary indicators for ranges of BMI indicating underweight, (BMI<18.5), normal weight (18.5≤BMI<25), overweight (25≤BMI<30) and obesity (30≤BMI).[15] These weight related outcomes are derived using clinically measured observations of height and weight. Secondary outcomes comprise safety perceptions, depression, food consumption and physical activity outcomes. Safety perceptions refer to self-reported measures related to insecurity. Depression is quantified based on 20 questions that are used to diagnose depressive syndrome in Mexico. These questions, designed and tested by researchers from the Mexican Institute of Psychiatry to help diagnosing depression syndrome, can be answered in negative or positive form, in which three possibilities are accepted: sometimes, many times, and all the time. Answers are given a

[12] MxFLS-1 collects information on 16 of Mexico's 32 states and is representative of the national population in 2002. MxFLS-2 and MxFLS-3 relocates and re-interviews almost 90 percent of the original households sampled through MxFLS-1. MxFLS re-interviews include individuals or households that grew out from previous samples and those who migrated, the latter of which added observations from other states.

[13] Data on military operations were acquired via direct request of SEDENA.

[14] Figure 2.2 shows the cumulative number of military operations by state for different years.

[15] BMI is a weight-to-height ratio calculated by dividing a person's weight in kilograms by the square of its height in meters.

value of 1 if negative (No) and a value of 2 to 4 if positive, according to a progressive order. Consequently, the scale of measurement that quantifies the depressive syndrome can take any value between 20 and 80. I use both the depression score and binary indicators based on thresholds determined by the Mexican Institute of Psychiatry that define mild, moderate and severe depression. Consumption outcomes consist of food expenses made a week prior to the interview at the household level. Consumption measures are additionally adjusted by household monthly income to disentangle the direct impact of military interventions on food consumption composition away from changes in economic activity that affected income and thus consumption. To explore if there are shifts in diet expenses, these are classified in groups based on caloric content and common food classifications. Physical activity comprises binary and hours per week indicators for weekly exercise and sports activities.

2.4 Methodology

The main sample consists of adults that are interviewed on all three waves and are 18 years or older at the time of their first interview. Since the exact timing of military interventions is ignored, operations are assigned by year of interview. My methodology consists of a generalized difference-in-differences approach taking advantage of the staggered nature of eradication and interception operations across states and years.[16] The main specification can be written in the following form:

$$y_{ist} = \alpha + \Pi M_{st} + X'_{ist}\beta + +\gamma_t + \gamma_s + \gamma_i + \varepsilon_{ist} \qquad (2)$$

where y_{ist} is the outcome variable for individual i in state s and year t, M_{st} are the number of military operations in state s and year t, X'_{ist} is a vector of demographic control variables, γ_t, γ_s and γ_i are year, state and individual fixed effects, respectively, α is the regression intercept, and

[16] Since the conflict started in December of 2006, the three waves from MxFLS provide information for two pre-treatment years (2002, 2005) and 5 post-treatment years (2006, 2009, 2010, 2011, 2012).

ε_{ist} represents the idiosyncratic error term. Individual fixed effects, γ_i, are essential as these account for time-invariant unobservables related to lifestyles, genetics, and preferences.[17] M_{st} corresponds to the sum of interceptions and eradications operations. I also generate estimates using both counts in the same specification under the caveat of collinearity. Since time fixed effects capture trends at the national level, I alternatively include region by year fixed effects to allow for different trends subnationally.[18] Since some individuals may move to safer locations in response to street violence, to control for selective migration I use the location where the individual lived during the first wave of MxFLS to assign exposure. I cluster errors at the state level to correct for the loss of independent variation within the states. If government interventions conditional on controls are exogenous, the following condition will hold:

$$E[\varepsilon_{ist}|s,t,i,X_{ist}] = 0. \qquad (3)$$

Alternatively, since weight related outcomes may not respond immediately, I use military operations from the previous year. Using the previous year operations could also improve the identification as the exact timing of operations within a year is unknown. Along the same lines, I study if the effect from cumulative operations using the sum of contemporaneous and previous years' operations. Furthermore, to study the dynamics associated with the impact of military operations, I implement distributed lag models including the five previous and subsequent years separately.

A latent concern from studying the relationship between street violence and obesity is endogeneity, particularly that arising from residential self-selection.[19] In the case of military

[17] Given the inclusion of individual fixed effects, state fixed effects are meaningful to the extent that individuals move across states. For specifications analyzing outcomes related to food consumption i refers to the household.
[18] The regional division used originates in the 2006 Mexican National Development Plan and was the one considered by MxFLS to do the sampling process. States belonging to each region are presented in Table 2.8.
[19] Residential self-selection in this context refers to the idea that people are not randomly assigned but choose where to live based on their preferences. Such endogeneity concerns may be more salient in other street violence measures like homicide or property crime rates.

operations this becomes an identification threat if people under socioeconomic conditions or preferences that promote obesity are constraint or prefer living in neighborhoods that are targeted more heavily by the military. In turn, my results could be driven by trends in state outcomes that are correlated with the evolution of the military interventions. Thus, I evaluate this proposition formally in an event study analysis. Specifically, I fit the following equation:

$$y_{sti} = \alpha + \sum_{j=-N}^{N} \pi_j 1(\tau_{st} = j) + \gamma_i + \lambda_s + \gamma_t + \beta X_{sti} + \epsilon_{sti} \tag{4}$$

where τ_{st} denotes the event year, defined so that $\tau_{st} = 0$ if the outcome at state s and year t corresponds to the first year s was intervened by the military, $\tau = 1$ if the outcome corresponds to one year after the first year s was intervened, and so on. Outcomes corresponding to $\tau \leq -1$ pertain to years prior to an intervention in the corresponding state. Coefficients are measured relative to $\tau = -1$. I consider a window of five years before and five years after the first intervention, N=5. The outermost indicators include all previous or subsequent periods beyond five years respectively. If military operations are exogenous, coefficients corresponding to $\tau \geq 0$ should be statistically different from zero, and coefficients corresponding to $\tau \leq -1$ close to zero.

Additionally, to judge if the effect found from military operations is rather due to variations in homicide rates, I provide estimates using municipal homicide rates rates (per 10,000) as controls for military operations but also as the main source of variation. I run specifications using either annual or monthly municipal homicide rates (per 10,000), the latter of which can be paired to the month of interview.[20]

[20] Homicide rates are assigned using MxFLS-1 as the exposure location, assuming that variations in homicide rates are unrelated to residence choices prior to the conflict.

2.5 Results

In this section I present the results from the empirical analysis, results on continuous outcomes correspond to ordinary least squares while results on binary outcomes correspond to linear probability models. The base specification (1) includes individual, year and state fixed effects.[21] Specification (2) adds demographics.[22] Specification (3) includes region by year fixed effects. Specification (4) alternatively uses the current residence instead of that corresponding to MXFLS-1 to assign treatment. Overall, controlling for selective migration seems to have little impact on estimates which can be explained by a small fraction of people migrating out of their initial location.[23] Estimates represent the effect of an additional military operation. As reference and to interpret estimates, the average number of military operations per year and state is of around 2.5 operations, with some states having consecutively more than 5 operations on a yearly basis.

Table 2.3 presents results from regressions of weight related outcomes on the number of operations. As it can be observed these results point to an increase in BMI as a result of military operations which is mainly resulting in shifts away from normal weight and into overweight although not into obesity.[24] Given that underweight does not vary in response to military operations within individuals, results for underweight are not presented. Estimates indicate that on average an additional operation increases BMI by 25 up to 76 g (grams). I interpret BMI coefficients in terms of grams (g) considering the average weight and height from the sample

[21] Although Mexico is not known for having extremely cold weather, I check for cyclicality concerns by running specifications using month indicators, which produce little changes in estimates.
[22] Demographics include age, marital status, years of education, employment status, household size, household earnings per month, and rural status.
[23] Roughly 300 out of 7,500 individuals in the sample migrated to a different state during the period of analysis. Specifications removing individuals who migrated out of the state they lived in 2002 show equivalent results.
[24] Estimates including only individual fixed effects point into larger impacts on weight gain, however, not including year and state fixed effects in this context may fail to capture important unobservables.

used are 70 kilograms (kg) and 1.6 meters (m). Similarly, an additional operation increases the probability of overweight by 0.3 up to 0.5% and reduces the probability of normal weight by 0.2 up to 0.4%. Assessing results for different groups suggests effects are positively larger among females, family dependents, younger, more educated, and middle-income individuals, which is in line with literature that shows effects of stress on weight differ based on age, gender and genetics (Foss et al., 2011).

Figures 2.3 to 2.6 show the results from the event study analysis. These graphs permit to test for pre-trends effects that would raise concerns about the validity of the identification strategy. In all these graphs, the value at time -1 represents a reference category set to zero that corresponds to a year prior to the event. The corresponding estimates suggest the hypothesis of military operations being exogenous cannot be rejected as there are no discernable pre-trends. The graphs also inform about year specific effects of military operations on outcomes considered. In the case of weigh related outcomes such results point that the impact arising from military operations could be compounding over the years. This is plausible as stressors could continue to have an effect years after exposure. To further explore this possibility, I use specifications using former years operations. Using operations from the previous year show slightly larger impacts. Using cumulative operations, which includes the sum of the contemporaneous and the last five years operations, point into similar conclusions. Implementing distributed lag models supports the event study results as operations from the same year and lags affect outcomes contrary to leads.

Since, there could be factors associated to states that both explain the military strategy and weight related outcomes, I run separate regressions dropping outlier states based on geographical size, population, gross domestic product per capita, and closeness to the border.

This analysis shows that the impact is higher in states bordering the US and those with smaller areas, but differences in population size and GDP do not seem to drive results. Given that more operations could reflect increased efforts to cover a larger ground and smaller states increase chances of exposure I alternatively adjust the number of military operations by the size of the state in square kilometers. Adjusting by size shows consistently similar results. Specifications including eradications and interceptions as separate counts are consistent with the main results although significance is lower which is occurs as the two counts are collinear.

Table 2.4 to Table 2.7 present results on potential mechanisms. Table 2.4 presents results on safety perceptions. As it can be observed from this table, people appear to be more afraid of being attacked or assaulted during day as well as during night, although effects during day are larger. Similarly, people report to go out at night less frequently. Although people report to feel less safe compared to five years ago such results are not significant across specifications. Table 2.5 presents results for depression symptoms. These results indicate that military operations increase the depression score on average and move individuals away from the range associated with no depression. Although effects are small, they suggest the prevalence of mild and moderate depression, although not that of severe depression, increase in response to military operations.

Table 2.6 shows results using consumption outcomes. Unadjusted food expenditures show mixed evidence, however, using food expenditures adjusted by income indicate that expenditures on food overall increase indicating that diminishes in consumption could be resulting from adverse economic conditions in line with former literature. All such estimates are, however, insignificant. Regarding compositional changes, out of home food expenses appear to increase proportionally more than home food expenditures, moreover, within home food expenditures, classifying them by caloric content shows that high caloric items increase more than low caloric items.

Table 2.5 shows that doing exercise as well as time doing exercise responds negatively, implying. Doing sports activities is unaffected although time doing sports activities does increase. The latter is consistent with literature pointing that stress and exercise activity are positively related among people already in the habit of exercise as its used as a way to cope with stress (Stults-Kolehmainen et al., 2014). Although estimates on physical activity are insignificant this may be reasonable. These results, however, cannot discard that physical activity associated with working, commuting, entertainment or other activities changed. This is suggested from results on Table 2.4 which show that in response to military operations people reported going out at night less frequently.

Controlling for homicide rates in addition to military operations as well as using them instead of military operations yield similar results. The effects from homicide rates, however, seem to hold only in the short term. Monthly homicide rates affect outcomes measured in the same month of interview, but annual homicide rates do not. Although variations in homicide rates are a direct outcome from the military operations these specifications help assessing if the contribution of military operations is meaningful or is just serving as a proxy for homicide rates. These estimates, however, may not be interpret as causal.

2.6 Discussion

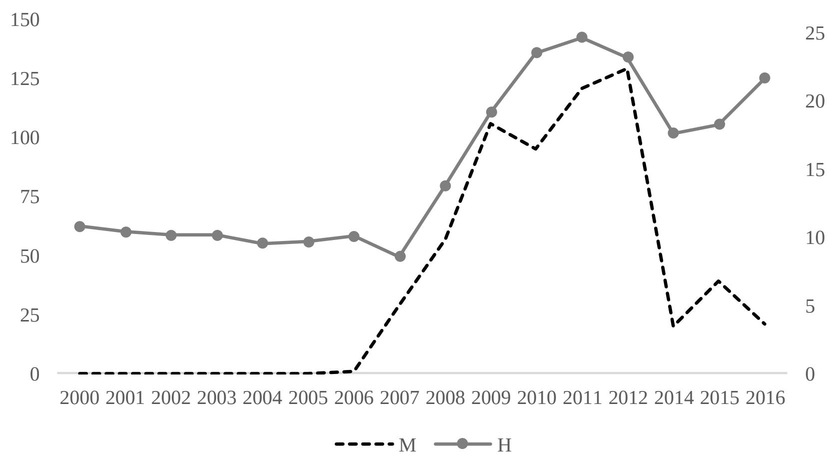
Overall results from this paper indicate that the military interventions that set off the “war on drugs” have a nontrivial impact on weight related outcomes. Although the conflict did not seem to affect the incidence of obesity, the increase in overweight could translate into future health issues affecting health capital. Some health problems linked to overweight and obesity are type two diabetes, high blood pressure, heart disease, certain cancers and pregnancy problems.

Although risks in terms of developing health problems are more salient among people with obesity, overweight poses similar threats. Since treating chronic health conditions linked to overweight and obesity pose large costs in terms of medical care spending, my findings inform policy makers in Mexico of potential subsequent health costs associated to the military conflict. The findings of this paper provide causal evidence that street violence does impact weight related outcomes providing support to the growing literature on the subject. Furthermore, analyzing potential mechanisms this work provides a better understanding of this relationship. Specifically, I observe an increase in the incidence of depression, particularly mild depression which is consistent with the medical literature relating weight gain to stress and depression. Exploring other potential mechanisms suggest weight changes are the result of changes in consumption patterns and physical activity although these results are not significant indicating metabolic and somatic responses to stress might play a rather important role driving this relationship.

Figures

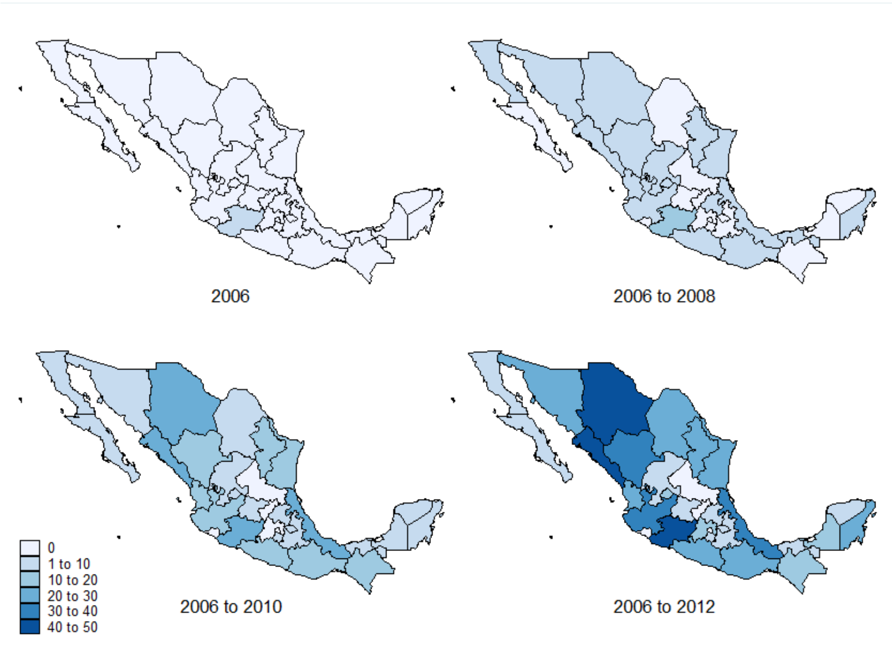
Figure 2.1. Military operations and homicides

Number of military operations and homicides (per 10,000)



Note: M refers to number of military operations and H refers to the homicide rate per 10,000 population.

Figure 2.2. Number of Military Operations



Note: Darker regions indicate a larger number of military operations.

Figure 2.3. Effect of military interventions on BMI, event study

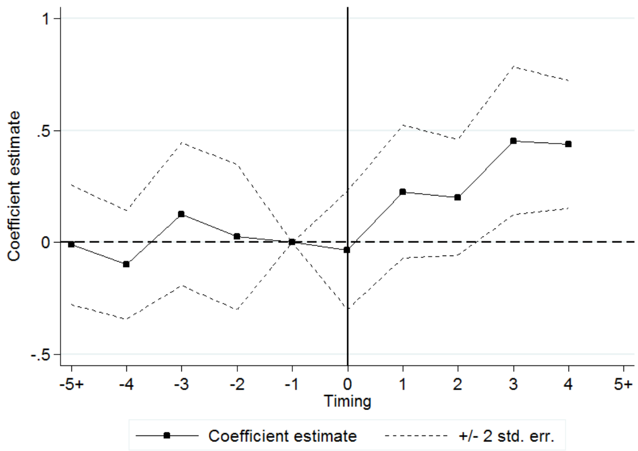


Figure 2.4. Effect of military interventions on overweight, event study

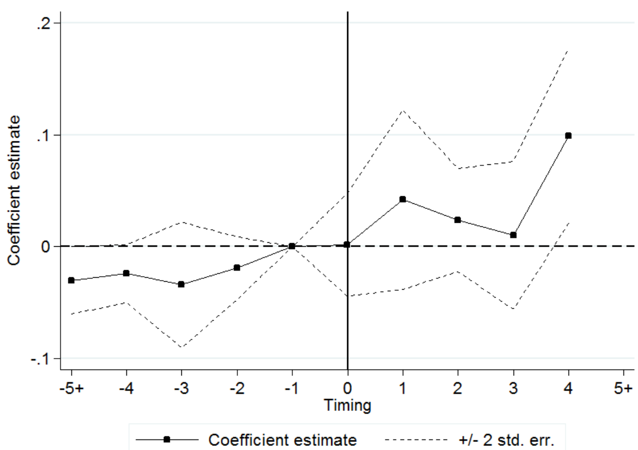


Figure 2.5. Effect of military interventions on obesity, event study

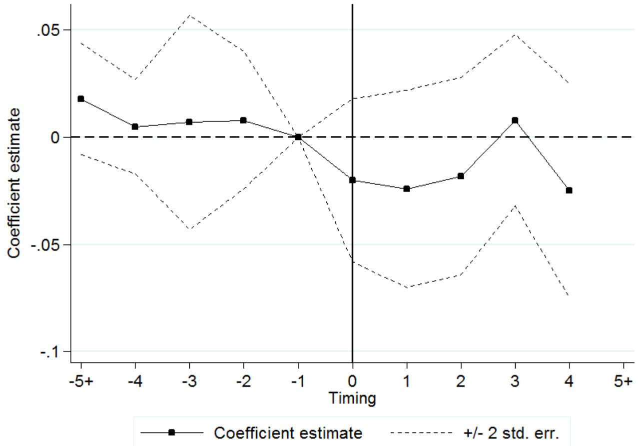
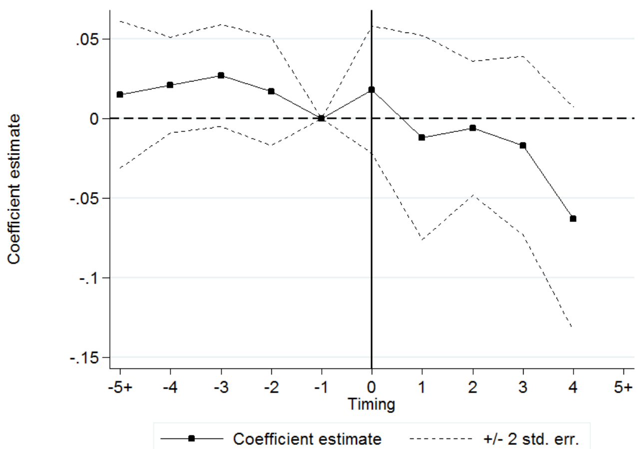


Figure 2.6. Effect of military interventions on normal weight, event study



Tables

Table 2.1. Descriptive statistics, main variables

	N. Obs.	Mean	Std.	Min.	Max.
Weight					
Body Mass Index (BMI)	22,443	27.86	5.28	12	66
Underweight (BMI<18.5)	22,443	0.02	0.13	0	1
Normal weight (18.5≤BMI<25)	22,443	0.29	0.45	0	1
Overweight (25≤BMI<30)	22,443	0.39	0.49	0	1
Obesity (30≤BMI)	22,443	0.31	0.46	0	1
Safety perceptions (ext. margins)					
Fear of assault at day time	21,445	0.17	0.37	0	1
Fear of assault at night time	21,445	0.21	0.41	0	1
Going out at night frequently	21,440	0.12	0.33	0	1
Safety vs. 5 years ago	21,445	0.86	0.35	0	1
Safety measure: changing transportation	21,445	0.07	0.25	0	1
Safety measure: switching routes	21,443	0.08	0.28	0	1
Safety perceptions (int. margins)					
Fear of assault at day time	21,445	1.55	0.88	1	4
Fear of assault at night time	21,445	1.67	0.98	1	4
Going out at night frequently	21,440	1.61	0.79	1	4
Safety vs. 5 years ago	21,445	1.85	0.64	1	3
Demographic controls					
Age	22,443	42.88	16.06	15	97
Lives in Rural Locality	22,443	0.47	0.5	0	1
Years of Education	22,443	9.72	4.56	0	23
Employed	21,463	0.56	0.5	0	1
Earnings per month (pesos)	19,284	1,569.82	5,546.59	1	333,333
Married	22,443	0.74	0.44	0	1
Household size	22,443	4.5	2.06	1	15

Notes: ext. stands for extensive margin and int. for intensive margin.

Table 2.2. Descriptive statistics, secondary variables

	N. Obs.	Mean	Std.	Min.	Max.
Depression					
Depression score (DS)	21,452	36.09	15.78	20	81
No depression (DS<36)	21,452	0.59	0.49	0	1
Mild depression (36≤DS<46)	21,452	0.15	0.36	0	1
Moderate depression (46≤DS<66)	21,452	0.2	0.4	0	1
Severe depression (66≤DS)	21,452	0.06	0.24	0	1
Self-reported health (ext. margins)					
Health status	21,465	0.95	0.23	0	1
Health status vs. one year ago	21,465	0.95	0.23	0	1
Health status for next year	21,465	0.95	0.23	0	1
Health status vs. same gender individuals	21,465	0.95	0.23	0	1
Health status (int. margins)					
Health status	21,465	3.48	0.69	1	5
Health status vs. one year ago	21,446	3.12	0.66	1	5
Health status for next year	21,456	3.3	0.66	1	5
Health status vs. same gender individuals	21,457	3.3	0.68	1	5
Risky behaviors (ext. margins)					
Alcohol	21,546	0.35	0.48	0	1
Smoking	21,470	0.15	0.35	0	1
Soft drinks	21,546	0.81	0.39	0	1
Risky behaviors (imt. margins)					
Smoking (weekly number of cigarettes)	21,470	4.24	20.75	0	420
Physical activity					
Exercise (ext. margin)	21,470	0.15	0.36	0	1
Exercise (daily hours)	21,470	0.24	0.9	0	20
Exercise (weekly days)	21,470	0.58	1.47	0	5
Exercise (implied weekly hours)	21,470	0.96	3.97	0	80
Sports (ext. margin)	21,467	0.1	0.3	0	1
Sports (weekly hours)	21,467	0.62	2.86	0	84
Household food expenditures (pesos)					
Home food	13,098	609.22	1064.29	0	71,587
High calories items	13,102	331.46	492.37	0	21,213
Low calories items	13,100	277.64	743.71	0	50,374
Out of home food	12,963	33.56	133.37	0	7,500
Household food expenditures (% of income)					
Home food	10,947	47.41	833.61	0	82,000
High calories items	10,950	25.65	530.05	0	53,100
Low calories items	10,948	21.75	311.48	0	28,900
Out of home food	10,839	2.66	109.58	0	10,000

Notes: ext. stands for extensive margin and int. for intensive margin.

