The Impact of Expense Shocks on Household Consumption

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Abstract

Expense shocks—large, unexpected household outlays—are a common source of liquidity risk. This paper studies how households absorb them by linking an original survey of more than 3,000 households to high-frequency transaction panel data. Between January 2025 and March 2026, 65% of households reported experiencing a shock, and the median shock equals 46% of monthly household income. Using a difference-in-differences design, we find that expense shocks moderately reduce food expenditures by 2.1% on average, with stronger effects among lower-income households. Consumption responses vary sharply with shock size: shocks worth half to one month of household income reduce food expenditures by 8.1%, while the effects of smaller and larger shocks are small and statistically indistinguishable from zero. This non-monotonicity partly reflects financing: households cover small shocks with cash and large shocks with loans, while medium-sized shocks are too large for liquid buffers but too small for costlier financing.

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1 Introduction

Households face substantial financial risk from liquidity shocks. A large literature studies how well households insure consumption against income shocks, such as job loss and other earnings disruptions (Blundell, Pistaferri, and Preston, 2008; Kaplan and Violante, 2010; Ganong et al., 2025). However, a second source of liquidity risk is large unexpected expenses. Expense shocks such as emergency medical bills or pressing home and vehicle repairs can impose substantial strain on household resources.

Expense shocks are at least as common as income shocks and comparable in size. Using the Consumer Financial Protection Bureau's (CFPB) Making Ends Meet (MEM) Survey, Fulford and Low (2026) show that expense shocks are more frequent than income shocks and similar or larger in size. Likewise, the Federal Reserve's 2025 Survey of Household Economics and Decision-Making (SHED) shows that expense shocks are a significant source of household financial risk: 59% of U.S. adults reported a major unexpected expense in the prior 12 months versus 7% who reported being laid off or losing a job.

Despite how common expense shocks are, it is difficult to estimate their effects on household consumption because doing so requires data measuring both the shocks and consumption for the same households, whereas most existing data sources measure only one. Administrative transaction data capture household spending in rich longitudinal detail but cannot be used to distinguish between planned increases in spending and involuntary outlays. By contrast, surveys such as the MEM identify expense shocks directly but are not linked to consumption data.

This paper asks how households' exposure to expense shocks affects consumption and financial distress as well as whether these effects vary with shock size and household resources. To answer these questions, we link information on expense shocks to consumption for the same households. We field a custom survey of Numerator panelists that records the timing, size, type, and financing of unexpected expenses, defined as costs that respondents did not anticipate, as well as measures of financial distress such as delinquency.[1] We link survey responses to the panelists' high-frequency expenditure data, creating a balanced monthly panel of about 3,700 households from August 2024 to June 2026.

We present four main findings. First, unexpected expenses are common and often large relative to household income, consistent with Fulford and Low (2026). Sixty-five percent of households report at least one expense shock between January 2025 and March 2026. The median shock is worth 46% of monthly household income; 28% of shocks exceed one month of

[1]Numerator is a market-research firm that collects receipt-level purchase data from a panel of 200,000 households.

income and 11% exceed three months. Although the incidence of expense shocks varies little across the income distribution, these outlays pose a larger burden for poorer households. The median shock is 60% of monthly income in the bottom quartile compared with 33% in the top quartile.

Second, we find that expense shocks have a modest, negative, and transitory effect on household consumption on average but substantially stronger effects on lower-income households. Using the staggered difference-in-differences (DID) framework of Callaway and Sant'Anna (2021), which exploits variation in both exposure to and the timing of expense shocks across households in our sample, we find that food expenditures fall by 3.4% in the month of a shock and revert to pre-shock levels thereafter. The implied elasticity of food consumption with respect to shock size relative to monthly income is approximately -0.02 at the mean shock size. This estimate is smaller in absolute value than prevailing measures of the monthly consumption response to transitory labor-income shocks. Food expenditures fall by 5.1% in the bottom income quartile and 4.1% in the second, while estimates in the upper half of the income distribution are smaller and statistically insignificant.

Third, we find the consumption response to expense shocks is non-monotonic in shock size. We split shocks into three bins: below half a month of income (approximately the median), between half a month and one month of income, and more than one month of income. Households experiencing shocks of half to one month of income reduce food expenditure by 8.1% and non-durable expenditure (e.g., food, alcoholic beverages, tobacco, household cleaning supplies, and personal-care consumables) by 7.7% on average over the four-month post-shock window. These estimates imply consumption elasticities of approximately -0.11, more than five times the pooled estimate and closer in magnitude to existing estimates for transitory income shocks in the literature. Smaller shocks have no statistically significant effect on either food or non-durable expenditures more broadly, nor do the largest shocks. The latter is particularly striking given the magnitude of the largest shocks: 28% of shocks exceed one month of income.

We provide suggestive evidence that the non-monotonicity in consumption responses reflects differences in how households finance shocks of different sizes, not which households experience them. Medium-sized shocks are not concentrated among lower-income households and are not concentrated among particular types of expense shocks (e.g., medical bill, vehicle repair). Keeping income bins and types of shocks constant, we still observe the largest consumption responses for shocks that are between half and one month of income. Credit access does not explain the pattern either, as the share of households whose credit card limits cover the shock falls steadily with shock size. Thus, credit constraints should bind hardest for the largest shocks, not medium ones. Instead, we find that financing methods shift as shocks

grow in size. The share of households using a formal loan rises from 2.5% for the smallest shocks to 13.7% for the largest, and financing occurs over a longer horizon. While savings and credit cards can absorb smaller shocks, they may be insufficient for medium and larger outlays. In such cases, households can use loans to smooth payments over many months, but this approach carries fixed costs worth paying only for large outlays. Medium-sized shocks fall in between, too large to absorb easily but too small to justify a loan.

Fourth, expense shocks are associated with substantial financial distress, even when their effects on consumption are modest.[2] Households experiencing a shock are 15 percentage points more likely to report an adverse financial outcome within six months, such as falling behind on loan, credit card, rent, or mortgage payments, compared with households that do not experience a shock. This association is particularly pronounced among poorer households: in the bottom income quartile, the gap is 24 percentage points. The ability to smooth consumption therefore need not imply that expense shocks impose small financial costs. Households may absorb unexpected outlays without cutting current consumption by taking on debt, depleting savings, or falling behind on other obligations.

Our paper contributes to two strands of literature. First, we contribute to the literature on household consumption insurance, which has primarily focused on income risk (Blundell, Pistaferri, and Preston, 2008; Kaplan and Violante, 2010; Jappelli and Pistaferri, 2010). Using annual data, Blundell, Pistaferri, and Preston (2008) estimate a pass-through of approximately 0.05 for transitory income shocks, suggesting that households are relatively well insured against temporary income fluctuations. More recently, using high-frequency transaction data, Ganong et al. (2025) find substantially larger consumption responses to monthly labor-income shocks, with an elasticity of approximately 0.22, particularly among households with limited liquid wealth. We extend this literature by estimating the consumption response to expense shocks at a comparable monthly frequency. Our pooled elasticity of -0.02 suggests that consumption is relatively well insured against unexpected expenses on average, although the elasticity of -0.11 for medium-sized shocks is closer to recent estimates for transitory income shocks.

Second, we contribute to a growing literature on expense shocks and their role in households' financial decisions. Using U.S. bank account data, Farrell and Greig (2015, 2017); Farrell, Greig, and Yu (2019) document substantial month-to-month expenditure volatility, large expense spikes, and insufficient liquid buffers to smooth these fluctuations. Using data from the MEM Survey, Fulford and Low (2026) measure the frequency, size, and credit-market consequences of unexpected expenses. Their quantitative model shows that expense

[2]These are descriptive comparisons of shocked and never-shocked households, not DID estimates as in consumption.

shocks play a central role in precautionary saving and in determining the marginal propensity to consume among U.S. households. Using Norwegian transaction data, Bergholt, Furlanetto, and Mori (2025) similarly highlight the importance of expenditure risk, finding that this margin, rather than income risk, accounts for most household consumption volatility.[3] A parallel literature studies health as a particularly salient source of expense risk. Hospital admissions and illness raise out-of-pocket medical spending, unpaid bills, and bankruptcy (Dobkin et al., 2018; Mahoney, 2015; Gross and Notowidigdo, 2011), and medical expense risk is an important motive for precautionary savings (De Nardi, French, and Jones, 2010; Gertler and Gruber, 2002).

We complement this work by measuring the effect of unexpected expenses on consumption, linking survey responses of self-reported expense shocks to a receipt-level consumption panel. Households appear to smooth large and small shocks, while consumption falls after medium-sized shocks. This pattern appears to be mediated by how households finance outlays. Our estimates provide direct targets and constraints for models of household responses to expense shocks. Such models will also need to accommodate the non-monotonic response, possibly by allowing for adjustment or transaction costs in the instruments households use to smooth expense shocks.

The remainder of the paper is organized as follows. Section 2 describes the survey and expenditure data. Section 3 documents the incidence, size, and financing of expense shocks. Section 4 presents the empirical strategy and estimates the effects of expense shocks on consumption and financial distress. Section 5 concludes.

2 Data

This paper uses two principal sources of data: household data from a large-scale custom survey and micro-data on household transactions from Numerator.[4] In this section, we describe each of these sources, detail the construction of our sample and key variables, and validate the transaction data we use against the Consumer Expenditure Survey (CEX).

[3] Additional quantitative models treat spending needs as a stochastic primitive and show that they can rationalize household debt, liquidity holdings, and the joint dynamics of income and consumption (Telyukova, 2013; Miranda-Pinto et al., 2025).

[4] For detailed information on the Numerator household panel and its use in academic research, see Allcott et al. (2026); Cooper, Seim, and Song (2026); Hristakeva, Liaukonytė, and Feler (2026); Hacıoğlu Hoke, Feler, and Chylak (2024, 2025); Hacıoğlu Hoke and Feler (2026).

2.1 Household Survey

In March 2026, we fielded a large-scale custom survey among Numerator panelists to collect information on households' experiences with unexpected expenses between January 2025 and March 2026. Appendix A contains the full questionnaire for the survey.

We define expense shocks as costs that the respondent "had no reason to anticipate, such as a sudden medical emergency or an urgent home or car repair." In doing so, we prime respondents to distinguish between expected, regular expenses and unforeseen outflows. Additionally, we focus on large unexpected expenses to distinguish expense shocks from regular day-to-day variation in expenses. In the same way that small variations in income paychecks do not constitute "income shocks," we focus on expense shocks that are most likely to constitute liquidity shocks.[5]

We ask respondents to report on the most recent expense shock they experienced in the last year. In making this choice, we weighed recording every shock respondents experienced over the last year using a longer survey against collecting more detailed information about one event. We chose the latter as respondents are likely to remember the circumstances of a recent shock more accurately than those of an earlier one, and because a shorter survey allowed us to access a larger sample.[6] One concern about this approach is recall bias: respondents may report a more memorable shock rather than the most recent one. This would overrepresent memorable shocks, such as those involving exceptionally large outflows or those of a type the respondent experiences frequently. If such bias were present, shocks that occurred further in the past would be systematically larger or more concentrated in particular types, because only memorable shocks would be recalled from that far back. Supplementary Table C.1 shows no such pattern: the observable characteristics of shocks, particularly their size and type, do not vary systematically with the month in which they occurred.

We collect several descriptive features of respondents' expense shocks, including their size in dollar amounts, the type of shock (e.g., medical bill, vehicle repair), the month in which the shock occurred, and the extent to which households were able to pay for the shock. Additionally, we ask participants how they paid for the shock, including whether they used liquid savings, retirement savings, or accessed other forms of liquidity. We use this information to evaluate whether payment behaviors differ by shock size and mediate the effects of expense shocks on consumption.

[5] In a later question, we ask respondents how unexpected the shock was right before the shock occurred. We use this measure to conduct robustness analyses on a subsample of households that reported a truly unanticipated shock.

[6] Survey respondents are compensated based on the expected time to complete the survey. A longer survey would have required more compensation per response, and therefore, we would have been budget-constrained to a smaller sample.

We also collect information on the financial circumstances of households before and after expense shocks occurred, including information on net household income, cash buffer, and credit access.[7] Additionally, we measure markers of financial distress after expense shocks, such as becoming past due on loans, credit cards, rent, or mortgages, having debt sent to collections, and receiving utility shutoff or vehicle repossession notices. We use this information to evaluate the extent to which expense shocks increase the financial vulnerability of households.

Finally, we use households that said they did not experience an expense shock as a control group to estimate the effect of expense shocks on consumption. For these households, we capture measures of household finances and markers of financial distress in October 2025, five months before the start of the survey.

2.2 Expenditure Data

We complement the survey information with consumer purchase data from Numerator. For each household's logged shopping trip, we observe the date of the transaction, retailer, details of the items purchased, the unit price of items, the number of units purchased, and total expenditures.

To capture household consumption across broad consumption categories (e.g., food and non-alcoholic beverages) and durability classes (e.g., non-durables), we aggregate item-trip-level data into a monthly dataset of household consumption by category for our survey respondents. For this aggregation, we construct a crosswalk mapping 277 Numerator department labels to 12 COICOP consumption categories and four durability designations (durable, semi-durable, non-durable, and service).[8]

Consumer Expenditure Survey (CEX) Benchmarking Following the literature on tests of consumption insurance, our primary outcome is food spending. Food is a necessity whose purchase is difficult to defer, so a decline in food spending is one of the clearest signs that a household is exposed to uninsured risk (Blundell, Pistaferri, and Preston, 2008). As a secondary outcome, we use non-durable consumption, which adds to food the other goods classified as non-durable under COICOP 2018, such as alcoholic beverages, tobacco, household cleaning supplies, and personal-care consumables.

[7] We define "cash-buffer" months as the number of months of household expenses the household could cover with current balances in checking and savings accounts.

[8] We use the Detailed Structure Tables and explanatory notes of the 2018 Classification of Individual Consumption According to Purpose (COICOP) Manual United Nations Statistics Division (2018) for this classification. See Appendix B for details.

Numerator captures food spending well. To assess coverage, we compare average monthly expenditures by category in 2024 across three sources: the CEX, the December 2024 Numerator Consumer Panel, and our analysis sample.[9] We find that the panel closely tracks the CEX for food: average monthly food spending is $535 in the panel and $519 in the CEX. These results mirror similar validation exercises conducted by Allcott et al. (2026).

2.3 Sample

Our main analysis sample is a balanced panel of 3,712 households who recorded food and non-durable purchases in every month from August 2024 to June 2026. Of the 4,576 survey respondents, we retain the 3,910 who report monthly household income and, among those who report experiencing an expense shock, the size of their shock. We then exclude 198 households who do not record food and non-durable purchases in every month, and merge survey responses for the remaining households with their monthly consumption data.

