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Saving by buying ahead: stockpiling in response to lump-sum payments

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Abstract
By purchasing larger quantities of goods and saving them for future consumption households are able to reduce transaction costs and acquire goods at a lower price per unit, presuming they can manage the transportation and storage costs. This study uses variations in state income tax refunds over time to estimate consumption responses to lump-sum payments. Households purchase around 20 per cent more of easily stored toilet paper in the months in which tax refunds are issued, but do not increase purchases of perishables such as bread and eggs. In addition to purchasing more goods at a lower per-unit price, households also appear to increase the time until their next purchase, which implies that they are saving goods for consumption over time. These in-kind savings allow people to smooth their consumption over time, much like pecuniary savings. Government payments that provide lump-sum payments can benefit consumers by providing additional liquidity to buy and store goods at a lower cost.

KEYWORDS
Consumption smoothing, stockpiling, saving, liquidity

JEL CLASSIFICATION
D1, D12, D14, D15

1 | INTRODUCTION

Typically, household finance models treat expenditures for non-durable goods as purchases that are consumed immediately; any inventory is incidental and transitory. As Baker, Johnson and Kueng (2020b) describe, inventories are an important factor for the finances of firms, but relatively understudied in the context of households. Indeed, the COVID-19 pandemic highlighted the importance of understanding how households manage short-run fluctuations in their liquidity levels to sustain consistent levels of consumption.[1]

Prior studies show that households engage in some stockpiling in response to lower prices[2] or in anticipation of supply shortages.[3] Other work examines how household purchases respond to changes in income, especially heterogeneity in the marginal propensity to consume non-durables.[4] In general, studies show that lower-income households tend to consume most of an unexpected lump-sum positive income shock, and even an expected one. They also are less responsive to anticipated supply shortages.[5] This is consistent with these households living 'hand to mouth' with little to no savings and no access to credit or other forms of liquidity to smooth consumption.

Saving storable goods, however, may serve as an otherwise unobserved form of consumption smoothing. Buying ahead, storing, and then consuming these goods are much like saving a financial asset in an account for future expenditures. To the extent that households can use liquidity to also purchase larger quantities at a lower per-unit cost, households can reach higher levels of consumer welfare within the same budget constraints. However, the costs of acquiring, transporting and storing a good are important considerations – the benefits of storing goods must outweigh these costs, otherwise households should simply save in a bank account.[6]

Griffith et al. (2009) document that lower-income households are less likely to purchase in bulk than those in the middle of the income distribution. Orhun and Palazzolo (2019) conclude that this is in part due to liquidity constraints among lower-income consumers. Given the low level of savings among low-income households,[7] government payments can relieve these consumers from cash liquidity constraints so that they can build up an inventory for the future *and* take advantage of lower per-unit costs. There is limited evidence on this phenomenon, however. Indeed, while prior studies show that reductions in benefit generosity are associated with household-level consumption hardships,[8] hardships are less likely to occur among people with at least some savings.[9] Stockpiles may serve an important function for households to facilitate minimum levels of consumption without relying on ongoing expenditures. The liquidity provided by lump-sum payments may also allow low-income households opportunities to expand consumption by buying at a lower per-unit cost.

Using rich transaction-level panel data, we observe purchases from grocery stores and other consumer retail stores at the shopping-trip level from 2004 to 2012 and across the US. Importantly, in certain years, certain households became eligible for more generous state earned income tax credit (EITC) supplement payments. We use data at the shopping-trip level to estimate purchases during the months that state tax refund payments are distributed. By tracking the same households over time, we can compare changes in spending patterns when households are eligible for larger state supplement payments compared with ineligible households in the same time periods and locations. We estimate spending changes based on variation in (1) states introducing an EITC supplement or changing the generosity of a state supplement, and (2) households becoming eligible through a change in income, a change in the number of children, or a change in marital status.

We focus on a ubiquitous good with few substitutes – toilet paper. We find that households increase the amount of toilet paper purchased by 20 per cent in response to a larger state income tax refund, while spending increased at a lower rate, a savings in the cost per unit. The increase occurs during one shopping trip during the months when state income tax refunds payments are distributed to households

[1] Baker et al., 2020a.

[2] Mela, Jedidi and Bowman, 1998; Hendel and Nevo, 2004, 2006, 2013.

[3] O'Connell, de Paula and Smith, 2021; Amaral, Chang and Burns, 2022.

[4] Agarwal, Liu and Souleles, 2007; Jappelli and Pistaferri, 2010; Parker et al., 2013; Kueng, 2018.

[5] O'Connell et al., 2021; Amaral et al., 2022.

[6] Anton and Varma, 2005; Garrod, Li and Wilson, 2019.

[7] Bhutta et al., 2020.

[8] See, for example, Gruber (1997), Schmidt, Shore-Sheppard and Watson (2016) and Kondratjeva et al. (2021).

[9] Browning and Crossley, 2009.

(February and March). Households are then less likely to make more purchases until that supply is consumed. We also find increases in the quantity of canned goods and paper products purchased, although these are smaller in magnitude. In contrast, we do not find similar purchasing behaviour for bread and eggs, items that are also ubiquitous, but perishable and hard to store.

Orhun and Palazzolo (2019) estimate intra-month bulk purchasing of toilet paper, showing that lower-income consumers are less likely to take advantage of sales (i.e. a temporary price reduction). Our paper expands on that study by exploiting variation in annual government payments due to year-to-year changes in eligibility for government payments, rather than intra-month cash flows. We are able to show the magnitudes of this response to changes in eligibility for a lump-sum payment. We also estimate falsification tests with perishable products, and among households who were not eligible for payments. This approach allows us to dig deeper into the mechanisms of the purchasing behaviour of low-income households.

The typical household's state EITC supplement is just under $600, about the same as total monthly spending – effectively, these payments double a household's potential consumption within a month. Our data mainly cover grocery store shopping, which means that there are other consumption responses we cannot observe. Saving stored goods is an economically meaningful savings channel for households when they receive government payments – potentially an important insight for researchers and policymakers.

Unlike prior studies that estimate the marginal propensity to consume when a household has a large financial windfall, this study focuses on how a relatively small increase in a household's spendable income shifts spending within a product group towards larger quantities. The motivation for consumers to make this shift, when they have the money to afford to do so, is twofold. First, they can spend money now for an inventory they can consume over time. Second, they can lower their per-unit costs by purchasing larger quantities. We show that consumers do indeed shift their spending towards stockpiled goods, and to the extent that low-income consumers engage in strategic shopping, prior work may underestimate the welfare consequences of changes to benefit payments, as well as income shocks in general. Moreover, to the extent that low-income households' liquidity constraints prevent buying larger quantities, these results may inform the mechanisms behind measured consumption inequality across the income distribution, including shifts that are not easily observed in aggregate data.

Government payments are one channel by which consumers can access liquidity to buy goods and save them for future consumption. The phenomenon we document in this study suggests policymakers should carefully consider the level, and also the timing and frequency of transfer payments – smaller regular payments may support ongoing expenditures, but not facilitate households to buy goods in larger quantity, at a lower per-unit cost, providing a stockpile for later use. By reducing liquidity constraints, larger lump-sum payments can help households to better maximise their utility over time.

The remainder of this study proceeds as follows. In Section 2, we highlight predictions from a simple model of bulk purchasing. In Section 3, we introduce the data, and in Section 4, we lay out our empirical strategy. In Section 5, we discuss results and robustness. In Section 6, we offer a discussion of the implications of these results.

2 | CONSUMPTION OF STORED GOODS

We develop a simple framework for buying storable goods to model a set of testable empirical predictions.[10] Toilet paper is an example of a universally purchased, essential and storable item. In our data, households buy about 18 rolls of toilet paper per month on average. Buying 36 rolls of toilet paper costs $37.38 when buying in packs of six, but only $32.16 when buying in packs of 12. Buying in larger sizes amounts to a 14 per cent lower cost per unit.

[10] See Baker et al. (2020b) for a similar set-up.

We assume that households have three options to allocate a lump-sum payment: (1) a storable consumption good, c; (2) a financial asset, a; (3) a non-storable numeraire good, n, normalised to the price of 1. We assume that households consume a constant amount of the storable consumption good c in each period, that this is a normal good with respect to changes in income, and that households do not have any uncertainty about their future consumption or prices. Note that c is available in two sizes: there is a smaller size that amounts to one period's consumption, available at a per-unit price p_s and a larger size that amounts to two periods' consumption that is available at a per-unit price p_l. Therefore, households can decide to purchase both periods' consumption in the first period (purchase $2c$ at $2p_l$) or purchase each period's consumption sequentially (purchase c at p_s each period). To recreate the fact that larger quantities come at a cheaper per-unit price, we assume that $p_l < p_s$. Households can store the excess amount of c if they incur a storage cost, $g(c)$.[11] Because this model is not about the effect of promotions on purchasing behaviour, prices are deterministic in this model.

Each time a household makes a trip to the store to purchase c or n, they must incur a fixed trip cost, t. The financial asset a yields an interest rate r in the next period. Buying a also incurs a fixed transaction cost, b. Households receive income Y every period and discount the future by β.[12] Households' lifetime utility over the two periods is given by the following expressions. If they choose to purchase the smaller (non-bulk) size, it is given by

$$\begin{aligned} U_{\text{no bulk}} = {} & u(c, Y - a - p_s c) - I\{c > 0 \text{ or } n > 0\}\, t - I\{a > 0\}\, b \\ & + \beta\left[u(c, Y + a(1+r) - p_s c) - I\{c > 0 \text{ or } n > 0\}\, t\right]. \end{aligned} \tag{1}$$

If they choose the bulk size, it is given by

$$\begin{aligned} U_{\text{bulk}} = {} & u(c,\ Y - a - 2p_l c) - g(c) - I\{c > 0 \text{ or } n > 0\}\, t - I\{a > 0\}\, b \\ & + \beta\left[u(c,\ Y + a(1+r)) - I\{c > 0 \text{ or } n > 0\}\, t\right]. \end{aligned} \tag{2}$$

If there are no savings (i.e. $a = 0$), then the small size is chosen ($U_{\text{no bulk}} > U_{\text{bulk}}$) if the following inequality holds,[13]

$$u(c,\ Y - p_s c) - u(c,\ Y - 2p_l c)\ >\ \beta\left[u(c,\ Y) - u(c,\ Y - p_s c)\right] - g(c), \tag{3}$$

where the left-hand side of equation (3) represents the consumption gain in period 1 (Γ) and the right-hand side is the consumption loss in period 2, net of storage costs. Note that the term in the square brackets on the right-hand side of the inequality will always be greater than zero.

Trip costs drop out of this framework, as both size options require trips to the store in both periods to purchase the perishable good n. The remaining comparison implies that the smaller size is chosen when the consumption gain in period 1 from purchasing the smaller size outweighs the consumption loss in period 2, net of storage costs. If $p_l < 0.5{*}p_s$ (i.e. the bulk discount is so large that it is cheaper to buy two periods' worth of the storable good), then the only reason to choose the smaller size would be a sufficiently high storage cost as there would be no consumption gain in period 1.

We use variation in household eligibility for and availability of state EITC supplements as a random shock to income (τ), to estimate changes in purchasing behaviour. The shock increases income in the first period but not the second period.[14] The decision to choose the non-bulk size instead of the bulk

[11] Where there is a convex function with $g'(c) > 0$ and $g''(c) > 0$.

[12] A period in this study is a month but tax refunds occur annually within certain months. For simplicity, we assume that Y is the same in both periods; however, this can be relaxed.

[13] Storing goods is preferred to saving if the storage costs are small compared to the transaction costs of depositing funds in an account (b), and if interest rates are low compared to per-unit savings made by buying in larger quantities. We assume households are liquidity-constrained and have no savings in period 1. Note that the incentive to save ($a > 0$) is lower when purchasing the large size compared with the smaller size.

[14] A model with more than two periods could incorporate household expectations for their permanent income. However, the incentives to make bulk purchases would remain during months with greater liquidity. We discuss these mechanisms in Section 5.

size is now given by

$$u(c,\ Y+\tau-p_s c)-u(c,\ Y+\tau-2p_l c)>\beta\left[u(c,\ Y)-u(c,\ Y-p_s c)\right]-\ g(c)\,. \tag{4}$$

If there is diminishing marginal utility from n, then Γ is now smaller than in equation (3), making the non-bulk size less attractive. Households are therefore more likely to choose the bulk size. For income-constrained households, the bulk size may not have been affordable initially (i.e. $Y < 2p_l c$). With income increased by τ, it is possible that some households can now afford the bulk size that they were not able to afford before, and might choose to buy bulk, if storage costs are low and the price discount is large.[15] It is worth keeping in mind that the quantity of the numeraire good purchased in the first period will also likely increase. If income-constrained households are able to afford the amount of necessities (both perishable and non-perishable) that they consume each period and they consume necessities at a constant rate, then an increase in income such as τ would only lead to an increase in the purchase of the storable necessities as these can be carried over into the next period. We test for this behaviour in our empirical analysis. This simple model yields the following predictions.

