Essays on Participant and Vendor Responses to Incentives in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC)

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Abstract

This dissertation studies how participants and food retailers involved in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) respond to the incentives created by the program. WIC's fixed quantity structure, stocking requirements, and clinic visits reflect costs and benefits for participants and retailers. These structures reflect broader incentives in the provision of safety net programs through private firms as in SNAP and Medicaid. This dissertation applies current econometric methods for estimating staggered adoption designs with panel data and partial identification under selection to administrative data on WIC in different policy settings.

The first essay examines how participants' use of their benefits changes if food retailers become unauthorized. US food assistance programs including WIC require that participants redeem benefits at authorized food retailers. Using a novel natural experiment in the WIC authorization of retailers, I find that a participant that loses access to an authorized retailer is less likely to participate in WIC and redeems a smaller share of their WIC benefits. These changes provide insight into the unintended consequences of food assistance program structure and how low-income individuals respond to changes in their food retail choice set.

Essay two evaluates the effect of the switch to Electronic Benefit Transfer (EBT) on the authorization outcomes of WIC vendors. WIC EBT aims to reduce stigma and transaction costs for participants but may increase costs for some vendors. This paper combines

novel administrative data from The Integrity Profile and the Store Tracking and Redemptions System with a new nationwide policy data on WIC EBT implementation. Using a staggered adoption difference-in-differences approach, we find the effects of the WIC EBT transition varied across different states. In sum, independent retailers are more likely to become unauthorized following WIC EBT implementation. The experience of the financial services provider contracted to implement WIC EBT by state may mediate the magnitude of the effect of EBT on vendor authorization outcomes.

The third essay returns to the question of how participants in food assistance programs respond to changes in the set of authorized food retailers, in this case examining whether participants' health outcomes and program health objectives are affected by changes to the set of authorized WIC vendors. This chapter examines birthweight as a primary health outcome for WIC participants. Birthweight affects children's later life outcomes and represents WIC's mission to support the health of pregnant people, infants, and children. One challenge with observing birthweight data through WIC administrative records is that a child's birthweight is only observed in the administrative data if the parent enrolls the child in WIC at birth. This implies substantial selection into treatment. Many infants' birthweight is not observed because the infant was not enrolled in WIC at birth. To account for this selection into the sample, I estimate bounds on the local average treatment effect of a vendor DQ on birthweight using Lee (2009) bounds. The paper finds minimal effects of changes in the vendor choice set from disqualifications on birthweight measures including low birth weight (LBW) and very low birthweight (VLBW). Results show no large effect even after adjusting for gestational age - whether or not the pregnancy went to term. This evidence can be interpreted in two ways. One possibility is that vendor DQ effects on participation and benefit redemption of pregnant people are not substantial enough to cause changes in birthweight. An alternative explanation is that participation effects are may cause changes in birthweight, but that in leaving the program themselves parents also do not enroll children, meaning that

the effect on birthweight cannot be detected.

Each of these three papers contribute to our understanding of how economic agents involved in the food retail environment and WIC respond to the incentives created by WIC's program rules. This research demonstrates that individuals (and to some extent firms) alter their behavior in response to the incentives of the program. These incentives can oppose each other, leading to ambiguous responses by agents, particularly firms that may face constraints set by broader chain policy. Taken together, the research in this dissertation provides evidence on when policy changes and structures in safety net programs lead to unintended consequences for economic agents, demonstrating that common program designs can affect households and firms in ways that are not expected by program administration.

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Essay 1

WIC Participant Responses to Vendor Disqualification

The opinions expressed herein represent those of the author and do not necessarily represent the position of the State of California.

1.1 Introduction

For safety net programs that provide in-kind benefits, participants generally first enroll in the program at a government agency, and then obtain their (physical) benefits. For two large U.S. food assistance programs - the Supplemental Nutrition Assistance Program (SNAP) and the Special Supplemental Nutrition program for Women, Infants, and Children (WIC) - participants obtain their food benefits from food retailers like grocery, convenience, or super stores. These private retailers redeem food instruments issued to participants by local agencies for the supplemental foods allowed. While the overall effect of food assistance program participation on health and economic outcomes is well-studied, the role of private actors, especially food retailers, as determinants of participation from the cycle described above are less well understood.

This paper studies a novel natural experiment that quasi-randomly removes authorized food retailers from the choice set of WIC participants. I study how the removal of authorized WIC food retailers affects participation in WIC and participants' use of their food benefits. I use unique administrative data from California for FY 2016-2019, a period when other changes to WIC policy were constant in the state. Administrative data allow me to directly link participants to salient vendors, weakening assumptions about which participants are affected by changes in WIC vendors.[1] Other safety net programs - including SNAP, Medicaid, and Medicare - require private actors to provide their services. This paper contributes to an emerging branch of the safety net literature, which studies how non-government actors affect participants. On the one hand, these actors are essential to access for participants. On the other hand, private actors may engage in bad behavior that increases costs to the safety net program and may deter participants from engaging with the program. I find that participants that are exposed to the disqualification of an authorized WIC retailer are 32% less likely to actively participate in WIC after the disqualification. For participants that remain on the program, their dollar value of benefits redeemed decreases by $5 per month (12% of the pre-treatment mean), although these effects may be due to switching to vendors with lower prices rather than obtaining fewer supplemental foods. I find substantial effects of changes in access to authorized food retailers on WIC participation. These findings demonstrate the importance of access to authorized private food retailers to obtain food assistance. Assuming this relationship holds for retailers that enter or exit WIC for reasons other than disqualification, these results indicate that access to authorized food vendors substantially affects WIC's benefits for eligible individuals.

My findings are consistent with previous work that found decreases in participation as a result of decreasing access to authorized food retailers that are necessary to obtain food ben-

[1] As noted in Cuffey and Beatty (2021), the effect of changes in the set of authorized vendors on participants' behavior is not necessarily bounded well by ZIP or county lines.

efits (Meckel 2020; Meckel, Rossin-Slater, and Uniat 2021). My findings are also consistent with other evidence from the food environment literature, which finds that some proposed approaches to policy questions in the food environment, like food taxes, are likely to have heterogeneous and regressive effects (Just and Gabrielyan 2016). While Allcott et al. (2019) find that preferences are a much more substantial driver of food demand than the food environment, participants in food assistance programs face constraints on their choice set of vendors. WIC participants face an even tighter set of constraints, with fewer authorized food retailers and a limited set of supplemental foods. This paper demonstrates that within this smaller choice set, the removal of an authorized food retailer affects participants' choices about remaining in the program and using their benefits. I show that private firms mediate WIC participants' access to their benefits and the program. Importantly, this suggests that access to private firms may affect participation and use of benefits in other safety net programs including SNAP and Medicaid.

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) is a large safety net program which provides targeted food and nutrition counseling benefits to at-risk pregnant and postpartum women and their children under 5. Nearly 50% of infants born in the US each year receive WIC benefits. In FY 2019, 6.4 million WIC participants received food benefits worth more than $3.14 billion (Food and Nutrition Service 2021). California has the most WIC participants of any state, with 928,943 participants in FY 2019. To obtain food benefits, participants must shop at food retailers that are authorized by their state WIC agency to redeem WIC benefits. California authorizes more than 4,000 food retailers to redeem WIC benefits each year. During the sample period (FY 2016-2019), between 4,200 and 4,500 vendors were authorized each year.

A substantial literature documents positive effects of WIC nutrition benefits on participants (see reviews in Hoynes and Schanzenbach (2016), Bitler and Seifoddini (2019), and Khanani

et al. (2010)). Infants whose mothers participate in WIC experience higher birthweight for gestational age (Rossin-Slater 2013) and these effects are concentrated among more disadvantaged households (Bitler and Currie 2005). Studying the roll-out of WIC, Hoynes, Page, and Stevens (2011) find that WIC initiation increased average birthweight and decreased the share of infants born with low birth weight. Birthweight gains can translate into substantial later-life health, education, and income improvements (Almond and Currie 2011a; Almond and Currie 2011b). Mothers who participate in WIC also gain more weight during pregnancy, have overall shorter hospital stays at delivery, and initiate prenatal care earlier in their pregnancy (Bitler and Currie 2005). Not all of these effects are directly caused by food benefits, since participants have access to nutrition education and breastfeeding counseling in addition to food benefits (Rossin-Slater 2013). However, participants make decisions about participation based on food benefits (Jacknowitz and Tiehen 2010) so that changing access to and use of food benefits can have indirect as well as direct effects.

State WIC agencies have the authority to remove vendors from authorization if they fail to comply with state or federal WIC regulations. Unauthorizing a vendor removes the retailer from the participants' choice set to redeem benefits. This paper examines how WIC participants' benefit use and program participation respond to changes in their vendor choice set. Using institutional details for support, I argue that removing a vendor from authorization is effectively random from the perspective of the participant. Studying how participants respond to this change in the set of available authorized retailers illustrates the role of the food environment in food assistance participants' use of their benefits.

The remainder of the paper provides institutional background (Section 2), describes the data used to estimate the results (Section 3), discusses the empirical methods (Section 4), lays out the results (Section 5), and concludes (Section 6).

1.2 WIC background

In this section, I describe WIC's institutional features for participants and food retailers. These details support assumptions that I make in the empirical model. I also define several of the processes that lead to my outcomes of interest and discuss their importance to WIC participants' outcomes and the benefits that are documented in other papers.

1.2.1 Participant certification

To be eligible for WIC, participants must satisfy several requirements. First, participants must have gross income less than or equal to 185% of the federal poverty guideline for their household. Any otherwise eligible individual who participates in SNAP, TANF, or Medicaid would satisfy WIC income requirements. To participate, women must be pregnant or no more than six months (if not breastfeeding) or 12 months (if partially or exclusively breastfeeding) postpartum. Children may participate until their fifth birthday. All participants must complete a nutritional risk assessment with a health professional and receive a "nutritionally at-risk" determination from this assessment. Direct experience of CDPH/WIC staff indicates that, effectively, all otherwise eligible individuals are nutritionally at-risk. A 2021 report by Insight Policy Research/Urban Institute for USDA FNS supports this experience, finding nutritional risk rates among otherwise eligible infants and pregnant women of 97%, 99.9% for children, and 100% for postpartum women (K. F. Gray et al. 2021). This implies that the income test is a binding constraint on participation while the nutritional risk requirement is effectively non-binding.

After their initial certification, pregnant women, infants, and children who will not turn 5 during their initial certification must complete a recertification procedure, including verifying income eligibility. Recertification timelines vary depending on the participant's category. For infants and children, recertification occurs yearly. Participants generally return to their

local WIC clinic for visits every three months.[2] At clinic visits, participants receive relevant nutrition and/or breastfeeding counseling for their stage of life. Clinic staff take anthropometric measurements and may ask participants questions about their health and diet at these visits to decide which food instruments are the best fit for participants. Participants obtain their food instruments (FIs) at clinic visits. FIs are each valid for one month. The valid start dates for the FIs are staggered between clinic visits.

I note that evidence from another US food assistance program - the Supplemental Nutrition Assistance Program (SNAP) - suggests that administrative hurdles like recertification are points at which many participants exit the program. A large share of these exits are by participants who remain eligible for the program (C. Gray 2019; Homonoff and Somerville 2020; Unrath 2021). Woelfel et al. (2004) finds that WIC participants identify recertification and appointments at clinics as significant barriers to using WIC services. It's plausible then that participants who are affected by a DQ would choose not to undertake any costs associated with recertification, which would remove them from the program approximately three months after their previous appointment. In this framework I assume that the change in the value of the program to participants that choose not to recertify is sufficiently large to outweigh benefits they would get from recertifying, holding the effort cost of recertifying costant. I expect that the full extent of participation changes may not show up in the data until all affected participants have passed their recertification point. This implies that the event window for participation after the disqualification should be at least three months. In Section 1.4 I describe my methods which allow for a six month adjustment period after the DQ to incorporate these lagged effects.

1.2.2 Benefit redemption

Certified participants take the FIs they receive at clinic visits to authorized WIC food retailers to redeem. When a FI is valid, the participant can exchange the FI for the allowed

[2]WIC participants receive up to three months worth of food instruments per clinic visit. However, various factors determine how frequently participants must return to the clinic to pick up benefits.

supplemental food(s) listed on the FI, in a similar manner to using a check. The participant selects their foods as if shopping normally. WIC provides a shopping guide; this and in-store signage can help participants find their approved supplemental foods. After selecting foods, the participants goes through checkout, separating WIC items from non-WIC items and handing the FI to the cashier. The participant signs the check, and then the cashier writes down the shelf prices of the foods, the date, and the total. The participant may need to show ID. The vendor sends the checks in to the Federal Reserve to be reimbursed.[3]

One type of FI works a bit differently. After 2009, WIC participants also receive a cash value voucher (CVV) as part of their benefits. This FI provides participants with a fixed dollar value to spend on fruits and vegetables rather than providing a fixed quantity of foods. This structure creates incentives for participants to respond to prices that do not exist with other FIs. CVVs also remove product choice (e.g. brand, flavor, and size) restrictions imposed by other FIs. Participants may find CVVs easier to use across different retailers for this reason. Anecdotal evidence from participants and WIC program staff indicate that stores that cater to WIC participants may bundle fruits and vegetables into amounts that match common CVV levels to facilitate those purchases, making these stores friendlier for CVV purchases.[4]

The reasoning for participants to change their redemption behavior after being exposed to a vendor DQ is similar to the reasoning for participants choosing not to recertify after a DQ - the marginal benefits of redeeming benefits have decreased and/or the marginal costs have increased. Changes to marginal benefits of obtaining supplemental foods at a different vendor after the DQ include an assortment of brands or types of authorized foods that do

[3]Since the end of my sample, California has transitioned to electronic benefit transfer technology for food instruments, loading benefits onto a debit card like instrument rather than issuing paper vouchers. See Section 1.3 for more detail.

[4]CVV values are fairly low, often $8 or $11, and can only be used for one transaction. The bundles avoid having substantial remaining balance on the FI or large out of pocket expenditures for bulk produce items that can be difficult to anticipate the price of.

not match the households' preferences, among other potential factors. One possibility is that participants were engaging in program fraud that benefited them with DQed vendors. In collaboration with CDPH/WIC's program integrity staff, we determine that participant fraud cannot feasibly entirely explain the magnitude of participation or redemption changes I observe. Changes to marginal costs of redeeming benefits include mismatch between the participant's native language and the language spoken at the store, a change in travel costs, and learning new locations and types of authorized foods available.

1.2.3 Vendor authorization

For both SNAP and WIC, participants redeem food benefits at authorized vendors. Food retailers must apply to receive SNAP or WIC authorization and meet requirements for authorization. Unlike SNAP, where vendor authorization, fraud determination, and sanctions are handled at the national level by USDA FNS, WIC vendor authorization and sanctions are managed at the level of state and tribal organization WIC agencies. These agencies have some latitude in determining requirements for vendors to be authorized, although there is a core authorization requirement set at the federal level. Vendors may be sanctioned for failing to comply with authorization requirements. I describe the key details of this process in Section 1.2.4. See Appendix 1.7 for more details on both vendor authorization and vendor sanctions.

Shelf price survey

One requirement for vendors during their authorization period is to complete a biannual survey of shelf prices. A CDPH/WIC representative contacts the vendor twice annually - once over a two week period in January/February and once over a two week period in July/August. The vendor responds with their minimum and maximum prices for a set of WIC authorized foods on the shelf on the day that they respond. The set of foods included in the survey is smaller than the set of all authorized foods. However, from this set of survey foods, I can construct 42 food instruments. These food instruments cover one third (33.8%)

of FIs redeemed in January, February, July, and August. From the minimum prices, I can construct an effective price floor for the full redemption of a food instrument by vendor peer group. Good coverage of these FIs in the shelf price survey window mean that a few months a year I have a reasonable measure of partial redemption.

1.2.4 Vendor sanctions

State agencies conduct various routine inspections of authorized vendors to monitor for violations of regulations. If a vendor is found to be non-compliant with either state or federal regulations during the course of an inspection, the vendor may be sanctioned. In this paper, I focus on one kind of sanction - disqualification (DQ). A DQ means that the vendor goes from being authorized to being unauthorized on the day of the DQ. Most common violations that lead to disqualifications are: losing SNAP authorization; displaying a pattern of overcharging; not posting prices; and not maintaining minimum stocking requirements.

CDPH/WIC's motivation when disqualifying and sanctioning vendors is to enforce the rules of the program. As noted above, many of these rules are intended to improve the participants' experience at the vendor. Others are meant for cost containment purposes - notably the maximum allowable department reimbursement (MADR). The MADR is a price ceiling that limits the redeemable value of supplemental foods. The MADR is specific to types of food instruments and groups of vendors. CDPH/WIC computes the MADR through a multiple week moving average of prices at full-line grocery stores and then adjusts prices based on the general cost level of the vendor group. Smaller vendors face a higher MADR while superstores have lower price ceilings. Vendors that are overcharging the state by requesting reimbursement for more than the shelf price for supplemental foods are raising their cost of food relative to a comparable competitor. Moving participants away from vendors that are overcharging lowers food costs. Lower food costs benefit other participants by increasing the amount of the block grant available to purchase food for other participants.

I focus on DQs because they create a sharp break in authorization status. In addition, DQs are caused by administrative actions of the state agency rather than an endogenous process of authorization/unauthorization driven by both demand for WIC benefits from participants and supply side factors. The common causes of disqualification listed above illustrate that many disqualifications occur for violations that participants are not feasibly involved in. For instance, it seems implausible that participants individually or collectively are the reason that vendors fail to correctly post prices or lose their SNAP authorization.[5] This suggests that participant demand is plausibly exogenous to the processes that cause vendor disqualification. Independence of participant behavior from DQs makes plausible the necessary assumption for the empirical approach I lay out in Section 1.4 that DQ timing is independent of unobservables. In addition, participants are unlikely to be aware of the sanction in process. Vendors are generally identified for disqualification from a set of high-risk indicators and from undercover and routine investigations by state workers. These actions are not observable to the participant. The vendor would have to reveal this information to the participant, requiring communication between the vendor and the participant, which I cannot entirely rule out. Vendors learn about the upcoming DQ prior to its effective date, although I cannot observe the timing of the notice of the DQ.

1.3 Data

Through an agreement with CDPH/WIC, I access administrative records on vendors, disqualifications, benefit issuance, benefit redemption, and participants. In addition, I am able to regularly communicate with WIC program staff to ask questions and obtain feedback. The administrative data is ideal to answer the question that I am interested in. Both the data and the institutional knowledge I gain from collaborating with CDPH/WIC staff contribute to the literature by allowing me to extend previous work to link participants and vendors and to deeply understand the program mechanisms and DQs. In this section I outline the

[5] As I discuss in the data section, I am also able to observe the violation(s) for which the vendor is disqualified, allowing me to conduct a robustness check excluding disqualifications for which participants are potentially involved.

CDPH/WIC data resources I use in this paper as well as any relevant insights from discussions with CDPH/WIC staff.

For all data, I use the months from October 2015 to June 2019. From April 2012 until June 2014, CDPH/WIC and vendors were undergoing some reorganization after a federal moratorium on vendor authorization .[6] During the moratorium CDPH/WIC also implemented cost containment strategies (Meckel, Rossin-Slater, and Uniat (2021) use this policy in their paper) and reworked the definitions for groupings of vendors to determine comparison units for price ceilings. The department also revised authorization criteria during this period. By October 2015, these changes were fully worked through. In June 2019 California began implementing electronic benefit transfer (EBT) technology with their authorized WIC vendors. Given evidence from other states (Hanks et al. 2018; Meckel 2020) that EBT implementation can affect vendor authorization and redemptions, I wanted to avoid confounding effects of disqualifications with EBT implementation and so end the sample prior to EBT implementation. In addition, data management systems for redemptions in particular changed with WIC EBT implementation, limiting my access to data post EBT.

1.3.1 Vendor data

For each vendor, the vendor records include a business name as well as a CDPH/WIC assigned vendor identifier. I observe some descriptive variables about the vendor, including the number of registers and the peer group of the vendor.[7] CDPH administrative files about vendors contain limited geographic information, including city, state, ZIP-5, and county. Finally, I observe the authorization and termination dates of the vendor.

[6] More information about the moratorium and related policies is available here: `https://www.calwic.org/storage/documents/webinars/VendorChangesCAWIC_7.16.2014.pdf`.

[7] Peer group is a CDPH/WIC designation that groups together vendors by type and size. Peer groups determine the relevant MADR. Peer group definitions include A50 status (vendors with $\geq 50\%$ of total sales from WIC benefit redemption); full-line grocery status (does the vendor stock enough varieties and amounts of dairy, fruit, vegetables, meats/poultry/seafood, cereals, and breads/tortillas), and the number of registers the vendor has.

1.3.2 Disqualification data

CDPH/WIC compiled and provided the universe of vendor disqualifications from 2014 through the end of 2019. This helps me avoid the problem of missing treatment timings for event study designs discussed in Schmidheiny and Siegloch (2019). Namely, in an event study or dynamic difference-in-differences design with K pre and post periods and outcome data from time 0 to time T, if treatment timings are observed for a period shorter than $-K$ to $T + K$, then some groups may be incorrectly assigned to untreated status when they are within K periods of treatment, biasing the estimate of the dynamic coefficients. More than 200 disqualifications[8] fall within the period such that I observe full data on controls and outcomes for at least six months prior to and after the disqualification.

Each disqualification record includes the vendor ID number - the same CDPH/WIC assigned ID as in the vendor files mentioned above and the redemption data I discuss below. I also observe the store name, city, and ZIP code again in this data. The CDPH/WIC Vendor Policy Unit records the type of violation for which the DQ occurred. California's Code of Regulations Title 22 determines the duration of the DQ based on the type of violation, so that I can infer the duration of the DQ from the violation type. Included in the data are the date at which the DQ becomes effective, whether the DQ was appealed and the outcome of the appeal, and in some cases I observe a date on which the vendor returned to WIC authorization.

1.3.3 Issuance data

Issuances represent the allocation of food instruments to WIC participants. Issuance generally happens at a WIC clinic, where the WIC staff provide the WIC participant with nutrition education, referrals to other relevant community services, and, if applicable, breastfeeding support. Supplemental foods are prescribed through the FI that best fits the nutritional needs of the participant.

[8] On average, fewer than 1 in 85 vendors will be DQed in a given year.

The FI issuance data detail which bundle of foods the FI contains. Food items contained in food instruments have specified sizes and varieties and in some cases specified brands.[9] The food instrument package may contain a single item or multiple food items.

The issuance data also detail the participant to whom the FI was issued, a CDPH/WIC assigned identifier for the individual food instrument, the date on which the FI was issued, and the valid start and end dates for the FI. Observing issuance data in addition to redemption data (described below) allows me to describe one dimension of participants' intensity of benefit use - the share of issued food benefits redeemed.

1.3.4 Redemption data

Redemption data connect participants to salient vendors. Redemption refers to the exchange of a food instrument for approved supplemental food(s) at an authorized vendor. All supplemental foods from a food instrument must be used at a single vendor on a single occasion, or any unredeemed supplemental foods are no longer available to the participant. However, households may receive multiple food instruments so that participants may redeem benefits at multiple vendors within a month. On average, participants who are affected by a DQ redeem benefits at 0.77 to 2.68 other non-DQed WIC authorized vendors in the months prior to their disqualification. More than 90% of WIC participants exposed to a DQ in my sample reveal a preference for a non-DQed WIC vendor prior to the DQ, indicating that they have a feasible alternative authorized vendor to redeem benefits. This outside option may or may not be able to entirely fit their preferences around WIC redemption, including location, open hours, brand selection, and customer service, among other features.

CDPH/WIC records on redemptions include both a CDPH/WIC assigned identifier for the

[9]See Figure 1.1 for the layout of an example WIC check, which highlights the specifications for types and sizes of WIC authorized supplemental foods. You can also see where the identifiers for the participant, the vendor, and the individual food instrument are located as well as the redemption date, the amount redeemed, the valid use dates, and identifying information about the food package.

participant redeeming the food instrument, the individual food instrument, and the vendor at which the FI is redeemed. This establishes a link between participants and vendors without making assumptions on the salience of vendors to participants that rely on geography or other proxies. In addition to the relevant identifiers, redemption data contain the amount the store charged for the FI redemption, the MADR for that FI, an approximate date on which the FI was redeemed, and the date on which the Federal Reserve reimbursed the vendor for the FI.

I note that the amount redeemed and MADR observed are at the food instrument level rather than at the food item level. I can only observe prices and price ceilings for the bundle of foods rather than individual items. This limits inference I can make about mark ups or price setting at the item level. I also cannot observe partial redemptions of FI. If I observe that a FI is redeemed for less than the MADR, I cannot immediately state whether that is because the vendor has prices set such that the full bundle of goods costs less than the MADR or the participant chooses not to redeem all of the items in the FI.

1.3.5 Participant data

CDPH/WIC administrative records on participants allow me to link participant identifiers to important certification and demographic information. CDPH/WIC assigns a unique identifier to each individual for the duration of their certification period. I refer to this identifier as the participant ID. This ID captures transitions across categories, food instruments, and certification periods as long as the participant remains in the program. I cannot identify the same participant returning to the program for a new spell, as the participant will receive a new participant ID. Certification data allow me to identify which participants are actively certified as authorized WIC participants. In addition, issuance data allow me to observe participants that are issued food instruments in a given month; having FI issued is the federal definition for participation. These certification or participation measures describe the extensive margin effects of salient vendor disqualification on WIC participants. In

addition, certification data allow me to impute approximate recertification dates for participants. Postpartum women and children turning 5 within a year of their current certification start date do not have any recertification date. With an approximate recertification date and some additional assumptions, I can estimate another extensive margin effect - whether WIC participants affected by a disqualification are less likely to complete their recertification procedure. Certification data include first ever certification date, current certification start and end dates, pregnancy end dates (for adult participants), participant's category (whether breastfeeding (B), a child (C), an infant (I), pregnant (P), or non-breastfeeding (N)).

Demographic data include the participant's race and ethnicity, a measure of income at time of certification, use of other safety net programs including Medi-Cal (California's Medicaid program), Temporary Assistance for Needy Families (TANF), and CalFresh - California's Supplemental Nutrition Assistance Program (SNAP). I also observe some WIC specific nutritional risk and health variables which I do not currently use in my analysis although they may be useful for future analyses of heterogeneity by participant risk.

Table 1.1 shows summary statistics of participants' characteristics by the month in which the participant is treated. In general participants in these treatment groups have fairly similar monthly income, household size, educational attainment, and participation in other assistance programs on average. TANF participation varies more on average across treatment groups than SNAP and Medi-Cal participation (Medi-Cal participation is not included in the table, but it's consistently at about 90% across treated participants). The treatment groups themselves contain quite different numbers of participants

1.4 Empirical methods

I use a stacked difference-in-differences methodology to evaluate the effect of salient vendor disqualifications on WIC participants. This approach uses ordinary least squares (OLS) to estimate models with participant and month of sample fixed effects (two-way fixed effects,

or TWFE) on treatment group specific subsamples. Estimating TWFE models on these subsamples ensures that the weights used to construct the average treatment effect across subsamples are positive and interpretable (Goodman-Bacon 2019; Callaway and Sant'Anna 2018; Chaisemartin and D'Haultfœuille 2020). For those who are interested in how the estiamtes from the stacked difference-in-difference approach compare to estimates using OLS on the full sample of data (pooled OLS, or POLS) and TWFE with the full treated sample, see Appendix 1.9.

In my setting, vendor disqualification occurs in 17 different months. I define treated participants as those who redeem benefits at a vendor that goes on to be disqualified, in the 6 months prior to the disqualification of the vendor. I'll compare participants that are exposed to a DQ that happens at month g to not-yet-treated participants. Not-yet-treated individuals are those that I ever observe redeeming food benefits at a vendor that goes on to be disqualified, but for whom the salient vendor is DQed at month $r \geq g + 6$.

