

Behavioral Food Subsidies*

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We conduct a pre-registered field experiment with low-income grocery shoppers to study how targeted behavioral interventions can improve the effectiveness of healthy food subsidies. Our unique design enables us to elicit choices and deliver subsidies both *before* and *at* the point of purchase. We examine the effects of two non-restrictive changes to the choice environment: giving shoppers a choice over the type of subsidy they receive, and introducing a waiting period before the shopping trip to prompt deliberation about the food purchase decision. Combined, our interventions substantially improve the effectiveness of subsidies, increasing healthy purchases by 61% relative to a choice-less subsidy restricted to healthy food, and 199% relative to an un-subsidized control group. We discuss how these low-cost, scalable interventions can help mitigate nutritional inequality.

KEYWORDS: nutrition, subsidy, agency, deliberation, waiting periods, field experiment

JEL Classifications: I12, D91, D12, C93

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1 Introduction

A growing literature has documented extensive socioeconomic inequality both in developing (Alvaredo, Assouad, and Piketty 2019) and developed countries such as the U.S. (Piketty and Saez 2014). Inequality in nutrition has received particular attention from both scientists and policy makers because dietary differences are viewed as both a downstream consequence of economic inequality (Drewnowski and Specter 2004), and as a contributor to its persistence (Wolf 2012). Poor diets are now 25% more prevalent amongst low-income individuals than higher-income individuals (Rehm, Peñalvo, Afshin, and Mozaffarian 2016). Recent research has shown that supply-side interventions aiming to increase access to healthy food have been limited in their capacity to close this gap (Allcott, Diamond, Dubé, Handbury, Rahkovksy, and Schnell 2019), suggesting that differences in demand could be driving dietary inequality. At the same time, while government programs such as the Supplemental Nutrition Assistance Program (SNAP) do generate beneficial long-run health consequences (Hoynes, Schanzenbach, and Almond 2016), their focus has largely been on food security rather than nutritional quality. Diets of SNAP enrollees are persistently poor—fruit and vegetable consumption among program participants is approximately half of the recommended intake, while higher-income non-participants consume over two-thirds of the recommendation (Cole and Fox 2008).¹

Recent work has examined the effectiveness of offering direct subsidies on purchases of healthy food (Bartlett, Klerman, Wilde, Olsho, Logan, Blocklin, Beauregard, and Enver 2014; Harnack, Oakes, Elbel, Beatty, Rydell, and French 2016). Restricting subsidies to a limited set of foods such as fruits and vegetables is a natural extension of recent proposals to improve nutrition through more paternalistic changes in food assistance (Schwartz 2017).² Studies on such *restricted* subsidies have found positive

¹Data are from the 1999-2004 National Health and Nutrition Examination Survey.

²This is motivated by the relatively low nutritional quality of SNAP purchases. For example, soda takes up the largest share and approximately 20% of SNAP spending goes towards sweets and sugary drinks. Efforts to place restrictions on SNAP purchases include recent proposals to offer in-kind SNAP benefits in the form of USDA Foods Packages (<https://www.npr.org/sections/thesalt/>

effects. For example, a USDA experiment found that restricted subsidies significantly increased purchases and consumption of fruits and vegetables (Bartlett et al. 2014).

A restricted healthy subsidy features limits on what is and what is not subsidized.³ Research suggests that paternalistic restrictions to food policy may backfire, for example, through a “reactance effect” (Brehm 1966; Brehm and Brehm 2013). Moreover, myopia can lead individuals who want to improve their diets to nonetheless consume sanctioned but unhealthy food (Read and van Leeuwen 1998), muting the effectiveness of policies aimed at stimulating healthier food choices. This paper studies how non-restrictive changes to the choice environment can overcome these hurdles and enhance the effect of subsidies on the purchases of more nutritious food.

We conducted a field experiment with low-income grocery shoppers where our outcome of interest is healthy food spending. We partnered with a smartphone-based market-research platform (Field Agent) to obtain repeated access to shoppers across the United States before, during, and after grocery shopping trips. Through the platform, shoppers were able to conduct their regular shopping trips at the time and location they desired, while we induced experimental variation into their budget sets and grocery shopping experience. This unique setting allowed us to elicit subsidy choices from shoppers through their smartphones. Additionally, we are able to push information to shoppers’ smartphones both before shopping trips and at the point of purchase—in the aisle of the grocery store. This allows us to manipulate the timing between subsidy-related information and the actual shopping trip. The Field Agent platform collected pictures of shopping receipts from all shoppers immediately following their shopping trip in order to track actual purchases and calculate subsidy payments.

We find that restricted subsidies are effective in spurring healthy food spending. Shoppers who were randomly assigned to receive a restricted healthy subsidy

2018/02/12/585130274/trump-administration-wants-to-decide-what-food-snap-recipients-will-get) or to restrict SNAP purchases to nutritious foods (<https://www.brookings.edu/testimonies/pros-and-cons-of-restricting-snap-purchases/>).

³Compared to other programs, such restrictions are similar to the spending limitations placed in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) program, while the SNAP program has favored a less restrictive approach.

on fruits and vegetables (“FV” hereafter) spent 123% more on FV purchases than an un-subsidized control group.⁴ This corroborates earlier findings. The focus of our paper is on the impact of costless, behavioral interventions which aim to enhance these subsidies by overcoming psychological factors which may be blunting their effectiveness.

We focus on two such interventions. The first manipulated the subsidy choice environment by endowing shoppers with greater agency over their subsidies. Instead of being restricted to a “healthy” FV subsidy, these shoppers were given the choice between the healthy subsidy and an “unhealthy” baked goods (“BG” hereafter) subsidy. We elicited binding subsidy choices from shoppers through their smartphones at different points in time—either before shopping or during the shopping trip. In the standard economic framework, this non-paternalistic expansion of subsidy choice should increase the average prices of FV relative to BG and result in *decreased* FV spending. However, besides blunting potential “reactance effects,” work in psychology and behavioral economics documents how active choice generates a preference for consistency, which would enhance the effects of the chosen subsidy on spending. In our context, this behavioral mechanism predicts that being able to *choose* a healthy subsidy will prompt more congruent behavior—increasing the purchase of healthy food. The results reveal that the positive behavioral effects of agency are strong enough to counteract the negative price effect of expanded subsidy choice. With 78% of shoppers choosing the healthy subsidy, expanding agency actually causes an overall 21 percent *increase* in FV spending relative to the case where all shoppers were restricted to the healthy subsidy. While the marginal effect is not statistically significant on its own, we can reject any meaningfully-sized negative effects of allowing for subsidy choice.

Our second behavioral intervention introduced a waiting period into the subsidy

⁴It is possible that the subsidies induced shoppers to concentrate their purchases into fewer trips to maximize their subsidy payments. In this case, our estimates of the impact of the subsidies will be inflated. However, this will not affect subsequent estimation of the impact of our behavioral interventions, because all interventions feature subsidies. Specifically, the incentive to concentrate shopping trips will be equally present across interventions. In fact, if we have inflated the estimated impact of a restricted healthy subsidy, then our interventions represent a greater relative improvements in effectiveness.

choice environment. Specifically, we randomly assigned shoppers to complete a pre-shopping task 4-48 hours prior to their next shopping trip. The unique smartphone platform allowed this pre-shopping task to enforce a delay between the arrival of information about the subsidy and the shopping trip on which the subsidy will be used.⁵ Laboratory experiments have demonstrated a casual effect of waiting periods in prompting more deliberative processing, leading to less myopic and more goal-oriented decision-making (Dai and Fishbach 2013; DeJarnette 2018; Imas et al. 2018).⁶ Importantly, this intervention is distinct from traditional healthy-eating interventions in the nutrition and behavioral literatures for two main reasons. First, it provides no new information that could alter the costs and benefits of food purchases. Second, it does nothing to separate grocery choices from consumption; rather maintains the timing of decisions. Nonetheless, we find that the marginal effect of a waiting period is a 29 percent increase in healthy food purchases relative to a subsidy without a waiting period.

Together, our interventions increase healthy food spending by more than 60 percent relative to the restricted healthy subsidy alone. Importantly, this increase comes at no additional marginal cost to the policy maker apart from the subsidy itself. Overall, we find that coupling food subsidies with these behavioral interventions *triples* observed purchases of healthy food, increasing FV spending by 199%, relative to a control group.

The positive impact of agency is consistent with the literature in psychology and behavioral economics on preferences for consistency, where a prior choice (e.g. selecting the healthy subsidy) prompts congruent behavior (e.g. purchasing healthy food). This effect is a common feature of several behavioral frameworks which build on the self-perception model of Bem (1967). These include the information-processing model of Falk and Zimmermann (2018), and the self-signaling model of Benabou and Tirole

⁵As in Imas, Kuhn, and Mironova (2018), we define a waiting period as a delay between information about a prospective choice and the choice itself. This information need not be new—as it is at the outset of the study—as deliberation can still facilitate the use of old information.

⁶Koenig and Schindler (2018) and Luca, Malhotra, and Poliquin (2017) use observational data to explore the effects of waiting periods for gun ownership.

(2004). In our setting, 83% of shoppers report a desire to increase FV consumption and the majority of the sample (78%) chooses the FV subsidy. As a result, a preference for consistency predicts that prompting an active subsidy choice can lead to an increase in FV spending.⁷

Importantly, an active subsidy choice allows shoppers to select a healthy subsidy as a means of sending themselves a *costly* signal of their intended shopping behavior—using their own choices to motivate subsequent behavior. In Section 4.3, we present evidence that shoppers pursue this strategy: 28% of shoppers leave money on the table by choosing the healthy subsidy. We show that this behavior cannot be explained by random errors or confusion; rather, it is consistent with self-signaling (Benabou and Tirole 2004).⁸

Food choice has long represented a canonical example of myopic behavior. However, our findings are conceptually distinct from work on time-inconsistent behavior (O’Donoghue and Rabin 1999), specifically on the use of commitment devices to overcome dynamic inconsistency. Our agency intervention differs from existing commitment devices considered in the literature for multiple reasons. First, most grocery purchases are made in advance of consumption and our intervention does not affect this relative timing. Thus, grocery purchases can be thought of as decisions by the long-run self and already represent a commitment opportunity for sophisticated shoppers. Subsidy choices are either made concurrent to or in advance of the grocery purchase, so they, likewise, represent decisions made by the long-run self. This leaves no scope to overcome misalignment between short-run and long-run preferences.⁹ Second, commitment typically comes with welfare tradeoffs between the short-run and long-run selves. In contrast to the limitations that standard commitment devices place on choice, our in-

⁷If the majority chose the unhealthy subsidy, then these predictions would not hold.

⁸Recent research has also shown that endowing people with greater agency mitigates myopia and encourages more goal-oriented behavior, which should further increase demand for healthy food (Gneezy, Imas, and Jaroszewicz 2020).

⁹To the degree that some grocery-store choices are for immediate consumption, we should see that reflected in the difference between in-store and before-shopping subsidy choice rates. We find no difference.

tervention *expands* the choice set. Since we maintain the relative timing of grocery purchases and consumption, our intervention should weakly enhance welfare for both the short-run and long-run selves.¹⁰

While the effects of waiting periods are not predicted by the workhorse β - δ model of present bias (O’Donoghue and Rabin 1999), they are consistent with the Gabaix and Laibson (2017) framework that models myopia as being generated by imperfect forecasts of future utility. In their setting, people are uncertain about the utility derived from potential future outcomes. By prompting deliberation, waiting periods reduce noise when forecasting utility. This process leads to choices that are less myopic and more in-line with a person’s underlying goals such as healthier eating.¹¹

We tested this deliberation channel by varying when shoppers made the subsidy choice: at the start of the waiting period (“Early Choice”) or at the end of the waiting period at the point of purchase (“Delayed Choice”). The framework of Gabaix and Laibson (2017) predicts that Early Choice can further increase the efficacy of waiting periods by narrowing the set of potential future outcomes. This leads to more focused deliberation, further decreasing myopia and spurring more goal-oriented decisions.¹² Indeed, we find that despite no differences in the healthy subsidy choice rates across all treatments, shoppers in the Early Choice treatment have the highest FV spending—over 60% more compared to the restricted healthy subsidy and 20% more than those in the Delayed Choice condition.

Our findings contribute to a growing literature that uses insights from psychology

¹⁰A rich literature has examined commitment opportunities where consumers select a seemingly dominated option as a means of restricting their choice set. See, for example, (Ariely and Wertenbroch 2002; DellaVigna and Malmendier 2006; Sadoff, Samek, and Sprenger 2020; Schwartz, Mochon, Wyper, Maroba, Patel, and Ariely 2014).

¹¹Section 3 considers how other models, such as goal-setting, adaptive reference points, and preference adaptation may relate to waiting periods.

¹²Note that the Early Choice treatment can also change purchase behavior by affecting the subsidy choice. While most grocery shopping is for future consumption, present-bias may tempt shoppers to select the unhealthy subsidy if it immediately precedes the purchase decision. Early Choice may allow shoppers to overcome this by separating the purchase decision from the subsidy choice, permitting them to “pre-commit” to the healthy subsidy. As discussed in Section 4, we find no evidence for this type of dynamic inconsistency in subsidy choice.

and behavioral economics to develop interventions in policy-relevant domains. Examples include employee productivity (Gosnell, List, and Metcalfe 2020), resource management (Hahn, Metcalfe, Novgorodsky, and Price 2016), education (Brownback and Sadoff 2019; Levitt, List, Neckermann, and Sadoff 2016), health (Volpp, John, Troxel, Norton, Fassbender, and Loewenstein 2008), and tax collection (Hallsworth, List, Metcalfe, and Vlaev 2017). We add to this line of work by designing and implementing novel interventions in a policy-relevant domain: subsidizing healthy food purchases. Our interventions are also unique in that they do not impose additional costs (relative to the restricted healthy subsidy) and can be easily incorporated into existing delivery mechanisms.