Balancing the panel on recorded expenditures serves two purposes. For households that did not report expense shocks, it fixes the control group so that every treatment cohort is compared against the same households. For all households, it defines a population of consistent reporters among whom a decline in recorded expenditure can be interpreted as a decline in consumption rather than a lapse in activity on Numerator's receipt-capture app.[10] This interpretation requires that expense shocks do not affect whether or how intensively households report purchases. Both our balancing restriction and Numerator's survey eligibility rules condition on reporting after the shock.[11] We therefore test for reporting effects among households whose eligibility depended only on reporting before and during the shock: those whose most recent expense shock occurred in March 2026.[12] Supplementary Table C.2 shows that expense shocks have no effect on the probability that a household reports any spending, food spending, non-durable spending, or on the number of transactions it logs.

Sample Representativeness Our sample is broadly representative of U.S. households in census regions, but underrepresents high-income households and households with children. Table 1 compares our sample with the March 2026 Numerator panel and the March

[9] Numerator's rolling monthly panels consist of users who meet platform usage requirements over the 12 preceding months. The December 2024 panel therefore provides a reasonable measure of consumer spending over the whole year.

[10] Ganong et al. (2025) impose a similar minimum-activity requirement before and after unemployment insurance benefit exhaustion events.

[11] Numerator surveyed only panelists who reported at least two shopping trips in each of the 12 months from March 2025 to March 2026 and visited at least five distinct retailers over that period.

[12] This analysis uses all households who report income and all households who experienced expense shocks in March 2026 who report shock size.

2026 Current Population Survey Annual Social and Economic Supplement (CPS ASEC) on household income, the presence of children, marital status, and census region. Relative to the population-weighted ASEC, our sample underrepresents households with income above $125,000 (25.0% versus 35.0%). Households with children are also less common in our sample than in the U.S. population (26.7% versus 35.6%), though comparisons with the ASEC on children and marital status may partly reflect differences in definitions (see table notes). Our estimates should therefore be read as average effects for this sample rather than for the U.S. population. For completeness, the table also reports individual-level characteristics.[13]

3 Anatomy of Expense Shocks

This section describes the incidence, type, size, and financing of expense shocks in our sample. We find shocks are common and often large relative to monthly income. Additionally, the methods households use to pay for expense shocks vary sharply with income and liquid savings.

Incidence Exposure to expense shocks is widespread and is more correlated with household liquidity than income. Sixty-five percent of our sample reports at least one large unexpected expense between January 2025 and March 2026. Across bins of gross annual household income, the share reporting a shock ranges only from 62% to 68%, including among households earning more than $125,000. Across bins of liquid buffer, the share of households reporting an expense shock is 77% among those whose checking and savings would cover less than one month of expenses and 53% among those with six or more months of buffer (Figure 1). This pattern could also arise mechanically if households with large savings do not consider large outflows as expense shocks, which is insightful in itself.

Types Most shocks are related to assets, and this mix is similar across the resource distribution. Among respondents' most recent shocks, home and appliance repairs account for 26%, vehicle repair and replacement for 25%, and medical and dental bills for 14% (Figure 2, Panel A). Veterinary bills, utility bill increases, legal expenses, moving, and computer or phone repairs account for smaller shares. When we group shocks into four categories (home, vehicle, medical, and other), home-related shocks, which include utility bills and moving/relocation costs, make up about 39% of shocks in every income quartile, and shares are similar across cash-buffer bins (Supplementary Table C.3).

[13] Like Numerator panelists generally, our respondents are more likely to be female, though such differences matter less for our household-level analysis.

Table 1: Sample Representativeness

	(1) In sample	(2) All users	(3) US households (CPS)
Education			
High school or less	18.3	21.8	32.5
Some college	19.2	20.3	15.3
College or technical degree	41.8	40.3	35.9
Some or completed graduate education	20.7	17.6	16.2
Gender			
Male	22.2	19.2	51.1
Female	77.0	80.0	48.9
Other	0.8	0.8	0.0
Household income			
- $20k	7.6	9.8	9.7
$20k-40k	13.2	13.2	12.7
$40k-60k	15.9	15.4	12.5
$60k-80k	13.1	13.6	11.1
$80k-100k	13.4	12.4	9.3
$100k-125k	11.9	11.1	9.8
$125k +	25.0	24.5	35.0
Children in household			
No	73.3	67.5	64.4
Yes	26.7	32.5	35.6
Marital status			
Never married	15.9	17.5	24.7
Married or partnered	62.9	62.8	49.4
Separated, divorced, or widowed	21.2	19.7	26.0
Census region			
Northeast	17.8	17.7	17.1
Midwest	25.4	22.9	21.0
South	39.7	40.3	39.3
West	17.2	19.1	22.6
	0.0	0.0	0.0
N	3,316	199,852	53,295

Notes: Column (1) is the balanced analysis sample and column (2) all Numerator panelists active in March 2026, both unweighted, with characteristics taken from Numerator's panelist demographics. Column (3) is householders aged 18 and over in the March 2026 CPS Annual Social and Economic Supplement, weighted by the ASEC household weight. Income is annual household income; in the CPS, total household money income in calendar 2025. In the CPS, children are the householder's own children of any age living in the household. College or technical degree includes trade or technical degrees and 2- and 4-year college degrees; the CPS has no some-graduate-school category, so such householders are classed by their highest degree. Married or partnered includes households living with a partner in columns (1) and (2); the CPS records legal marital status only, so cohabiting householders are classed by that status. Percentages are computed among households in each column with a valid response.

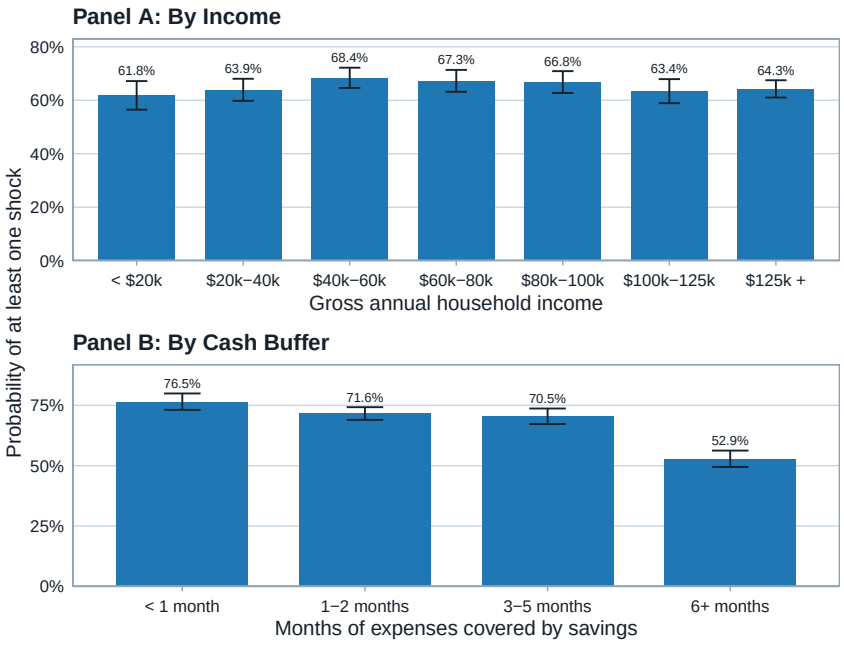


Figure 1: Probability of At Least One Expense Shock by Household Resources

Notes: Panel A shows the probability of reporting an expense shock by gross annual household income bin. Panel B shows the probability of reporting an expense shock by the months of household expenses that can be covered by current savings.

Size The typical shock is large relative to net monthly household income. The median shock is worth 46% of monthly income. The distribution is right-skewed: 28% of shocks exceed one month of income, 11% exceed three months, and 5% exceed six months, which pulls the mean up to 1.4 months of income.

Relative to income, shocks are larger for lower-income households. The median shock among households is worth 60% of monthly income in the bottom quartile and 33% in the top quartile. The tail is also thicker at the bottom: the 90th percentile is 3.8 months of income in the bottom quartile and 2.3 months in the top quartile.

Shock sizes also vary by type. Vehicle shocks tend to be the largest, worth on average 1.8 months of income. Home and medical shocks are smaller than the median shock (worth 40% and 44% of monthly income), but have substantial tails.

Payment Methods Households pay for most shocks with liquid assets and revolving credit. Conditional on making payments toward the most recent shock, 71% of respondents used checking and savings balances or charged the expense to a credit card and paid it off without carrying a balance, while 17% charged the expense to a credit card and carried a balance (Figure 2, Panel C; respondents could select more than one payment method). A

further 8.7% reported cutting other spending, which suggests a consumption response.

Higher-income and more liquid households are more likely to self-insure through cash holdings, while lower-income and less liquid households rely more on revolving credit and cutting other spending (Supplementary Table C.5). The use of savings and other cash equivalents rises from 65% in the bottom income quartile to 79% in the top, and from 54% among households with less than one month of buffer to 87% among those with six or more months. Financing expenses through a credit card paid off over time declines from 19% in the bottom income quartile to 13% in the top, and from 23% among the least liquid households to 9% among the most liquid. Cutting other spending follows the same pattern, declining from 10% in the bottom income quartile to 6% in the top, and from 11% among the least liquid households to 5% among the most liquid.

Where our survey overlaps with the Federal Reserve's 2025 SHED and Fulford and Low (2026), we find that features of expense shocks are similar. This supports using our sample to estimate the effects of expense shocks on household consumption. In our sample, 65% of households report a large unexpected expense, compared with 59% of SHED adults and 67.7% in Fulford and Low (2026). Vehicle and home repairs are among the most common shocks in all three.[14] Payment methods also resemble SHED respondents' answers for a hypothetical $400 expense, even though our median shock is much larger: 71.4% of our respondents paid with cash or its equivalent, compared with 63% in the SHED, and 17% carried a credit card balance, compared with 15% in the SHED.

[14]Share of most recent shocks in our survey vs. share in 2025 SHED and Fulford and Low (2026): vehicle 24.6%, 30%, and 32.6%; home and appliance 26.2%, 22%, and 22.9%.

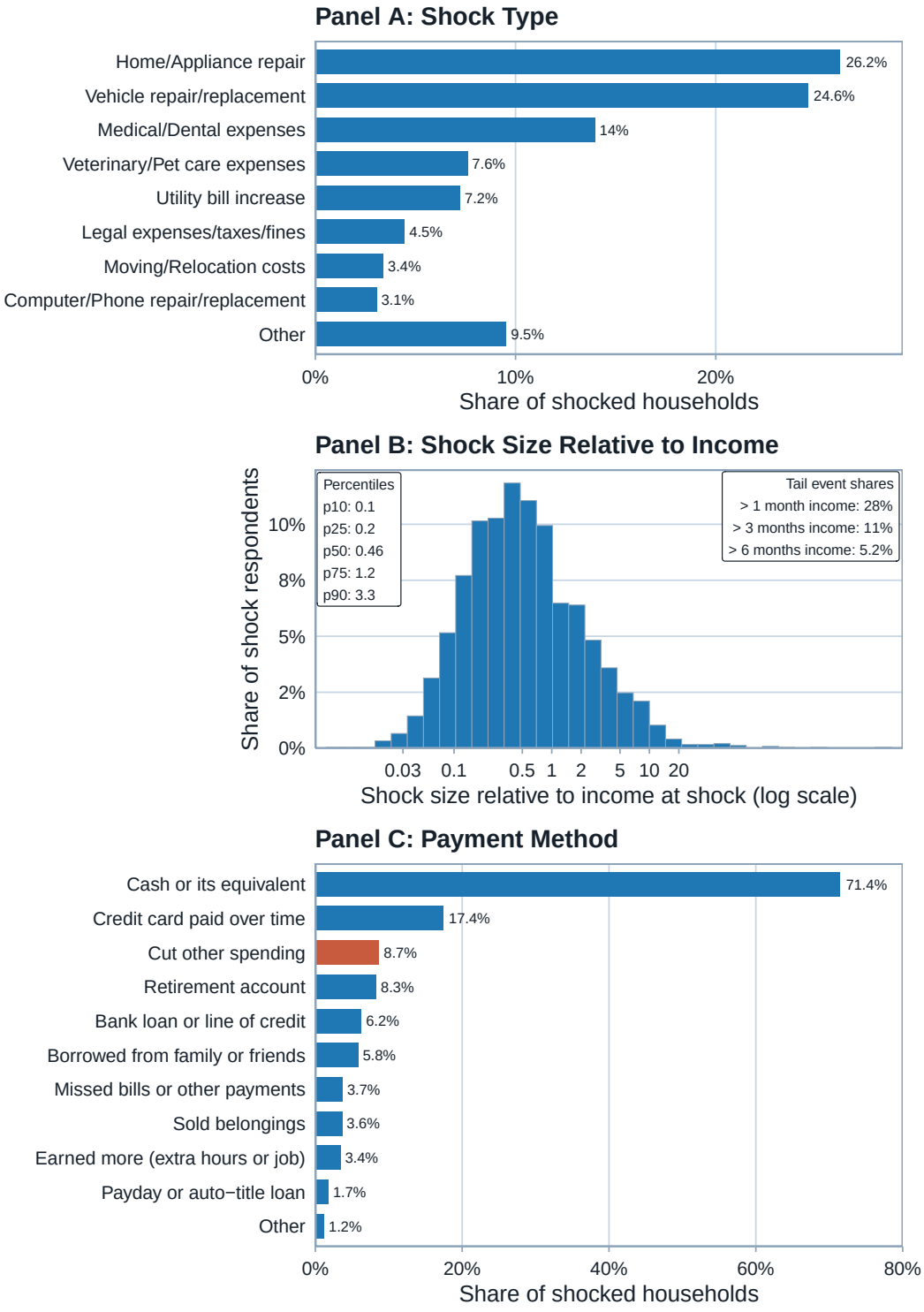


Figure 2: Type, Size, and Financing of Most Recent Expense Shock

Notes: Panel A is the type of the most recent shock. Panel B is the distribution of shock size relative to monthly income at the time of the shock (log scale). Panel C is how households paid for the most recent shock; respondents may select more than one payment method.

4 Effects of Expense Shocks on Household Consumption

In this section, we present descriptive and causal evidence for the effects of expense shocks on household consumption. Using the staggered DID framework of Callaway and Sant'Anna (2021), which accommodates staggered timing and heterogeneous treatment effects, we show that exposure to expense shocks has a real, negative, and transitory effect on household consumption. These average effects mask substantial heterogeneity. The effects of expense shocks on household consumption appear to be non-monotonic in shock size and are largest for households in the lowest income quartile.

4.1 Empirical Strategy

We classify a household as treated if it reports an expense shock between January 2025 and March 2026. Assuming the timing and incidence of expense shocks are exogenous, one might estimate their effect on consumption through a before–after comparison of outcomes among treated households. This comparison would be biased, however, because of seasonality: spending varies substantially from month to month for reasons unrelated to shocks. Figure D.2 shows that food and non-durable spending among never-treated households is 8–13% below its August 2024–June 2026 mean in February and 7–9% above it in December. A before–after comparison would therefore overstate the consumption drop after February shocks and understate it after November or December shocks.