I define treatment as a binary indicator that is equal to one for all periods that the salient vendor is disqualified. Treatment begins in the month in which the DQ starts. DQ duration ranges from 6 months to 6 years, so that treatment is turned "on" for all treated participants over the 6 month post-treatment horizon.

I construct treatment groups by the month in which the disqualification occurs. Although I observe the day of the disqualification, WIC benefits are valid for a month. This implies that the calendar month is the relevant time span for benefit issuance and redemption measurement. Participants are treated in the month in which their salient vendor experiences a disqualification. I limit my sample to participants that experience only a single disqualification, eliminating the possibility that a participant experiences multiple events during the sample for which the post-period for one event could potentially overlap with the pre-period

for another event.[10]

The main reason that I restrict my comparison units to not-yet-treated units is to increase the plausibility of the parallel trends assumption. In this setting, by assuming parallel trends I am assuming that the conditional expectation of month-to-month changes in the counterfactual (untreated) outcomes of the participants exposed to a vendor DQ in any month g is the same as the conditional expectation of month-to-month changes in the untreated outcomes of the comparison participants. I condition on variables that can affect the path of participation and benefit redemption. In particular, I hold constant the participant's type (child, infant, pregnant, breastfeeding, or non-breastfeeding postpartum) interacted with their certification start month and their salient milestones (birth month for children, pregnancy end date for adults) which jointly determine the number of months they can continue on WIC and when they would have to recertify. I also hold constant the WIC clinic they attend which represents the finest level of geography I observe. WIC clinics are a source of information on the WIC program for participants. Ensuring overlap in clinics decreases the probability that differences in outcomes between groups are driven by differences in information about the food retail environment or WIC benefits across clinics. With these conditioning variables, I maximize the match in counterfactual participation and redemption patterns between treated and control units.

A substantial and quickly developing literature documents the potential for bias away from the estimand in the estimate of the average treatment effect from computing a two-way fixed effects model across multiple treatment groups with staggered treatment timing. If treatment effects are not equal across all treatment groups, the weights assigned to treatment

[10]Some papers in the developing dynamic difference-in-differences literature, including Chaisemartin and D'Haultfœuille (2020), as well as several unpublished manuscripts (D. H. Sandler and R. Sandler 2014; Schmidheiny and Siegloch 2019), allow for multiple events given certain other assumptions hold. Participants who experience multiple DQs are a small share of the treated population ($< 5\%$) and they may have different unobservable characteristics than other treated participants.

groups by the OLS procedure aggregate the individual groups' treatment effect up to an estimate that is not equal to the parameter of interest (Chaisemartin and D'Haultfœuille 2020; Callaway and Sant'Anna 2018; Goodman-Bacon 2019). There are multiple approaches to resolving this challenge that require different assumptions, data structures, and computing systems. I implement the approach of Cengiz et al. (2019), which uses a stacked design, estimating the average treatment effect on the treated for each treatment group and then averaging them together to construct an overall average treatment effect. By estimating the treatment effects separately for each treatment group and then aggregating with weights chosen by the researcher, this procedure avoids the unknown and negative weighting problem of standard two-way fixed effects. To conduct inference on the average effect parameter, I use a percentile confidence interval constructed from a bootstrap procedure with 500 simulations.

The stacked estimation procedure is as follows:

1. I split the sample into the individual treatment groups. Treatment groups g are defined by the month in which the vendor is disqualified. Treatment groups include all participants that, in the previous 6 months, have redeemed benefits at a vendor that is disqualified in month g. Treatment groups also include all participants that are not-yet-treated in periods g through $g + 6$. These not yet treated participants redeem food benefits in the 6 months prior to month r where r is the month of disqualification for another vendor, and $r > g + 6 + 1$.

2. For individual treatment groups, I estimate the regression

$$y_{itg} = \hat{\alpha}_g + \hat{\beta}_g DQ_{it} + \hat{\delta}_{ig} + \hat{\gamma}_{tg} + \hat{\Xi}_g X_{itg} + \hat{\epsilon}_{itg}$$

   where y is an outcome - either a binary measure of participation, a continuous measure of redemptions (dollar value of benefits redeemed), a continuous measure of the quantity of food benefits obtained (share of benefits partially redeemed), or a continuous

measure of the price of food benefits obtained (share of benefits redeemed at MADR).

3. Compute a weighted average the treatment effects obtained from each regression $\hat{\beta}_g$ to obtain an average treatment effect $\overline{\hat{\beta}}$. Weight each $\hat{\beta}_g$ by its share of total observations.

4. To conduct inference on this $\overline{\hat{\beta}}$, take 500 samples s of size N with replacement, clustering by the month of treatment. For each sample, repeat the procedure from 1., 2., and 3. to obtain $\overline{\hat{\beta}}_s$. Order all $\overline{\hat{\beta}}_s$ from smallest to largest and find the 2.5^{th} and 97.5^{th} percentiles. I use these as the bounds of a 95% confidence interval on $\overline{\hat{\beta}}$.

The assumptions required for the validity of the stacked design procedure are that a conditional parallel trends assumption holds and that participants do not change their participation and redemption behavior prior to the treatment start month. I can write the conditional parallel trends assumption more formally as $E\left[Y^0_{i,t} - Y^0_{i,s} | \mathbf{X}_i, \mathbf{DQ}_i\right]$ equal for all t and s and all treatment groups. The no anticipation assumption, that potential outcomes of the treated units prior to the DQ are equal in expectation to their counterfactual outcomes in all periods prior to the DQ, can be written more formally as $E\left[Y^0_{i,t}\right] = E\left[Y^1_{i,t}\right]$ for all $t < g$. In Table 1.1 I compare summary statistics for pre-treatment demographic variables by treatment group, showing that in general households look relatively similar across treatment groups.

I estimate the stacked difference-in-difference specifications at the participant level for two main outcomes: a binary indicator that the participant is issued a FI in the calendar month and a continuous measure of the intensive margin of participation: the dollar value of food benefits redeemed. FI issuance is the official participation measure used by CDPH/WIC, making it a policy relevant outcome to consider. Dollar value of redemptions is an imperfect measure of how intensely a participant is using their benefits. Given that WIC is a fixed quantity program (with the exception of the cash value voucher, as discussed in Section 1.2.2), a change in the dollar value of benefits redeemed could indicate a change in prices rather than a change in quantity of supplemental foods obtained. However, without item

level transaction data (not available for California during my sample period), a more accurate measure of amount of benefits redeemed is not available.

I consider the magnitude of my estimates to evaluate whether changes in dollar value redeemed are substantial enough to plausibly constitute a change in quantity of supplemental foods obtained. I also estimate the effect of disqualifications on two additional outcomes that hold the quantity of goods redeemed and the value of goods redeemed constant to attempt to decompose the amount redeemed into price and quantity components. The first of these outcomes is the share of benefits partially redeemed. I use the shelf price survey data discussed in Section 1.2.3 to construct this measure for months where shelf price survey data is available. The share of benefits partially redeemed measures the proportion of FIs redeemed per households below the minimum price for the full bundle. More partial redemption means that households are not fully utilizing their supplemental foods. The second measure is the share of FIs redeemed at the MADR, described in Section 1.2.4. Redeeming benefits at the MADR may indicate a store that is high cost relative to its peers. Changing share of FIs redeemed at the MADR may reflect a change in the cost of benefits redeemed, although it may also indicate fewer items redeemed. In conjunction with the results on the share of partial redemptions, the share of items at the MADR can tell us something about vendor costs.

1.5 Results

Results in this paper use the stacked difference-in-differences method of Cengiz et al. (2019) to understand the effect of disqualifications on the set of California WIC participants affected by DQs between 2015 and 2019. These estimates suggest that vendor disqualifications affect WIC participation and redemption levels. I incorporate additional institutional knowledge to investigate the mechanisms behind redemption level change.

1.5.1 Extensive margin results

The first set of results examines how WIC participants respond to DQs on the extensive margin - being on WIC or maintaining WIC certification at all. Any participant who leaves WIC altogether is not receiving WIC supplemental foods or benefits from nutrition counseling or breastfeeding support. Individuals that no longer participate in WIC do not obtain benefits that accrue from clinic visits, such as breastfeeding support, nutrition counseling, or referrals to medical practitioners, in addition to no longer obtaining food benefits. To examine extensive margin effects, I use a binary measure of participation - whether or not the participant is issued a food instrument in the calendar month. Using this outcome, I estimate the procedure described in Section 1.4 on the set of all treated participants and not-yet-treated participants that satisfy the overlap condition described in Section 1.4, using Stata 17 and including covariates X_{it} for participant category, WIC clinic, and category interacted with certification start month and certification index month (birth month for non-adults and pregnancy end month for adults).

The results from this specification are in Figure 1.3. Although this graph may resemble a dynamic difference-in-difference specification, all of the results displayed here are static, averaging the treatment effect over all post-treatment periods (0 to 6 months after the DQ). The average treatment effect for each group is represented by a red dot. Red lines represent the analytical 95% confidence interval for each treatment group, constructed from standard errors clustered at the household level. The light blue bars show the size of each treatment group, measured by the number of participant IDs in the treatment group.

The weighted average treatment effect, averaging all the group-specific average treatment effects with weights applied by the number of treated units in each group, is represented by a dark blue line. The weighted average treatment effect shows that the probability of participating in WIC decreases on average by 30% after a DQ (relative to not-yet-treated

participants). The darker blue shaded area represents the area between the lower and upper bounds of the 95% confidence interval constructed from the bootstrap procedure. These estimates are relatively consistent across the treatment groups, and the bounds from the percentile confidence interval do not include zero, although the distribution is skewed towards zero. My results indicate that participants that experience a vendor DQ are significantly less likely to participate in WIC after the disqualification. The skew in the percentile confidence interval indicates that a subset of the DQs drives the magnitude of these results. However, estimates from each treatment group indicate that the effect is consistently negative although magnitudes vary.

1.5.2 Intensive margin results

The set of above results looked at the extensive margin - effects of a DQ on a binary measure of participation. In this section I estimate the effects of a DQ on an intensive margin outcome: the value of benefits redeemed, contingent on being issued FI. In order to be issued FI, participants must visit a clinic. This means that participants are obtaining the benefits of clinic visits regardless of their use of supplemental foods. In this section all participants in the sample are receiving FI throughout the sample window, so they are engaging in WIC at least in part.

The value of benefits redeemed theoretically depends on two things: the number of food items from the FI redeemed and the price of those food items. If prices stay the same and the participant redeems fewer items from the FI, the amount redeemed decreases. Policymakers and program administrators are interested in ensuring that participants redeem supplemental foods that they are issued. However, the data do not contain prices paid for individual food items or the number of food items redeemed out of the FI.[11] It is challenging to disentangle the effects of changing prices from changing redemption quantities. If prices

[11] I note that observing UPC level prices and quantities is possible with administrative data from states/periods with electronic benefit transfer in WIC, however California did not implement EBT technology until after the end of the study period.

shift as a result of changing vendors after a DQ, the direction of that shift is not immediately clear. Prices may decrease if the participant shifts from a vendor who has, for instance, been overcharging for WIC items to a non-overcharging vendor.[12] Or prices may increase, depending on the policy environment. Meckel (2020) finds evidence of increased prices for non-WIC participants after vendors leave WIC in Texas as a result of the EBT transition[13].

The results of implementing the stacked difference-in-difference procedure for the amount redeemed per participant are in Figure 1.4. The weighted average treatment effect of vendor disqualifications on amount redeemed per participant is a decrease of about $5 per month (6% of the pre-period redemption mean). Individual treatment groups' effects are less consistent for amount redeemed, but all the groups' estimates are negative or close to zero. The 95% confidence interval on this effect does not include zero, with the bounds fairly symmetric around the estimated WATE.

I examine two outcomes - a minimum and maximum price threshold - to attempt to disentangle these price and quantity components. I link in the shelf price survey discussed in Section 1.2.3 to construct a minimum price for all goods in a food instrument. This allows me to see if food instruments are only partly redeemed. I compare the amount redeemed for food instruments in the month of the survey at vendors like those surveyed to the shelf price indices to determine if the amount redeemed is likely to indicate a partial redemption - not purchasing all foods in the food instrument. I take the minimum prices for all items in the food instrument at each vendor and aggregate them to construct a minimum food instrument price by vendor. I then take the 25^{th} percentile of the minimum FI prices across vendors of the same size, and indicate if a participant redeemed a FI for less than the threshold.

[12] 30% of disqualifications in the data involve a vendor that is disqualified at least in part for overcharging.
[13] California and Texas have quite different WIC redemption environments - Texas requires WIC participants to redeem the lowest cost item available at a vendor.

Results with this outcome appear in Figure 1.5. The treatment effects vary in sign and magnitude across treatment groups, and the bounds on the weighted average treatment effect include zero. This indicates that on average partial redemptions do not change after vendor disqualification.

The maximum allowable department reimbursement (MADR) looks at the upper bound of prices to see how much overcharging changes in response to a vendor disqualification. The MADR is set by the department for all food instruments for vendors of a certain size. Benefits that are redeemed at the MADR are likely to be full redemptions at relatively high prices. If redemptions at the MADR fall after a DQ without corresponding increases in partial redemptions, it suggests decreasing overcharging for WIC participants rather than participants obtaining fewer supplemental foods.

Figure 1.6 shows the results from the MADR outcome. Overall, there is no significant change in the number of food instruments redeemed at the MADR after the vendor disqualification. Conditional on remaining in the program, if participants do not change the share of their benefits that are redeemed at the MADR, this suggests that the decrease in average benefits redeemed is not driven by a large shift away from vendors that strategically price foods to attain the MADR or that have high prices. There is variation in the treatment effects across treatment groups, suggesting that treatment group specific factors (like the composition of the violations for the vendors disqualified in that period) might affect the magnitude and direction of this effect.

In future versions of this work, I could also examine a specific case where it is somewhat easier to disentangle prices from quantities: infant formula FIs. Infant formula FIs contain only one product which is valuable and shelf stable, so that it is unlikely that participants partly redeem this FI. Changes in price should thus reflect vendor pricing changes rather

than quantity changes.

Overall, the results suggest that the main mechanism by which vendor disqualifications affect participants is that WIC participants are less likely to stay on the program after a salient vendor is disqualified. Participants who remain on the program redeem a lower dollar value of benefits. This could be the result of redeeming benefits at lower prices, or of obtaining fewer supplemental foods. These findings indicate that the availability of private firms to mediate access to a safety net program can affect participants' use of that program.

1.6 Conclusion

This project estimates the effect of store DQs on participant outcomes. I find that DQs decrease participants' probability of receiving any WIC benefits by 30%. For participants that continue to be issued FIs after the disqualification, I find that their dollar value of benefits redeemed drops by $5 per month, about 15% of pre-treatment levels. I use a unique set of administrative data to demonstrate that decreasing access to WIC authorized food retailers through disqualification decreases participation in and use of WIC benefits. The data allows me to link participants directly to DQed vendors. This paper shows that decreasing access to private actors that are necessary to use a safety net program can decrease use of the safety net by those that would otherwise would use the program. This result is relevant for many safety net programs that rely on private firms to provide benefits to participants, including SNAP and Medicaid.

The policy implications depend on the mechanisms by which disqualifications affect WIC participants and must weigh the costs of DQing a store (in terms of lost access) with the benefits of a DQ (in terms of reduced fraud, cost containment, and improved participant experience from eliminating bad actors). The main work of the paper quantifies costs from a DQ to participation and redemptions. Evidence on the benefits of WIC for participants implies that maintaining participation among eligible populations is important. Benefits from

cost containment would be relatively straightforward to measure by comparing average costs for redeeming benefits for participants shopping at a DQed store before and after the DQ. Other benefits – lower fraud and fewer bad actors – are more difficult to measure. Some of the policy implications of this work are likely to depend on how program integrity and cost containment behaviors change as a result of COVID-19 and the full implementation electronic benefit transfer (EBT) technology in all states.[14] However, the broad lessons from this project that reflect participants' marginal responses to changes in vendor access may hold even in a changing WIC environment. The results suggest that the set of available retailers can affect food assistance utilization of vulnerable households, particularly in a nutritionally at-risk population.

[14]USDA FNS imposed an October 2020 deadline for all states to transition to EBT (meaning that paper FIs are no longer used). Some states have not yet completed the transition, although most, including California, have. EBT technology allows states to examine prices and redemption behavior in ways that paper food instruments did not (e.g. at the UPC level). This granularity may affect cost containment and program integrity actions. Evidence from X. Li (2020) indicates that EBT reduced the incidence of redemption of unauthorized foods in Oklahoma and Meckel (2020) finds that EBT implementation in Texas reduced vendor fraud.

Figures

Figure 1.1: Sample WIC check

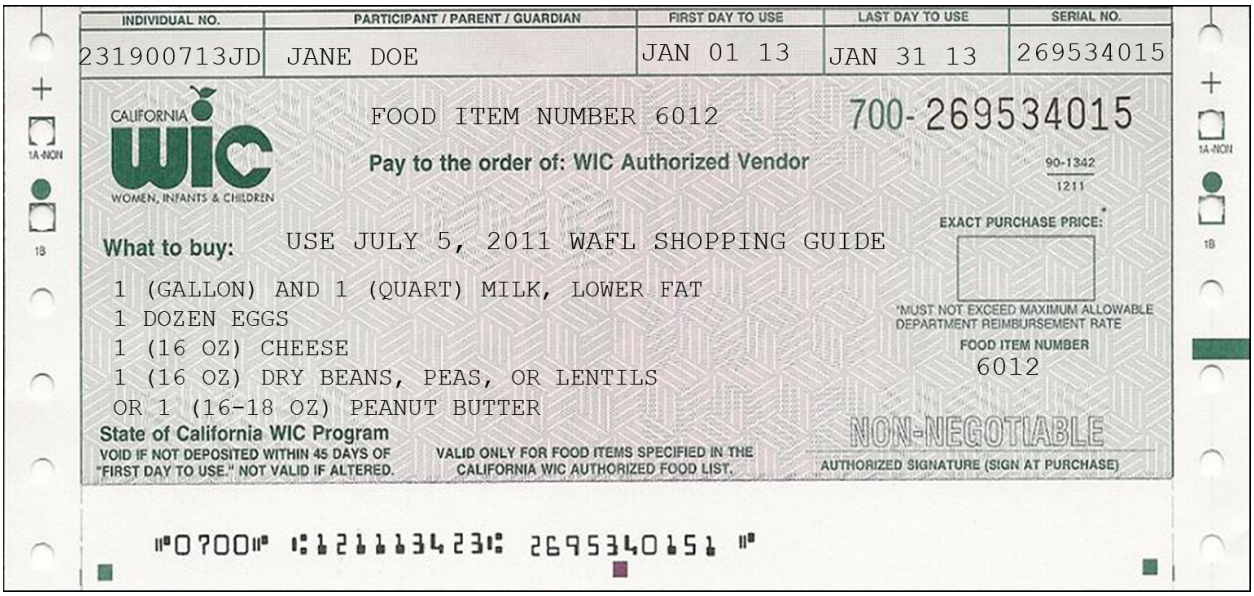


Figure 1.2: Vendor summary statistics over time

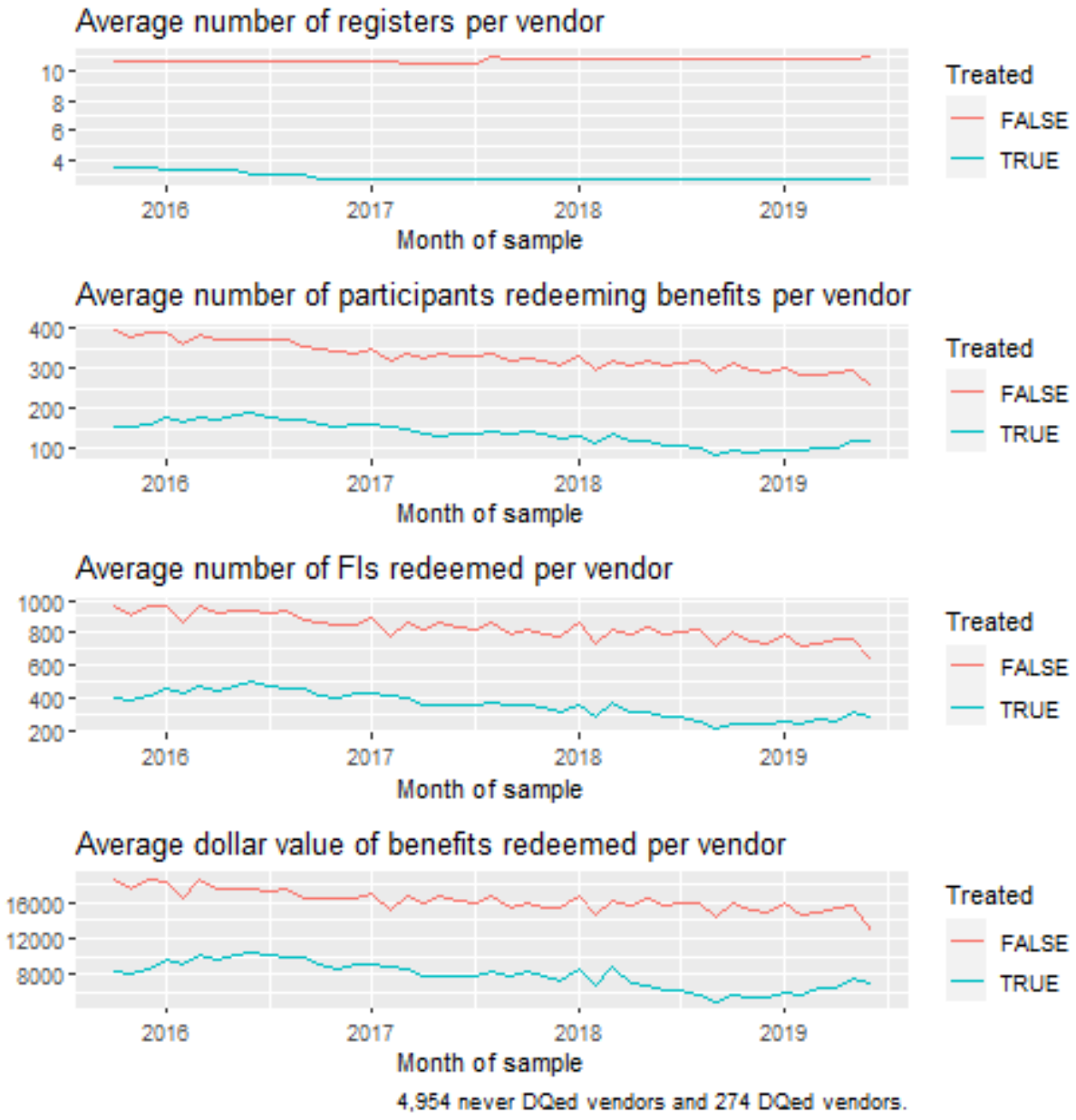


Figure 1.2: Vendor summary statistics over time (cont'd)

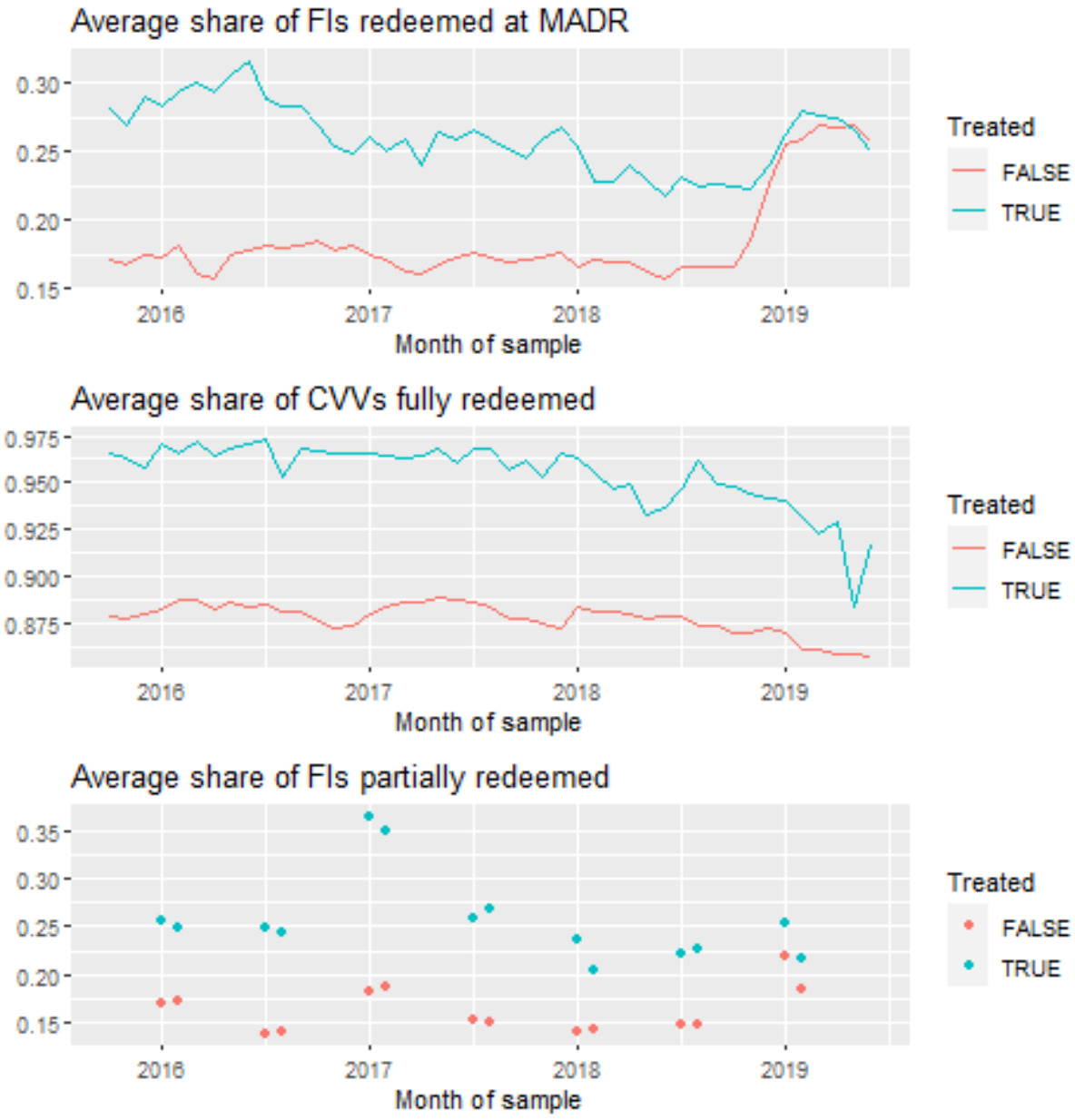


Figure 1.3: Effect of vendor disqualification on probability of actively participating in WIC by treatment group