Our findings also contribute to the literature on using non-paternalistic interventions to spur behavior change (Grüne-Yanoff and Hertwig 2016; Johnson, Shu, Dellaert, Fox, Goldstein, Häubl, Larrick, Payne, Peters, Schkade et al. 2012; Jung and Mellers 2016; Thaler and Sunstein 2009). We add to this work by demonstrating that giving people *more* choice and making seemingly irrelevant changes to the decision environment substantially increases the effectiveness of food subsidies. To the best of our knowledge, ours is the first paper to examine how increased agency and waiting periods can be used to enhance the effectiveness of policy-relevant interventions. As with a “nudge” Thaler and Sunstein (2009), our approach is non-paternalistic and reversible. By promoting deliberation and active choice, the interventions also inform the research on “boosts” Grüne-Yanoff and Hertwig (2016).

The paper proceeds as follows. Section 2 describes the methods and experimental design, including details about the mobile platform used in the experiment. Section 3 outlines the hypothesis development in our setting. Section 4 presents the results. We discuss our findings in Section 5 and conclude.

2 Experimental Design

The details of our experimental design and analysis were pre-registered on AsPredicted.org.¹³

Our experiment was conducted on a smartphone-based market-research platform called Field Agent. This platform is designed for crowd-sourced consumer research. Shoppers nationwide can find paid “jobs” to complete through their smartphones while conducting their typical shopping trips. This format allowed for unique experimental protocols that facilitated the study of shopping behavior in a natural environment. All of our shoppers across all treatments were able to conduct their shopping trips at whatever time and place they wanted without prior approval from us. Upon completion of a shopping trip and submission of all documents, Field Agent confirmed that all protocols were followed according to the assigned treatment.

There were three primary features of Field Agent that permitted our unique experimental design. First, Field Agent geo-tagged all responses and cross-referenced these location tags with the locations of grocery stores across the U.S. to guarantee that our shoppers were at the point of purchase while completing any of their in-store tasks. Second, Field Agent had an established electronic payments protocol with their users. This gave our experimental procedures credibility with the shoppers and guaranteed prompt transfers of subsidies and experimental payments. Finally, Field Agent collected pictures of all grocery shopping receipts uploaded from shoppers’ smartphones. These receipts were tabulated by workers on Amazon’s Mechanical Turk website who were blind to the experiment. Using these tabulated receipts, we calculated subsidy payments and measured treatment effects. The receipt timestamps also verified that shoppers completed their tasks prior to finalizing their purchases. Accord-

¹³We pre-registered the data collection and analysis for the primary treatments we focus on in this paper (Part 1). After conducting this experiment, we ran a follow up study to separately examine the impact of a waiting period without agency (Part 2). Treatments associated with Part 2 will be highlighted later in this section. We analyze these data in accordance with our pre-registered analysis plan with fixed effects for the data collection wave that capture any fixed differences between Part 1 and Part 2. Our pre-registration includes individual-level analysis of the impact of time preferences on shopping behavior that is forthcoming in a companion paper.

ing to Field Agent’s internal protocol, shoppers who violated the procedures—either timing or location—were first given a warning, and upon second-offense eliminated from the study. The experiment had no influence over this process.

To maintain a natural shopping environment, we asked all shoppers to continue with their normal shopping schedule—shopping at their regular store at their regular time. Our treatments then varied the delivery of food subsidies to shoppers’ smartphones during these trips. The study involved “healthy” subsidies offering 30% off purchases of fruits and vegetables (FV) and “unhealthy” subsidies offering 30% off purchases of baked goods (BG). Both subsidies were capped at \$10 per trip.¹⁴ According to Consumer Expenditure Survey (CES) data, these categories represent roughly equal percentages of food purchases by SNAP recipients and offer a clear valence to define a “healthy” and “unhealthy” option.¹⁵

2.1 Recruitment

Field Agent has over 1 million registered users on its smartphone-based shopping survey platform in the U.S. alone.¹⁶ We restricted our population to the subset of recently active users and recruited shoppers with a gross household income less than 185% of the federal poverty line (FPL) to take part in our study.¹⁷

We conducted our study in two parts. In Part 1, we collected data from four treatments and a control group. In Part 2, we repeated one of the treatments from Part 1 and included a new treatment. In both Parts 1 and 2, we targeted approximately

¹⁴Fruits and vegetables include fresh, canned, or frozen fruits or vegetables without added salt or sugar. Baked goods include bread, biscuits and rolls, muffins, cakes and cupcakes, pies and tarts.

¹⁵CES data span 1994-2003, limited to households that self-report as SNAP participants. FV and BG represent 8% and 9% of total food expenditure, respectively, and are purchased on 76% and 82% of days with at least \$20 in food spending, respectively. In our control group, shoppers spend more on BG than FV, so *ex-ante*, the BG subsidy is more valuable.

¹⁶Smartphone ownership is a common pre-condition for studying behavior of low-income participants (e.g. [Smith, Morgan, Plotnikoff, Dally, Salmon, Okely, Finn, and Lubans \(2014\)](#)) since the majority meet the requirement. For example, over 67% of Americans with incomes less than \$30,000 own smartphones (Pew Research Center: www.pewinternet.org/fact-sheet/mobile).

¹⁷Gross income of 185 percent of the FPL is the federal maximum for households to meet the income qualification for WIC subsidies. SNAP eligibility begins beneath 130 percent of the FPL. U.S. Department of Agriculture Food and Nutrition Service: www.fns.usda.gov/wic/wic-eligibility-requirements, www.fns.usda.gov/snap/recipient/eligibility.

150 shoppers for all but two treatments, which were given a slightly greater weight to increase statistical power. Our analysis will include fixed effects for Parts 1 and 2 to account for potentially non-random variation in behavior between the two.

For Part 1, we recruited shoppers in eight separate waves beginning in March 2018. The final endline surveys were completed by July 2018. Shoppers were randomized within each wave and allowed to complete the study at their own pace. This randomization will be discussed in more detail in Section 2.3. For Part 2, we recruited all shoppers in a single wave beginning in March 2019. The final endline surveys were completed by April 2019. The randomization occurred once at the beginning of March. All shoppers were given approximately eight weeks to complete four shopping trips. Within those eight weeks, shoppers could complete the study at their own pace.

To enroll in the study, all shoppers completed an income-screening survey. Qualifying shoppers were then invited to complete our baseline survey. The baseline survey collected characteristics of each shopper’s food household including income, household size, SNAP participation, a recent shopping receipt, a 24-hour food diary, time preferences, and food security questions asking about the availability and affordability of desired foods.

2.2 Treatments

All shoppers who completed the baseline in either Part 1 or Part 2 were assigned to a treatment that was fixed throughout the study, generating a fully between-subjects design. Shoppers in Part 1 were randomly assigned to one of **Control**, **Restricted**, **Agency**, **Waiting Period (Delayed Choice)**, **Waiting Period (Early Choice)**. Shoppers in Part 2 were randomly assigned to either **Restricted** or **Waiting Period (No Agency)**. All treatment instructions can be found in Appendix Section B. Our six treatments are:

- **Control** — Shoppers submitted photographs of their receipts, but received no subsidies.
- **Restricted** — All shoppers received the healthy subsidy. Subsidy information

was delivered in the store before the purchase decision.

- **Agency** — Shoppers chose between the healthy and unhealthy subsidies. Subsidy information was delivered in the store. Shoppers made their subsidy choice in the store before the purchase decision.
- **Waiting Period (Delayed Choice)** — Shoppers chose between the healthy and unhealthy subsidies. Subsidy information was delivered 4 to 48 hours before shopping. Shoppers made their subsidy choice in the store before the purchase decision.
- **Waiting Period (Early Choice)** — Shoppers chose between the healthy and unhealthy subsidies. Subsidy information was delivered and shoppers made their subsidy choice between 4 and 48 hours before shopping. Shoppers were asked to recall their subsidy choice in the store before the purchase decision.
- **Waiting Period (No Agency)** — All shoppers received the healthy subsidy. Subsidy information was delivered 4 to 48 hours before shopping. They were reminded about the subsidy in the store before the purchase decision.

Shoppers had the opportunity to complete four separate “Shopping Trips” under their assigned treatment. The Shopping Trips were designed to be as natural as possible with their timing and location decided by the shopper.¹⁸ Our only requirement was that the Shopping Trips be at least five days apart. After completing all four Shopping Trips, shoppers concluded the study with an endline survey measuring many of the same characteristics as the baseline survey. In addition to any subsidy or time-preference elicitation payments, shoppers were paid a flat fee of \$1 per completed survey with a bonus payment that guaranteed \$30 total if they completed all of the surveys in the study.

Upon assignment, shoppers learned the procedures for their treatment. Shoppers followed their assigned procedures and submitted surveys and receipts to Field Agent for verification. Any Shopping Trips or survey submissions that failed the verification checks were not reimbursed and our partners contacted the shoppers to explain the failure.

¹⁸In the endline survey, we asked shoppers if they changed their shopping schedule to accommodate the study. 62% said they integrated the study into their normal routine, and this response was balanced across treatments.

There are three types of tasks associated with Shopping Trips—Pre-Shopping Tasks, Shopping Tasks, and Post-Shopping Tasks. The timing of the tasks depended on the assigned treatment. Table 1 clarifies the different tasks and timing for each treatment.

Table 1. Shopping Trip Timeline by Treatment

	4 to 48 hours before grocery shopping <i>“Pre-Shopping Task”</i>	While shopping (in-store before purchase) <i>“Shopping Task”</i>	In-store after purchase <i>“Post-Shopping Task”</i>
Control			Submit receipt for participation payment
Restricted		Subsidy information delivered	Submit receipt for participation payment and shopping reimbursement
Agency		Subsidy information delivered and subsidy choice made	
Waiting Period (Delayed Choice)	Subsidy information delivered	Subsidy reminder and subsidy choice made	
Waiting Period (Early Choice)	Subsidy information delivered and subsidy choice made	Subsidy choice reminder	
Waiting Period (No Agency)	Subsidy information delivered	Subsidy reminder	

For shoppers assigned to a Waiting Period treatment, the Shopping Trip began with a Pre-Shopping Task. Any shopper assigned a Pre-Shopping Task needed to complete this task 4 to 48 hours prior to shopping. This window enforces a minimum waiting period on the shopper while also remaining flexible enough to limit its burden. As with all aspects of our study, this was completed at the shopper’s convenience. We encouraged shoppers to complete their Pre-Shopping Tasks at a time that allowed them to continue their normal shopping patterns within the specified 4- to 48-hour window. Failure to complete the Pre-Shopping task during this window would invalidate a submission and the shopper would be not eligible for the subsidy. In this event, they were asked to complete their trip as usual, and were then given one chance to complete a “make-up Pre-Shopping Task” before a later Shopping Trip. In our analysis, per our pre-registration, we include all data collected during these make-up trips.

Shoppers assigned to the Waiting Period (Early Choice) treatment made their selection between the healthy and unhealthy subsidy during the Pre-Shopping Task. Shoppers assigned to the Waiting Period (Delayed Choice) treatment received information about the subsidy choice they would later make as a part of their Shopping

Task on the upcoming Shopping Trip. Finally, Shoppers assigned to the Waiting Period (No Agency) treatment simply received information about the healthy subsidy they were assigned to receive on their upcoming Shopping Trip.

Shoppers in every treatment but Control were required to complete a Shopping Task inside the grocery store prior to finalizing the grocery purchases. During the Shopping Task, shoppers in the Agency and Waiting Period (Delayed Choice) treatments made a selection between the healthy and unhealthy subsidies. For shoppers in the Restricted and Waiting Period (No Agency) treatments, the Shopping Task simply delivered information about the healthy subsidy they were assigned to receive. Finally, shoppers in the Waiting Period (Early Choice) treatment were reminded of the subsidy choice they made on the Pre-Shopping Task. Field Agent used the timestamp and geo-tag from the completed Shopping Task to determine that the task was completed at the grocery store and prior to finalizing grocery purchases.¹⁹

Shopping Trips in every treatment finished with a Post-Shopping Task. This task required the shopper to submit a picture of their shopping receipt. For any treatment with a subsidy, this receipt determined their subsidy payment. For the control group, this finalized their Shopping Trip and ensured their participation payment.

Differences in the Post-Shopping Task highlight one complication in estimating the impact of the subsidies: we cannot observe every grocery shopping trip and shoppers may conduct extra-experimental shopping trips that they fail to report. A specific concern is that our subsidies may incentivize shoppers to concentrate their shopping into fewer trips to maximize their subsidy payments. For example, shoppers in Control may be more likely to conduct FV shopping on an extra-experimental shopping trip because they are not sacrificing subsidy payments by doing so. This distorts the estimated level effects of the subsidies. However, this concern does not apply to the impacts of the behavioral interventions—the primary focus of the paper—which are

¹⁹We cannot confirm that the Shopping Tasks were completed at the beginning of the Shopping Trip, but any delay between starting the trip and completing the Shopping Task should bias our estimated treatment effects downward because a greater share of the shopping would have been completed prior to our intervention.

identified *conditional on receipt of subsidies*. Shoppers with agency can select out of the FV subsidy. If these shoppers prefer to buy FV outside of the study, this could cause a *downward* bias in our estimated impact on FV spending. As shown below, the subsidy choice rates in the Waiting Period treatments were nearly identical to the Agency treatment. Because the interventions did not affect the relative price of FV, they also do not affect incentives for extra-experimental shopping trips.

2.3 Randomization

As each recruitment wave was completed, we conducted a stratified randomization with separate strata for 1) SNAP participation and 2) a stated desire to improve fruit and vegetable consumption.²⁰ The proportion of shoppers assigned to each treatment was similar but not constant for each recruitment wave, so our analysis will include fixed effects for the recruitment wave.²¹

Shoppers were informed of their randomly-assigned treatment along with the respective procedures when they opened their first task for the first Shopping Trip after the baseline. Thus, while differential attrition is a potential concern after the first Shopping Trip, differential selection at the time of assignment is not.