Table 2.3. Military operations and weight related outcomes

	(1)	(2)	(3)	(4)	(5)
BMI	0.130***	0.010	0.021**	0.030**	0.027**
	(0.016)	(0.009)	(0.010)	(0.012)	(0.012)
Dependent mean	27.89	27.89	27.89	27.89	27.89
Observations	21475	21475	18513	18513	18504
Overweight	0.003***	0.003*	0.004**	0.005***	0.004***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Dependent mean	0.39	0.39	0.39	0.39	0.39
Observations	21475	21475	18513	18513	18504
Obesity	0.008***	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Dependent mean	0.31	0.31	0.31	0.31	0.31
Observations	21475	21475	18513	18513	18504
Normal weight	-0.010***	-0.002*	-0.003	-0.004**	-0.004**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Dependent mean	0.29	0.29	0.29	0.29	0.29
Observations	21475	21475	18513	18513	18504
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Region by Year FE	No	No	No	Yes	Yes
MxFLS1 Used as Exposure Location	Yes	Yes	Yes	Yes	No

Note: All specifications include individual fixed effects. Demographics include age, marriage status, education level, working status, household size, household monthly income. Errors are clustered at the state level. Standard errors are displayed in parenthesis. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 2.4. Military operations and fear of attack or assault

	(1)	(2)	(3)	(4)	(5)
Fear of being attacked during day	0.006**	0.007*	0.007**	0.005**	0.005**
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Dependent mean	0.17	0.17	0.17	0.17	0.17
Observations	21449	21449	18491	18491	18486
Fear of being attacked during night	0.004	0.006*	0.006	0.005*	0.004
	(0.002)	(0.004)	(0.004)	(0.003)	(0.003)
Dependent mean	0.21	0.21	0.21	0.21	0.21
Observations	21449	21449	18491	18491	18486
Safety compared to five years ago	-0.014**	-0.005	-0.005	-0.003	-0.003
	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Dependent mean	0.71	0.71	0.71	0.71	0.71
Observations	21449	21449	18491	18491	18486
Going out at night frequently	-0.001	-0.004*	-0.005**	-0.006***	-0.006***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Dependent mean	0.12	0.12	0.13	0.13	0.13
Observations	21444	21444	18487	18487	18482
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Region by Year FE	No	No	No	Yes	Yes
MxFLS1 Used as Exposure Location	Yes	Yes	Yes	Yes	No

Note: All specifications include individual fixed effects. Demographics include age, marriage status, education level, working status, household size, household monthly income. Errors are clustered at the state level. Standard errors are displayed in parenthesis. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 2.5. Military operations and depression symptoms

	(1)	(2)	(3)	(4)	(5)
Depression score	-0.048	0.234**	0.209**	0.258***	0.251***
	(0.063)	(0.085)	(0.095)	(0.061)	(0.061)
Dependent mean	36.08	36.08	35.75	35.75	35.75
Observations	21365	21365	18420	18420	18415
Mild depression	-0.000	0.003*	0.001	0.002	0.002
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Dependent mean	0.15	0.15	0.15	0.15	0.15
Observations	21365	21365	18420	18420	18415
Moderate depression	-0.001	0.002	0.002	0.003*	0.003
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Dependent mean	0.20	0.20	0.19	0.19	0.19
Observations	21365	21365	18420	18420	18415
Severe depression	-0.000	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Dependent mean	0.06	0.06	0.06	0.06	0.06
Observations	21365	21365	18420	18420	18415
No depression	0.002	-0.006**	-0.004*	-0.005***	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Dependent mean	0.59	0.59	0.60	0.60	0.60
Observations	21365	21365	18420	18420	18415
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Region by Year FE	No	No	No	Yes	Yes
MxFLS1 Used as Exposure Location	Yes	Yes	Yes	Yes	No

Note: All specifications include individual fixed effects. Demographics include age, marriage status, education level, working status, household size, household monthly income. Errors are clustered at the state level. Standard errors are displayed in parenthesis. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 2.6. Military operations and food expenses

	(1)	(2)	(3)	(4)	(5)
Home food	5.079	4.411	5.513	0.822	0.777
	(5.226)	(5.569)	(6.268)	(2.177)	(2.276)
Dependent mean	47.73	47.73	47.78	47.78	47.78
Observations	10800	10800	10782	10782	10775
High calories items	3.271	3.028	3.752	0.714	0.766
	(3.266)	(3.420)	(3.895)	(1.192)	(1.217)
Dependent mean	25.83	25.83	25.86	25.86	25.85
Observations	10802	10802	10784	10784	10777
Low calories items	1.808	1.383	1.761	0.108	0.011
	(1.989)	(2.178)	(2.403)	(1.026)	(1.094)
Dependent mean	21.90	21.90	21.92	21.92	21.92
Observations	10801	10801	10783	10783	10776
Out of home food	0.583	1.028	1.146	0.340	0.363
	(0.576)	(0.783)	(0.848)	(0.237)	(0.243)
Dependent mean	2.68	2.68	2.68	2.68	2.69
Observations	10695	10695	10677	10677	10670
Individual FE	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Region by Year FE	No	No	No	Yes	Yes
MxFLS1 Used as Exposure Location	Yes	Yes	Yes	Yes	No

Note: All specifications include individual fixed effects. Demographics include age, marriage status, education level, working status, household size, household monthly income. Errors are clustered at the state level. Standard errors are displayed in parenthesis. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 2.7. Military operations and physical activity

	(1)	(2)	(3)	(4)	(5)
Exercise	-0.000	-0.002	-0.001	-0.001	-0.001
	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
Dependent mean	0.15	0.15	0.15	0.15	0.15
Observations	21383	21383	18433	18433	18428
Exercise (weekly hours)	-0.039*	-0.034	-0.037	-0.013	-0.015
	(0.020)	(0.028)	(0.030)	(0.020)	(0.020)
Dependent mean	0.95	0.95	0.99	0.99	0.99
Observations	21383	21383	18433	18433	18428
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Region by Year FE	No	No	No	Yes	Yes
MxFLS1 Used as Exposure Location	Yes	Yes	Yes	Yes	No

Note: All specifications include individual fixed effects. Demographics include age, marriage status, education level, working status, household size, household monthly income. Errors are clustered at the state level. Standard errors are displayed in parenthesis. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 2.8. Geographic regions

Region	States
South-Southeast	Campeche, Yucatán, Chiapas, Oaxaca, Quintana Roo, Tabasco, Guerrero, and Veracruz.
Center-west	Jalisco, Michoacán, Colima, Aguascalientes, Nayarit, Zacatecas, San Luis Potosí and Guanajuato.
Center	Ciudad de Mexico, Querétaro, Hidalgo, Tlaxcala, Puebla, Morelos and Estado de México.
Northeast	Tamaulipas, Nuevo León, Coahuila, Chihuahua y Durango.
Northwest	Baja California, Baja California Sur, Sonora and Sinaloa.

Note: This division originates in the 2006 Mexican National Development Plan, is intended to help coordinating national projects and was the one considered by MxFLS to do the sampling process. According to such division some states can belong to two regions: Puebla can belong to either the Center or South-Southeast, Chihuahua and Durango to the Northeast or Northwest, and Queretaro to the Center or Centerwest region. To avoid overlapping regions when creating Region by Year fixed effects either possibility is considered in alternative cases like the one outlined in this table. Source: Mexican Family Life Survey Users Guide and 2006 Mexican National Development Plan.

Table 2.9. Food expenditures categories

Categories	MxFLS survey items included
Home food	
Low caloric	
Fruits/Vegetables (non-starchy)	Bananas, apples, oranges, other fruits, onions, red tomatoes, chiles, and other vegetables.
Poultry and seafood	Chicken, chicken eggs, tuna/sardines, and fish/seafood.
Unrefined carbohydrates	Potatoes, milk, legumes, beans, rice, and other cereals.
High caloric	
Refined carbohydrates	Corn tortillas, bread/baguette, soup/pasta, cheese, and other dairy products.
Red meat	Beef, pork, and other animal products.
Fats and oils	Vegetable oil.
High in sugar and fat	Sodas; beverage juices, purified water, beverages as beer tequila rum and powder for preparing water; cookies; white sugar; coffee; other industrial/packaged products like pancakes, candies, potato chips, etc.
Out of home food	Meals outside of the household.

Note: Caloric content was determined based on Chávez et al. (2010).

Table 2.10. Variables description

Name	Description
Military eradications	Military operations targeting drug fields. Source: SEDENA.
Military interceptions	Military operations targeting drug-related traffic through land vehicle checkpoints and surveillance stations that focus on air and water routes. Source: SEDENA.
Homicide rates	Number of homicides adjusted by population size multiplied by 10,000. Source: INEGI.
Body Mass Index (BMI)	BMI is obtained by dividing individual measures of individual weight by the square of the corresponding height, where weight is expressed in kilograms and height in meters. Source: MxFLS.
Overweight ($30>BMI\geq25$)	Overweight is an individual measure taking the value of one if BMI is greater or equal to 25 and less than 30, and zero otherwise. Source: MxFLS.
Obesity ($BMI\geq30$)	Overweight is an individual measure taking the value of one if BMI is greater or equal to 30 and zero otherwise. Source: MxFLS.
High caloric food expenses	High caloric food expenses consist of household purchases of high calorically dense food items adjusted by number of household members.[a] Source: MxFLS.
Low caloric food expenses	Low caloric food expenses consist of household purchases of low calorically dense food items adjusted by number of household members.[a] Source: MxFLS.
Outside of home meals expenses	Outside of home meals expenses consists of household purchases of meals out of home adjusted by number of household members.[a] Source: MxFLS.
Exercise activity	Exercise activity refers to an individual binary indicator that takes the value of one if the individual reports doing exercise on the week before the interview took place and zero otherwise. Source: MxFLS.

[a] Table 2.10 lists food items available from MxFLS grouped by each category considered. Household expenses refer to purchases performed on different specific food items the week before the interview took place. For cases where last week purchases are not available purchases from last month are used.

3 Will Violent Crime Incentivize the Hiding of Small Firms?

3.1 Introduction

Does exposure to violent crime increase firm informality? While arguably an important question, to my knowledge it has not been yet addressed. This is remarkable given that in many developing countries the size of the underground economy is rather large and as such constitutes a pressing problem. As an example, underground economy represents around forty-five percent of the gross domestic product and more than seventy percent of the labor force in Latin America (Loayza et al., 2009). It is clear that a better understanding of key informality drivers is needed in order to better design policies to deal with this issue.

In this research I focus on the case of Mexico and take advantage of its notorious increase in violent crime in the context of the country's "war on drugs," a conflict between the government and the drug trafficking organizations that began in 2006 and created large variations in violence intensity. In addition, to address potential endogeneity concerns due to self-selection I rely on the literature that links weather and violent crime (Carleton and Hsiang, 2016). I employ temperature as an instrument that adequately complies with the exclusion restriction by focusing on non-agricultural firms and accounting for seasonal variations.

Interestingly, any observed causal link from violent crime exposure to increases in informality might signify that firms willing to become formal are in fact discouraged to do so by reasons that are beyond the typical determinants of informality currently in the literature, such as tax and regulation burden, financial market development, and the quality of the legal system (Inchauste et al., 2005). Becoming or staying informal to these firms can be costly in both additional effort and loss of resources in order to go underground as well as to avoid being uncovered and singled out by criminals. In fact, firm profits in Mexico have decreased during the war on drugs period, which is directly attributed to violent crime. For instance, this is the case of local businesses such as gas stations, drugstores, and professional offices, including medical doctors and lawyers, as they tend to shorten operating hours in order to reduce exposure to violence[25]. Similarly, the tourism industry, which is particularly large in the country, has shown a drastic decline in activity in locations known to be violent as well as in locations linked to roads where criminal activity is known to occur[26]. Furthermore, the fact that criminal organizations tend to charge firms with quotas or implicit taxes only tends to compound this problem[27].

The paper is organized as follows. The next section describes the data. Section 3 presents the empirical methodology. Section 4 presents the main findings. Section 5 concludes.

3.2 Data

The data come from three main publicly available sources. First, I draw quarterly individual information on businesses, including informality, from the National Survey on Employment and Occupation (ENOE), which is the primary source of labor force statistics in the country. It is a rotating panel survey where individuals are interviewed for five consecutive quarters and then replaced. Second, I collect data on homicide rates by quarter and municipality from official reports provided by the National Statistical Agency (INEGI). Finally, I use temperature averages by quarter and state from the National Water Commission (CONAGUA). Matching all available data allows us to study the period from 2005 to 2016.

The definition of informality employed is the same used by INEGI. Informal businesses are those firms owned by household's members that are not constituted as separate legal entities, lack complete accounts that permit a financial separation of their production activities, and/or are not registered under specific forms of national legislation, including tax or social security laws.

[25] http://www.excelsior.com.mx/2012/08/13/nacional/853135
[26] http://archivo.eluniversal.com.mx/estados/85953.html.
[27] https://www.proceso.com.mx/290237/para-pagar-cuota-al-narco-suben-kilo-de-tortilla-en-michoacan

The details on how the informality variable is constructed as well as the rest of variables in this paper is provided in Table 3.1.

3.3 Empirical Strategy

I focus on employers and self-employed individuals from the private sector who are sampled for the full five quarters of the corresponding rotating panel using information related to their main job[28]. My sample excludes those employed in the agriculture sector and those who are domestic employees. I focus on actively working individuals, as I am interested in tax registration shifts of ongoing businesses[29]. My set of controls includes demographic controls such as age, sex, years of education, as well as income level. In addition, I include municipal, quarter, and year fixed effects. The dependent variable is a dummy that reflects the informal status of the firm. As such, I employ a linear probability approach with the following reduced form:

$$y_{mitq} = \beta_0 + \beta_1 x_{mtq} + \gamma_m + \eta_t + \eta_q + \epsilon_{mitq}, \tag{1}$$

where y_{mitq} is the informality status associated with firm i in municipality m at year t in quarter q, x_{mtq} is the homicide rate (per 10,000) in municipality m at year t and quarter q, η_t stands for year fixed effects and η_q stands for quarter fixed effects and ϵ_{mitq} is the error term. The informality status takes a value of one if the firm is regarded as informal and zero otherwise. I use homicide rates per 10,000 as the explanatory variable. In addition, I correct for the lack of independent variation in homicide rates within municipalities clustering errors at this level. I alternatively include state-year fixed effects to allow for different trends at the subnational level and individual fixed effects to control for individual heterogeneity.

[28] By doing this, I exclude heads of firms that were unemployed, turned into employees or for some reason, were not interviewed.

[29] ENOE follows heads of firms, not firms themselves.

I further explore endogeneity concerns by instrumenting homicide rates using temperature, where the exclusion restriction assumes that variations in temperature do not directly affect the incentives to switch registration status after controlling for time fixed effects that account for seasonal and annual variations in weather. As outlined above, individuals pertaining to the agriculture sector are excluded from the analysis, further ensuring the instrument validity. This identification strategy also relies on the assumption that quarterly variations in weather changes on average do not differ significantly across municipalities within a state in a quarter.

3.4. Findings

The main results correspond to the sample consisting of business heads, i.e. employers and self-employed individuals observed for the full five quarters of the rotating panel. The first three columns in Table 3.2 show the linear probability model (LPM) results, which point that on average an additional homicide per 10,000 people each quarter increases the probability to be informal by in between 0.6 percent and 1.1 percent[30]. The last three columns in Table 3.2 show the results from the second stage least squares from the instrumental variables (IV) approach using temperature as an instrument, which indicate that such change corresponds to an increase between 13.7 percent and 17.3 percent in the probability to be informal. Common to IV findings, these estimations yield larger coefficients, which is likely the result of the fact that they measure LATE impacts. All results presented include sampling weights.[31]

Table 3.3 shows that my estimates are robust to changes in the way I treat formality and informality. In particular, I explore whether results are sensitive to dropping firms that show

[30] Probits yield virtually identical results.

[31] Specifications without sampling weights display similar results although the coefficients are slightly smaller. Since ENOE does not sample all municipalities each quarter, using sampling weights accounts for underrepresentation that may miss to capture variations in homicides.

consecutive changes in registration status during any of the five quarters that the firm is followed in the rotating panel. Since switches from and to formal registration status of a firm are not costless, one might argue that these quarterly changes could be due to potential misclassifications[32]. Panel A in Table 3.3 excludes firms showing at most one formal-informal registration switch in any of the five quarters of the rotating panel. Similarly, Panel B in the same table excludes firms showing one or more of such formal-informal registration switches. I observe that the findings in Table 3.3 are analogous to the one presented in Table 3.2[33].

3.5. Conclusions

I find that exposure to violent crime promotes informality. On average an additional homicide per 10,000 people each quarter increases the probability to be informal between 0.6 percentage points and 1.1 percentage points. These results are further corroborated when using an instrumental variables approach that is consistent with the exclusion restriction, which indicates that these estimates range between 13.7 percentage points and 17.3 percentage points.

It appears that exposure to violent crime increases the opportunity costs of informality as losses and spending on security measures may otherwise be used to afford formality. An additional mechanism at work may be fear. Owners may prefer to stay underground or reduce working hours in order to reduce exposure to violence. Finally, these findings help inform policymakers on the potential benefits that reducing violent crime can have in terms of incorporating firms into the regulated sector and thus increasing the tax base.

[32] Becoming informal restricts business networks due to unavailability of invoices and access to the financial system. Becoming formal, besides the paper work and tax payments implied, may be a risky move as it discloses the firm's existence.

[33] Results are robust to including those that are interviewed less than five times in the rotating panel. In addition, I also pursue additional robustness tests by employing alternative definitions of informality different from the official ones employed by INEGI. When using a definition based social security registration status, I find slightly less statistically significant results, which is reasonable, as firm owners do not necessarily act as workers in their own business. These findings are available upon request. Including individual fixed effects shows similar results for the LPM but the IV approach is not significant.

Table 3.1 Description of Variables

Variable	Definition
Firm Informality	Informality status based on ENOE's questions 4C-4D, 4E, and 4G as defined by INEGI. 4C and 4D classify a firm by means of its official name. 4E is "business or activity, a) has an establishment and office, b) has only an office, c) has only an establishment, d) does not have an office or establishment." 4G is "In this business or activity, a) do you use the services of an accountant to keep records? b) do you only use a notebook or write personal notes to keep accounting records? c) do you use an income booklet or do you have a cash register from the Ministry of Finance for small taxpayers? d) you do not keep any accounting records." If 4C and 4D identify the business as complex, informality equals 0. If not complex and 4E is a), b) or c), the value depends on 4G: informality is 1 if 4G is b) or d) and is 0 if it is a) or c). If not complex but 4E is d) then informality is 1. Source: ENOE (2005-2016).
Worker Informality	Informality based on social security status using question Q6D from ENOE: "Due to this job, do you have access to health services through: a) IMSS, b) ISSSTE, c) state ISSSTE, d) other institution, e) none." Variable equals one if response is (a) and 0 otherwise. IMSS refers to social security of private sector; ISSSTE refers to that of public sector. Source: ENOE (2005-2016).
Age	Age in years. Source: ENOE (2005-2016)
Sex	Binary variable taking the value of 1 if female and 0 if male. Source: ENOE from 2005 to 2016.
Education	Years of education based on the last academic degree reported by the individual. Source: ENOE from 2005 to 2016.
Income	Reported monthly income. Source: ENOE from 2005 to 2016.
Homicide rate	Number of homicides per quarter and municipality divided by population size multiplied by 10,000. Source: INEGI (2005, 2016) and CONAPO (2005, 2010).
Temperature	State and quarterly average temperature in Celsius. Source: NWC (2005, 2016)

Table 3.2. Violent Crime and Informality

	LPM			IV 2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Homicide rate	0.011***	0.009***	0.006***	0.156***	0.137***	0.173*
	(0.003)	(0.002)	(0.002)	(0.051)	(0.044)	(0.102)
Year FE	X	X	X	X	X	X
Quarter FE	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X
Demographics		X	X		X	X
State by year FE			X			X
Weak instruments KP Statistic	n.a.	n.a.	n.a.	41.08	41.02	16.95
10% maximal size critical value	n.a.	n.a.	n.a.	16.38	16.38	16.38
Observations	482761	482640	482640	482761	482640	482640

CEOs surveyed at least one time. Sample period corresponds to 2005-2016 and frequency of all variables is quarterly. Informality data corresponds to the individual level, homicides to the municipal level and weather data to the state level. Homicide rates are in per capita terms (per 10,000). Each column represents a different regression. Dependent variable mean is 0.64. Standard errors in parenthesis. All specifications include sampling weights. Demographic controls include age, sex, years of education and income level. Errors are clustered at the municipal level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 3.3. Robustness Tests

	LPM			IV 2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Homicide rate	0.010***	0.009***	0.006**	0.155***	0.139***	0.188*
	(0.002)	(0.002)	(0.002)	(0.054)	(0.047)	(0.110)
Weak instruments KP Statistic	n.a.	n.a.	n.a.	36.15	36.09	16.43
10% maximal size critical value	n.a.	n.a.	n.a.	16.38	16.38	16.38
Observations	391901	391806	391806	391901	391806	391806
Panel B						
Homicide rate	0.011***	0.010***	0.005**	0.156***	0.141***	0.178
	(0.003)	(0.002)	(0.002)	(0.055)	(0.048)	(0.113)
Dep. Variable mean	0.68	0.68	0.68	0.68	0.68	0.68
Weak instruments KP Statistic	n.a.	n.a.	n.a.	37.32	37.27	15.36
10% maximal size critical value	n.a.	n.a.	n.a.	16.38	16.38	16.38
Observations	374462	374372	374372	374462	374372	374372
Year FE	X	X	X	X	X	X
Quarter FE	X	X	X	X	X	X
Municipality FE	X	X	X	X	X	X
Demographics		X	X		X	X
State by year FE			X			X

Sample period is 2005-2016 and frequency of all variables is quarterly. Informality data correspond to the individual level, homicides to the municipal level and weather data to the state level. Homicide rates are in per capita terms (per 10,000). Standard errors are in parenthesis. All specifications include sampling weights. Demographic controls: age, sex, years of education and income level. Errors are clustered at the municipal level. Panel A excludes individuals where changes do not last more than a quarter but there is at most one such pattern along the five quarters. Panel B excludes individuals where changes do not last more than a quarter and there is one or more such patterns along the five quarters. Dependent variable mean is 0.67. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 3.4. First Stages

	(1)	(2)	(3)
Temperature	0.0169***	0.0169***	0.0073*
	(0.0053)	(0.0053)	(0.0041)
Year FE	X	X	X
Quarter FE	X	X	X
Municipality FE	X	X	X
Demographics		X	X
State by year FE			X
Observations	482761	482640	482640

Sample period is 2005-2016 and frequency of all variables is quarterly. Informality data corresponds to the individual level, homicides to the municipal level and weather data to the state level. Homicide rates are in per capita terms (per 10,000). Dependent variable mean is 0.64. Standard errors in parenthesis. All specifications include sampling weights. Demographic controls include age, sex, years of education and income level. Errors are clustered at the municipal level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

4 Fighting Against Hunger: A Country-Wide Intervention and its Impact on Birth Outcomes

4.1 Introduction

Despite being a thriving emerging economy, Mexico experiences an unbalanced distribution of wealth. This affects more than 53 million people who live below the national poverty line, which accounts for 44% of the total population (CONEVAL, 2016a). While stunting and anemia have declined in Mexico, undernutrition is still prevalent in several regions (Kroker-Lobos et al., 2014). Food poverty alone, defined as not having enough income to purchase the basic basket of goods, affects 24.6 million people in Mexico, equivalent to 20.1% of the total population (CONEVAL, 2016b).[34] All at once, overweight and obesity have increased rapidly among all age groups (Kroker-Lobos et al., 2014). The joint problem of undernutrition and obesity has motivated social assistance programs that not only provide food but that aim to improve nutrition, especially among infants. In Mexico, out of 40 million children and adolescents living in Mexico, more than half of them lives in poverty (UNICEF, 2018). This is particularly worrying as having a disadvantaged socioeconomic status has negative health and human capital implications that carry through into adulthood (Currie, 2009).