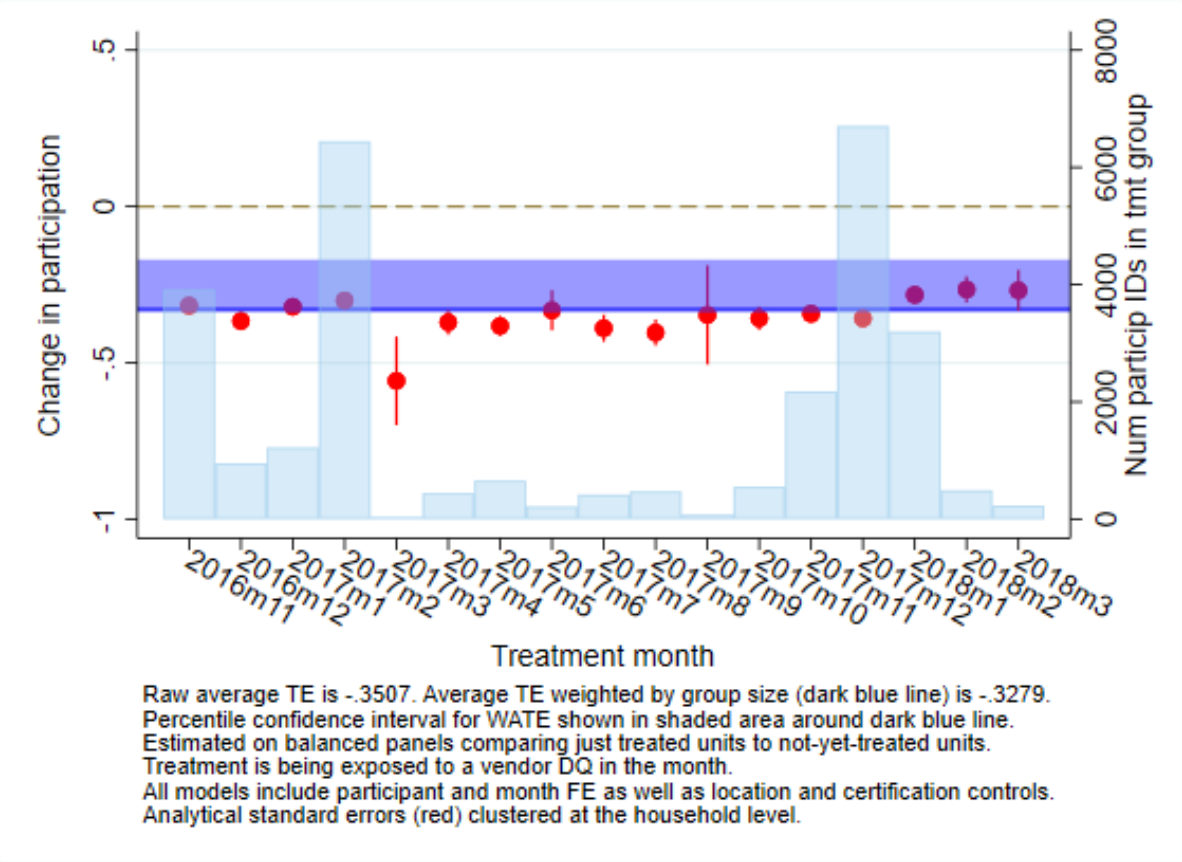


Figure 1.4: Effect of vendor disqualification on participant's amount of benefits redeemed by treatment group

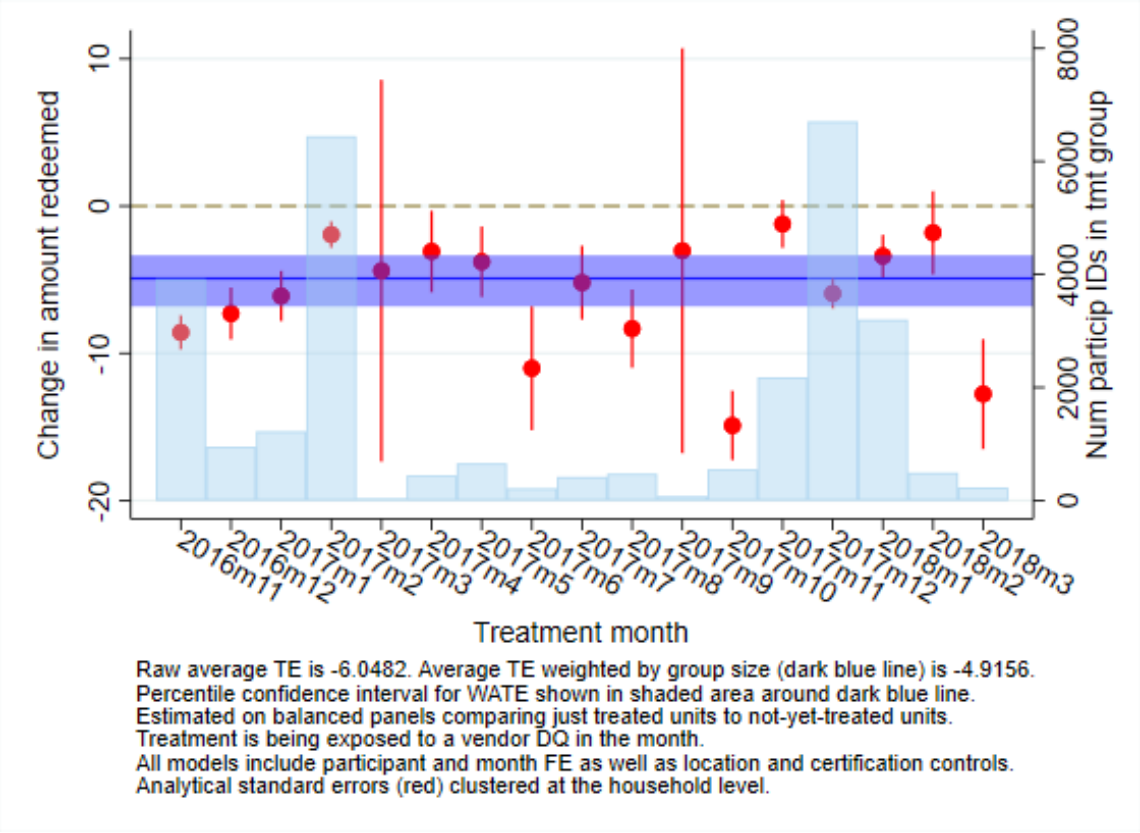


Figure 1.5: Effect of vendor disqualification on participant's share of benefits partially redeemed by treatment group

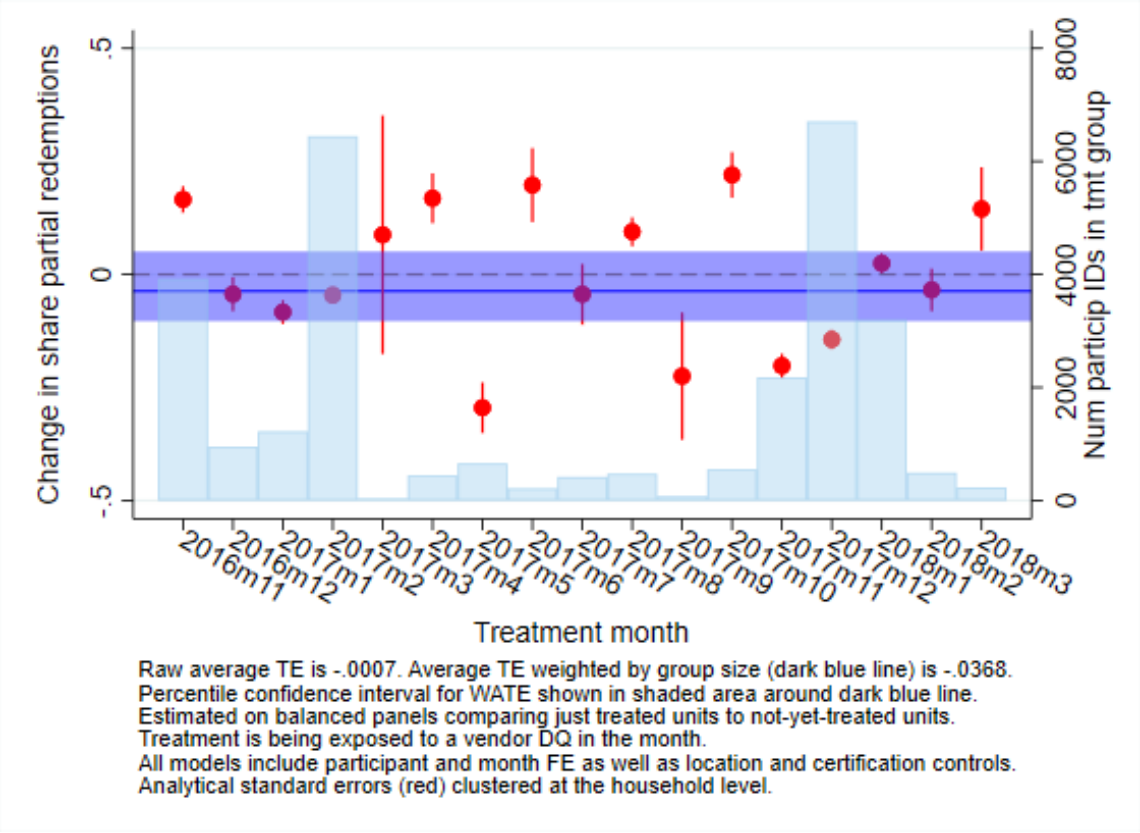
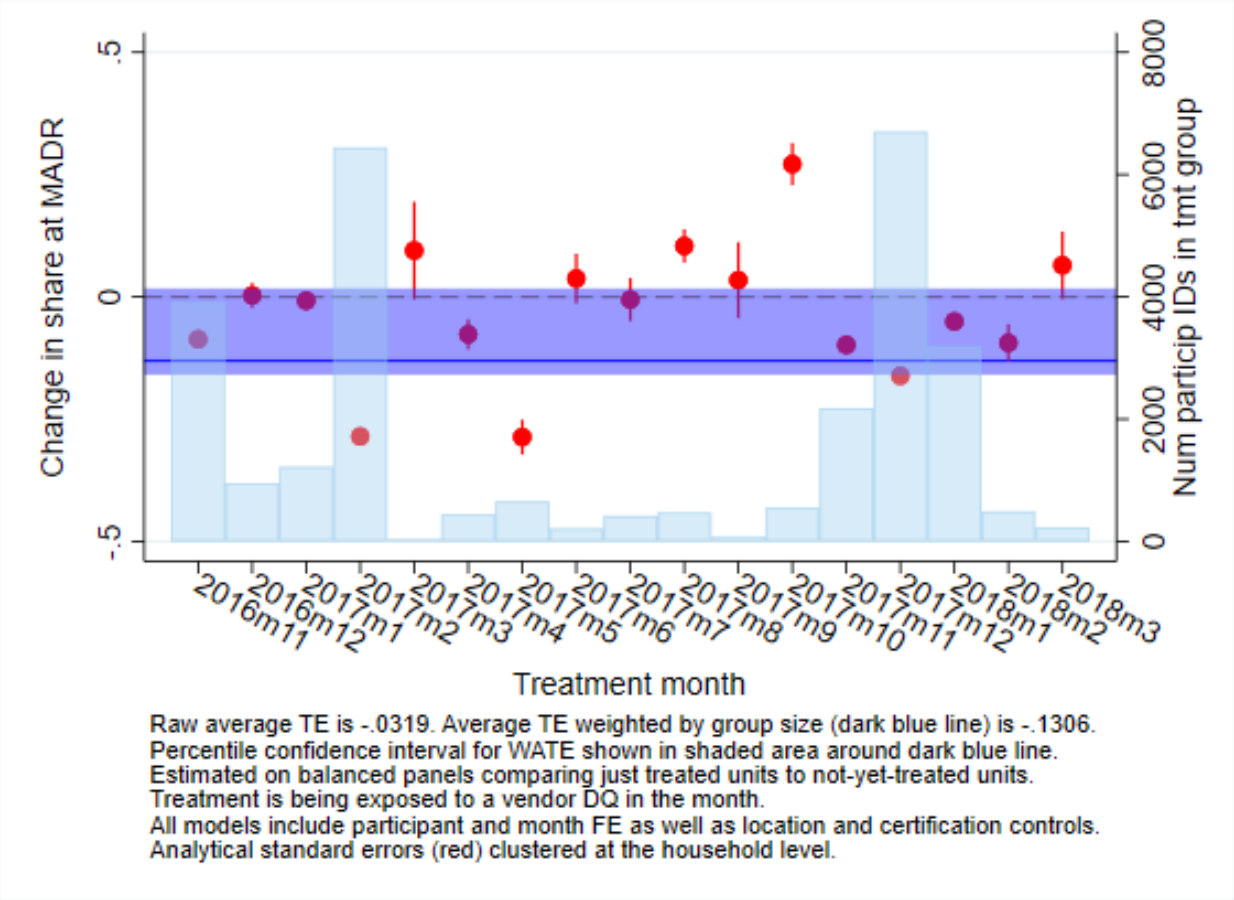


Figure 1.6: Effect of vendor disqualification on participant's share of benefits redeemed at the MADR by treatment group



Tables

Table 1.1: Summary statistics by treatment groups

DQ month	2016m5	2016m6	2016m7	2016m8	2016m9	2016m10
HH monthly	1.5e+03	1.5e+03	1.6e+03	1.8e+03	1.5e+03	1.5e+03
income ($)	(845.4)	(959.7)	(856.4)	(948.7)	(898.8)	(877.5)
HH head	11	11	10	11	11	10
highest grade	(2.842)	(2.986)	(3.147)	(2.771)	(2.474)	(2.679)
Hispanic	.8	.73	.97	.95	.8	.78
	(0.402)	(0.445)	(0.182)	(0.220)	(0.399)	(0.416)
TANF	.21	.23	.12	8.0e-02	.25	.23
	(0.410)	(0.419)	(0.328)	(0.271)	(0.431)	(0.423)
SNAP	.43	.48	.37	.3	.49	.46
	(0.494)	(0.499)	(0.482)	(0.457)	(0.500)	(0.498)
Mean age	8.7	8.8	8.6	9.9	9.5	8.1
	(11.67)	(11.49)	(11.30)	(12.45)	(11.50)	(11.18)
Family size	4.3	4.5	4.3	4.1	4.7	4.5
	(1.453)	(1.566)	(1.509)	(1.480)	(1.656)	(1.534)
Observations	9912	21944	2880	17540	5148	10812

Table 1.1: Summary statistics by treatment groups (cont'd)

DQ month	2016m11	2016m12	2017m1	2017m2	2017m3	2017m4
HH monthly income ($)	1.6e+03 (946.0)	1.7e+03 (1000.9)	1.5e+03 (878.6)	1.9e+03 (1008.8)	1.2e+03 (938.6)	1.8e+03 (916.3)
HH head highest grade	10 (2.847)	10 (3.323)	9.5 (3.690)	8.5 (3.911)	10 (2.878)	9.9 (2.851)
Hispanic	.81 (0.394)	.75 (0.434)	.82 (0.384)	.96 (0.184)	.35 (0.478)	.97 (0.179)
TANF	.13 (0.338)	.14 (0.344)	.23 (0.422)	8.4e-02 (0.278)	.38 (0.485)	.1 (0.301)
SNAP	.37 (0.483)	.41 (0.493)	.48 (0.500)	.27 (0.446)	.55 (0.498)	.34 (0.472)
Mean age	8.9 (11.99)	8 (11.31)	8.4 (11.68)	9 (11.97)	9.6 (11.68)	8.9 (12.24)
Family size	4.5 (1.575)	4.9 (1.910)	5 (2.062)	4.4 (1.434)	3.6 (1.612)	4.4 (1.406)
Observations	50986	13328	18375	103136	629	7974

Table 1.1: Summary statistics by treatment groups (cont'd)

DQ month	2017m5	2017m6	2017m7	2017m8	2017m9	2017m10
HH monthly	1.8e+03	1.4e+03	1.4e+03	1.4e+03	2.1e+03	1.4e+03
income ($)	(1049.2)	(841.9)	(805.4)	(928.7)	(1032.4)	(780.1)
HH head	12	11	10	9.8	6.4	11
highest grade	(2.712)	(2.311)	(2.867)	(3.237)	(4.147)	(2.354)
Hispanic	.26	.92	.92	.86	1	.95
	(0.437)	(0.271)	(0.273)	(0.348	(0)	(0.211)
TANF	7.7e-02	.26	.17	.13	7.6e-02	.2
	(0.266)	(0.439)	(0.375)	(0.340)	(0.265)	(0.402)
SNAP	.34	.51	.35	.38	.3	.34
	(0.472)	(0.500)	(0.478)	(0.485)	(0.458)	(0.473)
Mean age	9.4	8	7.4	8.7	9.1	8.3
	(12.84)	(11.16)	(11.19)	(12.06)	(12.24)	(11.24)
Family size	4.1	4.7	4.3	4.3	5.1	4.5
	(1.264)	(1.641)	(1.418)	(1.509)	(1.615)	(1.549)
Observations	12521	4320	8820	10450	1771	13248

Table 1.1: Summary statistics by treatment groups (cont'd)

DQ month	2017m11	2017m12	2018m1	2018m2	2018m3	2018m5
HH monthly income ($)	1.6e+03 (894.7)	1.7e+03 (1035.5)	1.8e+03 (1120.2)	1.4e+03 (986.8)	2.0e+03 (1056.7)	1.4e+03 (1078.7)
HH head highest grade	10 (2.631)	11 (2.329)	11 (2.299)	11 (3.106)	11 (2.646)	11 (2.228)
Hispanic	.91 (0.290)	.78 (0.415)	.91 (0.282)	.27 (0.444)	.29 (0.453)	.65 (0.477)
TANF	.13 (0.338)	.11 (0.309)	8.5e-02 (0.280)	.22 (0.416)	6.0e-02 (0.238)	.25 (0.430)
SNAP	.29 (0.455)	.3 (0.459)	.24 (0.429)	.47 (0.499)	.29 (0.454)	.45 (0.498)
Mean age	7.5 (11.69)	7.6 (11.66)	7.6 (12.05)	8.6 (12.33)	8.2 (12.56)	6.3 (10.91)
Family size	4.4 (1.488)	4.3 (1.455)	4.2 (1.491)	4.9 (2.033)	4.1 (1.398)	4.8 (1.739)
Observations	47456	87846	237660	24500	23256	14097

Table 1.1: Summary statistics by treatment groups (cont'd)

DQ month	2018m6	2018m7	2018m8	2018m9	2018m10	2018m11
HH monthly income ($)	1.9e+03 (1078.8)	1.7e+03 (1055.1)	1.5e+03 (983.0)	1.5e+03 (1013.1)	1.4e+03 (872.7)	1.4e+03 (906.3)
HH head highest grade	11 (3.067)	11 (2.706)	9.9 (3.733)	10 (2.960)	11 (1.986)	11 (2.783)
Hispanic	.86 (0.346)	.79 (0.410)	.85 (0.361)	.85 (0.359)	.84 (0.367)	.85 (0.360)
TANF	6.4e-02 (0.245)	.14 (0.344)	.1 (0.301)	.18 (0.382)	.26 (0.439)	.14 (0.344)
SNAP	.24 (0.427)	.33 (0.471)	.28 (0.449)	.4 (0.490)	.43 (0.496)	.29 (0.455)
Mean age	7.9 (11.78)	8 (11.93)	9.1 (12.90)	7.6 (11.50)	8.6 (11.70)	6.6 (11.10)
Family size	4.3 (1.431)	4.4 (1.566)	4.1 (1.361)	4.5 (1.784)	4.7 (2.116)	4.1 (1.448)
Observations	54500	174356	86400	13776	6670	164424

1.7 Appendix: Details on vendor authorization and sanctions

Food retailers must apply to state agencies to receive WIC authorization. The application requires standard information on the vendor (owner information, address, tax identification, business license information) as well as information on sales, prices of WIC products currently stocked, and varieties of infant formula stocks. In California, vendors may be authorized as a "WIC-only" vendor, also referred to as an A50 vendor, indicating that above 50% of the value of the vendor's annual food sales are from WIC redemptions (sales of WIC products to WIC participants using valid food instruments). Work by McLaughlin, Saitone, and Sexton (2019) indicates that A50 vendors in Los Angeles generally increase participant access and reduce program costs. This is particularly true during my sample period, after California implemented cost containment procedures and before changes to the program with the rollout of WIC EBT (McLaughlin, Saksena, et al. 2021). To finalize the application, the potential vendor must sign the vendor agreement, indicating that the vendor will comply with all WIC regulations (California Department of Public Health WIC Division 2014).

Authorized vendors must comply with state and federal policies in order to maintain their authorization. These policies include not overcharging participants, maintaining minimum stock of some WIC-specific goods, complying with audits, and not exchanging WIC benefits for unauthorized products, among many others (see 7 CFR B.II.A. §246.12). In many states, including California, vendors must also maintain valid SNAP (also known as CalFresh in California) authorization in order to be WIC authorized. Federal and state regulations require vendors to complete yearly training on their site and to send at least one employee to in-person WIC training every three years. Vendors may be sanctioned for failing to comply with policies.

The federal government mandates that state agencies (SAs) inspect at least 5% of their vendors for compliance each fiscal year, including inspecting all high risk vendors up to the 5% maximum and selecting the remainder of vendors for inspection randomly (7 CFR B.II.A. §246.12(j)(2)). Criteria for high risk vendors are determined by a minimum set of federal requirements (including high mean and low variance redemption) although states may add extra criteria at their discretion, subject to FNS approval. These criteria are not public knowledge. In discussing the selection of the remaining vendors to satisfy the 5% rule, state agency employees suggest that the vendors are often chosen for their proximity to driving routes to vendors on the high risk list so that the state agency inspector can inspect the non-high risk vendor on the same trip as the high-risk vendor. Conversations with employees at the California Department of Public Health (CDPH) WIC Division (CDPH/WIC) indicate that the agency tends to meet but not exceed the 5% minimum on inspections, given constraints on resources.

I note that federal standards set by the US Department of Agriculture Food and Nutrition Service (USDA/FNS) in almost all cases prevent disqualification of vendors that are essential for participant access.[15] For every potential disqualification, the Department conducts a participant access determination (PAD) to ensure that the vendor is not the only vendor available to participants within the relevant geography (based on USDA/FNS standards). If a vendor is found to be essential for participant access to benefits, the vendor would be fined rather than disqualified. In the study period, the Department has not changed any decision to disqualify a vendor based on a participant access determination. This means that – by federal standards – all participants shopping at a disqualified vendor have access to another WIC authorized vendor, so that any effect I find is caused by a shock to travel distance that is sufficiently small that the Department considers the participant to have adequate alternate access. However, if the time or search costs to the participant of switching vendors

[15] See 7 CFR 246.12(l) for details. Exceptions include very egregious violation as well as repeat or multiple violations.

are sufficiently high, or the benefit to the participant from obtaining their WIC foods sufficiently low, it may be rational for the participant to not seek an alternate vendor to redeem benefits in the face of a disqualification. Understanding whether these costs are material for participants has clear policy implications.

1.8 Appendix: Conceptual Framework

The underlying problem posed in this paper - how do WIC participants respond to the exogenous removal of a salient authorized vendor - focuses on a household level response to a decision made by a state agency about a profit-maximizing firm. I describe a toy theoretical model that predicts the household's response to this change in their vendor choice set.

I assume that the participant household is composed of an adult and child(ren) under the age of 5, and that the adult is the sole decision-making agent. While the preferences of the children factor into the decision-making process of the adult, I assume that the children in the household are too young to act as independent agents. The household's collective preferences about WIC eligible foods relative to all other consumption are measured by the taste parameter $\alpha_i \in [0, 1]$.

The household thus makes all relevant decisions about program certification and benefit use jointly through the adult. I further assume that the adult chooses the participation and certification decisions that align with the state of the world that maximizes household utility from consumption of WIC eligible foods w and all other goods n.[16] I assume that the household's preferences are rational and strongly monotonic in the two goods. In the state of the world where the household chooses to continue to participate and/or recertify in WIC, the household obtains an endowment $\underline{\underline{w}} > 0$ of the WIC good. It is feasible for the household to partially redeem their endowment of the WIC good. I denote the amount of

[16] I frame this as a decision about continued participation or recertification since all households in my data have been WIC participants at some point. I am much better equipped to test predictions about continued participation with this data relative to the decision to take up WIC.

the endowment consumed by the household as $\underline{w} \geq 0$. The household chooses $\underline{w} \leq \underline{\underline{w}}$ as part of their utility maximization problem. Partially redeeming WIC benefits issued with paper food instruments (as in my study period) means that either the household does not redeem at least one food instrument issued to a participant in the household or that, on any food instrument, not all of the items in the bundle are redeemed. Once a paper food instrument is at least partially redeemed, any unredeemed items on that FI cannot be redeemed by the household in the future. Total consumption of the WIC eligible good for a participating household is equal to consumption of the endowment $\underline{w}$ plus any additional amount of the WIC good purchased by the household w.

The household incurs a utility penalty b per unit of the WIC endowment consumed for using mental bandwidth. Shopping for WIC goods and completing nutrition counseling require both time and mental effort. Meckel, Rossin-Slater, and Uniat (2021) document a time penalty for households shopping with WIC in American Time Use Survey data, finding that WIC households spend at least 25% more time grocery shopping, on average, than otherwise comparable households. While Deshpande and Y. Li (2019) find evidence that crowding is a more important factor than time or information costs in reducing takeup of SSI in response to office closings, the higher frequency of food shopping relative to SSI enrollment may change the relative importance of these components in a food assistance context. For instance, Chauvenet et al. (2019) find that WIC participants report challenges finding WIC authorized items in stores and perceive stigma when redeeming benefits at checkout. Study participants from Chauvenet et al. (2019) also describe that they learn patterns for successful WIC benefit use in particular stores over time. Results from Weber et al. (2018) show that Illinois WIC participants' perceived value of their benefits decreases with factors like rudeness or impatience from other customers at stores, challenges at WIC clinics, and unclear labeling.

Evidence from behavioral economics suggests that using bandwidth can directly reduce util-

ity (Schilbach, Schofield, and Mullainathan 2016; Schofield and Venkataramani 2021). A per unit bandwidth penalty captures the marginal bandwidth penalty from stigma and shopping effort. A fixed bandwidth penalty b_0 in addition to the marginal penalty captures fixed costs of certification, recertification, and participation, particularly administrative burdens. Including this fixed cost would decrease the probability that participants recertify or continue to participate. This fixed penalty would cause a decrease in the probability that a household continues to participate or recertifies but would not change the household's decision about the level of consumption for n, w, or $\underline{w}$, conditional on participating. For this reason and notational simplicity I omit the b_0 parameter.

I do not incorporate any uncertainty about the probability of the recertification application being accepted as in Kleven and Kopczuk (2011) or misperception of the benefit amount as in Finkelstein and Notowidigdo (2019), since the household has previously been certified in WIC and can reasonably form certain expectations about acceptance given their current income and the benefits that they will receive at recertification.

The utility function in the world where the participant continues to participate or recertifies is $U(n^*+\alpha_i(w^*+\underline{w}^*)-b\underline{w}^*)$; in this world total consumption C is $n^*+\alpha_i(w^*+\underline{w}^*)$. The household's utility function in the counterfactual world without participation or recertification is $U(n^{*'}+\alpha_i w^{*'})$, with total consumption $n^{*'}+w^{*'}$. I make the standard assumptions that $U(\cdot)$ is quasi-concave, increasing, and continuously differentiable, and that $\lim_{C\to\infty} U(C) = \infty$. The conditions on $U(\cdot)$ are necessary and sufficient for the Kurush-Kuhn-Tucker conditions to provide a solution $(n^*, w^*, \underline{w}^*)$ to the utility maximization problem.

This set up is similar to that found in Kleven and Kopczuk (2011), although expands the outside purchase options of the household, and deviates from Finkelstein and Notowidigdo (2019) by not including an adjustment margin for labor supply. With this set up I assume

that the household's budget constraint is fixed over the short run for the participation and recertification window I consider. In addition to the short time span for labor supply adjustments, two other factors may constrain labor supply for WIC participants: (1) child care requirements and (2) maintaining income at levels low enough to qualify for non-WIC safety net programs. SNAP in general has a lower income threshold than WIC, as does Medicaid for non-pregnant adults. I suggest that the combination of short adjustment time, child care demands, and income restrictions for other programs constrain the labor supply of WIC participants so that participants are not adjusting their labor supply around the recertification process. There is empirical evidence to suggest that households do not adjust their income temporarily in order to obtain safety net program benefits; Pei (2017), using the Survey of Income and Program Participation, does not find evidence to support the hypothesis that Medicaid/CHIP participants adjust their income temporarily to achieve recertification. I note that Medicaid and CHIP are likely more valuable to a household than WIC benefits, implying that the incentives to adjust income would be less for WIC than for Medicaid/CHIP.