- Buying large quantities of storable goods is attractive if storage costs are low, and per-unit savings for larger quantities are large.
- Stockpiling is a way for (risk-averse) households to smooth consumption. We therefore expect purchases of storable goods to increase in response to increases in tax refund payments. Additionally, expanding the household budget makes bulk sizes affordable for income-constrained households.

We next describe how we can use transaction data to test the predictions from this framework to analyse whether and to what extent households engage in stockpiling.

3 | DATA

We use the Consumer Panel Data from Nielsen through the Kilts Center for Marketing at the University of Chicago Booth School of Business for the years 2004–12. The data are a representative panel of households who use in-home scanners to record shopping trip purchases intended for personal, in-home use. These data are also used in other studies of consumption, especially at the daily or weekly level.[16] The quantity and price for all items are tracked for each shopping trip.

Table 1 shows the characteristics of all the 21 product modules that are bought by at least 70 per cent of households. Out of these, nine are further bought by at least 90 per cent of households. Each universal product code (UPC) is classified as part of a product module. There are 1,075 product modules, which are again grouped into 125 product groups and 10 departments. There are only four product modules that are bought by at least 95 per cent of households: dairy milk, fresh bread, fresh eggs and toilet paper. All of these product modules are bought on more than 60 per cent of trips, but only toilet paper is easily purchased in larger quantities and stored.

Toilet paper is ideal for this analysis as it is a widely purchased product with few complements, or substitutes. (Table A.1 in the online appendix shows all product modules that are purchased by at least 90 per cent of households.) The more heterogeneous a product module is, the larger is the scope for unobserved idiosyncratic household preferences. The worry with heterogeneity within modules stems from having many substitutes that households might choose. Aggregating across a widely varied set of products becomes more difficult because differentiating between substitutes in terms of quality and taste is challenging. Toilet paper has a relatively small number of UPCs associated with it; while not

[15] Note that, with linear utility in n, the trade-off between the two sizes is the same as it was before the shock.

[16] Harding, Leibtag and Lovenheim, 2012; Broda and Parker, 2014; Orhun and Palazzolo, 2019.

TABLE 1 Classification of goods in consumer panel data

Code	Product	X% of households buy: ≥70%	≥90%	≥95%	Non-perish.	Bought >60% months	UPCs
1042	Fruit drinks (non-canned)	✓			✓	✓	3,995
1209	Seafood: tuna, shelf stable	✓			✓		631
1290	Soup, canned	✓	✓		✓		3,337
1344	Cereal, ready to eat	✓	✓		✓	✓	3,859
1362	Cookies	✓	✓		✓	✓	10,728
1421	Peanut butter	✓			✓		1,046
1484	Soft drinks, carbonated	✓	✓		✓	✓	7,363
1487	Water, bottled	✓			✓	✓	3,375
1492	Candy, chocolate miniatures	✓			✓		585
1493	Candy, chocolate	✓	✓		✓	✓	4,509
1553	Soft drinks, low-calorie	✓			✓	✓	2,469
3603	Yogurt, refrigerated	✓					3,747
3625	Dairy milk, refrigerated	✓	✓	✓		✓	5,843
4000	Bakery: bread, fresh	✓	✓	✓		✓	10,074
4100	Eggs, fresh	✓	✓	✓		✓	2,710
7035	Soap, bar	✓			✓		1,551
7245	Facial tissue	✓			✓		856
7260	Toilet tissue	✓	✓	✓	✓	✓	1,127
7734	Paper towels	✓			✓		1,038
8420	Pain remedies, headache	✓			✓		3,147
8449	Deodorant, personal	✓			✓		1,617

Note: This table shows characteristics of product modules bought by at least 70 per cent of the households in our sample. The code in the first column refers to the Kilts product module code. The last column shows the unique number of UPCs belonging to a given product module.
Source: Kilts Nielsen Homescan Consumer Panel 2004–12.

quite a commodity, there is a high degree of standardisation within this product category. Aggregating over substitutes is non-trivial and could lead to measurement error. Some goods could show an increase in purchases as the result of increased consumption of complementary goods rather than stockpiling. Toilet paper, however, is a widely purchased, relatively homogeneous, standalone product with very few substitutes or complements.[17] We certainly would predict stockpiling behaviour within a household for other commonly purchased goods; the challenge is that households appear to have diverse preferences and there are very few items that most households purchase regularly. Therefore, we focus on toilet paper to demonstrate the importance of bulk purchases of storable goods as a way to smooth consumption, before briefly considering an average effect across all product modules.

4 | EMPIRICAL STRATEGY AND IDENTIFICATION

We employ a differences-in-differences-in-differences (DDD) strategy and household fixed effects to identify the effect of a larger tax refund on spending and quantity purchased of different types of products. The identifying variation comes from changes in household-level eligibility for state EITC

[17] Some products are measured in counts, others in weight or volume. Quantities of toilet paper are straightforward to aggregate in terms of ‘rolls’.

supplements. Leveraging this variation allows us to estimate the causal impact of a transitory income shock from a lump-sum tax refund on stockpiling.

In our analysis, we focus on predicting eligibility for state EITC supplements rather than exact amounts that households would receive. The average generosity in states that offer a state EITC supplement is roughly 16 per cent of the federal EITC amount, with several states around 5 per cent of the federal EITC amount and only three states (Vermont, Minnesota and Washington DC) offering more than 30 per cent of the federal EITC amounts. The duration for which these policies have been in place ranges from one to six years in 2014.[18]

As shown in Table 2, 22 states offered an EITC supplement on top of the federal EITC by 2012. To generate a set of households that are plausibly eligible or marginally eligible for EITC supplements, we restrict our sample to households with at most $50,000 in household income in the prior year. This limit captures almost all groups eligible for EITC even after accounting for household size, and excludes households that earn above a limit never eligible. We also restrict the sample to households that are employed and non-retired because eligibility for the EITC arises only with earned wages.

We have continuous data on every shopping trip over approximately three years for each household that is in the sample. We then aggregate these data at a monthly or bi-monthly frequency level. To begin, however, we provide descriptive statistics at the household–year level, which include 21,555 unique households and 57,991 household–year observations. We observe household size, the race and age of the household head, household income levels, and the type of household dwelling unit.[19] Given household income and the number of children, we can predict which households are eligible for tax credits. We do not know if a household actually received a tax refund, but we do know if the household is eligible for the EITC, and based on state of residence, if they are eligible for a state EITC if one exists in a given year.

Table 3 shows the combinations of numbers of children and income that we use to identify households eligible for the EITC in our sample.[20] The shaded cells indicate EITC eligibility. The modal eligible single household has one child and a household income of $15,000–$19,999 while the

TABLE 2 Changes in state EITC 2004–12

Year	New	Increase	No changes 2004–12
2004		MD	IL**, MA, ME**, MN, NY, OK, RI**, VT
2005		DC	
2006	DE**, VA**	ME**	
2007	NM	IA**, KS	
2008	LA, MI, NC	DC, MD, NE, NJ, NM, OR	
2009		IN, MI, NC, NJ	
2010		KS	
2011	CT		
2012			

Note: This table shows changes in state EITC policies between 2004 and 2012. ‘New’ means a state EITC policy was introduced in that state in a given year. ‘Increase’ means a state increased the generosity of its state EITC policy. Three states also decreased generosity between 2004 and 2012. These states were: New Jersey in 2010, Wisconsin in 2011 and Michigan in 2012. We do not show decreases here as we do not use the variation in our analysis. The last column shows all the states that had a state EITC policy in place during the entire period, but no changes were made to the policy. ** indicates non-refundable credits for at least one year.

[18] In our event study analysis, we test the parallel trends assumption and whether state EITC supplements may have been introduced in response to other factors.

[19] See online Appendix A for more detail.

[20] The matrix shows eligibility for both married and non-married households. In 2004, eligible cells were identical for both of these groups. Since 2008, eligibility became more generous for married households. Therefore, by the end of the sample period in 2012, there are additional eligible groups of married households shown in a lighter shade of grey.

TABLE 3 Eligibility

Income ($)	Number of children								Total
	0	1	2	3	4	5	6	7	
Panel A. Single households									
<14,999	3,762	480	223	61	14	10	1	0	4,551
15,000–19,999	3,199	491	179	34	12	3	1	1	3,920
20,000–39,999	16,638	2,453	983	222	53	6	6	1	20,362
40,000–44,999	5,213	884	281	73	21	4	2	0	6,478
45,000–50,000	4,648	681	247	62	14	1	2	0	5,655
Total	33,460	4,989	1,913	452	114	24	12	2	40,966
Panel B. Married households filing jointly									
<14,999	477	174	132	46	10	2	1	2	844
15,000–19,999	321	87	89	41	10	3	1	1	553
20,000–39,999	3,404	1,269	1,010	320	119	17	9	8	6,156
40,000–44,999	2,201	825	786	256	78	24	4	1	4,175
45,000–50,000	2,692	1,188	996	305	84	19	9	4	5,297
Total	9,095	3,543	3,013	968	301	65	24	16	17,025

Note: This table shows the number of individuals in our sample for different income range, household size cells, and marital status. The shaded cells reflect households that are eligible for EITC and state EITC policies. The last column and last row of each panel show the totals. Panel A shows eligibility criteria for single households, and panel B shows eligibility criteria for married households. For single households, there is no change in eligible groups according to this definition from 2004–12. In panel B, the darker grey cells show eligible married households throughout the entire sample period 2004–12. The lighter grey cells show additionally eligible groups by the end of the sample period in 2012. These changes were implemented gradually starting in 2008.

modal eligible married household has two children and a household income of $20,000–$39,999. Assuming a household in the middle of each bracket, in 2012, at the average state EITC rate of 16 per cent, the modal single household would receive $507 in state EITC supplements while the modal married household would receive $578.

Looking closely at the cells in Table 3, households not eligible for the EITC are the largest sample. However, most of the ineligible households can be considered 'marginally eligible'. For example, a household with annual income between $20,000 and $39,999 is not eligible if they have no children but will become eligible with one child even if their income is unchanged. There are only three cells in Table 3 that are more than one income group, or one child, away from being eligible for the EITC (only one cell for married households by 2012). Importantly for our identification strategy, households change eligibility status over time (see Section 5.3).

Table 4 shows that about 20 per cent of our household–year observations are EITC eligible in a given tax year. EITC households are comparable with non-EITC households in terms of race and ethnicity, but tend to be younger and have children. The average monthly spending of $560 implies that the state EITC supplement payment almost doubles a typical household's monthly potential spending.

Several studies examine household responses to state EITC supplements payments.[21] These studies all rely on an intent-to-treat framework based on EITC eligibility as shown in Table 2. [22] Similar to Goodman-Bacon and McGranahan (2008), McGranahan and Schanzenbach (2013) and Browning and Collado (2001), we compare the same households in months in which tax refund payments are typically received across years with varying tax refund payments. In addition to state tax law changes,

[21] See Kovski et al. (2022), Collin et al. (2021), Kondratjeva et al. (2021), Silveus and Stoddard (2020), Lenhart (2019) and Baughman and Duchovny (2016).

[22] The Tax Policy Center of the Urban Institute/Brookings Institution maintain a record of state EITC laws at https://www.taxpolicycenter.org/statistics/state-eitc-percentage-federal-eitc.