To separate the effect of expense shocks from seasonal changes, we use a DID design that exploits variation in treatment timing across households. As treatment is staggered across fifteen months (Supplementary Figure D.1) and the effects of expense shocks on household consumption may change with the duration of exposure, we use the Callaway and Sant'Anna (2021) (CS) estimator, which compares each cohort with never-treated households. This approach avoids the bias that would result from using standard two-way fixed effects (TWFE) estimators, which would use earlier-shocked households as controls for later-shocked ones while the earlier households' responses are still evolving (Goodman-Bacon, 2021).[15] Figure 3 provides supportive evidence for such time-varying treatment effects. It plots average log consumption for treated and never-treated households in event time τ, normalized to $\tau = -2$.[16] The two series move in parallel before the shock, consistent with the parallel trends assumption, then diverge and converge again over subsequent months.

[15] In Supplementary Table C.7, we report TWFE estimates alongside CS estimates and document differences in magnitude and statistical significance.

[16] To construct the control series, we align never-treated consumption to each treatment cohort's calendar months, so that a given τ refers to the same calendar month for a cohort's treated and control households. We then average these cohort-specific series, weighting by the distribution of treatment timing.

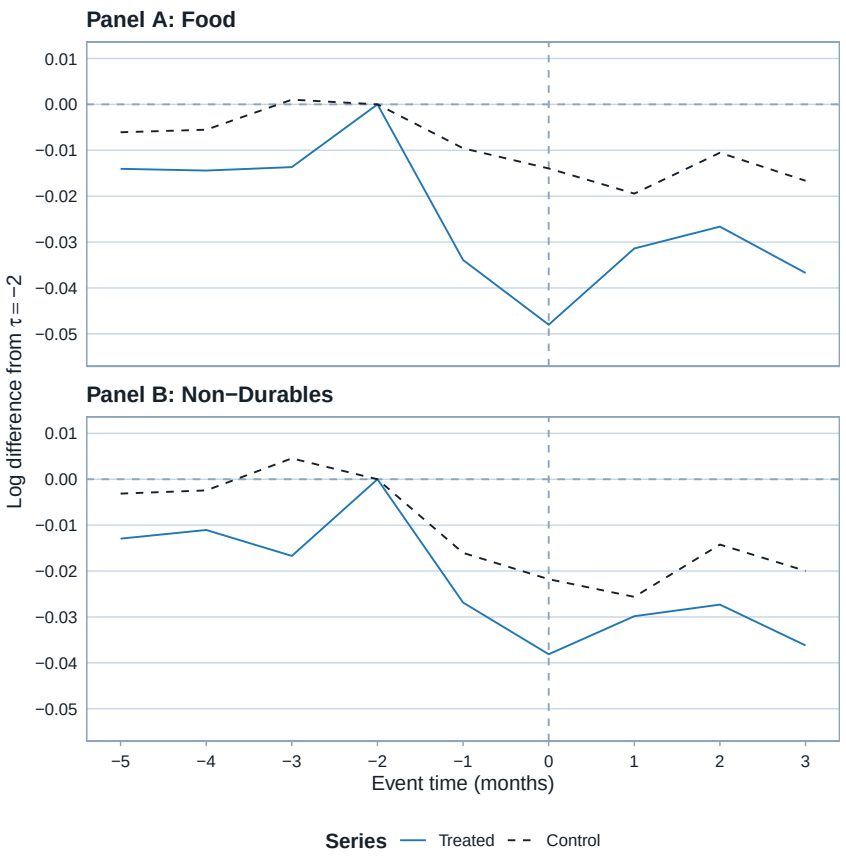


Figure 3: Average Expenditures by Treatment Status

Notes: Panel A plots average log expenditures on food and Panel B plots average log expenditures on non-durables for treated and never-treated households by event time τ (months relative to the shock), with each series normalized to zero at $\tau = -2$. The never-treated series aligns control households to each treatment cohort's calendar months, so that a given τ refers to the same calendar month for treated and control households, and averages these cohort-specific series weighted by the distribution of treatment timing.

Let G_i denote the month in which household i first reports an expense shock, with $G_i = \infty$ for households reporting no shock during the survey window, and let $\tau = t - g$ denote event time. Our parameter of interest is the group-time average treatment effect, the effect in month t of a first shock in month g:

$$ATT(g,t) = \mathbb{E}\left[Y_{i,t}(g) - Y_{i,t}(\infty) \mid G_i = g\right] \tag{1}$$

We estimate $ATT(g,t)$ using the Callaway and Sant'Anna (2021) estimator with never-treated households as the comparison group. As Figure 3 shows a consumption response at $\tau = -1$, consistent with households reporting shocks a month after they occurred, normalizing to $\tau = -1$ would bias estimates toward zero. We instead allow for at most one period of measurement error in the timing of the shock so that $Y_{i,t}(g) = Y_{i,t}(\infty)$ for $t < g-1$, use $g-2$

as a universal base period, and assume parallel trends relative to never-treated households:

$$\mathbb{E}\left[Y_{i,t}(\infty) - Y_{i,g-2}(\infty) \mid G_i = g\right] = \mathbb{E}\left[Y_{i,t}(\infty) - Y_{i,g-2}(\infty) \mid G_i = \infty\right] \quad \text{for all } t \geq g-1 \tag{2}$$

Restricting the sample to households with $G_i \in \{g, \infty\}$ and periods $s \in \{g-2, t\}$, we estimate $ATT(g,t)$ through the specification

$$Y_{i,s} = \alpha_{g,t} + \gamma_{g,t} C_i^g + \lambda_{g,t} \mathbf{1}\{s = t\} + \beta_{g,t}\left(C_i^g \cdot \mathbf{1}\{s = t\}\right) + \varepsilon_{i,s} \tag{3}$$

where $C_i^g = \mathbf{1}\{G_i = g\}$, and the OLS estimate $\hat{\beta}_{g,t}$ is a consistent estimator of $ATT(g,t)$.

We recover event-study coefficients by mapping group-time effect estimates to group-event time $\widehat{\beta}(g,\, t) = \widehat{ATT(g, g+\tau)}$ and averaging them across cohorts, using cohort shares among ever-treated households as weights:

$$\hat{\beta}_{es}(\tau) = \sum_{g \in \mathcal{G}} \hat{P}(G = g \mid G \leq \mathcal{T})\ \widehat{ATT}(g,\, g+\tau), \qquad \tau \in [-5, 3], \tau \neq -2 \tag{4}$$

We report uniform confidence bands from a multiplier bootstrap to account for multiple hypothesis testing over the full event-time window and assess parallel trends by testing whether the bands for $\tau \in \{-5, -4, -3\}$ contain zero.

Additionally, we recover overall treatment effect estimates by taking cohort-size-weighted averages of group treatment effects over the post-treatment window $\tau \in \{0, \ldots, 3\}$.

$$\hat{\beta}_O = \sum_{g \in \mathcal{G}} \hat{P}(G = g \mid G \leq \mathcal{T})\, \hat{\beta}(g) \qquad \hat{\beta}(g) = \frac{1}{4} \sum_{\tau=0}^{3} \widehat{ATT}(g,\, g+\tau) \tag{5}$$

Undated and Unobserved Shocks We identify the effects of expense shocks from the timing of each household's most recent outlay between January 2025 and March 2026 (the survey window). Our estimates of $ATT(g,t)$ could be biased by two kinds of expense shocks: outlays outside the survey window, which we do not observe, and earlier outlays within the survey window, which households report but do not date. We assess the sensitivity of our estimates to each of these concerns in turn (Supplementary Table C.7).

Additional shocks within the survey window, which affect 67% of treated households, could fall in their pre-periods. This could generate pre-trends or depress baseline consumption, attenuating $ATT(g,t)$ toward zero. Restricting the treated sample to the 31.7% of households reporting a single shock yields qualitatively similar estimates to our main specification, so we retain the full sample of treated households in our preferred specification for

statistical power.[17]

Conversely, shocks outside the survey window lead us to understate the effects of expense shocks. For cohorts shocked between January and May 2025, pre-periods extend before January 2025, so earlier unobserved shocks would attenuate estimates in the same way as having multiple reported but undated shocks. Among households that do not report shocks during the January 2025 through March 2026 sample period and serve as the control group, shocks between August and December 2024 or between March and June 2026 would contaminate the comparison group for the earliest and latest treated cohorts. To rule out both sources of bias, we restrict the sample to cohorts treated between June and December 2025, whose pre- and post-treatment periods fall entirely within the survey window.[18] Estimates from this sample are larger in absolute value than our main estimates and similarly significant, consistent with unobserved shocks attenuating our main estimates.

4.2 Average Effects of Expense Shocks on Household Consumption

We begin by estimating the average effect of experiencing an expense shock on household expenditures.

Expense shocks reduce household food expenditure in the month in which they occur, and this effect is not persistent. Figure 4 shows event-study estimates of the effect of expense shocks on log food expenditure (Panel A) and non-durables (Panel B). In the month of the expense shock ($\tau = 0$), food expenditures decline by 3.4%. This effect reflects a real change in household consumption, evidenced by a 3.8% decrease in food units purchased in the same period in Supplementary Figure D.3. For non-durables, we find no statistically significant effects on expenditures at any event time.

Our estimates imply a low elasticity of food consumption to expense shocks. Aggregated over the post-shock window, shocks reduce food expenditure by 2.1% on average (Table 2, Column 1), mainly through the contemporaneous response in Figure 4. This implies an elasticity of -0.02 at the mean shock size and -0.04 at the median, that is, an expense shock equivalent to 10% of monthly household income reduces food spending by 0.2 to 0.4%.

These elasticities are on the lower end of existing estimates of the consumption response to transitory income shocks. At monthly frequency, Ganong et al. (2025) find an elasticity of 0.22 for non-durable spending and groceries: a 10% transitory labor-income shock raises same-month spending by 2.2%. In annual panel data, Blundell, Pistaferri, and Preston

[17]This restriction yields 785 treated households, while the never-treated sample is unchanged. 24 treated households from the main sample do not report the number of shocks they experienced, so are excluded from this subsample by default.

[18]This restriction more than halves our sample of treated households to 1,126, and the never-treated sample remains unchanged.

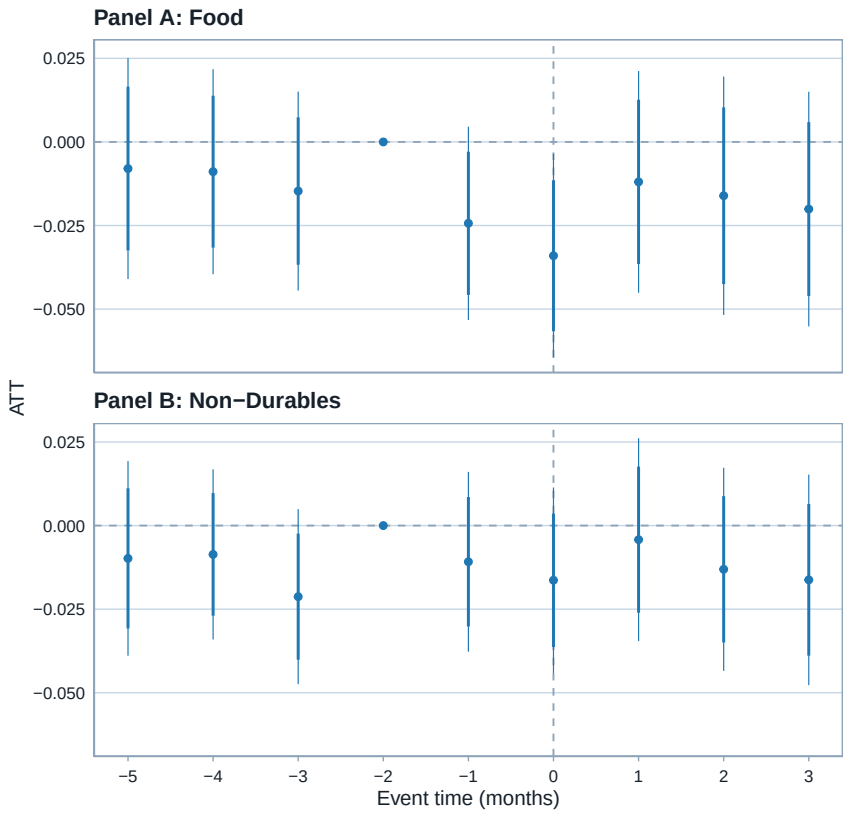


Figure 4: Event-Study Estimates: Expenditures

Notes: Panel A plots event-study estimates for the average effect of expense shocks on log expenditures for food, and Panel B plots estimates for non-durables. Thick bars reflect pointwise 95% confidence intervals, while thinner bars show 95% uniform confidence bands to account for multiple hypothesis testing over the studied event-time window.

(2008) estimate near-complete insurance against transitory shocks, with a pass-through of about 0.05. In absolute value, our elasticities are closest to Blundell, Pistaferri, and Preston (2008), even though they are measured at the same monthly frequency as Ganong et al. (2025).

Our pooled treatment effect estimates for food are robust to two restrictions on the treated sample, each addressing potential measurement error concerns with regard to expense shocks (Column 1, Supplementary Table C.7). First, households that anticipated their shock may have adjusted consumption beforehand, contaminating the pre-shock comparison period. Restricting the treated sample to households that retrospectively assigned the shock a probability of 25% or less yields an overall effect of −2.9% (main estimate: −2.1%). Second, misreported income or shock size would place households in the wrong shock-size bin. Restricting the treated sample to households that report both amounts consistently yields

Table 2: Consumption Response to Expense Shocks: Expenditure

	(1) Food	(2) Non-Durables
ATT	-0.021**	-0.012
	(0.010)	(0.009)
Mean-Implied Elasticity	-0.02	-0.01
Median-Implied Elasticity	-0.04	-0.03
N treated	2,424	
N never-treated	1,288	
Average shock as a share of income, %	135.4	
Median shock as a share of income, %	45.7	

Notes: The first row reports average treatment effects on the treated, standard errors in parentheses, for log expenditure on food (column 1) and non-durables (column 2). The estimation sample is restricted to households with observed food and non-durable expenditures in every month from August 2024 to June 2026. The ATT is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The elasticity rows report the elasticities implied by the ATT: the average treatment effect, in percent, divided by the average and median shock, respectively, as a share of net monthly take-home household income at the time of the shock, reported in the final two rows.

an estimate of -1.9%.[19]

We favor our back-of-the-envelope method of computing elasticities over alternatives such as regressing log outcomes on a continuous log normalized shock variable in a TWFE specification. Barring the standard issues that would bias the resulting estimates given the staggered treatment and time-varying treatment effects of our setting, a TWFE approach would require the assumption that the dose-response relationship between expense shocks and consumption is linear. As we show in Section 4.3, the effects of expense shocks on household consumption are non-monotonic. Thus, it would be more appropriate to apply the Callaway, Goodman-Bacon, and Sant'Anna (2025*b*) framework for estimating DID with continuous treatment. Conditional on correctly specifying the parametric form of the dose-response relationship, we could recover the slope of the dose-response function—i.e., the elasticity of consumption over the entire dose distribution and a scalar summary parameter. Appendix E shows we are presently underpowered to do this.