First, I model in a general way the household's optimal choices of n, w, and $\underline{w}$ in the two states of the world. A model without functional form restrictions shows the conditions under which the participant fully or partially redeems their WIC benefits, as well as building foundations for predictions about the household's extensive margin participation or recertification decision. I use the state of the world where the household does not continue to participate or recertify in WIC as the benchmark to compare to values when the household participates. The household faces two main constraints on their choices: a budget constraint and an endowment constraint. The endowment constraint prevents the household from consuming more than $\underline{\underline{w}}$ of the WIC endowment when they are certified. I also include non-negativity constraints on n, w, and $\underline{w}$.

The Lagrangian function in the case of WIC recertification is

$$\mathcal{L} = U(n + \alpha_i(w + \underline{w}) - b\underline{w}) - \lambda(p_n n + p_w w - I) - \mu(\underline{w} - \underline{\underline{w}}) + \nu n + \xi w + \pi \underline{w}$$

with first order conditions

$$\frac{\partial U}{\partial n} - \lambda p_n + \nu = 0 \tag{1.1}$$

$$\alpha_i \frac{\partial U}{\partial w} - \lambda p_w + \xi = 0 \tag{1.2}$$

$$(\alpha_i - b)\frac{\partial U}{\partial \underline{w}} - \mu + \pi = 0 \tag{1.3}$$

Considering equation (1.3), I define several cases for the parameters α_i and b and the multipliers μ and π which determine whether households fully or partially redeem benefits. First, I note that it is impossible for μ and π to both be nonzero at the same time. Complementary slackness conditions imply that $\mu > 0$ when $\underline{w} = \underline{\underline{w}}$ while $\pi > 0$ when $\underline{w} = 0$. Since both multipliers cannot be positive at the same time, three cases are possible. In Case 1, $\mu > 0$ and $\pi = 0$, implying $\underline{w} = \underline{\underline{w}}$. In Case 2, $\mu = 0$ and $\pi = 0$, implying $\underline{w} \in \left(0, \underline{\underline{w}}\right)$. In Case 3, $\mu = 0$ and $\pi > 0$, implying $\underline{w} = 0$.

Case 1 occurs when α_i is greater than b. In order for equation 1.3 to balance with $\mu > 0$ and $\pi = 0$, the overall term $(\alpha_i - b)\frac{\partial U}{\partial \underline{w}}$ must be greater than zero. By the increasingness of $U(\cdot)$, the partial derivative $\frac{\partial U}{\partial \underline{w}}$ is positive for all values of n, w, and $\underline{w}$. Thus $\alpha_i - b$ must be positive as well, implying $\alpha_i > b$.

Following the same reasoning as above, Case 2 occurs when α_i equals b. In this case both μ and π are zero, so that the term $(\alpha_i - b)\frac{\partial U}{\partial \underline{w}}$ must also be zero. Since $\frac{\partial U}{\partial \underline{w}}$ is always positive, $\alpha_i - b$ must be zero implying $\alpha_i = b$.

Finally, Case 3 occurs when α_i is less than b. In this case π is positive so that $(\alpha_i - b)\frac{\partial U}{\partial \underline{w}}$ must be negative, implying $\alpha_i < b$.

The following table shows the complete cases.

Case	Value of α_i relative to b	Value of μ, π	Value of $\underline{w}$
Case 1.	$\alpha_i > b$	$\mu > 0,\ \pi = 0$	$\underline{w} = \underline{\underline{w}}$
Case 2.	$\alpha_i = b$	$\mu = 0,\ \pi = 0$	$\underline{w} \in \left(0, \underline{\underline{w}}\right)$
Case 3.	$\alpha_i < b$	$\mu = 0,\ \pi > 0$	$\underline{w} = 0$

Intuitively, Case 1 occurs when the marginal value of the WIC endowment in the utility function - α_i - is greater than its marginal cost from the bandwidth penalty - b. The household chooses to consume all of the endowment in this case. Case 3 is the opposite of Case 1 - the marginal value of the WIC endowment is less than its marginal cost. In this case the household chooses to consume none of the endowment and does not choose to continue to participate or recertify to avoid any fixed cost associated with certification. The optimal values n^*, w^*, $n^{*'}$, and $w^{*'}$ are the same in the two states of the world in this case. The boundary between these two cases is Case 2, in which the marginal value of the WIC endowment in the utility function is exactly equal to its marginal bandwidth cost. In this case, the household partially redeems its WIC endowment, consuming some $\underline{w} \in \left(0, \underline{\underline{w}}\right)$. The value $\underline{w}$ is determined, along with n^* and w^*, through equations (1.1) and (1.2) as well as the budget constraint.

Intuition and results from the intensive margin model above clarify two features of the household's extensive margin - participation or recertification - choice. First, if $\alpha_i < b$ the household does not choose to participate or recertify. In this case the utility functions in the two states of the world are identical so that $(n^*, w^*) = (n^{*'}, w^{*'})$. In Cases 1 and 2 $\underline{w} > 0$,

and the household chooses to participate or recertify when

$$U(n^* + \alpha_i(w^* + \underline{w}^*) - b\underline{w}^*) \geq U(n^{*'} + \alpha_i w^{*'})$$

Comparing the utility functions for the two states of the world, the household's choice to participate in WIC depends on the direction of the difference in total consumption minus the bandwidth penalty: if
$[n^* + \alpha_i(w^* + \underline{w}^*)] - (n^{*'} + \alpha_i w^{*'}) > b\underline{w}^*$ then the household chooses to participate or recertify. Otherwise, the household does not continue in WIC.

The difference in total consumption $[n^* + \alpha_i(w^* + \underline{w}^*)] - (n^{*'} + \alpha_i w^{*'})$ is positive in Cases 1 and 2 by the strong monotonicity of the household's preferences. Consuming some of the endowment so that $\underline{w} > 0$ implies that total consumption in the household, holding the budget constraint constant, cannot decrease so that $n^* \geq n^{*'}$ and $w^* + \underline{w} \geq w^{*'}$. This implies, intuitively, that as long as the marginal value of the WIC endowment is at least as large as its marginal cost (Cases 1 and 2), the household chooses to continue in WIC. However, the magnitude of the difference in total consumption relative to the bandwidth penalty is not determined without functional form restrictions. This prevents the policymaker from assessing the exact change in the probability of participation or recertification for a given individual with a change in the bandwidth cost of participation and recertification b. However, assuming that α_i continuously distributed on the same interval as b, it is unambiguous that increasing the bandwidth cost of participation and recertification decreases the number of participants fully and partially redeeming WIC benefits. All else equal, increasing the household's taste α_i for the WIC good increases the probability of participation and recertification.

From the perspective of the policymaker - who cannot observe the household's taste parameter, although they control the size of the benefit $\underline{w}$ and the cost to participation and

recertification b - the probability that any particular household participates and/or recertifies depends on the location of the household's taste parameter relative to a function of the parameters b, $\underline{\underline{w}}$, p_n, p_w, and I. With a functional form imposed on the utility function, probability of participation is expressed as a rearranged version of

$$Pr\left(\alpha_i > \frac{b\underline{w}^* + (n^{*'} - n^*)}{\underline{w}^* - w^*}\right) \tag{1.4}$$

where $(n^*, w^*, \underline{w}^*, n^{*'}, w^{*'})$ are functions of $\underline{\underline{w}}$, p_n, p_w, I, α_i, and b.

Redeeming WIC benefits at a WIC vendor reveals a preference by the participant for that vendor. Participants' preferences over vendors may result from location, language, cultural affinity, available selection, or customer service, among other reasons. Disqualifying a preferred vendor constitutes an increase in the cost of participating, as the participant is moving away from their optimal set of vendors. Overall, the very general model I describe predicts a decrease in the probability of participation and recertification as a result of increasing the bandwidth cost from b to b' as well as a decrease in participants' total consumption and consumption of the WIC good. These decreases are driven by households whose taste for the WIC good α_i is within the margin of change from b to b'.

Given that I've predicted a decrease in the probability of recertification without any restrictions, I test this prediction in the empirical section. I measure only participation, as recertification is challenging to observe precisely in the participation data. Changes in participation capture non-recertification, so that changes in participation are an upper bound for the effect on recertification. I use a measure of participation for each individual in the household. Since some of the costs b are variable (e.g. shopping costs, picking up new food instruments) it's plausible that both recertification and participation would decrease after exposure to a disqualification as bandwidth costs increase. Participation decisions occur

each month, while recertification occurs only once per year for children and infants and at birth for pregnant individuals. Increased frequency means that it may be possible to observe empirically a significant participation effect with a less precise recertification effect. I note that, analytically, With a functional form restriction I could predict the change in the probability of recertification with a disqualification by calculating the partial derivative of equation (1.4) with respect to b.

I also test if there is an intensive margin response to disqualification. Do WIC participants reduce their consumption of WIC foods in response to a disqualification, even if they are certified participants? In addition to decreases in participation, it is plausible that in the short run (prior to the end of a certification period) participants would reduce their redemption of WIC benefits if the cost to redeem increased sufficiently. I note that with paper food instruments, the items on a particular food instrument must all be redeemed in one visit to one store. A decrease in redemptions must come from either an increase in partial redemptions - where a participant redeems some items on an FI but not all, perhaps because of stigma or effort costs associated with particular goods - or an increase in non-redemption, where none of the items on the FI are redeemed. For a household with multiple FIs, a decrease in amount redeemed could come from a combination of these mechanisms: increasing the share of partially redeemed and/or non-redeemed FIs.

The model above assumes a participant with rational preferences and a well-behaved utility function. Behavioral economics suggests that individuals are likely to make decisions that are not always consistent with these models. A behavioral economics framework here might suggest that cognitive biases are the cause of participant's non-participation in WIC, potentially missing out on economic benefits from supplemental foods and nutritional counseling. One such bias is hyperbolic discounting, in which the participant applies excess weight to the short-term costs of participation, including administration and shopping burdens, and

reduces the weight on long-run benefits when making decisions about participation and re-certification.

1.9 Appendix: Pooled OLS and TWFE results

Tables 1.2, 1.3, 1.4, and 1.5 below show the pooled OLS results for all treated units. The sample here is all participants that ever redeem benefits at a store that goes on to be DQed (prior to the DQ). For this sample, I estimate the specification

$$y_{it} = \alpha + \beta DQ_{it} + \mathbf{\Gamma}_{Clinic} + \mathbf{\Theta}_{Category} + \mathbf{PP}_{it} + \epsilon_{it}$$

where y_{it} is the relevant outcome specified in the table for participant i in month t, DQ_{it} indicates whether the participant i has been exposed to the DQ in month t, $\mathbf{\Gamma}_{Clinic}$ is a vector of indicator variables that participant i visits a given clinic (clinic fixed effects), $\mathbf{\Theta}_{Category}$ is a vector of category fixed effects, and $\mathbf{PP}_{it}$ is a set of fixed effects for participation pathways - unique combinations of category, birth month (if a child) or pregnancy end month (if an adult), and certification start month that determine the relevant rules for certification for individuals over time.

Table 1.2: POLS estimates of vendor DQ on participation, treated units only

DQ	-0.115**
	(0.037)
Observations	2173590

Standard errors in parentheses. Standard errors clustered by month of DQ.
Results show coefficients from OLS regression of participation on clinic and category fixed effects, as well as certification start month interacted with category and month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results from the pooled OLS specification have the same sign as the results from the stacked difference-in-differences approach in all cases. The magnitudes are uniformly smaller, and in the case of the amount of benefits redeemed, the specification is not statistically significantly different from zero in the pooled OLS approach.

Table 1.3: POLS estimates of vendor DQ on amount redeemed, treated units only

DQ	−1.158
	(1.075)
Observations	1195530

Standard errors in parentheses. Standard errors clustered by month of DQ. Results show coefficients from OLS regression of amount redeemed on clinic and category fixed effects, as well as certification start month interacted with category and month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.4: POLS estimates of vendor DQ on share at MADR, treated units only

DQ	−0.055
	(0.032)
Observations	1040157

Standard errors in parentheses. Standard errors clustered by month of DQ. Results show coefficients from OLS regression of the share of benefits redeemed at the MADR on clinic and category fixed effects, as well as certification start monthinteracted with category and month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.5: POLS estimates of vendor DQ on share full CVV, treated units only

DQ	−0.007***
	(0.001)
Observations	667705

Standard errors in parentheses. Standard errors clustered by month of DQ. Results show coefficients from OLS regression of the share of CVV benefits fully redeemed on clinic and category fixed effects, as well as certification start month interacted with category and month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Tables 1.6, 1.7, 1.8, and 1.9 show the pooled OLS TWFE results for all treated units. Estimated on the same sample as the pooled OLS results above, the specification now includes

participant fixed effects δ_i and month fixed effects η_t

$$y_{it} = \alpha + \beta DQ_{it} + \delta_i + \eta_t + \mathbf{\Gamma}_{Clinic} + \mathbf{\Theta}_{Category} + \mathbf{PP}_{it} + \epsilon_{it}$$

Again, the signs of the estimated effects are the same as in the stacked difference-in-difference specification, although with smaller magnitude. In this case, the participation results are not significantly different from zero.

Table 1.6: TWFE POLS estimates of vendor DQ on participation, treated units only

DQ	−0.056
	(0.030)
Observations	2173524

Standard errors in parentheses. Standard errors clustered by month of DQ.
Results show coefficients from OLS regression of participation on clinic,
participant, month, and category fixed effects,
as well as certification start month interacted with category and
month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.7: TWFE POLS estimates of vendor DQ on amount redeemed, treated units only

DQ	−2.646*
	(1.189)
Observations	1195343

Standard errors in parentheses. Standard errors clustered by month of DQ.
Results show coefficients from OLS regression of amount redeemed on clinic,
participant, month, and category fixed effects,
as well as certification start month interacted with category and
month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.8: TWFE POLS estimates of vendor DQ on share at MADR, treated units only

DQ	-0.061^{*}
	(0.027)
Observations	1039836

Standard errors in parentheses. Standard errors clustered by month of DQ.
Results show coefficients from OLS regression of share of benefits redeemed at MADR on clinic, participant, month, and category fixed effects,
as well as certification start month interacted with category and
month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.9: TWFE POLS estimates of vendor DQ on share full CVV, treated units only

DQ	-0.008^{***}
	(0.002)
Observations	666366

Standard errors in parentheses. Standard errors clustered by month of DQ.
Results show coefficients from OLS regression of share of CVV benefits fully redeemed on clinic, participant, month, and category fixed effects,
as well as certification start month interacted with category and
month of birth (for children)/pregnancy end month (for adults).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Essay 2

Impact of EBT in the WIC Program on Vendors and Participants: Evidence from Administrative Data on Vendor Authorization and Benefit Redemptions

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2.1 Introduction

The Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) served an average of 6.9 million children, infants, and pregnant and postpartum women a month in FY18. This was done by providing $3.4 billion worth of food through more than 47,000 authorized retailers.[1] About half of all infants born in the U.S. receive WIC benefits and WIC is the third largest domestic food assistance program. In addition to providing a package of nutritious foods to supplement diets, WIC participants receive referrals to other safety net and local programs along with nutrition education and breastfeeding support. WIC food benefits (the Food "Package") can be used to purchase foods high in nutrients of concern—including infant formula, fruits and vegetables, whole grains, milk, and other foods as specified in the package. Participants apply for WIC at WIC clinics, and if eligible, redeem their benefits at private vendors.

Retailers (also known as vendors) are a key part of the WIC program. Participants redeem benefits at authorized WIC vendors. Vendors must apply for WIC authorization and meet a set of federal and state-specific criteria to qualify. States decide if vendors are eligible and are responsible for monitoring vendors. Vendors must send employees to WIC-specific training on the use of WIC food instruments (FIs) and must stock minimum types and quantities of

[1] WIC spending makes up 0.5% of $747 billion total FY18 spending on food-at-home (FAH). For perspective, the largest U.S. food assistance program—the Supplemental Nutrition Assistance Program (SNAP)—provides benefits equal to 8% of U.S. FAH spending.

allowed program foods.[2] Importantly, WIC provides benefits through a quantity voucher, which entitles the participant to redeem the voucher for a bundle of pre-specified items at an authorized retailer, rather than a voucher for a fixed amount which can be used to buy allowed foods (like SNAP). This distinction means WIC participants are less price sensitive when shopping for items on the quantity voucher than if it were a voucher like SNAP, creating opportunities for firms to possibly mark up products, likely leading to higher program costs.[3] Despite the importance role retailers likely play in the functioning of WIC (efficient or not), the literature about the effects and determinants of the supply side of WIC is not well developed (we discuss this literature in Section 2.3).

In this paper, we extend this literature about the role of firms in WIC by exploiting an exogenous policy change to examine the effect of the change in WIC delivery from paper vouchers to an electronic benefit card known as eWIC on retailer outcomes including redemptions, participation in the program, and spillovers onto SNAP participating firms. We describe the relevant institutional details of this transition in Section 2.2We ask several questions. First, does the probability of a store being WIC authorized change in response to the transition from paper to electronic WIC benefits? Second, does the amount of WIC benefits redeemed change after this transition to electronic WIC benefits. Finally, is there heterogeneity by retailer characteristics for these effects? We also examine spillover effects onto SNAP redemptions, since EBT implementation in particular brings WIC benefit technology into line with SNAP benefit technology. Also, in many states WIC vendors are required to be SNAP authorized so changing WIC vendor policy may feasibly affect SNAP outcomes.

We make three contributions. First, we address this important gap at the intersection of

[2] An example of these minimum stocking requirements from California can be found at California Department of Public Health 2018.

[3] Since 2009, the WIC benefit has also included a cash value voucher for a set amount of fruits or vegetables without any added salt, sugar, or food. These cash value vouchers for fruits and vegetables are not associated with price insensitivity.

the WIC policy literature and research on food retailer characteristics and entry/exit. Second, we bring novel administrative data to bear by leveraging as yet unused store-level data combining administrative records on both WIC and SNAP authorized vendors and WIC redemptions to evaluate local responses to this change in state WIC disbursement policy. Finally, our work may inform policymakers that are concerned about the potential costs and benefits of policy changes and of WIC more broadly. Our finding that these policy changes—particularly eWIC—adversely affect independent WIC retailers suggests potential tradeoffs between reducing program costs (given that eWIC is in part a fraud reduction program (Meckel 2020) and that larger stores tend to markup WIC products less than smaller stores (Saitone, Sexton, and Volpe 2015)) and maintaining or increasing participants' access to WIC vendors (Meckel 2020; Meckel, Rossin-Slater, and Uniat 2021).

To identify the effects of interest, we exploit an exogenous policy change in WIC that affected retailers. State WIC programs began first piloting and then more broadly implementing electronic benefit transfer (EBT) technology for benefit redemption to replace the previous paper vouchers. While statewide EBT WIC (known as eWIC) implementation started in 2002, nationwide WIC implementation will not be complete until 2023.

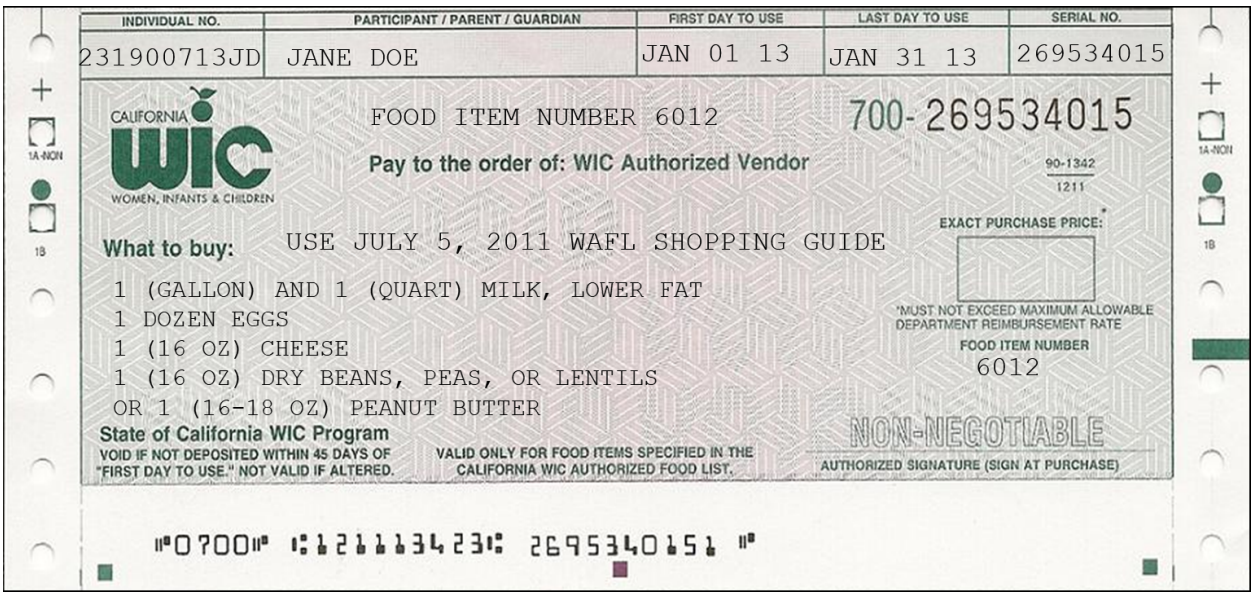
A priori, the effects of EBT implementation on food retailer behavior and WIC redemptions are ambiguous. The EBT transition may impose transaction costs on firms that cause smaller retailers to opt-out. On the other hand, the EBT transition may increase WIC participation and the share of benefits redeemed, thereby inducing WIC vendor authorization (Hanks et al. 2018), both because it reduces stigma and because it also allows recipients to split the individual categories across shopping trips. Moreover, effects may vary across store types and states using different approaches.

2.2 WIC EBT institutional details

To understand the mechanisms potentially driving participant and retailer behavior in the transition to WIC EBT, we describe the process of redeeming WIC benefits under paper vouchers and electronic benefit transfer. We contrast sources of fixed and variables costs under the two regimes, and highlight institutional features that may change incentives for firms and participants.

Under the paper voucher system, WIC participants obtain physical food instruments (FIs) from their WIC clinic once every three months during their certification period. Paper food instruments look like checks, except that they list individual foods rather than a dollar value. See Figure 2.1 for an example of a paper food instrument.

Figure 2.1: Sample California WIC paper food instrument



FIs are each valid for a month at a time on a rolling basis from the time of the clinic appointment until the next appointment or the end of the certification window. Participants take paper food instruments to retailers to redeem their benefits. The participant selects authorized foods that are available at the retailer and listed on their food instrument. For

instance, with this sample FI the participant may bring one gallon and one quart of non-organic 2% milk, one dozen eggs, a one pound block of part-skim mozzarella cheese, and a 16 oz jar of peanut butter to the cashier along with their food instrument. The cashier then adds up the prices of all the items and writes the total purchase price for the items on the FI in the designated box on the FI. The cashier inspects the participants photo ID to confirm that the FI belongs to the individual who is redeeming the FI and then the participant signs the FI. The retailer puts their information on the FI and then sends the paper voucher off to be reimbursed for the amount listed on the FI.

Note that when listing the items purchased by the participant above, we included all of the foods that this participant could have obtained. This would be a "full redemption," meaning that there were no supplemental foods not redeemed from the FI. Suppose instead that the participant only brought up one gallon of milk (instead of one gallon and one quart). This would be a "partial redemption" - not all of the supplemental foods allowed on the FI are redeemed. The procedure for redemption and reimbursement would be the same. However, the participant would not be able to redeem that quart of milk in the future. Paper food instruments may each only be used on one occasion.

Benefit redemption under EBT allows for more flexibility around partial redemptions. In comparison to redeeming benefits with a paper FI, under EBT participants reference their available supplemental foods on their state's WIC app (see Figure 2.3). The participant then finds their authorized foods on the shelf and brings them up to the register where they are scanned. The participant pays with their WIC EBT card (see 2.2) similarly to paying with a debit card. The retailer side WIC EBT software cross-references the scanned items against the set of allowable supplemental foods for that participant. The foods redeemed at this instance are then debited from the set of allowable foods for that participant for the month. In this way participants may redeem their allowed supplemental foods across

several shopping trips within the month. Participants' set of allowable foods refreshes once per month during their certification period, similar to having a new food instrument become valid once per month under paper instruments. Participants must still visit a WIC clinic once every three months to asses their nutritional status and the set of foods that is being issued to them.

Figure 2.2: Preview of California EBT card

Relative to paper FIs, EBT imposes higher fixed costs for retailers. These fixed costs come from the cost of adopting the WIC-specific EBT platform. While in general WIC authorized food retailers are also authorized to accept Supplemental Nutrition Assistance Program (SNAP) benefits using SNAP's EBT platform, the WIC EBT platform is distinct from the SNAP platform. WIC EBT rollout generally was more complex than SNAP EBT rollout, given that states needed to develop a platform that could track quantities of sets of authorized foods that are linked to ever-changing UPCs and potentially keep track of lowest cost brands. This complexity is evidenced by the much longer duration of EBT rollout (over more than 20 years). One early adopting state reverted to a paper-based system to revise their EBT system after an initial attempt at implementation. These challenges for states spill over into complex trainings for vendors with guidelines that change during the rollout process, potentially inducing a barrier to remaining authorized in the face of EBT implementation.

Vendors that are authorized at the time of the transition often receive the technology needed to accept EBT benefits for free, reducing the costs of uptake. Vendors that become authorized after the transition are liable for these fixed costs, potentially reducing the number of vendors willing to become authorized retailers. Finally, vendors that were overcharging the program effectively face lower profits from WIC as the costs of overcharging increase, relative to changing the price written on a paper FI. Meckel (2020) indicates that these vendors are likely to leave the program around the time of implementation. Working in the other direction to these costs for vendors are demand side changes that may induce increases in participation post-EBT.

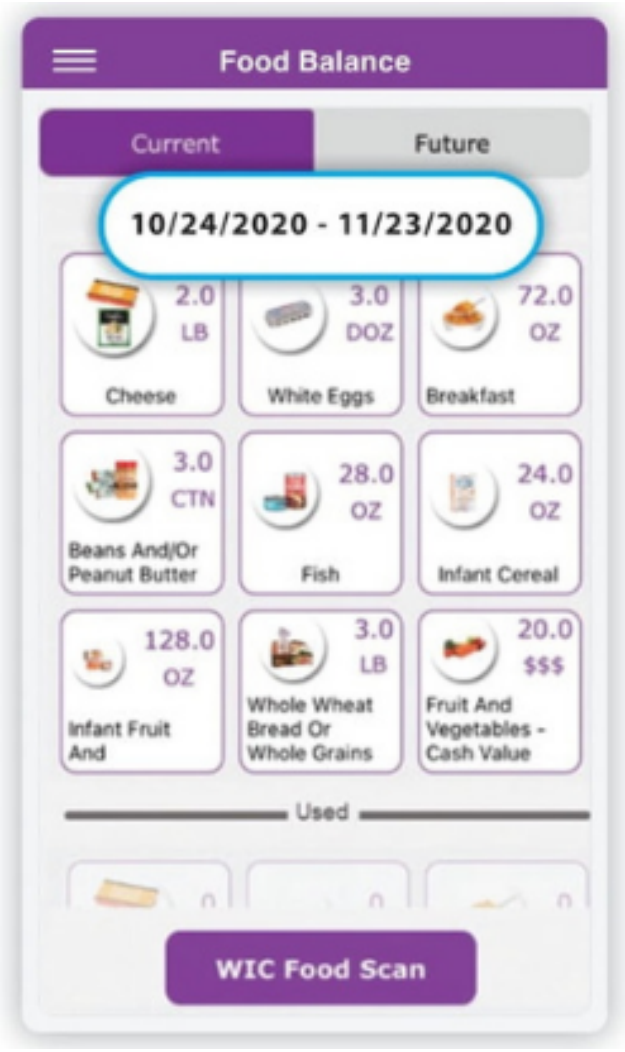


Figure 2.3: Preview of California WIC app

2.3 Literature review

In this section, we review previous work on the effect of EBT in food assistance program on participant and retailer outcomes, as well as some general work on WIC and retailers more broadly. In general papers on the SNAP and EBT transitions use a single state or a subset of a few states for their analysis. We focus closely on the most relevant papers.