TABLE 4 Sample characteristics by EITC eligibility

	Not eligible		EITC eligible		Total	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Monthly spending	539.82	405.58	613.52	477.41	559.68	427.37
<$14,999	0.00	0.04	0.34	0.47	0.09	0.29
Between $15,000 and $19,999	0.08	0.27	0.08	0.27	0.08	0.27
Between $20,000 and $39,999	0.48	0.50	0.38	0.49	0.46	0.50
Between $40,000 and $44,999	0.20	0.40	0.13	0.33	0.18	0.39
Between $45,000 and $50,000	0.23	0.42	0.07	0.25	0.19	0.39
High school grad	0.30	0.46	0.39	0.49	0.33	0.47
College grad	0.34	0.47	0.31	0.46	0.33	0.47
Married	0.26	0.44	0.39	0.49	0.29	0.46
Single	0.34	0.47	0.19	0.40	0.30	0.46
Employed female	0.66	0.47	0.52	0.50	0.62	0.49
Part-time employed female	0.34	0.47	0.48	0.50	0.38	0.49
Employed male	0.79	0.41	0.76	0.43	0.78	0.42
Part-time employed male	0.21	0.41	0.24	0.43	0.22	0.42
Black	0.11	0.31	0.12	0.33	0.11	0.32
Asian	0.01	0.12	0.02	0.13	0.01	0.12
Hispanic origin	0.04	0.19	0.06	0.23	0.04	0.20
One member	0.62	0.49	0.22	0.41	0.51	0.50
Two members	0.28	0.45	0.24	0.43	0.27	0.44
Three members	0.05	0.23	0.25	0.44	0.11	0.31
Four members	0.01	0.10	0.24	0.43	0.07	0.26
Five or more members	0.04	0.19	0.05	0.21	0.04	0.19
Children	0.19	0.67	1.10	0.90	0.43	0.84
Aged under 25	0.00	0.06	0.00	0.06	0.00	0.06
Aged 25–34	0.06	0.23	0.09	0.28	0.06	0.25
Aged 35–44	0.17	0.37	0.28	0.45	0.20	0.40
Aged 45–54	0.36	0.48	0.44	0.50	0.38	0.48
Aged 55–64	0.37	0.48	0.26	0.44	0.34	0.47
Aged over 65	0.15	0.36	0.08	0.27	0.13	0.34
Observations	42,369		15,622		57,991	

Note: The table shows sample characteristics broken down by EITC eligible and non-eligible households as well as the overall sample. Employment status refers to the head of the household.

we observe households that have a change in payments due to within-household changes in annual income, as well as within-household changes in the number of children or marital status.

4.1 | Empirical strategy and identification

Treatment *SEITC* is defined as the interaction of three variables that are observable: year t, state $s(i)$ and eligibility $e(i)$ of household i. Note that t and s together define availability of a state EITC law in state s in year t. Eligibility, $e(i)$, is itself determined by a combination of household income, marital status and number of children. This results in a binary treatment indicator that takes on the value of 1 when a household is eligible and living in a state that has a state EITC supplement. Our baseline specification to estimate the impact of lump-sum payments on expenditures is

$$\begin{aligned} Y_{imt} = {} & \alpha SEITC_{ts(i)e(i)} + \beta X_{it} + \delta_t + \kappa_m + \tau_{s(i)} + \gamma_{e(i)} + \mu_{s(i)t} + \nu_{e(i)s(i)} \\ & + \eta_{te(i)} + \rho_{\text{module}} + \epsilon_{imt}, \end{aligned} \tag{5}$$

where Y_{imt} is the outcome of interest (log(spending), quantity, stockpile, inter-purchase time) for household i in year t and month $m \in$ {February, March, April}, $s(i)$ is the state that household i lives in, and $e(i)$ is household i's eligibility status for state EITC. ρ_{module} is a set of product module fixed effects. We run regressions at the monthly level, separately for the months February, March and April, as well as for all three months together, the entire period over which tax refunds arrive (we control for month fixed effects κ only when we run the tax period regressions). Prior research suggests nearly three-quarters of lower-income taxpayers receive their tax refunds by March.[23] April serves as a control month as most payments occur in February or March. This specification compares spending and purchases in the same month across years.

X_{it} contains characteristics for household i in year t that are relevant to purchasing decisions, including income levels, the number of household members as well as the type of housing and receipt of benefits from the Women, Infants and Children (WIC) welfare programme. Households' type of housing serves as a proxy for storage space.[24] Single-family homes tend to be larger than apartments or mobile homes, providing more storage space for goods. The marginal amount of space that a household has for storing more packages of goods is an important factor in a consumer's decision to stockpile goods.[25]

In the specification without household fixed effects, the DDD strategy first compares eligible to non-eligible households and then compares this difference by state EITC eligibility. This specification is quite flexible and allows us to control for fixed effects and their interactions. Δ_t and $\tau_{s(i)}$ are year and state fixed effects, respectively, and $\gamma_{e(i)}$ is an indicator for EITC eligibility. Shocks to consumption that hit the economy of the US overall in a particular year are absorbed by δ_t and do not bias α.

$\mu_{s(i)t}$, $\nu_{e(i)s(i)}$ and $\eta_{te(i)}$ are the interactions of the fixed effects. Perhaps most important are the state–year fixed effects, $\mu_{s(i)t}$, which capture differences across states that do and do not offer EITC payments. Other policies that might have similar eligibility criteria and exist in states that also have a state EITC are captured by $\nu_{e(i)s(i)}$. Finally, federal policies or other factors affecting the eligible group at a given time are controlled for by $\eta_{te(i)}$.

$\gamma_{e(i)}$ captures the effect of eligibility for the EITC. As eligibility for state and federal EITC follows the same criteria, $\gamma_{e(i)}$ can be thought of as capturing the time-constant intent-to-treat effect of federal EITC eligibility, while $\eta_{te(i)}$ can be interpreted as capturing the time-varying intent-to-treat effect of state EITC eligibility.

We also analyse the introduction of state EITCs in an event studies framework. This framework is specified in equation (6). This includes separate estimates for the years leading up to the state EITC policy and the years after the policy is introduced. Also, instead of using only EITC introductions, this estimate includes variation from both introductions of EITC supplements and increases in generosity, depending on the state.

[23] Farrell, Greig and Hamoudi, 2018.

[24] See Hossain (2020) for a discussion.

[25] Ching and Osborne, 2020.

$$Y_{imt} = \sum_{k\in K} \alpha_k SEITCInc_{t+k,s(i)e(i)} + \beta X_{it} + \delta_t + \kappa_m + \tau_{s(i)} + \gamma_{e(i)}$$
$$+ \mu_{s(i)t} + \nu_{e(i)s(i)} + \eta_{te(i)} + \rho_{\text{module}} + \epsilon_{imt}, \tag{6}$$

where $K = \{\leq -3, -2, -1, 0, 1, 2, \geq 3\}$. *SEITCInc* indicates the year in which a state either introduces or increases the generosity of a state supplement. For each state, we use the most recent event to define *SEITCInc*.

The α_k coefficients are interpreted as the effect of the EITC treatment relative to the period before the introduction of the state EITC supplement. This specification also allows us to examine whether the parallel trends assumption holds in the DDD framework for pre-periods.

The permanent income hypothesis predicts that after the first year of receiving a state EITC supplement, households should form expectations for the additional payment. If households rationally smooth consumption, then any increase in expenditures caused by the state EITC at tax filing time should be smaller each successive year.

The third and preferred specification that we use contains household fixed effects, which are only easily interpretable in regressions with a single product module; thus, we consider household fixed effects within that product module. The meaning becomes less clear in regressions containing multiple product modules, and hence θ_i is only estimated in specifications where we study a single product module. This also explains why we do not include product module fixed effects in equation (7),

$$Y_{imt} = \alpha SEITC_{ts(i)e(i)} + \beta X_{it} + \delta_t + \kappa_m + \tau_{s(i)} + \gamma_{e(i)} + \mu_{s(i)t}$$
$$+ \nu_{e(i)s(i)} + \eta_{te(i)} + \theta_i + \epsilon_{imt}, \tag{7}$$

where θ_i are household fixed effects. In this specification, we cannot identify parameters of fixed household characteristics as these are captured in θ_i. Every coefficient is identified from households for which those variables change. The household fixed effects results are our preferred results as they use variation *within* households and remove unobserved effects that are constant within a household. In particular, α is identified from households who change their treatment status for one of three reasons: a state introduces an EITC in that year, they become eligible because their income, marital status or number of children changed, or they move to a state with a state EITC.

Overall, this approach adds several dimensions that have not been addressed in prior studies. First, we focus on changes in the state EITC payments as a form of transitory income (although we also provide estimates for the federal EITC). Second, we can compare purchases by type of good in terms of spending as well as quantity, at both a shopping-trip level and over short periods when EITC payments are distributed. Finally, we can estimate expenditure changes in the data using a differences strategy, as well as an event study around the date of the state EITC change. These approaches provide more precise estimates of how consumers respond to EITC payments.

4.2 | Threats to identification

Several factors support the premise that these estimates can be interpreted as a causal effect of state EITC supplements on consumption. First, we show parallel trends of eligible households, compared with ineligible households, in states with a state EITC supplement versus those without a state EITC supplement using an event study (Section 5.6). While Jones and Michelmore (2018) find some degree of correlation between state EITC generosity and factors such as state unemployment rate and tax revenue from 1992–2013, we do not find such correlations in the subperiod from 2002 to 2012. We do, however, find a positive correlation with the number of Medicaid beneficiaries (see Table B.1 in the online appendix). We also do not find a strong correlation of the state EITC supplements with other benefit amounts (only a slight correlation with Supplemental Nutrition Assistance Program benefits), which makes us more confident that we are isolating state EITC supplements with our approach. Any national-level shocks in prices, unemployment or other factors would affect households across states and not be systematically correlated across states and within state EITC eligibility characteristics.

Second, it is unlikely that households accurately foresee changes in their state tax refunds due to changes in state tax laws. Given the small magnitude of these payments, people may not find it worthwhile to invest in obtaining the knowledge needed to understand changes in state EITC supplements.

Third, we perform several robustness tests on a set of households whose eligibility status switches solely due to the introduction of a state EITC supplement, thereby ruling out that effects are driven by households that might be systematically changing their filing status to receive state EITC benefits (see Section 5.3). Selection into the federal EITC is controlled for in our design by estimating the effect of eligibility status, $\gamma_{e(i)}$. Prior studies on the federal EITC show that people respond at the intensive margin of work to claim a larger EITC payment.[26] After a state implements an EITC supplement, the rate of claiming the federal EITC will also tend to increase.[27] In our analysis, however, we focus on eligibility for these payments not actual payment receipt. Also, any variation in self-employment or work hours choices in the prior calendar year would not have a direct effect on consumption patterns only during our focal months of February and March relative to the rest of the year.[28]

Fourth, we show that eligible and non-eligible households are similar in their characteristics (see Section 5). We also examine the selection of states into having a state EITC supplement (see online Appendix B). Only 22 states provide a state-level EITC supplement in 2012, so we maintain a substantial 'never treated' comparison group.

5 | RESULTS

5.1 | Main results

5.1.1 | Toilet paper estimates

Table 5 shows the results of a fixed effect specification for toilet paper purchases at the shopping-trip level. Toilet paper is measured in units (rolls) and the average number of rolls bought per trip is 12.1; a pack of 12 rolls of toilet paper averages \$5.84 in price (49 cents per roll). The main estimate of interest is α (from equation (7)), which can be interpreted as the response of household expenditures to the potential lump-sum payment from the state EITC. γ is also interesting as it captures the effect of overall federal EITC eligibility. Throughout, we cluster standard errors at the household level due to the panel nature of our data and because purchases are correlated within households.

Panel A of Table 5 shows the effect of state EITC eligibility on units or rolls of toilet paper purchased in each period. In March, there is an increase of about 2.4 toilet paper rolls per trip, compared to the mean of 12 rolls; this is an increase of 20 per cent.[29] At the top end of the confidence interval, we cannot rule out that six additional rolls of toilet paper are bought, which would imply going from a pack of 12 rolls to a pack of 18 rolls. Corresponding to this increase in the quantity

[26] For examples, see Kleven (2019) and Wilson (2022).

[27] Neumark and Williams, 2020.

[28] Iselin, Mackay and Unrath (2021) study a selected sample of California tax data to show that 99 per cent of federal EITC filers qualify for the state EITC supplement, and 78 per cent of parents who are eligible for the state EITC supplement claim it. The fact that some eligible households do not actually receive payments makes our estimates more conservative; we would not expect those who do not receive a payment to change their consumption, thereby reducing the overall estimated effect sizes.

[29] Our empirical strategy compares months of February, March and April across years with or without state EITC supplements. Farrell et al. (2018) show that February and March are the months in which nearly all EITC-eligible households receive their tax refunds. We therefore consider April to be a 'control' month that allows us to make use of the within-year variation in household purchases, similar to Kueng (2018), among others.