2.4 Shopper Characteristics

In Part 1, 802 shoppers successfully completed the baseline survey. Treatment assignment was balanced on observables, which are reported in the left side of Table 2. In Part 2, 300 shoppers completed the baseline survey. Treatment assignment was again balanced on observables. This balance is recorded in the right side of Table 2.

²⁰We could not stratify based on baseline FV purchases as the receipts took too long to be tabulated.

²¹We increased the weight on the Agency and Waiting Period treatments in later waves order to improve our power to test those interventions.

Table 2. Balance of Shopper Characteristics by Treatment

	Part 1: Mar–Jul, 2018						Part 2: Mar–Apr, 2019		
	Control	Restricted	Agency	Waiting Period (Delayed)	Waiting Period (Early)	F-Test	Restricted	Waiting Period (No Agency)	F-Test
<i>Reported Dietary Satisfaction</i>									
Want more fruits & vegetables (1-3)	1.809	1.859	1.815	1.805	1.793	0.602	1.839	1.821	0.691
Have enough food to eat (1-4)	2.319	2.303	2.265	2.216	2.310	0.642	2.195	2.159	0.667
Can afford food (1-5)	2.908	2.923	2.741	2.719	2.841	0.374	2.879	2.715	0.206
Can afford fruits and vegetables (1-5)	2.433	2.408	2.370	2.286	2.483	0.693	2.336	2.113	0.141
<i>Shopper Characteristics</i>									
SNAP participant	0.397	0.401	0.407	0.443	0.400	0.906	0.349	0.351	0.971
Male	0.184	0.211	0.153	0.151	0.152	0.590	0.154	0.166	0.792
Household Size	3.596	3.662	3.635	3.703	3.586	0.971	3.732	3.662	0.743
Time discounting (standardized)	-0.081	-0.022	-0.101	-0.088	0.005	0.851	0.021	-0.096	0.308
<i>Baseline Grocery Receipts</i>									
Fruits and vegetable purchases (\$)	6.114	4.249	5.663	4.378	4.386	0.255	6.263	6.393	0.891
Baked goods purchases (\$)	11.025	13.486	12.367	10.580	7.666	0.335	1.937	1.520	0.221
Use EBT card for purchase	0.468	0.528	0.587	0.605	0.549	0.116	0.242	0.212	0.541
Observations	141	142	189	185	145		149	151	

F-test conducted as a joint test of equality across all treatments (robust standard errors), with the p -values reported.

Our baseline data show that the majority of our sample experiences both food insecurity and a desire to improve the nutritional quality of their diet: 67% reported some measure of food insecurity and 83% reported that they would like to consume more fruits and vegetables.

Our shoppers were geographically diverse, coming from across the United States. 40% of our sample reported participating in SNAP. The vast majority of our shoppers (83%) were female. While this is not representative of the overall population, the skew is representative of the gender disparity in grocery shopping that has been found by others (Bhattarai 2017). The majority of shoppers (53%) reported living in a city with fewer than 50,000 residents.

2.5 Attrition

Differential attrition is a potential concern for any field experiment involving repeated observations. Attrition could threaten our identification of treatment effects *if* shoppers with low valuations of our subsidies are more likely to attrit from our treated groups. Since shoppers did not know their treatment assignment until beginning their first Shopping Trip, attrition between the baseline and the first Shopping Trip cannot be attributed to the treatment assignment. However, differential attrition upon learning of the treatment procedures is a potential concern.

Table 3 quantifies the differences in attrition across the treatments. We first present the mean number of Shopping Trips completed by treatment. We find some differences in attrition amongst the Control group in Part 1 and the Restricted treatment in Part 2—both display higher completion rates. Fortunately, we see no significant differences in attrition for T_1 through T_4 in Part 1, which are used to test our main hypotheses of interest—the effects of agency and waiting periods on food purchases.

All subjects completed our baseline survey prior to the randomization. From this, we can directly test for selective attrition based on our outcome of interest, FV purchases. This is equivalent to testing the assumptions about selective attrition made in common bounding exercises such as Lee (2009). In Table 3, we test for selective

attrition based on baseline levels of FV spending. We present the relevant marginal effects from ordered probit regressions of attrition on baseline FV spending across treatments.²² We find that baseline FV spending does not explain any selective attrition between the baseline survey and Trip 1 (Panel A) or total attrition between baseline and endline (Panel C). Panel B shows that FV spending from a given Shopping Trip does not predict selective attrition for the next Shopping Trip. Since we can refute the selective attrition assumptions of common bounding exercises, we will proceed with our analysis in accordance with our pre-analysis plan.

²²Changes in participation take on three values: -1 is attrition, 0 is constant status, and 1 is re-joining following attrition (which is very rare).

Table 3. Shopping Trip Completion and Attrition by Treatment

	Part 1: Mar–Jul, 2018						Part 2: Mar–Apr, 2019		
	Control	Restricted	Agency	Waiting Period (Delayed)	Waiting Period (Early)	F-Test	Restricted	Waiting Period (No Agency)	F-Test
Shopping Trips completed	2.752	2.486	2.503	2.178	2.497	0.077	3.040	2.563	0.014
	<i>Panel A: Baseline to Trip 1</i>								
Coefficient: Treatment \times Baseline FV purchases	-0.001	-0.001	-0.000	-0.003	0.002	0.983	0.001	-0.004	0.443
	<i>Panel B: Trips 1 to 4</i>								
Coefficient: Treatment \times Lagged FV purchases	0.000	0.001	0.001	0.001	-0.002	0.675	-0.001	0.001	0.379
	<i>Panel C: Baseline to Endline</i>								
Coefficient: Treatment \times Baseline FV purchases	0.004	-0.003	-0.002	0.002	0.001	0.803	0.001	-0.005	0.350

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Panels A and C report results from probit estimates of dropping out of the sample from the baseline to Trip 1, and the baseline to the endline, respectively (robust standard errors). The coefficient estimates are the marginal effects of increasing baseline FV spending within each treatment. Panel B reports results from an ordered probit estimation (it is possible, though very rare, to miss a trip and come back) of changes in sample participation from trip to trip as a linear function of lagged FV spending (with standard errors clustered at the individual level). The coefficient estimates are the marginal effects of increasing lagged FV spending within each treatment. Wave fixed effects are included in all models. F-tests are conducted as a joint test of equality across all treatments, with the p -values reported.

3 Hypotheses

In this section, we present four hypotheses about the way in which subsidies will affect FV purchases and how our behavioral interventions will enhance the effectiveness of these subsidies.

Our first hypothesis concerns how healthy food subsidies will affect FV purchases.

Hypothesis 1: *Healthy subsidies will increase purchases of fruits and vegetables.*

As with all ordinary economic goods, the law of demand states that demand for FV will increase as price falls.

Hypothesis 2: *Giving shoppers agency in choosing between healthy and unhealthy subsidies will not decrease FV purchases relative to the restricted subsidy.*

This stands in contrast to the predictions of the standard neo-classical model. Our experiment allows us to compare shoppers who are restricted to FV subsidies to shoppers who have agency in choosing between FV and BG subsidies. The opportunity to choose between subsidies will weakly increase the price of FV and decrease the price of BG relative those restricted to FV subsidies. Standard economic theory predicts that this should decrease FV purchases.²³

Behavioral theories such as a demand for consistency (Falk and Zimmermann 2018) and self-signaling (Benabou and Tirole 2004, 2011) predict that giving shoppers an *active choice* between subsidies could actually increase FV spending. Falk and Zimmermann (2018) argue that an initial active choice impacts how subsequent information is processed, such that information that runs counter to this choice is either downweighted or ignored. This asymmetry in information processing leads to behavior that is consistent with the initial choice. In the self-signaling framework proposed in Benabou and

²³In Appendix Section A.1, we show why the standard model predicts that FV spending will be lower in the Agency treatment compared to the Restricted treatment under a broad set of conditions. We also show that the necessary conditions for a predicted increase in FV spending are inconsistent with our data.

Tirole (2004) and Benabou and Tirole (2011), people are uncertain about their true underlying preferences and look at prior choices to guide subsequent behavior. In this way, prior choices act as informative signals that spur individuals to act consistently.²⁴

This consistency effect is illustrated in the “foot-in-the-door” paradigm of Freedman and Fraser (1966). The authors asked people to complete a relatively burdensome task. Prior to this appeal, one group was contacted with a simple request to which the vast majority consented. The other group was presented with the same burdensome appeal without the small request. Those who agreed in the first stage behaved consistently with their initial choice. As a result, there was a higher rate of compliance amongst those who were presented with a small initial request compared to those who were not. Subsequent work has shown that the tendency to act consistently is driven by a change in attitude towards the target act (Bem 1967; Gneezy, Imas, Brown, Nelson, and Norton 2012).

In our setting, where the vast majority of our shoppers state a desire to increase FV consumption, both the demand for consistency and self-signaling models predict that the *act of choosing* a FV subsidy will cause a shopper to spend more on FV than if they had passively received it. Thus, agency presents shoppers with a technology for leveraging their own behavioral responses that does not exist under a restricted healthy subsidy. The demand for consistency can counteract the price effects of increasing agency, muting the impact of the latter, or even dominating it. The self-signaling framework makes an additional prediction: that shoppers will be willing to choose the healthy subsidy even if it means leaving money on the table, since this would generate a costly signal of their intention to eat healthier. We examine this prediction in Section 4.3.

Hypothesis 3: *Waiting periods between the delivery of subsidy information and the shopping decision will increase FV purchases.*

²⁴Prior choices can also act as strategic forward-looking signals to motivate oneself to behave consistently later on. For example, Andreoni, Kuhn, and Samuelson (2019) show that when subjects can select the parameters of a game they are about to play, they cooperate at higher rates than subjects placed into identically parameterized games without the choice.

Across three experiments, [Imas et al. \(2018\)](#) demonstrate that introducing waiting periods between information about a choice and the choice itself leads to more patient and far-sighted decisions. One possible psychological pathway for this effect is that waiting periods prompt deliberation and prospection of future utility outcomes ([Gilbert and Wilson 2007](#); [Wheeler, Stuss, and Tulving 1997](#)). [Gabaix and Laibson \(2017\)](#) formalize this in a theoretical model where an individual is uncertain about the future utility consequences of her choices (e.g. increasing FV spending) and generates imperfect forecasts by mentally simulating the potential outcomes. The forecasted utility consequences of a choice are estimated with noise that increases with time; outcomes further in the future have noisier distributions of potential consequences and as a result, are more heavily discounted. Mentally simulating these consequences because of the introduction of a waiting period generates unbiased signals of “true” utility, which, when combined with prior beliefs, reduce noise in the forecast. Under reasonable assumptions, this process leads to less myopic decision making.

Waiting periods may also affect food choices by allowing time for shoppers to set goals for their purchases. Research has shown that goal-setting and endogenously-established reference points can provide powerful motivation ([Fishbach and Ferguson 2007](#); [Hsiaw 2013](#); [Koch and Nafziger 2011](#)). Moreover, as shown theoretically by [Koszegi and Rabin \(2008\)](#), and empirically by [Heffetz \(2018\)](#), reference points may require time in order to “sink in.” The same may be true for preference adaptation as outlined by [Bernheim, Braghieri, Martinez-Marquina, and Zuckerman \(2019\)](#). Our waiting period intervention can provide the needed time for preference adaptation or for reference points to sink in, which will impact shopping behavior accordingly. We focus on the deliberation model here as it motivated our study design.

As with previous studies in behavioral economics, we consider the trade-off between the immediate pleasure and the delayed health consequences of food choices as an exercise in patience.²⁵ For this reason, we predict that introducing a waiting period between the delivery of subsidy information and the grocery shopping will lead shoppers

²⁵E.g. [Read and van Leeuwen \(1998\)](#), [Shapiro \(2005\)](#).

to take greater advantage of their healthy subsidies and spend more on FV.

Hypothesis 4: *The early subsidy choice—before the waiting period—will increase FV spending compared to the delayed subsidy choice*

Allowing shoppers to make a subsidy choice before the waiting period begins, as in our Waiting Period (Early Choice) treatment, can increase FV spending through two channels. First, if immediate consumption generates temptation, then Early Choice may overcome present-bias in subsidy selection. Present-biased preferences may lead shoppers who prefer the FV subsidy before entering the store to nonetheless choose the BG subsidy after entering due to the immediate temptation of the unhealthy food. Early Choice allows shoppers to commit to the FV subsidy before facing this temptation, increasing FV spending by lowering average FV prices. However, conditional on the chosen subsidy, models of present-bias make no predictions about the impact of early choice on purchasing behavior.

A second channel motivates our Hypothesis 4. Early Choice allows shoppers to narrow the set of potential future outcomes and reduce uncertainty around relative prices. In the imperfect forecasting model of [Gabaix and Laibson \(2017\)](#), this acts to decrease the number of prospective states that the individual must consider when deliberating on future utility consequences. Waiting periods are thus predicted to be even more effective in reducing noise around forecasts of long-run health consequences, which is predicted to further increase FV spending.

Our data can distinguish between these two mechanisms by analyzing subsidy choice rates and purchases conditional on a subsidy choice. Under present bias, Waiting Period (Early Choice) will increase FV purchases through greater FV choice rates, but conditional on subsidy choice, food purchases should be similar to Waiting Period (Delayed Choice). In contrast, the imperfect forecasting model predicts an increase in FV spending compared to Waiting Period (Delayed Choice) even if subsidy choice rates remain the same.

We now proceed to test these hypotheses in our data.