Previous work on WIC authorized vendors has focused on how the lack of price sensitivity due to WIC being a quantity voucher and the fact that WIC is a block grant have affected access and spending. Because it is a quantity voucher, participants have no incentive to be price sensitive, leading to USDA's instituting cost containment measures such as peer group limits on reimbursements to stores. Because the program is a block grant, more spending due to high prices in some stores reduces available funding for other locations. WIC policy also prevents vendors from charging WIC participants different prices than other consumers, but as we discuss below, this was hard to monitor with the paper vouchers. Finally, some firms serve mostly WIC participants (so called A50 stores).

Evidence about how A50 vendors affect program costs is mixed. Evidence from Oliveira and Frazão (2015) suggests that prior to the Child Nutrition and WIC Reauthorization Act of 2004 (P.L. 108-265), A50 vendors generally had higher costs per food instrument than other vendors. However, the cost containment policies implemented in the Act covered all authorized vendors and effectively curbed food costs at A50 vendors. Using data from the Greater Los Angeles area post-2004, McLaughlin, Saitone, and Sexton (2019) find that, relative to a counterfactual world without them, A50 vendors reduce program food costs and improve program access on average.

Several papers have found that small vendors can contribute to high program food costs. McLaughlin, Saksena, et al. (2021) document this phenomenon in the Los Angeles area.

Small vendors charged prices 59-92% higher than large vendors for a sample food instrument, although this premium decreased with increasing competition from large vendors. Rising costs at small food vendors are cited as the cause of California's WIC vendor authorization moratorium in 2011-2014 (Oliveira and Frazão 2015). However, while small stores constitute a larger share of authorized vendors and redemptions in SNAP than they do in WIC most WIC redemptions occur at larger vendors, particularly super stores (Tiehen and Frazão 2016). This suggests that program cost effects from small stores may be minor relative to offsetting effects from other stores, and may be important for participant accessibility. The WIC EBT rollout may affect program costs and have heterogeneous effects by small and large vendors, given the differences in redemption levels at baseline.

The Supplemental Nutrition Assistance Program (SNAP), formerly known as Food Stamps, went through the transition to EBT prior to WIC's EBT transition. Prior work on the EBT transition in SNAP has mostly focused on participants' responses (Klerman and Danielson 2011; Lovett and Xue 2017; Shiferaw 2020; Kuhn 2021). Shiferaw (2020) finds an increase in SNAP participation after the implementation of EBT in California. Klerman and Danielson (2011) find that EBT implementation increased total SNAP participation more than any SNAP policies of the 1990s or 2000s besides information-based categorical eligibility or simplified reporting. Lovett and Xue (2017) examine food security for Californian SNAP participants around the state's transition to SNAP EBT and find no long term effects on food security levels, although they find a temporary increase in food insecurity for several months after implementation. Shiferaw (2020) finds different effects for California's SNAP EBT implementation, showing increases in birthweight for lower-birthweight infants born to mothers likely to be on SNAP.

We review some findings from the literature on SNAP EBT implementation because it helps establish priors for the effects we might find in EBT implementation for WIC. We note that

differences between WIC and SNAP mean that these SNAP effects may not extent to WIC. As noted above, WIC benefits are quantity based—rather than value based—and allow a much narrower set of eligible goods than SNAP. This means that although EBT technology exists for SNAP it is not the same as that used later for WIC and we still expect some technology related implementation frictions. In addition, although in some states WIC-authorized retailers are a subset of SNAP-authorized vendors—because of state mandates requiring SNAP authorization as a pre-requisite for WIC authorization—in other states WIC authorized retailers may not have experience with any EBT technology, resulting in frictions for retailers. WIC participants are also composed of a different population than SNAP participants, being by definition pregnant and postpartum women and people with children under 5 years of age. This population may not necessarily respond to EBT implementation in the same way as the average SNAP participant.

We also benefit by drawing from previous work on the WIC EBT transition specifically. Most of the WIC EBT research to date examines specific states or vendors, implying that in expectation our results will consist of a weighted average of these results and include findings from states that have not yet been studied separately. In Section 1.5, we show state specific results to validate that we can approximately replicate findings from other papers, up to differences from using slightly different data and empirical approaches.

This paper is closest in spirit to Meckel (2020), which uses Texas' WIC EBT rollout to evaluate how program take-up, birth outcomes, and non-WIC prices respond to eWIC implementation. Meckel uses AC Nielsen's Homescan data set (2006–2009) to evaluate price responses, and administrative data from the Texas Department of State Health Services on births (2005–2009) and WIC authorized vendors (2007–2010). Meckel treats EBT implementation as a fraud reduction reform (likely in part the motivation). EBT technology could make fraud, in the form of price discrimination between WIC and non-WIC customers, more

costly for authorized vendors relative to redeeming paper food instruments. If vendors were using price discrimination to subsidize their WIC authorization, some vendors may choose to leave the program after EBT implementation. Vendors may also find EBT requirements burdensome even if they were not price discriminating prior to EBT implementation.

Meckel (2020) finds relatively large negative effects on WIC authorization for independent vendors (0.11 stores lost per county, 10.7% of the sample mean). The effect is negative but small and statistically insignificant for chain stores. Participation numbers show that in addition to these decreases in authorized stores by county, take-up of WIC by mothers at birth decreases 5.2% on average, with larger effects for poorer mothers. Evidence from Meckel (2020) suggests that vendors reduce price discrimination after EBT and that some of the costs from reduced price discrimination are transferred to non-WIC consumers in the form of higher prices (increases of 6.4% on average).

Negative effects on smaller vendors are consistent with findings from Phillips et al. (2014), which takes a more qualitative approach to evaluating WIC EBT implementation using data from Kentucky, Michigan, and Nevada. Phillips et al. find that in Kentucky, some small vendors chose to withdraw from WIC authorization rather than taking up the technology required for EBT. For small vendors that remained in the program, the ability to partially redeem WIC vouchers[4] after EBT implementation led to a decline in the amount of benefits redeemed at smaller vendors. However, larger vendors (those with multiple registers) in Nevada reported increases in WIC redemptions. Feedback from larger vendors in Kentucky and Michigan also included reports of increased WIC sales after EBT. Differences may also arise from transactions costs being lower in stores which can implemented eWIC at the main cash ragister, but higher for stores where they need a separate stand beside scanner to re-

[4]With a paper FI, a participant must redeem all foods on the FI in a single shopping trip. With EBT, the debit-card like instrument maintains a running tab of food benefits, so that participants can redeem the FI across more than one shopping trip and at multiple vendors.

deem the WIC benefits.

Hanks et al. (2018) evaluate eWIC implementation in Ohio using scanner data from a major grocery store chain and find positive effects of WIC EBT on redemption of WIC benefits among participants, but no change in non-WIC food spending. We note that the results from Hanks et al. (2018) are most directly comparable to the chain store results from Meckel (2020) and so do not directly contradict Meckel's findings. In addition, X. Li (2020) finds a shift in redemptions from small and medium WIC vendors to large vendors after EBT implementation in Oklahoma. Results from Hanks et al. (2018) suggest that we may find increases in WIC redemptions from eWIC implementation. It is possible, combining the evidence from Meckel, X. Li and Hanks et al., that redemptions may shift from independent stores to chain stores if independent stores close, meaning that there may also be no total effect on redemptions or indeed possibly a negative effect. Unfortunately we cannot distinguish redemptions at chain stores from redemptions at independent stores in our data. See Section 2.5.2 for more details.

Participant-side responses may also affect the redemption response we observe in our data. X. Li (2020) examines participant responses to WIC EBT implementation in Oklahoma. She finds a decrease in the per-participant cost of food across regions in Oklahoma after eWIC implementation and no significant persistent effect of eWIC on the WIC participation rate. The participation rate uses imputed eligibility and may be subject to measurement error as a result of this imputation. The participation rate response is less consistent with evidence on participation from SNAP's EBT transition (discussed above) and also departs from the qualitative evidence presented in Phillips et al. (2014). X. Li's finding on the decreasing cost of food per participant (which roughly translates to redemption values, holding constant the number of participants) suggests that effects on redemptions may be mixed across states and even within states across retailers given the findings from Hanks et al. (increasing

redemptions at a chain retailer) and Meckel (likely fraud reduction).

2.4 Conceptual framework

This paper relies on variation in the rollout of EBT in WIC across counties in the US to identify the effect of two policy changes that potentially increase costs to retailers (negative supply side shock) but also potentially improve the WIC participant experience and use of benefits (positive demand shock). In the literature review above, we document evidence of both the negative supply side shock (Meckel 2020; Gleason, Morgan, et al. 2011) and positive demand side shock (Phillips et al. 2014; Hanks et al. 2018; Andreyeva and Luedicke 2015). For WIC EBT, depending on the context and data used the demand side response may be ambiguous (X. Li 2020; Gleason and Pooler 2011). We thus expect to see evidence of increased demand by participants—increased benefit redemption and potentially increases in number of authorized vendors if positive demand shifts are large enough to induce new firms to enter. We also expect to see evidence of negative shocks to vendors—induced exits from authorization and potentially decreased benefit redemption if participant access is affected by vendor exits. On net, these effects may cancel, leading to an ambiguous result, or be heterogeneous in direction across subgroups of firms and participants.

In general, observable data on US food assistance programs represent equilibrium outcomes from the combination of demand from participants and supply from authorized vendors. Our outcomes—number of authorized stores and benefit redemptions—are determined in equilibrium or in the transition to it. This means we cannot disentangle the effects on supply shocks on retailers from demand shocks on participants.

Our setting offers a plausibly exogenous variation from a natural experiment to observe substantial changes in a food assistance program that targets a vulnerable population. We aim to understand the net effects of these policies on participants across the country. States implemented the transition to eWIC on different schedules, and within states, eWIC was

implemented at different times across regions which mostly match to counties. Thus, we will use approaches which compare locations adopting eWIC at different times to others not experiencing changes, accounting for concerns about pre-trends and treatment heterogeneity.

2.5 Data

This paper examines the effect of the transition to eWIC on the presence and number of food retailers authorized to redeem food benefits and the amount of benefits redeemed. The ideal data to answer this question would include the set of all authorized WIC vendors, including their redemptions and characteristics, as well as the set of potential WIC vendors, over time and across place along with data on the timing of policy variation. There are some differences between these ideal data and our data. First, vendor-level redemption data are not released to the public to avoid identifying confidential information about the vendor.[5] Second, we do not have comprehensive data on the existence of all firms who could possibly participate in WIC. Within states, WIC EBT or eWIC was rolled out at approximately the county level[6] over more than 15 years. We do not observe all years of EBT rollout in our vendor and redemption data because our sample ends in 2015 (the last states adopt in 2023). We hand collect data on the policy rollout. To observe authorized retailers, we use administrative data on the universe of WIC authorized retailers. The US Department of Agriculture Food and Nutrition Service (USDA FNS), which administers WIC federally, collects retailer data annually. We also use administrative data on WIC redemptions, aggregated to the ZIP level and redacted where necessary to preserve vendor confidentiality. In the redacted ZIP level redemption data we observe about 20% of total redemptions. To consider spillover effects of these two policy changes onto the largest US food assistance program, SNAP, we use administrative data on SNAP authorized retailers and SNAP redemptions by county (redacted where necessary to preserve vendor confidentiality) to evaluate these effects. We note that this is still an improvement over what has been done before due to the national

[5] §246.26(e)

[6] State WIC agencies determined the geography and timing of their WIC EBT rollout. County is the common denominator geography we use to standardize these rollouts.

scope of our data and the use of administrative data rather than scanner data which may not include WIC expenditures.

2.5.1 WIC EBT rollout

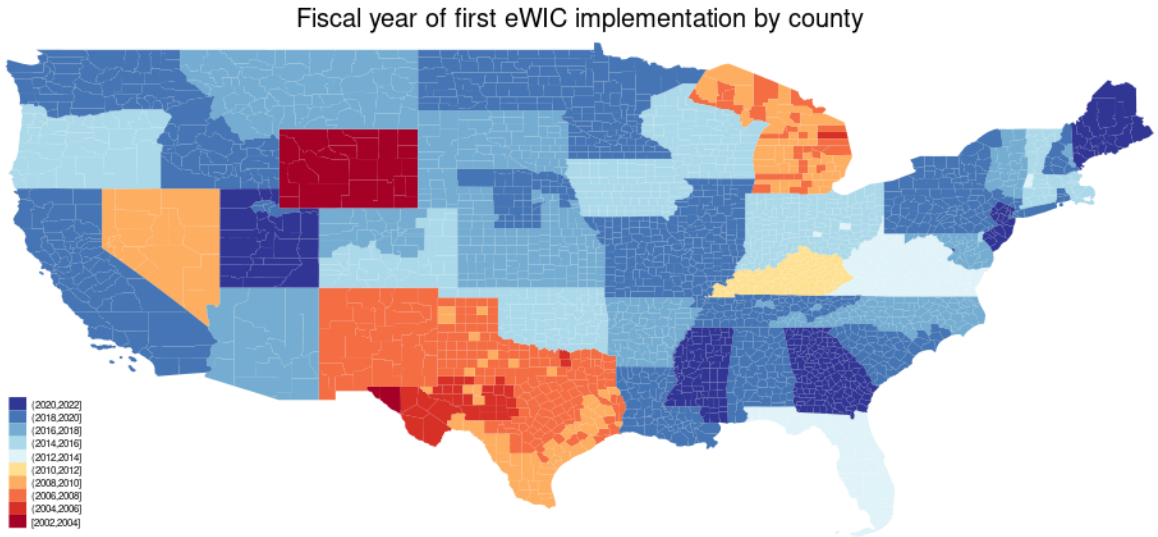
We collect data on the timing of EBT rollout for each of the 50 states and the District of Columbia. For each county, we record the date on which EBT implementation occurred, as reported in publicly available documents from state WIC agencies.[7] We also note whether a county is a pilot county. We model rollout with staggered permanent adoption, so that counties begin implementation at different times, and can only ever move from not treated to treated. This precludes complications like multiple events within a county. For some counties, the agency reports a range of implementation dates (for instance, 19 October 2015 to 16 November 2015 for Windsor County, Vermont). In this case, we record both the start and end date of implementation. We will in general use the start date of implementation in our analyses, although we will test robustness to this choice (see the Results section).

We observe the EBT transition in 40% (11,874 out of 29,288) of the ZIP codes in our sample (we note that there are fewer ZIP codes in our sample than in the US since not all ZIP codes contain stores). Since there are far fewer counties without a store in our sample, our sample covers almost all counties in the US (3,139) and we observe the EBT transition in 1,326, or 42%, of those counties.

Figure 2.4 shows the first fiscal year of eWIC implementation by county. From this figure we note that any state in which all counties have eWIC prior to fiscal year 2016 will be in our balanced panel of treated states at some point. States that do not implement eWIC prior to 2020 will be always be comparison units who are not yet treated for each treatment group. States that first implement eWIC between fiscal years 2016 and 2020 will serve as comparison units for early implementing states.

[7]Indian Tribal Organizations are not included in our analysis explicitly.

Figure 2.4: eWIC rollout



2.5.2 The Integrity Profile

We use data from The Integrity Profile (TIP) to construct our WIC store data and to measure WIC redemptions.

WIC authorized retailers

We use restricted-access data on the universe of WIC authorized retailers from The Integrity Profile provided through the cooperative agreement with USDA ERS. For fiscal years 2005–2018, we observe the vendor name, address information, and vendor type, as well as an indicator that the vendor is authorized in that year. We assign vendors to treatment timing based on the date at which the county the vendor is located in began implementing the relevant policy. The implementation start date captures when trainings for vendors are likely to be occurring as well as when the first participants will be receiving EBT and when EBT will be advertised to current participants and eligible populations. Treatment timing is not sharp because full rollout takes at least two months and in some cases more than a year. We also face a mismatch between treatment timing observed at the day level and outcomes observed at the fiscal year level. Our results are generally robust to defining treatment as a share of the year with EBT or a binary measure any part of the year with EBT.

We drop vendors listed as home food delivery contractors and direct distribution centers, since these types of vendors will in general not be transitioning to EBT[8] or represent participants' differences in the retailer experience after the package change. Together these two vendor types represent 640 out of 527,707 (0.12%) raw observations of authorized vendors.

When we visually examine the distribution of TIP stores, we observe that the distribution of stores approximately follows patterns of population density. This indicates that WIC stores are located where we expect them to be.

Figure 2.5: Location of WIC authorized food retailers

Redemptions data

We use ZIP level annual redemptions data from TIP for FY2009–2018. We received these records from USDA FNS via a Freedom of Information Act (FOIA) request. These data are reported for ZIP codes with 10 or more WIC authorized vendors. For each ZIP code in each year, we observe the sum of all WIC redemptions made within that ZIP code in that year.

Table 2.1 shows the share of total WIC redemptions in the US (column 1) and the share of total food cost in the US observed in our sample of large ZIP codes (those with at least 10 WIC vendors redeeming benefits in any year, column 2). We also show the number of ZIPs

[8]Mississippi and Vermont, the states with entirely non-retail WIC food distribution prior to eWIC, switched to retailer-based distribution when they implemented EBT.

in the TIP data that match to our redemptions sample (column 3) and the number that do not (column 4). We are only able to observe the level of national redemptions from FY2009 onward given the limitation of publicly available data on rebates received in FY2005-2008. In general we observe about 20% of all WIC redemptions in our sample.

Table 2.1: Share of WIC redemptions and ZIPs observed in our data

Fiscal Year	Observed WIC redemption share	Observed WIC redemption share of national food cost	Matched ZIPs	Unmatched ZIPs
2005	.	.3005255	577	15486
2006	.	.2736741	549	15358
2007	.	.265937	534	15336
2008	.	.2656398	530	15246
2009	.2023989	.2868938	566	15131
2010	.2040419	.2796799	565	15194
2011	.2283207	.2880793	634	15033
2012	.2161303	.2919797	616	14870
2013	.2186569	.3099126	619	14746
2014	.2075768	.2945765	596	14561
2015	.1982188	.2836222	561	14382
2016	.1885294	.2702682	527	14135
2017	.1808291	.2680154	509	14254
2018	.1673903	.2534139	456	14175

Observed WIC redemption share calculated as the ratio of WIC redemptions in our data to the sum of food cost and rebates received as recorded in FNS WIC data tables. We were only able to obtain rebate data for FY 2009-2019, via the Wayback Machine. In general rebates are about 1/4 to 1/2 of food cost. This means the unobserved ratio of WIC redemptions in our sample to total redemptions is about 66-80% of the always observed ratio of WIC redemptions in our sample to food cost. Ratio of WIC redemptions to total national food cost is thus an upper bound on the share of WIC redemptions in our sample since food cost includes rebates on infant formula while redemptions do not. Matched ZIPs have non-missing redemptions from our restricted use TIP data and at least one authorized store; unmatched ZIP have missing redemptions in restricted use TIP data and at least one authorized store.

To protect the privacy and confidentiality of both WIC participants and WIC vendors, we have no information on participant characteristics or type of food instrument redeemed in our data, nor are more detailed geography data available. This prevents us from examining heterogeneity in effects of eWIC implementation by any of these characteristics. That said, our data are the most detailed administrative data that cover all US WIC agencies that are available for study.

We also use a ZIP-to-county crosswalk from the Department of Housing and Urban Development by decade. No exact 1 to 1 mapping exists between ZIP codes and counties since some ZIP codes cross county borders. When necessary, we assign ZIP codes to treatment dates based on the earliest possible implementation date for the policy in the ZIP code, using the treatment timings for all the counties contained in the ZIP code. Mismeasurement of true treatment timing affects a small number of ZIP codes.

2.5.3 SNAP Store Tracking and Redemptions System (STARS) data

We use STARS data, obtained from USDA FNS by request, to estimate spillover effects on SNAP authorized vendors and redemptions from WIC EBT implementation. These are administrative data from USDA on vendors and redemptions in the SNAP program.

SNAP vendor data

We observe SNAP authorized stores from FY2005–FY2018. SNAP authorized store data includes the vendor's authorization start date, authorization end date, store type, and store address, including ZIP, state, and county. We use authorization start and end dates to infer whether SNAP vendors are actively authorized around the time WIC authorization data are collected, harmonizing the SNAP vendor data with the WIC vendor data collection frequency.

SNAP redemption data

We observe county-level SNAP redemptions for each month from FY2005–FY2018. We aggregate these data to the year level to make the frequency of the SNAP redemption data consistent with the WIC data. County-level observation makes this data easily mappable to the treatment timing data. SNAP redemptions data are suppressed for any county containing less than 3 SNAP retailers in any month.

2.6 Methodology

We wish to estimate the effects of the policy change in a given county on presence of WIC redeeming stores and WIC redemptions. Given that states vary in their timing, and counties are adopting at different times, it is important to document exogeneity of the policy. It is also important to allow for the possibility that treatment effects by location vary across time and over space according to when adoption happens. We take into account known possible problems with difference-in-differences research designs estimated using two-way fixed effects. The challenges of correctly estimating a parameter of interest (the ATE) in a two-way fixed effects (TWFE) model using ordinary least squares (OLS) with treatment effect heterogeneity are described in a recent and growing literature including Goodman-Bacon (2019), de Chaisemartin & D'Haultfeouille (2020), Imai & Kim (2019), and Callaway & Sant'Anna (2019), and Sun & Abraham among others.

We construct treatment-group specific estimates of the treatment effect for each of the policies following the procedure outlined in section III.C of Cengiz et al. (2019). We start by taking subsamples defined by the treatment groups for eWIC implementation and package change policies.

For eWIC, treatment group subsamples are defined by the state and fiscal year of treatment. For each treatment group, we then take the treated units for the relevant group and construct a set of comparison units across a balanced panel. The control units have constant treatment values for all periods. In all but one case, control units are untreated for the entire subsample.

Much of the literature about these possible issues emphasizes problems with treatment effects which vary across time in using previously treated units as "controls." Thus, we define a "clean" control as a unit that is untreated across the entirety of the balanced panel con-

structed (for k years prior to eWIC implementation for the treatment group through k years post eWIC implementation for the treatment group). As a specific example, if $k = 3$, stores or ZIP codes in Michigan treated in FY2008 will be compared to stores and ZIPs in any area treated after FY2011. This includes all states that implement eWIC from FY2012 onwards. This includes some states whose treatment effects we will evaluate (e.g. Ohio or Wisconsin) and some states we do not evaluate (e.g. New York or California). We allow a k year gap between the end of our data and the fiscal years for which we evaluate vendors or ZIP to ensure that all subsample panels can be balanced. When $k = 3$, the last fiscal year for which we evaluate ZIPs or vendors is 2015, as our data end in FY2018. Given a particular subsample of "clean" controls and treated units, we estimate a standard TWFE model using OLS, regressing an outcome y_{it} measured for unit i in fiscal year t on an indicator for treatment D_{it} in the unit and fiscal year as well as a constant α and unit i and fiscal year t fixed effects γ_i and δ_t, allowing for an error term ϵ_{it}.

$$y_{it} = \alpha + \beta D_{it} + \gamma_i + \delta_t + \epsilon_{it}$$

This method results in estimates of the average effect of the policy in each treatment group over the relevant window. We take a weighted average of the subgroup estimates by group size to represent the nationwide average effect of the policy. We thus avoid negative weighting issues induced by comparing later-treated units to units treated earlier in the sample. We can also examine heterogeneity in the treatment effects across treatment groups. To estimate the uncertainty around the overall average treatment effect, we use a wild bootstrap clustered at the unit level to construct a percentile confidence interval. This approach takes into account the covariance between the treatment group estimates that results from some of the same units being used as controls in multiple groups. For average treatment effects, all confidence intervals we show are constructed from the 97.5^{th} and 2.5^{th} percentiles of the bootstrap simulations. Confidence intervals shown for individual treatment groups

are constructed analytically in subsamples and do not take into account covariance between groups.

We generally consider authorization (at the individual store level and counts by ZIP code) and ZIP level redemptions. The administrative records we use for vendor authorization analysis - the Integrity Profile data - do not include a USDA assigned identifier for the vendor. We use vendor names and addresses to construct a unique identifier for name/address combinations. We treat this identifier as a vendor-level identifier. It is possible that there is measurement error in our definition of these identifier variables. This measurement error would affect our estimated changes within-vendor for authorization status. Authorization counts at the ZIP level will avoid these potential measurement errors by aggregating across all vendors in a ZIP. In this case, assuming the ZIP is recorded accurately in the TIP data, authorization aggregates by ZIP will be accurate counts.

Redemptions (at the ZIP level) are likely measured more precisely than WIC vendor authorization, since we use publicly available administrative data from USDA FNS for redemptions. Our vendor authorization data make assumptions about how vendors are defined (with name and address) and may be mismeasured if, for instance, a store's name changes without a disruption to authorization. However, the redemptions data only cover ZIP codes that FNS deems sufficiently large to avoid being able to identify single-store redemptions.

For ZIP level outcomes, in addition to estimating treatment effects in levels, we also standardize the outcome variables and estimate treatment effects in standard deviations.

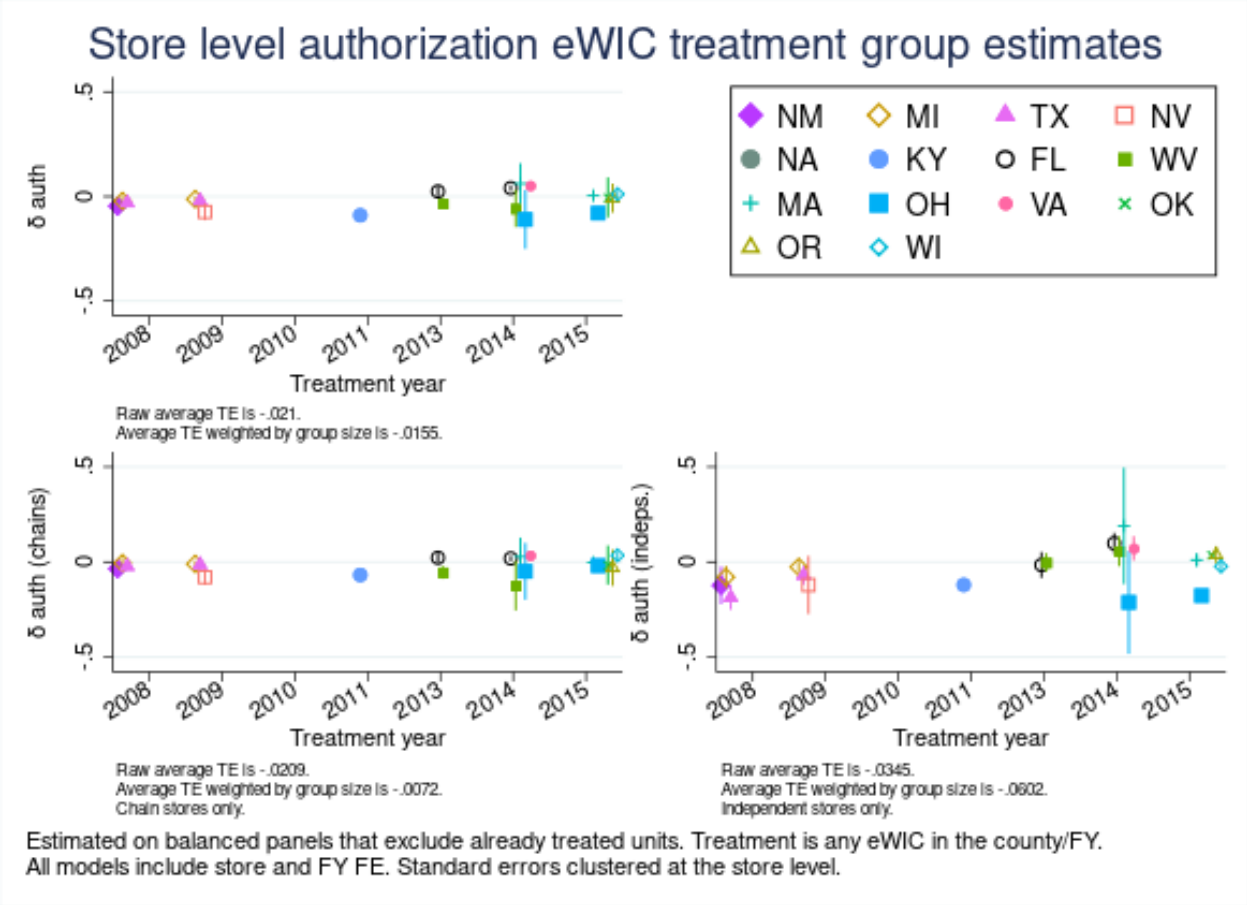
2.7 Results

Overall, we find minimal average effects of eWIC implementation on the number of WIC authorized retailers. Our estimation method allows us to examine treatment effects for individual subgroups while also constructing bounds on the estimates of the nationwide average

treatment effects that incorporate the uncertainty inherent in this estimate.