TABLE 5 Shopping-trip level, toilet paper purchases

	Panel A: units				Panel B: spending ($)				Panel C: days since last purchase				Panel D: inflow to stockpile			
	Feb–April	Feb	March	April	Feb–April	Feb	March	April	Feb–April	Feb	March	April	Feb–April	Feb	March	April
Eligible × EITC	0.734	0.687	2.360	−1.097	0.0559	−0.0864	0.658	−0.372	3.530	1.817	6.341	2.388	1.446	2.899	3.063	−1.321
	(0.607)	(0.723)	(1.322)	(1.060)	(0.233)	(0.292)	(0.296)	(0.344)	(1.374)	(2.555)	(2.267)	(1.996)	(0.939)	(1.261)	(2.082)	(1.694)
Eligible	0.0772	4.118	−2.705	3.340	0.243	3.641	0.0182	−0.214	8.960	61.23	−0.973	3.787	−2.063	−7.205	−5.411	11.20
	(1.812)	(1.638)	(1.709)	(5.232)	(1.026)	(0.636)	(1.621)	(2.750)	(13.57)	(14.35)	(9.287)	(22.70)	(3.011)	(4.710)	(5.535)	(6.007)
EITC	4.030	3.631	3.142	5.435	0.202	−0.422	0.637	0.400	0.268	−2.979	−6.554	3.803	6.768	9.277	3.000	7.960
	(1.855)	(2.974)	(2.755)	(2.492)	(0.659)	(0.989)	(1.174)	(0.898)	(5.033)	(8.823)	(8.261)	(6.723)	(2.806)	(4.816)	(3.904)	(4.854)
Two-family house	0.380	0.259	0.559	0.322	0.180	0.0900	0.254	−0.0315	−0.864	−2.086	1.443	−2.259	0.372	0.284	0.524	0.416
	(0.292)	(0.441)	(0.413)	(0.445)	(0.122)	(0.197)	(0.167)	(0.200)	(0.896)	(1.701)	(1.435)	(1.261)	(0.433)	(0.669)	(0.675)	(0.754)
Multi-family house	0.0469	0.528	0.169	0.0927	0.0976	0.115	0.188	0.143	0.0748	−0.487	0.867	0.308	−0.0701	0.523	−0.250	0.0169
	(0.276)	(0.331)	(0.446)	(0.442)	(0.106)	(0.153)	(0.132)	(0.167)	(0.712)	(1.244)	(1.075)	(1.060)	(0.424)	(0.522)	(0.757)	(0.763)
Mobile home/trailer	0.0757	−0.121	0.382	0.335	−0.0856	−0.245	−0.172	0.107	0.347	1.380	−0.104	−0.337	−0.652	−0.935	0.232	−0.520
	(0.349)	(0.377)	(0.648)	(0.579)	(0.115)	(0.165)	(0.162)	(0.215)	(0.803)	(1.410)	(1.309)	(1.227)	(0.550)	(0.633)	(1.091)	(0.864)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓				✓				✓				✓			
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State–year interaction	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State–income interaction	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	192,258	61,662	65,985	64,611	192,258	61,662	65,985	64,611	192,256	61,662	65,984	64,610	136,343	44,444	46,295	45,604
$\mathbb{E}(Y)$	12.23	12.12	12.38	12.19	5.87	5.85	5.91	5.86	42.35	43.91	41.71	41.58	−0.09	−0.46	0.24	−0.05

Note: The table shows the DDD analysis for toilet paper purchases at the shopping-trip level. The dependent variables are units (rolls) purchased, spending by trip, days since the last purchase and inflow to the stockpile. The inflow to the stockpile is calculated as follows. We regress monthly units of toilet paper on household fixed effects, which we interpret as average monthly consumption. The inflow to the stockpile is calculated as the difference between units purchased in a month and the average monthly consumption. The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

purchased, there is an increase in spending shown in Panel B of 66 cents (11 per cent). Given that, for the modal person, the state EITC amount is approximately equal to total monthly spending, this increase in spending on toilet paper seems reasonable: households are buying a larger quantity and spending relatively less on these purchases relative to the quantity.[30] This confirms our predictions from the simple model in Section 2.

Bell, Chiang and Padmanabhan (1999) suggest that stockpiling behaviour can be identified by increased purchase quantity along with increased inter-purchase times. Panel C tests this by using the days since the last purchase as a dependent variable. If there was no increase in these days, then this could indicate that households simply consumed more but did not store any stockpile. In Panel C, there is an increase of 6.3 days of the inter-purchase time – almost a week longer – or a 14 per cent increase in inter-purchase time relative to the mean.

Panels A, B and C show larger estimates in February for the state EITC supplement for eligible households (coefficient displayed in row 2 of Table 5). Eligibility criteria for state EITC and federal EITC are the same. If federal EITC receipts arrive slightly earlier than state EITC receipts, then this effect might be capturing the time constant increase in spending during tax time from the federal EITC relative to those households that are not eligible.[31] It also makes sense that these effects would be larger than the main treatment effect, as federal EITC amounts are larger than any state EITC payments.

Our simple model in Section 2 predicted that storage costs influence the decision to buy storable goods. Panel C shows that compared with living in a single-family house, households in other types of housing have fewer days between purchases (see Panel C of Table 5). This corresponds to our first prediction.

Finally, Panel D of Table 5 estimates the effect of state EITC eligibility on the inflow into storage at the monthly level. The actual stockpile of the household is unobserved, but the monthly household-level fixed effect models offer some helpful evidence.[32] Panel D shows that in February there is a statistically significant increase of three ‘extra’ rolls of toilet paper, defined as being more than the average monthly household consumption, but very close to zero in most other periods.

5.1.2 | Aggregated time period estimates

We continue with estimates of units and log spending at the monthly and biweekly level in Table 6. Like Table 5, these estimates include household-level fixed effects so that identification comes from within-household changes due to the introduction of state EITCs rather than the comparison of households in different states and with different eligibility statuses that might affect their consumption in unobserved ways. In addition to an increase in quantity in March, the aggregated quantity appears higher in February as well. There is an increase of roughly three rolls in February at the monthly level and March at the biweekly level. The fact that there is not much difference between these two coefficients suggests that the biweekly estimates capture the relevant increases in purchases. In March, there is roughly a 16 per cent increase in biweekly spending, but only an 11 per cent increase in monthly spending. This drives up the biweekly spending, but evens out over the entire month. Households appear to make one trip in which they buy a larger quantity of toilet paper as opposed to buying more across trips.

Figure 1 (‘No HH FE’ panel) shows spending and quantity effects (α) at the biweekly level from specifications without controlling for household fixed effects. Figure 2 does the same for monthly

[30] The average lump-sum state EITC refund for a married household with two children is \$578, and the average monthly total spending is \$613. The response in terms of toilet paper expenditures equates to a quantity elasticity of 16 per cent, and price elasticity of 10.4 per cent.

[31] It does appear that state refunds lag federal processes. See Aladangady et al. (2018) for a discussion.

[32] We regress monthly units of toilet paper on household fixed effects. We interpret this as average monthly consumption. The inflow is the difference between units purchased in a month and the average monthly consumption.

TABLE 6 Monthly and biweekly level, toilet paper purchases

	Panel A: monthly units (rolls)				Panel B: log monthly spending				Panel C: biweekly units (rolls)				Panel D: log biweekly spending			
	Feb–April	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**
Eligible × EITC	1.446	2.899	3.063	−1.321	0.0604	0.0777	0.110	−0.00867	1.309	1.848	3.264	−1.781	0.0589	0.0301	0.162	−0.0530
	(0.939)	(1.261)	(2.082)	(1.694)	(0.0332)	(0.0543)	(0.0552)	(0.0554)	(0.756)	(1.077)	(1.625)	(1.335)	(0.0322)	(0.0541)	(0.0489)	(0.0481)
Eligible	−2.063	−7.205	−5.411	11.20	0.00523	0.0718	−0.00428	0.285	−6.019	2.295	−6.685	−1.961	−0.157	0.489	−0.0413	−0.415
	(3.011)	(4.710)	(5.535)	(6.007)	(0.138)	(0.136)	(0.251)	(0.338)	(4.246)	(2.227)	(4.735)	(2.497)	(0.141)	(0.106)	(0.283)	(0.187)
EITC	6.768	9.277	3.000	7.960	0.0346	0.0497	0.00376	0.155	6.057	6.345	5.472	8.284	0.112	0.0380	0.215	0.310
	(2.806)	(4.816)	(3.904)	(4.854)	(0.114)	(0.210)	(0.182)	(0.234)	(2.367)	(4.116)	(3.230)	(3.561)	(0.111)	(0.207)	(0.179)	(0.189)
Two-family house	0.372	0.284	0.524	0.416	0.0105	−0.00676	0.0335	−0.0120	0.352	0.244	0.309	0.740	0.0144	−0.0141	0.0234	−0.000536
	(0.433)	(0.669)	(0.675)	(0.754)	(0.0188)	(0.0361)	(0.0321)	(0.0349)	(0.351)	(0.551)	(0.508)	(0.561)	(0.0185)	(0.0340)	(0.0303)	(0.0321)
Multi-family house	−0.0701	0.523	−0.250	0.0169	0.000604	−0.00123	−0.00329	−0.00198	−0.00315	0.673	0.144	0.274	0.00484	0.0128	−0.00629	0.00822
	(0.424)	(0.522)	(0.757)	(0.763)	(0.0175)	(0.0291)	(0.0284)	(0.0284)	(0.336)	(0.445)	(0.574)	(0.450)	(0.0176)	(0.0315)	(0.0259)	(0.0265)
Mobile home/trailer	−0.652	−0.935	0.232	−0.520	−0.0467	−0.0562	−0.0305	−0.0468	−0.0934	−0.683	−0.0743	0.308	−0.0143	−0.0327	−0.0219	0.00743
	(0.550)	(0.633)	(1.091)	(0.864)	(0.0193)	(0.0306)	(0.0318)	(0.0334)	(0.410)	(0.530)	(0.695)	(0.695)	(0.0186)	(0.0293)	(0.0311)	(0.0303)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓				✓											
Biweekly FE									✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State–year interaction	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State–income interaction	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	136,343	44,444	46,295	45,604	136,271	44,405	46,274	45,592	166,128	52,242	55,674	54,087	166,002	52,185	55,636	54,057
$\mathbb{E}(Y)$	18.09	17.66	18.49	18.1	8.68	8.52	8.82	8.69	14.59	14.38	14.62	14.38	7.01	6.94	6.98	6.93

Note: The table shows the DDD analysis for monthly and biweekly toilet paper purchases. The dependent variables are monthly or biweekly units (rolls) purchased (panels A and C) and monthly or biweekly log spending (panels B and D). The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

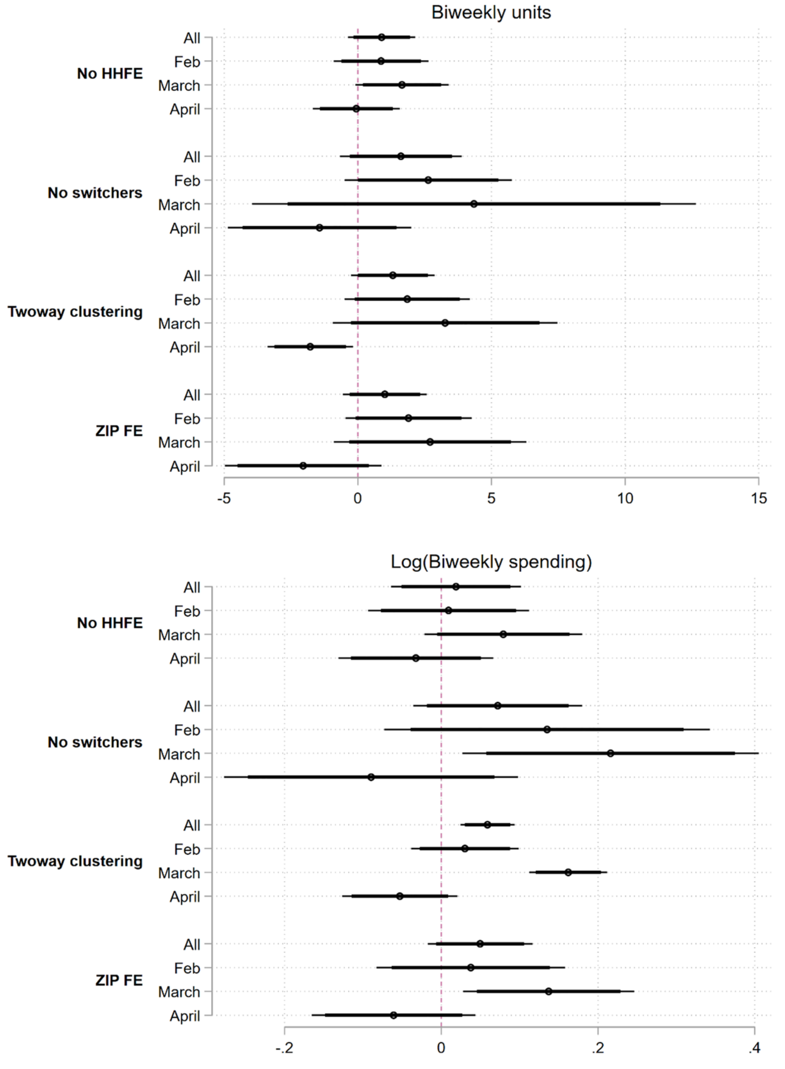

FIGURE 1 Robustness: biweekly, toilet paper

Note: The figure plots the triple differences coefficient (α) from equation (7) and 95 per cent as well as 90 per cent confidence intervals for biweekly units and log of biweekly spending on toilet paper separately for tax months (February, March, April) separately and in combination (All). The 'No HH FE' specification runs equation (7) without household fixed effects. 'No switchers' shows the triple DiD coefficient from equation (7) for households that do not switch eligibility for other reasons than the introduction of a state EITC policy. 'Twoway clustering' reports results for α with standard errors clustered by household and state instead of household only. Finally, 'ZIP FE' reports α for a version of equation (7) where we replace state fixed effects with ZIP code fixed effects.