Most recently, in 2013, the Mexican government set in motion a large national intervention named *Sin Hambre* (SH) (DOFa, 2013).[35] SH was a broad targeting and coordination strategy involving a large set of programs from several ministries aiming to fight hunger (Alderman et al., 2017). This was the flagship assistance policy in Mexico from 2013 to 2018 and it has been one of the most important large-scale food assistance policies implemented

[34] Food poverty as defined by CONEVAL refers to not having enough income to purchase the basic basket of goods.
[35] The program was also known as *Cruzada Nacional contra el Hambre* (CNCH) and can be literally translated as National Crusade Against Hunger. *Sin Hambre* can be translated as *No Hunger*.

in recent years in Mexico and Latin America. The scope of SH was ambitious as it involved more than 30 programs and represented average annual expenditures of around 1% of Mexico's GDP from 2013 to 2017 (ASF, various years).[36] This is a considerable sum compared to the 7.5% spent in public social expenditures in the same period in Mexico (OCDE, 2019).[37] Importantly, SH added new important programs that sought to improve nutrition intake of recipients: PAL-Sin Hambre (PAL-SH) and Comedores Comunitarios (CC) (ASF, various years; Alderman et al., 2017). PAL-SH constituted a switch from a former cash transfer into a transfer delivered utilizing a prepaid card which is restricted to designated grocery stores and specific food items, while CC consisted of the inception of several many feeding stations that provided meals at discounted prices. Internal evaluations by the government indicate that food and health access improved following SH, both for treated individuals and municipalities, CONEVAL (2015).[38] These evaluations, however, ignore health related outcomes and do not account for time trends.[39] Overall, there are no studies on the impact of SH on health-related outcomes at the national level. To my knowledge, there is only one related study that analyses the impact of CC at the local level. This work finds evidence that weight for height and high for weight measures for children under five years old improve in response to CC, one component of SH (Natalie et al., 2018). The study, however, is limited as it only focuses on CC, does it for a small sample from a single state, and does not account for time trends as there is no control group.

[36] Information obtained from reports issued by the *Auditoria Superior de la Federacion* (ASF) from 2013 to 2017. ASF is the national organization in charge of auditing governmental organizations in Mexico.
[37] Public social expenditure comprises cash benefits, direct in-kind provision of goods and services, and tax breaks with social purposes. It excludes social benefits not provided by general government.
[38] CONEVAL (2015) consists of two reports: the first refers to a panel survey conducted at the household on treated individuals while the second focuses on the aggregated results of five municipalities exposed to SH.
[39] Other studies available are mostly qualitative and focus on analyzing SH in terms of its design and implementation. Some examples of this research can be found in Caro et al. (2018), Martinez et al. (2016), or Gil et al. (2014).

Estimating the impact of SH in terms of birthweight seems a natural first stage assessment. Birthweight is known as the single most important indicator of infant health. Plus, infants are among the most vulnerable groups to hunger. Moreover, an established literature identifies that low weight at birth affects health during childhood and adulthood as well as human capital accumulation.[40] The need to investigate the impact of SH on birthweight is further motivated by the fact that eliminating infant malnutrition was one of the main objectives of SH. This paper estimates the impact of mother's exposure to SH on birth weight and the incidence of Low Birth Weight (LBW), which refers to births weighting less than 2,500 grams, of her child using vital statistics data on birth records. My identification strategy exploits the regional variation and timing of the municipality-by-municipality rollout of the nationwide program SH using a multiperiod difference-in-difference matching estimator as proposed by Imai, Kim, and Wang (2019).[41] This methodology pertains to recent literature that emphasizes that regression models with time and unit fixed effects are typically biased under the parallel trend assumption when units are treated at different points in time (Imai & Kim, 2011; Borusyak and Jaravel, 2017; Abraham and Sun, 2018; Athey and Imbens, 2018; Chaisemartin and D'Haultfoeuille, 2018; Goodman-Bacon, 2018). In a nutshell, the proposed methodological framework first selects a set of matched control units for each treated unit that on average is similar to the treated unit in terms of its outcome and covariate histories. Then it applies a difference-in-difference estimator to each treated unit and its synthetic control unit to adjust for unobserved time trends. Finally, it averages of all such differences.

[40] Pregnant women are among the most vulnerable groups to malnutrition. Each year around 20 million babies are born underweight (Fanzo et al., 2018).
[41] The matching method is available via the open-source statistical software, PanelMatch: Matching Methods for Causal Inference with Time-Series Cross-Sectional Data, at https://github.com/insongkim/PanelMatch.

The methodology chosen is ideal to evaluate the impact of SH due to its particular staggered rollout.[42] First, Imai, Kim, and Wang (2019) simplify the analysis of data where several units receive treatment multiple times and timing differs across units, which is the case of SH rollout.[43] Similar methodologies merely allow to study cases where one single unit is treated, require a large number of units not receiving the treatment at all, or entail complex modeling for each time period (Abadie et al., 2010; Arkhangelsky et al., 2018; Xu, 2017; Imai and Tatkovic, 2015). Second, this matching methodology allows estimating causal effects by synthetically constructing control units that suffice the parallel trend assumption. This is especially important in the context of SH as the order in which municipalities were treated was not randomly chosen and so estimates could be biased. As further explained in the next sections, the poorest municipalities were treated first followed by the next poorest. It is important to notice that poorer municipalities could derive larger benefits if health at the bottom of the income distribution exerts larger benefits (Deaton, 2003). Similarly, poorer municipalities could derive smaller benefits due to a lower income and availability of services. The design of the program thus makes its evaluation challenging. By comparing each treated unit to a control unit built to be similar to the treated observation in terms of outcome and covariate histories this methodology can provide a causal and unbiased estimate. Third, this method accounts for post-treatment bias since future treatments may be caused by past treatments, something present in SH due to its staggered design and for which other methodologies require complex modeling. Lastly, this methodology allows computing model-based standard errors for estimates in a non-computationally intensive way. See Imai, Kim, and Wang (2019) for a detailed literature review on alternative methods and their limitations depending on the application context. In addition to

[42] The methodology is described in more detail in the next sections.
[43] This process can be considered a generalization of the synthetic control method proposed by Abadie et al. (2010) in which only one unit receives the treatment.

the multiperiod difference-in-difference methodology mentioned above I use various other linear regression identification strategies as robustness checks.

My main results indicate that exposure to the program has a moderate impact on birthweight and LBW at best. Overall, I observe mixed evidence across specifications, with estimates leaning towards optimistic yet small effects. While some estimates are pessimistic, none of these are significant, nor economically or statistically. My preferred specification, using the propensity score weighting matching methodology, shows that SH reduces the fraction of LBW births by 0.1 percentage points. Although statistically insignificant, this estimate is economically relevant as it represents a decrease of 2.3% with respect to the mean value. In terms of birthweight in grams, under this same specification the impact of SH appears to be insignificant both economically and statistically as it represents a decrease of 0.4 grams which corresponds to a decrease of 0.01% with respect to the mean value. While the propensity score weighting methodology provides the best covariate balance, estimates from some refining methodologies are more optimistic. Overall, results indicate that SH reduces the fraction of LBW by at most 0.4 percentage points (6.7% with respect to the mean value) and increases birthweight in grams by at most 6 grams (0.19% with respect to the mean).

Studying the effect of the program on individuals considered as income eligible to social programs indicate larger impacts for some but not all specifications. In particular, the effects corresponding to the propensity score weighting methodology do show a reduction of 0.6 percentage points in the fraction of LBW (10.5% with respect to the mean value) and an increase of 9.2 grams in birthweight (0.29% with respect to the mean value). Estimating the impact of SH up to two years after the first year of exposure shows some evidence of longer-term benefits in line with contemporaneous impacts. Using alternative identification strategies that consist of triple difference in differences approaches that compare eligible to non-eligible individuals in treated municipalities against untreated municipalities yield estimates in line with the main results, there are beneficial impacts, but these are not observed across all specifications. Estimates may be driven by compositional effects associated with fertility. In this regard, while estimates for fertility rates show mix evidence, they overall show a reduction in response to the program. Such results could be due to conditions in the treated municipalities that reduce fertility rates and incentivize investments per child (Rosenzweig et al., 2009).

My work makes contributions in various regards. First, I am the first to evaluate the impact of SH in terms of birthweight and to provide an external evaluation of the program in terms of health outcomes. Second, I add to the literature on the impact of assistance policies on birthweight for large food assistance programs in developing contexts. Third, I provide one of the firsts practical applications of the matching method proposed by Imai, Kim and, Wang (2019) to a social program.

4.2 Sin Hambre program

SH was a broad coordination and targeting strategy launched in 2013 in Mexico that involved a large set of new and existing programs from several ministries. While SH involved several programs created in former administrations like *Seguro Popular* (SP), the universal health insurance policy, and *Prospera* (PDHO), the conditional on school attendance cash transfer, it also introduced new important programs that aimed to improve the nutrition intake of recipients like *PAL-Sin Hambre* (PAL-SH) and *Comedores Comunitarios* (CC) (ASF, various years; Alderman et al., 2017). PAL-SH constituted a switch from a former cash transfer into a transfer delivered by means of a prepaid card which is restricted to designated grocery stores and specific food items, while CC consisted of the inception of several many feeding stations providing meals at discounted prices (DOF, 2013b; SEDESOL, 2013a). While the original SH act included around 70 programs, this number decreased to 30 programs in later years (ASF, various years). In practice, according to government reports of SH on its attainments in terms of coverage, the main programs were the following: CC, LICONSA, PAL, PAL-SH, PAR, PCS, PDHO, PESA, PETC, PROCAMPO and SP. The Appendix A1 provides a summary with the description of each of these programs.[44]

SH is a food assistance policy that was rollout nationally in three stages, assigning a set of municipalities to each stage depending on the relative prevalence of poverty and food insecurity eventually covering all municipalities (DOF, 2013a). SH treated the most underserved municipalities in 2013, the next in 2014 and the rest in 2016. Following this staged strategy, PAL-SH and CC were introduced in selected municipalities while formerly existing programs like were mandated to prioritize their activities either increasing coverage or simply giving preference to eligible individuals from selected municipalities (DOF, 2013a). PAL-SH constitutes a switch from a former cash transfer into a transfer delivered by means of a prepaid card that is only usable in designated grocery stores and on selected food items (DOF, 2013b). CC consists of the inception of several many feeding stations providing meals for free or at subsidized prices to specific groups including pregnant women (SEDESOL, 2013a). Figure 2.1 shows the geographical distribution of treatment across municipalities and years. Details on the selection process are provided in Appendix A2.

[44] Given that each administration rebrands and modifies previous programs the acronyms and names provided might differ across time. Unless otherwise indicated, I use the names and acronyms corresponding to the period from 2000 to 2012 prior to the introduction of SH.)

SH was implemented in municipalities according to stages defined in federal rules.[45] In January of 2013, the passage of an act announced a first stage covering 400 municipalities in 2013 (SEDESOL, 2013b). In January of 2014, an amendment to the prior act announced a second stage covering 612 municipalities in 2014 and anticipated a third stage to occur in 2015 covering the rest of the municipalities (SEDESOL, 2014). By the time the first stage was announced second, and third stages were unanticipated. In practice, according to rollout information obtained directly from SEDESOL, the third stage was also unanticipated to recipients as it was postponed and started until 2016.[46] SH assigned municipalities to each stage depending on the relative prevalence of poverty and food insecurity essentially treating the most underserved set of municipalities in 2013, the next in 2014 and the rest in 2016. In the following paragraphs I describe the procedure followed by SEDESOL, the original documents detailing the procedure are referenced as SEDESOL (2013b) and SEDESOL (2014).

To determine eligibility into each stage, municipalities were ranked in descending order, those most underserved on top, based on four indexes measuring poverty and food insecurity based on information from the 2010 Census and the 2010 MCS-ENIGH, a module from the national income and expenditures survey.[47] The indexes used to rank municipalities were the following: the proportion of people under extreme poverty (P%), absolute number of people under extreme poverty (P#), proportion of people under both extreme poverty and food insecurity (PI%), and absolute number of people under both extreme poverty and food insecurity (PI#). These rankings were then used to establish thresholds that separated a stage from each

[45] Social assistance programs in Mexico are both funded and coordinated federally. This means that it is not up to the authority of local areas, in this case municipalities, when to start.
[46] The fact that the third stage started in 2016 comes from the rollout information obtained via request to SEDESOL.
[47] MCS-ENIGH refers to the Socioeconomic Conditions Module (MCS- Módulo de Condiciones Socioeconómicas) from the National Household Expenditure and Income Survey (ENIGH-Encuesta Nacional de Ingresos y Gastos en los Hogares).

other in order to roughly include 50% of the objective population during the first stage and an additional 25% by the second stage. The objective population amounted to 7.4 million people across the country, roughly 5% of the total population, SEDESOL (2013b). The corresponding assignment resulted in a first stage assisting 400 municipalities starting in 2013, and a second stage assisting the first stage plus 612 additional municipalities starting in 2014, and a third stage assisting former and remaining municipalities starting in 2016.[48] While most municipalities were selected based on these rankings several were chosen using more discrete rules.[49] According government reports, from 2013 to the first months of 2015, PAL-SH and CC, the main and new components, incorporated approximately 731 thousand families and 627 thousand beneficiaries, respectively, (PRESIDENCIA, 2015). Considering an average family of four individuals, which roughly corresponds to the household population divided by the total households, only considering PAL-SH a conservative estimate of the number of individuals treated lies around 3 million, roughly 2.5% of the total population.[50] A lower bound considering that individuals can enroll into multiple programs at once, and formerly created programs also enrolled more people as part of SH strategy. For instance, during the implementation of SH, LICONSA added 2.6 million beneficiaries while PDHO added 728 thousand families (PRESIDENCIA, 2015).

4.3 "Sin Hambre" and birthweight

Birthweight is known to be strongly associated with socio-economic factors (De Bernabé et al., 2004; Kramer, 1987; Cnattingius, 2004). At large, SH programs as described in the previous section consist of monetary transfers, in-kind transfers, discount prices, training plans, and health services. These programs are expected to affect birthweight by promoting nutrition intake.

[48] The number of municipalities in 2013, when the selection process was made, was 2,456. As of 2018, the number of municipalities is 2,458.
[49] There were 128 municipalities chosen based on such other considerations, 19 for the first and 119 for second stage.
[50] Formerly created programs, also increased the number of beneficiaries,

Similarly, other programs may have an indirect impact on birthweight as they provide health services and improve socioeconomic conditions of women in reproductive age in general.[51] Diverse components therefore may impact birth related outcomes due to exposure during and prior to pregnancy.

PAL-SH and CC, the new components introduced by SH, are particularly important since birthweight is widely known to be positively related to malnutrition during and prior to pregnancy (Verma et al., 2019). PAL-SH, which is a switch from its former version PAL, essentially rotates the budget set away from non-selected food items and expands it towards these items. CC, on the other hand, expands the budget set towards the food items included in meals provided. The extent to which these components drive results will depend on the number of recipients as well as other considerations. First, if households affected by PAL-SH and CC are majorly extramarginal, effects arising from PAL-SH and CC on this groups would be larger.[52] This is something likely as eligibility to PAL-SH and CC requires satisfying means tests that target low income households.[53] Second, PAL-SH and CC can potentially reduce maternal obesity and its negative consequences on newborns, including reducing larger than usual birth weight occurrences potentially having a negative effect on birthweight.[54] Although this second point is less likely, food assistance programs in developed settings have shown that buying food unrestrainedly leads to obesity in general (DeBono et al., 2012, Golan et al., 2008).

[51] For a detailed review on the medical literature about the determinants of birth weight and how transfers can affect it see Almond (2011).
[52] Households can be regarded as inframarginal or extramarginal depending on whether the in-kind food assistance exceed regular food expenditures. Households are inframarginal if food expenditures are above the allotment provided, in which case program functions as an unrestricted transfer. Households are extramarginal if the allotment is below regular food expenditures.
[53] Means tests prevents tagging as eligibility depends on several characteristics hard to manipulate together.
[54] Evidence shows that exposure to maternal obesity is a strong predictor for large for gestational age status (LGA), gestational diabetes, among other congenital anomalies (Boney et al., 2005; Leddy et al., 2008, Blomberg et al., 2010). Large for gestational age (LGA) is an indication of weight above the usual amount for the number of weeks of pregnancy that lies above the 90th percentile for that gestational age.

4.4 Data

My main data set refers to the effective rollout by year and municipality of SH. This was obtained via direct request from SEDESOL.[55] The data obtained provides different enrollment measures for all programs considered by SEDESOL to be associated with the strategy of SH. A municipality is treated if it has access to any of the programs part of SH. My primary data source for the outcome variables consists of vital statistics information on all birth records, a nationwide publicly available dataset administered by the Ministry of Health in Mexico. With around 2 million births per year, vital statistics observations allow to precisely estimate effects from SH. My main sample consists of women ages 18 to 44, from calendar years 2008 to 2017. I select 2008 as this is the first year publicly available. The structure of available variables is overall constant for the entire period. In addition, to explore issues related to fertility I use information on the number of births and infant deaths, both obtained from the National Institute of Statistics and Geography (Instituto Nacional de Estadística y Geografía-INEGI). Similarly, I focus on births from women in reproductive age from 2008 to 2017.

Because municipalities adopted SH at different stages, I compare SH treatment by virtue of municipality and date of birth. Using information on birth date I define treatment based on SH availability, with treatment depending on the timing of the outcome variable relative to the month the program was launched in the corresponding municipality. My main specification assigns SH a value of one if the program is available three months or one quarter prior to birth, a proxy of the beginning of the third trimester. This choice is based on related literate that points out that the third trimester of birth is the most important in determining weight (Almond et al.,

[55] SEDESOL stands for *Secretaria de Desarrollo Social* and can be translated as Social Development Ministry.

2011).[56] Alternatively, I assign treatment if the program is available two, three, and four quarters based on evidence that shows that birth weight is associated with antenatal maternal nutrition (Verma et al., 2016). To control for the possibility that mothers travel to give birth in a different location, treatment is assigned using the mother's municipality of residence and not the place of birth of the newborn.[57] Alternatively I assign treatment using the mother's municipality of birth.

Regarding the availability of treatment, as explained above, the rollout of SH occurred in three stages. The first stage was launched on April of 2013 and the second on March of 2014 according to official and popular press announcements.[58] Although the third stage was supposed to be launched on 2015 in practice, it started in 2016.[59] Different from the first and second stages, there is no official information on the actual start of this last stage. In my main specifications I assume the third stage started in March of 2016 in line with the previous stage.[60] Given that rollout information is available only at municipality by year level I assume a timing of adoption in line with the launch dates for each state.

Since no municipality can start earlier than the launch date, early adoption should not be a concern, however, delays could bias estimates. Such delays could occur due to federal funding insufficiencies or implementation issues at the municipal level. Assigning treatment according to the availability of the program as of one, two, three, and four months prior to birth can allow to identify potential systematic delays at the national level. If the program was in practice delayed for a quarter assigning treatment as of one quarter prior to birth will wrongly consider people that

[56] This is so because it is during this period when most weight is gained by the baby.
[57] My results could be biased downwards in absolute terms if unhealthier mothers seeking to obtain the subsidy move and deliver in treated municipalities. Similarly, assuming the program is effective, this would happen too if mothers living in treated municipalities during pregnancy deliver in untreated municipalities in search for better hospital services.
[58] Anecdotal evidence in newspapers suggests the time of adoption for the first and second stages coincides with the official launch date. No anecdotal information confirms or suggests a solid launch date for the third stage.
[59] Documentation associated with data obtained via request from SEDESOL mentions this explicitly.
[60] I test alternative scenarios for the start of the third stage.

are untreated as treated. In this case assigning treatment as two quarters prior to birth will be more accurate in identifying treatment, two quarters will be a proxy for one quarter given the delay. On the other hand, if the program is not delayed assigning treatment as of two quarters prior to birth will wrongly consider people that are treated in the first months of operation of the program as untreated. I use different assignments of treatment to identify better the actual timing of the program. In addition, to control for potential biases related to differences in the actual start of the program, I include the following pre-treatment variables interacted with a time trend: fraction of urban area, fraction less than 5 years of age, fraction 65 years or over, fraction with income less than the poverty line, the percent of land in the county that is farmland, and municipal population.[61]

4.5 Methodology: Multiperiod DiD for causal inference

Recent literature has emphasized that regression models with time and unit fixed effects are typically biased under the parallel trend assumption where units are treated at different points in time (Imai & Kim, 2011; Borusyak and Jaravel, 2017; Abraham and Sun, 2018; Athey and Imbens, 2018; Chaisemartin and D'Haultfoeuille, 2018; Goodman-Bacon, 2018). Imai, Kim and Wang (2019) propose a multiperiod difference-in-differences (DiD) methodology that eliminates this bias and consistently estimates the average treatment effect for the treated (ATT). In the midst of a fast growing literature on causal inference methods for panel data, the method proposed by Imai, Kim and Wang (2019) is especially appealing as it applies to cases where different units are treated at different points in time, where units can go back and forth between treatment and control conditions, and without the need to assume monotonicity.[62] This method

[61] Research on Food Stamps in the US documents that counties with a greater fraction of urban, black, or low-income population implemented the program in an earlier date and that counties with more land used in farming implement later (Hoynes et al., 2007; Almond et al., 2011).
[62] Monotonicity in this context refers to the case where treatment status increases stochastically within a group.

connects two-way fixed effects models, i.e. unit and time fixed effects models, to matching methods, relaxing linearity assumptions.

The identification strategy of this methodology, which is entirely nonparametric, assumes that the average potential outcomes under the control condition have a parallel time trend for the treatment and control groups without relying on a strict exogeneity assumption. This methodology consists of a weighted average of the two-time period two-group difference in difference estimators applied to each unit that switches from the control to the treatment condition.[63] The multi-period DID method employs a matching framework to assure the parallel trend assumption holds, essentially generating counterfactual outcomes for each treated observation in a given time period using observed outcomes from different time periods of the same unit.[64] Importantly, while a traditional DiD estimator requires the absence of causal relationship between past outcomes and current treatment, the multi-period DiD accounts for such dynamic causal relationships. I present their formulation for the multiperiod difference-in-difference estimator that accounts for both unit and time fixed effects.[65] This refers to the case with no time varying confounders, balanced panel dataset and matching based on the period prior to treatment.[66] The methodology can include time varying confounders, unbalanced panel datasets and matches based on further periods.

The methodology proposed by Imai, Kim and Wang (2019) evaluates the effect of a policy in t on both contemporaneous and future outcomes. For each unit $i = 1,2, \ldots, N$ at time $t = 1,2, \ldots, T$, a binary treatment indicator X_{it}, taking the value of 1 if treated and 0 otherwise, as

[63] Imai & Kim (2019) alternatively name this methodology as a Weighted Fixed Effects (WFE) due to the equivalence between the multi-period DID and WFE.
[64] This methodology extends to the case of balanced and unbalanced panel.
[65] A previous paper by Imai & Kim (2019) presented a similar model that accounts for unit fixed effects only.
[66] This formulation, its equivalence to the weighted two-way fixed effects estimator and the standard errors calculation can be found in Imai & Kim (2019) and Imai, Kim and Wang (2019).

well as a stream of outcome variables $Y_{i,t}, \ldots, Y_{i,t+F}$ is observed.[67] The number of leads, defined by F, represent the outcome of interest F time periods after the administration of treatment, where $F = 0$ represents the contemporaneous effect and $F = 1$ the effect a year after. For each treated observation (i, t) with $X_{it} = 1$ and $X_{i,t-1} = 0$, i.e. a treated observation should be untreated in the preceding period, the methodology selects control observations with an identical treatment history up to a certain number of periods, L. Since unit must be observed for F time periods after the treatment is administered and L time periods before the treatment is administered, different values of F will potentially imply different sets of treated and control units.

Once this matching set is determined for each treated observation, this set is refined to give more weight to those units that are most similar to each treated unit using matching and weighting techniques based on covariates and previous outcomes. The matching methods used, the Mahalanobis distance and propensity scores, basically select a subset including up to a specific number of most similar control units to the corresponding treatment unit. The weighting method used, the inverse propensity score, essentially generalizes matching methods by assigning weights giving more weight to those control units that are most similar to the treatment unit instead of giving an equal weight. Based on averages over this refined set, synthetic counterfactual outcomes are calculated. The effect of a policy is then calculated using a two by two difference-in-difference estimator to each treated unit and its synthetic control unit to adjust for unobserved time trends. Finally, it averages of all such differences. The calculated Average Treatment effect on the Treated (ATT) only considers treated units that have a non-empty matching set, i.e. units that change from the control to treatment condition and have at least one

[67] Where the outcome is assumed to be realized after the administration of the treatment.

counterfactual unit that is untreated while this transition happens. This is important to consider when interpreting results as not all observations are part of the calculation. Appendix A3 explains how the matching sets are constructed and how the ATT is calculated. The reader is referred to Imai & Kim (2019) and Imai, Kim and Wang (2019) for a complete description of the methodology.