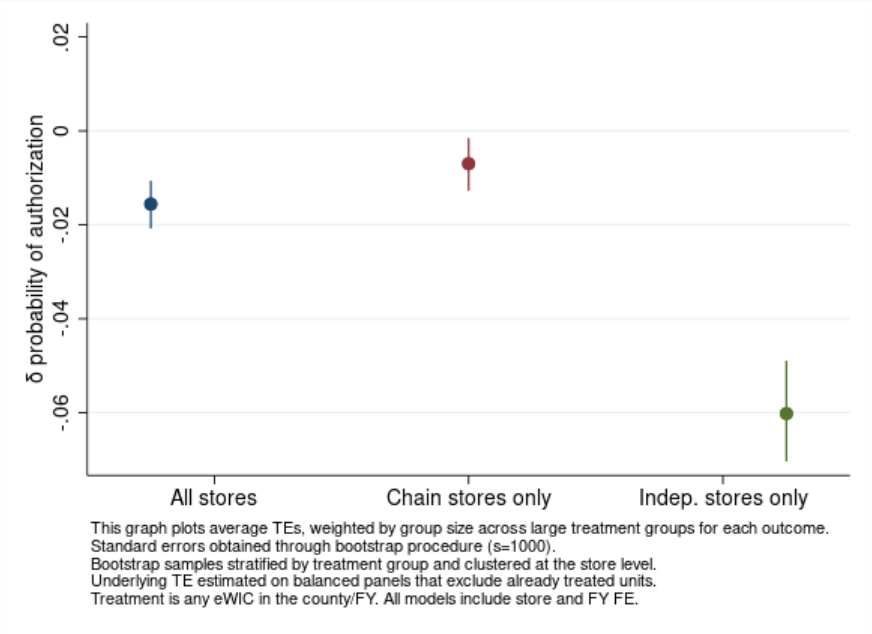
Results at the store level for eWIC show a general decrease in the probability that a store is authorized around eWIC implementation. These effects are similar in magnitude for the states that implement early relative to late, with small decreases in the total probability of being authorized spanning implementation dates from 2008 to 2015.

Figure 2.6: Store level authorization eWIC treatment group estimates



Decreases in authorization are largest for independent stores, with a weighted average treatment effect of -6% for independent stores compared to -0.8% for chain stores. This pattern holds for most of the treatment groups.

Figure 2.7: Average store level changes in authorization as a result of eWIC



Aggregated up to the ZIP level, we find a decrease of 0.06 stores per ZIP overall after eWIC implementation. Decomposing this effect into chain and independent stores, we find that most of the decrease (0.041 out out 0.06 stores per ZIP) is attributable to decreases in independent stores. Chain stores account for the remaining decrease of 0.02 stores per ZIP.

Figure 2.8: ZIP level authorization eWIC treatment group estimates

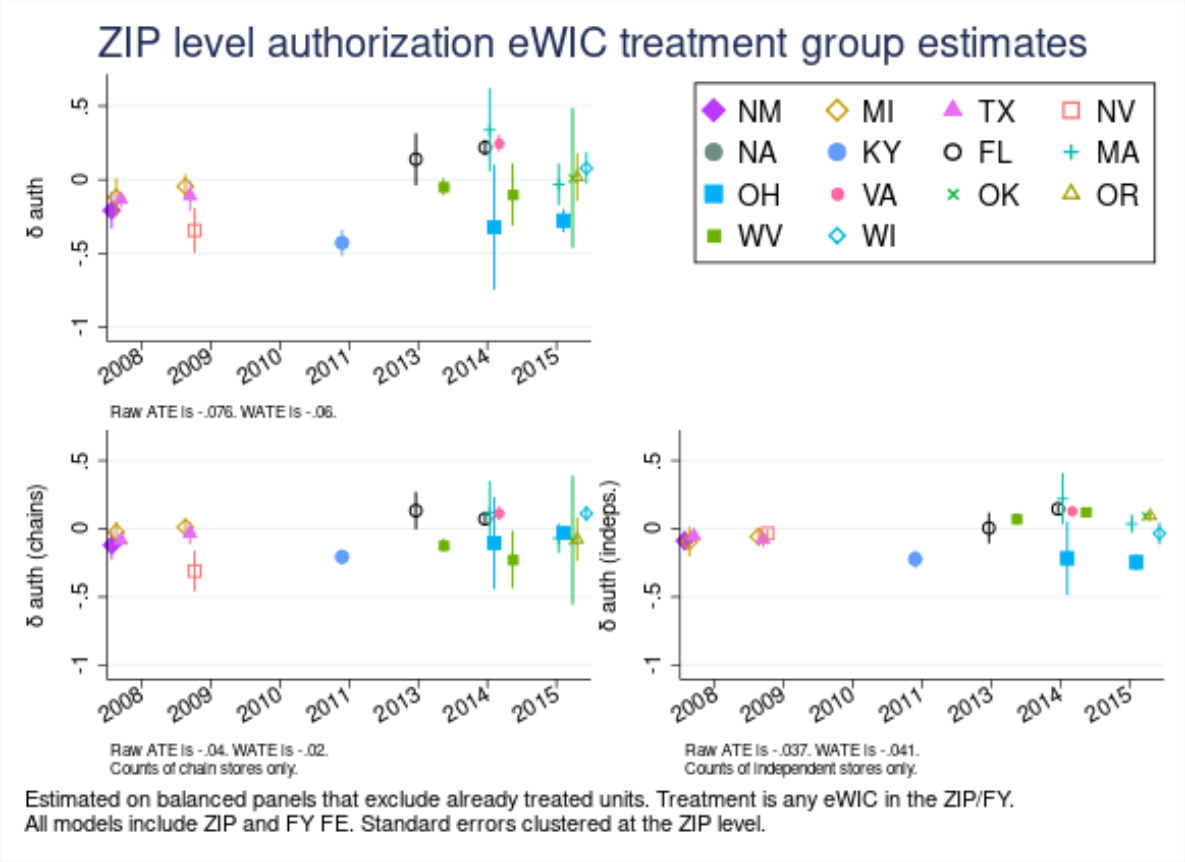
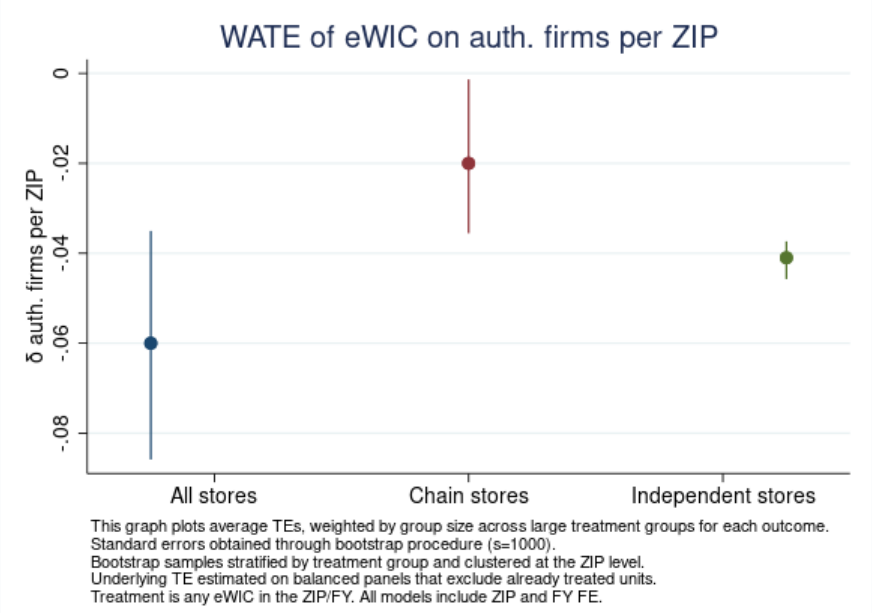


Figure 2.9: Average ZIP level changes in authorization after eWIC



However, we note that there are substantially more chain stores than independent stores per ZIP prior to eWIC implementation. To understand the change in stores relative to the pre-eWIC distribution, we analyze the change in counts of authorized stores, normalized to have mean zero and variance one within ZIP code. We interpret results from this normalized outcome as standard deviation changes in authorization or redemptions—by how many standard deviations of the original distribution did the outcome change after eWIC implementation?

The difference in magnitude between the effects on chain stores and the effect on independent stores becomes much more apparent after standardizing the outcome. No treatment group experienced more than a half of a standard deviation decrease in total, chain, or independent stores per ZIP after eWIC. On average, independent stores decreased by a three hundredths of a standard deviation, with fairly precise bounds estimated from our bootstrap procedure. The bounds on the change in chain stores per ZIP code after eWIC are less precise and include zero. The point estimate is 0.013 of a standard deviation increase in chain stores per ZIP as a result of eWIC. Overall there is a statistically insignificant effect of eWIC on the number of WIC authorized stores in a ZIP measured in standard deviations.[9]

[9]We also confirm an outcome from Meckel (2020), that independent stores in Texas were less likely to be authorized after eWIC implementation, and that the number of independent stores in ZIPs in Texas decreased after eWIC. However, our yearly data cannot entirely replicate the month-level dynamics reported in that paper, and we have data for a larger set of years than reported in Meckel. The replication results become less robust if we extend the set of years used for estimation beyond FY2007–2010.

Figure 2.10: ZIP level authorization (SDs) eWIC treatment group estimates

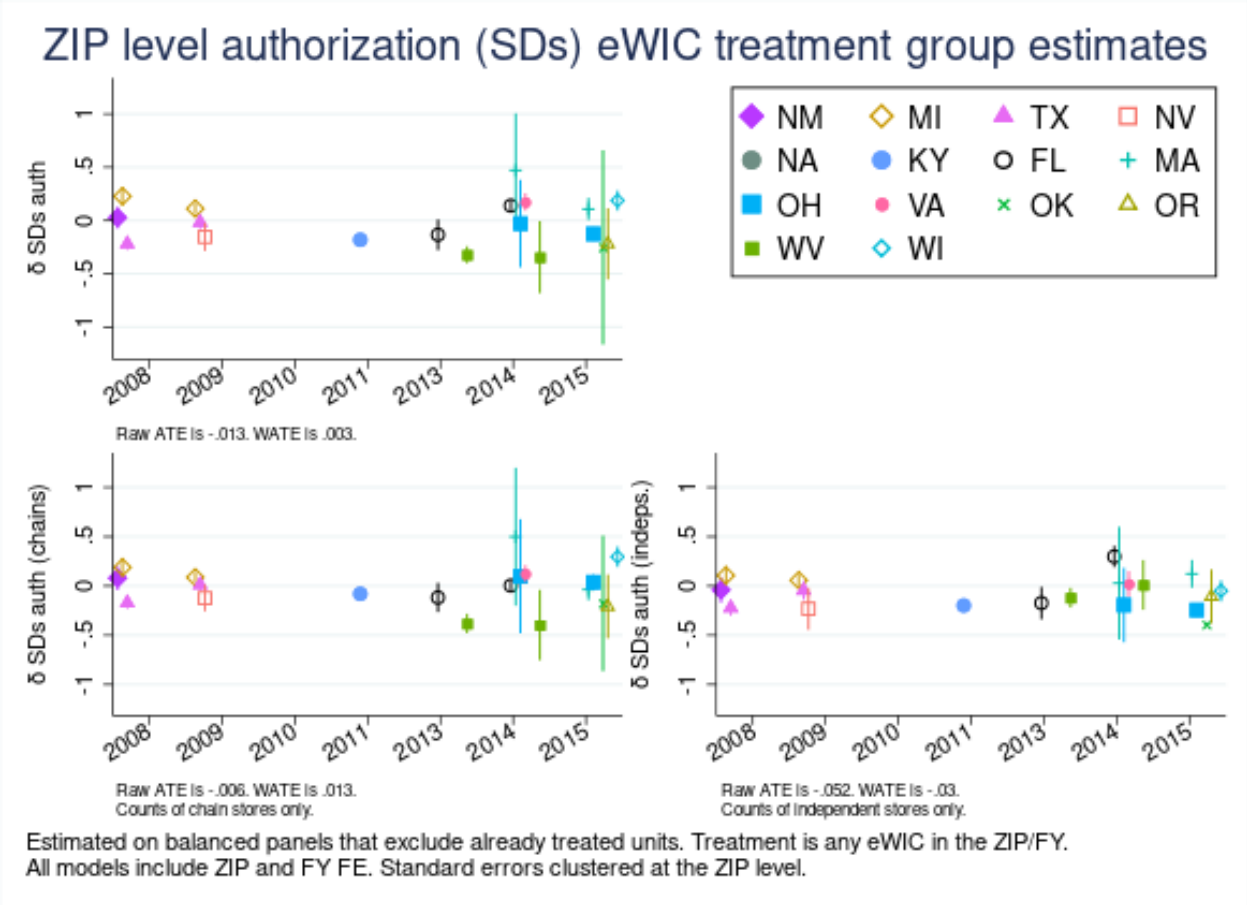
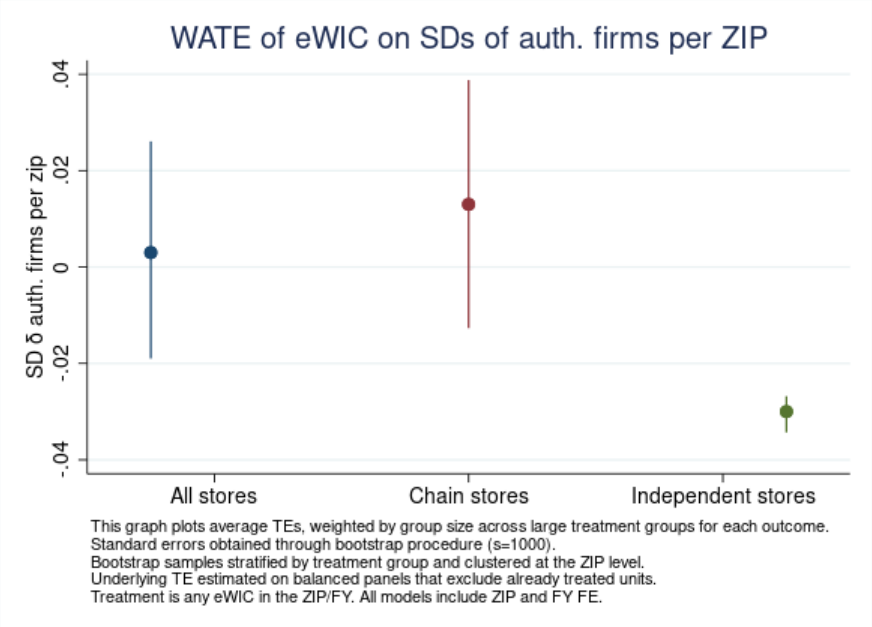


Figure 2.11: Average ZIP level SD changes in authorization as a result of eWIC



In addition to store counts and authorization, which can indicate WIC shopping accessibility for participants, we consider WIC redemptions as an outcome. WIC redemptions measure the intensity with which participants are using their benefits. We show redemption outcomes in standard deviations (SDs) as discussed above to avoid wide ranges in outcome sizes across ZIPs that are driven by ZIP size.

Figure 2.12: ZIP level redemption (SDs) eWIC treatment group estimates

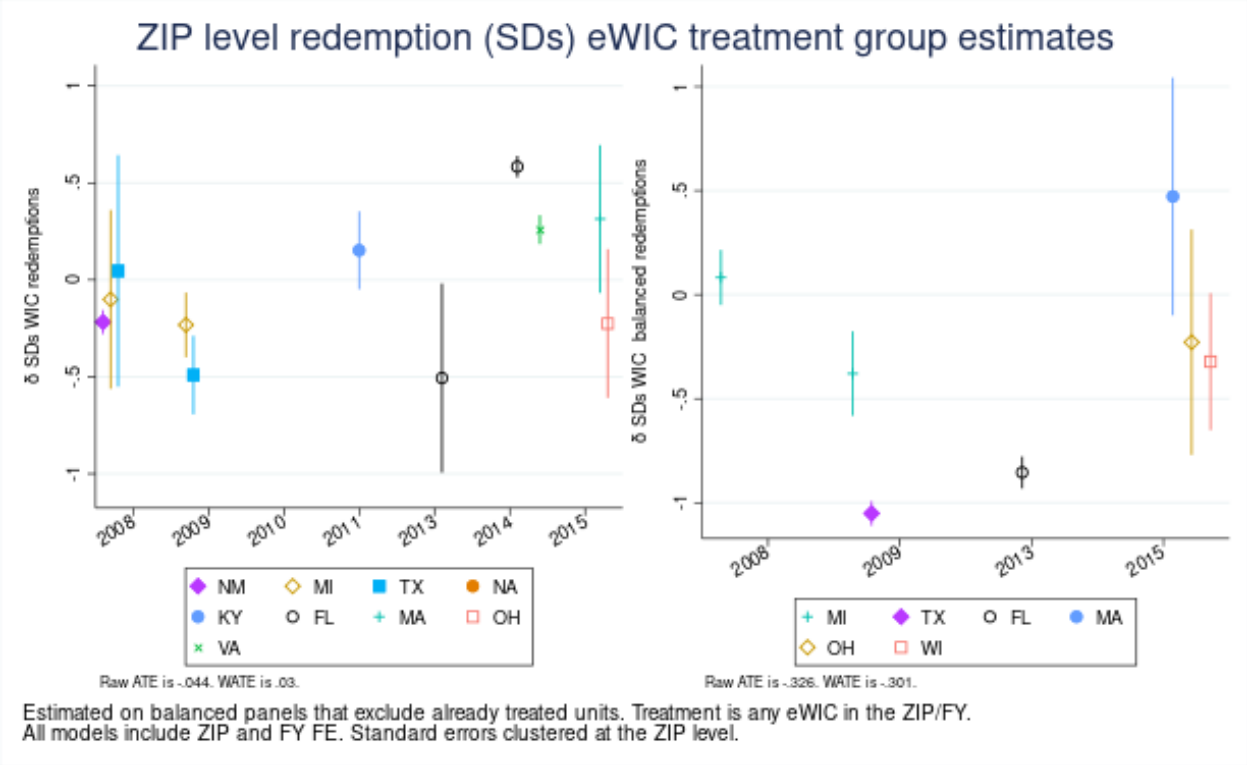
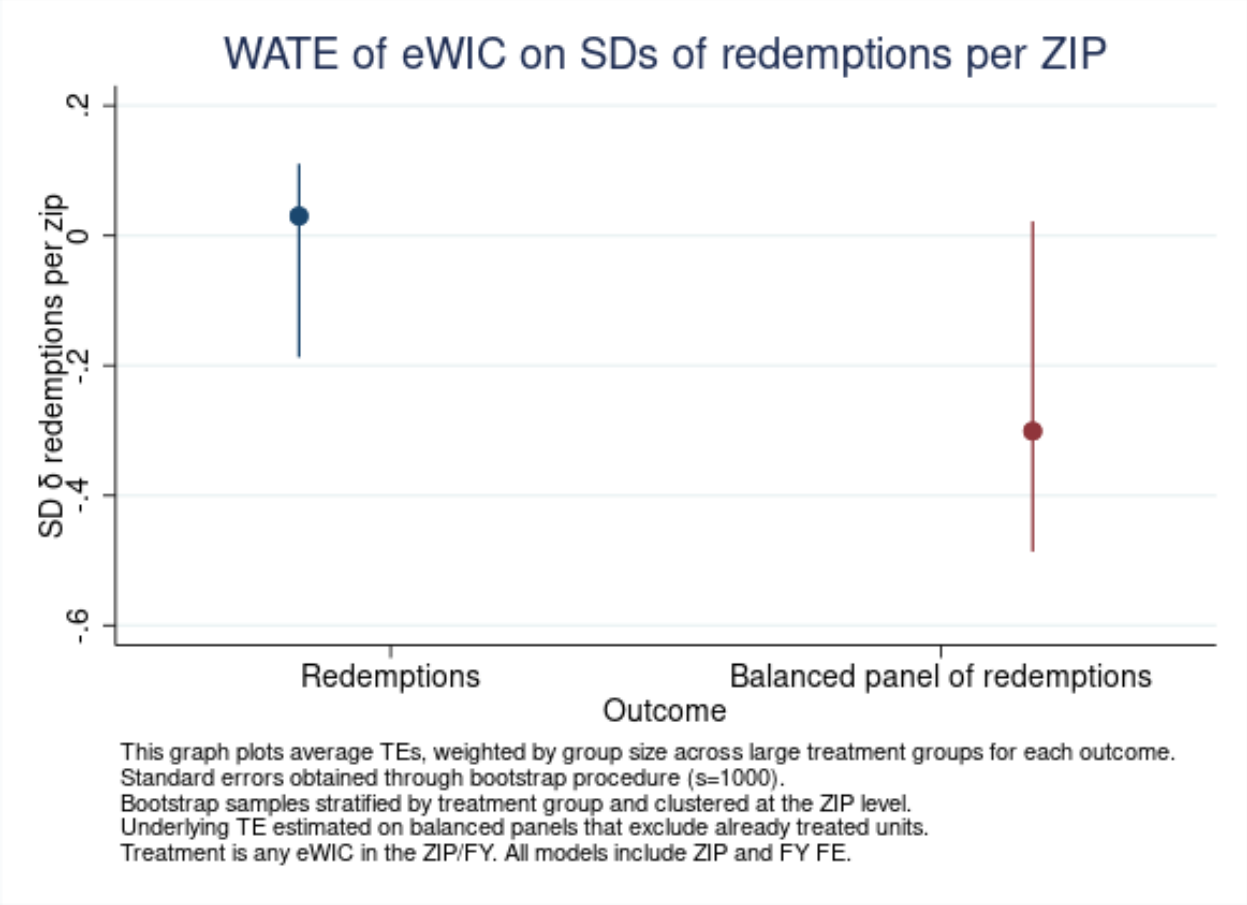


Figure 2.13: Average ZIP level SD changes in redemptions as a result of eWIC



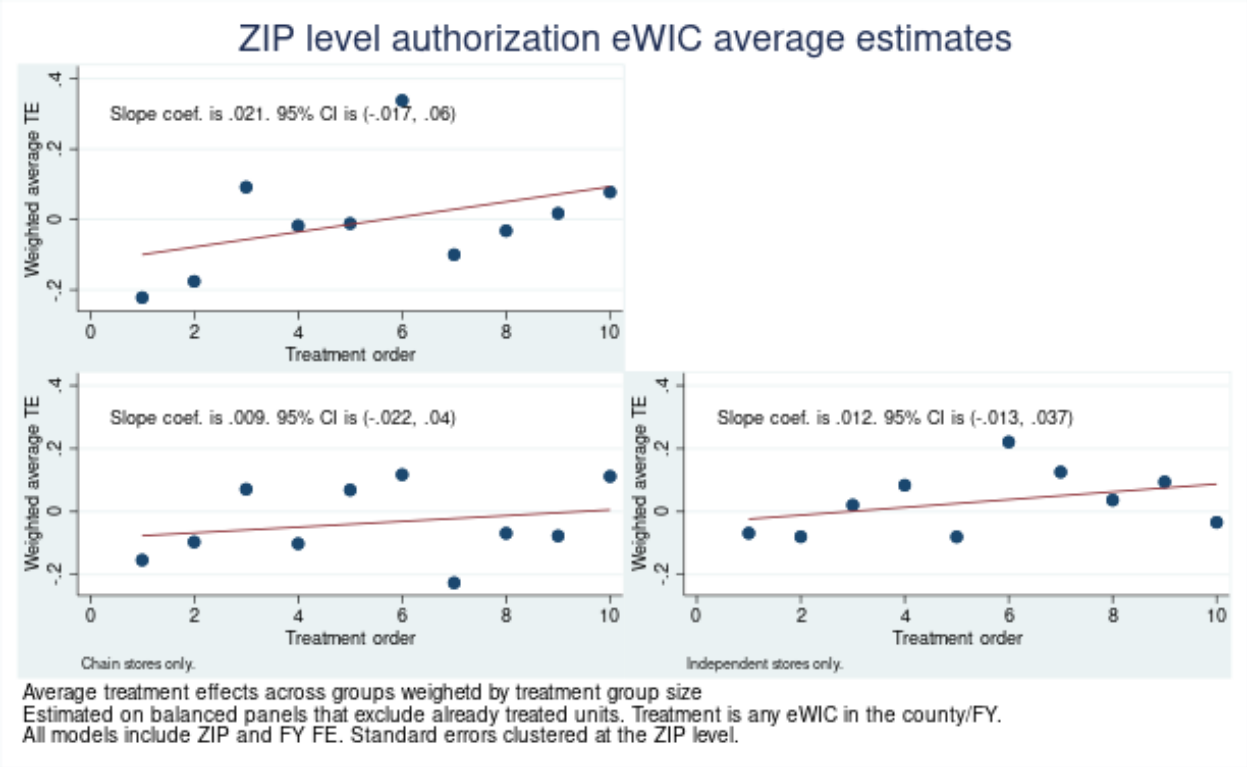
We note that significantly negative effects on redemptions after eWIC implementation only occur for early implementing treatment groups in our sample, while significantly positive effects occur in later years. Later implementing states may be able to learn from earlier implementing states, may have access to better base technology for electronic benefit transfer, and may be contracting with a EBT service provider (e.g. Solutran) that has more experience with WIC EBT in later implementation years.

Figure 2.14 suggests an association between the order of implementation and the weighted average treatment effect of eWIC on the change in the number of WIC vendors authorized in a ZIP code. Each panel of the figure shows that as processors (Solutran, FIS/CDP, and Conduent) gain experience implementing eWIC in states the average effect of eWIC on

vendors becomes more positive. This association holds for both chain (lower left) and independent (lower right) vendors, with a larger association for independent vendors, although none of the relationships are statistically significant. We caution against inferring too much from these results, and consider that they provide a possible mechanism for our findings and suggest direction for further research. Other possible explanations could be the use of least expensive brand rules, the breakdown of retailers using stand alone and integrated redemption machines, general economic conditions, or whether the new food package has been implemented.