amounts. When we control for household fixed effects, identification relies strongly on those switching their eligibility status. These switchers might be experiencing changes in their household structure (having an additional child or getting married, for example) that may impact both consumption patterns and EITC eligibility status. However, our results without household fixed effects are very similar to our main results; households changing eligibility status does not generate different estimates. Figures 1 and 2 ('Twoway clustering' panel) report standard errors for biweekly and monthly toilet paper spending and quantity that are clustered at both the household and the state level. While adding state-level clustering dampens the quantity results relatively more than the spending results, this does not alter our main conclusions.

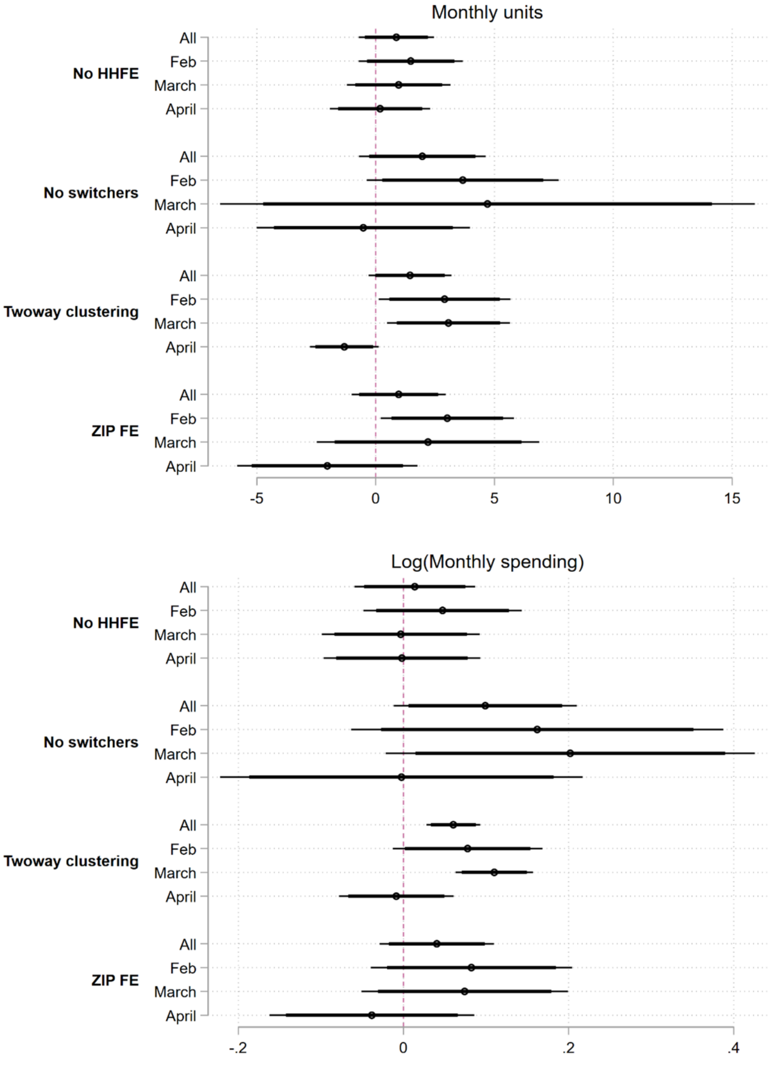

FIGURE 2 Robustness: monthly, toilet paper

Note: The figure plots the triple differences coefficient (α) from equation (7) and 95 per cent as well as 90 per cent confidence intervals for monthly units and log of monthly spending on toilet paper separately for tax months (February, March, April) separately and in combination (All). The 'No HH FE' specification runs equation (7) without household fixed effects. 'No switchers' shows the triple DiD coefficient from equation (7) for households that do not switch eligibility for other reasons than the introduction of a state EITC policy. 'Twoway clustering' reports results for α with standard errors clustered by household and state instead of household only. Finally, 'ZIP FE' reports α for a version of equation (7) where we replace state fixed effects with ZIP code fixed effects.

There consistently is no effect in April, which is reasonable given that most households receive their tax refunds in February and March. In addition, if most households eligible for the EITC are credit-constrained, payments should be spent close to the time of receipt, in February or March. Taken together, Table 6 suggests that households are purchasing and storing more toilet paper during income tax filing season, spending relatively less as they are buying at a cheaper per-unit price and consuming this saved supply over a longer period. These effects are in response to being eligible for a relatively small lump-sum state EITC payment. Our monthly results suggest savings of 6 per cent on average across all households.[33] These are likely to be a lower bound estimate for three reasons. First, as many as one in five eligible households will fail to receive an actual payment.[34] Second, some households may adjust in quality not quantity, and some may not take advantage of quantity savings opportunities, but still buy and store goods for future consumption (see Section 5.2). Third, some consumers will shift their consumption bundles based on the income effects of the reduction in per-unit prices.

It is notable across estimates that households living in multi-family dwellings or mobile homes purchase significantly fewer units relative to households in single-family homes, with households in multi-family units purchasing the least. This is consistent with storage costs being part of the decision to buy larger quantities.[35] For households deciding to buy and store goods, the marginal amount of space remaining should be a key consideration before buying larger quantities.

5.2 | Quality substitution

Households could potentially use state EITC payments to adjust along a quality margin by buying better quality products instead of buying in more quantity. While toilet paper is quite homogeneous, it can still vary in thickness. We run our DDD specification with the share of one-ply toilet paper purchased as a dependent variable, and we present the results in Table 7. We find decreases in the one-ply share that are very close to zero in March and April and similarly small increases in February. This evidence points to the fact that households are not primarily targeting quality as margin of adjustment. Regardless, higher-quality products would only reduce our unit-based estimates.[36]

5.3 | Robustness

We estimate four additional robustness exercises to help solidify the assumptions and findings already described. First, Table 8 shows that state EITC policies are not associated with changes in purchasing among people who are not eligible for EITC payments – a reassuring finding. Second, Table 9 shows that the estimates are not driven by the staggered introduction timing of state EITCs. Third, Figures 1 and 2 show that the effects among households already eligible for a new state EITC are similar to those among households who became eligible after a state EITC was implemented. Finally, these same figures show that our estimates are robust to the possibility of dynamic price changes by local retailers.

The first robustness exercise is shown in Table 8 for biweekly purchases of, and spending on, toilet paper. This differences-in-differences (DD) specification is for a population that is unaffected by changes in EITC policies: households with an annual income of over $100,000. While these estimates cannot control for state–year variation, the results suggest that there is no effect of state EITC policies for this higher-income group. We can rule out that the main results are due to other events or policies occurring simultaneously to state EITC policies.[37]

The second exercise is to address the potential bias arising from EITC policies being introduced across states in different years. Several studies highlight the potential for bias when policies are

[33] At the monthly level, a 16.5 per cent increase in quantity of toilet paper purchases in March versus 11 per cent increase in spending. At the biweekly level, a 22 per cent increase in quantity versus 16 per cent increase in spending.

[34] Iselin et al., 2021.

[35] Ching and Osborne, 2020.

[36] Households with certain wastewater systems, especially septic tanks, may only ever purchase one-ply toilet paper, which breaks down more readily. To include households that are ever observed purchasing two-ply toilet paper. If households are substituting to thicker toilet paper with their tax refund payments, we should see the share of one-ply toilet paper decrease in tax months.

[37] Figure D.1 in the online appendix shows corresponding event study results.

TABLE 7 Toilet paper quality, one-ply share

	Panel A: biweekly share				Panel B: monthly share			
	Feb–April	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**
Eligible × EITC	−0.00158	0.000267	−0.00215	−0.0125	−0.00328	0.000197	−0.0169	−0.00245
	(0.0250)	(0.0449)	(0.0447)	(0.0465)	(0.0256)	(0.0479)	(0.0476)	(0.0491)
Eligible	−0.0509	0.134	−0.0109	−0.0713	−0.0199	0.109	0.0566	0.0671
	(0.133)	(0.166)	(0.290)	(0.144)	(0.135)	(0.162)	(0.259)	(0.203)
EITC	0.166	0.107	0.0938	0.149	−0.0260	−0.0839	0.0993	−0.135
	(0.0865)	(0.153)	(0.149)	(0.154)	(0.0802)	(0.171)	(0.153)	(0.150)
Two-family house	0.00363	−0.0106	−0.00772	0.0103	0.000854	−0.0141	−0.00649	0.0128
	(0.0152)	(0.0289)	(0.0269)	(0.0292)	(0.0156)	(0.0304)	(0.0282)	(0.0314)
Multi-family house	−0.0327	−0.0371	−0.0296	−0.0543	−0.0365	−0.0483	−0.0318	−0.0547
	(0.0130)	(0.0234)	(0.0225)	(0.0235)	(0.0135)	(0.0247)	(0.0242)	(0.0253)
Mobile home/trailer	−0.0238	−0.0137	−0.0400	−0.0218	−0.0213	−0.0137	−0.0337	−0.0170
	(0.0146)	(0.0253)	(0.0254)	(0.0260)	(0.0145)	(0.0258)	(0.0252)	(0.0270)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Biweekly FE	✓	✓	✓	✓				
Monthly FE					✓			
State FE	✓	✓	✓	✓	✓	✓	✓	✓
State × year	✓	✓	✓	✓	✓	✓	✓	✓
State × income	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓
N	158,744	49,936	53,164	51,658	130,018	42,404	44,139	43,475
$\mathbb{E}$(Y)	0.3	0.3	0.29	0.3	0.3	0.3	0.29	0.3

Note: The table shows the DDD analysis for monthly (panel B) and biweekly (panel A) one-ply shares of toilet paper. Each cell shows the main treatment coefficient (coefficient on eligible households in states with a state EITC policy) for one type of product module (group) and month. Therefore, each cell shows the result from a different regression. 'N' refers to the number of observations for that regression. The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

TABLE 8 Biweekly level, toilet paper purchases for never EITC-eligible population (households with annual household income > $100,000)

	Panel A: biweekly units				**Panel B: log biweekly spending**			
	Feb–April	**Feb**	**March**	**April**	**Feb–April**	**Feb**	**March**	**April**
State EITC	−0.332	0.641	1.048	−2.973	0.00880	0.0763	−0.0273	0.0295
	(1.254)	(1.256)	(1.643)	(3.761)	(0.0405)	(0.0739)	(0.0664)	(0.0671)
Two-family house	−2.698	−2.611	−3.758	−1.382	−0.124	−0.134	−0.225	−0.000486
	(1.160)	(2.269)	(1.610)	(2.428)	(0.0486)	(0.113)	(0.0999)	(0.0964)
Multi-family house	−3.973	−2.884	−5.290	−5.587	−0.130	−0.123	−0.281	−0.0538
	(1.249)	(1.994)	(2.000)	(4.163)	(0.0396)	(0.0802)	(0.0736)	(0.0793)
Mobile home/trailer	−7.809	−5.524	−10.34	−18.43	−0.349	−0.366	−0.632	−0.670
	(2.721)	(5.613)	(3.488)	(7.316)	(0.180)	(0.407)	(0.317)	(0.258)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Biweekly fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓
N	43,206	13,579	14,845	14,105	43,185	13,571	14,837	14,100
$\mathbb{E}(Y)$	19.37	19.04	19.27	19.5	10.18	10.08	10.18	10.13

