The Role of Intermediaries in Administering In-kind Transfers: Evidence from SNAP and Food Retailers*

Seojung Oh †

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Abstract

As the U.S. government partners with private firms to deliver in-kind benefits to vulnerable populations, these firms serve as important intermediaries that can substantially affect the effectiveness of in-kind transfer programs. Despite their importance, little attention has been paid to how firms respond to these programs and their impact on benefit recipients. This study investigates grocery retailers' response to changes in the Supplemental Nutrition Assistance Program (SNAP) due to the adoption of Electronic Benefit Transfer (EBT) technology and subsequent implications for program efficiency and equity. Leveraging the staggered adoption of EBT across counties, I find that moving to EBT led to a 9.6% decrease in the number of SNAP-authorized retailers. This reduction is driven by declines in the number of small SNAP stores, with evidence suggesting that small stores' exits are due to the initial costs associated with setting up EBT. Subsequent analyses suggest that while EBT improved program efficiency by reducing administrative costs and increasing individuals' SNAP participation, these efficiency gains were largely concentrated in areas with relatively good access to large SNAP retailers.

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1 Introduction

U.S. safety net programs rely heavily on in-kind transfers that provide subsidies for specific goods or services such as food consumption, nutritional assistance, housing, medical services, and early childhood education (Moffitt, 2015). Since the U.S. government typically makes contracts with private firms to effectively deliver these in-kind transfers, these firms serve as important intermediaries that facilitate the provision of public assistance to vulnerable populations. Therefore, the design or structure of in-kind transfer programs that limit or disincentivize firms' participation could decrease the program's effectiveness by restricting recipients' access to providers. To date, however, firms' participation decisions in in-kind transfers and their impact on programs' effectiveness remain largely unexplored.

This paper fills this important gap in the literature, focusing on the Supplemental Nutrition Assistance Program (SNAP). SNAP is the nation's second-largest in-kind transfer program in terms of program expenditure.[1] As SNAP benefits are redeemable only at SNAP-authorized retailers, the number and types of participating grocery stores substantially affect both the accessibility and affordability of SNAP-eligible food items. For instance, if program parameters encourage SNAP participation among large supermarkets but discourage participation among small convenience stores, program efficiency might be improved, as supermarkets typically offer a wider variety of fresh produce at lower prices, thereby increasing SNAP benefits' real value. However, this strategy could make the program less accessible, particularly to people residing in high-poverty neighborhoods, as supermarkets tend to concentrate in low-poverty regions (Bitler and Haider, 2011).

In this study, I focus on the change in SNAP's benefit disbursement system—the Electronic Benefit Transfer (EBT) system that replaced paper "food stamp" vouchers with plastic cards. The EBT reform provides a unique opportunity to investigate how changes in SNAP's design could affect the participation decisions of food retailers and how these effects alter the balance between

[1]The largest in-kind program is Medicaid. In 2023, SNAP provided near-cash food vouchers to 42 million low-income individuals at the cost of $113 billion.

program efficiency and equity for recipients. While this new benefit disbursement system reduced ongoing costs of participating in SNAP by lowering the time and monetary costs associated with handling paper coupons, EBT also introduced new challenges related to learning and adopting the new technology. Considering that the reform started in the late 1980s when electronic payment systems were not prevalent (Food and Service, 1995; Gerdes and Walton, 2005), the financial and mental costs associated with the EBT adoption may have been burdensome for food retailers, particularly smaller stores that were less likely to be familiar with the system.

Figure 1 motivates the analysis. It shows a substantial decline in the number of SNAP-authorized food stores during the EBT rollout period, with the number dropping by more than 25% from the peak of 200,000 stores in 1993 to less than 150,000 stores in 2004. The decrease is mainly driven by a reduction in the number of small SNAP retailers, subsequently altering the composition of retailers authorized to accept SNAP benefits. The disparity suggests that small and large retailers may have been impacted differently by the EBT reform. Additionally, this decline in small SNAP stores may have simultaneously affected the program's efficiency and equity. For instance, efficiency gains could occur if SNAP recipients were encouraged to shop at higher-quality stores after the reform or if EBT increased SNAP takeup without raising administrative costs, there could be efficiency gains; however, if recipients living in areas without supermarkets faced reduced access to small SNAP retailers, the benefits from the EBT reform may not have extended to certain regions.

To identify the causal relationship between EBT implementation and SNAP participation among small and large retailers, I use a difference-in-differences (DD) research design, leveraging the staggered rollout of the EBT system across counties. The EBT system was implemented gradually across counties, providing multiple treatment timings. Since the conventional two-way fixed effects (TWFE) method could provide a biased estimate under this "staggered adoption" setting and heterogeneous treatment effects, I employ the heterogeneity-robust estimators suggested by Sun and Abraham (2021) and Callaway and Sant'Anna (2021).[2] My baseline specification in this study

[2]This is because the TWFE estimator includes "forbidden comparisons" between the treated and already-treated units, making the already-treated units a bad comparison group Goodman-Bacon (2021); De Chaisemartin and d'Haultfoeuille (2020); Sun and Abraham (2021). A group of recent econometrics papers demonstrates that a standard TWFE model under this staggered adoption setting does not identify a weighted average of unit-level average treatment

uses last-to-be-treated counties as counterfactuals. To check the robustness of my findings, I also use not-yet-treated counties as alternative counterfactuals.

The key identification assumption is the parallel trend assumption, which requires that, had it not been for the EBT reform, the trend of SNAP retailers would have similarly evolved over time between treated and control counties. I establish the quasi-randomness of the variation by showing that pre-EBT county characteristics and each county's EBT adoption timing exhibit little or weak association with small magnitudes, particularly with time-varying local economic factors and the 1996 welfare reform.[3] The pre-trend analysis using the events study framework also shows little evidence of the existence of pre-trends. These results are consistent with the institutional background, wherein the EBT rollout schedule was often delayed and interrupted by unforeseen circumstances.

I utilize multiple datasets. The first dataset contains information on county-level EBT adoption timing. I compiled extensive information on EBT adoption timing at the county-month level from various sources, which improves coverage and accuracy compared to previous data that contain only state and yearly-level information. The second dataset is the Historical SNAP Retailer Locator Data from the Food and Nutrition Service (FNS). This administrative data contains rich information about SNAP retailers, including store names, types, detailed addresses, and the start and end dates of SNAP authorization. Using this information, I separately look at the effect of EBT on small SNAP stores (including convenience stores, liquor stores, and small grocery stores) and large SNAP stores (including supermarkets, supercenters, and large grocery stores), since I hypothesize

effects unless a strong assumption holds that treatment effects are homogeneous across group and time. This is because the TWFE estimator under a staggered treatment comprises effects from not only the "good comparison" between the treated and not-yet-treated units but also "forbidden comparisons" between the treated and already-treated units. The existence of forbidden comparisons may cause a bias even in the sign of the estimator due to the possibility of a negative weighting problem. Even with a homogeneous treatment effect, the TWFE parameter may not reflect the most policy-relevant summary of treatment effects, as the weights imposed by the TWFE regression are somewhat arbitrary Goodman-Bacon (2021); De Chaisemartin and d'Haultfoeuille (2020); Sun and Abraham (2021). Roth et al. (2023) provides a great review of the papers.

[3] I focus on the local economic factors based on the finding of Ganong and Liebman (2018), that more than half of the decline in SNAP caseloads during 1992 and 2007 can be attributed to the booming economic conditions while the rest is explained by the welfare reform. As local unemployment rates decreased, fewer people qualified for the SNAP program during the time period. Additionally, the welfare reform reduced SNAP eligibility for specific groups, such as able-bodied adults without dependents (ABAWDs) and legal immigrants.

that those two types of stores would be differentially affected by the adoption of the new EBT technology. In addition, to investigate the question regarding SNAP accessibility for recipients, I utilize the administrative data on bi-annual SNAP caseloads and issuance data and the quarterly SNAP redemption amounts.

My estimates suggest a 9.6% reduction in the total number of SNAP retailers after the EBT adoption compared to the baseline mean. This reduction is primarily driven by small SNAP stores, which show a 13.1% decline in their number. Event study graphs show that the decline occurred gradually over time and that the participation of small SNAP stores had not recovered even four years after initial EBT adoption. In contrast, the impact on the number of large SNAP retailers is close to zero and statistically insignificant. When examining the share of stores participating in SNAP, I find similar patterns, with small food stores experiencing a decline while the share of large food retailers remained relatively stable. Combined, these changes led to a shift in the composition of SNAP-participating stores. Using the estimated coefficients, I calculate that the EBT reform can explain around 30% of the reduction in the number of authorized SNAP stores during the study period.

I explore two mechanisms that might have caused this differential decline in EBT adoption. First, I investigate the role of the enhanced fraud prevention effort following EBT implementation and the subsequent increase in disqualifications of fraudulent stores. The EBT system enables FNS to collect extensive SNAP transaction records, which a highly trained team of data analysts uses to detect any signs of benefit misuse, such as the exchange of SNAP benefits with cash or ineligible items. Once the misconduct is discovered, the store could be temporarily or permanently disqualified from the program. While I find a 66% increase in the number of permanent disqualifications among small SNAP vendors after the EBT, the absolute number of permanent disqualifications is so small that it explains at most 2% of the reduction in small SNAP stores. Next, I examine the potential impact of the initial EBT setup costs. Given that electronic payment technology was not widespread during the EBT rollout, adopting EBT might have imposed mental and financial burdens on many retailers, especially smaller ones with less experience with such systems. Consistent with this hypothesis, I find that the reduction of small retailers is indeed more pronounced in counties that adopted EBT earlier. Additionally, I find that in California, where the upfront cost was very low as the state implemented EBT during 2002-2004 and offered free EBT devices, the estimated impact of EBT on the number of small SNAP retailers is even positive, although not significant.

Finally, I explore the impacts of the EBT adoption on individuals' SNAP participation, particularly whether the impacts are heterogeneous between regions where people have relatively good access to large retailers and regions with few supermarkets. I find that after EBT, individuals' SNAP participation rate increased by 0.8%p, which is a 10.1% increase compared to the baseline mean. The amount of benefit issued also increased on average by $66,000 per county, or 17.8% compared to the baseline mean. The heterogeneity analyses show that while all regions experience an increase in the SNAP participation rate and the amount of benefit issued after EBT, the positive impacts are concentrated in counties with relatively better access to large food retailers and lower poverty rates. Given prior findings that large grocery retailers, such as chain supermarkets, are less likely to locate in areas with higher poverty rates, the evidence suggests that benefits from EBT—such as reduced benefit pick cost and reduced stigma, leading to increased benefit take-up—may be disproportionately enjoyed by individuals who are less in need among SNAP beneficiaries. Additionally, in counties where multiple supermarkets are available, I find suggestive evidence that after the EBT reform, SNAP recipients shifted their benefit redemption from smaller stores, such as convenience stores, to larger supermarkets. Given that supermarkets generally offer a wider variety of fresh grocery items at lower prices (Caspi et al., 2017), this shift in shopping patterns could suggest an increase in the real value of SNAP benefits. These findings highlight the importance of considering firm-side responses when designing or evaluating the changes in program structure in in-kind transfer programs.

This paper contributes to several literatures. First, it closely relates to a small but emerging body of work that investigates grocery retailers' responses to nutritional assistance programs. Prior studies demonstrate that food retailers optimize their pricing or program participation decision in response to SNAP Goldin et al. (2022); Leung and Seo (2023); Byrne et al. (2022) or the Special

Supplemental Nutrition Program for Women, Infants, and Children (WIC) program (Meckel, 2020). For instance, Byrne et al. (2022) shows that the expansion in SNAP's program size during the Great Recession led to increased SNAP participation among non-traditional food retailers (e.g., dollar stores and drug stores), enhancing SNAP store access for eligible households without causing major impacts on dietary healthfulness or price levels. In a broader context, Bitler et al. (2019) shows that the introduction of SNAP had substantial impacts on the food retail sector by increasing store numbers, sales, and employment.[4] My paper provides some of the first evidence that food retailers adjust their SNAP participation decision in response to SNAP dynamics.

Second, my paper connects to research evaluating the introduction of EBT technology in the SNAP program. Prior literature focuses on SNAP beneficiaries' responses to EBT adoption. Studies document the null or positive effect of EBT on SNAP caseloads (Figlio et al., 2000; McKernan et al., 2003; Bednar, 2011; Currie et al., 2001; Danielson et al., 2006; Melvin and Smith, 2022; Zhou et al., 2024),[5] improvement in within-month consumption smoothing after EBT (Kuhn, 2021), increase in the marginal propensity to consume SNAP-eligible food (MPCf) (Eck, 2018), and mixed findings on crime (Wright et al., 2017; Lovett, 2018). I contribute to the literature by providing the first evidence that the SNAP EBT reform induced supply-side responses, highlighting the importance of considering firms' participation when designing and assessing in-kind programs. I also provide strong evidence that EBT positively affected the individuals' SNAP participation rate using data that cover the entire U.S. with better accuracy. Additionally, My study provides policy implications for current and future initiatives aiming to enhance program effectiveness through EBT technology in other areas, such as encouraging EBT transactions in farmers' markets, emphasizing that lowering adoption costs is important for encouraging small businesses' SNAP participation.

Lastly, this study contributes to a small literature focusing on potential efficiency-equity tradeoffs

[4]Similar to the findings of Bitler et al. (2019), EBT-induced changes in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) program have positive impacts on sales volumes and employees working at WIC-authorized stores (Ambrozek et al., 2019).

[5]The first three papers report the null effect of EBT on SNAP caseloads, while the others estimate positive effects. These mixed results may stem from data constraints in earlier research, which rely on state-level variation although EBT adoption timing varied at the county level. Indeed, a recent study by Melvin and Smith (2022) using count-level data reports a positive impact of EBT on SNAP participation rates.

in in-kind programs. Meckel (2020) and Meckel et al. (2023) explore this issue in the context of WIC, a nutritional assistance program that provides quantity vouchers and a small cash-value-voucher to pregnant women, infants, and young children. Since quantity vouchers make WIC customers' purchasing decisions perfectly inelastic with respect to the prices of WIC food items, stores have an incentive to charge higher prices to WIC customers than to regular customers. Meckel (2020) finds that after the WIC EBT reform in Texas, which made it impossible to price discriminate, many WIC vendors stopped participating in the program, resulting in a decrease in WIC take-up. Similarly, Meckel et al. (2023) report that California's WIC cost-containment policy, which substantially reduced the number of small WIC vendors, negatively affected WIC take-up among foreign-born first-time mothers. My study contributes to this literature by providing the first evidence of a similar efficiency-equity tradeoff in the context of the SNAP program.

My research also makes a data contribution. I compile novel and extensive data on monthly EBT adoption timing at the county level, covering 98% of counties (excluding counties in Alaska, Hawaii, and Wyoming). I acquired information on planned rollout timing from government reports, archived state and county government websites, and the EBT Project Status Reports submitted to Congress. I obtained additional information from other sources, including information from EBT managers, other relevant reports, articles, and prior studies. In addition, to enhance accuracy and comprehensiveness of the data, I utilize the bi-annual (January and July) county-level caseloads and issuance data from FNS, which provides information on actual EBT implementation for a subgroup of counties. Considering that most prior studies had to use state-level data due to the data constraint, my novel dataset offers new opportunities for further research on the SNAP EBT reform.

The rest of the paper is constructed as follows. Section 2 describes the institutional background of SNAP and EBT adoption. Section 3 provides an explanation to illustrate the expected impacts of EBT on food retailers. Section 4 lists the sources of datasets used for this study, and Section 5 explains the empirical specification. Section 6 presents the main results, followed by Section 7 and Section 8, which unpack potential mechanisms and discuss the subsequent impact on program equity. Finally, Section 9 concludes.

2 Background

2.1 Overview of Supplemental Nutrition Assistance Program (SNAP)

SNAP[6] takes a pivotal role in the U.S. safety net. It is the largest nutritional assistance program and the second-largest in-kind transfer program, providing monthly food benefits to more than 42 million low-income individuals (12.5% of the U.S. population) at a cost of $113 billion in the fiscal year 2023. The primary goal of the program is to alleviate food insecurity and improve nutrition by supplementing low-income households' grocery budgets. Research shows that SNAP effectively achieves this objective. Individuals who have access to the program have lower levels of food insecurity (Schmidt et al., 2016) and spend more on food (Hoynes and Schanzenbach, 2015). SNAP is also one of the most effective programs at reducing child poverty (Hoynes and Schanzenbach, 2020). As such, ample literature documents SNAP is a long-term investment that improves later-life education, health, and labor market outcomes of exposed infants and children (Hoynes et al., 2016; Bitler and Figinski, 2019; Bailey et al., 2024). According to Bailey et al. (2024), SNAP has a large "bang for the buck" because it generates in the long run $62 benefits for each $1 of net government spending.