We also note that redemption outcomes are generally less positive for ZIP codes in our balanced panel (where we observe no missing values for that ZIP throughout the sample). These are the largest ZIP codes, since redemptions are unobserved when there are a small enough number of vendors in the ZIP that releasing redemptions could reveal redemptions data by vendor.

Figure 2.14: ZIP level authorization eWIC estimates by processor and order of implementation



2.8 Spillover Effects

Ex ante we expect that the largest effects of this policy change in WIC will be on the presence of WIC authorized stores and WIC redemptions. However, the similarity in technology between WIC EBT and SNAP EBT suggests that WIC EBT implementation may lower costs of SNAP participation for affected stores. For instance, stores that were not previously enrolled in either WIC or SNAP may have been deterred by technology requirements in SNAP and less profitability in WIC. For most vendors, SNAP will be more profitable than WIC; likely exceptions include A50 vendors. With the programs using similar technology, vendors may find enrolling in them both feasible and profitable. We use the methodology outlined above to examine the effect of eWIC on SNAP authorization at the store level and at the ZIP level. We use the entire set of STARS data for this analysis so that our findings coverall authorized SNAP stores at any point in time. Our findings suggest that eWIC increases

in the number of SNAP authorized vendors per ZIP in most of our treatment groups (10 out of 14) (figure 2.15), with an average increase of 0.28 stores per ZIP, mostly driven by independent stores (see figure 2.16). A store level analysis finds that on average, a firm's probability of being SNAP authorized very slightly increases after eWIC implementation (see figures 2.17 and 2.18).

Figure 2.15: ZIP level SNAP authorization eWIC treatment group estimates

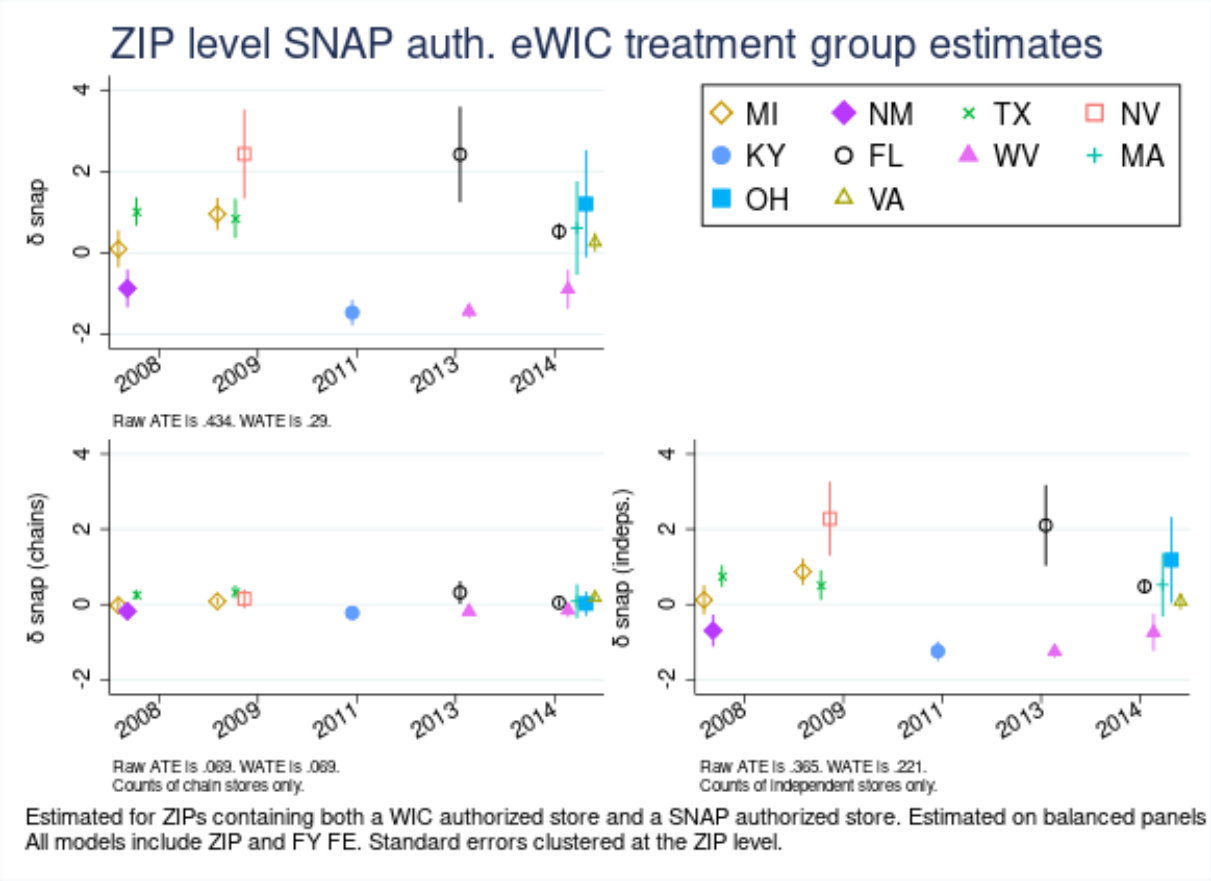


Figure 2.16: Average ZIP level changes in SNAP authorization as a result of eWIC

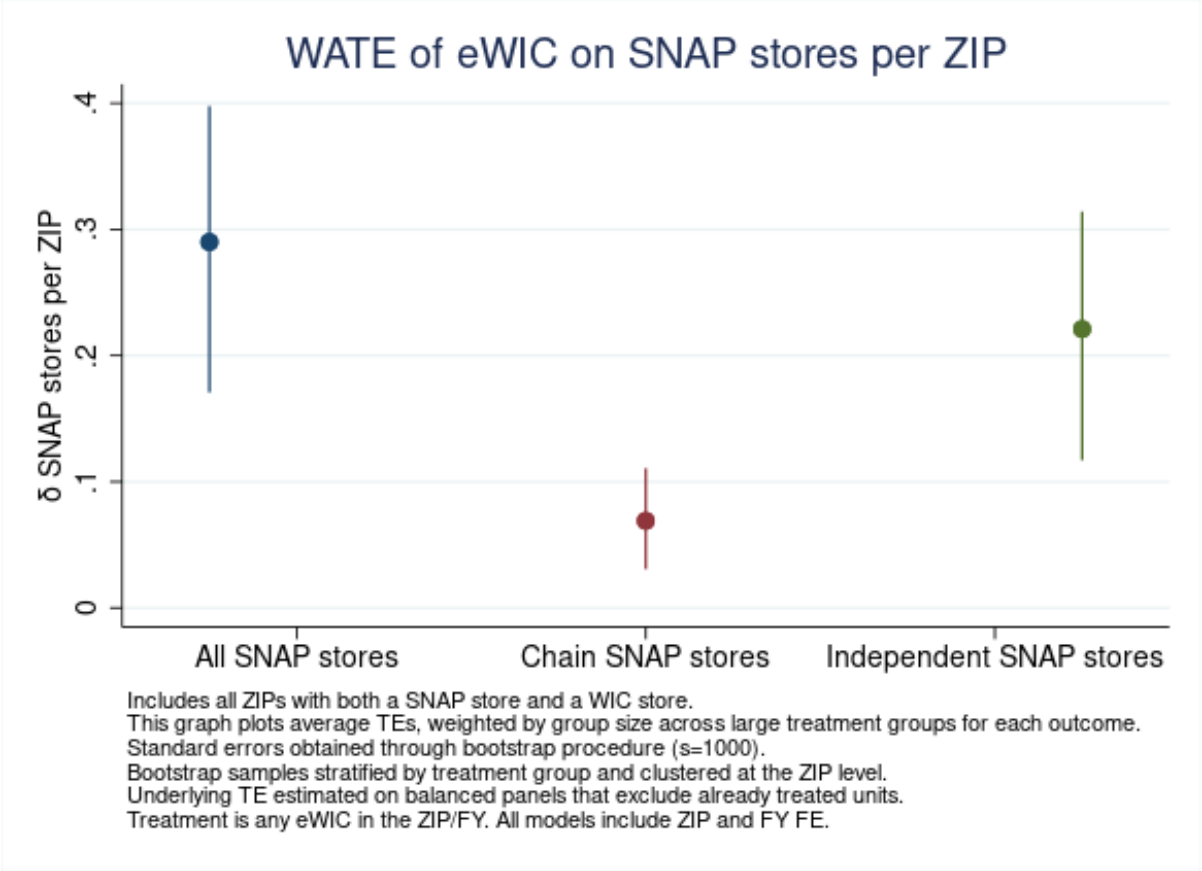


Figure 2.17: Store level SNAP authorization eWIC treatment group estimates

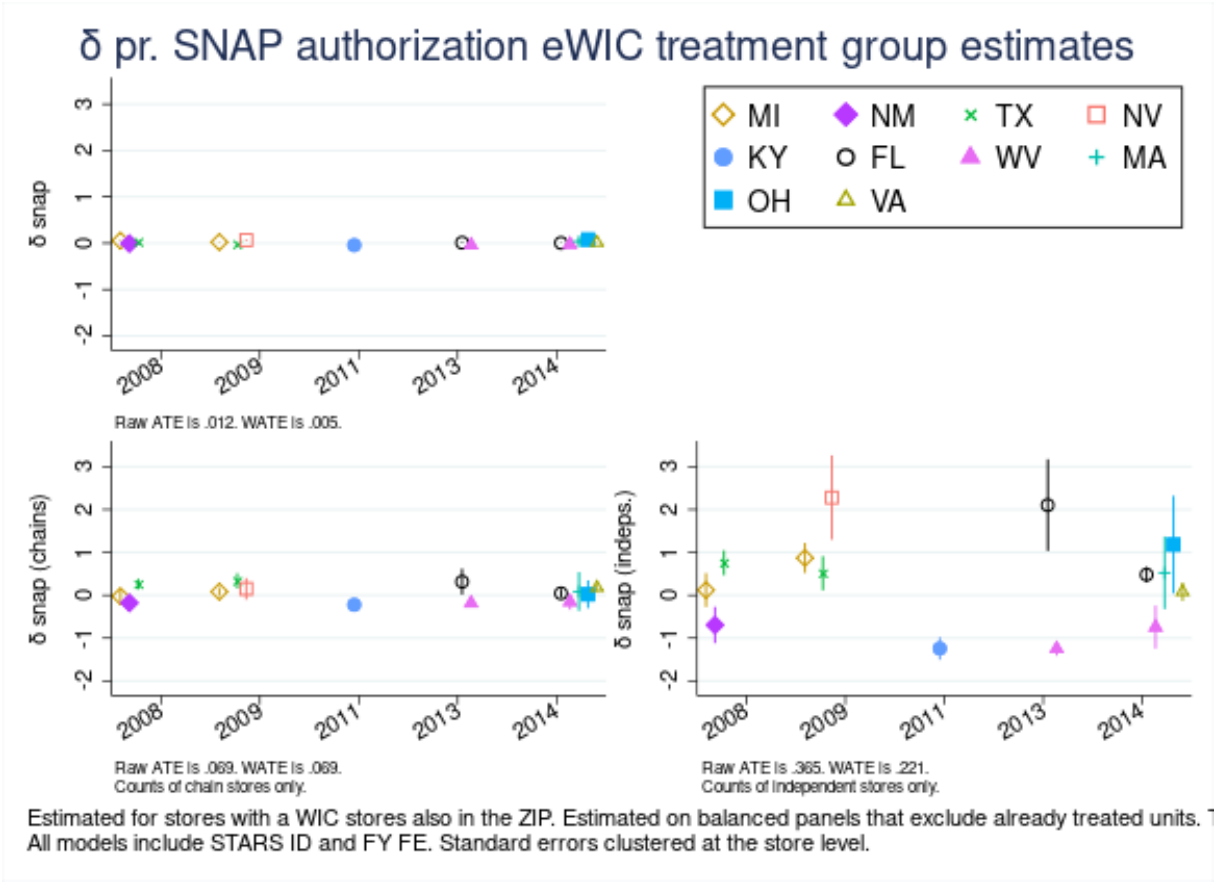


Figure 2.18: Average store level changes in SNAP authorization as a result of eWIC

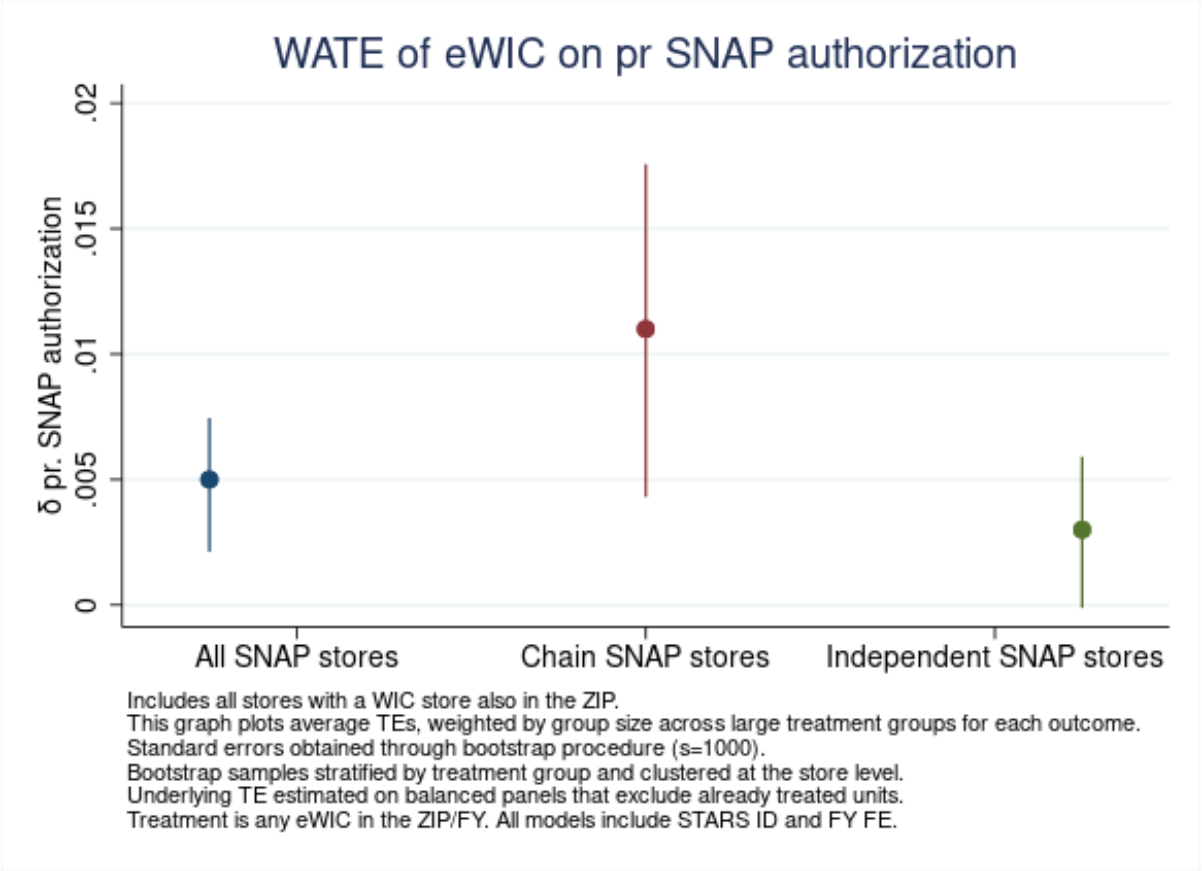


Table 2.2: Observed share of SNAP stores authorized only after eWIC (switchers)

Share switchers	Total switchers	Total stores
.1120334	54176	483570

Table 2.3: Observed share of SNAP stores authorized only after eWIC (switchers) with store characteristics

	Chain?	Share switchers	Total switchers	Total stores
Wholesaler	1	0	0	195
Commissary	0	.01	26	2379
Cooperative	1	.03	78	2301
Fishmonger	1	.04	585	14222
Supermarket	0	.05	14586	322101
Small Grocery	1	.05	8983	173303
Butcher/Carniceria	1	.06	1222	21268
Small Grocery	0	.06	26689	461422
Super Store	1	.06	2171	37323
Wholesaler	0	.062	26	416
Fishmonger	0	.06	3120	49543
Medium Grocery	1	.06	5655	89037
Large Grocery	1	.06	1586	24934
Medium Grocery	0	.06	14170	218842
Cooperative	0	.06	845	13039
Super Store	0	.07	19019	290758
Butcher/Carniceria	0	.07	5564	84630
Large Grocery	0	.07	4654	62959
Green Grocer	1	.09	1001	11713
Green Grocer	0	.09	3978	44616
Bakery	1	.09	2080	23257
Supermarket	1	.09	5850	65234
Delivery	1	.10	416	4030
Combination	0	.11	99294	926042
Bakery	0	.12	10452	87932
Convenience	1	.12	90246	726011
Combination	1	.14	8008	57187
Delivery	0	.15	3939	26871
Convenience	0	.15	343239	2310555
Farmers' Market	0	.19	10920	56329
Farmers' Market	1	.20	1846	9451
Direct Market Farm	1	.23	1079	4758
Direct Market Farm	0	.24	12922	54340

Comparing results for chain stores and independent stores, we see that the estimated increase in SNAP stores per ZIP is mostly driven by increases in SNAP participation among independent stores. The estimated effect on chain stores is only marginally statistically distinguishable from zero. The estimate for independent SNAP stores is about 0.2 stores per

ZIP while that for chain SNAP stores is less than 0.1 stores per ZIP. The overall estimate of the ZIP level effect is the sum of these two, at a statistically significant 0.3 store per ZIP increase in SNAP authorization (see figure 2.16).

Table 2.2 begins to show the incidence of SNAP stores that switch from being unauthorized to authorized only after eWIC implementation. One hypothesis is that these stores are taking advantage of the reduced costs of implementation, given that they must also have EBT technology in place to be WIC authorized. Table 2.2 shows the share and number of "switchers" (those only unauthorized prior to eWIC and authorized in a period after eWIC) in our sample.

Table 2.3 looks more carefully at switchers from non-SNAP to SNAP after eWIC implementation, breaking them down into groups defined by chain status and store type as defined in STARS. We note that other trends in food retailing (rise of dollar stores) and SNAP policy (farmers market promotion experiments) may also be driving these changes. In this table we rank the groups (as defined by store type and chain status) from lowest share of switchers after eWIC to highest share.

In Table 2.3, we impute switchers as those who are only authorized after eWIC implementation. Given time trends in store types (e.g. general increase in number of dollar stores over time) and changing SNAP policies (e.g. farmers market promotion programs) it is quite plausible that we are lumping in spurious changes in SNAP store type composition changes over time with our so-called switchers here.

We note that we are comparing many estimates of treatment effects and that considering multiple testing concerns it is plausible that these small point estimate effects are not statistically significantly different from zero. Taking the store level outcomes into consider-

ation along with the ZIP level outcomes, the weight of evidence suggests a small, although not robustly significant, increase in SNAP authorization after eWIC implementation. These results indicate that potential decreases in the authorization of independent WIC vendors are not associated with spillovers to the authorization of SNAP vendors—whether chain or independent.

2.9 Discussion and Conclusion

We have comprehensively examined the response of WIC vendors to the implementation of an important policy change in WIC—the transition from paper vouchers to electronic benefit transfer (EBT). Moving to EBT (eWIC) is a large policy change in WIC that stakeholders expected to improve the experience of participants by reducing stigma and increasing flexibility when obtaining supplemental foods. Policymakers also expected that EBT would decrease fraud among vendors. EBT constituted a substantial technological shift for the food retailers authorized to redeem WIC food benefits, requiring additional point-of-sale equipment and software as well as training. If EBT increases demand for WIC while potentially increasing costs, the net effect on vendors is *ex ante* unclear. This paper estimates the change in vendor authorization and participant benefit redemption in response to this policy change.

We use data from The Integrity Profile (TIP) provided by USDA FNS. With more than a decade of administrative data on the authorization of WIC vendors across the country, we are able to examine effects of this policy change in most states in which it has been implemented. This extends previous research on WIC EBT implementation which had focused on at most a few states. In addition, we incorporate advances in the econometric literature on difference-in-differences approaches in the presence of treatment effect heterogeneity across time and place of policy change to estimate our results.

We find that EBT implementation led to a small yet significant decrease in the number of WIC authorized independent vendors and no significant change in the number of WIC

authorized chain vendors. Finding decreases in independent vendors suggests that participant access to authorized food retailers may be declining. Examining changes in the value of benefits redeemed (standardized across ZIP codes) shows no significant change in redemptions on average across ZIP codes. This suggests that the amount of supplemental foods obtained by participants in the ZIP codes for which we have data do not change significantly after WIC EBT implementation. This indicates that the demand side effects of WIC EBT - increasing the flexibility of the redemption schedule, changing checkout processes to potentially reduce stigma, and increasing transparency of available goods through a phone and/or web app outweigh increased costs if access becomes more difficult.

We examine one potential mechanism for the decline in authorized vendors after EBT implementation: vendor interaction with the processor implementing EBT. Three firms—Conduent, FIS/CDP, and Solutran—implemented EBT in most states that have made the transition through 2015. We find evidence to support the hypothesis that each of these firms improved their implementation of WIC EBT as they had more experience with the implementation process. The average effect of WIC EBT implementation on vendors became more positive as processors had more experience implementing EBT. Processors may learn from previous implementations and incorporate that learning to improve subsequent implementations.

We consider potential spillover effects of WIC EBT implementation on SNAP authorization since the technology used to redeem WIC benefits after EBT is similar to the technology used to redeem SNAP benefits in the years in our sample. This suggests a positive relationship between SNAP and WIC EBT. In addition, some states require SNAP authorization for vendors to be WIC authorized. If WIC EBT decreases vendor authorization, the relationship between SNAP and WIC EBT may be negative. Our results generally show positive spillover effects of WIC EBT on the authorization of SNAP vendors. The magnitude of neg-

ative spillover effects from decreasing WIC authorized independent retailers are outweighed by positive effects from technology overlap.

Overall, this paper indicates that recent policy changes in WIC have led to a decrease in the number of non-chain WIC authorized food retailers. WIC redemptions and SNAP authorized retailers suggest that this change in authorization is not spilling over into effects on WIC redemptions or the availability of SNAP vendors.

Essay 3

Birthweight effects from changing access to authorized WIC vendors

The opinions expressed herein represent those of the author and do not necessarily represent the position of the State of California.

3.1 Introduction

Large nutrition assistance programs in the US rely on participants' access to private firms in order to obtain their supplemental foods. One such program is the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). WIC's objective is to improve the health of pregnant and postpartum people as well as infants and children up to age 5. It is important to understand if changes to vendor access result in meaningful health changes for the affected population. Prior work (Meckel, Rossin-Slater, and Uniat 2021; Meckel 2020) establishes that loss of access to private firms that are authorized to redeem benefits can affect participants' use of WIC benefits. In this paper, I extend this literature to answer the question: does losing access to authorized food retailers worsen food assistance participants' health outcomes? Answering this question expands our understanding

of how private actors' behavior mediates participants' outcomes for food assistance programs.

In this paper I focus on birthweight as a health outcome for children and pregnant people. Birthweight is a widely available and well-studied measure of fetal health that is linked to later life health, social, and economic outcomes (Almond and Currie 2011a). Having high enough birthweight and good fetal growth is thus a pregnancy outcome that carries substantial economic benefits as well as health benefits, as it can affect outcomes across the life course. Although birthweight may not always respond to negative shocks *in utero* or capture fetal health in all ways that matter for development (Almond and Currie 2011b), it is an outcome frequently used in the nutrition assistance literature - both for WIC and the Supplemental Nutrition Assistance Program (SNAP).

This paper examines how birthweight responds to exposure to the removal of an authorized WIC vendor due to disqualification during pregnancy. I identify pregnant WIC participants that redeem benefits at vendors that go on to be disqualified from WIC. Disqualified vendors are prohibited from redeeming WIC benefits due to noncompliance with program regulations. I link pregnant participants to the child they give birth to out of the pregnancy if the child enrolls in WIC and has a birthweight recorded in the WIC administrative data. I allow potential heterogeneity in treatment effects by trimester of exposure by estimating these effects separately. I account for known challenges in estimating birthweight effects of WIC by incorporating gestational age when constructing outcomes and accounting for the selection into observing birthweight (birthweight is only observed for infants that are enrolled in WIC after the pregnancy). My empirical approach identifies the causal effect of the removal of a WIC vendor due to disqualification on birth outcomes for infants that received WIC and were exposed to the disqualification *in utero*. This causal estimate expands our understanding of how access to authorized retailers affects food assistance participants health outcomes.

Prior work on WIC effectiveness establishes generally positive effects of WIC participation during pregnancy on birthweight and fetal growth for gestational age, although the magnitudes of those estimates vary across the literature (Bitler and Currie 2005; Joyce, Gibson, and Colman 2005; Joyce, Racine, and Yunzal-Butler 2008; Kowaleski-Jones and Duncan 2002; Ludwig and Miller 2005). Chapter 1 establishes that WIC participants in California that are exposed to the disqualification of an authorized retailer are less likely to participate in WIC. Connecting these strands of the literature, in this paper I examine whether decreases in participation due to changes in vendor access result in worse health outcomes for participants. This question increases our understanding of the effects of private providers of food assistance benefits on participants. In addition, this paper contributes to the literature on WIC participation and birth outcomes, accounting for known challenges (selection into participation and gestational age) with econometric methods and administrative data. Finally, this research helps quantify the costs of removing vendors from WIC, providing a value to balance against the benefits to the program and participants of fraud reduction.

This paper proceeds as follows: Section 2 discusses the data; Section 3 lays out the methods used to estimate the results; Section 4 presents the results and discusses them; and Section 5 concludes, summarizing the results, discussing the limitations of the paper, and suggesting policy implications.

3.2 Data

To answer the question of how a vendor disqualification affects birthweight of infants who were exposed to the disqualification *in utero*, I combine three data sources to generate a linked data set. The three data sources I combine are: 1) data on sanctions and disqualifications of authorized WIC vendors between 2015 and 2019, 2) data on redemption of WIC benefits from 2015 to 2019 that include the ID of the participant redeeming benefits and the ID of the food retailer that the benefits are redemeed at, and 3) participant certification data. I will provide more detail on each of these three data sources below. The final linked data set

observes each infant once, with a unique ID for each infant, an indicator of their treatment status (exposed to the DQ or not, and in which trimester the DQ occurred if treated), their birthweight, gestational age in weeks, date of birth, and the date of the DQ. I include additional demographic variables that I will describe in the Participant Data section below.

3.2.1 Sanction and Disqualification Data

Food retailers apply to CDPH/WIC to become authorized as WIC vendors. If the application is accepted, the vendor must comply with federal and state regulations in order to maintain vendor authorization. CDPH/WIC may conduct unannounced compliance buys, routine monitoring visits, or audits of inventory and records to assess vendors' compliance with regulations. If these monitoring efforts indicate that the vendor is not complying with the program regulations, the vendor may be sanctioned. Potential sanctions include disqualification (DQ), which means that the vendor is removed from authorization. Sanctions also include fines and citations. A DQed vendor is no longer able to redeem supplemental food benefits from WIC participants. Prior to any DQ, CDPH/WIC conducts a participant access determination to assess whether or not another WIC authorized retailer is geographically accessible to participants that redeem benefits at the potential DQ vendor. If no other potential vendor is available, the DQ will not proceed and the vendor will instead be subject to fines. For additional details on WIC vendor disqualification, see Chapter 1.