Note: The table shows the DD analysis for toilet paper purchases for households with annual income greater than $100,000. The dependent variables are biweekly units purchased (Panel A) and the log of biweekly spending (Panel B). The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members, and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

TABLE 9 Estimates by state: never treated states as control group

	Panel A: monthly units				Panel B: log monthly spending				Panel C: biweekly units				Panel D: log biweekly spending			
	Feb–April	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**
New Mexico	8.499	12.18	5.255	7.711	0.280	0.0506	0.156	0.494	7.814	8.781	5.698	8.212	0.300	0.0190	0.0977	0.629
	(3.164)	(4.602)	(4.823)	(6.809)	(0.226)	(0.252)	(0.359)	(0.411)	(1.828)	(5.152)	(4.534)	(2.485)	(0.158)	(0.288)	(0.285)	(0.347)
N	70,021	22,770	23,815	23,436	69,986	22,753	23,803	23,430	97,472	30,562	32,707	31,768	97,415	30,537	32,687	31,757
Connecticut	15.91	0.0106	54.32	2.470	0.142	0.00830	−0.0403	0.337	13.91	−1.931	47.38	2.938	0.0427	−0.134	−0.0942	0.151
	(10.18)	(2.929)	(39.22)	(3.215)	(0.176)	(0.185)	(0.373)	(0.211)	(9.172)	(2.177)	(33.20)	(2.276)	(0.159)	(0.162)	(0.394)	(0.175)
N	70,282	22,858	23,916	23,508	70,244	22,840	23,903	23,501	97,787	30,667	32,823	31,854	97,727	30,641	32,802	31,842
Louisiana	1.866	11.89	−1.811	0.364	0.110	0.245	0.133	0.0459	2.804	9.838	2.059	1.254	0.0617	0.0690	0.231	0.0102
	(2.428)	(6.550)	(3.698)	(3.286)	(0.0971)	(0.144)	(0.175)	(0.165)	(1.947)	(6.090)	(2.507)	(2.321)	(0.0816)	(0.143)	(0.152)	(0.137)
N	71,367	23,216	24,268	23,883	71,332	23,199	24,256	23,877	99,039	31,072	33,236	32,278	98,982	31,047	33,216	32,267
Michigan	1.088	4.302	0.471	−2.386	0.0553	0.144	0.166	−0.108	0.734	2.706	1.158	−2.597	0.0682	0.0859	0.224	−0.0794
	(1.253)	(1.962)	(2.080)	(2.019)	(0.0539)	(0.0917)	(0.0826)	(0.0903)	(0.982)	(1.609)	(1.539)	(1.568)	(0.0549)	(0.0887)	(0.0767)	(0.0832)
N	66,778	21,686	22,737	22,355	66,749	21,671	22,728	22,350	92,134	28,877	30,868	30,028	92,084	28,852	30,854	30,018
North Carolina	0.582	0.860	2.594	−2.485	0.0363	0.0133	0.0893	−0.0557	0.575	0.132	2.607	−3.193	0.0271	−0.0114	0.145	−0.124
	(1.420)	(1635)	(1.645)	(3.467)	(0.0511)	(0.0845)	(0.0846)	(0.0850)	(1.123)	(1.346)	(1.219)	(2.843)	(0.0491)	(0.0833)	(0.0705)	(0.0706)
N	75,076	24,417	25,569	25,090	75,041	24,400	25,557	25,084	103,681	32,541	34,824	33,726	103,623	32,516	34,803	33,715
Delaware	−3.250	5.084	−3.907	2.655	−0.0206	0.473	−0.501	0.335	−0.854	5.346	2.887	−0.527	0.125	0.322	−0.145	0.178
	(3.492)	(4.653)	(7.235)	(3.530)	(0.265)	(0.194)	(0.761)	(0.457)	(2.728)	(5.106)	(3.284)	(2.753)	(0.216)	(0.415)	(0.587)	(0.239)
N	69,836	22,714	23,758	23,364	69,801	22,697	23,746	23,358	97,219	30,481	32,637	31,676	97,162	30,456	32,617	31,665
Virginia	0.384	5.086	−2.324	0.715	0.127	0.284	−0.0423	0.0892	1.710	4.143	2.423	−0.493	0.178	0.198	0.156	−0.0674
	(2.270)	(4.793)	(4.486)	(3.254)	(0.160)	(0.281)	(0.303)	(0.303)	(2.017)	(4.479)	(4.458)	(2.990)	(0.129)	(0.296)	(0.272)	(0.248)
N	71,672	23,316	24,384	23,972	71,637	23,299	24,372	23,966	99,456	31,202	33,391	32,386	99,396	31,176	33,369	32,375

Note: This table shows results from estimating our baseline pre-post DiD specification (equation 5 including household fixed effects) for the seven states (Connecticut, Delaware, Louisiana, Michigan, North Carolina, New Mexico and Virginia) that were treated during our observation period. Control states in each case are the states that were never treated, i.e. they never introduced a state EITC policy up until 2012. Each cell shows the main treatment coefficient (coefficient on eligible households in states with a state EITC policy) from one such regression. 'N' refers to the number of observations for that regression. The dependent variables are monthly or biweekly units of toilet paper purchased (panels A and C) and monthly or biweekly log spending on toilet paper (panels B and D). The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. To keep the table sparse, we omit indicators for fixed effects here. The same fixed effects are used as in Table 6. We also divide the table by whether a state introduces a refundable or non-refundable state EITC. Standard errors in parentheses, clustered by household.

adopted over time.[38] Table 9 only includes comparison states that *never* change state EITC policies.[39] These estimates include only one treated state at a time, with states that never introduced a state EITC until 2012 as control group. These results are similar to Table 6, showing that households in the seven states that introduce a state EITC during our period of observation shift spending in similar ways.[40] These estimates are evidence that the prior differences-based coefficients are not driven by the staggered nature of our policy variation.

There are other methods that we considered to estimate a single average treatment effect. The approaches of both Callaway and Sant'Anna (2021) and Borusyak, Jaravel and Spiess (2023) to two-way fixed effects designs with staggered policy introductions are primarily targeted at DD settings. While incorporating a triple-differences strategy is possible using the approach used by Borusyak et al. (2023), our data present several challenges. The imputation approach Borusyak et al. use is data-intensive and requires relatively large numbers of observations in a given cell to estimate reliable results. Unfortunately, in our data, this approach leads to many dropped observations, which then further leads to multicollinearity issues given the high dimensionality of fixed effects.[41] The approach we use instead provides a consistent control group to compare with newly treated households, which avoids a comparison with already treated units and comes closest to the stacked approach of Deshpande and Li (2019) by treating each event (defined as a set of treated units in a given period) separately. This approach is enhanced by the fact that we have a large group of never-treated states, and a rich set of fixed effects. While we are unable to provide an overall average treatment effect, we are nevertheless confident that our event study allows us to understand heterogeneous effects across states.

The next robustness test addresses the concern that households are manipulating their eligibility for a state EITC. Variation in eligibility largely comes from households becoming eligible due to individual-specific changes in income, marital status, the number of children, or the state of residency. A variance decomposition exercise shows that 82 per cent of variation in treatment status is due to annual income changes the prior year, followed by 9.2 per cent due to state EITC introductions, 2.6 per cent due to changes in the number of children,[42] 0.19 per cent due to moves across states, and 0.03 per cent due to changes in marital status. Figures 1 and 2 ('No switchers' panel) only use the subset of people who are eligible due to state EITC policy introductions, showing similar patterns of biweekly and monthly toilet paper spending and consumption among households who do not switch eligibility status for any other reason than the introduction of a state policy. Even though we lose precision, these estimates support the idea that the main results are not driven by households switching eligibility status.

Our final robustness exercise is to control for retail store price variation during tax season. Hastings and Washington (2010) show that grocery stores increase intra-month prices when households receive food benefit payments. We run a specification with ZIP code fixed effects and show results in Figures 1 and 2 ('ZIP FE' panel) to control for local area price effects. These estimates correspond closely to our main effects in Table 6. [43]

[38] Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Athey and Imbens, 2022.

[39] Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Baker, Larcker and Wang, 2022; Athey and Imbens, 2022.

[40] Note that households in Delaware and Virginia are unlikely to receive state EITC payments since these state EITCs are not refundable.

[41] There are further data requirements that make it difficult to estimate (a) household fixed effects and (b) state–year trends, which are crucial for causal identification.

[42] Only the state of Wisconsin varies state EITC amounts based on the number of children.

[43] Even if stores did systematically raise prices in response to state EITC policies, this would only reduce the magnitude of our estimates.

5.4 | Perishable goods, canned goods and paper products

In Table 10, we estimate spending on common products that households purchase regularly but are perishable and difficult to store: eggs and bread. Each row displays the state EITC 'treatment effect' (α) for one product module (Table A.2 in the online appendix shows the mean values for reference). Focusing first on the monthly results, there is no statistically significant increase in units or spending for eggs in any of the months related to EITC payments. Indeed, there is no clear pattern, with estimates that are sometimes positive, sometimes negative. For bread, there is no statistically significant increase in units or spending in February or March, but there is an increase in April. Compared with the monthly mean of ounces of bread purchased, 7.5 ounces represent a 11.2 per cent increase. Spending increases at a similar rate, by 11.5 per cent. In contrast to toilet paper, the increase in spending is not lower in percentage terms than the increase in quantity. The effect is also occurring in April, which we consider more like a control month. For biweekly units, there is a slight overall increase in the quantity of bread purchased (across the three months), an increase of around 5.7 per cent relative to the mean, and no increase in spending. There is no month that indicates a precise increase in the amount of bread purchased, and no indication of more eggs being purchased.

In Table 11, we examine canned goods. Across the different aggregation levels, canned goods show more units purchased but this increase is only statistically significant in April, with a precisely estimated increase in monthly spending of 5 per cent at the monthly level and 4 per cent at the biweekly level, or an 8 per cent increase relative to the mean quantity of canned goods. We might expect canned goods to perform more like toilet paper. However, unlike toilet paper, canned goods are less likely to be bundled into multi-unit packaging in retail stores, and even if bulk packages are offered, consumers may not want to use large quantities of a single variety of canned good. Toilet paper is a more homogeneous good that is widely used, easily sold in larger quantities, and can be stored indefinitely (whereas canned food has an expiration date). Still, despite the problematic features of canned goods as a product category for this analysis, these estimates are generally in the direction we would predict – more purchases in tax refund months. The change in quantity also exceeds the change in spending, which is consistent with storing goods for future use.

Table 12 displays estimates for all paper products, which include toilet paper, as well as paper towels, facial tissue, paper napkins and disposable dishes.[44] These estimates do not include household fixed effects (see Section 4.1), so identification is not coming from the margin of household EITC eligibility. Table 12 shows more units purchased at the trip level, and no precise effect on spending. This is evidence of an increase in quantities at lower per-unit prices. The estimates are consistent with the toilet-paper-specific estimates, but less precise. There are many substitutes for facial tissues and paper towels, however, and these may not be goods households will want to stockpile for later use.

5.5 | All product modules

Table 13 includes all product modules together and estimates the specification described in equation (5). All regressions in Panels A and B include product module fixed effects that account for between-module heterogeneity. The estimates are all positive. None of the coefficients is precisely estimated, which is not surprising given that there are many product modules with different measured units, as well as different purchase frequencies. Unit estimates for March could be larger than spending estimates (based on confidence intervals), which is consistent with some efficiencies in purchases. Consumers may well use savings from purchasing in larger quantities to offset purchases of other goods they ordinarily would not buy or buy rarely, for example. Panel C aggregates spending on all product modules at the monthly level. This shows a more precisely estimated impact of being eligible

[44] We performed a fixed effects analysis for sanitary pads in Table C.1 in the online appendix, showing statistically significant increases in the quantity bought specifically for female-headed households.