4.6 Empirical Analysis

As explained in the data section, treatment at the individual level depends on availability at the month of birth. Since treatment is at the municipal level, I collapse data to municipality-year cells with the means from 2008 to 2017. As detailed above the treatment variable must be a binary variable taking the value of 1 or 0, therefore when collapsing individual level variables, I round up the treatment variable which would otherwise be a fraction, to be either 0 or 1.[68] Overall, the distribution of treatment shows that municipalities treated remain treated. The only exceptions occur for three municipalities that are untreated throughout the entire period and two municipalities that are treated until the second to last year and then become untreated in the last year.

Visualizing treatment variation across municipalities and years for SH is useful to understand better how the comparisons between treated and control observations is made. In line with Imai and Kim (2019), Figure 2.2 shows the distribution of treatment across years and municipalities, where blue and gray rectangles represent treated and control observations respectively. As it can be observed less than a fifth of all municipalities gets treated starting 2013, around a quarter gets treated starting 2014, and the remaining starts to be treated in 2016.

[68] This fraction rounds up to 0 or 1 in a municipality-year cell depending on the fraction of births treated. This depends on distribution of births across months and the month the program is launched.

Constructing the matched set for each treated observation based on treatment history requires to define the number of lags to adjust for, L. I condition this period to be five years, i.e. L=5. Five years is the longest period preceding treatment for first stage municipalities. Given the particular staggered design of SH, where virtually every unit remains treated, shorter periods of time should yield essentially the same result. Figure 2.3 illustrates the distribution of matched sizes of control units that share the same five years treatment history as a treated observation.[69]

Figure 2.3 indicates that 400 municipalities from the first stage can potentially be matched to any of the remaining 2,057 municipalities (rightmost bar), 612 municipalities from the second stage can be matched to any of the remaining 1,445 municipalities not part of the first and second stage (middle bar), and that the remaining 1,442 municipalities assisted during the third stage can be matched to the 3 municipalities never treated. This three units, being untreated, have themselves empty matched sets.

As explained in the previous section to estimate the ATT requires to define other matching criteria. Besides the length of the treatment history, L, which is set to a five years period as explained above, one needs to define the number of leads to be estimated, F. My main sample and results refer to the treatment effect on the contemporaneous period, $F = 0$. In addition, I consider up to two time periods after treatment implementation, $F = 2$.[70]

To satisfy the parallel trend assumption requires to adjust for confounders such as past outcomes and covariates. The main outcomes of interest to be analyzed are birth weight and low birthweight. Time-varying covariates include age, years of education, marriage status, insurance

[69] This distribution is virtually the same for shorter periods due to the staggered adoption of SH, however, periods of six and seven years would exclude municipalities from the first stage and second stage respectively.
[70] This is the maximum number of leads possible considering the first time the program is implemented is in 2013 and municipalities are observed from 2008 to 2017. As mentioned above, estimates are sensitive to the number of leads.

availability, per capita public investments, and per capita social assistance transfers.[71] Other covariates include 2010 municipality variables which are log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, percentage of income eligible population, and average income per capita. To assess whether there are extensive effects associated with the program, which would arise if SH also increased the number of births or decreased the number of fetal deaths. If SH increased the rate of pregnancies that lead to delivery, even if the program had beneficial impacts on birth outcomes, inframarginal births that would not otherwise occur could bias estimates in the opposite direction. I also present results on other birth related outcomes including very low birthweight, gestational age, height and gender.

An advantage of the methodology proposed by Imai, Kim and Wang (2019) is its transparency in how comparisons are made between treated and control observations. My results consider treated units as those municipalities from the first and second stage. Matching sets for the first stage originate in second and third stage municipalities and matching sets for the second stage originate in third stage municipalities. Due to the staggered implementation of the program, third stage municipalities essentially do not have any control units and so are not considered as treated units.[72]

Under the main specification, without controlling for any covariates, using the methodology proposed is equivalent to estimate the following two-way fixed effect model but assigning particular weights during the estimation process:

[71] Social assistance transfers are included to control for other expansions in social programs that occurred during this time period.

[72] My main sample excludes three municipalities that remain untreated throughout all the period of analysis. Including these three municipalities complicates interpreting results as matching methods do not result in balanced covariates. This occurs because including these three municipalities provides the exact same matching set of three municipalities for all third stage municipalities and these account for more than half of all municipalities. Researchers using this methodology should be aware of these considerations when interpreting their own results.

$$Y_{mt} = \gamma_0 + \gamma_1 SH_{mt} + \lambda_t + \lambda_m + \varepsilon_{mt}, \qquad (1)$$

where Y_{mt} is the outcome variable of interest in municipality m at time t, SH_{mt} is an indicator variable taking the value of 1 if a municipality is exposed to the program and 0 otherwise, λ_t refers to year fixed effects, λ_m refers to municipality fixed effects, and ε_{mt} refers to the error term. Without making using of the methodology proposed, estimating $\gamma_1$would require finding the value of $\widehat{\gamma}_{1_{FE}}$ that solves the following equation:

$$\widehat{\gamma}_{1_{FE}} = argmin \sum_{m=1}^{N} \sum_{t=1}^{T} \{(Y_{mt} - \overline{Y_m} - \overline{Y_t} + \overline{Y}) \qquad (2)$$

$$-\widehat{\gamma}_{1_{FE}}(SH_{mt} - \overline{SH_m} - \overline{SH_t} + \overline{SH})\}^2,$$

where the bar on top of the variables refer to means within municipalities, within years, and across all observations. For the outcome variable, Y_{mt}, $\overline{Y_m}$ is the within municipalities means, $\overline{Y_t}$ the within years mean, and $\overline{Y}$ the overall mean. The methodology proposed by Imai, Kim, and Wang (2019) gives instead a different weight to each observation:

$$\widehat{\gamma}_{1_{WFE}} = argmin \sum_{m=1}^{N} \sum_{t=1}^{T} W_{mt}\{(Y_{mt} - \overline{Y_m^*} - \overline{Y_t^*} + \overline{Y^*}) \qquad (3)$$

$$-\widehat{\gamma}_{1_{WFE}}(SH_{mt} - \overline{SH_m^*} - \overline{SH_t^*} + \overline{SH^*})\}^2,$$

where W_{mt} refers to weights and asterisks indicate weighted averages using W_{mt}. Intuitively, what these weights do is giving more weight to observations having a comparison set to build a synthetic control unit and less weight to those that do not. The reader is referred to Imai, Kim and Wang (2019) for a detail explanation on how these weights are calculated.

4.7 Empirical results

Figure 4.4 shows the covariate balance of different matching methods over the pre-treatment time period. The vertical axis shows the standardized mean covariate balance for SH treatment. The first column shows the results from the methodology before matching based on the treatment history. Using the nomenclature of Imai, Kim and Wang (2019), "Before Refinement" refers to the case where match is done based on treatment history only, without including covariates and outcome histories.[73] This column shows the results from matching based on the treatment history during the five-year period prior to administration.[74] The second, third, and fourth columns show the results from refining the matching sets based on the lagged outcomes and covariates using the different matching procedures mentioned in the methodology section. As noticed before, while the original sample includes three municipalities that are untreated throughout, I exclude these from the main results.[75]

Table 4.1 shows the average contemporaneous effect of SH on the fraction of births with Low Birth Weight (weight<2,500 grams) and the average Birth Weight (grams) using the Propensity Score Weighting methodology.[76] The Propensity Score Weighting methodology is my preferred specification as it provides the best balance in terms of covariate and outcome histories. All results are estimated considering a pretreatment history period of five years.[77]

[73] The "Before Matching" case does not refer to an unweighted two-way fixed effects specification. Results from linear regressions specifications that correspond to the unweighted two-way fixed effects case are provided below.

[74] Given the staggered implementation of SH, where municipalities remain treated after the first implementation, matching based on one and up to five periods of treatment history yield the same results.

[75] Including these municipalities slightly affect results without changing conclusions. Including these municipalities, however, make interpreting results less straight forward because these three municipalities provide the exact same matching set of exactly three municipalities for all third stage municipalities. Including these municipalities lead to covariate balances displaying large differences between the treatment and control groups. I similarly exclude two municipalities that go back to the control condition in 2017 after being treated in 2016.

[76] The main results use the mother's place of residence to assign treatment. Using the mother's place of birth yields similar results.

[77] Due to the staggered implementation of CNCH, alternatively including less than five lags yields essentially the same results.

Standard errors are based on 1,000 block bootstrap replicates. I assign treatment as of one, two, three, or four quarters prior to birth.[78] Each column shows the results associated with each treatment assignment. Table 4.1 indicates that SH results in a reduction of at most 0.1 percentage point, 2.3% with respect of the mean value, in the fraction of births with LBW, and an increase in terms of birthweight in grams of at most 1 gram, 0.03% with respect to the mean value. Overall, most benefits are observed from exposure as of the start of the second trimester, something that could be explained by an effective delay in the rollout of the program of about three months. Results from other refining methods associated with the Multiperiod Difference-in-Difference Methodology are more optimistic about the program both in terms of LBW and weight in grams. In terms of LBW, other refinement methods show a reduction of at most 0.4 percentage points, 6.7% with respect to the mean value. Regarding birthweight, other less rigorous refinement methods indicate an increase in birthweight of at most 6 grams, 0.19% with respect to the mean value. While most specifications point that the program was beneficial, there are some specifications that indicate otherwise. Table A4.4.1 in Appendix A4.4 shows the results from all refining methods used. While several estimates are statistically insignificant, these are relevant when compared to the mean value and most of them point into moderate benefits from the program.

To investigate if my results could be driven by compositional effects, I estimate the impact on SH on fertility rates (per 10,000) and fetal death rates (per 10,000). Overall, as shown in Table 4.2, while results are mixed, I find that overall both fertility rates and fetal death rates seem to decrease slightly. These effects on fertility could be the result of individuals choosing to

[78] Treatment is assigned as of at least one quarter prior to birth as this is a proxy for beginning of the 3rd trimester, which is when the newborn gains most of its weight. Treatment as of one quarter prior to birth will regard some untreated units as treated if the program was effectively implemented later than the official date. Similarly, treatment as of two quarters prior to birth will regard some treated units as untreated.

lower the quantity of children to promote quality in response to the program. The fact that fetal death rates decreased supports the claim that SH had a positive impact. Figure 4.5 shows the covariate balance for both fertility rates and fetal death rates.

Studying the effect of the program in the longer term shows similar results. Figure 4.6 and Figure 4.7 show the average effects for the three-year period after SH is implemented using also a pretreatment history period of five years for different matching methodologies.[79] Using one of the Imai, Kim and Wang (2019) methodology attributes, matching control units based on the future treatment sequence, I limit the comparison to that of treated units versus not treated units over this three year period. Under this setup, treated municipalities correspond to first stage municipalities, i.e. those treated from 2013 on, while control units are drawn from third state municipalities, i.e. those that are treated from 2016 on. Figure 4.6 indicates an overall consistent reduction in the fraction of births with LBW and a slight increase in average birth weight in grams.[80] Figure 4.7 indicates an overall slight reduction in both fertility and fetal mortality rates.

The multiperiod matching methodology assigns control units based on the composition of each municipality in terms of different demographic indicators including municipality variables as well as demographic characteristics. In turn, although not every woman is eligible, we could expect matched municipalities to have similar compositions in terms of income eligible women since the percentage of income eligible population at the municipal level is included in the list of covariates over which the matching procedure is done. Yet, in order to ensure that comparisons are made between similarly eligible individuals I restrict the sample to income eligible women. In order to do this, I replicate the methodology used by the Mexican government to determine

[79] $L = 5$ and $F = 2$.

[80] Notice that third stage municipalities are not considered as treated units because they do not have control units available to construct matching sets. Second stage municipalities are not part of the control group because these are treated from 2014 on.

eligibility at the household level. Using this methodology, income eligible women represent 5% of the total population. This is consistent with proportion of people originally targeted by SH, 5%, and the proportion of people treated, at least 2.5% considering PAL-SH alone during the first two years. The methodology is detailed in Appendix A4.6. Table 4.3 shows the results from a sample that only includes women that are estimated to be income eligible using the propensity score weighting. Table 4.3 indicates that SH was especially beneficial for income eligible women. Results, however, do not seem to be consistent across all matching methodologies as it can be observed in Table A4.6.2 in Appendix A4.6.

Table 4.4 and 4.5 show the results from using linear regression models. These results correspond to the unweighted two-way fixed effects version of Imai, Kim, and Wang (2019).[81] Without controlling for covariates there seems to be a reduction ranging from 0.02 to 0.3 percentage points in the fraction of births born with LBW, and an increase in average birth weight ranging from 3.28 to 5.17 grams. Controlling for covariates, however, reduces the size and significance of estimates. Figure A4.5.1 in Appendix A4.5 shows the estimates from event studies on the municipality-year means sample. These estimates point out to the existence of pretends that further justify the need of testing alternative approaches that can help sufficing the parallel-trend assumption. In the end, however, estimates from unweighted linear regressions are not entirely different from those coming from the matching procedure.

Comparisons using municipality-year means may fail to capture the impact of the program at the individual level. Figure A4.5.2, shows the event study results using the sample with individual observations. Although it is more difficult to rule out the existence of pretends, SH shows a detrimental impact in terms of both birthweight and LBW incidence. One particular

[81] As mentioned above, the “Before Matching” case, using the nomenclature of Imai, Kim and Wang (2019), corresponds refers to the case where match is not done on the treatment history. The “Before Matching” case does not refer to an unweighted two-way fixed effects specification.

concern to evaluate SH at the individual level is that, not every individual in the municipality receives the program. To take this into account, I use the measure of income eligibility previously described to implement a triple difference in difference where I compare the change from income eligible individuals to that of income ineligible individuals in response to treatment. The details on how this income eligibility index is calculated are shown in Appendix A4.6. If the program was effective, I would expect income eligible individuals to do better in treated locations after the program is launched. Table A4.6.3 shows the results from linear regressions interacting the treatment variable with a household level measure of income eligibility controlling for year fixed effects, municipality fixed effects, and covariates. Results from Table A4.6.3 indicate that income eligible individuals do not necessarily do better as a result of the program. Table A4.6.4 controls instead for year fixed effects, state fixed effects, and region by year fixed effects, allowing for more variation to be captured by the estimates. Results from this specification indicate that the program was beneficial for income eligible individuals. Similar results to those in Table A4.6.4 hold when discarding location fixed effects and only including year fixed effects.

As observed from the estimates above, there could be potential biases associated with fertility due to better nutrition prior to pregnancy. Better nutrition conditions could either promote inframarginal births or could deter pregnancies as parents decide to invest more in fewer babies. To tackle this problem, I difference off treatment by time of exposure for women that were already pregnant at the time of introduction. The main sample to be analyzed consists of births occurring since the first month the program was launched in the first stage in April of 2013 until 40 weeks ahead, the typical duration of pregnancy. A full description on this specification is provided in Appendix A4.7. Results from Table A4.7.1 indicate that being exposed for a longer period, since the first and second trimester of birth as opposed to the third trimester yield benefits in terms of birthweight and LBW. It seems, however, that income eligible individuals are not doing better with more exposure as it can be observed from differencing off income eligibility. Conclusions hold even while including state fixed effects as opposed to municipality fixed effects.

4.8 Discussion

Pregnant women are among the most vulnerable groups to malnutrition. Worldwide, each year 20 million babies are born underweight, the main cause being uterine malnutrition (Fanzo et al., 2018).[82] An established literature identifies that having a low birthweight at birth affects health during childhood and adulthood as well as human capital accumulation (Behrman et al., 2004). This evidence suggests that investing in improving nutrition of pregnant women can have large social benefits. In 2013, the Mexican government set in motion *Sin Hambre* (SH), one of the most important large-scale food assistance policies implemented in recent years in Mexico and Latin America representing an average annual expenditure of around 1% of Mexico's GDP from 2013 to 2017. This paper estimates the overall impact of SH on birth weight using a multiperiod difference-in-difference matching method as proposed by Imai, Kim and Wang (2019). The results from this paper have a causal interpretation as these satisfy the parallel trend assumption according to balance checking diagnostics.

My results indicate that exposure to the program has at best a moderate impact on birthweight and LBW. Overall, I observe mixed evidence across specifications, with estimates leaning towards optimistic yet small effects. Nevertheless, while some estimates are pessimistic, none of these are significant, nor economically or statistically. I argue this inconsistency may be

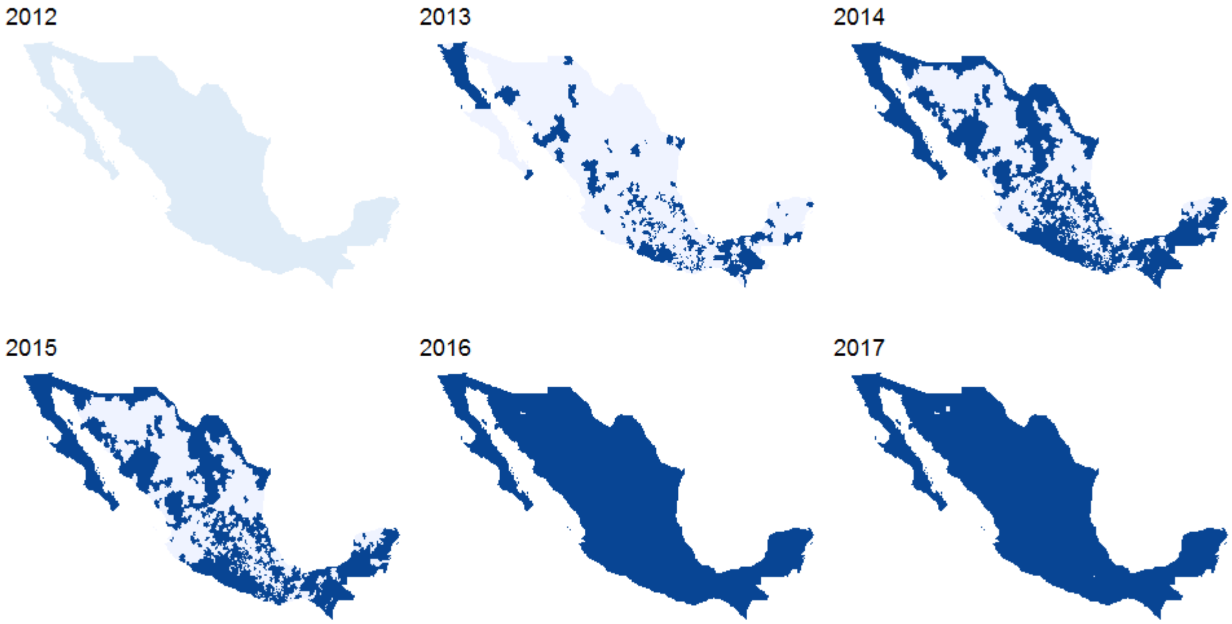
[82] Uterine malnutrition is a condition that has been associated with socio-economic factors, maternal lifestyles and medical risks (De Bernabé et al., 2004; Kramer, 1987; Cnattingius, 2004).

related to the program design which makes evaluating it challenging. Since the program prioritizes on poor municipalities and poor individuals, control units could be mechanically outperforming treated units if the impact was not large enough. Moderate benefits found, some of which are statistically and economically significant, are meaningful considering that even minor increases in birth weight positively affect adult health status, educational attainment, and earnings (Behrman et al., 2004).

My preferred specification, using the propensity score weighting matching methodology, shows that SH reduces the fraction of LBW births by 0.1 percentage points (2.3% with respect to the mean value). In terms of birthweight in grams, under this same specification the impact of SH represents a decrease of 0.4 grams (0.01% with respect to the mean value). Studying the effect of the program on individuals considered as income eligible to social programs indicate larger impacts, although not across all specifications. In particular, the effects corresponding to the propensity score weighting methodology do show a reduction of 0.6 percentage points in the fraction of LBW (10.5% with respect to the mean value) and an increase of 9.2 grams in birthweight (0.29% with respect to the mean value). Some of the estimates are in line with similar assistance programs. For instance, "Chile Crece Contigo", an early-life health and social welfare program implemented in Chile in 2007, has significant effects on birth weight of approximately 10 grams, Clarke et al. (2018). Similarly, these results appear to be comparable to the results from the food stamps program in the US which range from 2 to 5 grams for the African American subsample, Almond et al. (2011).

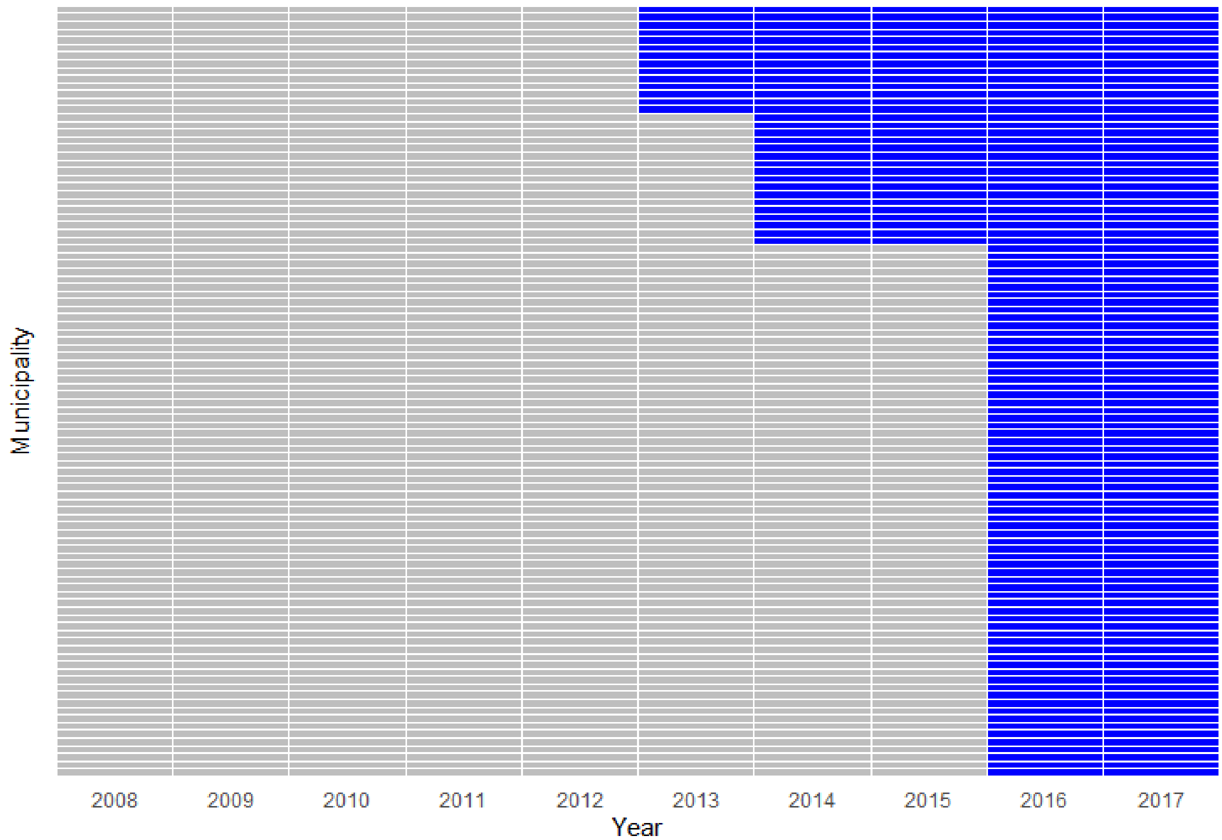
Figures

Figure74.1. Distribution of Treatment Across Municipalities and Years



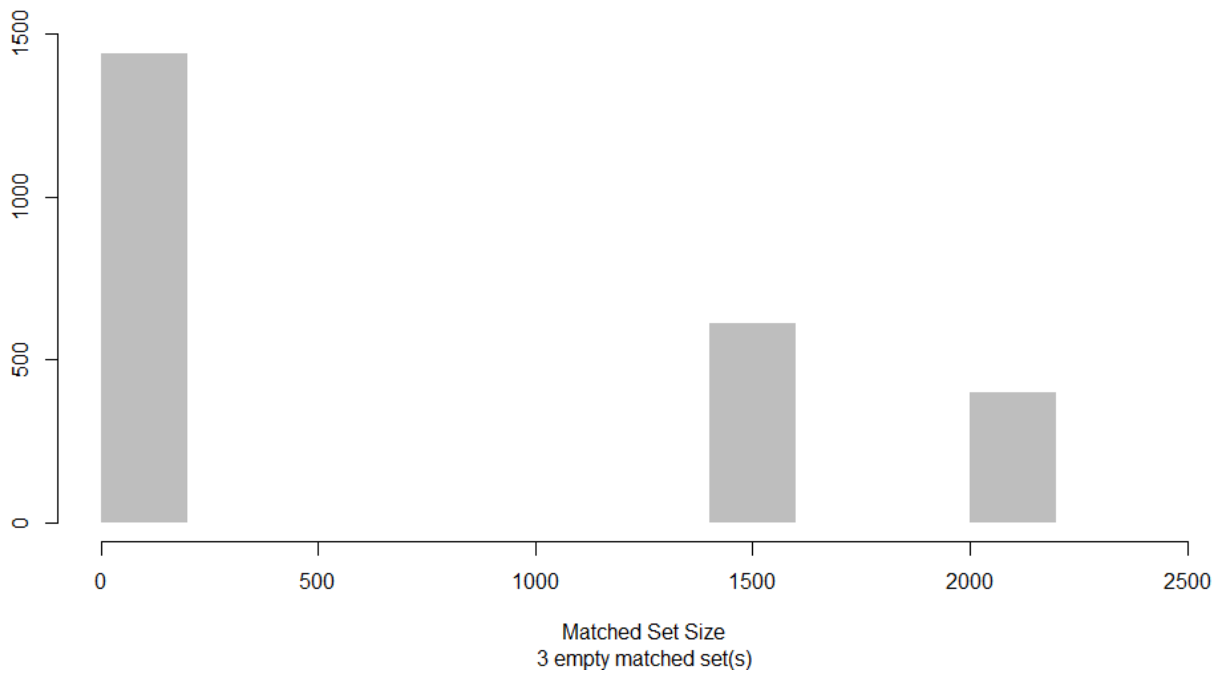
Note: Author's tabulations of SH implementation by municipality from 2012 to 2017 using information obtained via direct request from SEDESOL. The light areas represent untreated municipalities while dark areas represent treated municipalities.