4 Results

We find that each marginal intervention increases FV spending: in Part 1, the mean FV spending is \$4.03 in Control, \$8.67 in Restricted, \$9.31 in Agency, \$10.64 in Waiting Period (Delayed Choice), and \$12.17 in Waiting Period (Early Choice). In Part 2, the mean is \$11.23 in Restricted, and \$12.20 in Waiting Period (No Agency). To explore the impact of our treatments on FV spending we use a linear, random-effects regression with shopper-specific random effects and standard errors clustered at the shopper level (as specified in the pre-registration). Our estimation equation is given by Equation 1 below:

$$\text{FV}_{i,t} = \alpha + \Gamma' Z_i + \beta \times \text{FV}_{i,0} + \delta_{w(i)} + u_i + \varepsilon_{i,t} \quad . \quad (1)$$

$\text{FV}_{i,t}$ is the FV spending for shopper i on Shopping Trip t .²⁶ Z_i is a vector of indicator variables for either assigned treatments, or partitions of the treatment set. $\text{FV}_{i,0}$ controls for FV spending from the baseline survey to increase precision. Because the randomization weights changed slightly over the course of the study, we use assignment-wave fixed effects, $\delta_{w(i)}$, to ensure robustness. We do this both at a coarse level, with a fixed effect for Part 1 or 2, and at a fine level, with a “wave fixed effect” for every randomization group within Part 1. u_i is the shopper-specific random effect. We present results with and without the baseline control and assignment-wave fixed effects to demonstrate that the treatment effects are unaffected by their inclusion.

4.1 Effect of Subsidies

We first demonstrate the effectiveness of subsidies on increasing FV spending. Table 4 pools across subsidy treatments to present these effects, which are large and positive. Using the full specification in column (3), the average effects of all the subsidy treatments leads FV spending to increase by 164 percent—or 0.98 SD—relative to the Control group.

²⁶This is the pre-subsidy, gross spending before subtracting off any subsidy payments.

Table 4. Effect of Subsidy Treatments on FV Spending (\$)

Control Mean [SD]: 4.03 [6.71]	(1)	(2)	(3)
Subsidy	6.11*** (0.60)	6.33*** (0.60)	6.60*** (0.61)
Baseline Survey FV Spending (\$)		0.16*** (0.05)	0.15*** (0.05)
Part 1, Part 2 Fixed Effects	Y	Y	Y
Wave Fixed Effects	N	N	Y
Observations	2767	2767	2767
Clusters	805	805	805

*** $\Rightarrow p < 0.01$. Standard errors in parentheses are clustered at the shopper level. All specifications are linear random effects models.

Increases in FV spending can arise through a substitution effect that shifts purchases towards FV, through an income effect that increases spending across-the-board, or both. To show the effect of substitution towards FV, Table 5 presents the increase in the fraction of food spending on FV.²⁷ The subsidy treatments increase the share of food spending on FV by 15 percentage points from a mean of 13 percent in the control group. Appendix Table A.1 provides further evidence of the prominence of the substitution effect, finding no significant increases in total food spending. Additionally, Appendix Table A.2 shows that the marginal impact of a subsidy is, if anything, a negative for BG spending.

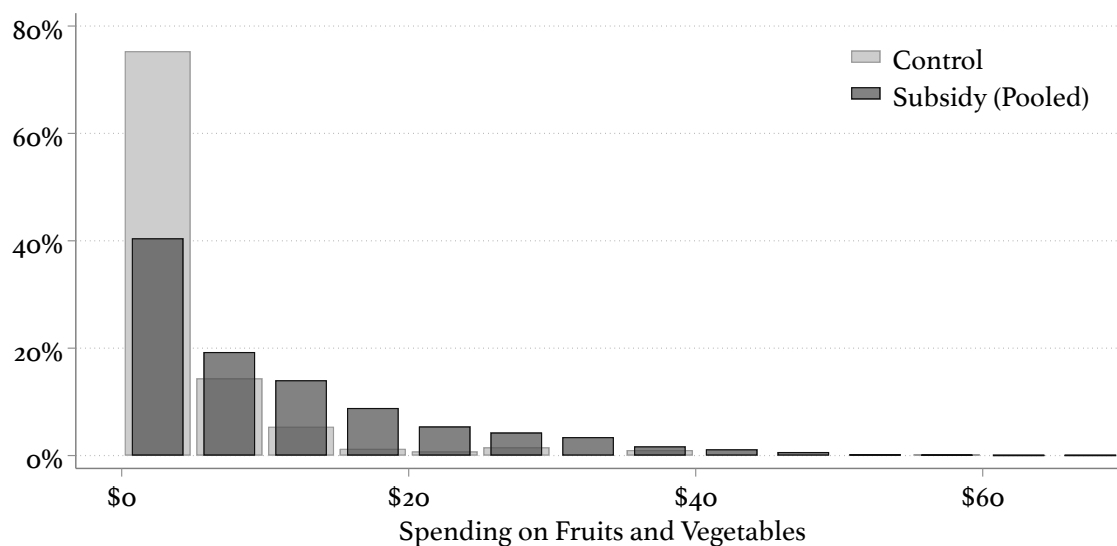
Figure 1 shows that the subsidy treatments have the largest effect on the quasi-extensive margin. Subsidies move a large mass of people who spend very little on FV (\$0 to \$5) toward spending intermediate amounts. Statistical tests show large and statistically significant differences in the distributions ($p < 0.001$).

²⁷Our receipt tabulations include fields for the receipt total and non-food spending. When both these fields are present, we define total food spending as the difference. In the case that the non-food expenditure field is missing, if imputed total food spending is negative, or if total FV and BG spending exceeds imputed total food spending, we use the sum of FV and BG spending as total food spending.

Table 5. Effect of Subsidy Treatments on FV Spending as a Fraction of Food Spending

Control Mean [SD]: 0.13 [0.22]	(1)	(2)	(3)
Subsidy	0.15*** (0.02)	0.15*** (0.02)	0.15*** (0.02)
Baseline Survey FV Fraction		0.16*** (0.04)	0.16*** (0.04)
Part 1, Part 2 Fixed Effects	Y	Y	Y
Wave Fixed Effects	N	N	Y
Observations	2745	2710	2710
Clusters	804	793	793

*** $\Rightarrow p < 0.01$. Standard errors in parentheses are clustered at the shopper level. All specifications are linear random effects models. We lose 22 observations and 1 shopper relative to the count in Table 4 because of receipts with no food items. We lose 35 observations and 11 shoppers in columns (2) and (3) relative to the count in column (1) due to baseline survey receipts with no food items.

**Figure 1.** Histogram of fruit and vegetable spending.

4.2 Behavioral Food Subsidies

Panels A – D of Figure 2 summarize the impact of each of our treatments from Part 1. Along the x-axis, the treatments grow in the intensity of the intervention. Recall that Waiting Periods (Early Choice) combines all of our behavioral interventions: agency, waiting periods, and early choice. Panels A and B show that increasing the intensity of the interventions has a strong, positive effect on the mean FV spending and no effect on the mean BG spending. When all interventions are combined in the Waiting Periods (Early Choice) treatment, there is an \$8.22 increase in FV spending. This is 61% larger than the effect of the Restricted subsidy treatment and more than triple the mean FV spending in the control group of \$4.03. Panel C corroborates these findings, estimating the treatment effects on FV spending after controlling for pre-intervention FV spending. Panel D performs a similar treatment effect estimation and shows a uniform null-effect on BG spending across treatments after controlling for pre-intervention BG spending.²⁸

²⁸Figure A.3 in the Appendix reproduces Panel C for each of the four shopping trips in the study. All treatments represent significant improvements over the control at the 99% confidence level in all trips. The increasing effect with treatment intensity is most noticeable in Trips 1 and 2.

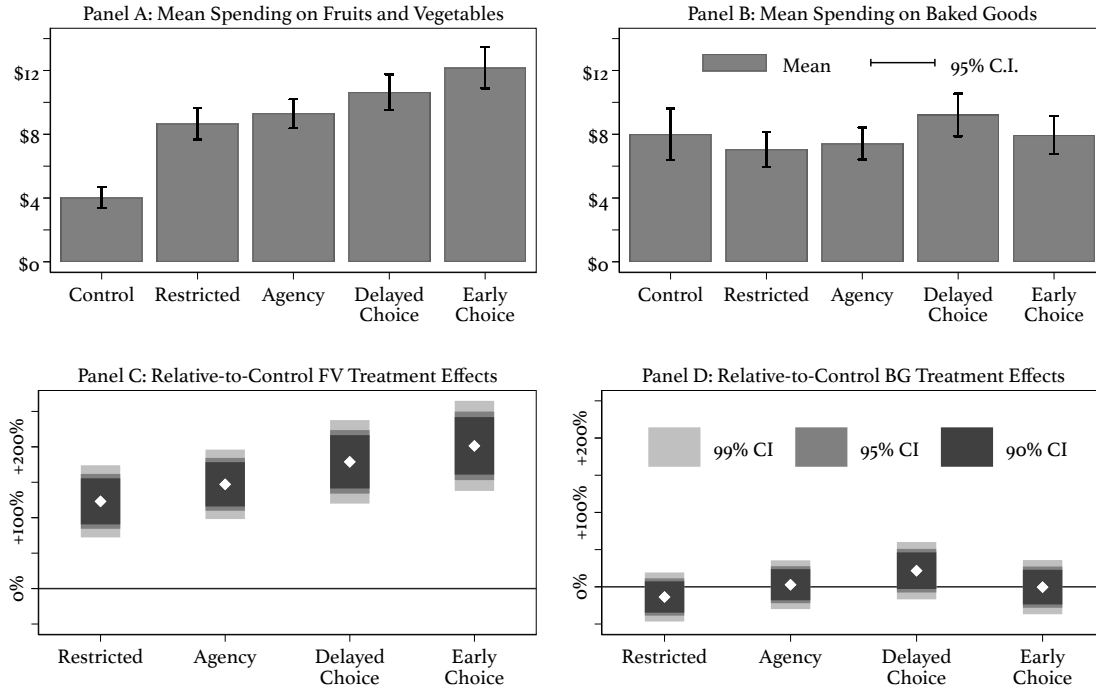


Figure 2. Panels A & B: Mean spending; Panels C & D: Treatment effects (Part 1 Only)

We now estimate the marginal impacts of agency and waiting periods on subsidy effectiveness. According to our hypothesis, a preference for consistency can only counteract the price effect of increased agency if the majority of shoppers choose the FV subsidy from the larger choice set. Indeed, we find that 78 percent of shoppers under the agency intervention select the FV subsidy.

Importantly, we find *no treatment effects on the FV subsidy choice rate*. The FV subsidy is selected 76 percent of the time in the Agency treatment, 79 percent of the time in the Waiting Period (Delayed Choice) treatment, and 79 percent of the time in the Waiting Period (Early Choice) treatment. We fail to reject the hypothesis that these rates are equal across all three treatments ($p = 0.82$). Equal selection rates means that any differences in average behavior imply differences in behavior *conditional* on subsidy choice, that is, differential subsidy effectiveness.

Table 6 shows the effects of our two primary behavioral interventions, agency and

waiting periods. Column 1 presents the estimated effect of the subsidies and the added, marginal effect of agency. Column 2 repeats this specification looking at the marginal effect of waiting periods. Column 3 jointly estimates the marginal impact of each intervention.

Table 6. Effect of Agency and Waiting Periods on FV Spending (\$), Combined Data

Control Mean [SD]: 4.03 [6.71]	(1)	(2)	(3)
Subsidy	5.11*** (0.81)	5.77*** (0.65)	5.11*** (0.81)
Agency	1.97** (0.80)		1.06 (0.87)
Waiting Period		1.76*** (0.61)	1.46** (0.67)
Baseline Survey FV Spending (\$)	0.15*** (0.05)	0.16*** (0.05)	0.15*** (0.05)
Part 1, Part 2 Fixed Effects	Y	Y	Y
Wave Fixed Effects	Y	Y	Y
Observations	2767	2767	2767
Clusters	805	805	805

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses are clustered at the shopper level. All specifications are linear random effects models.

Agency

Column 1 of Table 6 shows that the agency treatments lead to \$1.97 more FV purchases than treatments without agency. This significant increase in healthy food spending is 39 percent of the impact of the subsidy itself. When controlling for the waiting periods intervention, the marginal effect of agency is \$1.06—a 21 percent increase over the Restricted subsidy—though the effect is not statistically significant (Column 3). Importantly, 22 percent of shoppers with agency do not select the FV subsidy, increasing the price of FV relative to the Restricted treatment. The law of demand predicts that this will diminish FV spending, but our results suggest the opposite. In fact, we can rule out any decrease in FV spending more than \$0.64 at the 5% level. That is,

consistent with our second hypothesis, the behavioral effects associated with increased agency more than compensate for the price effects of some shoppers choosing the BG subsidy.

One concern with repeatedly eliciting subsidy choices from shoppers is that they may engage in inter-temporal substitution, alternating between subsidized FV purchases one week and subsidized BG purchases in a later week. We see no evidence of this in the subsidy choices: 58 percent of shoppers never change their subsidy choice throughout the study and only 7 percent change *and* change back to their original subsidy choice. For all shoppers, the previous subsidy choice has a large, positive, and statistically significant correlation with the current subsidy choice. Additionally, there are no aggregate trends with respect to subsidy choice across time.

Despite the positive average effect, for the minority of shoppers who choose the unhealthy subsidies, increased agency effectively incentivizes BG purchases. Thus, an important concern is whether our intervention has a negative impact on the FV spending of some of our shoppers. Appendix Figure A.4 in shows the CDFs of FV spending in the control treatment and the pooled agency treatments. The CDF of the agency treatments dominates the CDF of the control treatment. That is, regardless of a shopper’s unsubsidized FV spending, our agency intervention does not decrease her FV spending, even if the FV subsidy is not chosen.²⁹

Waiting Periods

Column 2 of Table 6 shows that the waiting period treatments generate \$1.76 more FV spending than treatments without a waiting period—a 31 percent increase in effectiveness. Column 3 shows that accounting for agency, the marginal impact of waiting periods alone generates \$1.46 more in FV spending—a 28 percent increase over the

²⁹A related concern is that the impact varies by wealth, leaving the poorest households unaffected. This concern is mitigated by the fact that our entire sample is low-income (< 185% of the FPL). Moreover, Appendix Table A.3 shows that the subsidies do not have significantly different impacts by welfare status. We do find that welfare-recipient households are less likely to select the FV subsidy, but the difference in choice rates does not produce differential estimates of the treatment effects.