[19]We elicit income and shock size both as a categorical range and as a dollar figure. When the dollar figure falls outside the selected range, we replace it with the range midpoint (income: $N = 688$; shock size: $N = 248$). This restriction drops those imputed observations, leaving 2,704 households, of which 67% are treated.

4.3 Effects of Expense Shocks on Household Consumption by Shock Size

The small pooled average treatment effects estimated in Section 4.2 mask a larger and more persistent response among households with medium-sized shocks. For households whose shocks equal 0.5–1 months of income, 18.6% of treated households, food expenditure is 6.5–11.4% lower and non-durable expenditure 6.9–8.7% lower over the post-treatment window, as shown by Figure 5. By contrast, responses in the pooled sample are smaller and limited to the month of the shock (Figure 4).

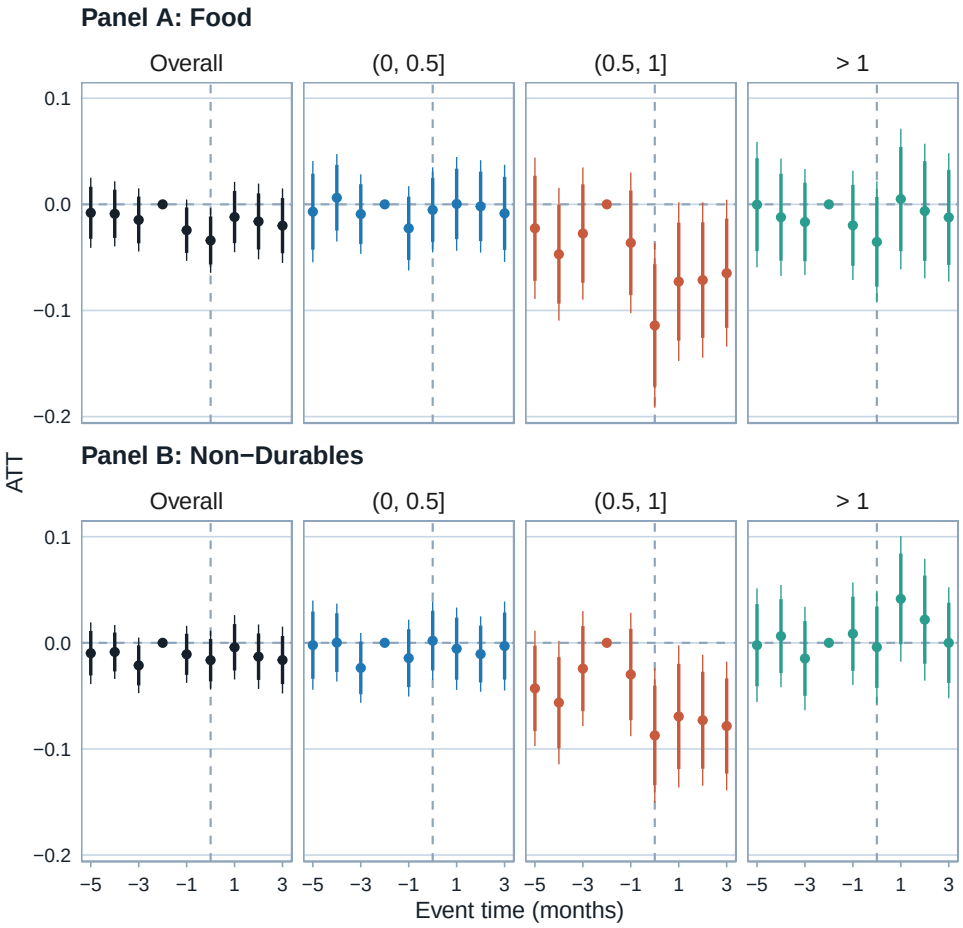


Figure 5: Event-Study Estimates by Shock Size: Expenditures

Notes: Panel A plots event-study estimates for the average effect of expense shocks on log expenditures for food, and Panel B plots estimates for non-durables, for all treated households and by the size of the shock normalized by net monthly household income. Thick bars reflect pointwise 95% confidence intervals, while thinner bars show 95% uniform confidence bands to account for multiple hypothesis testing over the studied event-time window.

Aggregated over $\tau \in [0, 3]$, households with medium-sized shocks reduce food expenditure

by 8.1% and non-durable expenditure by 7.7%, compared with 2.1% for food in the pooled sample (Table 3). This group alone accounts for roughly 70% of the pooled food effect, and its implied consumption elasticity of −0.11 for both categories is about five times the pooled elasticity at the mean shock size. We find no evidence of a statistically significant consumption response among households with smaller and larger shocks for either outcome. Our confidence intervals, however, do not rule out economically meaningful responses in these groups. Our results are robust to alternative specifications, such as restricting treated households to those with plausibly unexpected shocks, excluding treated households with imputed income and shock size values, and choosing a later reference period ($\tau = -3$).[20] This suggests the consumption response to expense shocks is non-monotonic in shock size.

Table 3: Consumption Response by Shock Size: Expenditure

	(1) Overall	(2) (0, 0.5]	(3) (0.5, 1]	(4) > 1
Panel A: Food				
ATT	-0.021**	-0.004	-0.081***	-0.012
	(0.010)	(0.013)	(0.022)	(0.018)
Mean-Implied Elasticity	-0.02	-0.02	-0.11	-0.00
Median-Implied Elasticity	-0.04	-0.02	-0.11	-0.01
Panel B: Non-Durables				
ATT	-0.012	-0.004	-0.077***	0.015
	(0.009)	(0.012)	(0.018)	(0.016)
Mean-Implied Elasticity	-0.01	-0.02	-0.11	0.00
Median-Implied Elasticity	-0.03	-0.02	-0.11	0.01
Share of treated, % (N = 2,424)	100.0	53.1	18.6	28.2
Average shock as a share of income, %	135.4	22.9	73.3	388.0
Median shock as a share of income, %	45.7	20.2	70.5	233.8

Notes: Column (1) uses the full balanced sample. Columns (2)–(4) group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock. Each column uses all 1,288 never-treated households as controls. The ATT effect is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The mean- and median-implied elasticities are the average treatment effect, in percent, divided by the column's average and median shock, respectively, as a share of net monthly take-home household income at the time of the shock, reported in the final two rows.

On its own, variation in statistical significance along the normalized shock size distribution is not sufficient evidence against monotonicity. We therefore directly test whether overall effects differ between adjacent bins, using standard errors that account for the covariance between estimates that share the same never-treated comparison group (Appendix **??**). For both food and non-durables, the effect on consumption is significantly larger for shocks of 0.5–1 months of income than for smaller shocks, and significantly smaller for shocks above

[20] See Supplementary Table C.7 for more details.

one month of income than for shocks of 0.5–1 months (Supplementary Table C.12). We conclude that the consumption response is non-monotonic in shock size for both outcomes.

The non-monotonic relationship between shock size and consumption responses is not an artifact of how we bin shock size. Figure 6 plots average treatment effects over the full distribution of shock size as a share of income, estimated parametrically using the Callaway, Goodman-Bacon, and Sant'Anna (2025*b*) DID framework for continuous treatments.[21] For non-durables, we find statistically significant effects for shocks worth 50% to 83% of income under uniform 95% confidence bands, which account for testing at every shock size simultaneously. Effects peak at shocks worth 70% of income (ATT −6.4%) and decline toward zero on either side, mirroring the binned estimates. Pointwise 95% confidence intervals widen the significant range to 45%–100% of income.[22] For food, the pattern is the same but less precisely estimated. Pointwise 95% confidence intervals show significant effects for shocks between 45% and 95% of income, peaking at 70% of income (ATT −6.1%). Under uniform bands, no shock size produces a statistically significant difference. We attribute this to limited power—our current sample is not large enough to precisely estimate effects at each shock size across the full distribution.

What might explain the non-monotonicity of consumption responses? One possibility is that shock size proxies for other determinants of the consumption response, such as household resources or shock type. If poorer households are more likely to experience medium-sized shocks, their larger consumption responses could reflect a resource mechanism—they lack the liquidity and savings to finance these outlays without cutting consumption. Alternatively, if particular kinds of shocks elicit larger consumption responses and are concentrated in the middle bin, for example because their payment is not deferrable (as in the case of car repairs, which must typically be paid before the vehicle is returned), shock type could explain the pattern. We fail to find support for either mechanism. First, Table 4 shows little variation in the distributions of income (Panel A) and shock types (Panel B) across shock size bins. Second, consumption effects are consistently largest in the medium bin within income quartiles and within shock types.[23] Appendix Table C.10 examines heterogeneity in pass-through across household characteristics.

Differences in credit access also do not explain our results. Credit constraints would generate a non-monotonic response only if households with medium-sized shocks were the least able to shift expenses onto revolving credit. Instead, the share of households whose available credit card limit covers the shock declines monotonically with shock size, from

[21] Our preferred specification is a quadratic polynomial with three spline knots. See Appendix E for details.

[22] Results are similar with four and five spline knots: uniform bands show significant effects over 60%–105% and 64%–83% of income, respectively.

[23] See Supplementary Tables C.9 and C.11.

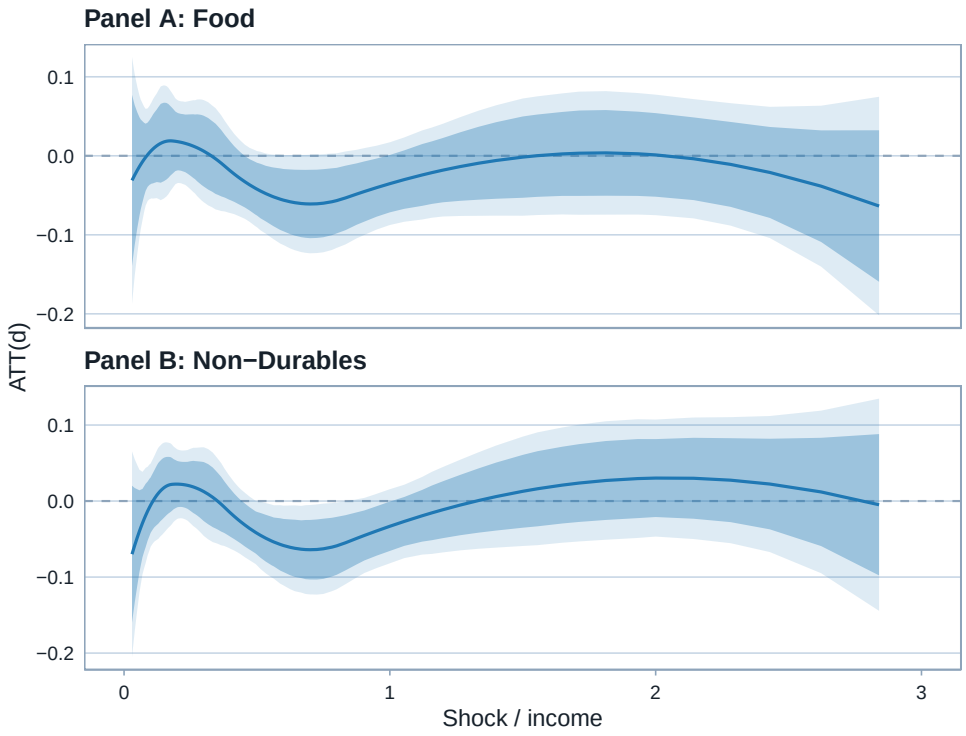


Figure 6: Consumption Response by Continuous Shock Size: Expenditures

Notes: Panel A plots estimates of $ATT(d|d)$, the average effect of an expense shock on log food expenditures, and Panel B plots estimates for non-durables, among households whose shock equals d, where d is shock size as a share of monthly income. Estimates use the Callaway, Goodman-Bacon, and Sant'Anna (2025*b*) estimator with a quadratic B-spline in d with three interior knots, averaged across timing groups and post-shock months $\tau \in [0, 3]$. Darker bands are pointwise 95% confidence intervals; lighter bands are 95% uniform confidence bands, which hold simultaneously across all values of d.

66.7% in the smallest bin to 51.8% in the largest (Panel C). If households only relied on credit cards to pay for expense shocks, the largest shocks would produce the largest consumption responses.

In practice, households finance expense shocks in many ways, and the mix of payment methods they use changes as outlays grow. This might explain the null consumption effects for the largest shocks. Among households who report paying off at least some of the outlay, the share using a formal loan (bank, credit union, payday, or home equity) rises from 2.5% in the smallest bin to 6.6% in the middle bin and 13.7% for shocks larger than one month of income (Panel D). Cash, liquid savings, and revolving credit can absorb small shocks, but these sources run short as outlays grow. Formal loans and retirement withdrawals supply larger amounts of liquidity and spread repayment over a longer horizon, but carry fixed costs, such as origination fees and early-withdrawal penalties, that may be worth paying only for large outlays. Consistent with this interpretation, larger shocks take longer to

repay: 76.5% of households in the smallest shock bin report having paid off their shock within three months of its occurrence, compared with 52.5% in the largest shock bin (Panel E). By spreading repayment over many months, these instruments reduce the need to finance outlays through reductions in consumption in any single month, which could explain why consumption does not respond to the largest shocks.[24]

4.4 Effects of Expense Shocks on Household Consumption by Household Income

Consumption declines after a shock are concentrated in the bottom half of the income distribution. We estimate average treatment effects separately by quartile of net monthly household income, comparing treated households with never-treated households in the same quartile (Table C.9; Panel A for food, Panel B for non-durables). Food expenditure falls by 5.1% in the bottom quartile and 4.1% in the second, and non-durable expenditure falls by 3.3% and 3.6%. In the top two quartiles, the estimates are smaller and not statistically significant.

Consumption falls most after medium-sized shocks, not the largest ones. We split treated households by shock size relative to monthly income: below half a month, half a month to one month, and above one month. Shocks below half a month of income reduce spending only in the bottom income quartile, by 7.8% for food and 5.9% for non-durables. Shocks worth half a month to one month of income reduce food and non-durable spending by about 8–10% in each of the bottom three quartiles, but not in the top quartile. Shocks worth more than one month of income yield smaller estimates that are mixed in sign and statistically insignificant at the 5% level.

4.5 Effects on Other Financial Outcomes

We document small effects on food consumption on average, with larger impacts on lower-income households and for medium-sized shocks. We now show an association between the occurrence of expense shocks and financial distress within six months of the shock, using the subset of treated households whose most recent reported expense shock had occurred at least six months before the survey was fielded. Although these outcomes are self-reported within the survey, they provide insight into the effects of expense shocks beyond household consumption.

[24] In future work, we intend to explore the welfare implications of this shift in smoothing technology in a model of consumption and financing that depends on shock size.