2.2 SNAP and Food Retailers

Like other in-kind transfers, SNAP operates as a partnership between the government and private firms in the market. During 1998 and 2004, 45-58% of food retailers participated in SNAP.[7] This partnership is designed to ensure that SNAP households have flexible store choices and access to a variety of food items. In addition, it is believed that a competitive market environment incentivizes

[6]The 2008 Farm Bill changed the name of the program from the "Food Stamp Program" to the "Supplemental Nutrition Assistance Program" to fight stigma attached to the program.

[7]This estimate is based on the author's calculation using retailer information from the Historical Retailer Locator data and County Business Pattern data.

SNAP retailers to maintain low prices for SNAP-eligible items (Oliveira et al., 2018).

Many types of retailers participate in SNAP, including tiny corner stores and huge superstores. While large SNAP retailers comprise only 20% of participating stores, they redeem roughly 75-85% of benefits (Ollinger et al., 2021). Food access surveys conducted in 1996 and 2012 indicate that in those years, approximately 90% of respondents used supermarkets or supercenters as their primary redemption stores, and made use of smaller stores as supplementary stores (Ohls et al., 1999; Ver Ploeg et al., 2015). This suggests that large good retailers' SNAP participation is crucial for SNAP beneficiaries to make use of their benefits, while small SNAP stores play a less pivotal role. However, small food stores' SNAP participation may still be important in neighborhoods with limited access to large grocery stores.

FNS, a federal agency under USDA, oversees the authorization process and transaction activities of grocery retailers that participate in SNAP. To obtain SNAP authorization, store owners must submit applications and meet one of two requirements. First, a store must consistently stock three varieties of staple food items in each of the four categories: (a) fruits or vegetables, (b) dairy products, (c) meat, poultry, or fish, and (d) bread or cereals. Second, a store must generate more than 50% of its revenue from staple food sales. Once a store's SNAP application is approved, it is allowed to sell SNAP-eligible food items to SNAP customers.

FNS also takes responsibility for guiding and monitoring authorized retailers to ensure their understanding and compliance with program rules. If a SNAP store breaks program rules, then FNS can sanction the store.[8] Depending on the severity of violations, stores may receive various penalties, ranging from official warnings to temporary or permanent disqualifications from the SNAP program, sometimes together with civil money penalties. In 1993, at most $815 or 3.8% of issued benefits were misused (Macaluso, 1995). While larger retailers and chain stores rarely violate regulations, smaller and independently owned stores have significantly higher rates of noncompliance (Wilson, 2021).[9]

[8]The most common misconduct is exchanging SNAP benefits with cash. Selling any SNAP-ineligible item (e.g., alcoholic beverages) to SNAP customers is another type of fraudulent behavior.

[9]In 1995, FNS officially estimated the amount of trafficked benefits for the first time. Estimates were calculated based on 11,000 undercover investigations between March 1991 and March 1994 (Macaluso, 1995). Those investigations

2.3 The Electronic Benefit Transfer (EBT) Reform

Before 1990, SNAP benefits were disbursed by a booklet of paper coupons (or food "stamps")[10]. Monthly benefits were distributed to recipients either through in-person visits to nearby welfare offices or via mails. The paper coupon system imposed significant burdens on the government, stores, and beneficiaries. For the government, the cost of printing, distributing, redeeming, and destroying massive quantities of paper coupons each month were substantial. In addition, tracking benefit diversion was extremely difficult and expensive. SNAP retailers faced the challenges of collecting and classifying SNAP coupons, finding a bank that offered deposit services, and making frequent trips to deposit coupons. Recipients faced stigma associated with using paper coupons, as coupons are easily identifiable at a checkout lane. They were also at risk of losing or having their food vouchers stolen.

Recognizing these problems, efforts to replace the paper coupon system with the EBT system began in the mid-1980s. The EBT system delivers government benefits electronically to beneficiaries through a magnetic stripe card that looks and works like a plastic debit card.[11] Once a household's SNAP application is approved, a primary benefit recipient receives an EBT card with a four-digit personal identification number (PIN). SNAP benefits are loaded to recipients' EBT cards every month, which can be used at any authorized SNAP vendors. The charge is then deducted from the person's SNAP benefit account and immediately credited to the authorized vendor's account.

The first EBT pilot program took place in 1984 in Reading, Pennsylvania. Subsequently, the Hunger Prevention Act of 1988 established several other pilot programs, which demonstrated the feasibility and effectiveness of transitioning to the EBT system.[12] The success of the pilot programs

were not randomly conducted; the main targets were stores that were perceived as most fraud-prone, such as small stores and independently owned stores. Therefore, their estimate on the store violation rate (share of investigated stores caught trafficking) is highly likely to be overstated. Even with this "upper bound" measure, the amount of SNAP benefit diversion was not large. In the fiscal year 1993, the estimated trafficking amount was $815 million, which is 3.8% of the total benefit issued and can be translated as less than 4 cents of every $1 SNAP benefit. Privately owned small stores and specialty stores show the highest store violation rates. Estimates for the subsequent years report similar patterns (Wilson, 2021).

[10]See Appendix Figure A.1 for an example of paper food stamp coupons.

[11]See Appendix Figure B.1 for an example of an EBT card.

[12]These early pilot programs took place in Baltimore, Maryland in 1989, Ramsey County, Minnesota in 1991, and Albuquerque, New Mexico in 1991.

fueled political support for its nationwide adoption. In 1996, Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) mandated a nationwide transition to EBT by October 1, 2002. As illustrated in Figure 2, the mandate significantly accelerated the adoption of EBT. Five states, including California, Delaware, Iowa, Maine, and West Virginia, received a waiver from FNS to extend the implementation deadline.[13] The nationwide rollout was completed in July 2004.

Although state governments developed their EBT rollout schedules, there was a considerable amount of randomness involved in the process. This was due to the difficulty in accurately predicting the timeline for completing all necessary steps, including selecting a third-party EBT processor through a bidding process, negotiating with them, and obtaining FNS' approval on the rollout plan.[14] Typically, in each state, a pilot program was implemented in one or two counties, followed by a gradual statewide rollout. I further explore the quasi-randomness of the rollout variation in Section 5.2.

State governments typically operated pilot programs in select counties to test and refine the EBT implementation process before expanding it in stages. Government officials and staff from third-party EBT processors notified retailers about the upcoming EBT rollout via mail and conducted several rounds of information sessions prior to the implementation. After following the planned schedule, they began installing EBT devices in each retailer and distributing plastic cards to new SNAP recipients during the application process. Ongoing recipients received their EBT cards when they visited the office for regular services. Some states, such as California, offered free EBT devices to retailers who opted for this option, but this provision was not universal. Prior to September 2014, roughly 50% retailers received free EBT devices (Wilson, 2021). There were also some restrictions; for example, in Vermont, the free device was only available for one year unless

[13]The delays were due to issues such as "high EBT costs in comparison to paper issuance costs, lack of sufficient staff resources, lack of technical expertise, and competing priorities" (Food and Service, 2003).

[14]Some states did the bidding individually, while others did it as an alliance of many states. There are three EBT alliances. (1) Southern Alliance States (SAS), including Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, Tennessee, West Virginia, (2) Western States EBT Alliance (WSEA), including Alaska, Arizona, Colorado, Hawaii, Idaho, Nevada, Nebraska, Washington, Wyoming, and (3) Northeast Coalition of States (NCS), including Connecticut, Maine, Massachusetts, New Hampshire, New York, Rhode Island, Vermont. States without any alliance and providing EBT individually are: California, Delaware, DC, Illinois, Indiana, Iowa, Kansas, Maryland, Michigan, Minnesota, Montana, New Jersey, New Mexico, North Carolina, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Texas, Utah, Virginia, Wisconsin, plus Washington D.C..

the retailer's average monthly sales from SNAP customers exceeded %100. At the time, not many stores had already adopted electronic payment systems. In 1994, only 8.6% of authorized retailers had a Point of Sale (POS) system, and these were mostly concentrated among larger retailers (Food and Service, 1995).

3 Impacts of EBT on Retailers' SNAP Participation

The introduction of EBT changed the incentives for food retailers to participate in SNAP. Table 1 provides an overview of the potential changes in both costs and revenue that may have resulted from EBT adoption.

Table 1: Changes in retailers' incentives in participating in SNAP

		Changes due to EBT
Cost	Ongoing costs	Coupon processing cost (-), staple food stocking cost (·)
	Upfront (fixed) costs	EBT setup cost (+), learning costs (+)
Revenue	Regular channel	New entry of SNAP customers (+)
	Trafficking revenue	Greater risk of being apprehended (-)

Note: This table illustrates the potential changes in retailers' cost and revenue associated with SNAP participation after the EBT reform. The symbols +, ·, and - imply an increase, no change, and decrease, respectively.

Changes in Costs

EBT reduced the ongoing costs of SNAP participation for retailers, particularly by eliminating time and labor expenses associated with managing paper coupons, including counting, bundling, and depositing them. Surveys from early EBT pilot programs in Maryland, Minnesota, and New Mexico show that retailers generally preferred the EBT system over the paper-based coupon system (Quiñones and Kinsey, 2000).

However, EBT also introduced new upfront fixed costs related to setting up the EBT system, learning the technology, and training employees. Although some states attempted to mitigate these adoption costs by offering free EBT machines or reimbursing installation fees, such assistance was not universal. In some cases, these free provisions were only available for a limited time or required minimum monthly SNAP sales. For smaller retailers, there could be some intangible costs, such as the mental burden towards the adoption of new technology. Larger chain retailers, on the other hand, were better positioned to overcome these challenges with the support of parent corporations.

Changes if Revenues

From a revenue perspective, EBT was expected to increase SNAP-eligible food sales for retailers by increasing individuals' SNAP participation. The government anticipated that EBT would encourage more eligible households to enroll in SNAP by reducing welfare stigma and lowering participation barriers for low-income households. However, the growth of U.S. economy during the 1990s and the subsequent decline in SNAP caseloads, particularly following the 1996 welfare reform, may have led some retailers to anticipate a decrease in SNAP customers. The actual impact on revenue likely varied by store type and depended on the shopping preferences of both newly enrolled and continuing SNAP beneficiaries.

Another important change caused by EBT was its impact on illegal revenue streams. Under the paper coupon system, some stores engaged in illegal activities to generate revenue, such as exchanging SNAP vouchers for a discounted amount of cash or selling ineligible items. With EBT, the USDA gained access to extensive SNAP benefit transaction records, which allowed for better detection of suspicious transaction activities that were previously difficult to monitor. The data enabled USDA's data analyst team to investigate suspicious patterns more effectively and send investigators to stores suspected of fraud and sanction those engaging in illegal practices. For stores that relied on these measures to generate additional profits, the potential revenue would have decreased due to this increased risk of detection. This change likely had a greater impact on smaller, independently owned stores, which historically showed higher rates of fraud.

Hypotheses and Research Design

Considering these factors, I hypothesized that smaller food retailers were more likely to be disincentized from participating in SNAP after the EBT reform. Smaller food stores were less likely to have the necessary electronic payment systems or the familiarity needed to implement EBT easily, making the adoption process more costly for them. Moreover, since over 80% of SNAP revenues were consistently redeemed at large retailers during the rollout, smaller vendors may have had limited prospects of generating enough SNAP revenue to cover the new upfront costs. Additionally, given that small stores, such as convenience stores or independently owned small groceries, have historically been most prone to fraud, the reduction in potential fraud revenue and the government's enhanced fraud detection capabilities may have further discouraged their participation or led to more disqualifications.

To test this hypothesis, I analyze the impacts of EBT on SNAP participation of food retailers separately for small and large stores. Additionally, I examine whether regions with a smaller base of SNAP customers, such as counties with lower poverty rates, experienced a larger reduction in the number of small SNAP stores to check whether the profit potential indeed played an important role. To understand the underlying mechanisms, I first investigate the role of the increased disqualification of small stores due to enhanced fraud detection. I then assess the importance of the financial burden related to upfront adoption costs.

4 Data

4.1 Data on EBT Rollout

I compiled county-level data on EBT implementation timing from various sources, including government reports and archived websites. The primary sources for identifying pilot counties and overall rollout timelines were the EBT Project Status Reports from 1999, 2000, 2002, and 2004. These reports provide summarized information on the EBT project status for all 50 states, the District of Columbia, and the territories. From these reports, I extracted information on the timing of the initial pilots, the names of pilot counties, and the start and completion dates of statewide EBT operations, along with relevant institutional details. This data was cross-referenced and supplemented with additional information from other sources, including archived state and county government websites, reports, emails, and phone calls from EBT managers in selected states, relevant studies, and newspaper articles. A comprehensive description of the data collection process can be found in Appendix B.

To enhance the accuracy of the EBT rollout information, I also used administrative data on county-level SNAP caseloads and issuance amounts provided by FNS. This biannual (January and July, FY 1989-2022) data improves the accuracy and comprehensiveness of the planned rollout timings, as it reflects actual implementation records rather than planned schedules. The primary source of this administrative data is Form FNS-388a, a document submitted by state governments to FNS on a monthly basis to report SNAP caseloads and benefit issuances for each county. States provide separate forms for each issuance system, such as paper and EBT systems. By examining when EBT issuance records first appeared in a given county, I identified the six-month window in which the county began issuing SNAP benefits through the EBT system. [15] [16]

By combining these sources, I compiled the EBT implementation timings of 3,088 counties, covering 98% of counties and over 99% of the U.S. population. I acquired the precise month and year of EBT adoption for 1,648 counties, and a range of potential implementation timings for the remaining 1,440 counties. On average, the timing range for counties without pinpointed EBT adoption timing is 3.7 months, with the longest range being 12 months. For approximately 90% of counties, the information obtained from the state EBT rollout plans and the administrative data on actual implementation was consistent.[17] I exclude Alaska, Hawaii, and Wyoming due to the lack of EBT rollout information. I additionally drop three counties where the EBT pilot was implemented before 1990, including Baltimore City (Maryland), Berks County (Pennsylvania), and Ramsey County (Minnesota).

Although the EBT rollout data is collected at a monthly level, I aggregate it at a quarterly level

[15]For example, if information about EBT issuance in a particular county appears in July 2000 but not in January 2000, I infer that the county began using the EBT system sometime between February and July 2000.

[16]While Melvin and Smith (2022) is the first to use issuance type data from the bi-annual FNS reports to identify when EBT replaced paper coupons, I enhance the coverage and accuracy of the EBT rollout timeline by supplementing and cross-referencing this data with information from various other sources.

[17]Regarding the remaining 10% of counties, Appendix **??** provides potential explanations for the observed discrepancies and decisions made for coding.

in all analyses to avoid creating small-sized treatment timing cohorts, a group of counties that were treated at the same time. This is because having sufficiently large timing cohorts is important for identifying cohort-specific treatment effects. For counties with a timing *range*, I define the start of the range as the time of treatment. This approach avoids misclassifying counties as untreated when they had actually already implemented EBT. This decision is particularly important for event study analyses that explore dynamic effects over time, as misclassification could result in false conclusions about pre-trends.

4.2 Data on Retailers

4.2.1 Administrative Data on Authorized SNAP Retailers

I obtained the Historical SNAP Retailer Locator Data from the FNS website. This administrative dataset contains rich information on store name, type, address, geodetic coordinates, and authorization start and end dates for any SNAP retailer authorized at any point since 1990. The data categorizes retailers into 17 types: convenience stores, small grocery stores, meat/poultry specialties, fruit/vegetable specialties, seafood specialties, bakery specialties, medium grocery stores, large grocery stores, supermarkets, superstores, wholesalers, combination grocery/others, food buying co-ops, delivery routes, farmers' markets, military commissaries, unknown. I exclude four categories unrelated to my study, including wholesalers, delivery routes, farmers' markets, and military commissaries, which represent 1.67% of the total observations.

As store categories are self-reported by owners, some stores with the same name were occasionally classified into different categories (e.g., 7-Eleven was classified as both a small grocery store and a convenience store). I clean the data by reclassifying stores, such as assigning large convenience store chains like 7-Eleven and Circle K to the convenience store category and reclassifying stores with certain specialty keywords (e.g., meat, seafood, bakery) as specialty stores. Additionally, I create two new categories: "liquor stores," based on keywords in store names such as liquor, LQ, beer, or wine, and "dollar stores," based on keywords of dollar, $1, 1.00, or 98 or less. I define "small stores" as convenience stores, small grocery stores, and liquor stores, and "large stores" as supermarkets, superstores, and large grocery stores. Two other categories used in the analysis are “specialty stores", which include all four types of specialty stores, and “others", which include medium grocery stores, dollar stores, co-ops, and combination grocery stores (primarily drug store chains such as CVS, Rite Aid, and Walgreens).

Given the institutional background suggesting that chain stores under large parent companies and independently owned stores may behave differently, I imputed each small store’s chain status based on the store name, following the methodology in Meckel (2020). Specifically, a store was classified as a chain if it satisfies one of two criteria: (1) the store’s name contains an outlet number (e.g., 7 ELEVEN 26349, CIRCLE K #08683) or the store’s name is the same as any store with an outlet number, or (2) there are 2 or more duplicates in the store names within the same state and the same calendar year. This method classifies 59% of small stores as chain stores.