Restricted subsidy. The significant, positive effect of waiting periods provides evidence for our third hypothesis.³⁰

We now separately consider the impact of allowing shoppers to make their subsidy choice prior to the waiting period (Early Choice) compared to after the waiting period is over (Delayed Choice). We previously established that Early Choice has no impact on subsidy choice. Table 7 exhibits results on FV purchases, presenting the individual impacts of each of the two treatments above the impact of subsidies themselves. First, we find that the waiting period with Delayed Choice causes FV spending to significantly increase by \$2.42, or 47 percent, over the Restricted subsidy. The waiting period with Early Choice causes an even larger increase in FV spending over the Restricted subsidy—\$3.18, or 63 percent.

While the difference between treatments is directionally in favor of Early Choice further increasing FV spending, it is not significant. These results are therefore only suggestive evidence for our fourth hypothesis. Notably, because we do not find any impact of the interventions on subsidy choice rates, these positive effects point to Early Choice making the waiting periods more effective.

³⁰Table 6 estimates the marginal impact of waiting periods across environments both with and without agency. We can estimate the marginal effect of waiting periods in the absence of agency by using a subsample of our data. This lower-powered test shows that the effect size falls slightly to \$1.14 and is not significant ($p = 0.34$) due, in part, to the increased standard errors.

Table 7. Effect of Waiting Periods and Choice Timing on FV Spending (\$)

	(1)
Waiting Period (Delayed Choice)	2.34** (1.03)
Waiting Period (Early Choice)	3.16*** (1.08)
Baseline Survey FV Spending (\$)	0.09* (0.05)
Constant (Restricted Subsidy)	4.34*** (0.74)
Early Choice - Delayed Choice: 0.81 ($p = 0.522$)	
Part 1, Part 2 Fixed Effects	N/A
Wave Fixed Effects	Y
Observations	1075
Clusters	327

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses are clustered at the shopper level. All specifications are linear random effects models. Sample is restricted to T1: Restricted, T3: Waiting Periods (Delayed Choice), and T4: Waiting Periods (Early Choice).

4.3 Subsidy choice as a costly signal

All shoppers made their subsidy choices prior to finalizing their purchases. Given this structure, a money-maximizing shopper could simply ignore the subsidy, make their grocery shopping decisions, and then select the subsidy that would maximize their subsidy payment at check out. Despite this available strategy, we observe many shoppers leaving money on the table.

Shoppers are significantly more likely to sacrifice money in order to select the healthy subsidy rather than the unhealthy one. We find that a minimum of 28 percent of shoppers selecting the FV subsidy would have walked away with more money had they chosen the BG subsidy *even without accounting for changes in behavior that would result from choosing the BG subsidy*. These shoppers sacrifice an average of \$3.37 each (101 percent of the mean subsidy). This compares to 17 percent of shoppers sub-optimally selecting the BG subsidy, losing an average of \$2.16 (64 percent of the mean

subsidy).³¹ Both the difference in proportions and the difference in average gains from the subsidy selection are statistically significant at the 1% level, meaning that the costly over-selection of healthy subsidies cannot be attributed to random choice errors, confusion, or misunderstanding.

One can interpret this behavior as shoppers taking advantage of agency to engage in costly signaling to spur increased purchases of healthy food. That is, consistent with the self-signaling framework of [Benabou and Tirole \(2004\)](#), shoppers select the FV subsidy with the understanding that this will motivate them to purchase more FV even if they will still ultimately spend more on BG. [Figure 3](#) captures the magnitude of this costly signaling for all shoppers who chose the FV subsidy. All shoppers below the 45-degree line would have earned more in subsidy payments had they chosen the BG subsidy; as shown in the figure, some forgo a significant amount of money by actively choosing the FV subsidy.

³¹We assume that spending on the subsidized category is a conservative upper-bound for counterfactual spending without the subsidy. For example, if Shopper A selects the FV subsidy and spends \$9 on FV and \$10 on BG, we classify this choice as costly. If Shopper B selects the FV subsidy and spends \$9 on both FV and BG, we do not classify this choice as costly, even though it most likely is.

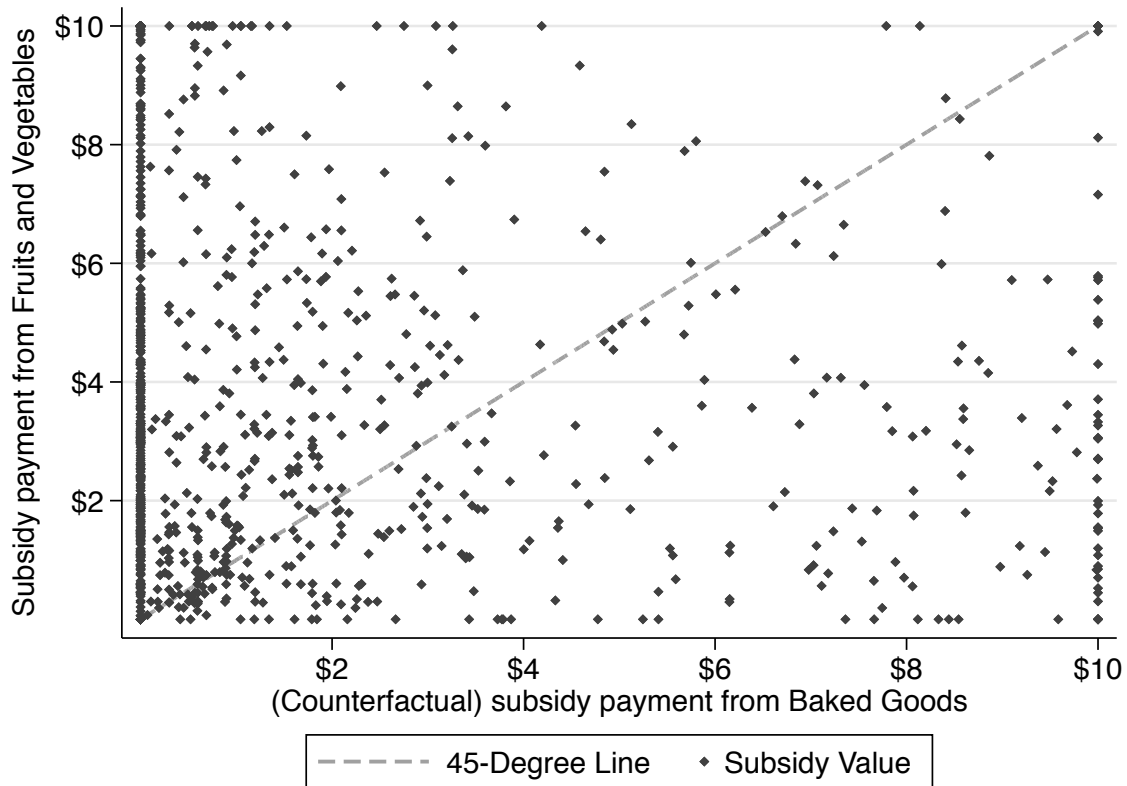


Figure 3. Healthy subsidy payments and counterfactual unhealthy subsidy payments for subjects who select the FV subsidy.

5 Conclusion

In this paper, we test the effectiveness of behavioral food subsidies in encouraging healthy food spending. We offer both policy-relevant and theoretical takeaways from our work. The vast majority of our shoppers state a desire to consume more FV, and prices appear to be an important reason why that desire is not satisfied. Relative to our control group, offering a restricted healthy subsidy more than doubles FV spending (a 124% increase). Next, we show that the effectiveness of healthy food subsidies can be substantially improved by increasing agency over those subsidies and introducing waiting periods. Combined, our interventions tripled FV spending (a 199% increase) relative to the control group. Because the behavioral interventions have no direct costs to the provider relative to the restricted subsidy, incorporating them into a food

assistance program can potentially have an outsized positive impact on the cost-benefit ratio of the program. According to [Castner and Henke \(2011\)](#), more than 20% of SNAP benefits are spent on the day they are disbursed, and more than half are spent by the end of the first week. Thus, informing participants about their healthy subsidy the day before disbursement could prompt deliberation prior to this sizable shopping event.

We implement agency and waiting periods in the context of food choice, a complex decision with many temptations that may cause a well-intentioned shopper to stray from their goals. Our interventions help shoppers overcome these short-run temptations and encourage more future-focused food choices. In the case of agency, we find evidence that expanding the choice set allows shoppers to signal their intentions through a (potentially costly) active decision in favor of the healthy subsidy, which generates consistent shopping behavior. We propose that waiting periods impact behavior by prompting shoppers to deliberate over their upcoming choices and adopt a more future-focused mindset.

Future research should explore the effectiveness of these non-restrictive interventions for promoting goal-oriented decisions in other domains. For example, wellness programs may be enhanced by presenting participants with a small choice set of incentive plans. Moreover, our agency intervention was framed as a choice between healthy and unhealthy subsidies. However, the behavioral mechanism that we believe drives this impact—a preference for consistency—does not necessarily imply that subsidy choices must be between healthy and unhealthy options. Shoppers also faced the same choice on all four Shopping Trips in the study. We believe there would be substantial value in future research to understand the use of agency to increase user buy-in. For example, the impact of a subsidy choice between two healthy options or the influence of changes to the subsidy choice set over time.

A substantial impediment to leveraging interventions in a subsidy program is operationalizing them in a market without the kind of user interface that we employed for this study. Our study featured substantial direct communication with shoppers, with treatments designed to test both theory and applications. These interventions are

nonetheless feasible with existing infrastructure. As a part of the Healthy Incentives Pilot (HIP), the USDA has already developed a model for modifying EBT software to identify an individual as a participant in the HIP, and assign them rebates based on spending in a particular food category. Our interventions could be implemented using a similar model. While we allowed for subsidy choices to change for each Shopping Trip, a simpler implementation could allow for SNAP participants to make a subsidy selection upon enrollment. In this way, early subsidy choice could be built in to the program. Alternatively, if policy-makers wanted to increase the frequency of choice, a subsidy selection system could be integrated into existing online portals that states use to allow recipients to check the EBT/SNAP balances. Future research may be needed to understand the relationship between agency and frequency of choice.

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A Online Appendix

A.1 Classical Utility Maximization

Consider the consumer's utility maximization problem. Suppose preferences over FV and BG spending are given by a Cobb-Douglas utility function: $U(FV, BG) = FV^\alpha \cdot BG^{1-\alpha}$. Demand for FV is given by $FV^* = \alpha \cdot \frac{M}{p_F}$, where p_F is the price of FV and $\$M$ is the money dedicated to spending on the two goods. When we switch from subsidizing FV to offering a choice between FV and BG subsidies, one of two things will happen. Either the shopper selects the FV subsidy, and p_F and FV^* are unchanged, or the shopper selects the BG subsidy, which increases p_F , and thus decreases FV spending. This optimization process is demonstrated graphically in Appendix Figure A.1. Here, we show these budgets and the utility-maximizing indifference curves for two individuals: one with $\alpha = 0.75$ who will choose the FV subsidy, and one with $\alpha = 0.25$ who will choose the BG subsidy. In Panel A we show how both individuals behave when faced with the FV subsidy, and in Panel B, we show how they respond to the subsidy choice. The consumer with $\alpha = 0.25$ selects the BG subsidy, and in doing so, substitutes to reduce FV spending from $F2$ to $F3$.

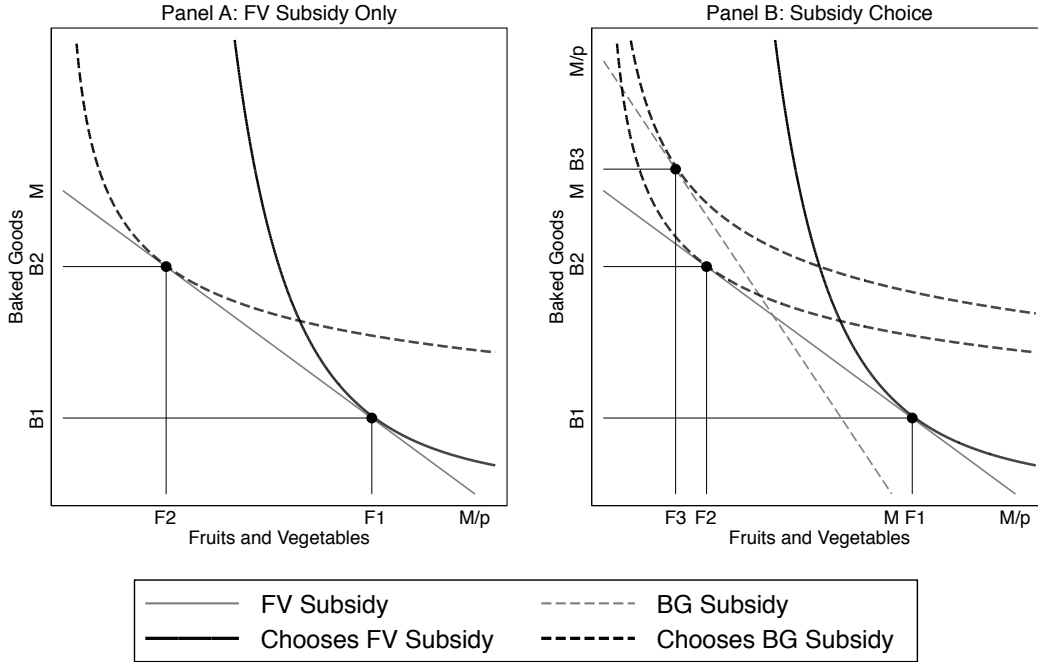


Figure A.1. Cobb-Douglas Utility Maximization Behavior from Subsidy Budgets

To consider a situation in which the consumer selecting the BG subsidy might increase their FV spending as a result, we need a utility function that allows for more complementarity. Consider the CES utility function: $U(FV, BG) = (\alpha FV^\sigma + (1 - \alpha)BG^\sigma)^{\frac{1}{\sigma}}$. Here, $\sigma = \frac{1}{1-\gamma}$ is the elasticity of substitution and α is the share parameter.