Table 4: Shock Characteristics by Shock Size

	(1) Overall	(2) (0, 0.5]	(3) (0.5, 1]	(4) > 1
Panel A: Income quartile				
Q1 (Lowest)	0.266	0.227	0.332	0.297
Q2	0.284	0.277	0.281	0.300
Q3	0.228	0.245	0.212	0.205
Q4 (Highest)	0.222	0.251	0.175	0.199
N (respondents)	2,424	1,288	452	684
Panel B: Shock type				
Home	0.388	0.410	0.347	0.373
Vehicle	0.246	0.234	0.212	0.289
Medical	0.216	0.218	0.270	0.175
Other	0.151	0.137	0.170	0.162
N (respondents)	2,424	1,288	452	684
Panel C: Credit card access				
No credit	0.161	0.146	0.132	0.209
Insufficient credit	0.217	0.187	0.219	0.273
Sufficient credit	0.622	0.667	0.648	0.518
N (respondents)	2,350	1,256	438	656
Panel D: Payment method (select all that apply)				
Cash or its equivalent	0.714	0.744	0.686	0.675
Credit card paid over time	0.174	0.181	0.215	0.129
Bank loan or line of credit	0.062	0.025	0.066	0.137
Payday or auto-title loan	0.017	0.015	0.017	0.023
Retirement account	0.083	0.055	0.092	0.132
Cut other spending	0.087	0.079	0.090	0.101
Other	0.145	0.114	0.147	0.208
N (respondents)	2,241	1,221	423	597
Panel E: Share of shock paid within 3 months				
None	0.072	0.049	0.059	0.120
Some	0.211	0.145	0.234	0.310
Most	0.044	0.038	0.056	0.045
All	0.674	0.768	0.651	0.525
N (respondents)	1,946	999	372	575

Notes: Columns group shocked respondents by expense size relative to net monthly household income. Cells are shares of respondents answering each question. Each panel's N row counts the respondents in that column who answered that panel's question, so N differs across panels. Panel A: quartiles of net monthly household income at the time of the shock, cut over the full balanced sample of shocked and never-shocked respondents, so shocked respondents need not split evenly across them. Panel B: Home is home and appliance repairs, utility and rent increases, and moving; Medical is medical, dental and veterinary. Panel C: whether the household had a credit card at the time of the shock and, if so, enough available credit to cover it. Panel D conditions on respondents who made any payment toward the shock. Cash or its equivalent is checking and savings accounts or a credit card paid in full at the end of the month, as in the Federal Reserve's SHED; bank loan or line of credit includes home equity lines of credit; Other pools borrowing from family and friends, buy now, pay later and payment plans, working more, missed payments, selling belongings, insurance and other. Panel E is restricted to respondents whose shock occurred in or before December 2025, so the three months after the shock fall before the March 2026 survey. Some and Most pool follow-up answers of about 0–50% and about 75–100%.

Table 5: Consumption Responses by Shock Size and Income: Expenditure

	(1) Overall	(2) Q1 (Lowest)	(3) Q2	(4) Q3	(5) Q4 (Highest)
Panel A: Food					
Overall	-0.020**	-0.051**	-0.041**	-0.015	0.034
	(0.010)	(0.020)	(0.019)	(0.021)	(0.021)
(0, 0.5]	-0.003	-0.078***	0.004	0.012	0.034
	(0.013)	(0.027)	(0.027)	(0.026)	(0.028)
(0.5, 1]	-0.081***	-0.092**	-0.105***	-0.097**	-0.003
	(0.022)	(0.047)	(0.035)	(0.045)	(0.043)
> 1	-0.012	0.018	-0.081***	-0.019	0.056
	(0.017)	(0.032)	(0.031)	(0.046)	(0.039)
Panel B: Non-Durables					
Overall	-0.013	-0.033*	-0.036**	0.004	0.017
	(0.009)	(0.018)	(0.017)	(0.020)	(0.020)
(0, 0.5]	-0.005	-0.059**	-0.010	0.023	0.014
	(0.012)	(0.024)	(0.025)	(0.023)	(0.025)
(0.5, 1]	-0.077***	-0.082**	-0.091***	-0.085**	-0.042
	(0.019)	(0.039)	(0.031)	(0.041)	(0.039)
> 1	0.015	0.041	-0.050*	0.020	0.060*
	(0.017)	(0.031)	(0.029)	(0.045)	(0.034)
Share of treated, % (N = 2,424)	100.0	26.6	28.4	22.8	22.2
Share of never-treated, % (N = 1,080)	100.0	22.0	26.9	21.2	29.8

Notes: Column (1) uses the full balanced sample. Columns (2)–(5) group households by quartile of net monthly take-home household income, measured at the time of the shock for shocked households and in October 2025 otherwise. Each quartile's shocked households are compared with never-treated households in the same quartile. Rows group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock, so each cell restricts shocked households on both dimensions. The outcome is log expenditure. The ATT is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Expense shocks are associated with substantially higher financial distress, especially among lower-income households. Six months after a shock, treated households are 15 percentage points more likely than never-treated households to report any marker of distress, more than doubling the never-treated rate of 8.5% (Table 6).[25] Expense shocks are also strongly associated with delinquency—treated households are 7 percentage points more likely to be past due on a loan or credit card and 5.3 points more likely to be past due on rent or a mortgage, relative to never-treated rates of 2.7% and 1.3%. In absolute terms, these gaps are largest in the bottom income quartile. Among the poorest households in our sample, treated households are 24 points more likely to report any distress (base rate 24.2%), 13.9 points more likely to be past due on a loan or credit card (base rate 8.5%), and 12.7 percentage points more likely to be past due on rent or a mortgage (base rate 3.3%). Collectively, these results are consistent with the increase in financial distress after expense shocks documented by Fulford and Low (2026).

Financial distress also varies with shock size, but unlike consumption, it does not peak at medium-sized shocks (Table C.13). Instead, distress is comparably high among households with small and medium shocks and much lower among those with large shocks. Among the poorest households, the distress gap between treated and never-treated households is 10.3 percentage points larger for shocks of at most half a month of income than for shocks above one month. Similarly, the gap is 10.4 points larger for shocks of half to one month of income than for shocks above one month. We observe similar patterns across all income quartiles and multiple distinct measures of financial distress. Combined with the consumption results, this suggests that households absorb shocks of different sizes in different ways. Households with smaller shocks do not cut down on consumption but are more likely to fall behind on payments, suggesting they smooth consumption at the cost of short-run financial distress. Households with medium-sized shocks both cut consumption and fall behind on payments. Households with the largest shocks show smaller differences on both margins. This is consistent with the financing patterns in Table 4, where households cover small shocks with savings and credit cards and large shocks with instruments that spread repayment over time.

5 Conclusion

We find that expense shocks are a significant source of risk for households: they are common and often large relative to monthly income. These shocks modestly reduce food and nondurable consumption on average, but average effects mask larger declines among lower-income households. The consumption response to expense shocks also appears to be non-

[25] Outcomes for never-treated households are measured retrospectively from October 2025 to March 2026.

Table 6: Financial Distress

	(1) Overall	(2) Q1 (Lowest)	(3) Q2	(4) Q3	(5) Q4 (Highest)
Any financial distress	14.9***	24.0***	11.2***	4.0	4.2
	(1.4)	(3.9)	(2.7)	(2.3)	(1.8)
	[8.5]	[24.2]	[8.5]	[5.5]	[3.2]
90+ days past due on loan/credit card	7.0***	13.9***	4.0	1.7	1.4
	(0.9)	(2.8)	(1.6)	(1.4)	(0.9)
	[2.7]	[8.5]	[2.6]	[1.8]	[0.6]
90+ days past due on rent/mortgage	5.3***	12.7***	2.3	0.8	0.7
	(0.8)	(2.2)	(1.4)	(1.0)	(0.5)
	[1.3]	[3.3]	[2.2]	[0.9]	[0.0]
Debt sent to collections	2.6	-0.4	2.6	0.4	1.1
	(0.9)	(3.0)	(1.7)	(1.5)	(1.0)
	[4.5]	[14.7]	[3.7]	[2.7]	[1.0]
Utility shutoff notice	3.4***	4.4	3.9	1.8	1.1
	(0.7)	(2.0)	(1.5)	(1.3)	(1.2)
	[2.0]	[4.3]	[1.8]	[1.4]	[1.6]
Auto repossession notice	2.1***	2.7**	2.6**	1.7	1.0
	(0.4)	(0.8)	(0.8)	(0.8)	(0.6)
	[0.0]	[0.0]	[0.0]	[0.0]	[0.0]
Share of treated, % (N = 1,337)	100.0	30.3	26.3	21.4	22.1

Notes: Each cell is the treated minus never-treated difference in the share reporting the outcome, in percentage points. Heteroskedasticity-robust standard errors are in parentheses; the never-treated mean, in percent, is in brackets. Column (1) compares all treated with all never-treated respondents. Columns (2)–(5) compare them within each quartile of net monthly household take-home income, estimated jointly with quartile fixed effects. Outcomes cover the six months after the shock (treated) or October 2025–March 2026 (never-treated). Treated respondents are those whose shock fell in or before September 2025, so the six months after it end by March 2026. Any financial distress means at least one of the five outcomes below it. Stars use Holm–Bonferroni-adjusted p-values within the table: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

monotonic in shock size: spending falls most after shocks of half a month to one month of income, while smaller and larger shocks produce little change in food and non-durables. We conclude that this pattern likely reflects differences in financing. Larger shocks are more likely to be financed with formal loans and other costly instruments that can insulate contemporaneous spending. Expense shocks are also associated with substantially more financial distress in the following months, especially in the bottom income quartile.

In upcoming work, we will ask whether a model of consumption smoothing with fixed adjustment costs and costly borrowing can rationalize this non-monotonic response. The model will provide a quantitative measure of the impact of expense-shock volatility, taking into account the features of the shock process and the strategies households use to smooth them.

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Appendix

A Full Questionnaire

Legend. *Blue text is skip logic and notes, not shown to respondents.* NS = "I'm not sure / I don't recall"; PNA = "I prefer not to answer". All questions require a response. Question numbers match the survey platform.

Screening: In this survey, we will ask you about unexpected expenses your household may have faced during the last year. Please keep in mind the difference between expected and unexpected expenses. Expected expenses are costs you can plan for in advance, such as rent, utility bills, or routine car maintenance. Unexpected expenses are costs you had no reason to anticipate, such as a sudden medical emergency or an urgent home or car repair. As you answer, please only consider unexpected expenses and try not to include expenses you had anticipated or planned for.

1. Since the beginning of 2025, did your household have a large and unexpected expense?
   Yes; No; NS
   Respondents who answer "No" or NS skip to the end of the survey. All later questions are shown only to those who answer "Yes".
2. How many times since the beginning of 2025 did your household have a large and unexpected expense?
   1 time; 2 times; 3 times; 4 times; 5 to 10 times; More than 10 times; NS

The expense and how it was paid: Please think about the most recent time your household had a large and unexpected expense.

3. For the most recent time, what type of large and unexpected expense was this?
   A major out-of-pocket medical or dental expense; An unplanned gift or loan to a family member or friend; A major vehicle repair or replacement; A major home or appliance repair; A computer or mobile phone repair or replacement; Legal expenses, taxes or fines; Funeral or burial expenses; An increase in childcare or dependent care expenses; A large and unexpected increase in rent payment; Moving or other relocation costs; A major veterinary or pet care expense; A high electric, heating/cooling, or other utility bill; Some other large and unexpected expense (please specify)
4. About how much money was the total cost for this large and unexpected expense? Please estimate the full amount of this expense even if you did not pay all of it at once or if you are paying over time.
   $0–$50; $50–$100; $100–$200; $200–$300; $300–$500; $500–$700; $700–$1,000; $1,000–$1,500; $1,500–$2,000; $2,000–$3,000; $3,000–$5,000; $5,000–$10,000; $10,000–$20,000; More than $20,000; NS
5. To the best of your memory, what was the approximate total cost for this large and unexpected expense?
   Shown if: any answer other than NS to Q4.
   (Insert number)

6. Did you pay the full amount of this large and unexpected expense within three months after you had this expense?

   Yes, I paid all of the expense within three months; No, I paid only some of the expense within three months; No, I did not pay any of the expense within three months; NS

7. Within three months after you had this large and unexpected expense, about how much of it had you paid off?
   Shown if: "No, I paid only some of the expense within three months" to Q6.

   I only paid very little (close to 0%); I paid about one-quarter (25%) of the expense; I paid about half (50%) of the expense; I paid about three-quarters (75%) of the expense; I paid almost all (almost 100%) of the expense; NS

8. How did you pay for this large and unexpected expense? Please select all that apply.
   Shown if: "Yes, I paid all of the expense within three months" or "No, I paid only some of the expense within three months" to Q6.

   I used money in checking and savings accounts; I used money in retirement accounts; I paid it using my credit card and paid the full amount at the end of the month; I paid it using my credit card and paid the amount over multiple months; I borrowed from family and/or friends; I used a payday or auto-title loan; I took a loan from a bank or a credit union; I took out or used a home equity line of credit; I reduced my spending on other expenses; I missed some bills or other payments; I increased my income by working more hours or at another job; I sold some belongings; Other (please specify); NS

9. At the time you had this large and unexpected expense, what was your household's monthly take-home income (your household's monthly income after taxes)? Please include all sources of income such as income from jobs, any social security or government payments, and any retirement income.

   $0–$500; $500–$1,000; $1,000–$1,500; $1,500–$2,000; $2,000–$3,000; $3,000–$4,000; $4,000–$5,000; $5,000–$7,000; $7,000–$10,000; $10,000–$15,000; $15,000–$20,000; $20,000–$25,000; More than $25,000; NS; PNA

10. About how much was your household's monthly take-home income (your household's monthly income after taxes) at the time you had this large and unexpected expense?
    Shown if: any answer other than NS or PNA to Q9.

    (Insert number)

11. Approximately when did this large and unexpected expense occur?

    In January, February, or March of 2025; In April, May, or June of 2025; In July, August, or September of 2025; In October, November, or December of 2025; In January, February, or March of 2026; NS

12–16. In approximately what month did you have this large and unexpected expense?
    Shown if: any answer other than NS to Q11. One question is shown for the quarter selected in Q11.

    The three months of that quarter (for example, January 2025, February 2025, or March 2025); NS

Anticipation and financial resources at the time of the expense:

17. Right before this large and unexpected expense occurred, what did you think were the chances

this event would occur?

I did not think it would happen at all (about 0% chance); I thought it was unlikely to happen (about 25% chance); I thought it was equally likely and unlikely to happen (about 50% chance); I thought it was likely to happen (about 75% chance); I was certain it would happen (about 100% chance); I'm not sure

18. Looking back at this large and unexpected expense, could your household have taken steps to prevent it from happening or reduce its cost?