Finally, I calculated the number of each type of SNAP-authorized retailer for each county-quarter from the first quarter of 1990 to the last quarter of 2002, the regulatory deadline mandated by the federal government. I exclude counties that adopted the EBT after the deadline from my analysis.

4.2.2 Data on Grocery Retailers from the County Business Patterns (CBP) data

I supplement the county-quarter panel on SNAP-authorized retailers with data on all retailers in each market from the County Business Patterns (CBP) dataset. CBP provides annual information on the number of establishments with paid employees, by industry and employment size. I use data from 1998 to 2002 because the industry classification system changed in 1998 from the Standard Industrial Classification (SIC) to the North American Industry Classification System (NAICS), making it difficult to crosswalk the two codes for fine subcategories. This dataset covers 1,956 counties that adopted EBT in or after 1998, representing 67% of the U.S. population.

I define small grocery retailers as convenience stores (NAICS 445120), gas stations with convenience stores (NAICS 447110), beer, wine, and liquor stores (NAICS 445310), supermarkets and other groceries with 0-4 paid employees (NAICS 445110). Large grocery retailers are defined as supermarkets and other groceries conditional with 20 or more employees (NAICS 445110) and warehouse clubs and supercenters (NAICS 452910).

4.3 Administrative Data on Disqualifications of SNAP Stores

To explore the impact of EBT on sanction activities against SNAP trafficking, I use administrative data on stores permanently disqualified from SNAP between 1990 and 2002. The data includes store names, addresses, and the date of the sanction. I merge this data with the Historical SNAP Retailer Locator data using store names, addresses, and sanction dates (authorization end dates in the case of historical SNAP data) as matching keys. A few observations without matched records in the SNAP retailer data are dropped. After matching and cleaning, I had a total of 11,969 store disqualification cases.

4.4 Administrative Data on SNAP Caseloads, Issuances, and Redemptions

To analyze the effects of EBT on SNAP participation, issuance, and redemptions, I used two additional datasets from FNS. The first dataset provides county-level SNAP caseloads and issuance amounts of January and July. I construct a balanced county-bi-annual panel for 2,415 counties, covering approximately 80% of the U.S. population.

The second dataset, also obtained from FNS, contains data on the dollar amount of SNAP redemptions by store type in each county. Due to privacy concerns, data is redacted in counties if very few stores of a particular type are located, resulting in some missing observations. I create a balanced county-quarter panel with 950 counties that contain information on SNAP redemption amounts from convenience stores and supermarkets throughout the study period. I exclude the period from the first quarter of 1990 to the third quarter of 1992 because redemption amounts for this period are too small to match national-level data from USDA and are therefore deemed unreliable. To the best of my knowledge, this paper is some of the first to use administrative data on SNAP redemptions by store type.

5 Empirical Method

This study leverages the rich variation in both across- and within-state EBT adoption timings to identify the causal impact of EBT on retailers' SNAP participation decisions. Appendix Figure C.1 illustrates the geographical variation in the EBT implementation timing covering 48 states. This hand-collected county-level variation offers two advantages in identification. First, it provides a greater degree of EBT rollout variation for causal identification, compared to prior studies that rely on state-year data or county-level data with limited geographic coverage. Second, using the county-level variation is essential to address the measurement error and attenuation bias arising from using statewide implementation dates to define the treatment timing. While the across-state variation is an important part of my identifying variation as 27 out of 48 states in my sample completed the statewide rollout within 6 months, the within-state variation is also considerable. Although only 10 states had a rollout exceeding 1 year,[18] roughly 43% of the population and 36% of SNAP-authorized retailers were concentrated in those ten states in 1990. By incorporating this within-state variation, I try to address the problem of attenuation bias.

Two aspects of the EBT reform make it a typical example of a "staggered adoption setting". First, the nationwide EBT implementation took more than 15 years, generating forty-four "treatment timing cohorts", defined as a group of counties that were treated simultaneously. Second, the transition into the EBT system was permanent in that once a county was treated, it never switched back to the paper system. Figure 3 shows the distribution of forty-four treatment timing cohorts.[19] On average, 70 counties are included in one treatment timing cohort, which often contains multiple states. For instance, the largest treatment timing cohort is the group of 1997Q3, which includes 330 counties from 10 states. In addition, the figure shows a concentration of 192 counties from four states in the third quarter of 2002, likely to meet the federal deadline of October 1st, 2002.

While researchers have conventionally employed the two-way fixed effect (TWFE) model to estimate the average treatment effect of the treated (ATT), a series of econometrics papers have

[18] States of Arizona, Maryland, Mississippi, and Texas took 5 quarters to implement EBT. New York took 8 quarters. New Mexico, New Jersey, California, Ohio, and Iowa took 20, 21, 28, 30, and 41 quarters, respectively.

[19] Note that I aggregate my monthly level data into a quarterly level to avoid having too many small timing cohorts.

recently shown that the specification could be problematic under the staggered rollout setting with heterogeneous treatment effects. Employing the TWFE analysis in a setting with multiple treatment timings may result in a biased estimation because of “forbidden comparisons" that use already-treated units as counterfactuals and the negative weight problem due to those bad comparisons, unless a strong assumption holds that the treatment effect is homogeneous across treatment timing cohorts and over time (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021) (see Appendix Section D for a more detailed discussion on this issue). Furthermore, Sun and Abraham (2021) shows that this problem cannot be addressed by making the TWFE model more dynamically flexible by adding time-to-event dummies. Under the existence of multiple treatment timings, lead and lag coefficients of the dynamic TWFE model may be contaminated by treatment effects from other event time periods unless strong assumptions are imposed.

To address this issue, I adopt an estimator robust to heterogeneous effects to cleanly identify treatment effects under the staggered setting. My main empirical specification uses Interaction-Weighted (IW) estimator developed by (Sun and Abraham, 2021, henceforth “SA"). The SA method addresses the problem of the possible forbidden comparisons in two steps. First, it estimates the “cohort-specific ATT" of each treatment timing cohort g for each time-to-event l, denoted by $CATT(g,l) = E[Y_{c,g+l} - Y^{Control}_{c,g+l} | G_c = g]$, where c denotes each unit (a county in my setting) and G_c is a time a unit c is treated. In this step, the SA method compares the outcome of each treatment timing cohort with a carefully chosen counterfactual group, explicitly excluding cases of forbidden comparison between treated and already-treated units. The estimation for $CATT(g,l)$ is based on the following equation (1):

$$y_{ct} = \alpha + \sum_{g \notin C} \sum_{l \neq -1} \delta_{g,l} \left(1\{G_i = g\} \cdot EBT^l_{c,t} \right) + \lambda_c + \eta_t + \epsilon_{c,t} \tag{1}$$

where C is a set of counterfactual counties and $EBT^l_{c,t} = 1\{t - G_c = l\}$ is an indicator that assigns 1 if a county c is l periods away from the initial EBT adoption at calendar time t. λ_c is county fixed effects that control the time-invariant idiosyncratic confounders and η_t is the time fixed effects for excluding the impact of national shocks on the outcome variable.[20] Under the parallel trends assumption and no anticipation assumption, $\hat{\delta}_{g,l}$ is an unbiased and consistent estimator of $CATT(g,l)$. I check the validity of the assumptions in Subsection 5.2. Standard errors are clustered at the county level throughout all specifications.

The second step aggregates the estimates of $CATT(g,l)$ from the first step to construct a summary estimator, the Interaction-Weighted estimator. Each county is weighted equally, as my goal is to estimate the average number of SNAP-authorized stores in a county that were changed after the EBT adoption compared to the pre-EBT level. The IW estimator for the dynamic effects is constructed by the following equation:

$$\theta_l^{dynamic} = \sum_g \delta_{g,l} Pr\left\{G_i = g \mid G_i \in [-l, T-l]\right\} \tag{2}$$

where T is the latest event time in the study window. Here, the weights, $Pr\left\{G_i = g \mid G_i \in [-l, T-l]\right\}$, are estimated based on the sample shares of each timing cohort at each event time, treating counties as a level of observations. For instance, if an event dummy of $l = \bar{l}$ is estimated using two treatment timing cohorts, say 20 counties of the cohort g=1995Q1 and 30 counties of the cohort g=1996Q1, then the weights for $CATT(1995Q1, \bar{l})$ and $CATT(1996Q1, \bar{l})$ would become 0.4 and 0.6.

Similarly, one can construct the DD estimator using estimates of $CATT(g,l)$. I assign the same weight for each county-time observation for this aggregation. The aggregated DD estimator is:

$$\theta^{DD} = \sum_l \sum_g \delta_{g,l} \cdot \frac{N_g}{\sum_g N_g \cdot T_g^{post}} \tag{3}$$

where N_g is the number of counties in the treatment cohort g and T_g^{post} is the number of post-treatment event time units for the treatment cohort g, including the event time $l = 0$. Therefore, $\frac{N_g}{\sum_g N_g \cdot T_g^{post}}$ is the weight applied to each CATT estimated in the first step, $\delta_{g,l}$.

[20]Note that this equation (1) is basically an *extended* version of the standard TWFE model, $y_{ct} = \alpha + \beta EBT_{ct} + \lambda_c + \eta_t + \epsilon_{ct}$, where interactions between timing cohort indicators and dummies for relative time periods are added.

5.1 Main Sample and the Choice of Counterfactuals

I use 192 counties of the treatment timing cohort 2002Q3, which adopted EBT right before the regulatory deadline of October 1st, 2002, as my counterfactual group.[21] One can see in Figure 3 that the share of the timing cohort 2002Q3 is relatively larger than that of other timing cohorts, perhaps due to a concentration of adoptions to meet the deadline. This group includes 2 counties from California, 80 counties from Mississippi, 88 counties from Nebraska, and 22 counties from Virginia. Panel B of Appendix Figure C.1 illustrates these counties in the darkest blue color, and as can be seen in the map, this counterfactual group consists of counties from various parts of the nation.

I drop 227 counties that adopted EBT after the regulatory deadline, including 54 counties from California, all 3 counties of Delaware, all 98 counties of Iowa except one pilot county, all 16 counties from Maine, 1 county from Mississippi, and 55 counties from West Virginia (see Panel C of Appendix Figure C.1 for the locations of those counties). I drop them from my main sample as their adoption timings are less likely to be quasi-random.[22] This leaves 2,848 counties from 37 treatment timing cohorts in my main sample.

5.2 Validity Checks for Key Identification Assumptions

For $CATT(g, l)$ to successfully identify the treatment effect of a given timing cohort g at each event time l, two key assumptions are required: the parallel trends assumption and the no anticipation assumption.

The parallel trends assumption requires that had it not been for the treatment, the average

[21]The SA method allows using either never-treated units or last-to-be-treated units as a counterfactual. As no county remained untreated in my setting, I choose the last-to-be-treated units as counterfactual.

[22]All these states received waiver to extend their adoption timing after the deadline, as they were considered as “facing unusual barriers to implementing an electronic benefit transfer system," such as “high EBT costs in comparison to paper issuance costs, lack of sufficient staff resources, lack of technical expertise, and competing priorities" (Food and Service, 2003). For instance, California received a waiver from FNS because the EBT implementation was delayed due to California’s unique SNAP (called as *CalFresh* in California) environment. Unlike most of the other states where a state government administers SNAP, CalFresh is administered at each county government. This decentralized system made it more difficult for California to procure its EBT system, delaying statewide implementation. This unique feature that could make California very likely to differ from counties that adopted EBT beforehand is also a reason that I do not use the timing cohort of 2004Q2 as my counterfactual, although the very last adoption of EBT occurred in 2004Q2.

outcome evolution in any treatment timing cohort (counties that were treated at the same time) would have evolved similarly to that of the counterfactual group, the last-to-be-treated counties, without the inclusion of time-varying covariates ("unconditional" parallel trends assumption).[23] This assumption would be violated if the EBT rollout schedule is systematically correlated with changes in county characteristics that might also affect the local retailing environment. For instance, if counties tended to adopt EBT when they were about to experience an economic downturn with an increasing need for SNAP, this would bias the causal estimate because not just the EBT adoption but also the changing retailing environment would affect retailers' SNAP participation decision.

To assess the exogeneity of the EBT rollout schedule across counties, I conduct three tests. First, I examine whether some pre-EBT county characteristics were strongly associated with the county's EBT adoption timing in my main sample. The idea is that if early and late EBT adopters have systematically different characteristics before the adoption, they might have different outcome evolutions even without the EBT reform. I focus on variables such as the share of the population under the poverty line, the size of the SNAP program, the employment-to-population ratio,[24] population size, variables related to the 1996 welfare reform, and various demographic characteristics such as shares of population Black, Hispanic, young (under 6), and old (65 or over), and the urban area dummy in or before the first quarter of 1990. In Appendix Section E, I regress the index of the year-quarter of the EBT adoption of a county, where the first quarter of 1990 is indexed as 1 and subsequent quarters are incremented by 1, on these various pre-EBT variables. The results, presented in Appendix Table E2, indicate that a county with a higher share of Hispanic, young, or older populations tended to adopt EBT earlier. while some non-randomness exists in the rollout timing, the magnitudes are small. For instance, given the 25th and 75th percentiles of Hispanic population share (0.37 and 2.37), moving from a county at the 75th percentile to one at the 25th percentile is associated with a three-month delay in EBT adoption during the 14-year period from

[23] A "conditional" parallel trends assumption requires a weaker exogeneity constraint compared to the unconditional parallel trends assumption because it requires parallel trends among counties that have similar characteristics.

[24] I use employment to population ratio instead of county unemployment rate because the unemployment rate at the county level is calculated based on a model rather than based on the actual statistic.

1990 to 2004.[25] Additionally, I do not find any significant relationship between EBT adoption timings and several potential confounders, such as pre-EBT poverty rate, SNAP program size, economic conditions, and the 1996 welfare reform. However, as this test reveals some non-randomness in the EBT rollout timings, I present results both with and without time-varying county covariates in the main analysis to assess the impact of these controls on my findings.

Second, I examine whether the exact timing of EBT adoption was systematically correlated with county level "trend" in local economic conditions. I specifically focus on local economic conditions such as employment and wage levels because they could be a critical confounding factor. Ganong and Liebman (2018) show that the booming economic conditions are closely associated with the decline in SNAP caseloads between 1992 and 2007, while Byrne et al. (2022) find that retailers respond to the SNAP program size. To test whether EBT implementation is associated with trends of local employment or wage trends, I estimate Equations (1) and (2) using variables reflecting county-specific economic conditions as an outcome, including employment-to-population ratio and per capita wage from Quarterly Census of Employment and Wages (QCEW). Panel A and B of Appendix Figure F.1 show that there was no trend change in these local economic measures before and after the EBT implementation. The overall DD estimates in Appendix Table F3 also show insignificant and very small effect, suggesting the rollout timing was not determined by the local economic changes that could independently affect stores' SNAP participating decision.

Third, in Section 6, I examine results from event study analysis to see the pre-treatment trends in the dynamic effect of EBT on the number of SNAP retailers. The result in Figure 4 does not show any sign of pre-trends. Importantly, I compare results between specifications with and without time-varying county covariates, including a natural log of the county population, demographic characteristics such as a percentage of population Black, Hispanic, young (age 0-5), and old (age 65 or more), the employment-to-population ratio, and indicators for the AFDC waiver timing and TANF implementation,[26] to check if the inclusion of controls have significant impacts on findings.

[25] See Appendix Section E for discussion on the interpretation of the magnitudes for other variables.

[26] The AFCD waiver and TANF indicators assign 1 once a state where a county c is nested starts to implement the AFDC waiver or TANF. If a state did not implement the waiver, the AFDC waiver indicator for a county c in that state is coded as zero. The TANF indicator contains the TANF implementation timing of a state. Once TANF started, I

I find that the inclusion of various county covariates does not change the estimates of the dynamic effects, suggesting little impact of time-varying county characteristics.

The no anticipation assumption requires that any eventually treated unit does not exhibit anticipatory behavior prior to the treatment. This assumption is highly likely to be held in my setting, as it was technically impossible to use EBT cards before the actual implementation. EBT cards were first distributed after the planned implementation date, and SNAP customers only began to use them then. State governments and EBT processors began implementing policies to assist some SNAP retailers with limited resources with EBT adoption (such as offering free EBT devices) after the planned date.

One might still be concerned that counties adopting EBT shortly before the deadline might systematically differ from those adopting EBT earlier, and thus could have exhibited different outcome trends even without the EBT adoption. To assess the robustness of my choice of counterfactual, I use not-yet-treated counties as an alternative counterfactual. As not-yet-treated counties consist of counties treated at relatively similar times to the treated counties, the counterfactual group more closely resembles treatment timing cohort. [27] Because the SA method does not allow the not-yet-treated group to be used as a counterfactual, I adopt another heterogeneity-robust estimator developed by Callaway and Sant'Anna (2021) that allows researchers to use the not-yet-treated as a comparison.

6 Main Results

6.1 Impacts of EBT on the Number and Share of SNAP Retailers

To investigate how the EBT reform impacted grocery retailers' SNAP participation, I estimate the dynamic treatment impacts of EBT from Equations 1 and 2, using the number of the SNAP-authorized retailers of each county as an outcome variable. Figure 4 shows the treatment effect

coded the AFDC variable as zero to separately measure the TANF period and the AFDC waiver period.