We use $M = 12$, which is roughly the average sum of FV and BG spending for shoppers that do not receive a subsidy, and the subsidy price of \$0.70 (as opposed to an unsubsidized price of \$1) that we use in the study.

The set of parameters that allows subsidy choice to result in increased FV spending is small and shown in Figure A.2. Shoppers will not select the BG subsidy unless $\alpha < 0.5$. However, there is no non-zero elasticity of substitution for which the selection of the BG subsidy will increase FV spending until the share parameter falls to about 0.1. Once the share parameter is in that range, low elasticities will permit this, although there is no non-zero share parameter for which this will happen if the elasticity of substitution is 0.5 or higher.

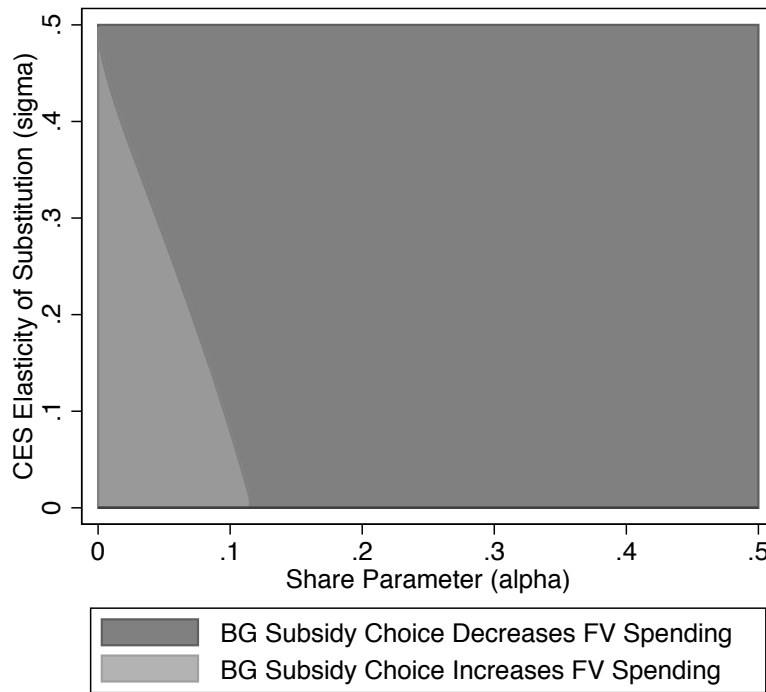


Figure A.2. Impact of Subsidy Choice on FV Spending by CES Parameters

A.2 Impact of Treatments on Total Food Spending

To consider the income effect, we look at the total spending on food items in the pooled subsidy groups. We do not observed a statistically significant spending increase, meaning that our effects are largely driven by substitution towards FV.

Table A.1. Effect of Subsidy Treatments on Total Food Spending (\$)

Control Mean [SD]: 51.37 [50.21]	(1)	(2)	(3)
Subsidy	0.99 (3.83)	0.99 (3.87)	1.50 (3.98)
Baseline Survey FV Spending (\$)		0.12*** (0.03)	0.12*** (0.03)
Part 1, Part 2 Fixed Effects	Y	Y	Y
Wave Fixed Effects	N	N	Y
Observations	2767	2767	2767
Clusters	805	805	805

*** $\Rightarrow p < 0.01$. Standard errors in parentheses are clustered at the shopper level. All specifications are linear random effects models.

A.3 Impact of Treatments on BG Spending and Spending Fractions

Appendix Table A.2 shows in Columns 1-3 that the impact of allowing BG subsidy choice on BG spending ranges from \$1.07 to \$1.51 across specifications. The difference-in-differences impact of agency by BG vs. FV spending is not statistically significant, despite the natural prior here would be that allowing for the BG subsidy increases BG spending by more than it increase FV spending.

A consequence of the increases in both FV and BG spending that result from agency and waiting periods is that there is a muted effect of these interventions on the fraction of food spending devoted to FV. Columns (4)-(6) of Table A.2 shows these estimates. Indeed, to the extent that agency or waiting periods increase the fraction of spending on FV, it appears to occur through the Waiting Period (Early Choice) treatment, which has a positive and significant effect on the fraction of food spending on FV.

Table A.2. Effect of Treatments on BG Spending (\$) and Spending Fractions

	Total BG Spending			Fraction Spent on FV		
	(1)	(2)	(3)	(4)	(5)	(6)
Subsidy	-1.06 (1.02)	-0.25 (0.94)	-1.05 (1.02)	0.150*** (0.023)	0.152*** (0.018)	0.150*** (0.023)
Agency	1.07** (0.66)		1.30* (0.68)	0.007 (0.022)		0.004 (0.024)
Waiting Period		1.02** (0.43)	0.65 (0.45)		0.006 (0.016)	0.005 (0.018)
Baseline BG Spending (\$)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)			
Baseline FV Fraction (%)				0.157*** (0.043)	0.158*** (0.044)	0.158*** (0.044)
Control Mean [SD]:	8.00 [16.15]			12.9% [21.6]		
Part 1, Part 2 Fixed Effects	Y	Y	Y	Y	Y	Y
Wave Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	2767	2767	2767	2710	2710	2710
Clusters	805	805	805	793	793	793

*** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Standard errors in parentheses are clustered at the subject level. All specifications are linear random effects models. In Columns (4)-(6), we lose 22 observations and 1 subject relative to the count in Table 6 because of receipts with no food items. We lose another 35 observations and 11 subjects due to baseline survey receipts with no food items.

A.4 Impact of Treatments by Shopping Trip

We expect the treatment effects to be largest during the first shopping trip, and indeed they are. Figure A.3 reproduces Panel C of Figure 2 separately for each shopping trip using coefficients from a single model where treatment indicator variables from Study 1 are interacted with shopping trip indicator variables. We also include un-interacted shopping-trip indicator variables to allow FV spending in Control to vary by trip. We maintain the linear random-effects model with standard errors clustered at the shopper level, a control for baseline FV spending, and wave fixed effects. Unlike Panel C of Figure 2, we leave the vertical axis in \$ to avoid showing percentage effects with a base that shifts across panels. Mean control FV spending fluctuates non-monotonically: \$4.63 in in Trip 1, \$3.33 in Trip 2, \$4.61 in Trip 3, and \$3.48 in Trip 4. Therefore, in percentage terms, the smaller absolute effects in Trip 2 are actually similar to those in Trip 1, whereas the even smaller absolute effects in Trip 3 are substantially smaller than the others, and the effect sizes rebound somewhat in Trip 4.

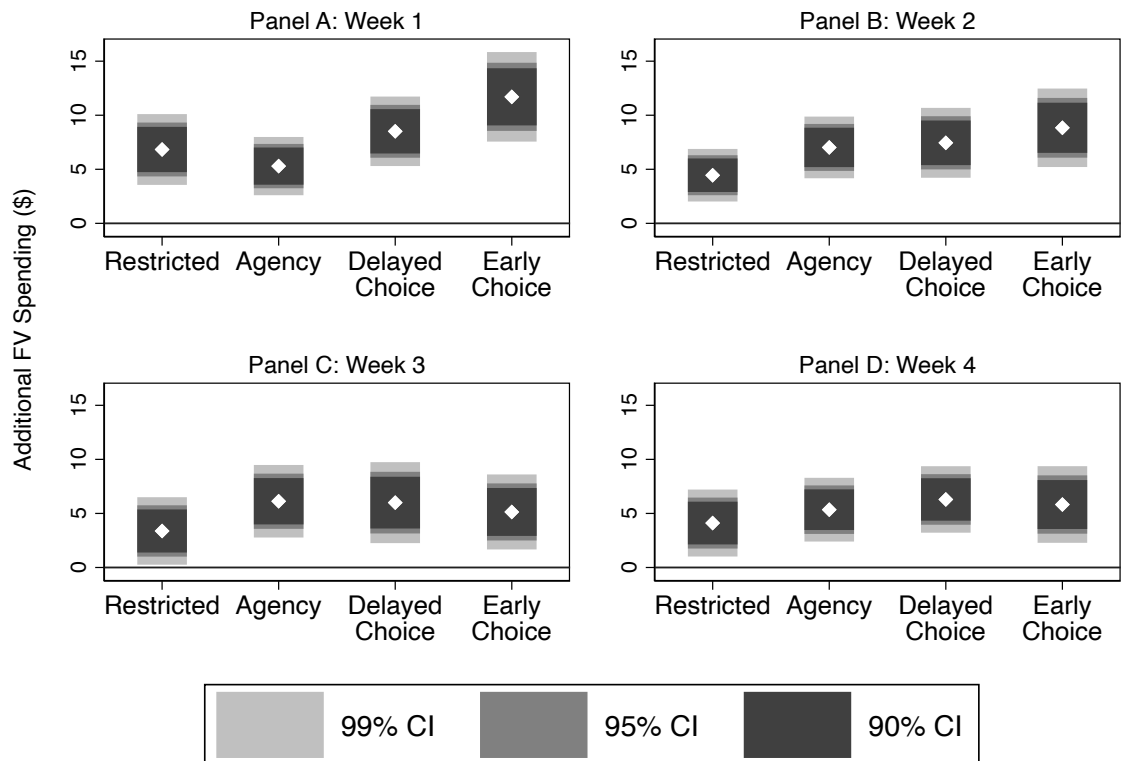


Figure A.3. Relative-to-Control FV Treatment Effects, Study 1

A.5 FV Spending of Shoppers Choosing BG Subsidy

We find that the average shopper in the agency treatment – pooled across those choosing the FV and BG subsidies – spends more on FV than without the subsidy or subsidy choice. While we focus on this pooled group for identification purposes, it is important to consider the 22% of households that select the less healthy BG subsidy; are they worse off in terms of FV spending than they were without the subsidy? If so, a subsidy choice program might not be desirable even if it has a positive average effect. In Figure A.4, we show the CDFs of FV spending in the control treatment and the agency treatments.

The agency treatments distribution dominates the control treatment distribution, indicating that the BG subsidy does not lead to lower FV spending than would prevail without any subsidy. In other words, if the BG subsidy lead to a substitution effect that lowered FV spending relative to control, and we assume that selection into the BG subsidy is monotonic in the FV spending level, the CDFs should cross at a density of 0.22: the fraction of the sample in the agency treatments selecting the BG subsidy. However, this is not the case.

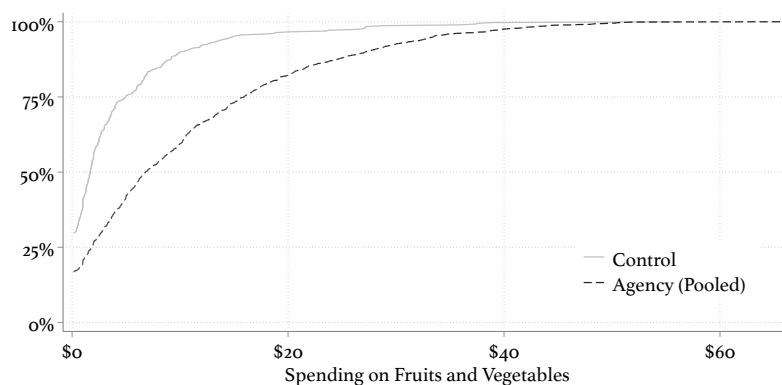


Figure A.4. Cumulative Distribution of FV Spending

A.6 Differential Effects by Welfare Recipient Status

While all shoppers in our study are from households with income under 185% of the FPL, that range still permits considerable variation across households in terms of wealth. If the subsidies and behavioral interventions are ineffective for the neediest households, especially those already receiving food assistance, then the scope for using these subsidies is smaller. While we do not have income information for many shoppers in the sample, we can measure whether the shopper is a participant in a welfare program. There are two ways we can do this. First, shoppers are asked if they receive SNAP funds in the initial screening. Second, we can observe whether any transaction was paid for using Electronic Benefit Transfer (EBT) in our study. SNAP recipients may not use EBT every time they shop, and EBT can be used to access funds from programs other than SNAP. Therefore, we define a welfare recipient as someone who falls into either category. 55% of shoppers in our sample are identified as welfare recipients.

We find that welfare recipients are roughly 8% less likely to select the FV subsidy in the agency treatments ($p = 0.020$).³² To evaluate whether this difference in choice rates is substantial enough to make our interventions less effective at increasing FV spending, in Table A.3 we re-estimate a number of important treatment effects in the paper, allowing for a heterogeneous impact by welfare recipient status.

First, we note that when we split the sample in half, we lose power to identify strong, statistically significant effects of agency and waiting periods for just the non-welfare recipients, even though the effects remain sizeable. However, for welfare recipients, the effects are slightly larger, and statistically significant; agency increases FV spending by \$2.11 ($p = 0.019$, column (2)) on average, and a waiting period increases FV spending by \$1.93 ($p = 0.020$, column (3)). Without agency or a waiting period, welfare-recipients households spend roughly \$1 less on FV than non-recipients per trip, but

³²Estimate is from a linear regression of a shopper’s mean subsidy choice on their welfare-recipient status with study part and randomization wave fixed effects, and a control for baseline FV spending, with robust standard errors.

Table A.3. Heterogeneous Effect of Interventions on FV Spending by Welfare Status

Sample:	All	Subsidy	
	(1)	(2)	(3)
Welfare Recipient	0.023 (1.058)	-1.037 (0.979)	-1.020 (0.835)
Subsidy	7.178*** (0.023)		
Subsidy \times Welfare	-0.927 (1.254)		
Agency		1.693 (1.189)	
Agency \times Welfare		0.420 (1.303)	
Waiting Period			1.522* (0.914)
Waiting Period \times Welfare			0.409 (1.245)
Baseline DV	0.146*** (0.051)	0.160*** (0.059)	0.162*** (0.057)
Observations	2767	2379	2379
Clusters	805	700	700

*** $\Rightarrow p < 0.01$, * $\Rightarrow p < 0.10$. Standard errors in parentheses are clustered at the subject level. All specifications are linear random effects models that feature fixed effects for the parts of the study and randomization wave.

this difference is not statistically significant. The subsidy effect remains very large and statistically significant in column (1), and while it is slightly less effective for welfare recipients, the difference in effects is not significant. Overall, there is no significant evidence of differential effects by welfare-recipient status.