Figure 4.2. Distribution of Treatment across municipalities and years for SH



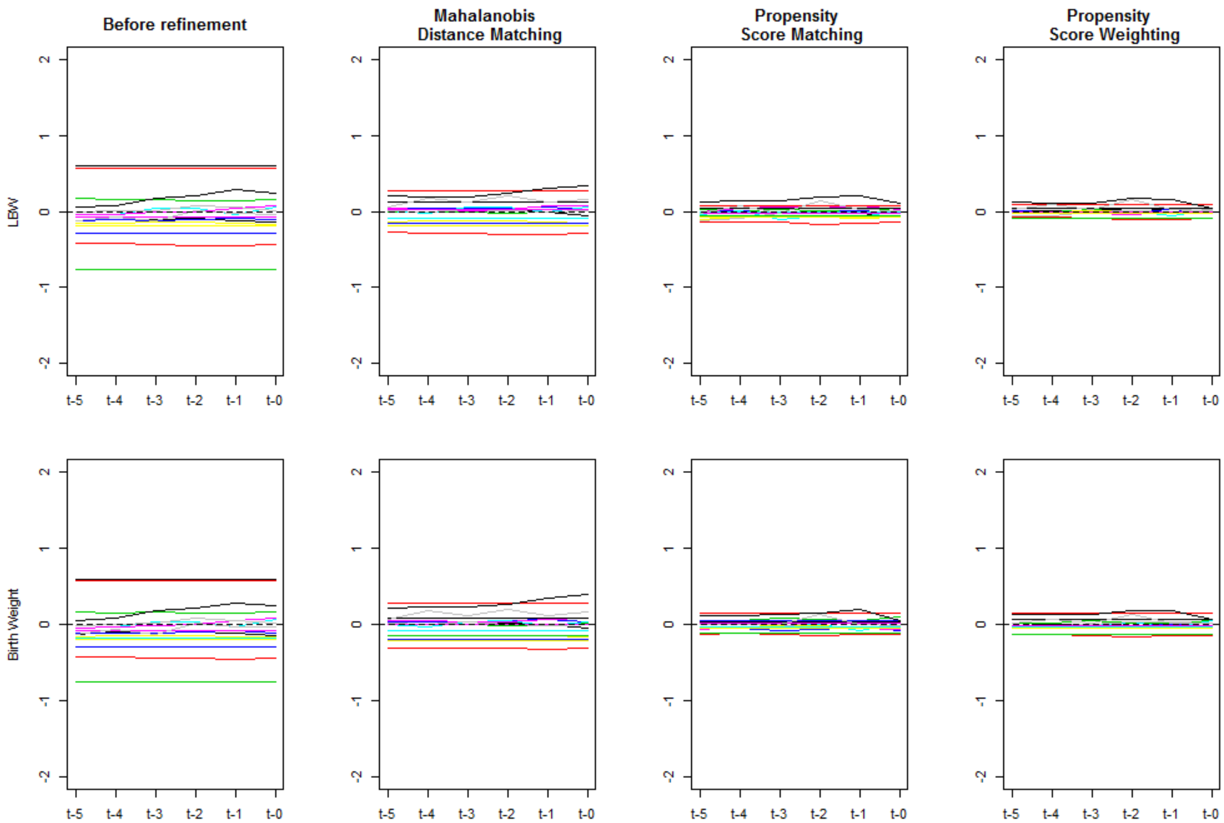
Note: Figure 4.2 shows the distribution of treatment for SH based on a random sample of 100 municipalities. Blue and gray rectangles represent treatment and control municipality-year observations respectively.

Figure 4.3. Frequency Distribution of the Number of Matched Control Units



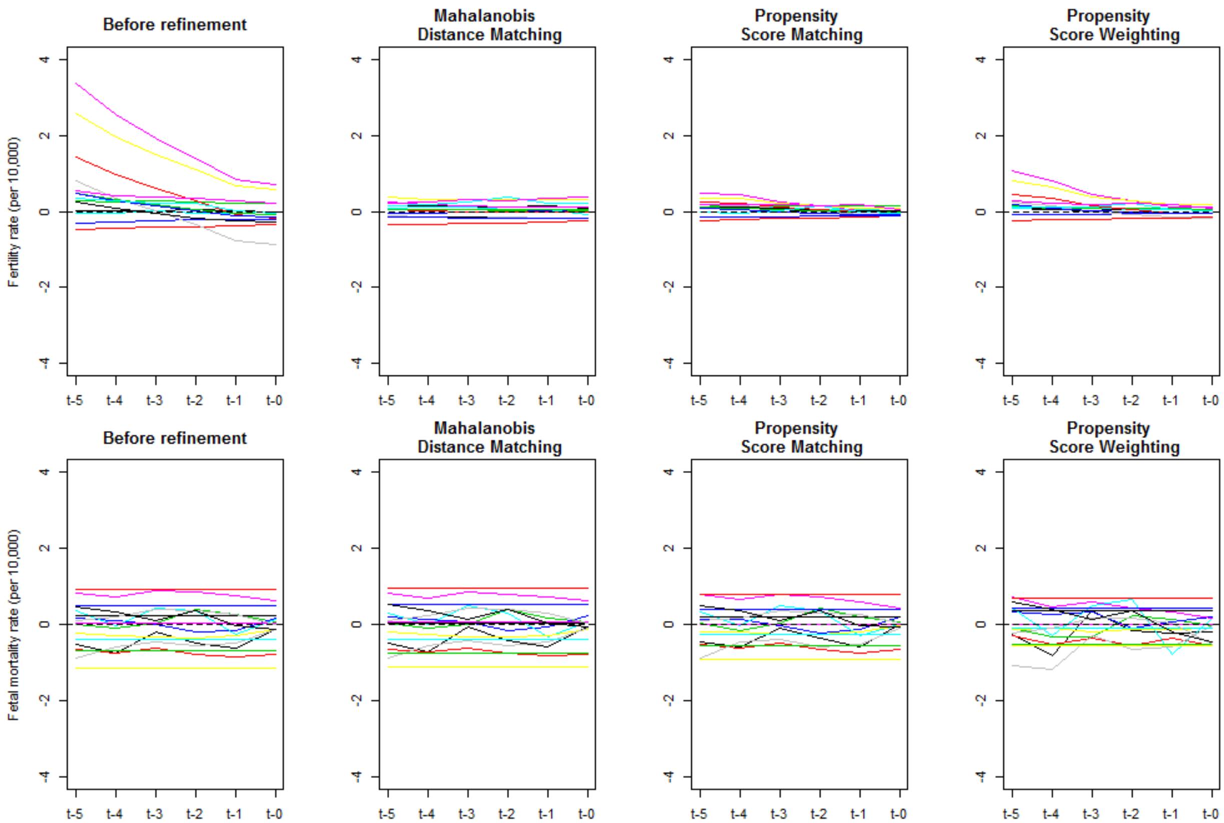
Note: Figure 4.3 shows the distribution of match sizes of control units that share the same treatment history as a treated observation for five years prior to the treatment year. The horizontal axis refers to different potential sizes for matching sets while the vertical axis refers to the frequency of sizes.

Figure 4.4. Covariate Balance Under Different Matching Methods, Birthweight and Low Birth Weight



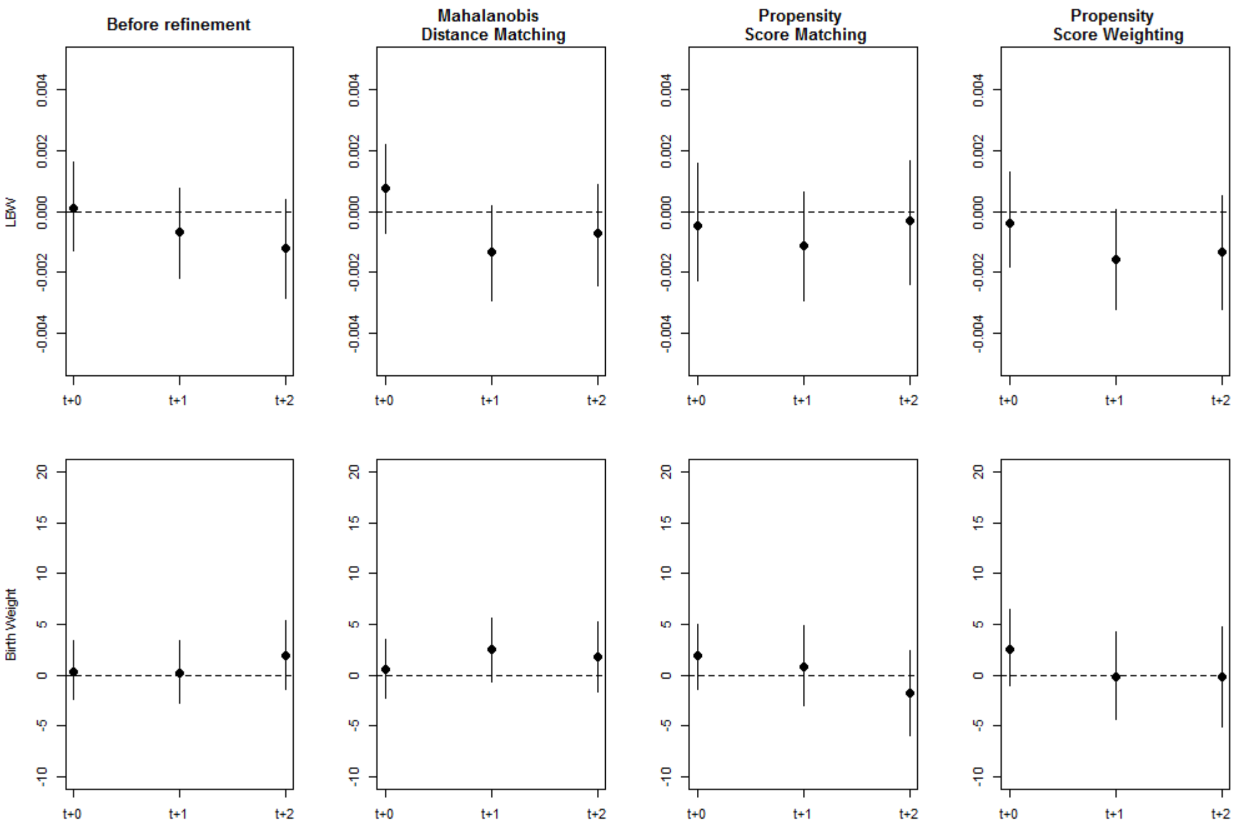
Notes: Each plot shows the standardized mean difference (vertical axis) over the pre-treatment time period of five years (horizontal axis). The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. The treatment is assigned as of 3 months prior to birth (proxy for beginning of the 3rd trimester). The first column, Before Matching, refers to the balance before matching on the five-year treatment history. The second column, Before Refinement, refers to the balance after matching on the five-year treatment history but before any refinement method. The remaining columns show the covariate balance after applying different refinement methods. Lines represent the balance of the lagged outcome variables and covariates over the pre-treatment period.

Figure 4.5. Covariate Balance Under Different Matching Methods, Fertility and Fetal Deaths



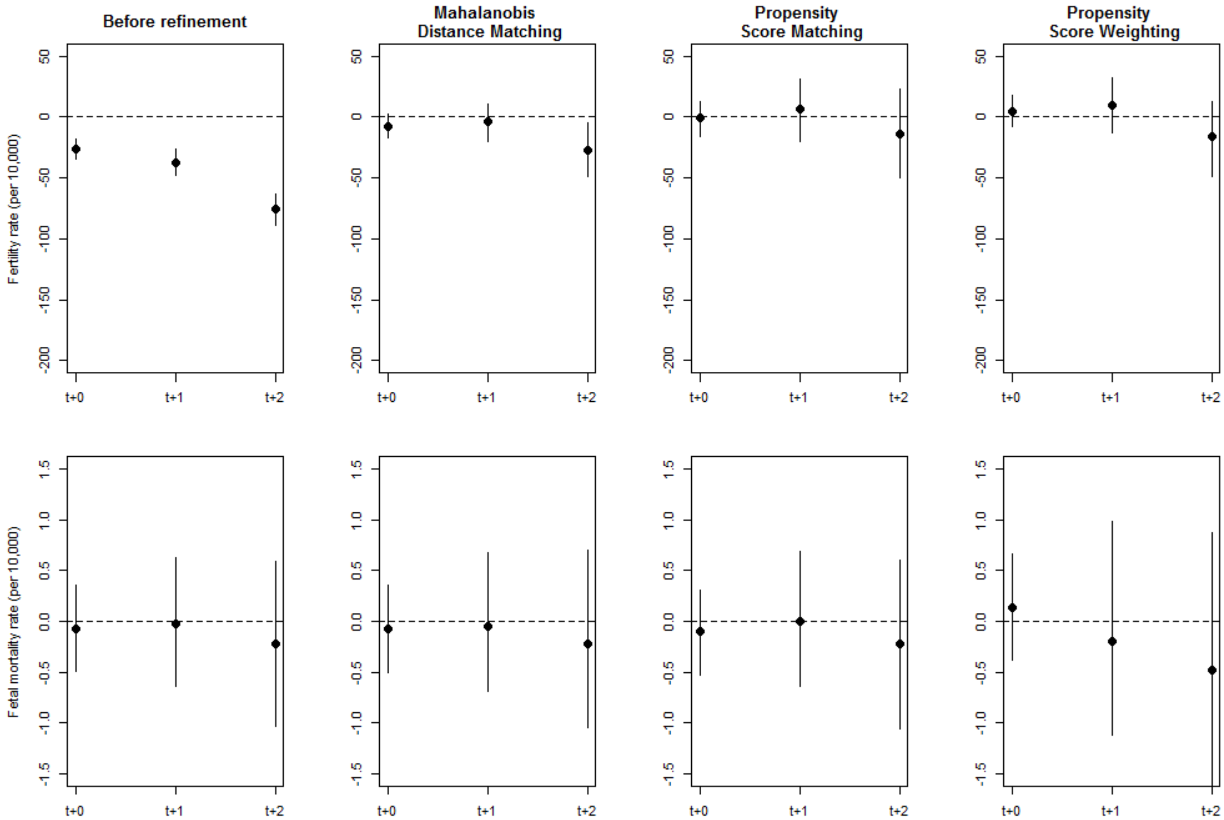
Notes: Each plot shows the standardized mean difference (vertical axis) over the pre-treatment time period of five years (horizontal axis). The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. The treatment is assigned as of 3 months prior to birth (proxy for beginning of the 3rd trimester). The first column, Before Matching, refers to the balance before matching on the five-year treatment history. The second column, Before Refinement, refers to the balance after matching on the five-year treatment history but before any refinement method. The remaining columns show the covariate balance after applying different refinement methods. Lines represent the balance of the lagged outcome variables and covariates over the pre-treatment period.

Figure 4.6. Estimated Average Longer-Term Effects of SH on Birthweight and Low Birth Weight



Notes: Each plot estimates the average effects of SH on the outcome variable of interest. The treatment is assigned as of 3 months prior to birth (proxy for beginning of the 3rd trimester). The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. Estimates adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e. L=5. The estimates for the average effects of SH are shown for the three-year period after the transition, i.e., F=4. Five different refinement methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. The vertical bars represent 95% confidence intervals based on 1,000 block bootstrap replicates.

Figure 4.7. Estimated Average Longer-Term Effects of SH on Fertility and Fetal Death Rates



Notes: Each plot estimates the average effects of SH on the outcome variable of interest. The treatment is assigned as of 3 months prior to birth (proxy for beginning of the 3rd trimester). The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. Estimates adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e. L=5. The estimates for the average effects of SH are shown for the three-year period after the transition, i.e., F=4. Five different refinement methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. The vertical bars represent 95% confidence intervals based on 1,000 block bootstrap replicates.

Tables

Table 4.1. Estimated Average Contemporaneous Effects of SH on Birthweight Outcomes Multiperiod Difference-in-Difference Methodology using Propensity Score Weighting

	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
A. Fraction < 2,500 grams				
SH	0.0001	-0.0014	-0.0005	-0.0011
Std. Dev.	(0.0008)	(0.0011)	(0.0007)	(0.0009)
% Impact (coef/mean)	0.22%	-2.32%	-0.75%	-1.75%
N. of obs.	19,870	19,870	19,870	19,870
B. Birthweight in grams				
SH	1.0517	-0.4031	-0.2495	0.0372
Std. Dev.	(1.5787)	(1.6843)	(1.47)	(1.5044)
% Impact (coef/mean)	0.033%	-0.01%	-0.0%	0.00%
N. of obs.	19,870	19,870	19,870	19,870

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. The estimation sample includes means by municipality for years including 2008-2017 where municipalities including cells with less than 25 observations are dropped. Estimates corresponding adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e., L=5. Five different methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, average income per capita, and percentage of income eligible population), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors correspond to block bootstrap replicates. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 4.2. Estimated Average Contemporaneous Effects of SH on Fertility and Fetal Deaths Multiperiod Difference-in-Difference Methodology using Propensity Score Weighting

	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
B. Fertility rate per 10,000				
SH	-9.10	-0.65	-3.58	-3.58
Std. Dev.	(4.8648)	(7.9361)	(5.8401)	(5.6046)
% Impact (coef/mean)	-6.74%	-0.48%	-2.65%	-2.65%
N. of obs.	19,500	19,500	19,500	19,500
C. Fetal death rate per 10,000				
SH (1 qrt)	-0.07	-0.20	-0.20	-0.20
Std. Dev.	(0.206)	(0.3725)	(0.3635)	(0.3662)
% Impact (coef/mean)	-0.65%	-1.80%	-1.77%	-1.77%
N. of obs.	19,500	19,500	19,500	19,500

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. Estimates adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e., L=5. Five different methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, average income per capita, and percentage of income eligible population), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors from the matching methods correspond to block bootstrap replicates. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 4.3. Estimated Average Contemporaneous Effects of SH on Birthweight Outcomes for Income Eligible Individuals, Multiperiod Difference-in-Difference Methodology using Propensity Score Weighting

	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
A. Fraction < 2,500 grams				
SH	0.0042	-0.0063	0.0034	0.0034
Std. Dev.	(0.0033)	(0.0069)	(0.0032)	(0.003)
% Impact (coef/mean)	6.92%	-10.5%	5.63%	5.63%
N. of obs.	1,040	1,040	1,040	1,040
B. Birthweight in grams				
SH (1 qrt)	-18.2769	9.2506	2.639	2.639
Std. Dev.	(9.2983)	(10.7741)	(7.7195)	(7.9856)
% Impact (coef/mean)	-0.58%	0.29%	0.08%	0.08%
N. of obs.	1,040	1,040	1,040	1,040

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. The estimation sample includes means by municipality for years including 2008-2017 where municipalities including cells with less than 25 observations are dropped. Estimates corresponding adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e., L=5. Five different methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, average income per capita, and percentage of income eligible population), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors correspond to block bootstrap replicates. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 4.4. Linear Regression Estimates on the Effect of SH on Birthweight Outcomes, unweighted two-way fixed effects not including covariates

`	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
A. Birthweight < 2,500 grams				
SH	-0.00194***	-0.00252***	-0.00315***	-0.00324***
	(0.00066)	(0.00077)	(0.00072)	(0.00071)
% Impact (coef/mean)	-3.16%	-4.11%	-5.14%	-5.29%
N. of obs.	19870	19870	19870	19870
B. Birthweight in grams				
SH	3.28**	3.99***	5.09***	5.17***
	(1.43)	(1.55)	(1.49)	(1.50)
% Impact (coef/mean)	0.10%	0.10%	0.16%	0.16%
N. of obs.	19870	19870	19870	19870
Year fixed effects	X	X	X	X
Mun. fixed effects	X	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. Each column shows the estimate corresponding to each of these timings. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, average income per capita, and percentage of income eligible population) each interacted with a linear time trend, age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. The estimation sample includes municipality by year means from 2008 to 2017. Standard errors are in parentheses. Standard errors are clustered at the municipality level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table 4.5. Linear Regression Estimates on the Effect of SH on Birthweight Outcomes, unweighted two-way fixed effects including covariates

`	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
A. Birthweight < 2,500 grams				
SH	-0.00018	-0.00063	-0.00028	-0.00039
	(0.00065)	(0.00076)	(0.00070)	(0.00069)
% Impact (coef/mean)	-0.29%	-1.03%	-0.46%	-0.64%
Dep. var. mean	0.06131	0.06131	0.06131	0.06131
N. of obs.	19850	19850	19850	19850
B. Birthweight in grams				
SH	0.65	1.08	0.41	0.52
	(1.38)	(1.51)	(1.44)	(1.44)
% Impact (coef/mean)	0.00%	0.03%	0.00%	0.00%
Dep. Variable mean	3153.53	3153.53	3153.53	3153.53
N. of obs.	19850	19850	19850	19850
Year fixed effects	X	X	X	X
Mun. fixed effects	X	X	X	X
Covariates	X	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. Each column shows the estimate corresponding to each of these timings. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, average income per capita, and percentage of income eligible population) each interacted with a linear time trend, age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. The estimation sample includes municipality by year means from 2008 to 2017. Standard errors are in parentheses. Standard errors are clustered at the municipality level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Appendices

A Appendix to 4 – Supplemental Tables and Figures

A4.1. SH's main assistance programs description

Acronym[1]	Objective
CC	Consists of the inception of several many food kitchens providing meals for free or at subsidized prices.
PAL	Originally devised to provide unconditional cash transfers were PDHO could not due to the unavailability of schools and clinics. With the introduction of PAL-SH, PAL remained to provide unconditional cash transfers where the unavailability of designated stores would prevent the debit card use.
PAL-SH	Switch from PAL cash transfer to a transfer delivered by means of a prepaid card that is only usable in designated grocery stores and on selected goods.
LICONSA	Although not a program itself, is a parastatal firm that supplies milk at subsidized prices.
PAR	Offers food items and other staples at discounted prices by means of designated stores and is operated by DICONSA, a federal agency.
PCS	Promoted training and implemented actions towards reducing post-harvest losses due to storage, transportation and commercialization.
PET	Provides temporary income assistance to individuals 16 years or older that observe a decrease in income due to economic, social, emergencies or disasters. Income is provided in exchange of work for local social projects.
PDHO[2]	Cash transfers conditional on school attendance of children, cash transfers conditional on clinic visits of both children and old household members, and food supplements for women during pregnancy or lactancy and children five years old or less.
PESA	Installs family gardens and provides monetary transfers to people performing agricultural, aquaculture and fishing activities.
PETC	School free school lunch meals to students in public schools.
PROCAMPO	Monetary transfers to local farmers.
SP	Social protection in health that offers public insurance to all citizens, specially aiming to promote health coverage for those not formally employed.