[27]Note that when using the not-yet-treated counties as a control group, the comparison group changes by each treatment timing cohorts.

estimates and their 95% confidence intervals for the number of small SNAP retailers in Panel A and the number of large SNAP retailers in Panel B, covering 16 quarters before and after EBT implementation.[28]

My preferred specification is the model with the SA method without time-varying controls, as it allows me to directly examine the validity of the unconditional parallel trends assumption. In Panel A, estimates from this model, denoted as dots and red-colored lines, are close to zero and mostly insignificant before the EBT adoption, working positively with the assumption. After the EBT adoption, the number of small SNAP retailers exhibit a gradual decline over time. On the other hand, in Panel B, estimates for large SNAP retailers for the post-EBT period are close to zero and mostly statistically insignificant. I plot estimation results from three other specifications to check the robustness of the findings. Adding time-varying county covariates does not change the point estimates and confidence intervals (denoted as triangles and green lines). This little impact of time-varying controls again confirms the validity of the unconditional parallel trend assumption. I also test whether the results change if I run the event study analysis with the perfectly balanced panel consisting of 1,478 counties that adopted EBT between 1994Q1-1998Q2 and 192 counterfactual counties that adopted EBT in 2002Q3, right before the regulatory deadline. The result (denoted as squares and blue lines) shows that the size of the post-EBT coefficients slightly decreased, but the general decreasing patterns still hold. Finally, I use an alternative counterfactual group of not-yet-treated counties using the CS specification. The result, denoted as short dashes and purple lines, is very similar to those from SA specifications, suggesting that my results are robust to the choice of counterfactuals.

Columns (1) and (2) of Table 2 present the corresponding DD estimate from the SA specification without time-varying covariates based on Equations 1 and 3. The result in Column (1) indicates that the EBT reform led to a decrease of 3.8 small SNAP stores per county, or 13.1% compared to the pre-EBT mean. In contrast, Column (2) indicates that the impact of EBT on the number of large

[28]The earliest event time (l=-16) and the last event time (l=16) in Figure 4 are estimated from 2,822 and 1,694 counties, which cover 99.1% and 59.5% of the main sample of 2,848 counties, respectively. I exclude coefficients before event time $l = -16$ and after event time $l = 16$ to avoid having estimation results from an overly imbalanced panel.

retailers is minimal, with an increase of only 0.1 stores (1.2% compared to the pre-EBT mean) at a low significance level.[29] In Column (3), I estimate the impacts of EBT on the number of total SNAP retailers, including small, large, specialty, and other types of SNAP-authorized retailers. The result shows that after EBT, the number of total SNAP stores in each county decreased by 4.6 on average. Considering the decline in SNAP-authorized retailers during the 1990s, from a peak of 61.5 stores per county in 1993 to 43.5 in 2002, I estimate that the EBT reform can be attributed to 26% of the decrease during this period.

To more directly measure the impacts of EBT on the popularity of SNAP participation among grocery retailers, I do the same analysis using the *share* of SNAP-authorized stores as an outcome variable, both for the overall market and separately by store size. This share is calculated as the ratio of SNAP-authorized stores to the total number of stores in the market, including both SNAP and non-SNAP stores. The CBP data for the denominator covers the period of 1998-2002, leaving 1,432 counties in the sample.[30][31] Panels A and B of Figure 5 show patterns similar to Figure 4. The ratio of small SNAP stores to total small stores decreased after the EBT, while little impact is found on the ratio of large SNAP stores throughout all specifications.[32]

Columns (4) and (5) of Table 2 contain the corresponding DD estimates of the impacts of EBT on the ratio of SNAP stores. I assign weights when estimating these three models based on the county population to give larger weights to larger counties that tend to have larger number of retailers.[33] The results indicate that the share of small SNAP stores among stores in the small store

[29] In Appendix Table G4, I estimate the DD estimates using two other retailer categories, SNAP-authorized specialty stores and other types (including medium-sized grocery stores, dollar stores, combination grocery stores, and food-buying co-ops), as an outcome variable. The estimated DD coefficients show a reduction in the number and share of SNAP-authorized stores among specialty stores and other store categories, suggesting that stores other than large store categories were also negatively impacted by the EBT reform.

[30] See 4.2.2 for details on how I define the small and large grocery store categories from the CBP data using the NAICS code.

[31] I plot the dynamic effects for shorter periods to avoid having a panel that is too imbalanced in terms of the event time. Specifically, I cut the panel at the event time l=-4 and l=12, as 1,087 and 1,308 counties have non-missing information on the denominator for the event time l=-4 and l=16, respectively, not making the panel structure severely imbalanced.

[32] Unlike Figure 4, I do not run analysis using a balanced panel because the sample size becomes too small if I restrict it to the perfectly balanced panel.

[33] I use the county population instead of the denominators (the number of small, large, and total grocery retailers) as some counties have zero stores, thereby having zero weights. However, even if I apply the denominators as weights, the estimation results are very similar.)

category substantially decreased by 8.9%, whereas the share of SNAP-authorized large retailers remain similar after the EBT implementation. Overall, the result in Column (6) shows that the EBT reduced the share of SNAP stores by 5.1%. This result clearly implies that the popularity of the SNAP program decreased after the EBT reform, particularly among small stores, such as small grocery stores, liquor stores, and convenience stores.

Lastly, I conduct a placebo analysis using the number of overall small and large grocery retailers as an outcome variable, considering that it is very unlikely that the EBT reform had a causal impact on the overall grocery retailing environment. As expected, both the dynamic effects on the number of small and large retailers (in Appendix Figure H.1) and the corresponding DD estimates (in Appendix Table H5) indicate that EBT did not substantially impact the number of small and large retailers in the market.

6.2 Heterogeneity by Regional Characteristics and Store Ownership

I explore potential heterogeneity in the treatment effects across two dimensions. First, I examine whether the decline in small SNAP stores is concentrated in certain regions, particularly those with limited access to large food retailers, to assess whether these areas were disproportionately impacted by the decrease in small SNAP stores. Specifically, I analyze heterogeneity by county poverty rate,[34] urbanicity,[35] and access to large SNAP stores, using the number of large SNAP stores per 1,000 people in poverty in each county as a proxy for accessibility.[36] Second, I investigate whether stores historically more prone to fraud experienced a greater decline, as reducing fraudulent

[34]I define high and low poverty neighborhoods based on the poverty rate of each county in 1989, before the EBT rollout started. Counties whose poverty rate is above the median of 1989 are defined as high-poverty neighborhoods, and the rest are defined as low-poverty neighborhoods.

[35]I define urban and rural areas based on the Rural-Urban Continuum Codes for the year 1983. I define counties under the following categories as urban areas: central or fringe counties of metro areas of 1 million population or more, counties in metro areas, nonmetro counties with urban population of 2,500 or more, adjacent to a metro area. Counties under the following categories are defined as rural areas: nonmetro counties with urban population of 2,500 or more, not adjacent to a metro area, nonmetro counties that are completely rural or less than 2,500 urban population.

[36]I define a county as having relatively better access to large SNAP stores if the county has more than 1.1 stores per 1,000 people in poverty, which is the median value of the store-to-poverty variable in 1990Q1. The rest are defined as counties with worse access to large SNAP stores.

transactions was a key goal of the EBT reform.[37] To do this, I separately examine the impacts on small SNAP vendors that are independently owned versus chain stores, as independently owned stores have historically exhibited higher trafficking rates, according to FNS estimates.[38]

The results in Table 3 indicate that the decline in small SNAP stores after EBT implementation was more pronounced in counties with lower poverty rates, urban counties, and in places where the population in poverty had access to a larger number of SNAP stores. The finding of a larger reduction in small SNAP stores in low-poverty counties aligns with my hypothesis that SNAP's profitability may have played a key role, as a smaller SNAP customer base in these regions would make it harder for retailers to generate substantial profits from SNAP transactions. Additionally, the larger reduction of small SNAP stores in urban areas suggests a possibility that small stores may have become less attractive as redemption locations for beneficiaries who lived near larger SNAP-accepting retailers, leading to a decline in SNAP-related revenue for small stores. I explore this possibility more in Section 8. Moving to Columns (5) and (6), although both counties with relatively better and worse access to large SNAP retailers experienced an average reduction of 4 small SNAP stores per county, the implications of this decline could differ. In counties with limited access to large SNAP retailers, low-income families likely faced fewer alternatives for redeeming their benefits, while in counties with better access to supermarket-type large groceries, the reduction might be less problematic.

Finally, in Columns (7) and (8), I find that both independently owned small stores and chain small stores that participated in SNAP decreased after EBT. The reduction is slightly larger among chain stores, but the magnitudes based on the 95% confidence interval of the point estimate overlap.

[37]Note that (Meckel, 2020) finds that the WIC EBT reform in Texas during 2005-2009 negatively affected the WIC participation of non-chain, independently owned stores because the new EBT system reduced fraudulent revenue stores were able to make from conducting price discrimination between WIC and non-WIC customers. I investigate whether similar patterns emerge in the context of SNAP EBT.

[38]The first official FNS estimates of store trafficking rates are calculated for the fiscal year 1993 (Macaluso, 1995). The estimated store trafficking rate is higher among independently owned small stores (12.0%) than the trafficking rate of publicly owned small stores (1.7%). These patterns also hold in subsequent estimates in 1996-1998, 1999-2002, and 2005 (Cline and Aussenberg, 2018).

7 Mechanisms

In this section, I explore two channels that may have driven small SNAP stores' responses to the introduction of EBT: the fraud control effort associated with the EBT reform and the cost of EBT adoption.

7.1 Fraud Prevention Effort

First, I examine whether the fraud prevention effort of FNS, an agency under USDA that monitors the SNAP program, after the EBT reform by disqualifying fraudulent stores was the driving factor behind the reduction of small SNAP stores. An important part of the transition to the EBT system is to enable FNS to collect and monitor comprehensive SNAP transaction records that had not been available under the paper coupon system. Using the data, a highly trained team of data analysts investigates signs of unusual or suspicious transactions, such as exchanging SNAP benefits with cash (commonly referred to as "trafficking of SNAP benefits") or selling any ineligible item. If such activity is detected, FNS sends trained investigators to the store to investigate potential fraudulent activities. As the EBT system facilitated the government's store monitoring system, it might have induced substantially more sanctions against the trafficking of SNAP benefits, thereby reducing the number of SNAP vendors that were historically more fraud-prone.[39]

To investigate this channel, I estimate Equations 1 and 3 using the number of permanently disqualified stores as an outcome variable.[40] Permanent disqualifications are a good proxy for the number of revealed trafficking cases, as "disqualification for trafficking is generally permanent" (Cline and Aussenberg, 2018). Due to the low number of permanently disqualified cases and the prevalence of zero disqualifications at the quarterly level, I aggregate the panel data to an annual

[39] Appendix Figure I.1 illustrates the yearly trend of the number of permanent disqualifications by types of retailers. Most disqualifications indeed occurred in small stores and specialty stores. Around 1994, at the early stage of the EBT rollout, cases of permanent disqualifications started to increase substantially. However, the level of disqualification cases is not high, with less than 250 cases per year occurring throughout the study period.

[40] Due to the data constraint, I only use the "permanently" disqualified stores for the analysis. Permanent disqualifications account for more than half of the total disqualifications, according to Retailer Management Annual Reports from FNS. All reports can be downloaded on the FNS website (link: https://www.fns.usda.gov/snap/retailer/data).

level.

Figure 6 illustrates the dynamic effect of EBT on permanent disqualifications of small stores. The main specification, denoted as red-colored circular dots, shows a clear increase in permanent disqualifications after EBT. The corresponding DD estimate indicates that after EBT, permanent disqualifications increased by 69% compared to the average cases during the period.[41] However, the point estimates are so small that they explain roughly 2% of the average reduction in the number of small SNAP vendors after EBT. I plot results from two other specifications to show the robustness of the result to the inclusion of controls and the alternative counterfactual group. This result is consistent with findings from Beatty et al (2024), which use data on both permanent and temporary disqualifications and find a causal impact of EBT on overall disqualifications.

Results from this exercise suggest that the increase in disqualifications after EBT is not the main reason for the reduction of small SNAP Stores. However, the clear increase in permanent disqualifications shows that the EBT reform improved the USDA's store activity monitoring system, thereby achieving its goal of enhancing integrity of the SNAP program by sanctioning fraudulent stores to some extent. Additionally, the reform may also have indirectly improved program integrity by deterring participating stores that previously engaged in activities not allowed by USDA from continuing such behavior. This is likely due to store owners perceiving an increased risk of apprehension associated with the EBT system, as the elimination of trafficking was frequently cited as a key objective of the reform.

7.2 Cost of EBT Setup

Next, I investigate whether the cost of EBT adoption was a reason behind the reduction of small SNAP stores. While state governments provided some support for retailers, such as free EBT devices and installation fee reimbursement, this might still have been a substantial mental and financial burden for some retailers, especially small ones, without experience with electronic

[41]For this exercise, I do no use the mean of the first quarter of 1990 as the disqualification cases are too low.

transactions.[42] Given that it was only after the mid-1990s that the new payment system gradually became familiar,[43] this first-time EBT adoption cost could have been more prevalent problems in counties that adopted EBT earlier. Thus, I hypothesize that the transition to EBT was more burdensome in counties that adopted it earlier.

To test this hypothesis, I aggregate the cohort-specific ATTs separately by county adoption timings. Specifically, I estimate the summary ATT for three groups: counties that adopted EBT voluntarily before the 1996 Welfare Reform Act mandated the adoption (early adopters: timing cohort of 1996Q2 and before), counties that adopted EBT soon after the EBT adoption was mandated (mid adopters: timing cohorts of 1996Q3-1999Q4), and finally, counties that adopted EBT close to the regulatory deadline (late adopters: timing cohorts of 2000Q1-2002Q3). I also add analysis on California as a unique case study. The state provided free EBT-only devices to participating retailers and adopted EBT during a time when the new payment system was becoming more prevalent.[44] I exclude San Bernardino and San Diego counties, which implemented very early pilot programs in 1997 before California's EBT project was fully scheduled. I use eleven counties that were among the last to adopt EBT as counterfactuals.

Consistent with my hypothesis, Table 4 shows that the decline in the number of small SNAP stores is most pronounced in counties with early EBT adoption. The magnitude of the effect diminishes in counties with later implementation. I even find a positive DD estimate in California, although it is not statistically significantly different from zero. These results, while being suggestive, point towards the possibility that the first-time EBT adoption cost might play an important role.

[42] According to (Food and Service, 1995), more than 90% of stores participating in Food Stamp Program in 1994 did not pre-equip POS machine, particularly small stores.

[43] The number and share of electronic payments started to decrease gradually since the mid-1990s, exceeding the number of check payments in 2003 Gerdes and Walton (2005).

[44] Although ideally, I would analyze the heterogeneous effects by different free EBT-only device policies across states (e.g., full provision vs. partial provision), the state-level information on that policy is not available. Consequently, the analysis in this section focuses on adoption timing and California's distinctive case.

8 Discussion on Program Efficiency, Accessibilit, and Equity

In this section, I discuss how the adoption of EBT could have affected the effectiveness of the SNAP program, focusing on efficiency, accessibility, and equity.

If EBT successfully reduced SNAP administrative costs without limiting access for those in need, it would imply an improvement in program efficiency. Evidence suggests that the EBT system did indeed decrease administrative costs. EBT was designed to reduce the costs associated with SNAP benefit issuance and redemption while maintaining cost neutrality, ensuring that expenses would not exceed those of the paper coupon system.[45] For example, the Maryland EBT Demonstration reported a reduction in cost per case after adopting EBT (Logan et al., 1994).[46]

Additionally, evidence suggests that SNAP participation increased after the adoption of EBT. One might assume that the reduction in small SNAP stores could have increased the travel distance to primary grocery stores, thereby raising travel costs. However, this effect is unlikely in areas with access to large grocery stores. Many SNAP recipients reported using large grocery stores as their primary shopping location, while using small SNAP stores only as supplemental options. Indeed, studies show either neutral or positive effects of EBT on SNAP participation rates at the national level, with increases ranging from 1.6% to 12% based on the specification used (Currie et al., 2001; Danielson et al., 2006; Melvin and Smith, 2022; Zhou et al., 2024). In line with these findings, my analysis shows a 0.8pp increase in SNAP participation rates, which can be translated into a 10.1% increase compared to the baseline mean (Column (1) of Panel A of Table 5).[47] This positive effect likely reflects reduced time costs associated with benefit pickup and the decreased stigma linked to using EBT.

While these findings suggest efficiency gains from transitioning to the EBT system, concerns about equity remain. In particular, I focus on counties where access to large, high-quality food

[45]The Farm Security and Rural Investment Act of 2002 eliminated this cost neutrality requirement.

[46]I recently obtained state-level data on SNAP administrative costs for 1994–2004 and plan to add analyses on the impact of EBT implementation on these costs.