TABLE 10 Specifications by type of good: perishables

	Panel A: monthly units				Panel B: log monthly spending				Panel C: biweekly units				Panel D: log biweekly spending			
	Feb–April	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**	**Feb–April**	**February**	**March**	**April**
Toilet paper	1.446	2.899	3.063	−1.321	0.0604	0.0777	0.110	−0.00867	1.309	1.848	3.264	−1. 781	0.0589	0.0301	0.162	−0.0530
	(0.939)	(1.261)	(2.082)	(1.694)	(0.0332)	(0.0543)	(0.0552)	(0.0554)	(0.756)	(1.077)	(1.625)	(1.335)	(0.0322)	(0.0541)	(0.0489)	(0.0481)
N	136,343	44,444	46,295	45,604	136,271	44,405	46,274	45,592	166,128	52,242	55,674	54,087	166,002	52,185	55,636	54,057
Eggs	0.586	−0.769	1.568	0.733	0.00190	−0.0116	0.00528	−0.00747	−0.120	−0.532	1.352	0.145	−0.00259	0.0204	0.00986	−0.0171
	(1.040)	(1.532)	(1.719)	(1.636)	(0.0280)	(0.0453)	(0.0429)	(0.0461)	(0.636)	(1.033)	(1.090)	(1.039)	(0.0228)	(0.0379)	(0.0328)	(0.0368)
N	165,320	53,687	55,712	55,921	165,229	53,662	55,680	55,887	218,230	67,391	71,887	70,742	218,046	67,339	71,817	70,684
Bread	5.102	3.108	5.239	7.479	0.0764	0.0892	0.0433	0.115	2.460	0.256	1.492	2.512	0.0501	0.00418	0.0101	0.0901
	(2.258)	(3.085)	(3.168)	(3.126)	(0.0330)	(0.0473)	(0.0457)	(0.0482)	(1.228)	(1.723)	(1.703)	(1.632)	(0.0261)	(0.0380)	(0.0379)	(0.0387)
N	473,350	150,551	164,443	158,356	211,596	69,383	71,687	70,526	317,812	95,132	102,940	97,372	317,451	95,014	102,812	97,262
All	1.003	0.317	1.834	0.900	0.0118	0.00350	0.0139	0.0175								
	(1.339)	(1.485)	(1.542)	(1.600)	(0.0123)	(0.0134)	(0.0134)	(0.0131)								
N	15,031,692	4,865,726	5,146,343	5,019,623	15,028,293	4,861,430	5,141,641	5,015,222								
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Monthly FE	✓				✓											
Biweekly FE									✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State × year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State × income	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Dem. controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: The table shows a comparison of the DDD analysis for monthly and biweekly purchases for toilet paper, eggs, bread, and all product modules. Each cell shows the main treatment coefficient (coefficient on eligible households in states with a state EITC policy) for one type of product module (group) and month. Therefore, each cell shows the result from a different regression. 'N' refers to the number of observations for that regression. The dependent variables are monthly or biweekly units purchased (panels A and C) and monthly or biweekly log spending (panels B and D). The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

TABLE 11 Monthly and biweekly level: canned goods purchases

	Panel A: monthly units (oz)				Panel B: log monthly spending				Panel C: biweekly units (oz)				Panel D: log biweekly spending			
	Feb–April	**Feb**	**March**	**April**	**Feb–April**	**Feb**	**March**	**April**	**Feb–April**	**Feb**	**March**	**April**	**Feb–April**	**Feb**	**March**	**April**
Eligible × EITC	3.070	2.892	1.570	5.007	0.0189	−0.00177	0.00806	0.0512	3.039	3.457	1.713	4.465	0.0199	0.00555	0.0177	0.0416
	(1.928)	(2.562)	(2.575)	(2.384)	(0.0186)	(0.0225)	(0.0217)	(0.0214)	(1.600)	(2.124)	(2.077)	(1.951)	(0.0197)	(0.0230)	(0.0224)	(0.0222)
Eligible	0.333	5.327	−12.85	8.593	−0.0143	−0.0158	−0.0499	0.0259	−1.315	3.514	−13.82	7.381	−0.0352	−0.0124	−0.104	0.0124
	(9.730)	(12.73)	(7.972)	(12.00)	(0.0779)	(0.0902)	(0.0785)	(0.0854)	(8.071)	(10.03)	(6.533)	(10.17)	(0.0829)	(0.0927)	(0.0851)	(0.0875)
EITC	2.048	−4.068	10.12	0.157	0.0190	0.101	0.0132	−0.0512	1.485	−3.743	6.758	3.914	0.00682	0.0447	−0.0125	−0.0119
	(4.854)	(7.025)	(6.752)	(7.055)	(0.0556)	(0.0675)	(0.0705)	(0.0750)	(4.127)	(6.147)	(5.450)	(5.831)	(0.0547)	(0.0667)	(0.0653)	(0.0727)
Two-family house	−3.151	−2.627	−3.433	−3.393	−0.0139	−0.0115	−0.0119	−0.0190	−2.659	−2.145	−3.116	−2.578	−0.00891	−0.00806	−0.00760	−0.0128
	(1.199)	(1.516)	(1.296)	(1.398)	(0.00923)	(0.0103)	(0.00978)	(0.0103)	(0.984)	(1.239)	(1.023)	(1.098)	(0.00963)	(0.0106)	(0.0101)	(0.0106)
Multi-family house	−5.442	−5.647	−5.537	−5.087	−0.0282	−0.0244	−0.0249	−0.0348	−5.448	−5.613	−5.635	−5.083	−0.0354	−0.0314	−0.0370	−0.0424
	(0.808)	(0.927)	(0.890)	(0.923)	(0.00647)	(0.00698)	(0.00701)	(0.00705)	(0.641)	(0.729)	(0.675)	(0.715)	(0.00678)	(0.00715)	(0.00719)	(0.00732)
Mobile home/trailer	−4.092	−3.826	−3.782	−4.608	−0.0264	−0.0207	−0.0255	−0.0329	−3.518	−3.457	−3.064	−3.783	−0.0299	−0.0252	−0.0285	−0.0381
	(0.662)	(0.812)	(0.795)	(0.740)	(0.00720)	(0.00791)	(0.00790)	(0.00779)	(0.576)	(0.690)	(0.676)	(0.636)	(0.00768)	(0.00827)	(0.00839)	(0.00816)
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓				✓				✓				✓			
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State × year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State × income	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Product module FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	1,595,037	529,705	544,484	520,848	1,594,046	529,367	544,161	520,518	1,809,542	588,422	611,947	578,462	1,808,291	587,994	611,541	578,048
$\mathbb{E}(Y)$	69.74	71.25	69.72	68.23	3.78	3.8	3.82	3.73	60.55	61.47	59.25	58.42	3.28	3.27	3.23	3.18

Note: The table shows the DDD analysis for all canned goods purchases. The dependent variables are monthly or biweekly units (ounces) purchased (panels A and C) and monthly or biweekly log spending (panels B and D). The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members, and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

TABLE 12 All paper products (trip level)

	Panel A: units (counts)				Panel B: spending			
	Feb–April	**Feb**	**March**	**April**	**Feb–April**	**Feb**	**March**	**April**
Eligible × EITC	5.836	8.849	3.277	6.009	0.0560	0.0247	0.116	0.0288
	(3.487)	(4.941)	(4.446)	(4.576)	(0.136)	(0.155)	(0.162)	(0.160)
Eligible	2.249	18.45	−21.59	10.44	−0.379	−0.390	−0.310	−0.334
	(17.04)	(24.81)	(12.34)	(22.33)	(0.479)	(0.639)	(0.426)	(0.607)
EITC	6.417	−15.55	18.40	17.10	−0.176	−0.198	−0.117	−0.199
	(9.253)	(13.02)	(13.81)	(13.20)	(0.397)	(0.484)	(0.511)	(0.515)
Two-family house	−5.417	−4.826	−5.921	−5.640	−0.240	−0.232	−0.275	−0.212
	(2.088)	(2.930)	(2.330)	(2.360)	(0.0663)	(0.0788)	(0.0701)	(0.0738)
Multi-family house	−10.69	−10.87	−11.22	−9.991	−0.529	−0.465	−0.589	−0.530
	(1.365)	(1.657)	(1.547)	(1.607)	(0.0480)	(0.0545)	(0.0514)	(0.0531)
Mobile home/trailer	−7.765	−8.170	−7.077	−8.144	−0.360	−0.376	−0.335	−0.369
	(1.301)	(1.629)	(1.582)	(1.501)	(0.0539)	(0.0569)	(0.0605)	(0.0583)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Month fixed effects	✓				✓			
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
State × year	✓	✓	✓	✓	✓	✓	✓	✓
State × income	✓	✓	✓	✓	✓	✓	✓	✓
Product module FE	✓	✓	✓	✓	✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓
N	613,011	197,571	210,751	204,689	613,011	197,571	210,751	204,689
$\mathbb{E}(Y)$	98.55	102.68	97.78	95.35	4.01	3.99	4.02	4.01

Note: The table shows the DDD analysis for all paper products (the sum of rolls of toilet paper purchased and units of facial tissue and paper towels, as well as disposable dishes). The dependent variables are units purchased in a trip (Panel A) and trip-level spending (Panel B). The first column of each panel pools the months February to April, while the following three columns of each panel look separately at February, March and April. Demographic controls are: bins of household income, the number of household members and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses. Clustered by household.

TABLE 13 Expenditure estimates and state EITC, all product modules

	Panel A: monthly units				Panel B: log monthly spending				Panel C: Log aggregate monthly spending				Panel D: spending share on storable goods			
	Feb–April	February	March	April	Feb–April	February	March	April	Feb–April	February	March	April	Feb–April	February	March	April
Eligible × EITC	1.003	0.317	1.834	0.900	0.0118	0.00350	0.0139	0.0175	0.0575	0.0437	0.0582	0.0708	-0.000104	−0.00115	−0.000138	0.00108
	(1.339)	(1.485)	(1.542)	(1.600)	(0.0123)	(0.0134)	(0.0134)	(0.0131)	(0.0320)	(0.0358)	(0.0364)	(0.0363)	(0.00189)	(0.00294)	(0.00298)	(0.00286)
Eligible	0.673	−0.328	−1.516	3.800	−0.000563	0.00368	−0.0346	0.0296	0.231	0.239	0.174	0.283	−0.00555	0.0133	−0.0361	0.00644
	(5.137)	(5.160)	(5.551)	(5.864)	(0.0447)	(0.0472)	(0.0485)	(0.0476)	(0.108)	(0.129)	(0.120)	(0.121)	(0.00696)	(0.0148)	(0.0100)	(0.0149)
EITC	−0.576	3.842	−2.523	−2.698	0.0125	0.0303	−0.00738	0.0162	0.0768	0.192	0.0278	0.0116	0.00391	0.00512	0.00509	0.00202
	(4.669)	(4.751)	(5.457)	(6.078)	(0.0362)	(0.0400)	(0.0404)	(0.0405)	(0.0929)	(0.110)	(0.111)	(0.113)	(0.00619)	(0.0103)	(0.0102)	(0.0100)
Two-family house	−2.080	−1.607	−2.264	−2.366	−0.00578	−0.00149	−0.00764	−0.00819	−0.00714	−0.00392	−0.000814	−0.0166	−0.00221	−0.00339	0.000766	−0.00380
	(0.690)	(0.734)	(0.741)	(0.763)	(0.00683)	(0.00713)	(0.00697)	(0.00720)	(0.0156)	(0.0169)	(0.0164)	(0.0166)	(0.00102)	(0.00173)	(0.00182)	(0.00157)
Multi-family house	−3.988	−3.571	−3.983	−4.362	−0.0143	−0.0111	−0.0167	−0.0148	−0.0210	−0.00939	−0.0249	−0.0284	0.00100	0.00140	0.00128	0.000369
	(0.529)	(0.556)	(0.563)	(0.566)	(0.00449)	(0.00463)	(0.00465)	(0.00463)	(0.0104)	(0.0110)	(0.0109)	(0.0111)	(0.000905)	(0.00149)	(0.00132)	(0.00137)
Mobile home/trailer	2.115	2.309	1.817	2.217	−0.0127	−0.00943	−0.0153	−0.0132	0.0760	0.0865	0.0689	0.0727	−0.000466	−0.00162	0.000957	−0.000668
	(0.666)	(0.661)	(0.702)	(0.729)	(0.00501)	(0.00512)	(0.00518)	(0.00529)	(0.0133)	(0.0141)	(0.0140)	(0.0142)	(0.00112)	(0.00189)	(0.00159)	(0.00163)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month fixed effects	✓				✓				✓				✓			
State fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State–year interaction	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State–income interaction	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Product module FE	✓	✓	✓	✓	✓	✓	✓	✓								
Household fixed effects													✓	✓	✓	✓
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	15,031,692	4,865,726	5,146,343	5,019,623	15,018,293	4,861,430	5,141,641	5,015,222	292,863	96,789	98,057	98,017	292,863	96,789	98,057	98,017
$\mathbb{E}$(Y)	138.10	131.41	141.96	140.48	25.2	23.16	26.37	25.93	354.34	339.32	367.99	355.51	0.06	0.07	0.06	0.06

Note: The table shows DDD analysis for all product modules. Panels A and B show within-product module effects. Panels C and D aggregate across all product modules for each household. The first column of each panel pools the months February to April, while the following three columns of each panel show February, March and April. Log monthly spending is the log of spending on a given product module by month whereas Log aggregate monthly spending is the log of the sum of spending on all product modules by month. Spending share on storable goods is the share of spending on canned goods and paper products. Demographic controls are: bins of household income, the number of household members, and an indicator for whether WIC was ever received. The omitted category for housing is single family homes. Standard errors in parentheses, clustered by household.