1. Each administration rebrands and modifies the name of social assistance programs. I use the acronyms and names corresponding to the period from 2000 to 2012 prior to the introduction of SH: Comedores Comunitarios (CC), Sistema de Distribuidoras Conasupo (DICONSA), Leche Industrializada Conasupo (LICONSA), Programa de Apoyo Alimentario (PAL), Programa de Apoyo Alimentario Sin Hambre (PAL-SH), Programa de Abasto Rural (PAR), Programa de Coinversion Social (PCS), PET (Programa de Empleo Temporal), Programa de Desarrollo Humano Oportunidades (PDHO), Proyecto Estratégico para la Seguridad Alimentaria (PESA), Programa Escuelas de Tiempo Completo (PETC), Programa de Apoyos para el Campo (PROCAMPO), and Seguro Popular (SP).
2. It is commonly known as Oportunidades because that was the name it held when the conditional components were introduced in 2002. From 2012 to 2018 it changed its name to "Prospera." A somewhat different version of "Oportunidades" was named "Progresa" from 1994-2000 and "Solidaridad" before that.

A4.2. SH, Selection Process of Municipalities

To select municipalities into each stage, SEDESOL first ranked all municipalities in descending order, placing the most underserved on top, based on four indexes: P%, the proportion of people under extreme poverty, P#, the absolute number of people under extreme poverty, PI%, the proportion of people under both extreme poverty and food insecurity, and PI#, the absolute number of people under both extreme poverty and food insecurity.[83] According to SEDESOL, these indexes were calculated using information from the Census and the ENIGH from 2010 as this was the latest year available in 2012, a year before the program was launched. In order to determine the first stage municipalities, a threshold for each index was determined to select the first municipalities that jointly accounted for at least 50% of the estimated objective population in 2012, i.e. the set of people under extreme poverty and food insecurity.[84]

The selection process followed by SEDESOL had the objective of covering all states in the country and in doing so, at least half of the objective population. For the first stage, SEDESOL started by selecting the highest ranked municipalities out from each index abovementioned: up to 167 based on P%, 184 based on P#, 140 based on PI%, and 150 based on PI#.[85] A municipality is eligible if it ranks above the corresponding threshold for either of the four indexes. After eliminating duplicates, these sets of municipalities together comprise a list of 381 municipalities from 28 different states. To increase national representativity SEDESOL included 19 municipalities based on other related criteria.[86] First, it included six municipalities from four states not encompassed in the former selection to include all 32 states. Second, it

[83] See SEDESOL (2013b) and SEDESOL (2014) for more details.
[84] The first stage covered 51.7% of the objective population (SEDESOL, 2013b).
[85] Other than assisting 50% of the objective population the exact reasoning behind each of these thresholds is not explicit in publicly available documents.
[86] The exact motivation behind these thresholds is not expressed in public documentation.

included 9 municipalities that held a significant proportion of the objective population.[87] Third, it included 4 municipalities where the abovementioned indexes had relevant increments from 2008 to 2010. This latter process led to a total of 400 municipalities for the first stage.

For the second stage, SH determined thresholds to reach, together with the first stage, at least 75% of the objective population.[88] For the second stage, SEDESOL selected the first 455 municipalities out of each index producing a set of 900 municipalities, including 397 from the first stage. In the same vein as in the first stage, additional municipalities were added to accommodate the following considerations. First, it added 4 municipalities to increase national representativity including one municipality from Colima, a state not previously selected. Second, it included 4 municipalities affected by natural disasters in 2013 in the state of Guerrero. Third, it included 45 border municipalities not considered in the first stage. Fourth, it included 28 municipalities from the states of Michoacán, Mexico, and Guerrero from "Tierra Caliente," a region characterized by a hot climate mainly inhabited by indigenous communities that were not contained within the first stage.[89] Finally, it included 31 municipalities to reinforce state level strategies that were in line with SH. These considerations in addition to the inclusion of three municipalities that were selected during the first stage based on alternative rules leads to a total of 1,012 municipalities for the second stage, out of which 612 are new additions.[90] The third stage corresponds to the remaining municipalities in the country.

[87] These 9 municipalities belong to four out of six municipalities that together hold 56% of the national objective population.
[88] The second stage covered an additional 26.7% of the objective population (SEDESOL, 2014).
[89] "Tierra Caliente" is a region within the states of Michoacán, Mexico, and Guerrero characterized by a hot climate mainly inhabited by indigenous communities.
[90] (SEDESOL, 2014).

A4.3. Multiperiod Difference in Difference Matching Methodology

Following Imai & Kim (2019) and Imai, Kim and Wang (2019), to form the matching sets for any treated observation (i, t) three sets of observations need to be defined: the within-unit matched set M_{it}, the within-time matched set, N_{it}, and the adjustment set A_{it}. The within-unit matched set M_{it} consists of an observation of that unit i in the previous period if such observation is untreated. The within-time matched, N_{it}, set is defined as a group of control observations from units in the same time period that share the same treatment history as i from time $t-1$ to $t-L$, where L is the number of lags L characterizing the treatment history. The adjustment set, A_{it}, contains those untreated observations in the previous period corresponding to the units in N_{it}.

$$M_{it} = \{(i', t'): i' = i, t' = t-1, X_{i't'} = 0\} \quad (1)$$

$$N_{it} = \{(i', t'): i' \neq i, t' = t, X_{i't'} = X_{i,t'} \; \forall \; t' = t-1, \ldots t-L\} \quad (2)$$

$$A_{it} = \{(i', t'): i' \neq i, t' = t-1, X_{i't'} = X_{i,t'} \forall \; t' = t-1, \ldots t-L\} \quad (3)$$

Equations (1), (2) and (3) define these sets for each treated observation with $X_{it} = 1$ and $X_{i,t-1} = 0$, i.e. a treated observation should be untreated in the preceding period. It is possible to have some observations with empty sets and so with no control observations sharing the same treatment history. This must be considered when interpreting results as these cases will be excluded from the calculations. The multi-period DiD estimator of the Average Treatment effect on the Treated (ATT), $\hat{\tau}(F, L)$, is an average of two-time period two-group DiD estimators for any observation (i, t) for which there is a change from control to treatment condition:

$$\tau\widehat{(F,L)} = \frac{1}{\sum_{i=1}^{N}\sum_{t=1}^{T} D_{it}} \sum_{i=1}^{N}\sum_{t=1}^{T-F} (D_{it}(Y_{\widehat{\iota,t+F}}(1) - Y_{\widehat{\iota,t+F}}(0)), \quad (4)$$

where $D_{i1} = 0$ for all i, $D_{it} = X_{it} \cdot 1\{|M_{it}||N_{it}| > 0\}$ for $t > 1$ and

$$\widehat{Y_{i,t+F}(x)} \tag{5}$$

$$= \begin{cases} Y_{i,t+F}, & if\ X_{it} = 1 \\ Y_{i,t+F-1} + \frac{1}{|N_{it}|} \sum_{(i',t) \in N_{it}} Y_{i',t+F} - \frac{1}{|A_{it}|} \sum_{(i',t') \in A_{it}} Y_{i',t'+F}, & if\ X_{it} = 0. \end{cases}$$

The counterfactual outcome for observation (i, t) consists of the difference between the observed outcome in that period and its outcome in the previous period minus the difference between the average outcomes across the same two time periods for the control units of (i, t). Notice that $\hat{\tau}$ depends on F and L. Since unit must be observed for F time periods after the treatment is administered and L time periods before the treatment is administered, different values of F will affect the set of treated and control units. The method also allows to analyze the effect of stable policies by finding matched control units based on the future treatment sequence. In doing so the method classifies units as treated if treatment is in place for F time periods and their corresponding control units as those that are untreated during that same period.

Finally, the methodology proposed needs to adjusts for other confounders such as past outcomes and time-varying covariates to better satisfy the parallel trend assumption by refining the match control sets using different matching and weighting methods. The matching methods used, the Mahalanobis distance and propensity scores, basically select a subset including up to a specific number of most similar control units to the corresponding treatment unit. The weighting method used, the inverse propensity score, essentially generalizes matching methods by assigning weights giving more weight to those control units that are most similar to the treatment unit instead of giving an equal weight.

A4.4. All Multiperiod Difference-in-Difference Methodologies by Time of Implementation

Table A4.4.1. Estimated Average Contemporaneous Effects of SH on Birthweight Outcomes, all Multiperiod Difference-in-Difference Methodologies

SH implemented as of X quarters prior to birth	Multiperiod Difference-in-Difference Methodology			
	Before Refinement	Mahalanobis Distance Matching	Propensity Score Matching	Propensity Score Weighting
	(1)	(2)	(3)	(4)
A. Fraction < 2,500 grams				
SH (1 qrt)	0.0000	0.0004	0.0006	0.0001
Std. Dev.	(0.0007)	(0.0008)	(0.0007)	(0.0008)
% Impact (coef/mean)	-0.00%	0.61%	0.97%	0.22%
SH (2 qrts)	-0.0031*	-0.004*	-0.0012	-0.0014
Std. Dev.	(0.0009)	(0.0011)	(0.0009)	(0.0011)
% Impact (coef/mean)	-5.09%	-6.73%	-1.94%	-2.32%
SH (3 qrts)	-0.0007	-0.0022	0.0001	-0.0005
Std. Dev.	(0.0007)	(0.0007)	(0.0009)	(0.0007)
% Impact (coef/mean)	-1.17%	-3.70%	0.15%	-0.75%
SH (4 qrts)	-0.0009	-0.0025	-0.0006	-0.0011
Std. Dev.	(0.0007)	(0.0008)	(0.0011)	(0.0009)
% Impact (coef/mean)	-1.58%	-4.13%	-1.07%	-1.75%
N. of obs.	19,870	19,870	19,870	19,870
B. Birthweight in grams				
SH (1 qrt)	0.3155	0.1274	1.0403	1.0517
Std. Dev.	(1.4558)	(1.453)	(1.7592)	(1.5787)
% Impact (coef/mean)	0.010%	0.004%	0.03%	0.033%
SH (2 qrts)	4.5596**	5.9203***	1.6047	-0.4031
Std. Dev.	(1.8991)	(2.2218)	(1.8721)	(1.6843)
% Impact (coef/mean)	0.14%	0.187%	0.05%	-0.01%
SH (3 qrts)	0.7589	3.0141**	-2.0921	-0.2495
Std. Dev.	(1.3797)	(1.4146)	(1.7739)	(1.47)
% Impact (coef/mean)	0.02%	0.09%	-0.06%	-0.0%
SH (4 qrts)	1.0092	3.2779**	-2.4026	0.0372
Std. Dev.	(1.3036)	(1.4348)	(1.7976)	(1.5044)
% Impact (coef/mean)	0.03%	0.10%	-0.07%	0.00%
N. of obs.	19,870	19,870	19,870	19,870

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. Estimates corresponding adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e., L=5. Five different methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors correspond to block bootstrap replicates. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table A4.4.2. Estimated Average Contemporaneous Effects of SH on Fertility and Fetal Deaths, all Multiperiod Difference-in-Difference Methodologies

SH implemented as of X quarters prior to birth	Multiperiod Difference-in-Difference Methodology			
	Before Refinement	Mahalanobis Distance Matching	Propensity Score Matching	Propensity Score Weighting
	(1)	(2)	(3)	(4)
A. Fertility rate per 10,000				
SH (1 qrt)	-20.85***	-7.50	-10.01	-9.10
Std. Dev.	(4.1661)	(5.0461)	(6.5431)	(4.8648)
% Impact (coef/mean)	-15.4%	-5.55%	-7.41%	-6.74%
SH (2 qrts)	2.23	7.36*	2.52	-0.65
Std. Dev.	(4.2145)	(4.0624)	(4.8557)	(7.9361)
% Impact (coef/mean)	1.65%	5.45%	1.87%	-0.48%
SH (3 qrts)	-10.98	-1.11	-3.50	-3.58
Std. Dev.	(3.956)	(4.6362)	(6.6676)	(5.8401)
% Impact (coef/mean)	-8.13%	-0.82%	-2.59%	-2.65%
SH (4 qrts)	-10.98	-1.11	-3.50	-3.58
Std. Dev.	(4.0744)	(4.5538)	(7.0903)	(5.6046)
% Impact (coef/mean)	-8.13%	-0.82%	-2.59%	-2.65%
N. of obs.	19,500	19,500	19,500	19,500
B. Fetal death rate per 10,000				
SH (1 qrt)	-0.08	-0.07	-0.04	-0.07
Std. Dev.	(0.2209)	(0.2348)	(0.2531)	(0.206)
% Impact (coef/mean)	-0.77%	-0.64%	-0.37%	-0.65%
SH (2 qrts)	0.106	0.032	-0.18	-0.20
Std. Dev.	(0.23)	(0.2353)	(0.3157)	(0.3725)
% Impact (coef/mean)	0.91%	0.27%	-1.62%	-1.80%
SH (3 qrts)	0.16	0.09	-0.24	-0.20
Std. Dev.	(0.2402)	(0.2574)	(0.3587)	(0.3635)
% Impact (coef/mean)	1.43%	0.77%	-2.14%	-1.77%
SH (4 qrts)	0.16	0.09	-0.24	-0.20
Std. Dev.	(0.251)	(0.2556)	(0.3572)	(0.3662)
% Impact (coef/mean)	1.43%	0.77%	-2.14%	-1.77%
N. of obs.	19,500	19,500	19,500	19,500

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. Estimates adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e., L=5. Five different methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors from the matching methods correspond to block bootstrap replicates. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table A4.4.3. Estimated Average Contemporaneous Effects of SH on Birthweight Outcomes for Income Eligible Individuals, all Multiperiod Difference-in-Difference Methodologies

SH implemented as of X quarters prior to birth	Multiperiod Difference-in-Difference Methodology			
	Before Refinement	Mahalanobis Distance Matching	Propensity Score Matching	Propensity Score Weighting
	(1)	(2)	(3)	(4)
A. Fraction < 2,500 grams				
SH (1 qrt)	-0.0054	-0.0053	0.001	0.0042
Std. Dev.	(0.0039)	(0.0037)	(0.003)	(0.0033)
% Impact (coef/mean)	-9.00%	-8.8%	1.71%	6.92%
SH (2 qrts)	-0.0024	-0.0008	0.0007	-0.0063
Std. Dev.	(0.0037)	(0.0032)	(0.0036)	(0.0069)
% Impact (coef/mean)	-3.95%	-1.27%	1.09%	-10.5%
SH (3 qrts)	0.0007	0.0024	0.0017	0.0034
Std. Dev.	(0.0031)	(0.0027)	(0.0031)	(0.0032)
% Impact (coef/mean)	1.15%	4.07%	2.80%	5.63%
SH (4 qrts)	0.0007	0.0024	0.0017	0.0034
Std. Dev.	(0.0031)	(0.0028)	(0.0033)	(0.003)
% Impact (coef/mean)	1.15%	4.07%	2.80%	5.63%
N. of obs.	1,040	1,040	1,040	1,040
B. Birthweight in grams				
SH (1 qrt)	-2.2758	-2.1188	-11.9217	-18.2769
Std. Dev.	(7.2284)	(7.5535)	(8.2151)	(9.2983)
% Impact (coef/mean)	-0.07%	-0.06%	-0.3%	-0.58%
SH (2 qrts)	-1.873	0.7235	0.356	9.2506
Std. Dev.	(6.432)	(6.8954)	(7.6776)	(10.7741)
% Impact (coef/mean)	-0.0%	0.02%	0.01%	0.29%
SH (3 qrts)	-2.7034	0.1239	-0.0995	2.639
Std. Dev.	(6.2846)	(6.4161)	(7.1635)	(7.7195)
% Impact (coef/mean)	-0.0%	0.00%	-0.00%	0.08%
SH (4 qrts)	-2.7034	0.1239	-0.0995	2.639
Std. Dev.	(6.2731)	(6.3386)	(7.1355)	(7.9856)
% Impact (coef/mean)	-0.0%	0.00%	-0.00%	0.08%
N. of obs.	1,040	1,040	1,040	1,040

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of 2, 3, and 4 quarters prior to birth. The estimation sample includes means by municipality for years including 2008-2017 where municipalities with cells including less than 25 observations are dropped. Estimates corresponding adjust for treatment, outcome and covariate histories during the five-year period prior to treatment, i.e., L=5. Five different methods are considered. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita), age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors correspond to block bootstrap replicates. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

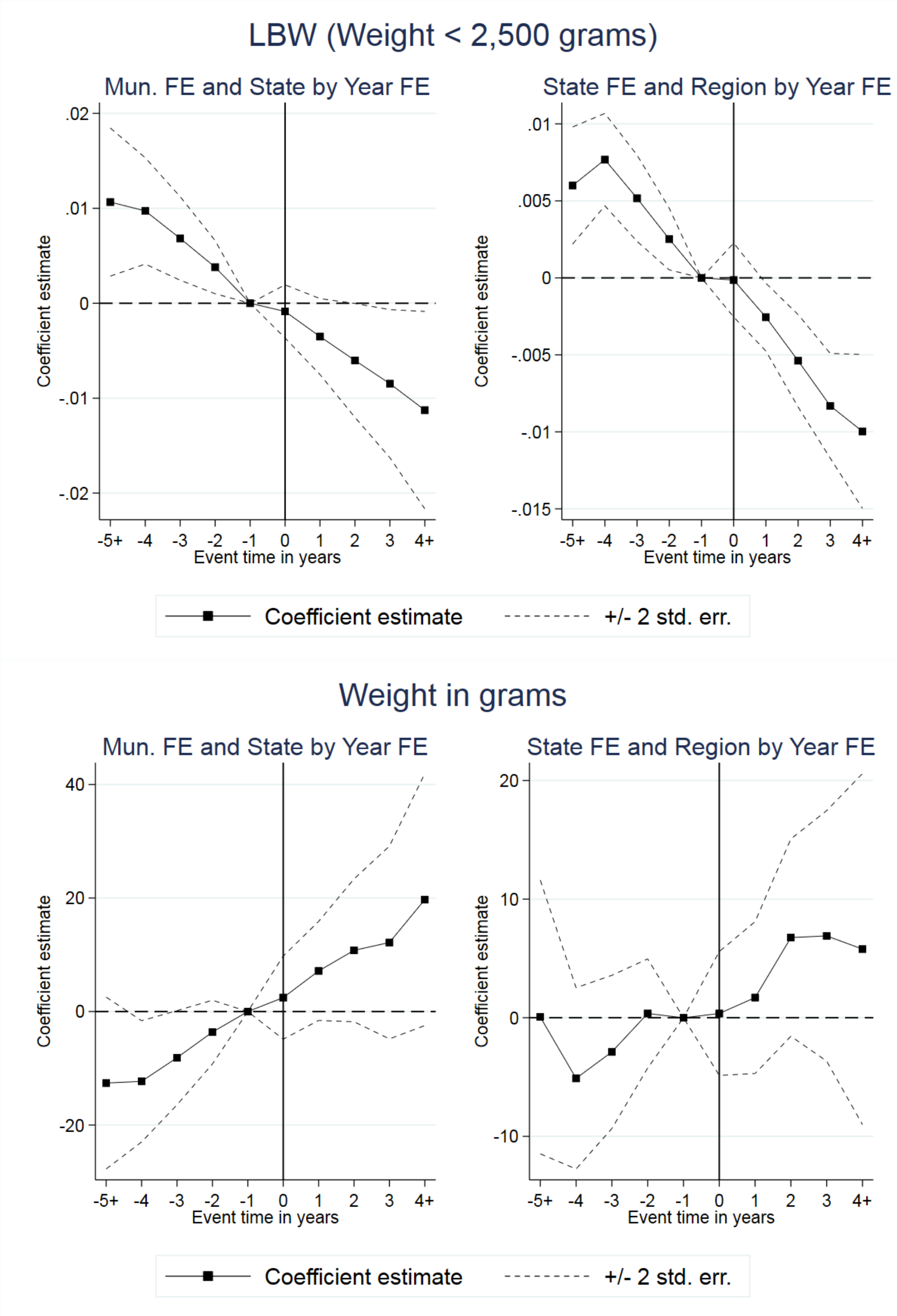
A4.5. Event study results

In this section I show the results from an event study on both the municipality-year means as well as the individual level dataset for years from 2008 to 2017. For this purpose, I fit the following equation:

$$y_{it} = \alpha + \sum_{j=-5}^{4} \pi_j 1(\tau_{imt} = j) + \gamma_m + \lambda_t + \gamma_{st} + \beta X_{imt} + \epsilon_{it} \quad (1)$$

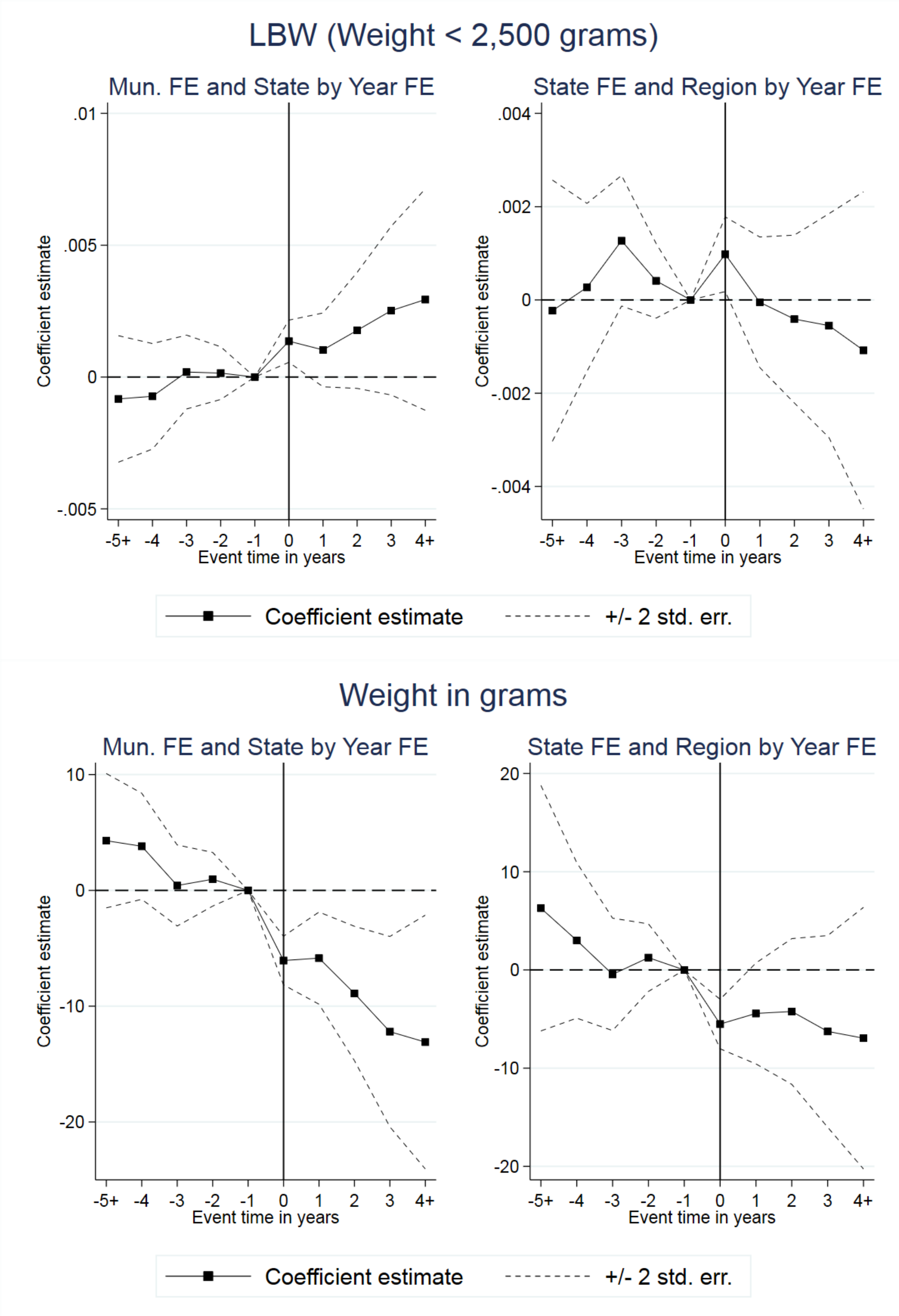
where τ_{imt} denotes the event year, defined so that $\tau_{imt} = 0$ if the outcome observed at municipality m and year t corresponds to the first year m was intervened, $\tau_{imt} = 1$ if the outcome corresponds to one year after the first year m was intervened, and so on. X'_{imt} is a vector of demographic control variables at the municipal and individual level, γ_t refers to year fixed effects, γ_m refers to municipality fixed effects, γ_{st} refers to state by year fixed effects to allow for different trends subnationally, α is the regression intercept, and ε_{it} represents the idiosyncratic error term. Outcomes corresponding to $\tau_{imt} \leq -1$ pertain to years prior to an intervention in the corresponding state. Coefficients are measured relative to $\tau = -1$, the omitted category. I consider a window of five years before and four years after the first intervention. The outermost indicators include all previous or subsequent periods beyond five years respectively. If SH is exogenous, coefficients corresponding to $\tau \geq 0$ should be statistically different from zero, and coefficients corresponding to $\tau \leq -1$ close to zero.