[47]I conduct analysis using the bi-annual data on SNAP caseloads from FNS and county population from US Intercensal County Population Data. I downloaded the US Intercensal County Population Data from the NBER website (Link to the website).

retailers is limited. The reduction in the number of small SNAP-authorized retailers may have increased travel distances for residents in these areas, potentially limiting access to SNAP benefits. If the benefits of EBT did not reach these underserved regions, it raises concerns about the equity of the program's impact.

I use two proxies to assess county residents' accessibility to large food retailers. The first proxy is the number of large retailers per 10,000 people in the first half of 1997.[48] Counties with fewer than 1.7 large retailers per 10,000 people, which is the median value, are considered to have limited access. The second proxy is the number of large SNAP retailers per 1,000 people in poverty in the first half of 1990. Counties with fewer than 1.1 large retailers per 1,000 people in poverty, again the median value, are classified as having limited access to large SNAP retailers, as defined in Section 6.2. This measure aims to identify regions where SNAP-eligible individuals face potentially low access to large grocery retailers. I additionally use the 1989 county poverty rate as another indirect measure of limited access to large food retailers, based on the assumption that higher-poverty neighborhoods may have relatively lower access to large food grocers, as large chain supermarkets may have less incentive to locate in high-poverty neighborhoods due to infrastructure, zoning, crime, or other challenges (Bitler and Haider, 2011). Counties with poverty rates above the 1989 median are classified as high-poverty, again following the definition in Section 6.2.[49] For this analysis, data is aggregated in a bi-annual format, corresponding to FNS' SNAP participation and issuance data, which is collected every January and July. When using the SNAP participation rate as an outcome, I apply weights by the denominator, the county population.

Column (1) of Panel A of Table 5 shows a positive impact of EBT on the SNAP participation rate, as mentioned earlier. In Panel B, I find that the average dollar amount of SNAP benefit issuance increased by $66,000 per county, representing a 17.7% increase compared to the baseline mean. Columns (2) and (3) indicate that both the participation rate and issuance amount increased

[48]I use data from the first half of 1997, as it is the earliest available information on the number of large retailers in each county from the County Business Pattern dataset.

[49]The Economic Research Service (ERS) began providing Food Desert Locator data in 2006, which includes ERS's official estimates of food desert status for each census tract. I chose not to use this data to define food desert status at the county level because it lacks pre-EBT information and may present endogeneity issues, given that EBT implementation could have influenced a county's food desert status.

more significantly in counties with better access to large grocery retailers. Similarly, Columns (4) and (5) show that the size of the SNAP program expanded considerably more in counties where low-income individuals had relatively better access to large SNAP retailers. Columns (6) and (7) reveal a pattern where counties with lower poverty rates experienced a larger increase in both the SNAP benefit issuance amount and SNAP participation. These findings suggest that the benefits of EBT on SNAP beneficiaries were not equally distributed across all regions. These findings suggest that the benefits of EBT were not distributed evenly across all regions. Counties with better access to high-quality large grocery stores experienced greater positive effects, implying that the benefits of EBT were concentrated in areas with good access to large SNAP retailers.

In addition, I use data on quarterly SNAP redemption amounts at the county and store type level to explore whether SNAP recipients began redeeming larger amounts of benefits at supermarkets following the EBT reform.[50] [51] This shift could be attributed to several factors: the decrease in convenience stores, reduced stigma associated with EBT, or the centralization of shopping by a single cardholder, which may have led to a concentration of purchases at primary stores. Regardless of the reason, supermarkets generally offer a wider variety of food items at lower prices, suggesting that an increase in benefit redemptions at these stores could have been beneficial for SNAP recipients in these regions. If supermarkets indeed offered lower prices, this would increase the real value of SNAP benefits. Even if prices were similar, having access to a wider range of healthy food options could have positively impacted nutritional outcomes. For this analysis, I use a balanced panel of 950 counties with non-missing information on SNAP redemption amounts from both convenience stores and supermarkets during the study period. To control for the mechanical impact of changes

[50] I specifically focus on SNAP-authorized convenience stores and supermarkets, as these are the two most prevalent store types, and thus are less affected by data suppression rules for counties with a very small number of SNAP stores of a particular type.

[51] It is worth noting that while small stores make up the majority of SNAP-authorized retailers, most SNAP benefits are redeemed at large stores. For example, Castner and Henke (2011) reported that 84% of SNAP benefits were redeemed at supermarkets and supercenters. Additionally, Ohls et al. (1999) found, based on a nationally representative survey conducted in 1996, that nearly 90% of low-income individuals primarily shop at supermarkets, regardless of their SNAP participation status. When asked about other types of stores they sometimes used, 50% reported neighborhood grocery stores, and 41.6% mentioned convenience stores (multiple choices allowed). Given these findings, one could infer that the decline in SNAP participation among small stores would not significantly affect accessibility for low-income individuals unless there are few or no large grocery stores participating in SNAP in the vicinity.

in the number of small and large SNAP stores on redemption amounts, I normalize the SNAP redemption data by dividing the total redemption amount by the number of stores of each type in Q4 1993, when the number of SNAP vendors was highest. Inflation adjustments are made based on 1990 dollars.

Figure 7 shows that the normalized average SNAP redemption amount decreased sharply in authorized convenience stores (Panel A) while increasing significantly in authorized supermarkets (Panel B). Combined with the findings in Table 3, which show that small SNAP stores declined more in counties where large SNAP stores were more prevalent, these results suggest a shift in SNAP redemption patterns from smaller to larger stores following EBT adoption, potentially leading to improved program efficiency in regions with existing supermarkets during the study period.

9 Conclusion

This study shed light on the important role that private firms play in the effective delivery of in-kind government transfers, with a focus on the SNAP program. Specifically, I examine the impact of the EBT adoption, which transformed SNAP's benefit disbursement system from paper vouchers to plastic cards that function like debit cards. To address data limitations on EBT adoption timings, I compile a novel county-month level dataset that significantly improves the coverage and accuracy of information on EBT adoption timings from various sources.

By leveraging the staggered rollout of EBT rollout across counties and employing heterogeneity-robust estimators, I find that EBT led to a substantial reduction in the number and share of small SNAP-authorized retailers. The overall decrease in the number of SNAP retailers is primarily driven by a decline in small stores (13.1% compared to the baseline mean). Evidence suggests that this reduction can be attributed to the upfront costs associated with setting up the EBT system.

While EBT implementation improved program efficiency through reduced administrative costs and increased individuals' SNAP participation, the exits of small food stores from the SNAP program raise concerns for low-income households with limited access to large supermarkets.

Although recipients in regions with ample access to large retailers benefited from the EBT, those in areas lacking supermarkets may not have experienced the same advantages. I find that the individuals' SNAP participation rates increased across all regions, but the gains were much higher in counties where people had relatively better access to large food retailers. These results suggest that while EBT improved program efficiency, these efficiency gains were largely concentrated in areas with relatively good access to large SNAP retailers.

Policy implications from this study are significant. To enhance the effectiveness and equity of in-kind transfer programs, it is essential to address the barriers small businesses face when adopting new technologies like EBT. Lowering adoption costs and providing support for small retailers can encourage their continued participation in programs like SNAP. This approach is particularly relevant for current and future policies aimed at expanding EBT technology usage in other areas, such as promoting EBT transactions at farmers' markets. By reducing financial and technical hurdles, policymakers can ensure broader retailer participation, thereby improving access for recipients across diverse communities.

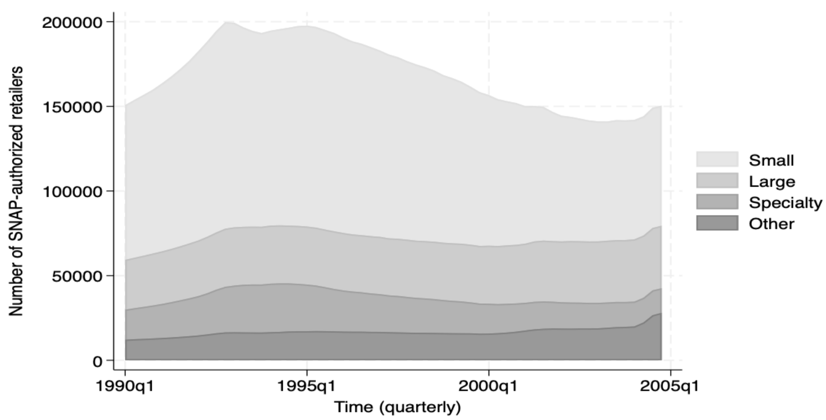

Figure 1: Trend of SNAP-authorized retailers, by store type

Note: This figure plots the trend of the number of SNAP-authorized retailers by subgroups, using Historical Retailer Locator Data from FNS. “Small" category consists of small grocery stores, convenience stores, and liquor stores. “Large" contains large grocery stores, supermarkets, and superstores. “Specialty" are specialty stores, including meat and poultry specialties, seafood specialties, fruit and vegetable specialties, and bakeries. “Others" contains medium-sized grocery stores, dollar stores, combination grocery stores, and food-buying co-ops.

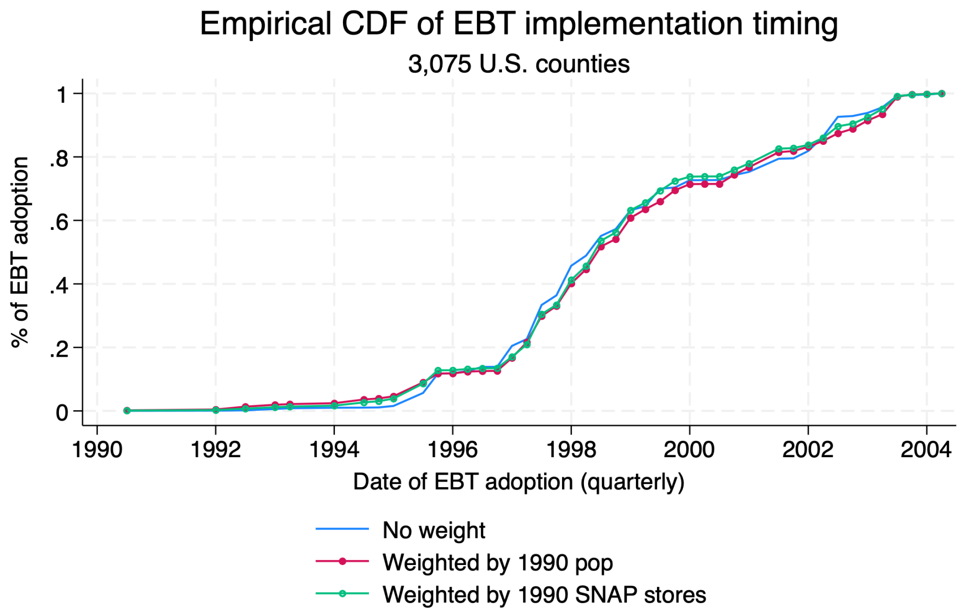

Figure 2: EBT rollout across U.S. counties

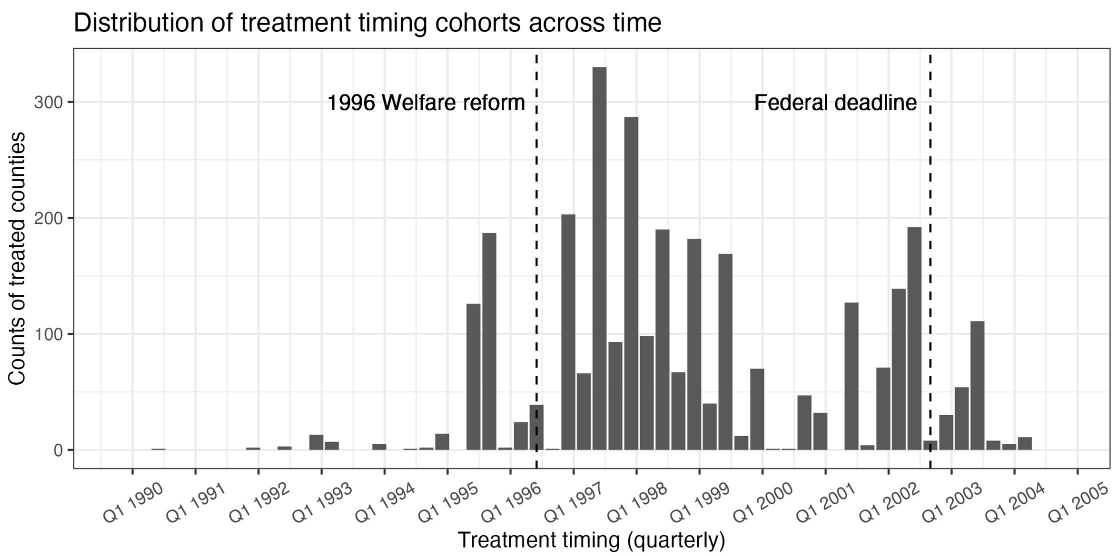

Figure 3: Distribution of the treatment timing cohorts

Note: The two dashed lines in the graph represent the timings that the EBT implementation was mandated by the 1996 welfare reform law (August 1996, left line) and that the deadline for the implementation (October 2002, right line).

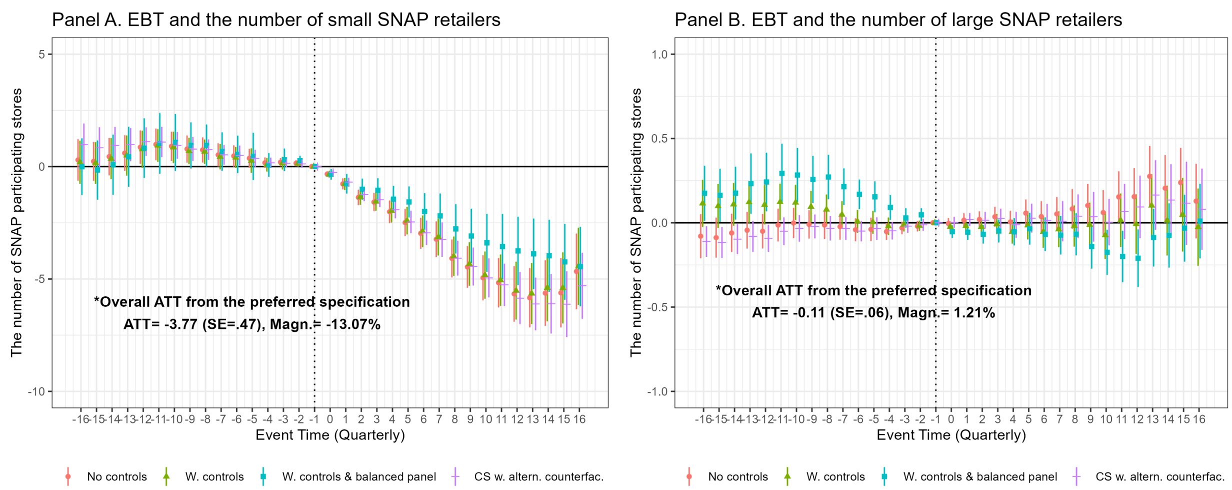

Figure 4: Impacts of EBT on retailers' SNAP participation

Note: In each Panel, I provide an estimate of the overall DD estimator based on the SA specification without control variables.

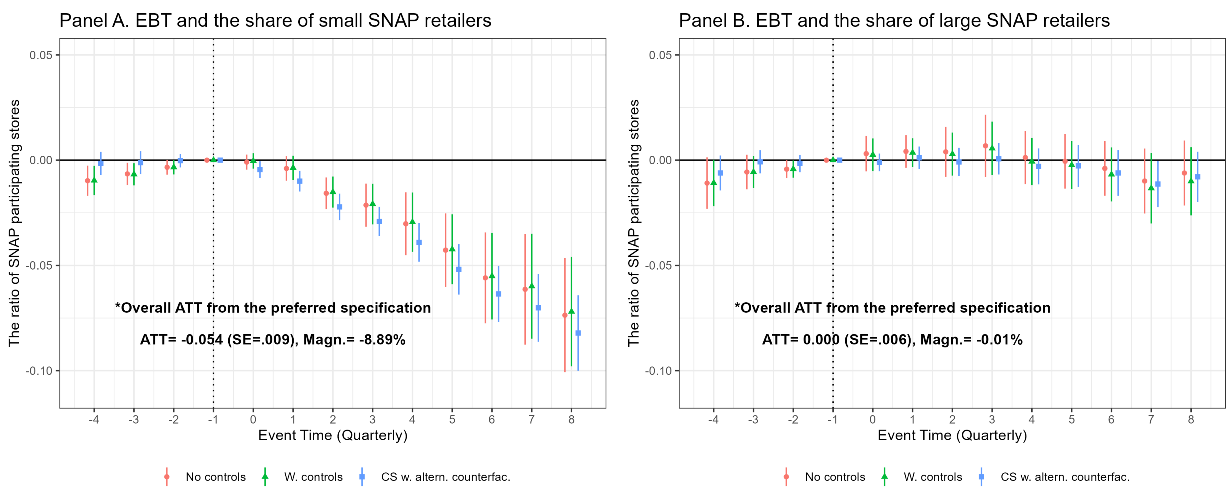

Figure 5: Impacts of EBT on the ratio of SNAP retailers to total retailers

Note: Panel C and D have the same y-axis range as Panel A and B of Figure 4, respectively, for an easy comparison.