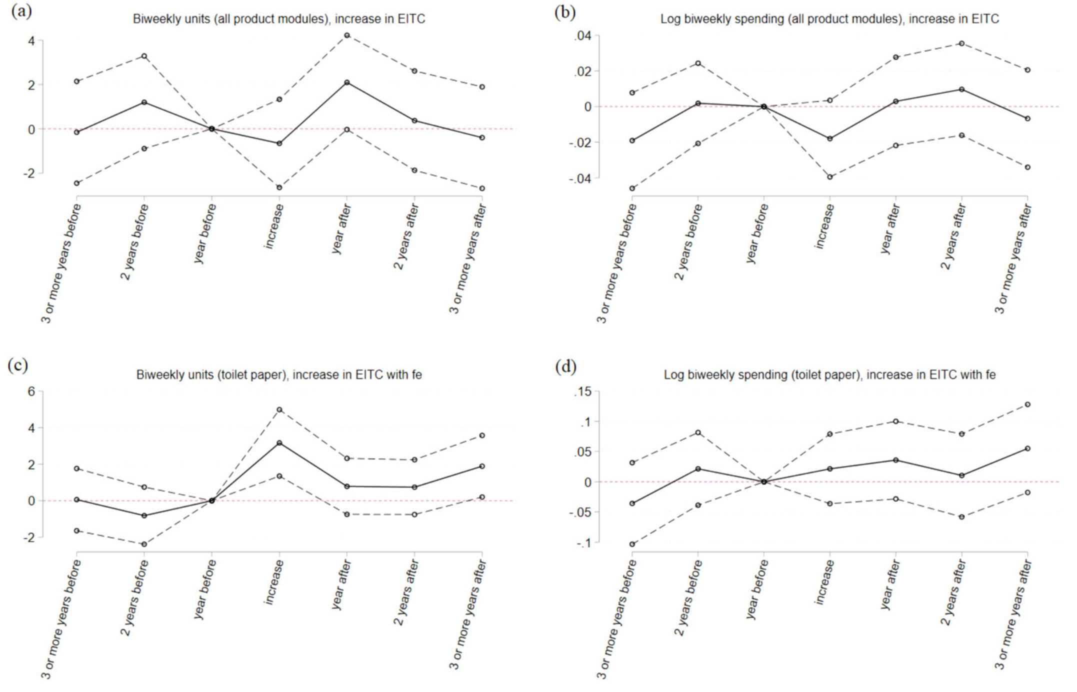

FIGURE 3 Event studies for biweekly units and spending

Note: This figure shows event study coefficients and 95 per cent confidence intervals for biweekly units and spending on all product modules (panels a and b) and for toilet paper (panels c and d) during tax months (February, March and April). All coefficients are relative to the year before the introduction or most recent increase in generosity of state EITC policies.

for state EITC payments in a state and year that the supplement is available. There is a 5.8 per cent increase in aggregate monthly spending in all tax months.

5.6 | Event study

Finally, in Figure 3, we show purchasing trends using an event study framework around the year a state changed its EITC. This is helpful to corroborate the DDD estimates in the previous tables, as well as to examine changes in responses over time. Figure 3(a) displays biweekly units for all product modules. Figure 3(b) shows the log of biweekly spending for all product modules. Figures 3(c) and (d) focus on biweekly units and log biweekly spending on toilet paper. The event is defined as the most recent increase in state EITC – either an introduction (as in Delaware, Virginia, Louisiana and Connecticut) or an increase in generosity (as in Maine, Iowa, Washington DC, Maryland, Nebraska, New Mexico, Oregon, Indiana, Michigan, North Carolina, New Jersey and Kansas). In Figures 3(a) and (b), none of the estimates of EITC eligibility is statistically significant, although based on the confidence intervals, there could be changes of up to 4 per cent in spending the year after the EITC introduction or benefit increase. Like the other aggregated estimates, any increase in consumption on specific products is being masked across all product modules.

The event study estimates in Figures 3(c) and (d) are only for toilet paper. These figures show an increase in units in the year the EITC policy is introduced, which corresponds well with the prior fixed effects estimates. Three or more years after the introduction of the policy, households are still purchasing one additional roll of toilet paper when they are eligible for state EITC and state EITC is available. This corresponds to the theoretical intuition that there is an increase in consumption if

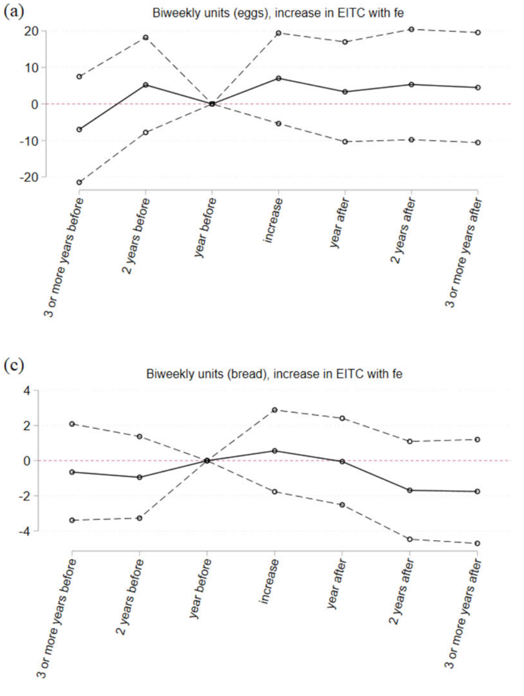

FIGURE 4 Event studies for bread and eggs

Note: This figure plots event study treatment coefficients and 95 per cent confidence intervals for biweekly units and the log of biweekly spending for eggs (panels a and b) as well as bread (panels c and d). All coefficients are relative to the year before the introduction or most recent increase in generosity of state EITC policies.Our event study design also serves as a test of the parallel trends assumption as it accounts for differential introduction times and compares the treatment and control groups in the years prior to introduction. We can reject the hypothesis of pre-trends. Relative to the year before the introduction, there is not a single statistically different pre-treatment coefficient (see also event studies with placebo treatments in Figure D.2 in the online appendix). Overall, the event studies show similar effects in the first year since state EITC policy introductions, as is shown in the estimates in Table 6, in terms of quantity shifts and savings from buying in bulk.

permanent income increases by an unanticipated but permanent shock, but this increase is moderated over time. Given frequent changes to state EITC eligibility, households likely have uncertainty about the exact amount of the EITC payment. Households cannot perfectly smooth consumption as they cannot predict by how much permanent income has increased.[45] Figures 4(a)–(d) show the event study results for bread and eggs; there is no response to the introduction of the state EITC supplement.

6 | CONCLUSION

Households making less than $50,000 annually who are eligible to receive state EITC supplement lump-sum payments buy more of a widely purchased, non-perishable, transportable good during income tax filing months. Based on triple-differences estimates, as well as with household fixed effects, eligible households appear to increase their purchases of toilet paper by 20 per cent per shopping trip in the months tax refund payments are distributed. Purchase times also increase by 14 per cent, suggesting that households are storing toilet paper for later use. Spending on toilet paper only increases by about 11 per cent, which further suggests that households are saving by buying in bulk. We do not find similar increases in purchases among goods that are perishable. These

[45] Hsieh, 2003; Caldwell, Nelson and Waldinger, 2023.

results are consistent with past studies showing consumer stockpiling of goods.[46] However, our study exploits variation in household liquidity levels using a plausibly exogenous state tax policy, showing stockpiling also allows people to save goods, which they can afford to buy at lower per-unit prices, for future consumption. We estimate about a 6 per cent rate of return for households by engaging in this behaviour – a reasonable alternative to the low-yield savings accounts common in this era.

Consumer preferences are idiosyncratic across the wide variety of goods tracked in our retail scanner data. Toilet paper is an exemplar good for measurement reasons but demonstrates that people can use savings from lower per-unit costs to stockpile using income tax refund proceeds. In theory, each consumer may behave similarly by stockpiling across storable goods in their choice set, even if they are not as commonly purchased as toilet paper. If buying in larger quantities allows for even a modest level of acquiring goods at lower prices, this expands people's budget constraints and allows them to reallocate their spending to achieve a different bundle of goods, thereby achieving a higher level of utility. If the same patterns of stockpiling are possible for other storable goods, the households in this sample could be 'saving' by buying in larger quantities and stockpiling across their budget set.

These results could also be evidence of non-financial motivations for consumers to save in stored goods. For example, buying a stockpile could serve as a commitment device – people may know they otherwise would be tempted to spend these lump-sum payments on other, less-productive goods and services.[47] Lump-sum payments could otherwise be treated as 'found money' rather than regular income, and spent in less optimal ways.[48] These annual payments may also serve as a reminder for consumers that they can take advantage of lower per-unit costs – something they might not otherwise focus on when they are struggling to make ends meet with their regular income.[49]

Beyond any behavioural factors that may motivate consumer behaviour, this study provides further evidence of the extent of within-household responses to shocks. Aguiar and Hurst (2007) show that older households retired from work trade off time spent searching and shopping for goods to obtain lower prices, for example. Our study cannot estimate time spent shopping, but we do show a shift in the characteristics of the shopping basket to lower per-unit costs. Aguiar and Hurst (2007) estimate a lower bound of a 7 per cent gain from additional time spent shopping, a similar level as our estimated savings of 6 per cent. The low-income, working-age adults in our study may not have the same ability to spend time shopping as retirees, but simply by buying in quantity they can achieve similar gains. Another example of a related study is Griffith, O'Connell and Smith (2016) who show that consumers maintained a level of calories purchased even while reducing their food expenditures by searching out lower prices and substitute products. These authors show a 3 per cent lower average price paid per calorie during a recession – again in a similar range as our estimates from buying in quantities. Consumers appear to be remarkably resilient at finding strategies to maximise intra-temporal utility.

Understanding the range of strategies consumers use to sustain consumption, including stockpiling during income tax refund season, helps inform ways to better model the complex processes underpinning trends in economic inequality. Aggregated data on expenditures commonly used in micro and macro studies may not detect how consumers are shifting their behaviour. This may be an important factor as researchers study the differential effects of inflation across the income distribution.[50] Consumption levels are likely to be less volatile than income or consumer spending data would indicate as households are finding ways to use stockpiles, substitute to lower-cost goods, or buy at lower per-unit prices. Given rising levels of inflation in the costs of goods, households' ability to smooth consumption may become even more important to understand. While not reflected

[46] Narasimhan, Neslin and Sen, 1996; Hendel and Nevo, 2004, 2006; Sun, 2005; Nevo and Wong, 2019.

[47] Brunnermeier, Papakonstantinou and Parker, 2017; John, 2020; Zaki and Todd, 2023.

[48] Milkman and Beshears, 2009; Feldman, 2010; Jones, 2010; Zhang and Sussman, 2018.

[49] Karlan et al., 2016.

[50] Jaravel, 2021.

in savings rates or financial wealth statistics, stored goods provide a way for marginal households to support future consumption.

Finally, this study has implications for the optimal design of benefit programmes, especially as policymakers consider how to evaluate the adequacy of benefit levels provided by safety net programmes. Policy analysts have debated trade-offs between paying out less frequent, larger lump sums, versus payments of smaller amounts more frequently. More frequent, regular income may have benefits for people's ability to manage their cash flow[51] and some researchers argue that more frequent benefits are more valuable to households.[52] These payments could also help people to overcome behavioural biases such as procrastination and self-control problems.[53] Other studies, however, argue that lump-sum payments facilitate durable consumption and household savings.[54] Our study shows that lump-sum government payments facilitate consumers to purchase and store goods as another form of consumption smoothing, especially among households who may otherwise be liquidity-constrained. The optimal benefits policy may include both regular monthly payments as well as annual lump sums, as provided through the EITC.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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