B Instructions

In this section, we include the instructions and protocols for each of the surveys that the shoppers may have seen. For the baseline survey, that protocol includes the elicitation of time preferences. Assignment occurred after the baseline survey. The first instructions shoppers received for their assigned treatment were given at the beginning of their survey for Shopping Trip 1.

All shoppers saw an introduction identical to the control group. The instructions for submitting pictures of receipts was also identical across treatments. Where the

individual treatments differed from the control group, we will indicate in their subsection.

B.1 Baseline

Hi Agents! This is the first survey of the 4-6-week grocery shopping study. You can take this survey anywhere. You are required to have the following BEFORE accepting this survey:

- A recent itemized receipt from a recent grocery shopping trip you took to a store with a produce section.
- A blank piece of paper and pencil/pen.

When you have both required items available, you can begin the survey. There are 10 questions and 2 photos.

Have fun!

Thank you for your interest in our study. Our study seeks to investigate grocery choices. The study will cover 6 weeks and will require that you submit receipts from your grocery purchases over that timeframe. You will submit these receipts in the form of a photo attached to a Field Agent Job. This Job may involve answering a series of questions both during your shopping trip and in between shopping trips.

The payment for participation is \$30 total (\$1 per survey with the balance paid at the end with the successful completion of all study requirements.

In addition to the participation payments, the majority of participants will have the opportunity to receive up to \$40 in free groceries.

It is very important that all participants complete all 6 weeks of the study, so please don't agree to participate unless you have time to finish the entire study.

As a bonus for completing this study, we have included three "Bonus Questions" that offer you a chance to earn additional money. We will randomly choose agents to receive this bonus money, so payment for this question is NOT guaranteed.

For these Bonus Questions, we are going to show you 3 different scenarios (2 now, and 1 at the end of this survey) and ask you to select your preferred option in each scenario.

For each Bonus Question you answer today, you will have about a 1 in 50 chance of winning the amount determined by your selection. So, treat each Bonus Question as if it will determine your actual bonus payment.

Additional money earned from any Bonus Question will be deposited directly into your Field Agent account. However, you will have a choice about when to receive this bonus.

As a reminder, you will be paid the full \$30 for successfully completing the full study no matter your answers to the bonus questions and whether or not you are selected for bonus payment.

1. **Bonus Question #1:** In this question, your bonus grows larger the longer you wait for it.

The earliest you can choose to receive your bonus is 1 week from today. If you choose to receive it 1 week from today, it will be \$50. If you choose to wait longer to receive your bonus—up to a maximum of 27 weeks from today—it will grow by some amount.

Below, there are a number of combinations of waiting times and bonus amounts that you can choose.

Which is your preferred option?

- (a) Receive \$50 in 1 week
- (b) Wait 2 weeks, receive \$53
- (c) Wait 3 weeks, receive \$54
- (d) Wait 5 weeks, receive \$55
- (e) Wait 7 weeks, receive \$56
- (f) Wait 9 weeks, receive \$57
- (g) Wait 11 weeks, receive \$58
- (h) Wait 13 weeks, receive \$59
- (i) Wait 16 weeks, receive \$60
- (j) Wait 19 weeks, receive \$61
- (k) Wait 23 weeks, receive \$62
- (l) Wait 27 weeks, receive \$63

2. **Bonus Question #2:** This question is similar to Bonus Question #1, except shifted by a week.

Here, you have the option of receiving the bonus today. Below are the combinations of waiting times and bonus amounts that you can choose from.

Which is your preferred option?

- (a) Receive \$50 right away
- (b) Wait 1 week, receive \$53
- (c) Wait 2 weeks, receive \$54
- (d) Wait 4 weeks, receive \$55
- (e) Wait 6 weeks, receive \$56
- (f) Wait 8 weeks, receive \$57
- (g) Wait 10 weeks, receive \$58
- (h) Wait 12 weeks, receive \$59
- (i) Wait 15 weeks, receive \$60
- (j) Wait 18 weeks, receive \$61
- (k) Wait 22 weeks, receive \$62
- (l) Wait 26 weeks, receive \$63

Great! For the next few questions, we want you to think about the group of people with whom you share a budget for food.

If you are a member of a family living together, this is probably the entire household. If you live with roommates and don't pool money together for food, this would mean just you.

In general, think of the entire group you plan for when you go grocery shopping. We will refer to this group as your food household from now on. We are now going to ask some questions about your food situation. Please provide an answer as it relates to your food household.

3. Which of these statements best describes the food eaten in your food household in the last 30 days?
 - (a) Enough of the kinds of food we want to eat
 - (b) Enough, but not always the kinds of food we want to eat
 - (c) Sometimes not enough to eat
 - (d) Often not enough to eat

4. In the last 30 days, how often did you worry about whether your food would run out before you got money to buy more?
 - (a) Almost always
 - (b) Most of the time
 - (c) About half of the time
 - (d) Some of the time
 - (e) Almost never

5. In the last 30 days, how often did you feel like your food household couldn't afford to eat well-balanced (healthy) meals because you couldn't afford it?
 - (a) Almost always
 - (b) Most of the time
 - (c) About half of the time
 - (d) Some of the time
 - (e) Almost never

6. Do you think your food household eats the right amount of fruits and vegetables?
 - (a) Yes, we eat the right amount
 - (b) No, we should eat more
 - (c) No, we should eat less

We are now going to ask some questions about your health situation.

7. How would you rate your physical health status?
- (a) Excellent
 - (b) Very good
 - (c) Good
 - (d) Fair
 - (e) Poor
8. Thinking about the past couple weeks, how often do you find that you have difficulty maintaining energy, focus, or attention?
- (a) Almost always
 - (b) Most of the time
 - (c) About half of the time
 - (d) Some of the time
 - (e) Almost never

Food Diary: You will need a piece of paper and a pencil for this activity. We would like to know what a typical day of food consumption is like for you. If there are any children in your food household, then we would also like to know what a typical day of consumption is like for one child. To make this easy, just think about the food you ate yesterday.

On a piece of paper, create four sections for yesterday's food: breakfast, lunch, dinner, and snacks. In each section, try to remember and list everything you ate. If you know the quantity you ate, please include it. If you do not remember the quantity, please still list the item.

If there are any children in your food household, please make a separate chart for one child.

See the example below, of what a food diary might look like. Once you complete your food diary, please take a readable photo of it. Make sure your photo is clear and in focus. Make sure that we can read your food diary entries.

My Food Diary

Breakfast	Lunch	Dinner	Snacks
Cheerios and milk	Pizza (2 slices)	Grilled chicken	Banana
Apple	Breadsticks	Baked potato	Popcorn (1 bag)
Coffee	Coke (1 can)	Mac and Cheese	
		Side salad with Ranch	
		Iced Tea	

Figure B.5. Example Food Diary

Shopping Receipt: In your study confirmation email, we asked you to begin saving your receipts from your grocery shopping trips. Please take a photo of an itemized receipt from your most recent Stock-Up grocery-shopping trip.

As a reminder, a “Stock-Up” shopping trip is a trip to a grocery store—somewhere that has a produce section with multiple aisles—where you purchased food or ingredients to prepare multiple meals at home.

Some additional requests:

- If your receipt is too long to fit into a readable photo, please neatly tear it into two or more sections to put next to one another for the photo.
- Make sure your photo is clear and in focus. Make sure that we can read the items listed on the receipt.
- Please take your picture with a vertical (portrait) orientation.

9. **Bonus Question #3:** Now that you’ve had a little more time to think about Bonus Question #2, you have another chance to make a selection from the same set of options in Bonus Question #2. This will not replace your answer to Bonus Question #2: it is a different question and your choice from it may count separately from Bonus Question #2.

Recall that, for this question, a \$50 bonus is available today. Also recall that if you wait to receive your bonus—up to a maximum of 26 weeks from today—it will increase as you wait longer.

Below are your options with different waiting times and bonus amounts that you can choose.

Taking time to think about it, which is your preferred option?

- (a) Receive \$50 right away
- (b) Wait 1 weeks, receive \$53
- (c) Wait 2 weeks, receive \$54
- (d) Wait 4 weeks, receive \$55
- (e) Wait 6 weeks, receive \$56
- (f) Wait 8 weeks, receive \$57
- (g) Wait 10 weeks, receive \$58
- (h) Wait 12 weeks, receive \$59
- (i) Wait 15 weeks, receive \$60
- (j) Wait 18 weeks, receive \$61
- (k) Wait 22 weeks, receive \$62
- (l) Wait 26 weeks, receive \$63

That is all the questions we have for you today. In the next few days, you will see the first grocery-shopping survey of the study in your Field Agent app under the heading “Just For You.” This survey will contain all of the instructions you will need to complete the study.

Make sure to OPEN AND READ THE INSTRUCTIONS for your next survey BEFORE your next grocery-shopping trip. This survey will include all instructions for the study. You do not have to alter your shopping schedule for this survey, just remember to begin your next survey before your next usual grocery shopping trip.

Remember that the store you go to MUST:

- Have a produce section with multiple aisles of fruits and vegetables.
- Provide itemized receipts.

If you have any questions or concerns about this study please email us at Support@Fieldagent.net. Put “Grocery Shopping Study” in the subject line.

See you soon!

B.2 Control

B.2.1 Common Introduction

Hi Agents!

Take this survey the next time you go on a grocery-shopping trip.

Remember that the store you go to must:

- Have a produce section with multiple aisles of fresh fruits & vegetables
- Provide an itemized receipt

WAIT until you arrive at the store to do this survey. If you are not at the grocery store right now, close the survey and re-open it when you arrive. If you are at the grocery store right now, please continue with the survey.

You cannot go backwards in the study, so read and answer all questions carefully.

Have fun!

B.2.2 Common Receipt Submission

Now, go ahead and complete your shopping trip. The next screen will ask for a picture of your itemized grocery receipt. Take a picture of the receipt before going home from the store (it’s fine to be in your car or the parking lot)

When you have finished shopping and have paid for your purchases continue to the next screen.

PHOTO 1: Take a photo of your itemized grocery shopping receipt while still at the store. You can be in your car, but DO NOT LEAVE THE LOT.

- The receipt must be clear and in focus – Your reimbursement is dependent upon the receipt being clear and in focus.
- We must be able to read all of the items that you purchased today.
- Make sure we can see the name of the store you shopped at.
- Make sure we can see the date and time of your purchase.

- Please take the picture with a vertical orientation.
- If the receipt is too big to capture in one photo, carefully tear the receipt and arrange it so that it can all fit into 1 picture.

Take a picture of your itemized grocery receipt.

That is all the questions we have for you today.

You must wait at least 5 days to complete your next grocery shopping trip survey.

If you have any questions, email us at Support@fieldagent.net before you submit this survey. Put “Grocery Shopping Study” in the subject line.

B.3 Restricted

[Begin with Common Introduction]

B.3.1 Common Restricted Subsidy

For this grocery-shopping trip, we will reimburse you for some of your groceries. Please read the instructions carefully.

Any item in the fruits and vegetables category will qualify for the partial reimbursement. Fruits and vegetables are defined below:

- Fresh fruits and vegetables
- Canned fruits and vegetables
- Frozen fruits and vegetables
- Processed or prepared products that simply contain some fruits and vegetables do not qualify. For example, frozen fruit qualifies, but frozen juice concentrate does not because of the added sugar. Cabbage qualifies, but prepared coleslaw does not because of the added mayonnaise. Canned tomatoes qualify but canned pasta sauce does not because of the added salt.

You will be reimbursed 30% of the price on all items on your receipt in the fruits and vegetables category, up to a maximum reimbursement of \$10.

For example:

- If you spend \$30 on fruits and vegetables, you will receive a reimbursement of $(30\% \text{ of } \$30) = \9 .
- If you spend \$25 on fruits and vegetables, you will receive a reimbursement of $(30\% \text{ of } \$25) = \7.50 .
- If you spend \$33.33 or on fruits and vegetables, you will receive the maximum reimbursement of \$10.

Your reimbursement will be credited to your Field Agent account within 7 days of this shopping trip. We will use the receipt you submit at the end of this survey to calculate your reimbursement.

Do you understand?

- Yes [Proceed to next question]
- No

[Continue with Common Receipt Submission]

B.4 Agency

[Begin with Common Introduction]

B.4.1 Common Choice Procedure

For this grocery-shopping trip, we will reimburse you for some of your groceries. You will need to choose which types of foods to be reimbursed for.

Please read the instructions carefully.

There are two food categories for you to choose from:

1. Fruits and vegetables
2. Baked goods

Here's a list of what falls into each category:

Fruits and Vegetables:

- Fresh fruits and vegetables
- Canned fruits and vegetables
- Frozen fruits and vegetables
- Processed or prepared products that simply contain some fruits and vegetables do not qualify. For example, frozen fruit qualifies, but frozen juice concentrate does not because of the added sugar. Cabbage qualifies, but prepared coleslaw does not because of the added mayonnaise. Canned tomatoes qualify but canned pasta sauce does not because of the added salt.

Baked Goods:

- Bread, biscuits and rolls
- Muffins, cakes and cupcakes
- Pies and tarts
- Crackers, chips, pretzels and other snacks do not qualify.
- In general, if you can buy it at a bakery, it counts. For example, sandwich bread qualifies, but a prepared sandwich does not. Muffins, English muffins and bagels qualify but a package of crackers does not.

For whichever category you choose, you will be reimbursed 30% of the price on all items on your receipt in that category, up to a maximum reimbursement of \$10.

For example:

- If you choose Fruits and Vegetables and spend \$30 on items in that category, you will receive a reimbursement of (30% of \$30) = \$9.
- If you choose Baked Goods and spend \$25 on items in that category, you will receive a reimbursement of (30% of \$25) = \$7.50.
- If you spend \$33.33 or more on items in your chosen category, you will receive the maximum reimbursement of \$10.