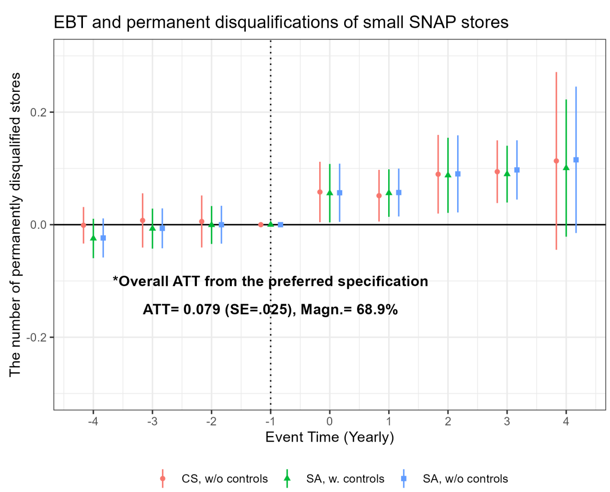

Figure 6: Impacts of EBT on Permanent Disqualification of SNAP Stores

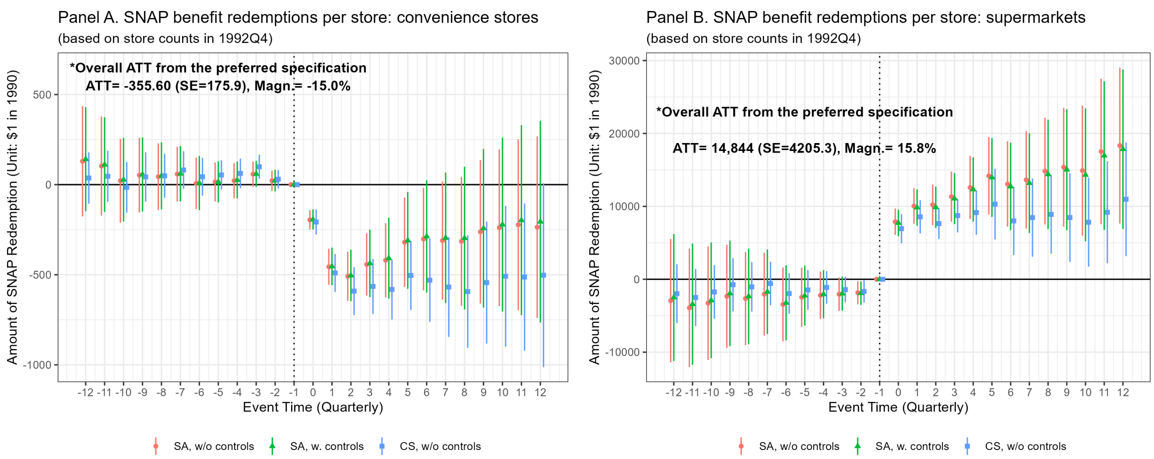

Figure 7: Impacts of EBT on the SNAP benefit redemption amount per store: convenience stores and supermarkets

Table 2: Impacts of EBT on the number of SNAP retailers, by store type

	Number of SNAP stores			Share of SNAP stores		
	Small	Large	Total	Small	Large	Total
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	-3.774***	0.111*	-4.550***	-0.054***	0.000	-0.031***
	(0.477)	(0.062)	(0.617)	(0.009)	(0.006)	(0.006)
Pre-EBT mean	28.87	9.16	47.46	0.61	0.9	0.62
Magn. compared to mean	-13.07	1.21	-9.59	-8.89	-0.01	-5.06
Obs	2848*50	2848*50	2848*50	1437*18	1437*18	1437*18

Note: The table contains DD estimates of the average treatment effects of EBT on the number and ratio of SNAP retailers by store types from Equations 1 and 3. Each column reports the estimation result from a separate regression. "Small" includes convenience stores, liquor stores, and small grocery stores, and "Large" includes supermarket, superstores, and large grocery stores. Unit of observations are county x year/quarter cells. The models with the number of SNAP stores as an outcome variable includes 2,848 counties from the first quarter of 1990 to the second quarter of 2002, while the models with the ratio of SNAP stores as an outcome variable includes 1,437 counties from the first quarter of 1998 to the second quarter of 2002. The models in Columns (1), (2), and (3) weight every county X time cell equally and their pre-EBT means are based on the average of the 1990Q1 outcomes. Columns (4), (5), and (6) weight each county by the population size and their pre-EBT means are based on the weighted average of the 1990Q1 outcomes, with 1998 county population being the weight. Standard errors in parentheses are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Impacts of EBT on the number of small SNAP stores, across regions and store ownership

	Regional characteristics						Store ownership	
	Poverty rate		Urbanicity		Accessibility to large SNAP stores			
	High	Low	Urban	Rural	Better	Worse	Indep.	Chain
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	-2.354***	-5.810***	-5.861***	-0.203	-3.917***	-4.115***	-1.658***	-2.116***
	(0.601)	(0.725)	(1.327)	(0.283)	(0.433)	(0.773)	(0.252)	(0.273)
Pre-EBT mean	29.32	28.38	47.9	12.45	20.7	37.05	13.22	15.64
Magn. compared to mean	-8.03	-20.47	-12.24	-1.63	-18.92	-11.11	-12.54	-13.53
Obs	1470*50	1378*50	1319*50	1529*50	1423*50	1423*50	2848*50	2848*50

Note: The table contains DD estimates of the average treatment effects of EBT on the number and ratio of SNAP retailers by store types from Equations 1 and 3. Each column reports the estimation result from a separate regression. The models in Panel B weight each county by the population size and their pre-EBT means are based on the weighted average of the 1990Q1 outcomes, with 1990 county population being the weight. Standard errors in parentheses are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of EBT on the number of small SNAP stores: by adoption timing and for California

	Early adopters (1990Q1-1996Q1)	Mid adopters (1996Q2-1999Q4)	Late adopters (2000Q1-2002Q3)	Case of California (2002Q3-2004Q2)
	(1)	(2)	(3)	(4)
DD	-4.59	-3.61	-2.14	4.09
	(0.95)	(0.51)	(0.28)	(8.06)
Pre-EBT mean	34.76	30.1	22.48	103.45
Magn. compared to mean	-13.2	-12.01	-9.54	3.95
Obs	2848	2848	2848	56
(Aggregation)	(363)	(1801)	(684)	.

Note: Standard errors in parentheses are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Impacts of EBT on the SNAP participation rate and issuance amount

	(1) All	(2) Few large stores	(3) Many large stores	(4) Few large SNAP stores	(5) Many large SNAP stores	(6) High pov	(7) Low pov
Panel A. Outcome: SNAP participation rate							
ATT	0.008***	0.006***	0.016***	0.009***	0.010***	0.012***	0.008***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.000)	(0.003)	(0.002)
Pre-EBT mean	0.08	0.08	0.1	0.11	0.05	0.13	0.05
Magn. compared to mean	10.09	7.85	16.5	8.06	19.09	9.35	16.26
Obs	2243*25	1095*25	1099*25	1095*25	1097*25	1228*25	1015*25
Panel B. Outcome: Monthly benefit issued (unit: $1,000, 1990 dollars)							
ATT	66.104*	74.868	37.727***	4.563	189.941**	-2.409	393.332**
	(34.730)	(61.628)	(3.613)	(42.108)	(96.332)	(26.132)	(171.945)
Pre-EBT mean	374.1	600.07	148.95	554.94	194.02	414.82	324.76
Magn. compared to mean	17.67	12.48	25.33	0.82	97.9	-0.58	121.11
Obs	2243*25	1095*25	1097*25	1095*25	1097*25	1228*25	1015*25

Note: The table contains DD estimates of the effects of EBT on the SNAP participation rate and issuance amount, calculated from the Sun and Abraham method based on the equation 1 and 3. Each column reports the estimation result from a separate regression. Unit of observations are county x bi-annual cells. The sample includes 2,243 counties from the first half of 1990 to the second half of 2002 that has nonmissing information on the SNAP participation and issuance amount. The pre-EBT mean and median are based on the 1990 outcomes. Standard errors in parentheses are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

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Appendices

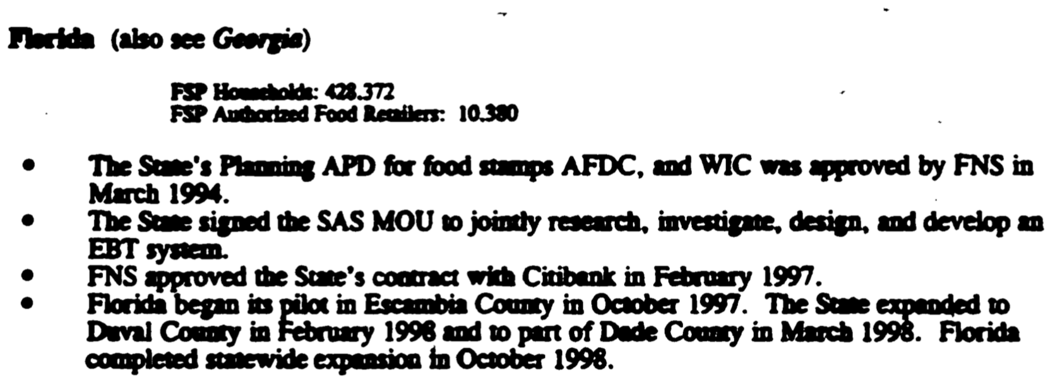
A Paper Food Stamp Coupons and EBT Cards

Figure A.1: Paper Food Stamp Coupons

Figure A.2: EBT Cards

B Sources of planned EBT rollout timings

Information on pilot counties, pilot timings, and the period of the statewide rollout from EBT Project Status Report of 1999, 2000, 2002, 2004.



Florida (also see *Georgia*)

FSP Households: 428.372
FSP Authorized Food Retailers: 10.380

- **The State's Planning APD for food stamps AFDC, and WIC was approved by FNS in March 1994.**
- **The State signed the SAS MOU to jointly research, investigate, design, and develop an EBT system.**
- **FNS approved the State's contract with Citibank in February 1997.**
- **Florida began its pilot in Escambia County in October 1997. The State expanded to Duval County in February 1998 and to part of Dade County in March 1998. Florida completed statewide expansion in October 1998.**

Figure B.1: Example of EBT Project Status Report (Florida)

Appendix Table B1 describes various sources I rely on to gather the EBT rollout timing at the county level.

Table B1: Sources of data on EBT rollout timings

State	Sources	FNS availability
Alabama	Alabama DHR website	Y
Arizona	Manager's memory	Y
Arkansas		Y
California	Archived state website	Y
Colorado		Y
Connecticut		
Delaware	Stegman et al. (2003)	
District of Columbia		Y
Florida		Y (some missing)
Georgia		Y
Hawaii		Y (some missing)
Idaho	Manager's memory	
Illinois		
Indiana	Archived state website	
Iowa	Info from a state EBT manager	Y
Kansas		Y
Kentucky		Y
Louisiana		Y
Maine	Maine newsletter	
Maryland	MD Evaluation Report	Y
Massachusetts	MA Field Operations Memo	
Michigan		Y

Minnesota		Y
Mississippi	MI DHS report	Y
Missouri	Wright et al. (2018)	Y
Montana		
Nebraska	Info from a state EBT manager	
Nevada		Y
New Hampshire		
New Jersey	Info from a state EBT manager	Y
New Mexico		Y
New York	Report from OTDA, Order from NY DTA, Delaware county website	
North Carolina	Henderson county website	Y
North Dakota		Y (some missing)
Ohio	Report: Evaluation of the Expanded Off-Line EBT System in Ohio	Y
Oklahoma		Y
Oregon		
Pennsylvania		Y
Rhode Island		
South Carolina		Y
South Dakota		Y (some missing)
Tennessee		Y
Texas		Y
Utah		Y (some missing)
Vermont		
Virginia	Virginia DSS release	Y (some missing)
Washington	ESA Program Briefing Book	Y (some missing)
West Virginia	Archived state website	
Wisconsin	Archived state website	Y

Examples of the sources: Archived state websites of California, Indiana, and West Virginia.

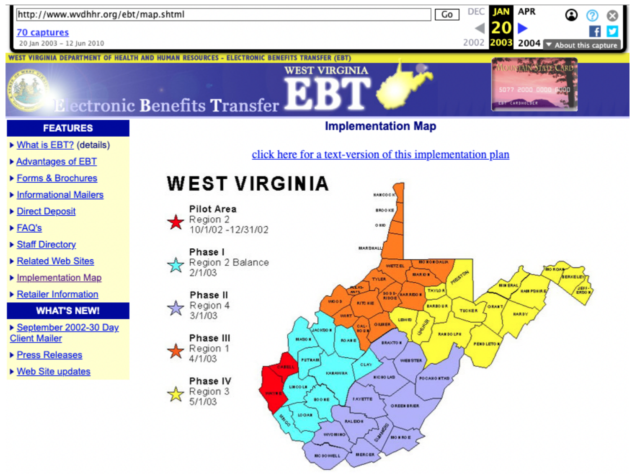


Figure B.2: Example of an archived website (West Virginia)

C The EBT rollout patterns across counties

Appendix Figure C.1 illustrates the EBT rollout across counties separately by three periods: (1) before the 1996 welfare reform law that mandated the EBT adoption, (2) between 1997 and the deadline of the third quarter of 2002 (October 1st, 2002), and (3) after the deadline. Note that counties in Panel C were “waiver counties" that received a waiver and adopted the EBT after the deadline, including all or a part of California, Iowa, Delaware, Iowa, Maine, and West Virginia counties.

In addition, Appendix Figure C.2 illustrates the cumulative distribution of the EBT implementation by state. The statewide rollout patterns can be categorized into three groups: (1) states that implement a pilot on a small scale and soon fully expand the EBT, such as Arkansas, Louisiana, and Montana, (2) states that implement a pilot on a bigger scale and fully expand EBT, such as Massachusetts, Rhode Island, and South Dakota, (3) states that gradually implement the EBT at a multiple phase, such as California, Florida, and New Jersey.