Disqualifications become effective on a particular day, creating a sharp boundary between when participants can redeem benefits at the vendor and when they may not. I assume that the timing of these disqualifications is random from the perspective of the participant. In general, vendor disqualifications result from actions in which participants are not involved, such as overcharging participants, failing audits, or not posting prices or signage. I assume that the timing of the vendor disqualification treatment is as good as random for the participants.

3.2.2 Redemption Data

Redemptions data link WIC vendors and WIC participants. Redemption data contain records of each food instrument (FI) exchanged by a WIC participant for supplemental foods at an authorized food retailer. For each redemption, the administrative data contain records of the unique serial number assigned the FI, the unique ID for the participant the FI was issued to, the unique ID of the vendor that redeems the benefits, the amount the participant redeemed the FI for, an approximate date on which the participant redeemed the FI, the type of FI - a unique identifier that can link to the supplemental foods allowed in the FI, and the price ceiling that applies to that FI at that time. Identifiers for vendors and participants reveal where participants prefer to redeem their benefits. This allows me to link vendor data, including treatment timing, to participant data about pregnancy and birth outcomes. For this paper, I will only use the identifying information and timing from the redemption data.

3.2.3 Participant Data

The administrative data I use include participant data for pregnant people and infants that I will use to link the disqualification treatment to infants that are exposed to the disqualification *in utero* and then have their birthweight recorded in the WIC administrative data. Birthweight data are collected at the WIC clinic when the infant is enrolled in WIC. This means that there is some selection into observing the birthweight data - the infant that results from the pregnancy must be enrolled in WIC in order to observe the birthweight. As found in Chapter 1, participants that are exposed to the disqualification are less likely to continue in WIC, so that participants that do continue or choose to enroll their children may be systematically more attached to the program. Participants who are more attached to the program may be more likely to follow prenatal care or nutrition advice and thus have higher birthweights all else equal. An alternative narrative is that participants are more attached to the program because they are needier. Lower SES individuals have systematically lower birthweights (Abu-Saad and Fraser 2010). In either case, selection is an issue in this setting.

I will address the issue of selection in the methods section.

I use other participant data from the administrative data source. For pregnant participants, I observe unique identifiers for the individual as well as the family they belong to. Unique family IDs allow me to link parents and children, which I discuss below. I observe certification start and end dates for the participant, an expected delivery date, and the end date of the previous pregnancy (if applicable). For infants, I observe their unique individual participant identifiers as well as their family ID. I observe the infants' certification start and end dates, date of birth, and birthweight (measured in ounces).

3.2.4 Linking data

I construct two major links for this paper: one between sanctioned vendors and pregnant participants, and one between pregnant participants and the infants that result from those pregnancies. To link sanctioned vendors and pregnant participants, I follow a very similar procedure to that in Chapter 1. First, I link the vendor IDs of the DQed vendors on to the redemption data for all participants. I restrict this data to households that are ever exposed to a disqualification. I further limit the sample to households that experience only one disqualification, to eliminate challenges of estimating treatment effects for multiple treatments. Also, households that are treated multiple times may not represent the general population of WIC participants. The resulting dataset contains only participants that are ever exposed to a DQ, with their treatment timing and individual and household identifiers.

The next linkage brings together data from pregnant participants and their babies. I start with the set of treated households that ever contain a pregnant participant. To link participants I rely on the fact that WIC participants from the same family are assigned a unique family identifier. For pregnant people, the administrative data contain an expected delivery date (EDD). This expected delivery date is a benchmark for 40 weeks since conception (full term). I use a fuzzy match with a bandwidth of 6 weeks prior to the EDD and 4 weeks

after the EDD to link infants born within the range defined by the band around the EDD to pregnant participants within the same family. In future work I will assess robustness to different bandwidth choices. I then assign the expected delivery date of the participant to the infant that results from the pregnancy.

With these two links, I have vendor disqualification timing linked to participant data with birthweight information (where not missing), expected date of delivery, and actual delivery date (the child's date of birth). The final data set contains one observation per individual. It contains the universe of children that are ever exposed to a disqualification. This includes infants that are exposed to the disqualification *in utero*. In the methods section, I describe how I compare treated children - those exposed in one of the trimesters of pregnancy - to other children exposed to a DQ but not during pregnancy.

3.2.5 Constructing outcomes

Using the above data, I need to construct some additional variables to implement the estimation method. First, previous papers in the literature on WIC participation's effect on birthweight note that gestational age is a significant determinant of birthweight. Also, people that are pregnant for longer (higher gestational age) have more time in which to enroll in WIC. Together, these relationships imply that WIC participants may have higher birthweights than non-WIC participants simply due to differences in gestational age (Joyce, Gibson, and Colman 2005; Joyce, Racine, and Yunzal-Butler 2008). To account for this concern, I use low birthweight at term (LBW term) as an outcome. LBW term is defined as falling below the 2000 gram/70.55 oz cutoff for low birthweight at 40 weeks gestational age or more. To compute gestational age, I assume that a 40 week pregnancy would end at the expected delivery date listed in the pregnant participant's certification file. I take the date of birth of the infant, subtract the expected delivery date, divide by seven, round down to the next integer, and finally add 40. This means that a baby born between 1 and 6 days prior to the expected delivery date has a gestational age of 39 weeks, a baby born between

the expected delivery date and 6 days after the expected delivery date has a gestational age of 40 weeks, etc. I take gestational age and construct an indicator that the infant was born at 40 weeks or more and that the infant is low birthweight. This indicates a LBW term birth.

I compute several other indicators of low birthweight that are common in the literature to assess the degree of the effect of vendor disqualifications on participant birthweights. The first indicator is low birth weight (LBW), an indicator that a baby is born weighing less than 2,500 grams or 5 pounds 8 ounces. The second indicator is very low birth weight (VLBW), meaning that the infant weighs less than 1,500 grams or 3 pounds 4 ounces at birth. These indicators are straightforward to compute from the birthweight data, and are correlated with later life outcomes.

I will treat exposure to the DQ in trimesters one, two, and three as separate treatments, recognizing that maternal nutrition affects fetal growth less in later stages of pregnancy (Neufeld et al. 2004; Wu et al. 2004). For this reason, vendor disqualification is more likely to affect birthweight during the first trimester, especially compared to the third trimester when fetal growth is less sensitive to maternal nutrition. To calculate trimesters, I take the expected delivery date and subtract 280 days to find the estimated date of conception. This is the start of week 1 of the pregnancy. Trimester 1 spans from the estimated date of conception to the end of the 12th week of gestation. Trimester 2 covers the 13th week of gestation to the end of the 26th week of gestation. The third trimester is from the 27th week to the end of the pregnancy. Conditioning on gestational age holds constant the duration of pregnancy, removing the potential confounder that participants that enroll in the first trimester may be systematically different from participants that enroll later, and for this reason may have different birthweights even in the absence of WIC. I hold gestational age constant to avoid this confounding.

3.3 Methods

The random assignment of DQ timing to participants makes this an ideal setting to look at the effect of shocks to maternal access to a safety net program on birthweight. However, I face substantial attrition in the treated sample, as I do not observe the birthweight of infants that are not enrolled in WIC. To account for this selection, I will implement the Lee (2009) method to bound the average causal effect of WIC vendor disqualification on birthweight.

The procedure relies on two assumptions - complete independence of the treatment from the potential outcomes in terms of birthweight and selection into observing birthweight, and the monotonicity assumption that treatment only increases or decreases monotonic effect of treatment on selection into the sample - to construct bounds that contain a local average treatment effect (LATE). The LATE is the average treatment effect for the set of participants that will always appear in the birthweight data. I will refer to these participants as the always responders, because the infant is always enrolled in WIC after birth so that the birthweight is observed. Assuming that the treatment is random implies the complete independence assumption. My results depend on assuming that 1) among participants that are ever exposed to a DQ, timing of the disqualification is random and 2) being exposed to the disqualification *in utero* can only decrease or increase the probability of observing a birthweight for the infant. As discussed above, randomness of treatment timing among treated participants is reasonable in this context. I find it plausible to assume that all pregnant participants become less likely to enroll their child in WIC after birth when they are exposed to the disqualification during the pregnancy instead of after the pregnancy. While the magnitude of the deterrence effect on enrolling the infant will vary across participants, including across breastfeeding and non-breastfeeding families, I assume the effect is at least slightly negative for all participants.

In my setting, I will estimate Lee (2009) bounds separately for exposure to the DQ dur-

ing either the first, second, or third trimester, and for groups defined by the calendar month in which the participant is exposed to the DQ. In each case I compare to the set of children who are exposed to DQs after birth. I use the package `leebounds` by Semenova (2020), available on GitHub, to compute the bounds. The intuition behind the Lee (2009) bounds is as follows. Random assignment of the treatment makes comparing means across the treatment and comparison groups a valid estimator of the average treatment effect in the case of no selection. The worst-case scenario for bias in the presence of selection is that all of the excess observations - observations for which the outcome is observed in one group but not the other - are the largest or smallest in terms of observed outcome magnitudes. The estimator will treat these worst case scenarios as the upper and lower bounds for the local average treatment effect of the always responders.

I describe the estimation procedure for the bounds for each of subsamples defined by trimester of exposure and DQ month. The estimation procedure occurs in two steps, with the first step for trimming the largest excess observations, estimating the lower bound, and the second step for trimming the smallest excess observations, estimating the upper bound. First, sort the treatment and comparison groups by the magnitude of the observed birthweights. Next, determine whether the treated or comparison group has a larger share of observations with non-missing birthweight. In the group with a larger share of observed outcomes, remove the smallest (largest) observations in order until the proportion of missing observations across the two groups is equal. The upper bound is estimated as the difference in means between the treated group and the comparison group after trimming to remove the smallest observations from the group with less attrition. The lower bound is the difference in means after trimming to remove the largest observations from the group with less attrition.

This method results in upper and lower bounds for the average treatment effect by groups of treatment trimester and vendor DQ month. To get a sense of averages for larger groups,

for instance across all vendor DQ months for the same treatment trimester, I make a crude approximation of these larger group bounds by averaging the upper and lower bounds for the subgroups, weighting by the number of treated units with observed birthweight in each subgroup.

3.4 Results

In this section I describe the results of the Lee bounds procedure described above. Figures 3.1 to 3.4 show the Lee bounds for the four different outcomes: birthweight measured in ounces, a binary measure of low birthweight, a binary measure of very low birthweight, and a binary measure of low birthweight at term. All of these figures show the bounds for each group of participants defined by the month in which they are exposed to the DQ, and for the treated participants which trimester they were exposed to the DQ in. The results in red are for the effect of a DQ on birthweight in the first trimester, in green the effect of a DQ on birthweight for the second trimester, and blue the effect of a DQ on birthweight for the third trimester. Each of the colored bars shows bounds for one event/trimester group. The grey bar outlined in the respective colors shows the average upper and lower bounds across DQ timing for each of the trimester groups. For all trimester panels, the dashed black lines show the overall average upper and lower bounds on the effect of a DQ on the birthweight outcomes across event timings and across trimester.

The results for birthweight in ounces in Figure 3.1 show that there is no significant effect of the vendor DQ on birthweight across trimesters of exposure. In all three trimesters and on average, the bounds include zero. The bounds are wider in later trimesters, but this may be due to larger numbers of subgroups creating wider bounds. These wider bounds in later trimesters hold across all of the measured outcomes.

The results for low birthweight, very low birthweight, and low birthweight at term show at best only marginally significant results. When rounding to two decimal points, all in-

tervals include zero, so that magnitudes are small even when bounds do not include zero. In general the directions are as expected, showing an increase in low birthweight and low birthweight at term in the first trimester. Other effects are too small or include zero. There is a weak increase in low birthweight outcomes for infants born to WIC participants that were exposed to a vendor DQ in the first trimester. It is possible that splitting the samples into DQ month/trimester groups has reduced estimation power and that examining larger groups would improve precision and representativeness. Finally, this approach does not include covariates beyond trimester and DQ month. Taking an approach like that in Semenova (2021) or Tauchmann (2014) may help inform the results by including covariates in addition to weakening the monotonicity assumption.

3.5 Conclusion

3.5.1 Summary

In this paper, I linked vendor sanction, redemption, and participant administrative data to examine how birthweight - an important health outcome for children - responds to the removal of an authorized WIC vendor during pregnancy. I applied the bounds method from Lee (2009) to resolve the problem of differential attrition across treated and control groups. The results generally show minimal effect of the DQ on birthweight. There is some indication of negative effects of the vendor DQ on birthweight when exposed in the first trimester, particularly after controlling for gestational age. Overall, these results suggest a minimal pathway from vendor disqualification to birth outcomes, although average effects may be covering up larger impacts on particularly vulnerable subgroups. Estimation that takes into account important covariates is worth further investigation.

3.5.2 Limitations

A substantial challenge with this project is the very high attrition and low rates of observed birthweights. With better data, it might be possible to determine the presence or absence of a significant effect of DQs on birthweight. Linked MediCal data is not available for

this project. As mentioned in the methods section above, an approach that incorporates covariates to somewhat weaken the monotonicity assumption and to reveal how effects vary across subgroups besides trimester. The current monotonicity assumption is quite strong. Implementing the method in Semenova (2021) would weaken the monotonicity assumption.

3.5.3 Policy implications

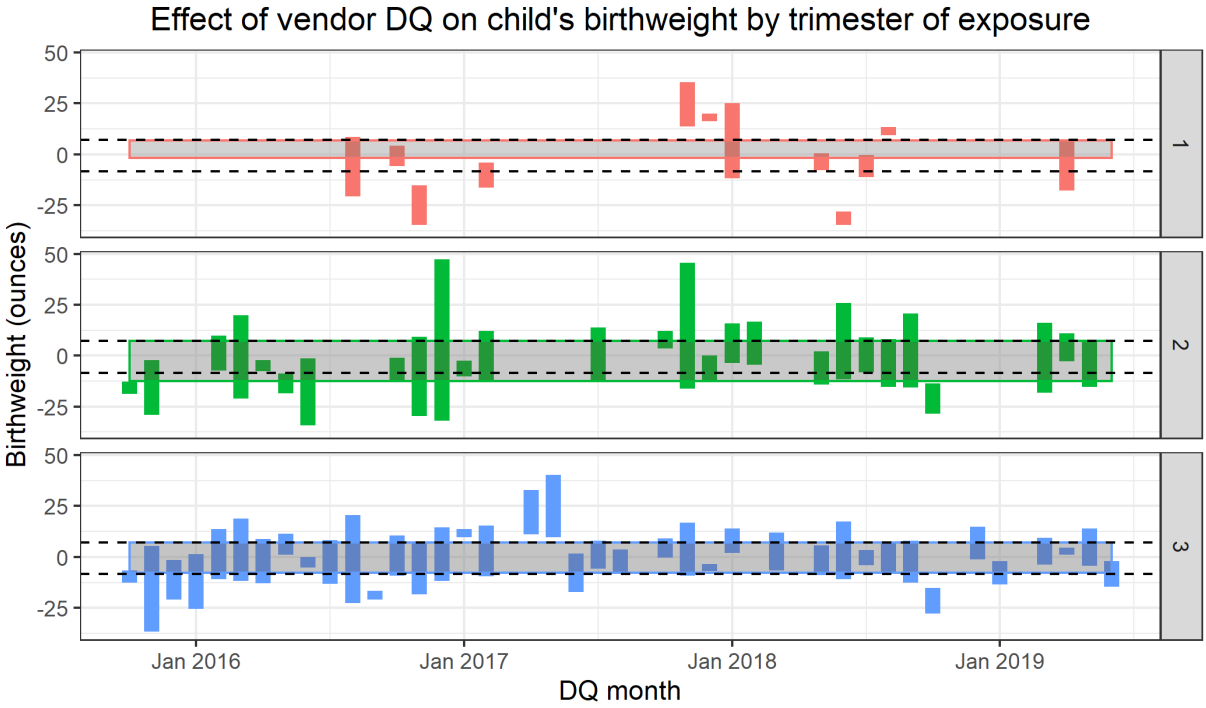
The minimal evidence found in this paper for effects of DQs on birthweight outcomes indicates that birth outcomes may not be a pathway by which DQs affect WIC participants. This is helpful to know in terms of bounding the costs of DQs. Substantial benefits accrue to participants and the state as the result of DQs. It is challenging to place a value on the benefit of stopping an activity that is illegal as well as harming participants and the WIC program. However, understanding the potential costs and unintended consequences that result from DQs allow practitioners that understand these benefits to weigh the two. Limiting the magnitude of health impacts from DQs on WIC participants and their children reduces the costs and increases the probability that the benefits of DQs outweigh the costs. The effects of DQs on participants may be limited to participation effects, bounding the cost of disqualifications.

3.6 Tables and Figures

Table 3.1: Summary statistics by trimester of exposure to DQ

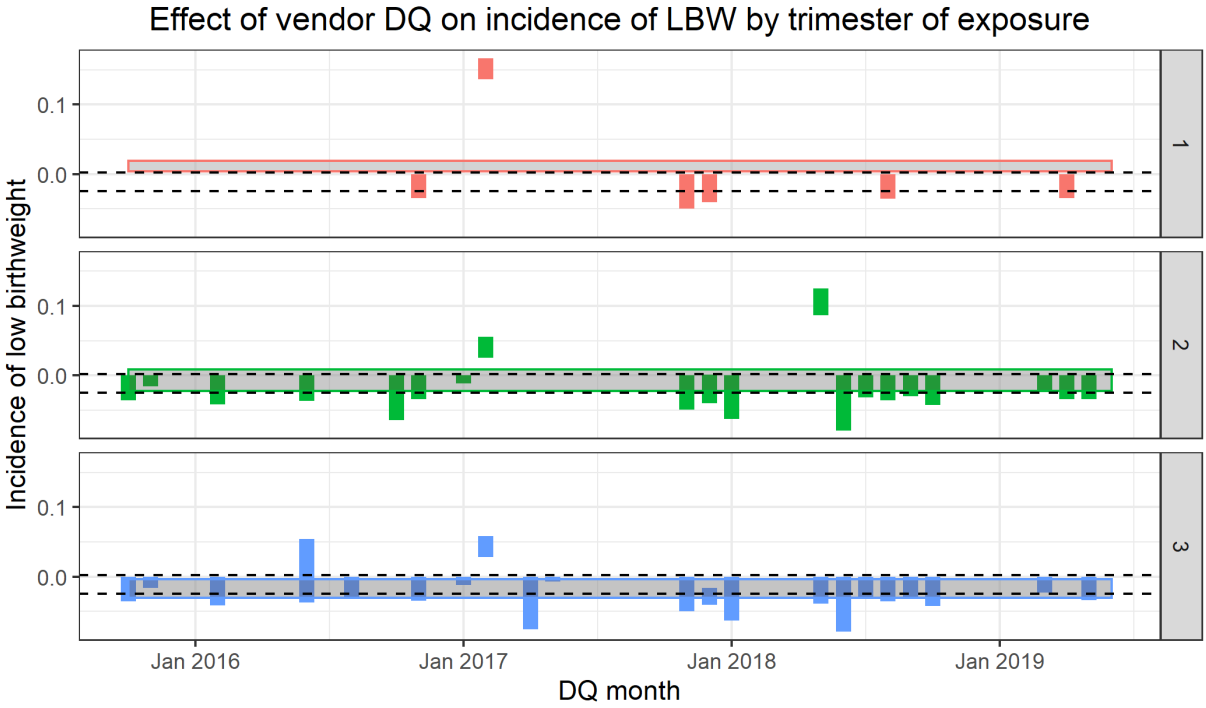
	Trimester of DQ			No DQ
	1	2	3	in utero
N	7,550	45,219	80,159	6,992,757
English	0.58	0.60	0.60	0.58
Spanish	0.41	0.37	0.37	0.38
High grade	10.59	10.72	10.74	10.66
MediCal	0.90	0.92	0.91	0.90
SNAP	0.31	0.28	0.29	0.38
Income	1717.58	1641.36	1629.75	1651.52
Family size	4.25	4.20	4.14	4.34
Hispanic	0.85	0.82	0.81	0.83
Black	0.06	0.07	0.08	0.06
Birthweight (oz)	130.46	113.22	115.95	117.75
LBW	0.02	0.02	0.01	0.04
VLBW	0.00	0.01	0.00	0.00
SGA	0.03	0.16	0.14	0.11
LBW term	0.01	0.01	0.01	0.03

Figure 3.1: Effect of vendor DQ on birthweight



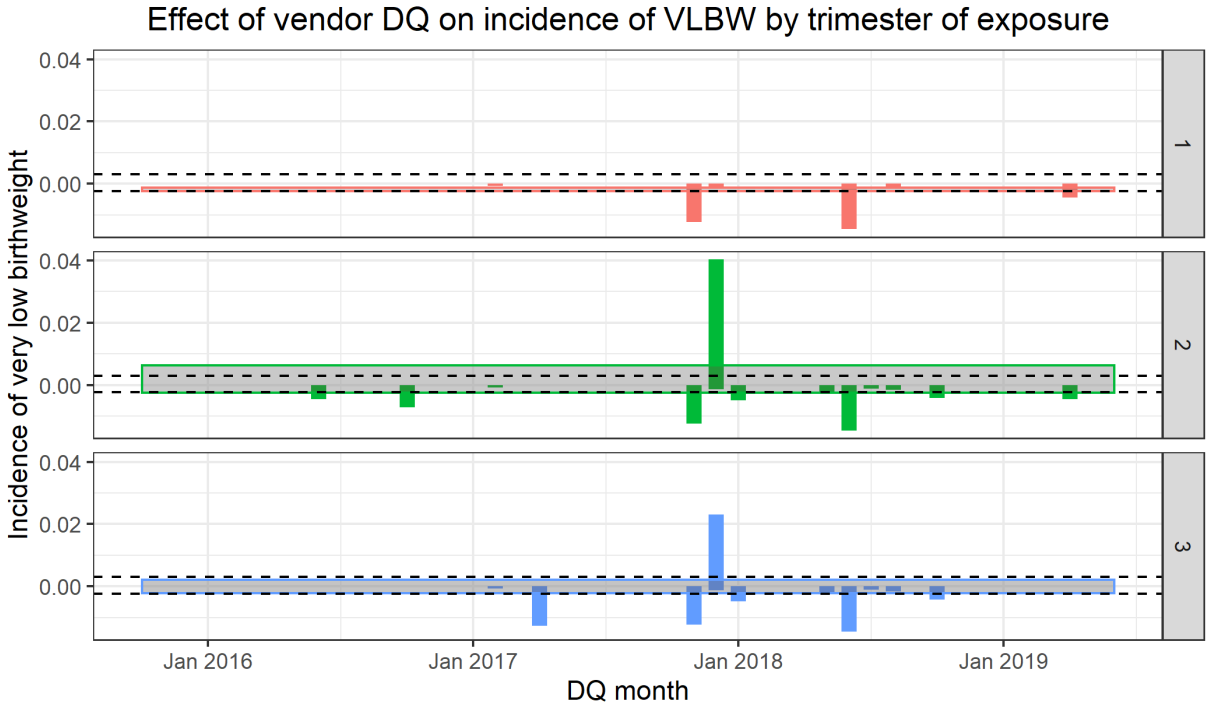
Plots show Lee (2009) bounds estimated on subsample composed only of individuals that are ever exposed to a DQ. Treated units are those that are exposed to a DQ in utero in the indicated trimester. Comparison units are those that are exposed to a DQ after birth. All participants in a subsample experience a DQ in the same month. Average lower and upper bounds by trimester, weighted by the number of treated participants for which an outcome is observed, are shown in the shaded area. Overall weighted average lower and upper bounds are shown as dashed lines in black.

Figure 3.2: Effect of vendor DQ on incidence of low birthweight



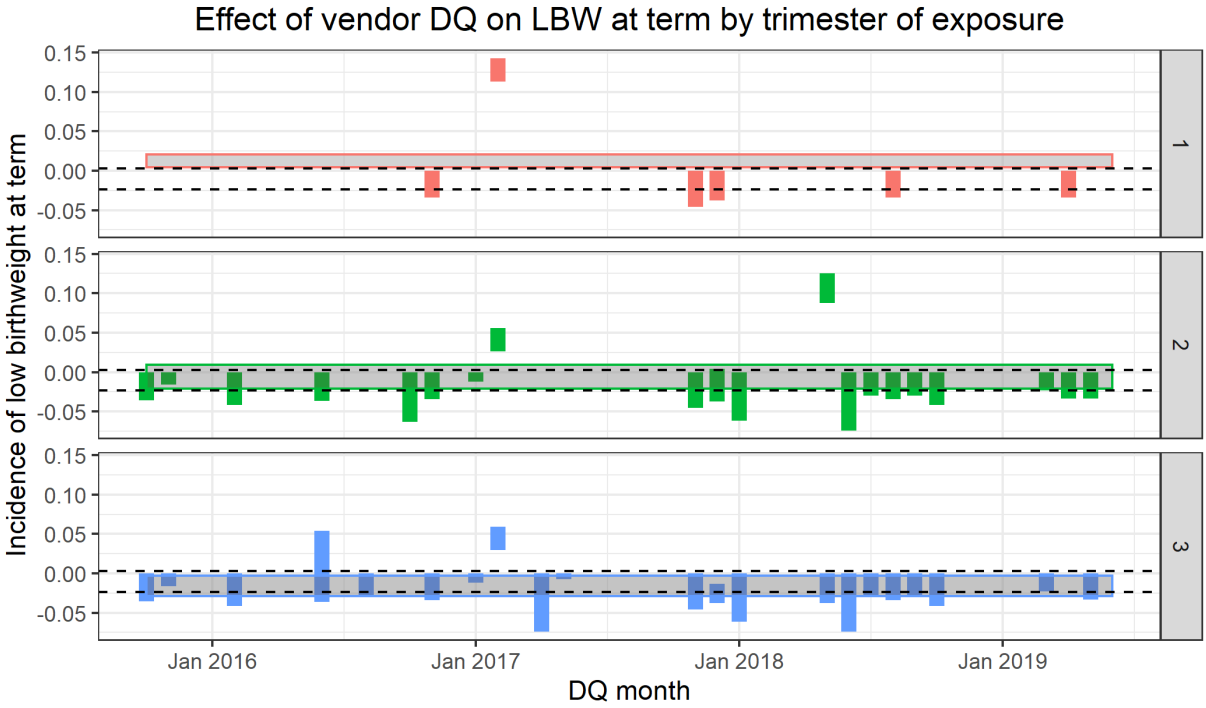
Plots show Lee (2009) bounds estimated on subsample composed only of individuals that are ever exposed to a DQ. Treated units are those that are exposed to a DQ in utero in the indicated trimester. Comparison units are those that are exposed to a DQ after birth. All participants in a subsample experience a DQ in the same month. Average lower and upper bounds by trimester, weighted by the number of treated participants for which an outcome is observed, are shown in the shaded area. Overall weighted average lower and upper bounds are shown as dashed lines in black.

Figure 3.3: Effect of vendor DQ on incidence of very low birthweight



Plots show Lee (2009) bounds estimated on subsample composed only of individuals that are ever exposed to a DQ. Treated units are those that are exposed to a DQ in utero in the indicated trimester. Comparison units are those that are exposed to a DQ after birth. All participants in a subsample experience a DQ in the same month. Average lower and upper bounds by trimester, weighted by the number of treated participants for which an outcome is observed, are shown in the shaded area. Overall weighted average lower and upper bounds are shown as dashed lines in black.

Figure 3.4: Effect of vendor DQ on incidence of low birthweight at term



Plots show Lee (2009) bounds estimated on subsample composed only of individuals that are ever exposed to a DQ. Treated units are those that are exposed to a DQ in utero in the indicated trimester. Comparison units are those that are exposed to a DQ after birth. All participants in a subsample experience a DQ in the same month. Average lower and upper bounds by trimester, weighted by the number of treated participants for which an outcome is observed, are shown in the shaded area. Overall weighted average lower and upper bounds are shown as dashed lines in black.

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