Your reimbursement will be credited to your Field Agent account within 7 days of this shopping trip. We will use the receipt you submit at the end of this survey to calculate your reimbursement.

Which category would you like to get the 30% reimbursement on? [*Randomized list*]

- Fruits and Vegetables
- Baked Goods

Now, go ahead and complete your shopping trip. The next screen will ask for a picture of your itemized grocery receipt. Take a picture of the receipt before going home from the store (it's fine to be in your car or the parking lot)

When you have finished shopping and have paid for your purchases continue to the next screen.

[Continue with Common Receipt Submission]

B.5 Waiting Period

B.5.1 Pre-Shopping Survey

Hi Agents! We have 2 short surveys for you for your next grocery-shopping trip of over \$25. We'll call these surveys 1) your "Planning Survey" and 2) your "Shopping Survey."

This is the Planning Survey (1 of 2).

- You **MUST** complete this survey 4 to 48 hours **BEFORE** you go grocery shopping.
- If you are already at the store, or will shop sooner than 4 hours from now, just wait to do this survey until the next grocery-shopping trip. We will wait for you.
- If you aren't going on a grocery shopping trip in the next 48 hours, close the survey and re-open it 4-48 hours before your next grocery trip.

DO NOT:

- Do not complete this survey in the parking lot of the store. Complete this survey at home. When you are at the store, you will complete the Shopping Survey.

WE WILL:

- Verify that you have completed this survey between 4 and 48-hours before your shopping trip, we'll compare the time you submit this Planning Survey to the timestamp on your shopping receipt.

NEXT:

- If you will complete a grocery-shopping trip in the next 4-48 hours, please continue with the survey.
- Remember that the store you go to must:
 - Have a produce section with multiple aisles of fruits and vegetables.
 - Provide itemized receipts.

You cannot go backwards in the study, so read and answer all questions carefully. Have fun!

For your upcoming grocery-shopping trip, we will reimburse you for some of your groceries. Any item in the fruits and vegetables category will qualify for the partial reimbursement. Fruits and vegetables are defined below:

Fruits and Vegetables:

- Fresh fruits and vegetables
- Canned fruits and vegetables
- Frozen fruits and vegetables
- Processed or prepared products that simply contain some fruits and vegetables do not qualify. For example, frozen fruit qualifies, but frozen juice concentrate does not because of the added sugar. Cabbage qualifies, but prepared coleslaw does not because of the added mayonnaise. Canned tomatoes qualify but canned pasta sauce does not because of the added salt.

You will be reimbursed 30% of the price on all items on your receipt in the fruits and vegetables category, up to a maximum reimbursement of \$10.

For example:

- If you spend \$30 on fruits and vegetables, you will receive a reimbursement of $(30\% \text{ of } \$30) = \9 .
- If you spend \$25 on fruits and vegetables, you will receive a reimbursement of $(30\% \text{ of } \$25) = \7.50 .
- If you spend \$33.33 or more on fruits and vegetables, you will receive the maximum reimbursement of \$10.

Your reimbursement will be credited to your Field Agent account within 7 days of this shopping trip. We will use the receipt you submit at the end of your Shopping Survey to calculate your reimbursement.

On your upcoming Shopping Survey:

You will be required to answer the first survey question BEFORE you finish shopping. Then, you can check out and take a photo of your receipt.

(NOTE: If you miss the 4-48-hour window, your Shopping Survey will direct you to a “Make-Up Planning Survey.” You will be required to re-take this Planning Survey and wait another 4-48 hours before shopping.)

Select “I understand” to indicate that you have read the instructions and understand the reimbursement offer.

B.5.2 Shopping Survey

Hi Agents! This is survey 2 of 2, your “Shopping Survey.”

COMPLETE THIS JOB IF:

- If it has been between 4-48 hours since submitting the Planning Survey (1 of 2), and you are at the grocery store. Accept this job and answer the first 2 questions BEFORE you shop for groceries and check out.
- If you missed the 4-48 hour time window – Life happens. We get it. Accept this survey and let us know. You will be directed on how to take the Make Up Surveys.
- If you still need to get your groceries now, that is perfectly fine. You will need to wait until your next grocery-shopping trip to complete the Make-Up surveys. We will wait for you.

WE WILL:

- Verify that you have answered the first two questions before purchasing your groceries.

NEXT:

- Remember that the store you go to must:
 - Have a produce section with multiple aisles of fresh fruits and vegetables
 - Provide an itemized receipt

You cannot go backwards in the survey, so read and answer all questions carefully. Have fun!

We asked you in the Planning Survey to wait a minimum of 4 hours and a maximum of 48 hours before shopping and completing this Shopping Survey. Tell us about how long it has been since you completed your Planning Survey for this week. An estimate is OK. We’ll double check for you.

If it has not been 4 hours, please wait to complete this Shopping Survey until it has been at least 4 hours.

If it has been longer than 48 hours (2 days), we need you to complete a Make Up Planning Survey. Don't worry, we'll direct you there.

About how long has it been since you completed your Planning Survey for this grocery shopping trip?

- 4-48 hours
- 48 hours or more

Great! Let's get shopping.

[Continue with Common Restricted Subsidy]

[Continue with Common Receipt Submission]

B.6 Waiting Period with Agency

B.6.1 Pre-Shopping Survey

Hi Agents! We have 2 short surveys for you for your next grocery-shopping trip of over \$25. We'll call these surveys 1) your "Planning Survey" and 2) your "Shopping Survey." This is survey 1 of 2: your Planning Survey.

Each week you will take this Planning Survey at least 4 hours, and no more than 48 hours BEFORE you head to the grocery store for a grocery shopping trip to purchase food. Remember that the store you go to must:

- Have a produce section with multiple aisles of fruits and vegetables.
- Provide itemized receipts.

You MUST complete this Planning Survey at least 4 hours BEFORE you go to the store for your shopping trip. Do not wait to complete this survey in the parking lot of the store. When you are at the store, you will complete the Shopping Survey. If you are already at the store, or will shop sooner than 4 hours from now, close the survey and re-open it 4-48 hours before your next Stock Up grocery trip. If you aren't going on a Stock Up grocery shopping trip in the next 48 hours, close the survey and re-open it 4-48 hours before your next Stock Up grocery trip.

To verify that you have completed this survey between 4 and 48-hours before your shopping trip, we'll compare the timestamp on your shopping receipt to the time you submit this Planning Survey. If you will complete a grocery shopping trip in the next 4-48 hours, please continue with the survey.

You cannot go backwards in the study, so read and answer all questions carefully. Have fun!

For your upcoming grocery-shopping trip, we will reimburse you for some of your groceries. When you begin your Shopping Survey this week, you will need to choose which types of foods to be reimbursed for. Please read the instructions carefully.

There are two food categories for you to choose from:

1. Fruits and vegetables
2. Baked goods

Here's a list of what falls into each category:

Fruits and Vegetables:

- Fresh fruits and vegetables
- Canned fruits and vegetables
- Frozen fruits and vegetables
- Processed or prepared products that simply contain some fruits and vegetables do not qualify. For example, frozen fruit qualifies, but frozen juice concentrate does not because of the added sugar. Cabbage qualifies, but prepared coleslaw does not because of the added mayonnaise. Canned tomatoes qualify but canned pasta sauce does not because of the added salt.

Baked Goods:

- Bread, biscuits and rolls
- Muffins, cakes and cupcakes
- Pies and tarts
- Crackers, chips, pretzels and other snacks do not qualify.
- In general, if you can buy it at a bakery, it counts. For example, sandwich bread qualifies, but a prepared sandwich does not. Muffins, English muffins and bagels qualify but a package of crackers does not.

For whichever category you choose, you will be reimbursed 30% of the price on all items on your receipt in that category, up to a maximum reimbursement of \$10.

For example:

- If you choose Fruits and Vegetables and spend \$30 on items in that category, you will receive a reimbursement of $(30\% \text{ of } \$30) = \9 .
- If you choose Baked Goods and spend \$25 on items in that category, you will receive a reimbursement of $(30\% \text{ of } \$25) = \7.50 .
- If you spend \$33.33 or more on items in your chosen category, you will receive the maximum reimbursement of \$10.

Your reimbursement will be credited to your Field Agent account within 7 days of this shopping trip. We will use the receipt you submit at the end of your Shopping Survey to calculate your reimbursement.

We will ask you at the store which category you would like to receive the 30% reimbursement for (Baked goods or Fruits & Vegetables).

You will be required to answer that question BEFORE you finish shopping. Then, you can check out and take a photo of your receipt.

(NOTE: If you miss the 4-48-hour window, your Shopping Survey will direct you to a “Make-Up Planning Survey.” You will be required to re-take this Planning Survey and wait another 4-48 hours before shopping.)

Select “I understand” to indicate that you have read the instructions and understand the reimbursement offer.

B.6.2 Shopping Survey

Hi Agents! This is survey 2 of 2, your “Shopping Survey.” Take this survey once you are at the grocery store for your grocery shopping trip. You can go to any grocery store you typically shop at as long as it has a produce section and provides itemized receipts.

Remember that the grocery store you go to must:

- Have a produce section with multiple aisles of fruits and vegetables.
- Provide itemized receipts.

WAIT until you arrive at the store to do this survey. If you are not at the grocery store right now, close the survey and re-open it when you arrive. If you are at the grocery store right now, please continue with the survey and answer the questions BEFORE you finish shopping.

If you have missed your 4-48 hour window, you will be directed on how to take a Make Up Planning Survey 4-48 hours before a Make-Up Shopping Survey. If you still need to complete this shopping trip, that is perfectly fine, but wait until your next grocery shopping trip to complete the Make-Up surveys. Accept the Make-Up survey for further directions.

You cannot go backwards in the study, so read and answer all questions carefully. Have fun!

We asked you in the Planning Survey to wait a minimum of 4 hours and a maximum of 48 hours before shopping and completing this Shopping Survey. Tell us about how long it has been since you completed your Planning Survey for this week. An estimate is OK. We’ll double check for you.

If it has not been 4 hours, please wait to complete this Shopping Survey until it has been at least 4 hours.

If it has been longer than 48 hours (2 days), we need you to complete a Make Up Planning Survey. Don’t worry, we’ll direct you there.

About how long has it been since you completed your Planning Survey for this grocery shopping trip?

- 4-48 hours
- 48 hours or more

Great! Let's get shopping.

[Continue with Common Choice Procedure]

[Continue with Common Receipt Submission]

B.7 Commitment

B.7.1 Pre-Shopping Survey

Hi Agents! We have 2 short surveys for you for your next grocery-shopping trip of over \$25. We'll call these surveys 1) your "Planning Survey" and 2) your "Shopping Survey." This is survey 1 of 2: your Planning Survey.

Each week you will take this Planning Survey at least 4 hours, and no more than 48 hours BEFORE you head to the grocery store for a grocery shopping trip to purchase food. Remember that the store you go to must:

- Have a produce section with multiple aisles of fruits and vegetables.
- Provide itemized receipts.

You MUST complete this Planning Survey at least 4 hours BEFORE you go to the store for your shopping trip. Do not wait to complete this survey in the parking lot of the store. When you are at the store, you will complete the Shopping Survey. If you are already at the store, or will shop sooner than 4 hours from now, close the survey and re-open it 4-48 hours before your next grocery trip. If you aren't going on a grocery shopping trip in the next 48 hours, close the survey and re-open it 4-48 hours before your next grocery trip.

To verify that you have completed this survey between 4 and 48-hours before your shopping trip, we'll compare the timestamp on your shopping receipt to the time you submit this Planning Survey. If you will complete a grocery shopping trip in the next 4-48 hours, please continue with the survey.

You cannot go backwards in the study, so read and answer all questions carefully.

Have fun!

For your upcoming grocery-shopping trip, we will reimburse you for some of your groceries. In this survey, you will need to choose which types of foods to be reimbursed for.

[Continue with Common Choice Procedure]

B.7.2 Shopping Survey

We asked you in the Planning Survey to wait a minimum of 4 hours before shopping and completing this Shopping Survey. Tell us about how long it has been since you completed your Planning Survey for this week. An estimate is OK. We'll double check for you.

If it has not been 4 hours, please wait to complete this Shopping Survey until it has been at least 4 hours.

If it has been longer than 48 hours (2 days), we need you to complete a Make Up Planning Survey. Don't worry; we'll direct you there.

About how long has it been since you completed your Planning Survey for this grocery shopping trip?

- 4-48 hours
- 48 hours or more

Great! Let's get shopping.

For this grocery-shopping trip, we will reimburse you for some of your groceries. You previously chose which types of foods to be reimbursed for. It is important that you remember this correctly for us to reimburse you.

Which of the two categories did you choose to be reimbursed for?

1. Fruits and vegetables
2. Baked goods

Here's a list reminding you of what falls into each category:

Fruits and Vegetables:

- Fresh fruits and vegetables
- Canned fruits and vegetables
- Frozen fruits and vegetables
- Processed or prepared products that simply contain some fruits and vegetables do not qualify. For example, frozen fruit qualifies, but frozen juice concentrate does not because of the added sugar. Cabbage qualifies, but prepared coleslaw does not because of the added mayonnaise. Canned tomatoes qualify but canned pasta sauce does not because of the added salt.

Baked Goods:

- Bread, biscuits and rolls
- Muffins, cakes and cupcakes
- Pies and tarts
- Crackers, chips, pretzels and other snacks do not qualify.
- In general, if you can buy it at a bakery, it counts. For example, sandwich bread qualifies, but a prepared sandwich does not. Muffins, English muffins and bagels qualify but a package of crackers does not.

For whichever category you chose, you will be reimbursed 30% of the price on all items on your receipt in that category, up to a maximum reimbursement of \$10.

Your reimbursement will be credited to your Field Agent account within 7 days of this shopping trip. We will use the receipt you submit at the end of this Shopping Survey to calculate your reimbursement.

[Continue with Common Receipt Submission]