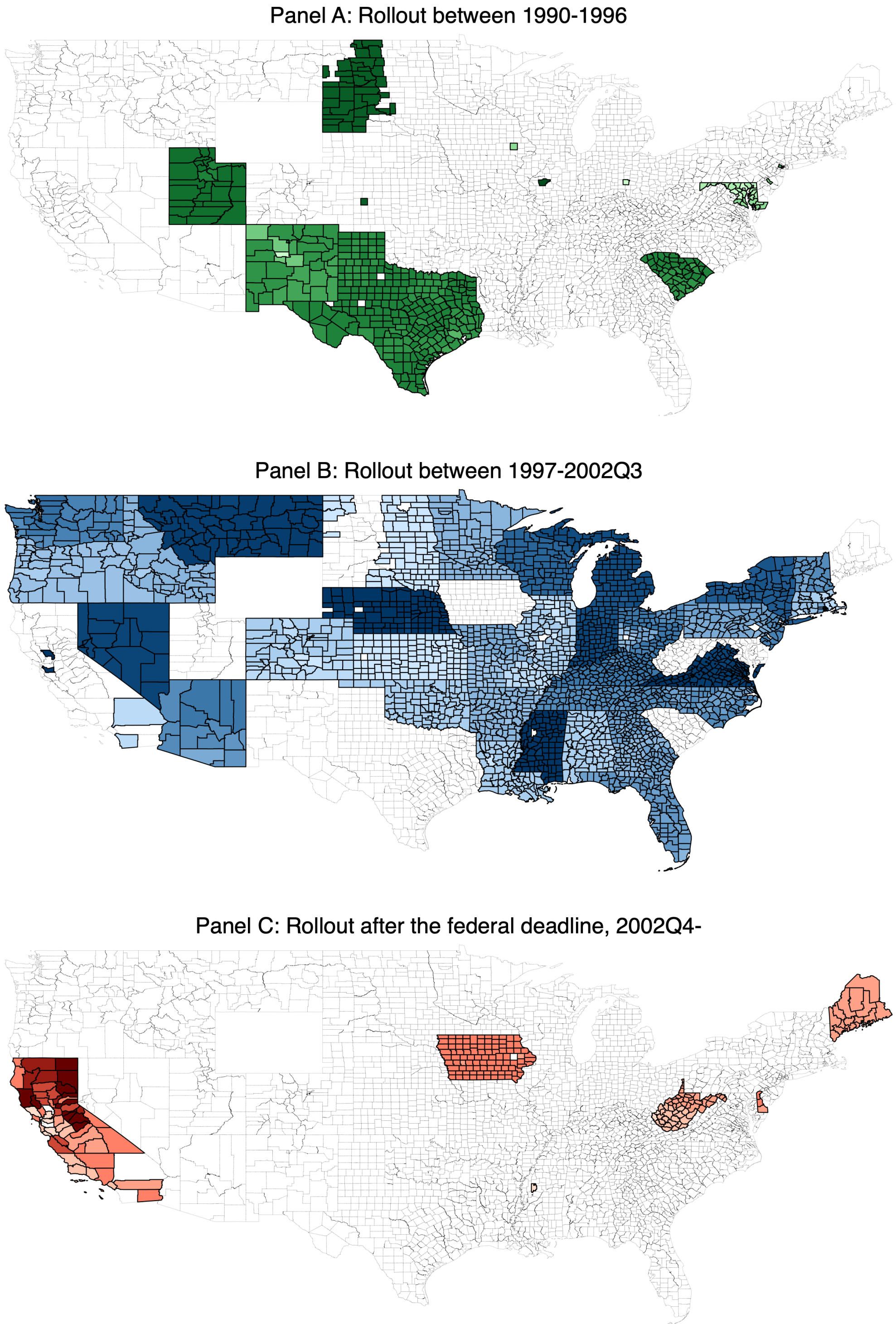


Figure C.1: Maps of the EBT rollout across counties, 1990-2004

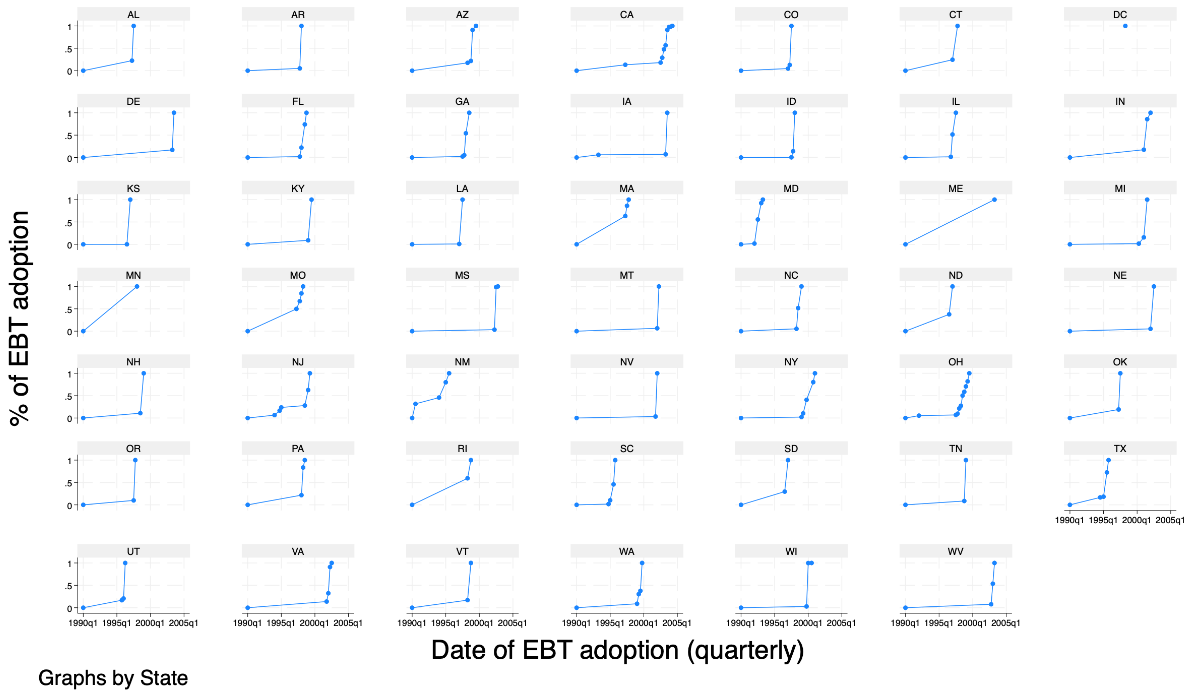


Figure C.2: Empirical CDF of EBT implementation across counties, by state

D Bacon Decomposition of the TWFE Parameter

Conventionally, researchers have employed the two-way fixed effect (TWFE) model as in Equation 4 to estimate the average treatment effect of the treated (ATT):

$$y_{ct} = \alpha + \beta EBT_{ct} + \lambda_c + \eta_t + \epsilon_{ct} \tag{4}$$

where c is a county, t is a year/quarter, EBT_{ct} is an indicator that assigns 1 if EBT was implemented in a county c at time t and zero otherwise, λ_c is a county fixed effect, η_t is a time fixed effect, and ϵ_{ct} is the error term. The parameter of interest is β, a Difference-in-Differences (DD) parameter identifying a causal impact under a parallel trend assumption with homogeneous effects over time as well as across timing cohorts if more than one treatment timing exists.

In his influential paper, Goodman-Bacon (2021) provides an anatomy of the TWFE estimator ($\hat{\beta}$ from Equation 4) in a staggered adoption setting. He shows that it is a weighted average of three types of 2x2 DD comparisons: (1) treated vs. never-treated, (2) treated vs. not-yet-treated (but will eventually be treated later), and (3) treated vs. already treated at some earlier time. When the never-treated group does not exist in my case, the TWFE estimator becomes the weighted average of 2x2 DD estimators of types (2) and (3). The third comparison type is commonly referred to as a "bad" or "forbidden" comparison, as it is the main source of the problem that causes a bias or a "negative weights problem" if treatment effects are heterogeneous (De Chaisemartin and d'Haultfoeuille, 2020). Intuitively, if a dynamic effect exists and already-treated units are still experiencing treatment effects, then comparing them could lead to a misleading result even in terms of the *sign* of the effect. In addition, weights attached to these 2x2 DD estimators are determined somewhat arbitrarily by the model, making it hard to interpret the coefficient. Appendix Section D contains a diagnostic test for gauging the extent to which bad comparisons may be problematic in my setting.

I test the extent to which the forbidden comparisons impact the coefficient β from the equation 4 by utilizing a diagnostic test proposed by Goodman-Bacon (2021). The test decomposes β from the equation 4 into a full set of 2x2 DD estimators and their weights and plots all pairs of the 2x2 DD estimate and its weight to visually investigate the impact of the bad comparisons. As my setting contains 44 treatment timing cohorts without any never-treated units, the parameter β is comprised of 1,892 (44 treatment timing cohorts multiplied by 43 others) possible 2x2 DD estimators. I use the panel of 3,075 counties covering 1990Q1-2004Q4 to estimate and decompose the TWFE coefficient.

Panel A and B of Appendix Figure D.1 contain the results of the Bacon decomposition. Panel A is based on the regression of EBT on the number of small SNAP retailers, while Panel B is based on the regression on the number of large SNAP retailers.

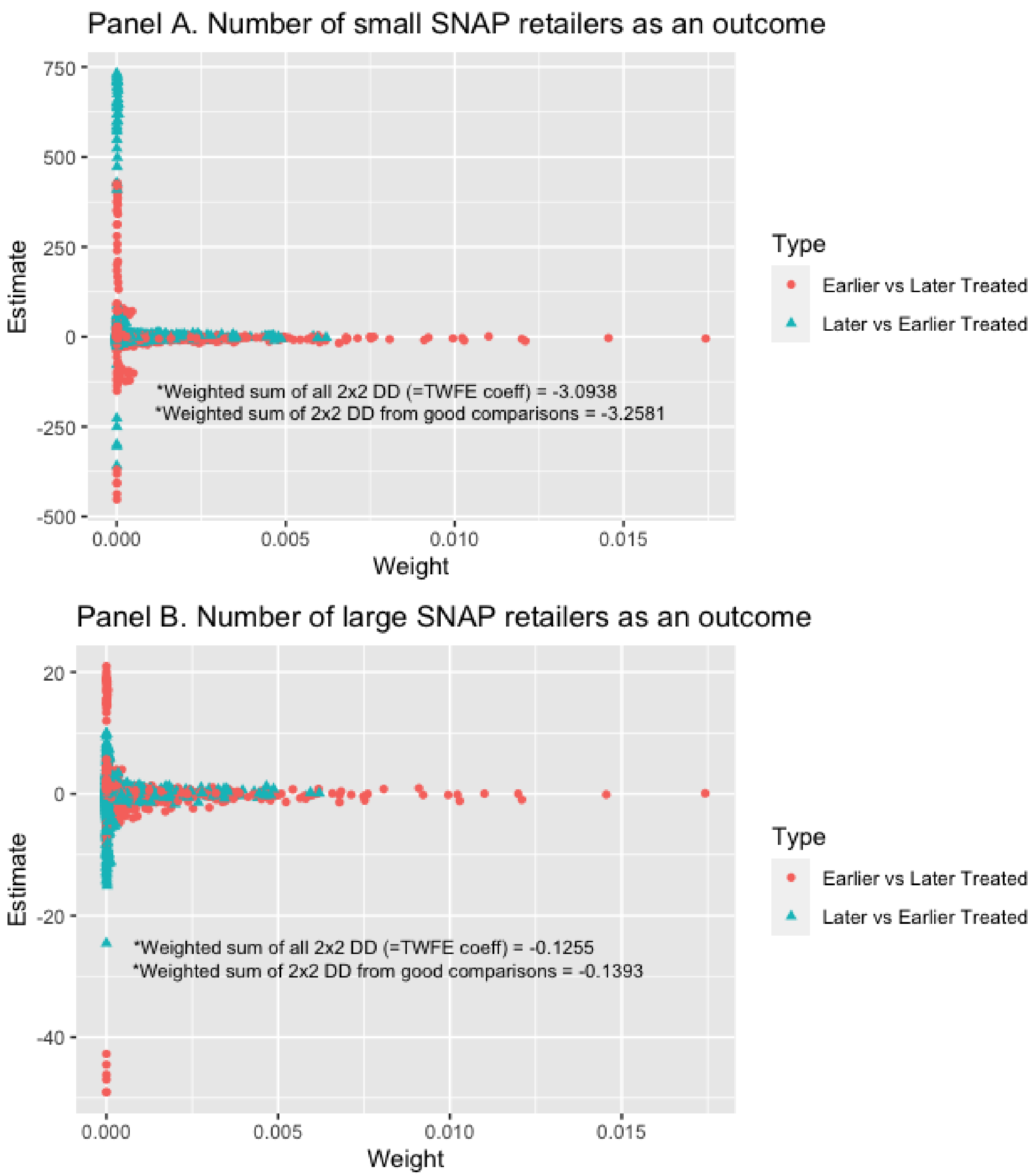


Figure D.1: Bacon decomposition

The two plots indicate that the sign and absolute value of the TWFE estimate are not significantly impacted by the bad comparisons. For both plots, there are no particularly large weights assigned to these forbidden comparisons. The TWFE estimate for the number of small SNAP retailers is -3.0938, and the weighted average of 2x2 DD from good comparisons is -3.2581, showing the same sign and roughly similar magnitudes (baseline mean of the outcome variable of pre-EBT period=29.74). Similarly, the TWFE estimate and the weighted average of 2x2 DD from good comparison for large SNAP retailers also exhibit the same sign and similar magnitude (baseline mean of the outcome variable of pre-EBT period=9.578).

Although the bias does not seem large, I choose not to use the TWFE estimator for my analysis for two reasons. First, despite the small bias, I would like to exclude possible bad comparisons from my specification to get more accurate point estimates as well as assign weights more carefully for a clearer interpretation of the overall ATT. Second, the event study analysis is necessary because I expect the effect of EBT reform to evolve dynamically. For example, store owners might learn more about the benefits and limitations of EBT over time. However, as shown in Sun and Abraham (2021), the TWFE-ES analysis may not reveal the true pre-trends and dynamic treatment effect under the staggered adoption setting unless strong assumptions hold.

E Quasi-randomness of the EBT Rollout Variation

In this section, I investigate the extent of the quasi-randomness of the EBT rollout variation. Specifically, I examine whether some pre-EBT county characteristics are associated with the EBT rollout timing, including macroeconomic conditions, the size of the SNAP program, variables related to the 1996 welfare reform such as AFDC waiver and TANF timing, county population size, demographic compositions, and urbanicity, actually predict the exact rollout dates.

Although it is unlikely that the within-state variation was determined completely randomly,[52] institutional backgrounds on how the EBT schedule was set suggest some randomness in the state's implementation schedule. The EBT adoption plan was frequently delayed due to unforeseen issues such as no bidder, failed negotiation, and legal disputes, adding extra randomness to the variation. For instance, Iowa's bid process was delayed substantially as it first had to address State legislation requiring retailers to have their own EBT equipment and pay a transaction fee. Then, since there was no bidder, the state had to issue another RFP. The contract was finalized in July 2002 and approved by FNS in August 2002, 4.5 years after the state released its first RFP. The state received a waiver and implemented an EBT rollout between June 2003 and October 2003. Moreover, as the implementation was forced by the federal government during a specific time span rather than left entirely voluntary to a state's own will, it is less concerning that there might be a systematic difference between adopters and never-adopters.

To formally test the extent to which pre-treatment county characteristics predict the treatment schedule across- and within-state, I follow a method of Hoynes and Schanzenbach (2009). Specifically, I estimate the following equation:

$$y_c = \alpha + X_c^{preEBT} B + u_c \tag{5}$$

where y_c is a year/quarter of the EBT adoption, with the first quarter of 1990 indexed as 1 and subsequent quarters incrementing by 1. $X_c^{preEBT} B$ is a vector comprising various county characteristics in the first quarter of 1990 or before, prior to the EBT rollout within my sample. Included are a log of population in Q1/1990, demographic characteristics such as percentage of population Black, Hispanic, young (age 0-5), and old (age 65 or more) in Q1/1990, poverty rate in 1989, employment to population ratio, urban/rural status in 1983, timings of the AFDC waiver and TANF implementation,[53] and a log of SNAP caseloads in July 1989. These variables assess the predicting power on the EBT adoption timing of a county size in terms of population, demographic composition of those likely to be eligible for SNAP, a proxy for the share of potential SNAP-eligible population in poverty, county-specific macroeconomic conditions, urbanicity, contemporaneous changes in other welfare programs, and the size of the SNAP program. I also include an indicator for 293 counties that do not have information on the number of SNAP households from the FNS388 data. Standard errors are clustered at the state level throughout all specifications. I conduct this analysis using my main sample of 2,848 counties.

[52]For instance, states usually chose pilot counties based on the size or EBT readiness. For instance, Missouri picked a large place first (Wright et al., 2017), while Arizona chose small counties (author's phone call with a state EBT manager).

[53]The AFDC waiver variable contains the AFDC waiver adoption timing conditional on doing an AFDC waiver. If a state did not implement the waiver, it is coded as zero. Similarly, TANF variable contains the TANF implementation timing of a state. Once TANF started, I code the AFDC variable as zero to separately measure the TANF period and the AFDC waiver period.