    Yes, we could have prevented it or reduced the cost of the expense (for example, with preventive care, maintenance, or insurance); No, there was nothing we could have done to prevent it or reduce the cost; I'm not sure

19. At the time you had this large and unexpected expense, about how much money did your household have in all your checking and savings accounts (excluding retirement accounts)?

    $0–$100; $100–$500; $500–$1,000; $1,000–$2,500; $2,500–$5,000; $5,000–$7,500; $7,500–$10,000; $10,000–$20,000; $20,000–$50,000; More than $50,000; NS; PNA

20. At the time you had this large and unexpected expense, about how many months of your household's typical monthly expenses could you cover using money you have saved in all your checking and savings accounts (excluding retirement accounts)?

    Less than 1 month; 1 month; 2 months; . . . ; 12 months; More than 12 months; NS

21. At the time you had this large and unexpected expense, did you have access to a credit card that could cover the full expense cost?

    Yes, and I had enough available credit to cover the full expense; Yes, but I did not have enough available credit to cover the full expense; No, I did not have access to any credit cards; NS

22. At the time you had this large and unexpected expense, what was your credit score?

    780–850; 720–780; 660–720; 600–660; 540–600; 300–540; NS; PNA

Aftermath and expectations:

23. Looking back at when you experienced this large and unexpected expense, did you experience any additional, smaller unexpected expenses during the same month?

    Yes; No; NS

24. To the best of your memory, what was the approximate total cost for all unexpected expenses you experienced that month?

    Shown if: "Yes" to Q23.

    (Insert number)

25. Within six months after this large and unexpected expense, did any of the following occur? Please select all that occurred.

    I became 90 or more days past due on a loan or credit card; I became 90 or more days past due on my rent or mortgage; One or more of debts was sent to a collection agency; I received a utility shutoff notice; I received an auto repossession notice; I received an eviction or foreclosure notice; None of the above happened; NS; PNA

26. Looking forward to the next 12 months, what are the chances your household will experience a large and unexpected expense?

    0% chance; 10%; 20%; . . . ; 90%; 100% chance; I'm not sure

End of survey. All respondents, including those screened out at Q1, see the following message and are then redirected to an external URL. Thank you for taking our survey. Your response is very important to us.

B Data Appendix

Survey Design and Measurement Respondents were instructed not to consider anticipated or planned expenses, such as rent, utility bills, or routine car maintenance. In a separate question, we asked respondents to report, retrospectively, how likely they had thought their most recent expense shock was before it occurred. We use this measure to test the robustness of our findings against a subsample of more credibly "unexpected" expense shocks.

To reduce measurement error, we elicit income and shock size in two ways: respondents select a categorical range and separately report a dollar amount. When the reported amount falls outside the selected range, we replace it with the range midpoint.[26] For the open-ended top income and shock size ranges, we cap values at $25,000 and $20,000, respectively.

Sample Construction We received 4,576 survey responses. Of these, we excluded 9 duplicate responses and 1 response missing a respondent identifier. We further excluded respondents who did not report whether they had experienced an expense shock (N=225), reported a shock but did not report the size of the shock or the month in which it occurred (N=204), and experienced a shock but did not report household income at the time of the shock (N=227). Finally, we restrict our sample to the set of respondents who were observed transacting on food and non-durables in every month over the period August 2024–June 2026, which yields 3,712 households.

Consumption Crosswalk Numerator identifies products at the item-brand level (e.g., Skippy Natural Peanut Butter) and groups these items into a hierarchy of nested bins: sub-categories (Peanut Butter), categories (Nut Butters), departments (Condiments), and sectors (Grocery).

We build our crosswalk at the department level for two reasons. First, sub-categories and categories are sparsely populated. Second, departments resemble COICOP classes in that they aggregate items by common function and intended use; each department therefore maps onto a single COICOP class and, in most cases, a single durability designation. By contrast, sectors span multiple COICOP classes and are therefore too broad for our purposes.

[26] This imputation applies to 688 households for income and 248 treated households for shock size.

C Additional Tables

Table C.1: Shocks by Recall Horizon

	(1) Overall	(2) 2025 Q1	(3) 2025 Q2	(4) 2025 Q3	(5) 2025 Q4	(6) 2026 Q1	(7) p-value
Panel A: Shock size							
Shock amount ($)	5,561	5,197	5,626	5,203	6,455	5,324	0.303
	(10,143)	(9,320)	(10,249)	(9,188)	(11,499)	(10,340)	
Shock / monthly income	1.35	1.29	1.50	1.33	1.53	1.13	0.124
	(2.53)	(2.41)	(2.73)	(2.35)	(2.67)	(2.53)	
Panel B: Shock type							
Home	0.388	0.381	0.390	0.419	0.353	0.398	0.280
Vehicle	0.246	0.237	0.230	0.242	0.274	0.242	0.544
Medical	0.216	0.232	0.202	0.193	0.222	0.227	0.505
Other	0.151	0.151	0.179	0.146	0.151	0.132	0.442
N (respondents)	2,424	544	392	508	518	462	
Panel C: Share of shock paid within 3 months							
None	0.070	0.058	0.074	0.085	0.072	0.063	0.476
Some	0.200	0.208	0.208	0.252	0.175	0.153	0.002
Most	0.040	0.039	0.049	0.050	0.039	0.026	0.378
All	0.690	0.695	0.669	0.613	0.714	0.757	0.000
N (respondents)	2,403	538	390	504	514	457	
Observations	2,424	544	392	508	518	462	

Notes: Columns group shocked respondents in the balanced sample by the calendar quarter of their most recent large, unexpected expense. Panel A reports means with standard deviations in parentheses, over respondents with the variable non-missing; both are winsorised at the 1st and 99th percentiles, and the shock is expressed relative to net monthly household income in the second row. Panels B and C report shares of respondents answering each question. The last column reports the p-value from Welch's one-way test (Panel A) or a chi-squared test (Panels B and C) that the mean or share is equal across the quarters. Panel B: Home is home and appliance repairs, utility and rent increases, and moving; Medical is medical, dental and veterinary. Panel C: Some and Most pool follow-up answers of about 0–50% and about 75–100%.

Table C.2: Expense Shocks and Platform Use

	(1) Pooled	(2) Pre only Jan–Mar 2026	(3) Post only Mar–Jul 2025	(4) Whole Aug–Dec 2025
Any transaction	0.000	0.003	-0.001	-0.000
	(0.001)	(0.003)	(0.001)	(0.002)
	[0.997]	[0.994]	[0.999]	[0.998]
Any food	-0.000	-0.000	-0.002	-0.001
	(0.001)	(0.003)	(0.002)	(0.003)
	[0.995]	[0.994]	[0.997]	[0.996]
Any non-durable	-0.000	0.002	-0.000	-0.002
	(0.001)	(0.003)	(0.001)	(0.002)
	[0.997]	[0.994]	[0.999]	[0.998]
Transactions	-0.262	-0.209	-0.829	0.004
	(0.311)	(0.671)	(0.669)	(0.535)
	[50.853]	[49.715]	[49.564]	[50.412]
N treated	2,558	488	695	958

Notes: Callaway and Sant'Anna overall ATTs over income-sample households, each column's treated households against all 1,352 never-treated households, with one month of anticipation and a universal base period ($\tau = -2$). The overall ATT averages the group-time effects over $\tau = 0$–3. Numerator's March 2026 panel requires minimum platform use from March 2025 to March 2026. Pooled: every treated household, including those shocked in January–February 2025, who enter no other column. Pre only: shocked in January–March 2026, so the pre-shock months fall inside that window and the post-shock months run past it. Post only: shocked in March–July 2025, so the post-shock months fall inside it and the pre-shock months start before it. Whole: shocked in August–December 2025, so the whole window $\tau = -5$–3 falls inside it. Outcomes are monthly and unconditional, with a month without a recorded transaction counted as 0: any transaction, any food or non-durable spending, and the number of transactions. Standard errors in parentheses, clustered by household (multiplier bootstrap); treated mean at $\tau = -2$ in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Shock Types

	(1)	(2)	(3)	(4)	(5)
Panel A: By income quartile					
	Overall	*Q1*	*Q2*	*Q3*	*Q4*
Home	0.388	0.403	0.385	0.362	0.400
Vehicle	0.246	0.211	0.258	0.272	0.245
Medical	0.216	0.184	0.222	0.246	0.214
Other	0.151	0.202	0.135	0.120	0.141
N (respondents)	2,424	645	689	552	538
Panel B: By cash buffer					
	Overall	*< 1 month*	*1-2 months*	*3-5 months*	*6+ months*
Home	0.387	0.367	0.367	0.415	0.408
Vehicle	0.246	0.287	0.241	0.225	0.238
Medical	0.218	0.213	0.238	0.212	0.193
Other	0.150	0.133	0.154	0.148	0.161
N (respondents)	2,258	460	810	547	441

Notes: Shocked households in the balanced sample. Cells are shares of respondents answering the question. Income quartiles are of net monthly income at the shock; cash buffer is the months of expenses checking and savings would cover. Overall is over each panel's respondents. Home is home and appliance repairs, utility and rent increases, and moving; Medical is medical, dental and veterinary.

Table C.4: Shock Size

	(1) Mean	(2) p10	(3) p25	(4) p50	(5) p75	(6) p90	(7) N
Overall	1.35	0.10	0.20	0.46	1.21	3.33	2,424
Panel A: By income quartile							
Q1	1.65	0.13	0.26	0.60	1.43	3.84	645
Q2	1.51	0.12	0.22	0.50	1.50	4.17	689
Q3	1.24	0.10	0.18	0.40	1.07	3.33	552
Q4	0.92	0.07	0.14	0.33	1.05	2.29	538
Panel B: By shock type							
Home	1.09	0.09	0.17	0.40	1.14	2.65	940
Vehicle	1.79	0.13	0.24	0.50	1.67	6.09	596
Medical	1.21	0.11	0.20	0.44	1.00	2.37	523
Other	1.54	0.10	0.20	0.58	1.37	3.73	365

Notes: Shocked households in the balanced sample. Shock size is the expense divided by net monthly household income at the shock, winsorised at the 1st and 99th percentiles. Income quartiles are of net monthly income at the shock. Panel B: Home is home and appliance repairs, utility and rent increases, and moving; Medical is medical, dental and veterinary.

Table C.5: Shock Payment Methods

	(1)	(2)	(3)	(4)	(5)
Panel A: By income quartile					
	Overall	*Q1*	*Q2*	*Q3*	*Q4*
Cash or its equivalent	0.714	0.652	0.709	0.713	0.794
Credit card paid over time	0.174	0.189	0.190	0.181	0.128
Bank loan or line of credit	0.062	0.057	0.054	0.072	0.069
Payday or auto-title loan	0.017	0.035	0.014	0.017	0.002
Retirement account	0.083	0.106	0.078	0.070	0.075
Cut other spending	0.087	0.095	0.110	0.074	0.059
Other	0.145	0.220	0.156	0.101	0.091
N (respondents)	2,241	577	643	515	506
Panel B: By cash buffer					
	Overall	*< 1 month*	*1-2 months*	*3-5 months*	*6+ months*
Cash or its equivalent	0.716	0.537	0.702	0.754	0.866
Credit card paid over time	0.173	0.244	0.191	0.162	0.085
Bank loan or line of credit	0.063	0.072	0.061	0.067	0.054
Payday or auto-title loan	0.019	0.025	0.017	0.015	0.019
Retirement account	0.083	0.077	0.099	0.092	0.046
Cut other spending	0.085	0.109	0.091	0.088	0.049
Other	0.146	0.281	0.132	0.138	0.051
N (respondents)	2,082	402	749	520	411

Notes: Shocked households in the balanced sample. Cells are shares of respondents answering the question. Income quartiles are of net monthly income at the shock; cash buffer is the months of expenses checking and savings would cover. Overall is over each panel's respondents. Select all that apply, so shares sum to more than one. Cash or its equivalent is checking and savings accounts or a credit card paid in full at the end of the month, as in the Federal Reserve's SHED; bank loan or line of credit includes home equity lines of credit; Other pools borrowing from family and friends, buy now, pay later and payment plans, working more, missed payments, selling belongings, insurance and other.

Table C.6: Consumption Response to Expense Shocks: Units

	(1) Food	(2) Non-Durables
ATT	-0.023**	-0.013
	(0.010)	(0.009)
Mean-Implied Elasticity	-0.02	-0.01
Median-Implied Elasticity	-0.05	-0.03
N treated	2,424	
N never-treated	1,288	
Average shock as a share of income, %	135.4	
Median shock as a share of income, %	45.7	

Notes: The first row reports average treatment effects on the treated, standard errors in parentheses, for log units purchased on food (column 1) and non-durables (column 2). The estimation sample is restricted to households with observed food and non-durable expenditures in every month from August 2024 to June 2026. The ATT is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The elasticity rows report the elasticities implied by the ATT: the average treatment effect, in percent, divided by the average and median shock, respectively, as a share of net monthly take-home household income at the time of the shock, reported in the final two rows.