Figure A4.5.1. Event Study Analysis for Low Birth Weight and Birthweight using municipality by year means



Notes: Each figure plots coefficients from an event-study analysis. Coefficients are defined as years relative to the year SH is implemented in the municipality. The specification includes municipality fixed effects, year fixed effects, state by year fixed effects, and controls at the municipality and individual level. The estimation presented in this figure refers to the dataset with municipality-year means for years including 2008-2017.

Figure A4.5.2. Event Study Analysis for Low Birth Weight and Birthweight using individual observations



Notes: Each figure plots coefficients from an event-study analysis. Coefficients are defined as years relative to the year SH is implemented in the municipality. The specification includes municipality fixed effects, year fixed effects, state by year fixed effects, and controls at the municipality and individual level. The estimation sample includes individual observations from 2008-2017.

A4.6. Income Eligibility

The socioeconomic variables selected are both common to CUIS, a questionnaire that is conducted on virtually any household applying for registration to a social program, and MCS-ENIGH, a national survey on income and expenditures of households.[91] Based on these variables the government estimates the following linear specification:

$$ln\, Z_i = S_i{}'\beta + \epsilon_i. \qquad (1)$$

where ln refers to the natural logarithm, Z_i is income, $S_i{}'$ is the vector of socioeconomic variables and ϵ_i is the idiosyncratic error. The least squares model is estimated using observations from MCS-ENIGH leading to the following equation:

$$ln\widehat{Z_i} = S_i{}'\hat{\beta}. \qquad (2)$$

where $\widehat{Z_i}$ is the estimated income and $\hat{\beta}$ is the estimation of β. The income of a particular household is then calculated substituting the values of each of the selected variables for a particular household j into equation (2) to obtain the predicted income of that household:

$$ln\widehat{z_j} = s_j{}'\hat{\beta}. \qquad (3)$$

where $\widehat{z_j}$ is the predicted income for household j, $s_j{}'$ is the vector of selected variables with values for j and $\hat{\beta}$ is the same vector of coefficients from (2). Since, households sampled in CUIS are not necessarily sampled in MCS-ENIGH, this estimation corresponds to an out of sample prediction.[92] Once the government has the predicted income for a household it classifies it as income eligible if the predicted income lies below the threshold selected by the government uses for that year distinguishing between rural and urban areas. The criterium or threshold used varies from one year to another and is known as *Linea de Bienestar Minimo* (LBM). I replicate

[91] Encuesta Nacional de Ingreso y Gasto de los Hogares 2008 (MCS-ENIGH 2008). Cédula Única de Información Socioeconómica (CUIS).
[92] From the government perspective, estimating income using this methodology and not obtaining it directly from a survey reduces chances of manipulation by recipients.

this methodology using the MCS-ENIGH and the corresponding thresholds for each year in my period of analysis.

Since MCS-ENIGH is released every two years I use the information from MCS-ENIGH from a particular year on the vital statistics information of that year and the following to make the out of sample prediction.[93] Instead of using household demographics included in CUIS to predict household income, I use the demographics associated with mothers included in birth records to predict household income. These demographics include age, education level, marriage status, size of locality, health insurance status, type of health insurance, working status, and type of job, all of which are characteristics observed in both the vital statistics and the MCS-ENIGH datasets.

Table A6.1 shows the prediction accuracy within sample. Based on the list of socioeconomic variables from the official methodology I select an analogous set of variables available in the socioeconomic information in birth records. According to Table A6.1, around 80% of the observations are correctly classified as eligible or ineligible, and around 20% are incorrectly classified as eligible or ineligible. The predictive accuracy of my methodology is similar to that reported by the government for 2012, where around 83% of the observations are correctly classified and 17% are incorrectly classified, SEDESOL (2015).

Using this indicator, I implement a triple difference in difference comparing eligible to ineligible individuals within treated locations:

$$y_{itm} = \gamma_0 + Eligibility\ (LBM) + \gamma_1 SH_{itm} \otimes Eligibility\ (LBM) \quad (1)$$

$$+\gamma_3 X_{itm} + \gamma_m + \gamma_t + e_{itm},$$

[93] I use 2008 MCS-ENIGH for 2008 and 2009, 2010 MCS-ENIGH for 2010 and 2011, 2012 MCS-ENIGH for 2012 and 2013, and so forth.

where y_{itm} represents the outcome variable; SH_{itm} takes the value of 1 if the birth was exposed to the program; X_{itm} is a vector of individual and municipal level control variables; γ_m represent municipality fixed effects; γ_t represents year fixed effects, and e_{itm} is the error term. If the program had a beneficial impact, we would expect that eligible individuals do better in places that are exposed to the program as opposed to those that are in unexposed locations. As it can be observed from Tables A6.2, it appears that this is not necessarily the case. To discard this possibility, I test for specifications that allow for more variation by not controlling for municipality and year fixed effects. Instead, Table A6.3 shows the results from controlling for state and year fixed effects while controlling for subnational trends, using region by year fixed effects, as well as the poverty indexes used to select municipalities into each stage of SH. These results show that the program was beneficial for income eligible women in exposed locations relative unexposed locations.

Table A4.6.1. Income Eligibility Estimation Accuracy

Model	Classification	
	Correct	Incorrect
	(1)	(2)
2008	86%	14%
2010	81%	19%
2012	82%	18%
2014	82%	18%
2016	87%	13%

Each row represents accuracy measures for within sample predictions using information from MCS-ENIGH for different years. The first column shows the fraction of women correctly categorized to be eligible or ineligible. The second column indicates the fraction of women incorrectly categorized as eligible or ineligible.

Table A4.6.2 Linear Regression Estimates on the Effect of SH on Birthweight Outcomes, interacting treatment condition with income eligibility

`	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
A. Birthweight < 2,500 grams				
SH	0.00036	-0.00066*	-0.00042	0.00087**
	(0.00038)	(0.00039)	(0.00046)	(0.00042)
Eligibility (LBM)	0.00155***	0.00141**	0.00141**	0.00136**
	(0.00057)	(0.00057)	(0.00056)	(0.00055)
SH ⊗ Eligibility (LBM)	0.00081	0.00123	0.00134*	0.00162**
	(0.00075)	(0.00077)	(0.00079)	(0.00079)
Dep. var. mean	0.06558	0.06558	0.06558	0.06558
N. of obs.	17,023,336	17,023,336	17,023,336	17,023,336
B. Birthweight in grams				
SH	-5.19***	-1.76*	-0.47	-5.17***
	(0.99)	(0.93)	(1.06)	(1.07)
Eligibility (LBM)	-11.50***	-11.46***	-11.56***	-11.58***
	(1.41)	(1.39)	(1.38)	(1.36)
SH ⊗ Eligibility (LBM)	-1.71	-1.93	-1.82	-1.93
	(1.72)	(1.72)	(1.74)	(1.78)
Dep. var. mean	3147.61	3147.61	3147.61	3147.61
N. of obs.	17,023,336	17,023,336	17,023,336	17,023,336
Year fixed effects	X	X	X	X
Mun. fixed effects	X	X	X	X
Covariates	X	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of one quarter prior to birth, each column shows the estimate corresponding to each of these timings. Eligibility (LBM) is a dummy variable that takes the value of 1 if an individual is income eligible and 0 otherwise. The estimation sample includes individual level observations from 2008 to 2017. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate, percentage of income eligible population, and average income per capita) each interacted with a linear time trend, age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors are clustered at the municipality level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table A4.6.3 Linear Regression Estimates on the Effect of SH on Birthweight Outcomes, interacting treatment condition with income eligibility

`	SH implemented as of X quarters prior to birth			
	1 qrt	2 qrts	3 qrts	4 qrts
A. Birthweight < 2,500 grams				
SH	0.00003	-0.00094**	-0.00083	0.00026
	(0.00045)	(0.00045)	(0.00052)	(0.00050)
Eligibility (LBM)	0.00304***	0.00286***	0.00283***	0.00270***
	(0.00061)	(0.00060)	(0.00059)	(0.00058)
SH ⊗ Eligibility (LBM)	-0.00167**	-0.00136*	-0.00140*	-0.00114
	(0.00080)	(0.00082)	(0.00083)	(0.00085)
Dep. var. mean	0.06558	0.06558	0.06558	0.06558
N. of obs.	17,023,336	17,023,336	17,023,336	17,023,336
B. Birthweight in grams				
SH	-4.92***	-1.67	-0.57	-4.39***
	(1.46)	(1.45)	(1.65)	(1.56)
Eligibility (LBM)	-16.36***	-16.23***	-16.24***	-16.14***
	(1.89)	(1.86)	(1.85)	(1.80)
SH ⊗ Eligibility (LBM)	2.74	2.69	2.98	2.91
	(2.13)	(2.14)	(2.20)	(2.23)
Dep. var. mean	3147.61	3147.61	3147.61	3147.61
N. of obs.	17,023,336	17,023,336	17,023,336	17,023,336
Year fixed effects	X	X	X	X
State fixed effects	X	X	X	X
Region by year fixed effects	X	X	X	X
2010 Municipality Poverty Indexes	X	X	X	X
Covariates	X	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on SH implementation dummy. The treatment is assigned as of one quarter prior to birth, each column shows the estimate corresponding to each of these timings. Eligibility (LBM) is a dummy variable that takes the value of 1 if an individual is income eligible and 0 otherwise. The estimation sample includes individual level observations from 2008 to 2017. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita) each interacted with a linear time trend, age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors are clustered at the municipality level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

A4.7. Effect of SH on women already pregnant at the time of the introduction

The objective of the following specifications is to difference off treatment by time of exposure for women that were already pregnant at the time of introduction. I center the analysis on women living in the first set of municipalities treated, i.e. the first stage. In addition, this potentially corrects for residential section bias of women moving to municipalities they anticipated to be treated in later stages. I alternatively use women from each of the other stages. The main sample to be analyzed consists of births occurring since the first month the program was launched in the first stage until 40 weeks ahead, the typical duration of pregnancy. Succinctly, I use births that occurred from April of 2013 to January of 2014 only in first stage municipalities.[94] I start by running the following specification:

$$y_{itm} = \gamma_0 + \gamma_1 SH2Trim_{\,itm} + \gamma_2 SH1Trim_{itm} + \gamma_3 X_{itm} + \gamma_m + e_{itm}, \qquad (1)$$

Where, y_{itm} represents the outcome variable; $SH1Trim_{\,mt}$ takes the value of 1 if the baby was born in October, November, December of 2013 or January of 2014 and 0 otherwise; $SH2Trim_{\,mt}$ takes the value of 1 if the baby was born in July, August or September of 2013 and 0 otherwise; $SH3Trim_{\,mt}$, the omitted category, takes the value of 1 if the baby was born in April, May or June of 2013 and 0 otherwise; X_{itm} is a vector of individual control variables; γ_m represent municipality fixed effects; and e_{itm} is the error term. If the program was effective among these women, those receiving the treatment for a longer period, i.e. births occurring farther ahead from the starting date, should show better results.

In addition to the main specification I also interact the independent variables with the indicator for individual level income eligibility:

[94] First stage municipalities launch the program on 4/1/2013 (4/1/2013 + 40 weeks = 1/6/2014).

$$y_{itm} = \gamma_0 + Eligibility\ (LBM) + \gamma_1 SH2Trim\ _{itm} \otimes Eligibility\ (LBM) + \qquad (2)$$

$$\gamma_2 SH1Trim_{itm} \otimes Eligibility\ (LBM) + \gamma_3 X_{itm} + \gamma_m + e_{itm}.$$

Table A4.7.1 Linear Regression Estimates on the Effect of SH on Birthweight Outcomes, sensitivity to time of introduction for already pregnant women during the first stage of implementation

	(1)	(2)	(3)
A. Birthweight < 2,500 grams			
SH- 1st trimester	-0.00332***	-0.00326***	-0.00331***
	(0.00062)	(0.00062)	(0.00061)
SH- 2nd trimester	-0.00204***	-0.00175**	-0.00151**
	(0.00072)	(0.00069)	(0.00069)
Dep. var. mean	0.06087	0.06087	0.06075
N. of obs.	835719	835719	783148
B. Birthweight in grams			
SH- 1st trimester	7.48***	6.26***	5.91***
	(1.35)	(1.35)	(1.39)
SH- 2nd trimester	0.34	-2.12	-2.29*
	(1.43)	(1.30)	(1.31)
Dep. var. mean	3140.20	3140.20	3138.91
N. of obs.	835719	835719	783148
Mun. fixed effects		X	X
Covariates			X

Notes: Each column is from a separate regression of the outcome variable on SH implementation dummy. Sample consists of births occurring since the first month the program was launched in the first stage until 40 weeks ahead in time. Treatment is defined based on the time of birth relatively to the start of the program in April of 2013. $SH - 1st\ Trimester$ takes the value of 1 if the baby was born in October, November, December of 2013 or January of 2014 and 0 otherwise; $SH - 2nd\ Trimester$ takes the value of 1 if the baby was born in July, August or September of 2013 and 0 otherwise; $SH - 3rd\ Trimester$, the omitted category, takes the value of 1 if the baby was born in April, May or June of 2013 and 0 otherwise. Eligibility (LBM) is a dummy variable that takes the value of 1 if an individual is income eligible and 0 otherwise. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita) each interacted with a linear time trend, age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors are clustered at the municipality level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Table A4.7.2 Linear Regression Estimates on the Effect of SH on Birthweight Outcomes, sensitivity to time of introduction for already pregnant women during the first stage of implementation

	(1)	(2)	(3)
A. Birthweight < 2,500 grams			
Eligibility (LBM)	-0.01000***	-0.00164	-0.00110
	(0.00178)	(0.00138)	(0.00169)
SH- 1st trimester	-0.00369***	-0.00357***	-0.00362***
	(0.00065)	(0.00064)	(0.00064)
SH- 2nd trimester	-0.00211***	-0.00179**	-0.00177**
	(0.00077)	(0.00074)	(0.00074)
SH- 1st trimester ⊗ Eligibility (LBM)	0.00390**	0.00352**	0.00330*
	(0.00176)	(0.00174)	(0.00175)
SH- 2nd trimester ⊗ Eligibility (LBM)	0.00293	0.00317	0.00314
	(0.00206)	(0.00206)	(0.00204)
Dep. var. mean	0.06075	0.06075	0.06075
N. of obs.	783,148	783,148	783,148
B. Birthweight in grams			
Eligibility (LBM)	-12.96	-8.45**	-11.70***
	(9.78)	(3.33)	(4.01)
SH- 1st trimester	8.37***	6.72***	6.78***
	(1.50)	(1.49)	(1.47)
SH- 2nd trimester	1.12	-1.41	-1.60
	(1.64)	(1.47)	(1.47)
SH- 1st trimester ⊗ Eligibility (LBM)	-9.98***	-8.30**	-7.09*
	(3.80)	(3.65)	(3.69)
SH- 2nd trimester ⊗ Eligibility (LBM)	-7.51*	-8.22**	-8.16**
	(4.26)	(4.06)	(4.06)
Dep. var. mean	3138.91	3138.91	3138.91
N. of obs.	783,148	783,148	783,148
Mun. fixed effects		X	X
Covariates			X

Notes: Each column is from a separate regression of the outcome variable on SH implementation dummy. Sample consists of births occurring since the first month the program was launched in the first stage until 40 weeks ahead in time. Treatment is defined based on the time of birth relatively to the start of the program in April of 2013. $SH - 1st\ Trimester$ takes the value of 1 if the baby was born in October, November, December of 2013 or January of 2014 and 0 otherwise; $SH - 2nd\ Trimester$ takes the value of 1 if the baby was born in July, August or September of 2013 and 0 otherwise; $SH - 3rd\ Trimester$, the omitted category, takes the value of 1 if the baby was born in April, May or June of 2013 and 0 otherwise. Eligibility (LBM) is a dummy variable that takes the value of 1 if an individual is income eligible and 0 otherwise. Controls include 2010 municipality variables (log of population, percentage of land in farming, percentage of population five years or younger, percentage of population 65 years or older, unemployment rate and average income per capita) each interacted with a linear time trend, age, years of education, marriage status, insurance availability, per capita public investments, and per capita social assistance transfers. Standard errors are in parentheses. Standard errors are clustered at the municipality level. *Coefficient is significant at 10% level. **Coefficient is significant at 5% level. ***Coefficient is significant at 1% level.

Bibliography

Abadie, A., Athey, S., Imbens, G.W. and Wooldridge, J., 2017. When Should You Adjust Standard Errors for Clustering? (No. w24003). National Bureau of Economic Research.

Abadie, A., Diamond, A. and Hainmueller, J., 2010. Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American statistical Association, 105(490), pp.493-505.

Aceves-Martins, M., Llauradó, E., Tarro, L., Solà, R. and Giralt, M., 2016. Obesity-promoting factors in Mexican children and adolescents: challenges and opportunities. Global health action, 9(1), p.29625.

Ajzenman, N., Galiani, S. and Seira, E., 2015. On the distributive costs of drug-related homicides. The Journal of Law and Economics, 58(4), pp.779-803.

Alderman, H., Gentilini, U. and Yemtsov, R. eds., 2017. The 1.5 Billion People Question: Food, Vouchers, or Cash Transfers?. World Bank Publications.

Almond, D., Chay, K.Y. and Lee, D.S., 2005. The costs of low birth weight. The Quarterly Journal of Economics, 120(3), pp.1031-1083.

Almond, D., Hoynes, H.W. and Schanzenbach, D.W., 2011. Inside the war on poverty: The impact of food stamps on birth outcomes. The Review of Economics and Statistics, 93(2), pp.387-403.

Amarante, V., Manacorda, M., Miguel, E. and Vigorito, A., 2011. Do cash transfers improve birth outcomes? Evidence from matched vital statistics, social security and program data(No. w17690). National Bureau of Economic Research.

Arias, J. and Esquivel, G., 2012. A note on the side effects of the war on drugs: labor market outcomes in méxico. Unpublished working paper.

Attanasio, O. and Vera-Hernandez, M., 2004. Medium-and long run effects of nutrition and child care: evaluation of a community nursery programme in rural Colombia.

Attanasio, O., Gómez, L.C., Heredia, P. and Vera-Hernández, M., 2005. The short-term impact of a conditional cash subsidy on child health and nutrition in Colombia.

Atuesta, L.H. and Paredes, D., 2016. Do Mexicans flee from violence? The effects of drug-related violence on migration decisions in Mexico. Journal of Ethnic and Migration Studies, 42(3), pp.480-502.

Auditoria Superior de la Federación (ASF), 2013. Mexico, Mexico City. Secretaría de Desarrollo Social, Sistema Nacional para la Cruzada Contra el Hambre, Auditoría de Desempeño: 13-0-20100-07-0275. Retrieved on July 25, 2019 from https://www.asf.gob.mx/Trans/Informes/IR2013i/Documentos/Auditorias/2013_0275_a.pdf

Auditoria Superior de la Federación (ASF), 2014. Secretaría de Desarrollo Social Cruzada Nacional contra el Hambre Auditoría de Desempeño: 14-0-20100-07-0226 DS-058. Retrieved on July 25, 2019 from https://www.asf.gob.mx/Trans/Informes/IR2014i/Documentos/Auditorias/2014_0226_a.pdf

Auditoria Superior de la Federación (ASF), 2015. Secretaría de Desarrollo Social, Cruzada Nacional contra el Hambre Auditoría de Desempeño: 15-0-20100-07-0278, 278-DS. Retrieved on July 25, 2019 from https://www.asf.gob.mx/Trans/Informes/IR2015i/Documentos/Auditorias/2015_0278_a.pdf

Auditoria Superior de la Federación (ASF), 2016. Secretaría de Desarrollo Social, Cruzada Nacional contra el Hambre, Auditoría de Desempeño: 16-0-20100-07-0258, 258-DS. Retrieved on July 25, 2019 from https://www.asf.gob.mx/Trans/Informes/IR2016ii/Documentos/Auditorias/2016_0258_a.pdf

Auditoria Superior de la Federación (ASF), 2017. Secretaría de Desarrollo Social, Cruzada Nacional contra el Hambre. Auditoría de Desempeño: 2017-0-20100-07-0263-2018, 263-DS. Retrieved on July 25, 2019 from https://www.asf.gob.mx/Trans/Informes/IR2017b/Documentos/Auditorias/2017_0263_a.pdf

Barber, Sarah L., and Paul J. Gertler. "The impact of Mexico’s conditional cash transfer programme, Oportunidades, on birthweight." Tropical Medicine & International Health 13.11 (2008): 1405-1414.

Barham, Tania. "A healthier start: the effect of conditional cash transfers on neonatal and infant mortality in rural Mexico." Journal of Development Economics 94.1 (2011): 74-85.

Basu, S. and Pearlman, S., 2017. Violence and migration: evidence from Mexico’s drug War. IZA Journal of Development and Migration, 7(1), p.18.

Behrman, J.R. and Rosenzweig, M.R., 2004. Returns to birthweight. Review of Economics and statistics, 86(2), pp.586-601.

BenYishay, A. and Pearlman, S., 2013. Homicide and Work: The Impact of Mexico's Drug War on Labor Market Participation.

Bernal, R., Fernández, C., Flórez Nieto, C.E., Gaviria, A., Ocampo, P.R., Samper, B. and Sánchez, F., 2009. Evaluation of the early childhood program Hogares Comunitarios de Bienestar in Colombia. Available at SSRN 1486209.

Bharadwaj, P., Eberhard, J. and Neilson, C., 2013. Health at birth, parental investments and academic outcomes. University of California at San Diego Working Paper Series, pp.1862-1891.

Björntorp, P. and Rosmond, R., 2000. Obesity and cortisol. Nutrition, 16(10), pp.924-936.

Björntorp, P., 2001. Do stress reactions cause abdominal obesity and comorbidities? Obesity reviews, 2(2), pp.73-86.

Black, R.E., Victora, C.G., Walker, S.P., Bhutta, Z.A., Christian, P., De Onis, M., Ezzati, M., Grantham-McGregor, S., Katz, J., Martorell, R. and Uauy, R., 2013. Maternal and child undernutrition and overweight in low-income and middle-income countries. The lancet, 382(9890), pp.427-451.

Blomberg, M.I. and Källén, B., 2010. Maternal obesity and morbid obesity: the risk for birth defects in the offspring. Birth Defects Research Part A: Clinical and Molecular Teratology, 88(1), pp.35-40.

Boney, C.M., Verma, A., Tucker, R. and Vohr, B.R., 2005. Metabolic syndrome in childhood: association with birth weight, maternal obesity, and gestational diabetes mellitus. Pediatrics, 115(3), pp.e290-e296.

Bose, M., Oliván, B. and Laferrère, B., 2009. Stress and obesity: the role of the hypothalamic–pituitary–adrenal axis in metabolic disease. Current opinion in endocrinology, diabetes, and obesity, 16(5), p.340.

Brooks, A.M., Byrd, R.S., Weitzman, M., Auinger, P. and McBride, J.T., 2001. Impact of low birth weight on early childhood asthma in the United States. Archives of pediatrics & adolescent medicine, 155(3), pp.401-406.

Brown, R. and Velásquez, A., 2017. The effect of violent crime on the human capital accumulation of young adults. Journal of Development Economics, 127, pp.1-12.

Brown, R., 2015. The Mexican drug war and early-life health: The impact of violent crime on birth outcomes. Department of Economics, University of Colorado Denver.

Brown, R., Montalva, V., Thomas, D. and Velásquez, A., 2017. Impact of violent crime on risk aversion: Evidence from the Mexican drug war (No. w23181). National Bureau of Economic Research.

Carleton, T. and S. Hsiang (2016). "Social and Economic Impacts of Climate", Science, 353, 6304, aad9837

Caro, F.B., Hernández, E.Y.R., Fajardo, K.D.G., Viveros, S.S. and Torres, R.M., 2018. Nivel de Seguridad Alimentaria en beneficiarios de Comedores Comunitarios del programa Cruzada Nacional contra el Hambre (México). Revista española de nutrición comunitaria= Spanish journal of community nutrition, 24(3), p.6.

Casper, R.C., 1998. Depression and eating disorders. Depression and anxiety, 8(S1), pp.96-104.

Castillo, J.C., Mejia, D. and Restrepo, P., 2013. Illegal drug markets and violence in Mexico: The causes beyond Calderón. Universidad de los Andes typescript.