Table E2: Pre-EBT County Characteristics And EBT Adoption Timing

Size of the SNAP program and county economy do not predict the adoption timing

	(1)	(2)	(3)	(4)	(5)	(6)
Pre-EBT county characteristics						
Need for the SNAP program						
% in poverty in 1989	-0.044	-0.036	-0.005	0.214	0.218	-0.002
	(0.126)	(0.124)	(0.011)	(0.173)	(0.173)	(0.025)
Size of the SNAP program						
Ln(SNAP caseloads in July 1989)	1.397	1.464	0.066	-0.675	-0.065	-0.180
	(1.062)	(1.058)	(0.134)	(0.808)	(0.946)	(0.391)
Dummy for missing FNS388 data	13.663	14.257	0.001	-6.221	-0.124	-4.422
	(10.095)	(10.054)	(0.691)	(8.595)	(9.802)	(3.928)
Economic conditions						
Empl.to.pop. ratio in 1990Q1	2.065	2.282	-0.028	-0.189	0.341	-0.138
	(1.683)	(1.661)	(0.096)	(2.119)	(2.344)	(0.596)
1996 Welfare reform						
AFDC waiver dummy	-14.422	-11.711		23.223	31.462	
	(49.823)	(50.458)		(46.574)	(47.004)	
Timing of AFDC waiver	0.127	0.108		-0.158	-0.214	
	(0.355)	(0.360)		(0.331)	(0.333)	
Timing of TANF impl.	0.829	0.809		1.237	1.273	
	(0.946)	(0.957)		(0.910)	(1.056)	
County size						
Ln(pop in 1990Q1)	-1.284	-1.233	-0.236*	0.402	-0.081	-0.284
	(1.018)	(1.009)	(0.132)	(1.046)	(1.055)	(0.563)
Demographic characteristics						
% Black in 1990Q1	-0.017	-0.017	-0.016**	-0.053	-0.053	-0.016
	(0.072)	(0.073)	(0.007)	(0.085)	(0.086)	(0.030)
% Hispanic in 1990Q1	-0.243***	-0.243***	-0.006	-0.227***	-0.206***	-0.029
	(0.044)	(0.044)	(0.007)	(0.069)	(0.063)	(0.023)
% young (0-5) in 1990Q1	-1.037*	-1.039*	-0.011	-1.002*	-0.787	-0.040
	(0.597)	(0.593)	(0.038)	(0.528)	(0.527)	(0.271)
% old (65+) in 1990Q1	-0.392**	-0.396**	0.006	-0.065	-0.059	0.019
	(0.192)	(0.189)	(0.008)	(0.155)	(0.150)	(0.055)
Urbanicity						
Urban area dummy in 1983	0.188	0.211	-0.005	-0.112	-0.171	-0.102
	(0.242)	(0.251)	(0.016)	(0.356)	(0.382)	(0.122)
F-stat	6.80	7.69	.	2.80	1.97	.
Prob > F	0.000	0.000	.	0.005	0.05	.
Excluding pilots		Y			Y	
FE: State			Y			Y
Weighted: 1990 pop				Y	Y	Y
N	2848	2768	2848	2848	2768	2848
Adj.R2	0.20	0.20	0.98	0.16	0.14	0.89

Note: The table shows the relationship between the county characteristics in or before 1990Q1 and the EBT rollout timing in that county. Each column contains a separate regression. The dependent variable is the year/quarter of the EBT adoption, with the first quarter of 1990 being indexed as 1 and subsequent quarters incrementing by 1. All models exclude information of Alaska, Hawaii, Wyoming, and three very early pilot counties (Baltimore City of Maryland, Berks County of Pennsylvania, and Ramsey County of Minnesota). The model without pilot counties in Column 2 excludes 86 counties. Regression models in Columns 4-6 are weighted by the 1990 county population. Standard errors in parentheses are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

The first three columns of Appendix Table E2 contain results from a separate OLS regression, where each county is weighted equally to be consistent with my main specification. According to the result in Column (1), the F-statistic indicates that the joint F-tests reject the null hypothesis that there is no predictive power in the set of covariates on the right-hand side of the equation. This result indicates that some pre-EBT county characteristics may have affected the EBT rollout schedule. Looking at each coefficient, I confirm that the size of SNAP recipients and the employment-to-population ratio, which I suspect are possible important confounders, have little predicting power on the EBT adoption timing of each county. These weak associations suggest that EBT implementations were not scheduled based on the need for SNAP. I also do not find any association between the EBT adoption timing and the variables related to the 1996 welfare reform, such as the AFDC waiver dummy, the timing of AFDC waiver, and the timing of TANF implementation. County demographic characteristics such as shares of Hispanic, young, and old population groups show some statistically significant association with the EBT adoption timing. The negative coefficients suggest that counties with a higher share of these demographic groups, who might potentially be SNAP-eligible, tend to adopt the EBT system earlier. However, considering the base level of each population's share, the magnitudes of the associations are not large. Given the 25th and 75th percentiles of Hispanic population share (0.37 and 2.37), moving from a county at the 75th percentile to one at the 25th percentile is associated with a 0.5-quarter delay in EBT adoption (=-0.243*(2.37-0.37)) or a three-month during the 14-year period from 1990 to 2004. Similarly, moving from a county at the 75th percentile to a county at the 25th percentile of the share of young (from 9.35 to 7.91) and old populations (from 17.26 to 12.05), respectively, is associated with a 1.5-quarter and 2-quarter delay in the EBT implementation. Column (2) contains the result from the specification without pilot counties to exclude counties whose implementation timings are clearly not randomly selected. The result does not change much, regardless of the inclusion of pilot counties. The adjusted R2 indicates that much of the variation across and within the state remains unexplained, implying some randomness in the EBT rollout schedule.

Column (3) provides the result from the specification that adds state dummies to examine EBT rollout patterns within each state. The result suggests little relationship with SNAP size or employment status. Some associations are found with the county population size and share of the Black population. The magnitudes are small: moving from a county at the 75th percentile to a county at the 25th percentile of the natural log of county population (from 10.920 to 9.243) and the share of Black population (from 11.5663 to 0.1458), respectively, is associated with a 0.4-quarter and 0.2-quarter delay in the EBT implementation. The R2 becomes much larger than the previous two models without state fixed effects, indicating that much of the variation comes from across-state variation.

The last three columns provide results from weighted least square (WLS) regressions where each county is weighted by its 1990 population. I provide results from the WLS regressions to be consistent with some specifications with population weights in the main manuscript. In addition, I compare the results with and without weights in case more populous counties have better quality of data and thus better information. The results are generally similar.

From this analysis, I conclude that pre-EBT county characteristics and EBT adoption timing of each county exhibit at most a weak association with small magnitudes. However, as some correlations are not negligible, in the main analysis, I present the results both with and without control variables to check if the inclusion of covariates substantially affects the result. I confirm that controlling the time-varying county characteristics, including county demographic characteristics, macroeconomic trends, and the 1996 welfare reform status, together with the county and year-quarter fixed effects, does not affect the main result.

F Correlation between the EBT Adoption Timing and County Employment and Wages

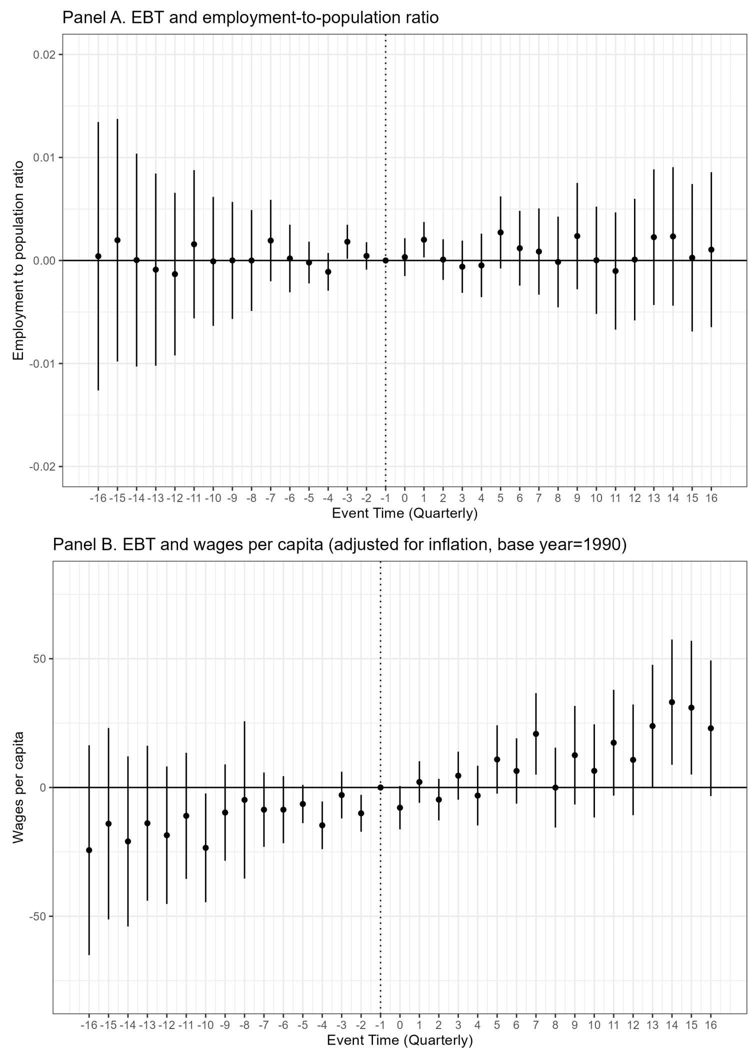


Figure F.1: Event study graph: EBT and Economic Indicators

Table F3: Correlation between the EBT adoption timing and county economic conditions

Outcome:	Employment-to-population ratio (1)	Quarterly wage per capita (2)
ATT	0.002	16.405*
	(0.016)	(52.764)
County FE	Y	Y
Time FE	Y	Y
Mean	0.53	1574.9
N	142400	142400
Adj.R2	0.95	0.95

Note: The quarterly wage from QCEW is divided by the county population and adjusted for inflation based on 1990 dollars. Standard errors in parentheses are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

G Analysis on specialty stores and other stores

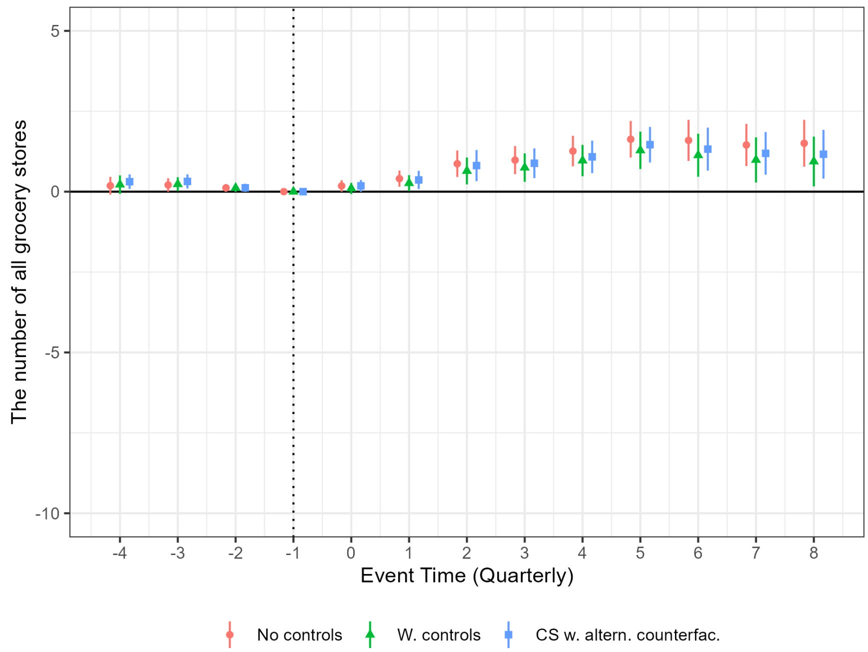
Table G4: Impacts of EBT on the number and ratio of SNAP retailers by store type: Specialty and other stores

	Number of SNAP stores		Share of SNAP stores	
	Specialty (1)	Others (2)	Specialty (3)	Others (4)
ATT	-1.074***	-0.041*	0.187**	0.003
	(0.170)	(0.024)	(0.078)	(0.006)
Pre-EBT mean	5.62	0.73	3.8	0.2
Magn. compared to mean	-19.11	-5.56	4.93	1.46
Obs	2848*50	1437*18	2848*50	1437*18

Note: The table contains DD estimates of the average treatment effects of EBT on the number and ratio of SNAP retailers by store types from Equations 1 and 3. Each column reports the estimation result from a separate regression. “Specialty" includes meat and poultry specialties, seafood specialties, fruit and vegetable specialties, and bakeries. “Other" includes medium-sized grocery stores, dollar stores, combination grocery stores, and food-buying co-ops. Unit of observations are county x year/quarter cells. The models with the number of SNAP stores as an outcome variable include 2,848 counties from the first quarter of 1990 to the second quarter of 2002, while the models with the ratio of SNAP stores as an outcome variable include 1,437 counties from the first quarter of 1998 to the second quarter of 2002. The models in Columns (1) and (2) weight every county X time cell equally and their pre-EBT means are based on the average of the 1990Q1 outcomes. Columns (3) and (4) weight each county by the population size and their pre-EBT means are based on the weighted average of the 1990Q1 outcomes, with 1998 county population being the weight. Standard errors in parentheses are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

H Analysis on all stores

Panel A. EBT and the number of small retailers



Panel B. EBT and the number of large retailers

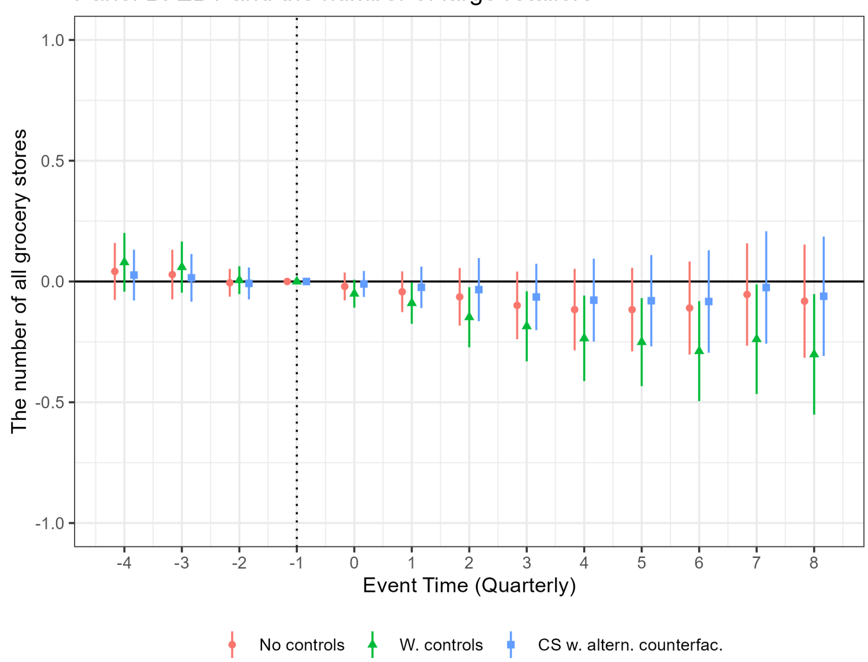


Figure H.1: Impacts of EBT on the number of retailers, by store size

Table H5: Impacts of EBT on the number of overall grocery retailers

	Number of overall grocery retailers		
	Small	Large	Total
	(1)	(2)	(3)
ATT	1.583***	-0.114	2.262***
	(0.273)	(0.080)	(0.437)
Pre-EBT mean	53.98	12.42	102.58
Magn. compared to mean	2.93	-0.92	2.21
Obs	1437*18	1437*18	1437*18

Note: The table contains DD estimates of the average treatment effects of EBT on the number of grocery retailers by store types from Equations 1 and 3. Each column reports the estimation result from a separate regression. I define small and large retailers based on NAICS code."Small" includes convenience stores (NAICS 445120), gas stations with convenience stores (NAICS 447110), beer, wine, and liquor stores (NAICS 445310), and supermarkets and other groceries with 0-4 paid employees (NAICS 445110). "Large" includes supermarkets and other groceries with 20 paid employees or more (NAICS 445110) and warehouse clubs and supercenters (NAICS 452910). Unit of observations are county x year/quarter cells. All models include 1,437 counties from the first quarter of 1998 to the second quarter of 2002. Standard errors in parentheses are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1.

I Trend of Permanent Disqualifications

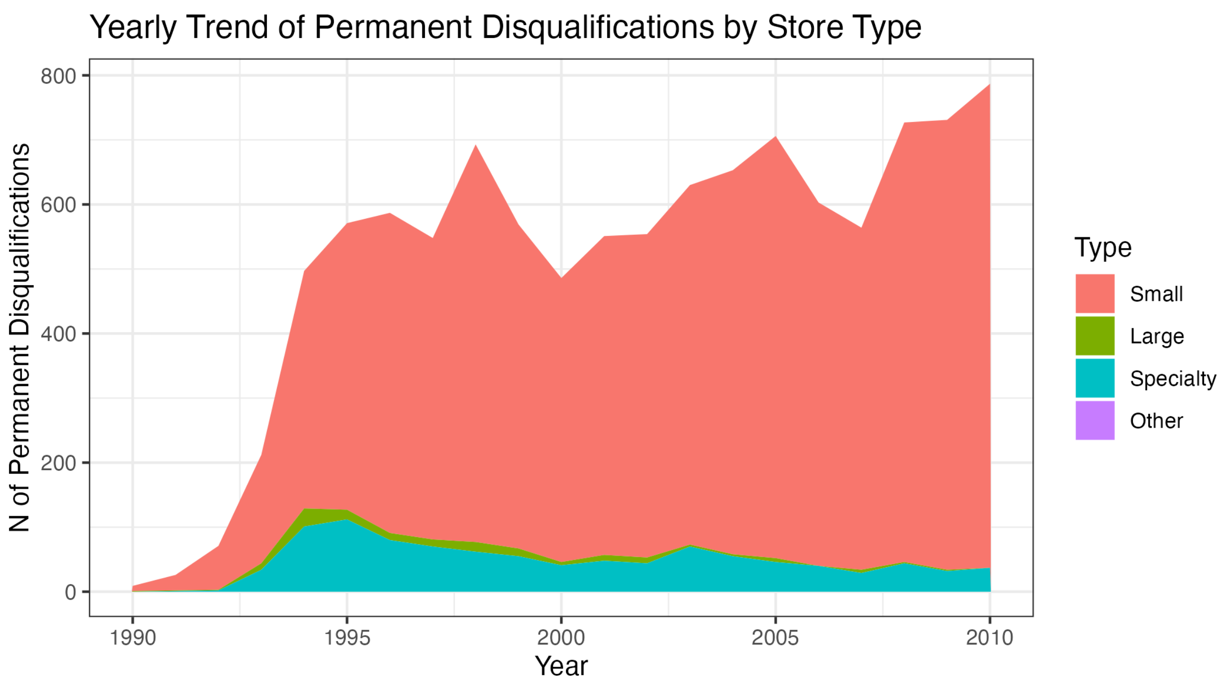


Figure I.1: National Trend of Permanent Disqualification of SNAP Stores

References in Appendix