Table C.7: Robustness of Consumption Responses: Expenditure

	(1) Overall	(2) (0, 0.5]	(3) (0.5, 1]	(4) > 1
Panel A: Food				
(1) Main	-0.021**	-0.004	-0.081***	-0.012
	(0.010)	(0.013)	(0.022)	(0.018)
(2) TWFE	-0.012	-0.001	-0.057***	-0.004
	(0.008)	(0.011)	(0.018)	(0.014)
(3) Unpredictable shocks	-0.029**	0.004	-0.126***	-0.030
	(0.013)	(0.018)	(0.029)	(0.022)
(4) Excl. imputed	-0.019*	-0.000	-0.089***	-0.007
	(0.011)	(0.015)	(0.025)	(0.020)
(5) Norm. to $\tau = -3$	-0.006	0.005	-0.053**	0.004
	(0.010)	(0.014)	(0.024)	(0.018)
(6) Norm. to $\tau = -1$	0.004	0.019	-0.045**	0.008
	(0.010)	(0.013)	(0.021)	(0.020)
(7) Single shock	-0.010	0.003	-0.066*	0.004
	(0.017)	(0.023)	(0.037)	(0.032)
(8) Shocked Jun.–Dec. 2025	-0.028*	-0.015	-0.104***	-0.001
	(0.015)	(0.020)	(0.030)	(0.025)
Panel B: Non-Durables				
(1) Main	-0.012	-0.004	-0.077***	0.015
	(0.009)	(0.012)	(0.018)	(0.016)
(2) TWFE	-0.002	0.002	-0.046***	0.018
	(0.007)	(0.010)	(0.015)	(0.013)
(3) Unpredictable shocks	-0.022*	-0.000	-0.115***	-0.002
	(0.011)	(0.016)	(0.024)	(0.022)
(4) Excl. imputed	-0.007	0.003	-0.086***	0.027
	(0.010)	(0.014)	(0.022)	(0.019)
(5) Norm. to $\tau = -3$	0.009	0.019	-0.053**	0.030*
	(0.009)	(0.013)	(0.021)	(0.015)
(6) Norm. to $\tau = -1$	-0.002	0.010	-0.047**	0.006
	(0.008)	(0.012)	(0.019)	(0.016)
(7) Single shock	-0.002	0.012	-0.088***	0.026
	(0.016)	(0.022)	(0.032)	(0.029)
(8) Shocked Jun.–Dec. 2025	-0.020	-0.008	-0.093***	0.007
	(0.014)	(0.017)	(0.028)	(0.024)
Share of treated, % (N = 2,424)	100.0	53.1	18.6	28.2

Notes: ATT estimates under various specifications. Column (1) uses the full balanced sample. Columns (2)–(4) group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock. Each column compares shocked households in the group of interest to all never-treated households. Row (1) is the main specification. The ATT effect is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. Row (2) is two-way fixed effects: the coefficient on a post-shock indicator in a regression with household and month fixed effects, dropping $\tau = -1$, with standard errors clustered by household. Rows (3)–(8) modify the main specification. Row (3) restricts shocked households to those who reported thinking the shock they experienced had at most a 25% chance of occurring before it occurred. Row (4) excludes shocked households whose reported shock size or income was bin-imputed. Row (5) normalizes treatment effects to 0 at $\tau = -3$. Row (6) normalizes treatment effects to 0 at $\tau = -1$. Row (7) restricts shocked households to those who reported exactly one large, unexpected expense since the beginning of 2025. Row (8) restricts shocked households to those shocked between June and December 2025 and the sample to January 2025 through March 2026, the month of the survey, so every shocked household's five pre-shock and three post-shock months are observed and never-treated households are observed only up to the survey. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Dynamic Effects by Shock Size: Expenditure

	(1) $\tau=-5$	(2) $\tau=-4$	(3) $\tau=-3$	(4) $\tau=-1$	(5) $\tau=0$	(6) $\tau=1$	(7) $\tau=2$	(8) $\tau=3$
Panel A: Food								
Overall	-0.008	-0.009	-0.015	-0.024*	-0.034*†	-0.012	-0.016	-0.020
	(0.012)	(0.012)	(0.011)	(0.011)	(0.012)	(0.013)	(0.013)	(0.013)
(0, 0.5]	-0.007	0.006	-0.009	-0.023	-0.005	0.000	-0.002	-0.008
	(0.018)	(0.016)	(0.014)	(0.015)	(0.015)	(0.017)	(0.017)	(0.018)
(0.5, 1]	-0.023	-0.047*	-0.028	-0.036	-0.114*†	-0.073*	-0.071*	-0.065*
	(0.025)	(0.024)	(0.024)	(0.025)	(0.029)	(0.028)	(0.028)	(0.026)
> 1	-0.000	-0.012	-0.017	-0.020	-0.035	0.005	-0.006	-0.012
	(0.022)	(0.021)	(0.019)	(0.019)	(0.022)	(0.025)	(0.024)	(0.023)
Panel B: Non-Durables								
Overall	-0.010	-0.009	-0.021*	-0.011	-0.016	-0.004	-0.013	-0.016
	(0.011)	(0.009)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)
(0, 0.5]	-0.002	0.000	-0.024	-0.014	0.002	-0.006	-0.011	-0.003
	(0.016)	(0.014)	(0.013)	(0.014)	(0.014)	(0.015)	(0.014)	(0.016)
(0.5, 1]	-0.043*	-0.056*	-0.024	-0.030	-0.087*†	-0.069*†	-0.073*†	-0.078*†
	(0.021)	(0.022)	(0.020)	(0.022)	(0.024)	(0.025)	(0.023)	(0.023)
> 1	-0.002	0.006	-0.015	0.009	-0.004	0.041	0.022	-0.000
	(0.020)	(0.018)	(0.018)	(0.018)	(0.020)	(0.022)	(0.021)	(0.019)

Notes: The first row in each panel uses the full balanced sample (N treated = 2,424, N never-treated = 1,288). The remaining rows group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock. Each row compares shocked households in the group of interest to all never-treated households. Estimates are the cohort-size-weighted average of $ATT(g,t)$ at each event time τ, normalized to 0 at $\tau=-2$, which is omitted. Standard errors in parentheses. * marks estimates significant at the 5% level pointwise. † marks estimates for which the 95% uniform confidence band, simultaneous over $\tau=-5$ to 3 within each row with a sup-t critical value from the multiplier bootstrap, excludes zero.

Table C.9: Consumption Responses by Shock Size and Income: Expenditure

	(1) Overall	(2) Q1 (Lowest)	(3) Q2	(4) Q3	(5) Q4 (Highest)
Panel A: Food					
Overall	-0.020**	-0.051**	-0.041**	-0.015	0.034
	(0.010)	(0.020)	(0.019)	(0.021)	(0.021)
(0, 0.5]	-0.003	-0.078***	0.004	0.012	0.034
	(0.013)	(0.027)	(0.027)	(0.026)	(0.028)
(0.5, 1]	-0.081***	-0.092**	-0.105***	-0.097**	-0.003
	(0.022)	(0.047)	(0.035)	(0.045)	(0.043)
> 1	-0.012	0.018	-0.081***	-0.019	0.056
	(0.017)	(0.032)	(0.031)	(0.046)	(0.039)
Panel B: Non-Durables					
Overall	-0.013	-0.033*	-0.036**	0.004	0.017
	(0.009)	(0.018)	(0.017)	(0.020)	(0.020)
(0, 0.5]	-0.005	-0.059**	-0.010	0.023	0.014
	(0.012)	(0.024)	(0.025)	(0.023)	(0.025)
(0.5, 1]	-0.077***	-0.082**	-0.091***	-0.085**	-0.042
	(0.019)	(0.039)	(0.031)	(0.041)	(0.039)
> 1	0.015	0.041	-0.050*	0.020	0.060*
	(0.017)	(0.031)	(0.029)	(0.045)	(0.034)
Share of treated, % (N = 2,424)	100.0	26.6	28.4	22.8	22.2
Share of never-treated, % (N = 1,080)	100.0	22.0	26.9	21.2	29.8

Notes: Column (1) uses the full balanced sample. Columns (2)–(5) group households by quartile of net monthly take-home household income, measured at the time of the shock for shocked households and in October 2025 otherwise. Each quartile's shocked households are compared with never-treated households in the same quartile. Rows group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock, so each cell restricts shocked households on both dimensions. The outcome is log expenditure. The ATT is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Consumption Response by Shock Size and Household Characteristics: Expenditure

	(1)	(2)	(3)	(4)	(5)
Panel A: Food					
Overall	-0.021**	-0.020*	-0.021**	-0.017	-0.018*
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
(0, 0.5]	-0.004	-0.006	-0.008	-0.004	-0.006
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
(0.5, 1]	-0.081***	-0.075***	-0.076***	-0.071***	-0.071***
	(0.022)	(0.023)	(0.023)	(0.023)	(0.023)
> 1	-0.012	-0.009	-0.008	-0.007	-0.007
	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)
Panel B: Non-Durables					
Overall	-0.012	-0.014	-0.016*	-0.014	-0.015
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
(0, 0.5]	-0.004	-0.008	-0.010	-0.007	-0.009
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
(0.5, 1]	-0.077***	-0.076***	-0.077***	-0.075***	-0.075***
	(0.018)	(0.020)	(0.020)	(0.019)	(0.020)
> 1	0.015	0.014	0.013	0.013	0.013
	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)
Restricted sample		X	X	X	X
Included covariates					
Income quartile			X		X
Cash buffer quartile				X	X

Notes: Rows group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock. Each row compares shocked households in the group of interest to all never-treated households. Column (1) uses the full estimation sample without covariates (N treated = 2424, N never-treated = 1288). Columns (2)–(5) restrict the sample to households that report both income and cash buffer (N treated = 2258, N never-treated = 997). Column (2) estimates the main specification on the restricted sample. Column (3) controls for income quartile. Column (4) controls for cash buffer quartile. Column (5) controls for both variables. The ATT effect is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. Multiplier-bootstrap standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Counts are distinct households.

Table C.11: Consumption Responses by Shock Size and Shock Type: Expenditure

	(1) Overall	(2) Home	(3) Vehicle	(4) Medical	(5) Other
Panel A: Food					
Overall	-0.021**	-0.019	-0.026	-0.026	-0.010
	(0.010)	(0.016)	(0.019)	(0.021)	(0.023)
(0, 0.5]	-0.004	-0.018	-0.021	0.018	0.034
	(0.013)	(0.021)	(0.024)	(0.029)	(0.035)
(0.5, 1]	-0.081***	-0.055	-0.080*	-0.118***	-0.076*
	(0.022)	(0.038)	(0.048)	(0.044)	(0.039)
> 1	-0.012	0.003	-0.007	-0.035	-0.034
	(0.018)	(0.028)	(0.031)	(0.045)	(0.045)
Panel B: Non-Durables					
Overall	-0.012	-0.007	-0.006	-0.027	-0.016
	(0.009)	(0.015)	(0.016)	(0.018)	(0.021)
(0, 0.5]	-0.004	-0.003	-0.002	-0.008	-0.007
	(0.012)	(0.020)	(0.020)	(0.026)	(0.029)
(0.5, 1]	-0.077***	-0.053	-0.113***	-0.093**	-0.056
	(0.018)	(0.033)	(0.039)	(0.037)	(0.040)
> 1	0.015	0.013	0.039	-0.006	-0.002
	(0.016)	(0.028)	(0.032)	(0.039)	(0.039)
Share of treated, % (N = 2,424)	100.0	38.8	24.6	21.6	15.1

Notes: Column (1) uses the full balanced sample. Columns (2)–(5) group shocked households by the type of the most recent large, unexpected expense: home (home or appliance repair, a utility or rent increase, and moving or relocation costs), vehicle (repair or replacement), medical (medical, dental and veterinary expenses), and a residual other group (computer or phone repair, childcare, legal expenses, taxes and fines, gifts or loans to family and friends, and funeral costs). Respondents who selected "other" are classified from their free-text answer where it matches one of the listed types and stay in the other group where it does not. Each type's shocked households are compared with all 1,288 never-treated households. Rows group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock, so each cell restricts shocked households on both dimensions. The outcome is log expenditure. The ATT is the cohort-size-weighted average of $ATT(g,t)$ over event times 0–3, normalized to 0 at $\tau = -2$. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.12: Testing for Non-Monotonicity: Expenditure

	(1) (0, 0.5]	(2) (0.5, 1]	(3) > 1
Panel A: Food			
ATT	-0.004	-0.081***	-0.012
	(0.013)	(0.022)	(0.018)
Difference from previous bin	–	-0.077***	0.069**
		(0.026)	(0.028)
Non-monotonicity p-value		0.016	
Panel B: Non-Durables			
ATT	-0.004	-0.077***	0.015
	(0.012)	(0.018)	(0.016)
Difference from previous bin	–	-0.073***	0.092***
		(0.022)	(0.025)
Non-monotonicity p-value		0.001	

Notes: Average treatment effects on the treated for log expenditures on food (Panel A) and non-durables (Panel B). Columns group shocked households by the reported expense shock as a share of net monthly take-home household income at the time of the shock. Each ATT compares shocked households in the column's group to the full never-treated group. Estimates are Callaway–Sant'Anna, normalized to 0 at $\tau = -2$, with multiplier-bootstrap standard errors in parentheses. Difference from previous bin is the column's ATT minus the ATT in the column to its left; its standard error is analytic, from both estimates' unit-level influence functions, which accounts for the never-treated households they share. The non-monotonicity p-value tests whether the effect rises then falls, or falls then rises, across the three groups: it is the larger of the two differences' p-values when the differences have opposite signs, and 1 otherwise, so it rejects at a given level only when both differences are significant at that level and of opposite sign. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.13: Financial Distress by Shock Size

	(1) Overall	(2) Q1 (Lowest)	(3) Q2	(4) Q3	(5) Q4 (Highest)
Panel A: Any financial distress					
Never-treated mean	8.5	24.2	8.5	5.5	3.2
Overall	14.9***	24.0***	11.2***	4.0	4.2
	(1.4)	(3.9)	(2.7)	(2.3)	(1.8)
(0, 0.5]	17.9***	30.1***	16.5***	7.4	6.2
	(1.9)	(4.7)	(3.7)	(3.1)	(2.5)
(0.5, 1]	18.0***	31.0***	12.4	-1.7	3.3
	(2.9)	(6.1)	(5.3)	(3.0)	(3.8)
> 1	7.6***	10.4	1.7	1.0	0.5
	(2.0)	(5.1)	(3.4)	(3.2)	(2.4)
Panel B: 90+ days past due on loan/credit card					
Never-treated mean	2.7	8.5	2.6	1.8	0.6
Overall	7.0***	13.9***	4.0	1.7	1.4
	(0.9)	(2.8)	(1.6)	(1.4)	(0.9)
(0, 0.5]	8.1***	18.1***	4.8	2.1	2.3
	(1.3)	(3.8)	(2.2)	(1.8)	(1.4)
(0.5, 1]	10.3***	20.2***	7.9	0.1	-0.6
	(2.2)	(5.2)	(3.9)	(2.1)	(0.5)
> 1	3.1	3.8	0.2	2.0	0.6
	(1.3)	(3.5)	(1.9)	(2.4)	(1.3)
Panel C: 90+ days past due on rent/mortgage					
Never-treated mean	1.3	3.3	2.2	0.9	0.0
Overall	5.3***	12.7***	2.3	0.8	0.7
	(0.8)	(2.2)	(1.4)	(1.0)	(0.5)
(0, 0.5]	6.5***	17.4***	3.5	1.0	1.2
	(1.1)	(3.2)	(2.0)	(1.3)	(0.8)
(0.5, 1]	4.2*	10.5*	0.8	-0.9	NA
	(1.5)	(3.9)	(2.3)	(0.6)	
> 1	3.7**	7.5	1.5	1.7	NA
	(1.2)	(3.0)	(2.0)	(1.9)	
Share of treated, % (N = 1,337)	100.0	30.3	26.3	21.4	22.1

Notes: Each panel is one outcome; Any financial distress means at least one of the outcomes in Panels B and C. The first row of each panel is the never-treated mean, in percent. The Overall row compares all treated with never-treated respondents; each row below restricts the treated to one shock-size bin (shock as a share of net monthly take-home income). Columns, estimates and outcome windows are as in Table 6. Stars use Holm–Bonferroni-adjusted p-values within each panel: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Additional Figures

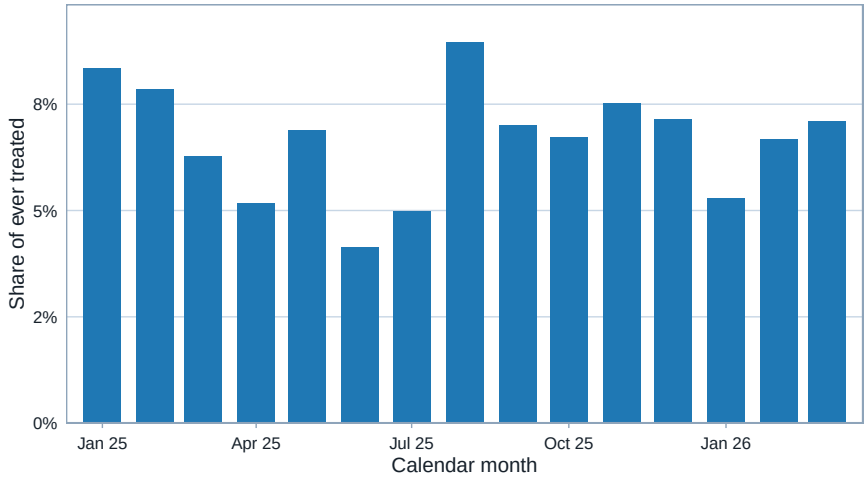


Figure D.1: Distribution of Shock Timing

Notes: This figure plots the distribution of treated households in our sample by the month of their most recent expense shock.