Cawley, J. and Meyerhoefer, C., 2012. The medical care costs of obesity: an instrumental variables approach. Journal of health economics, 31(1), pp.219-230.

Chávez, V.A. and Pérez-Gil, R.F., 2010. Composición de alimentos. Valor nutritivo de los alimentos de mayor consumo.

Chou, S.Y., Grossman, M. and Saffer, H., 2004. An economic analysis of adult obesity: results from the Behavioral Risk Factor Surveillance System. Journal of health economics, 23(3), pp.565-587.

Ciardi, C., Jenny, M., Tschoner, A., Ueberall, F., Patsch, J., Pedrini, M., Ebenbichler, C. and Fuchs, D., 2012. Food additives such as sodium sulphite, sodium benzoate and curcumin inhibit leptin release in lipopolysaccharide-treated murine adipocytes in vitro. British journal of nutrition, 107(6), pp.826-833.

Clarke, D., Cortés Méndez, G. and Vergara Sepúlveda, D., 2018. Growing together: Assessing equity and efficiency in an early-life health program in chile.

Cnattingius, S., 2004. The epidemiology of smoking during pregnancy: smoking prevalence, maternal characteristics, and pregnancy outcomes. Nicotine & Tobacco Research, 6(Suppl_2), pp.S125-S140.

CONEVAL (National Council for the Evaluation of Social Development Policy- Consejo Nacional de Evaluacion de la Politica de Desarrollo Social Nacional), 2010. Methodology for Multidimensional Poverty Measurement in Mexico. Mexico City, Mexico. Retrieved on October 4, 2017 from http://www.coneval.org.mx/rw/resource/coneval/med_pobreza/MPMMPingles100903.pdf

CONEVAL, 2015. "Resultados Intermedios de la Cruzada Nacional contra el Hambre." Retrieved on May 31, 2018 from https://www.coneval.org.mx/Evaluacion/ECNCH/Documents/CONEVAL_%20Resultados%20intermedios_CNCH.pdf

CONEVAL, 2016a. "Poverty in Mexico and its states 2016 (Spanish)." Mexico City, Mexico. Retrieved on December 9, 2017 from http://www.coneval.org.mx/Medicion/MP/Paginas/Pobreza_2016.aspx.

CONEVAL, 2016b. "Medición de la Pobreza, Evolución de las dimensiones de pobreza." Retrieved on June 8, 2019 from https://www.coneval.org.mx/Medicion/Paginas/Evolucion-de-las-dimensiones-de-pobreza.aspx

Courtemanche, C.J., Pinkston, J.C., Ruhm, C.J. and Wehby, G.L., 2016. Can changing economic factors explain the rise in obesity?. Southern Economic Journal, 82(4), pp.1266-1310.

Cunha, J.M., 2014. Testing paternalism: Cash versus in-kind transfers. American Economic Journal: Applied Economics, 6(2), pp.195-230.

Currie, J. and Cole, N., 1993. Welfare and child health: The link between AFDC participation and birth weight. The American Economic Review, 83(4), pp.971-985.

Currie, J. and Gahvari, F., 2008. Transfers in cash and in-kind: Theory meets the data. Journal of Economic Literature, 46(2), pp.333-83.

Currie, J. and Hyson, R., 1999. Is the impact of health shocks cushioned by socioeconomic status? The case of low birthweight (No. w6999). National bureau of economic research.

Currie, J., 2009. Healthy, wealthy, and wise: Socioeconomic status, poor health in childhood, and human capital development. JoUrnal of economIc lIteratUre, 47(1), pp.87-122.

Cutler, D.M., Glaeser, E.L. and Shapiro, J.M., 2003. Why have Americans become more obese?. The Journal of Economic Perspectives, 17(3), pp.93-118.

De Bernabé, J.V., Soriano, T., Albaladejo, R., Juarranz, M., Calle, M.E., Martínez, D. and Domínguez-Rojas, V., 2004. Risk factors for low birth weight: a review. European Journal of Obstetrics & Gynecology and Reproductive Biology, 116(1), pp.3-15.

De la Miyar, J.R., 2015. Breaking Sad: Drug-Related Homicides and Mental Well-Being in Mexico. LACEA Working paper.

De Vriendt, T., Moreno, L.A. and De Henauw, S., 2009. Chronic stress and obesity in adolescents: scientific evidence and methodological issues for epidemiological research. Nutrition, Metabolism and Cardiovascular Diseases, 19(7), pp.511-519.

DeBono, N.L., Ross, N.A. and Berrang-Ford, L., 2012. Does the Food Stamp Program cause obesity? A realist review and a call for place-based research. Health & place, 18(4), pp.747-756.

Dell, M., 2015. Trafficking networks and the Mexican drug war. The American Economic Review, 105(6), pp.1738-1779.

Devaney, B.L., 2003. Program and services to improve the nutrition of pregnant women, infants and young children. Nutrition–Pregnancy, p.30.

DOF (Official Gazette of the Federation - Diario Oficial de la Federacion), 2013a. "DECRETO por el que se establece el Sistema Nacional para la Cruzada contra el Hambre (Spanish)." Mexico City, Mexico. Retrieved on November 25, 2016 from http://dof.gob.mx/nota_detalle.php?codigo=5285363&fecha=22/01/2013.

DOF (Official Gazette of the Federation - Diario Oficial de la Federacion), 2013b. "ACUERDO por el que se emiten las Reglas de Operación del Programa de Apoyo Alimentario, para el ejercicio fiscal 2014 (Spanish)." Mexico City, Mexico. Retrieved on December 15, 2016 from http://www.dof.gob.mx/nota_detalle.php?codigo=5328234&fecha=27/12/2013.

Enamorado, T., López-Calva, L.F. and Rodríguez-Castelán, C., 2014. Crime and growth convergence: Evidence from Mexico. Economics Letters, 125(1), pp.9-13.

Enamorado, T., López-Calva, L.F., Rodríguez-Castelán, C. and Winkler, H., 2016. Income inequality and violent crime: Evidence from Mexico's drug war. Journal of Development Economics, 120, pp.128-143.

Epel, E., Lapidus, R., McEwen, B. and Brownell, K., 2001. Stress may add bite to appetite in women: a laboratory study of stress-induced cortisol and eating behavior. Psychoneuroendocrinology, 26(1), pp.37-49.

Fanzo, J., Hawkes, C., Udomkesmalee, E., Afshin, A., Allemandi, L., Assery, O., Baker, P., Battersby, J., Bhutta, Z., Chen, K. and Corvalan, C., 2018. 2018 Global Nutrition Report: Shining a light to spur action on nutrition.

Figlio, D., Guryan, J., Karbownik, K. and Roth, J., 2014. The effects of poor neonatal health on children's cognitive development. The American Economic Review, 104(12), pp.3921-3955.

Fish, J.S., Ettner, S., Ang, A. and Brown, A.F., 2010. Association of perceived neighborhood safety on body mass index. American journal of public health, 100(11), pp.2296-2303.

Flegal, K.M., Graubard, B.I., Williamson, D.F. and Gail, M.H., 2005. Excess deaths associated with underweight, overweight, and obesity. Jama, 293(15), pp.1861-1867.

Foss, B. and Dyrstad, S.M., 2011. Stress in obesity: cause or consequence?. Medical hypotheses, 77(1), pp.7-10.

Gil, B., Méndez, O. and Sobrino, A., 2014. Food policy and local participation: A case study of'Cruzada Nacional Contra el Hambre'. In Conference Paper. Workshop on the Ostrom Workshop (Vol. 5, pp. 18-21).

Golan, E., Stewart, H., Kuchler, F., Dong, D. and Kirlin, J.A., 2008. Can low-income Americans afford a healthy diet?. Amber Waves, 6(5), p.26.

Golpea la violencia al turismo en Chapala, 2012, May 22, El Universal. Accessed April 24, 2017, http://archivo.eluniversal.com.mx/estados/85953.html.

Gutiérrez-Romero, R. and Oviedo, M., 2014. The good, the bad and the ugly: The socio-economic impact of drug cartels and their violence in Mexico (No. wpdea1407).

Hernández-Borbolla, M., 2018. Peña y Calderón suman 234 mil muertos y 2017 es oficialmente el año más violento de la historia reciente de México. Huffington Post México (23 noviembre 2017). Retrieved from https://www.huffingtonpost.com.mx/2017/11/23/pena-y-calderon-suman-234-mil-muertos-y-2017-es-oficialmente-el-ano-mas-violento-en-la-historia-reciente-de-mexico_a_23285694/ on121218

Hill, J.O. and Melanson, E.L., 1999. Overview of the determinants of overweight and obesity: current evidence and research issues. Medicine and science in sports and exercise, 31(11 Suppl), pp.S515-21.

Hoynes, H., Miller, D. and Simon, D., 2015. Income, the earned income tax credit, and infant health. American Economic Journal: Economic Policy, 7(1), pp.172-211.

Hoynes, H., Page, M. and Stevens, A.H., 2011. Can targeted transfers improve birth outcomes?: Evidence from the introduction of the WIC program. Journal of Public Economics, 95(7), pp.813-827.

Hoynes, H.W. and Schanzenbach, D.W., 2009. Consumption responses to in-kind transfers: Evidence from the introduction of the food stamp program. American Economic Journal: Applied Economics, 1(4), pp.109-139.

Huesca-Reynoso, L., Lopez-Salazar, R. and Palacios Esquer, M.D.R., 2016. The Food Support Program and the Comprehensive Social Policy in the Crusade Against Hunger in Mexico.

REVISTA MEXICANA DE CIENCIAS POLITICAS Y SOCIALES, 61(227), pp.379-407.
Imai, K., Kim, I.S. and Wang, E., 2019. Matching Methods for Causal Inference with Time-Series Cross-Sectional Data.
Inchauste, G. E. Dabla-Norris and M. Gradstein (2005) “What Causes Firms to Hide Output” IMF Working Paper 05/160, Washington, DC.
Jarillo, B., Magaloni, B., Franco, E. and Robles, G., 2016. How the Mexican drug war affects kids and schools? Evidence on effects and mechanisms. International journal of educational development, 51, pp.135-146.
Jiménez-Cáliz, E. 2017, “Desde 2006, 10 mil denuncias de abusos cometidos por el Ejército” Milenio, January 12, 2017. Accessed on August 22, 2018 http://www.milenio.com/politica/2006-10-mil-denuncias-abusos-cometidos-ejercito.
Kiecolt-Glaser, J.K., Habash, D.L., Fagundes, C.P., Andridge, R., Peng, J., Malarkey, W.B. and Belury, M.A., 2015. Daily stressors, past depression, and metabolic responses to high-fat meals: a novel path to obesity. Biological psychiatry, 77(7), pp.653-660.
Kramer, M.S., 1987. Determinants of low birth weight: methodological assessment and meta-analysis. Bulletin of the world health organization, 65(5), p.663.
Kroker-Lobos, M.F., Pedroza-Tobías, A., Pedraza, L.S. and Rivera, J.A., 2014. The double burden of undernutrition and excess body weight in Mexico. The American journal of clinical nutrition, 100(6), pp.1652S-1658S.
Lakdawalla, D., Philipson, T. and Bhattacharya, J., 2005. Welfare-enhancing technological change and the growth of obesity. The american economic review, 95(2), pp.253-257.
Laven, G.T. and Brown, K.C., 1985. Nutritional status of men attending a soup kitchen: a pilot study. American journal of public health, 75(8), pp.875-878.
Law, C.M. and Shiell, A.W., 1996. Is blood pressure inversely related to birth weight? The strength of evidence from a systematic review of the literature. Journal of hypertension, 14(8), pp.935-942.
Leddy, M.A., Power, M.L. and Schulkin, J., 2008. The impact of maternal obesity on maternal and fetal health. Reviews in obstetrics and gynecology, 1(4), p.170.
Leroy, J.L., Gadsden, P., Rodriguez-Ramirez, S. and De Cossío, T.G., 2010. Cash and in-kind transfers in poor rural communities in Mexico increase household fruit, vegetable, and micronutrient consumption but also lead to excess energy consumption. The Journal of nutrition, 140(3), pp.612-617.
Lim, S.S., Dandona, L., Hoisington, J.A., James, S.L., Hogan, M.C. and Gakidou, E., 2010. India's Janani Suraksha Yojana, a conditional cash transfer programme to increase births in health facilities: an impact evaluation. The Lancet, 375(9730), pp.2009-2023.
Loayza N V, Servén L, Sugawara N (2009) Informality in Latin America and the Caribbean. The World Bank Policy Research Working Paper 4888. World Bank, Washington, DC.
Lucas, A., Morley, R. and Cole, T.J., 1998. Randomised trial of early diet in preterm babies and later intelligence quotient. Bmj, 317(7171), pp.1481-1487.
Luppino, F.S., de Wit, L.M., Bouvy, P.F., Stijnen, T., Cuijpers, P., Penninx, B.W. and Zitman, F.G., 2010. Overweight, obesity, and depression: a systematic review and meta-analysis of longitudinal studies. Archives of general psychiatry, 67(3), pp.220-229.
Mangge, H., Summers, K., Almer, G., Prassl, R., Weghuber, D., Schnedl, W. and Fuchs, D., 2013. Antioxidant food supplements and obesity-related inflammation. Current medicinal chemistry, 20(18), pp.2330-2337.

Márquez-Padilla, F., Pérez-Arce, F. and Rodríguez-Castelán, C., 2015. The (Non-) Effect of Violence on Education: Evidence from The 'War on Drugs' in Mexico. Oliver G, Wardle J, Gibson EL (2000) "Stress and food choice: a laboratory study." Psychosomatic Medicine, 62(6): 853–65.

Martínez, M.C.V., Romero, D.R. and Cardona, M.C.J., 2016. Blindly measurement. Performance evaluation in the National Crusade against Hunger. Gestión y Análisis de Políticas Públicas, (16).

Metcoff, J., Costiloe, P., Crosby, W.M., Dutta, S., Sandstead, H.H., Milne, D., Bodwell, C.E. and Majors, S.H., 1985. Effect of food supplementation (WIC) during pregnancy on birth weight. The American Journal of Clinical Nutrition, 41(5), pp.933-947.

Ministry of Health, 2017. "Alive newborns database." Mexico City, Mexico. Accessed on September 21, 2017 from http://www.dgis.salud.gob.mx/contenidos/basesdedatos/std_nacimientos_gobmx.html.

Moffitt, R.A. and Moffitt, R. eds., 2003. Means-tested transfer programs in the United States (pp. 199-290). Chicago: University of Chicago Press.

Natalie, P.G., Benito, S.I., Beatriz, C., Lomelí, Z., Arturo, T.D. and Alfonso, M.M., 2018. Impact of the SINHAMBRE Community Kitchen Program on malnutrition in rural Chiapas through the Food Security approach. Población y Salud en Mesoamérica, 16(1), pp.44-76.

OCDE (Organisation for Economic Co-operation and Development), 2019. Social and Welfare Issues, Social Expenditure Database. Retrieved on June 9, 2019 from https://www.oecd.org/social/expenditure.htm

Paes-Sousa, R., Santos, L.M.P. and Miazaki, É.S., 2011. Effects of a conditional cash transfer programme on child nutrition in Brazil. Bulletin of the World Health Organization, 89, pp.496-503.

Pelletier, D.L. and Frongillo, E.A., 2003. Changes in child survival are strongly associated with changes in malnutrition in developing countries. The Journal of nutrition, 133(1), pp.107-119.

Pfutze, T., 2014. The effects of Mexico's Seguro Popular health insurance on infant mortality: An estimation with selection on the outcome variable. World Development, 59, pp.475-486.

Philipson, T.J. and Posner, R.A., 1999. The long-run growth in obesity as a function of technological change (No. w7423). National bureau of economic research.

PRESIDENCIA, 2015. Mexico City, Mexico. "Resultados de Sin Hambre (Spanish)." Retrieved on August 2, 2016 from https://www.gob.mx/epn/articulos/resultados-de-sin-hambre.

Ranabir, S. and Reetu, K., 2011. Stress and hormones. Indian journal of endocrinology and metabolism, 15(1), p.18.

Robles, G., Calderón, G. and Magaloni, B., 2013. The economic consequences of drug trafficking violence in Mexico. Poverty and Governance Series Working Paper, Stanford University.

Rosenzweig, M.R. and Zhang, J., 2009. Do population control policies induce more human capital investment? Twins, birth weight and China's "one-child" policy. *The Review of Economic Studies*, *76*(3), pp.1149-1174.

Rosenzweig, M.R. and Zhang, J., 2013. Economic growth, comparative advantage, and gender differences in schooling outcomes: Evidence from the birthweight differences of Chinese twins. Journal of Development Economics, 104, pp.245-260.

Rosmond, R., Dallman, M.F. and Björntorp, P., 1998. Stress-related cortisol secretion in men: relationships with abdominal obesity and endocrine, metabolic and hemodynamic abnormalities. The Journal of Clinical Endocrinology & Metabolism, 83(6), pp.1853-1859.

Rossin-Slater, M., 2013. WIC in your neighborhood: New evidence on the impacts of geographic access to clinics. Journal of Public Economics, 102, pp.51-69.

Rytter, M.J.H., Kolte, L., Briend, A., Friis, H. and Christensen, V.B., 2014. The immune system in children with malnutrition—a systematic review. PloS one, 9(8), p.e105017.

Santaliestra-Pasías, A.M., Rey-López, J.P. and Moreno Aznar, L.A., 2013. Obesity and sedentarism in children and adolescents: what should be bone?. Nutricion hospitalaria, 28(5).

SEDESOL (Ministry of Social Development – Secretaria de Desarrollo Social), 2013a. "Specific Guidelines of the Program 'Comedores Comunitarios', within the framework of the Crusade against Hunger (Spanish)." Mexico City, Mexico. Retrieved on November 25, 2016 from https://www.gob.mx/cms/uploads/attachment/file/11055/Lineamientos_Prog_Comedores_Comunitarios.pdf.

SEDESOL, 2013b. National Crusade against Hunger Technical Note, 2013. "Technical note on the National Crusade against Hunger selection procedure of the priority 400 municipalities (Spanish)." Mexico City, Mexico. Retrieved on November 25, 2016 from http://www.sedesol.gob.mx/work/models/SEDESOL/Cruzada/4_Nota_Tecnica_Del_Procedimiento_De_Seleccion_De_Los_400_Municipios_Prioritarios_De_La_Cnch.pdf.

SEDESOL, 2013c. Target population of the National Crusade Against Hunger, Information Memo, National Crusade Against Hunger, (Spanish). Mexico City, Mexico. Retrieved on October 1, 2016 from http://www.coneval.org.mx/Medicion/MP/Paginas/Medicion-de-la-pobreza-municipal-2010.aspx.

SEDESOL, 2013e. Diagnóstico del diseño de la Cruzada Nacional contra el Hambre. México. Política Social de Nueva Generación y Cruzada Nacional contra el Hambre: Documento conceptual. México (versión para discusión V.140813) (mimeo). Retrieved on August 9, 2019 from http://www.sedesol.gob.mx/work/models/SEDESOL/PDF/POLITICA_SOCIAL_DE_NG_Y_CNCH.pdf

SEDESOL, 2014. National Crusade against Hunger Technical Note, 2014. "National Crusade against Hunger second stage municipalities' selection procedure (Spanish)." Mexico City, Mexico. Retrieved on November 25, 2016 from http://sinhambre.gob.mx/wp-content/uploads/2014/05/Seleccion_Municipios_de_la_Segunda_Etapa_de_la_CNCH.pdf.

SEDESOL, 2015. "Lineamientos de Evaluación de Condiciones Socioeconómicas de los Hogares." Mexico City, Mexico. Retrieved on December 2, 2019 from http://www.normateca.sedesol.gob.mx/work/models/NORMATECA/Normateca/1_Menu_Principal/2_Normas/2_Sustantivas/Lineamientos_Evaluacion_CSH/Anexo_A_Lineamientos_2015.pdf

Sepúlveda, Jaime, et al. "Aumento de la sobrevida en menores de cinco años en México: la estrategia diagonal." Salud Pública de México 49 (2007): s110-s125.

Skoufias, E., Unar, M. and González-Cossío, T., 2008. The impacts of cash and in-kind transfers on consumption and labor supply: Experimental evidence from rural Mexico. The World Bank.

Solera, C. and González-Antonio, H. “La violencia transformó por completo sus vidas” Excelsior, August 13, 2012. Accessed on April 24, 2017 http://www.excelsior.com.mx/2012/08/13/nacional/853135.

Strouse, C., Perez-Cuevas, R., Lahiff, M., Walsh, J. and Guendelman, S., 2016. Mexico's Seguro Popular Appears To Have Helped Reduce The Risk Of Preterm Delivery Among Women With Low Education. Health Affairs, 35(1), pp.80-87.

Strully, K.W., Rehkopf, D.H. and Xuan, Z., 2010. Effects of prenatal poverty on infant health: state earned income tax credits and birth weight. American Sociological Review, 75(4), pp.534-562.

Stults-Kolehmainen, M.A. and Sinha, R., 2014. The effects of stress on physical activity and exercise. Sports medicine, 44(1), pp.81-121.

Sturm, R., 2002. The effects of obesity, smoking, and drinking on medical problems and costs. Health affairs, 21(2), pp.245-253.

Suter, P.M. and Tremblay, A., 2005. Is alcohol consumption a risk factor for weight gain and obesity?. Critical reviews in clinical laboratory sciences, 42(3), pp.197-227.

Tierra sin ley, frontera entre Zacatecas y Jalisco, 2011, September 26, El Universal. Accessed on April 24, 2017 http://archivo.eluniversal.com.mx/estados/82239.html.

Torres, S.J. and Nowson, C.A., 2007. Relationship between stress, eating behavior, and obesity. Nutrition, 23(11), pp.887-894.

Traversy, G. and Chaput, J.P., 2015. Alcohol consumption and obesity: an update. Current obesity reports, 4(1), pp.122-130.

UNICEF, 2018. “Informe Annual UNICEF Mexico 2018. Avances y desafíos para la niñez y la adolescencia en México.” Mexico City, Mexico. Retrieved on August 9, 2019 from https://www.unicef.org/mexico/informes/informe-anual-unicef-m%C3%A9xico-2018

United States Census Bureau, 2016, US Department of Commerce, Newsroom. Accessed July 1, 2017, https://www.census.gov/newsroom/facts-for-features/2016/cb16-ff16.html

Velásquez, A., 2010. The economic burden of crime: Evidence from Mexico. Economist.

Victora, C.G., Barros, F.C., Assunção, M.C., Restrepo-Méndez, M.C., Matijasevich, A. and Martorell, R., 2012. Scaling up maternal nutrition programs to improve birth outcomes: a review of implementation issues. Food and nutrition bulletin, 33(2_suppl1), pp.S6-S26.

Violencia en Ciudad Juárez atemoriza a todos, 2009, September 3, Univision. Accessed on April 24, 2017 http://www.univision.com/noticias/narcotrafico/violencia-en-ciudad-juarez-atemoriza-a-todos.

Watkins, M.L., Rasmussen, S.A., Honein, M.A., Botto, L.D. and Moore, C.A., 2003. Maternal obesity and risk for birth defects. Pediatrics, 111(Supplement 1), pp.1152-1158.

World Bank, 2019. Poverty & Equity Data Portal. Retrieved on June 8, 2019 from http://povertydata.worldbank.org/poverty/country/MEX

World Health Organization, 2017, Global Health Observatory Data Repository, Overweight. Accessed July 1, 2017, http://apps.who.int/gho/data/view.main.CTRY2430A?lang=en

World Health Organization, 2017, Global Health Observatory Data Repository, Obesity. Accessed July 1, 2017, http://apps.who.int/gho/data/node.main.A900A?lang=en

Yu, E. and Lippert, A.M., 2016. Neighborhood Crime Rate, Weight-Related Behaviors, and Obesity: A Systematic Review of the Literature. Sociology Compass, 10(3), pp.187-207.

Vita

Francisco Beltran Silva was born in Mexico City, Mexico. He received his bachelor's degree in Economics from Mexico Autonomous Institute of Technology (ITAM) in 2012. He received his master's degree in Economics from Georgia State University in 2019.

Francisco Beltran Silva joined the Department of Economics in the Andrew Young School of Policy Studies at Georgia State University in 2015. His research focuses on Development, Health, and Public Economics. He received the University Doctoral Fellowship, Second Century Initiative (2CI), from the Andrew Young School of Policy Studies at Georgia State University, and a Doctoral Scholarship from the Mexican National Council on Science and Technology (CONACYT).