Figure D.2: Seasonality of Consumption

Notes: The figure plots monthly food and non-durable expenditure for never-treated households in the balanced sample from August 2024 to June 2026. For each category, we take the cross-household mean of log monthly expenditure in each calendar month, subtract its average over the 23-month window, and exponentiate. Each point is therefore the percentage deviation of that month's geometric mean expenditure from the window average.

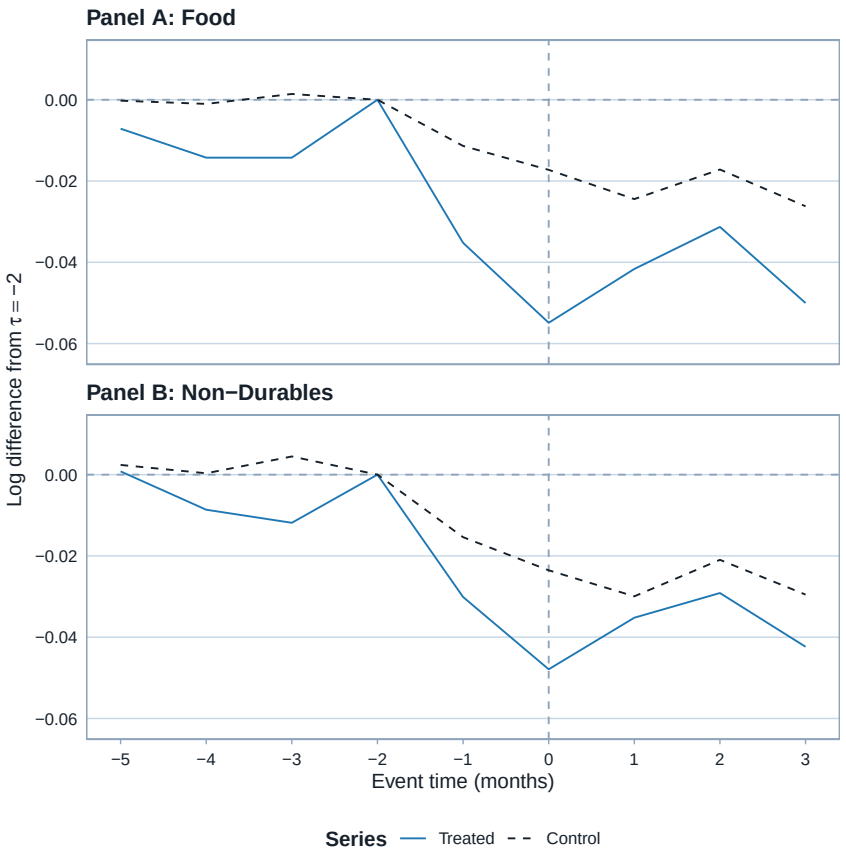


Figure D.3: Average Consumption: Units

Notes: Panel A plots average log units purchased of food and Panel B plots average log units purchased for non-durables for treated and never-treated households by event time τ (months relative to the shock), with each series normalized to zero at $\tau = -2$. The never-treated series aligns control households to each treatment cohort's calendar months, so that a given τ refers to the same calendar month for treated and control households, and averages these cohort-specific series weighted by the distribution of treatment timing.

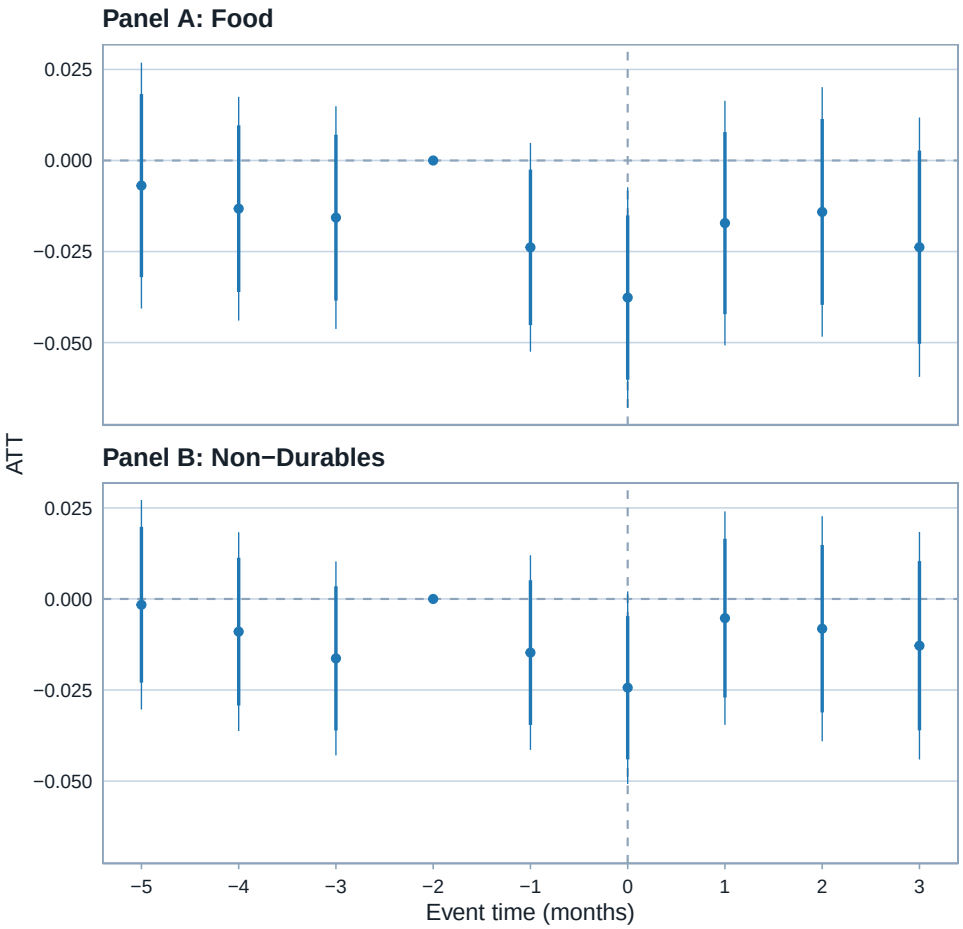


Figure D.4: Event-Study Estimates: Units

Notes: Panel A plots event-study estimates for the average effect of expense shocks on log units purchased for food, and Panel B plots event-study estimates for non-durables. Thick bars reflect pointwise 95% confidence intervals, while thinner bars show 95% uniform confidence bands to account for multiple hypothesis testing over the studied event-time window.

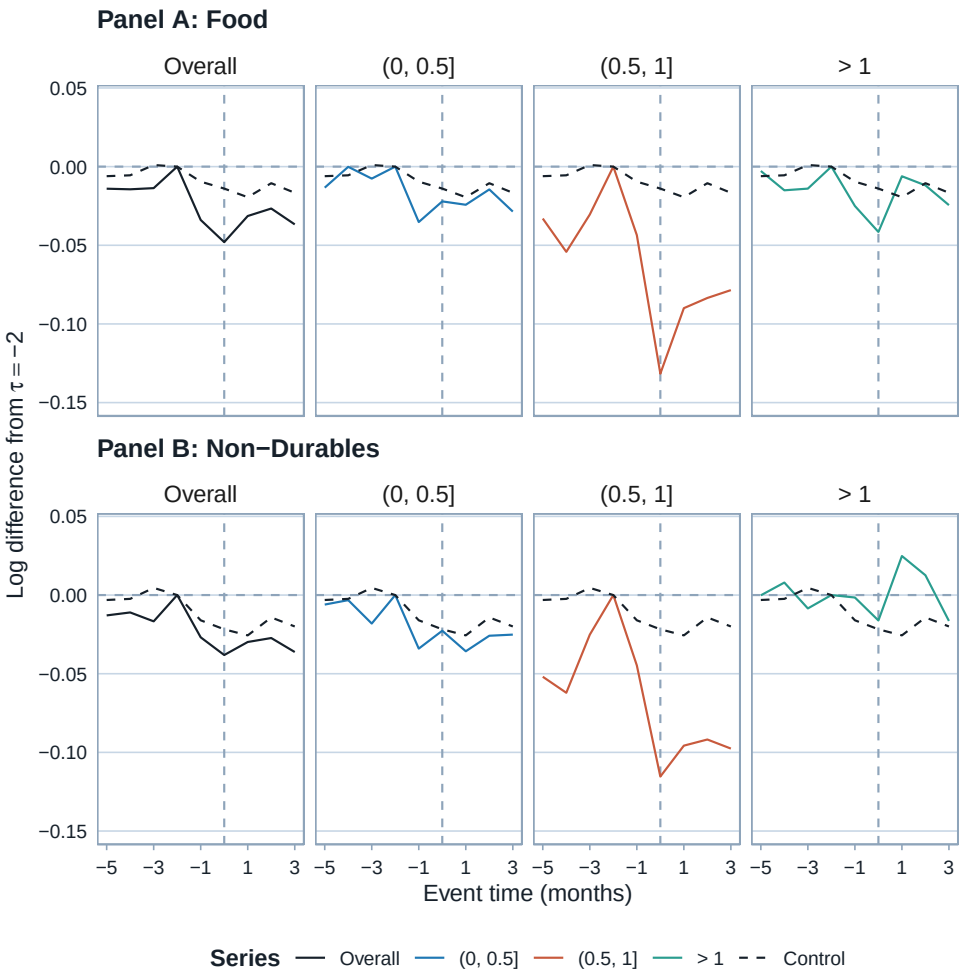


Figure D.5: Average Consumption by Shock Size: Expenditures

Notes: Panel A plots average log expenditures on food and Panel B plots average log expenditures on non-durables for treated and never-treated households event time τ (months relative to the shock) for all households and by shock size. Each series is normalized to zero at $\tau = -2$. The never-treated series aligns control households to each treatment cohort's calendar months, so that a given τ refers to the same calendar month for treated and control households, and averages these cohort-specific series weighted by the distribution of treatment timing.

E Continuous DID

E.1 Effects across the distribution of shock sizes

Do the effects of an expense shock depend on its size? To answer this question, we estimate the effects of expense shocks over the entire support of the normalized shock distribution by leveraging the Callaway, Goodman-Bacon, and Sant'Anna (2025*b*) framework for DID with a continuous treatment and staggered timing.

As in the binary treatment case, we define cohorts G_i, assigning never-treated households to $G_i = \infty$. We define $D_i \geq 0$ as the size of household i's shock as a share of net monthly household income, set to 0 for never-treated households.

Our parameter of interest is

$$ATT(g, t, d \mid g, d) = \mathbb{E}\left[Y_{i,t}(g, d) - Y_{i,t}(\infty) \mid G_i = g,\, D_i = d\right], \qquad t \geq g \tag{6}$$

the average effect in month t of a shock of size d experienced in month g across all treated households in cohort g.

Under parallel trends alone, we cannot causally interpret differences in $ATT(g, t, d \mid g, d)$ across doses as dose-driven changes in treatment effects because this might confound the causal effect of a larger shock with systematic differences between households that experience larger and smaller shocks. To allow for such a dose-response interpretation, we assume strong parallel trends following Callaway, Goodman-Bacon, and Sant'Anna (2025*b*): within each cohort g, households that receive dose d experience, on average, the same outcomes under dose d as all other treated households in cohort g would have experienced under that dose. Under this assumption, $ATT(g, t, d \mid g, d)$ equals the average effect of dose d across the whole cohort

$$ATT(g, t, d \mid g, d) = ATT(g, t, d) \equiv \mathbb{E}\left[Y_{i,t}(g, d) - Y_{i,t}(\infty) \mid G_i = g,\, D_i > 0\right] \tag{7}$$

Estimation and Aggregation Callaway, Goodman-Bacon, and Sant'Anna (2025*b*) propose a data-driven nonparametric estimator of the dose-response that chooses its flexibility from the data, and recommend it when the number of treated units is large. However, because the number of treated households in each cohort-month may be sparse for some specifications, we impose the parametric assumption

$$ATT(g, t, d) = \psi(d)'\beta_{g,t}, \qquad d > 0 \tag{8}$$

where $\psi(d)$ is a quadratic B-spline basis with interior knots at quartiles of D_i among treated

households, and $\beta_{g,t}$ varies freely across cohorts and months.

We estimate $ATT(g,t,d)$ through the specification

$$Y_{i,s} = \alpha_{g,t} + C_i^g \, \psi(D_i)'\gamma_{g,t} + \lambda_{g,t}\mathbf{1}\{s=t\} + (C_i^g \cdot \mathbf{1}\{s=t\}) \, \psi(D_i)'\beta_{g,t} + \varepsilon_{i,s} \tag{9}$$

estimated on the sample of households with $G_i \in \{g, \infty\}$ in periods $s \in \{g-2, t\}$, where $C_i^g = \mathbf{1}\{G_i = g\}$, D_i is the size of household i's shock (set to zero for never-treated households), and $\psi(\cdot)$ is a vector of basis functions that includes a constant.

Following Callaway, Goodman-Bacon, and Sant'Anna (2025*b*), we recover the average treatment effect on the treated as a function of normalized shock size by taking cohort-size-weighted averages of group average effects over the same post-treatment window as the binary estimates:

$$ATT(d) = \sum_{g \in \bar{\mathcal{G}}} P(G_i = g \mid G_i \leq \mathcal{T}) \, ATT(g,d), \qquad ATT(g,d) = \frac{1}{4}\sum_{t=g}^{g+3} ATT(g,t,d) \tag{10}$$

We implement estimation using the 'contdid' R package (Callaway, Goodman-Bacon, and Sant'Anna, 2025*a*), making two key changes to the base functions so that our dose-response functions are consistent with our binary ATT estimates: we set the base period to $\tau = -2$ rather than the default $\tau = -1$, and we restrict group-time-dose aggregation to the periods $\tau \in [0, 3]$. We report pointwise 95% intervals using bootstrap standard errors, and uniform 95% bands over the grid of dose values used in estimation.