Expense shocks matter*

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Abstract

Macroeconomic and household finance research often carefully accounts for income shocks but completely neglects expense shocks. We use a survey linked to credit bureau records to estimate expense shocks' frequency, size, and financial implications. Expense shocks are more common and larger than income shocks. Very large expense shocks are several times more common than very large income drops. Researchers can use our results and publicly available data to account for expense shocks. When we do so in an otherwise standard model, we find: (1) many costly decisions to increase liquidity such as asset liquidations or loan defaults are driven not by strategic considerations but by expense shocks, (2) smoother consumption implies less insurance, not more, (3) expense shocks explain most precautionary wealth, and (4) expense shocks mostly determine the marginal propensity to consume. Hence, expense shocks matter, and they matter more than income shocks in many important contexts.

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1 Introduction

> [Ms.Audet's] free fall into unsustainable debt began last December when her car made a horrible, sputtering sound, and died... medical bills in the thousands arrived for her Crohn's disease. She missed two rent payments. And then the landlord raised her rent $248 a month.
>
> —Rukmini Callimachi, "I Live in My Car", *New York Times*, October 17, 2023.

The liquidity shocks households experience have major implications for many topics in economics, including financial distress and loan default, precautionary saving, and households' marginal propensity to consume. Because liquidity shocks are so important, studying the size and implications of income shocks has formed a core research agenda in economics for decades. Yet, as in Ms. Audet's case described above, households also face unexpected and critical spending needs or "expense shocks." Foundational early theory (Friedman, 1957; Lucas, 1976; Bewley, 1977) and copious empirical evidence suggest that expense shocks could be important, yet modern models typically neglect them. There are several potential reasons for this neglect. Expense shocks are hard to quantify. Intuition that income shocks matter more appears widespread. Previous work on expense shocks usually focuses on specific subcategories—especially medical expenses—and so understates the importance of expense shocks overall. And it is not clear whether and how models should incorporate expense shocks because so few do.

In this paper, we use thousands of survey responses linked to credit bureau records to measure the frequency, size, persistence, and financial implications of many different kinds of expense shocks, including unexpected medical, auto, home, legal, and childcare expenses. These unique data allow us to provide the first empirical estimates of the size and frequency of all expense shocks combined. We find that many individual expense shock categories—including but not limited to medical expense shocks—are important, and together expense shocks are more common and larger than income shocks. Hence, most economic research underestimates the magnitude of liquidity shocks by at least half. Especially large expense shocks costing more than 80 percent of income are far more common than similarly-sized income drops. We provide many details about expense shocks that should allow researchers to fully account for them, and most of the data behind our results are now publicly available.

To explore the theoretical implications of our results, we use our new estimates to discipline an otherwise standard model with endogenous expense shocks (De Nardi, French, and Jones, 2010). Many decisions economists study, such as loan default or asset liquidation, involve paying a cost to obtain liquidity. In our model, expense shocks contribute to most of these costly tradeoffs and primarily drive almost 40 percent of them. Therefore, research on any such tradeoffs—ranging from bankruptcy and mortgage default to home sales and early 401(k) withdrawals—should account for expense shocks. In the model, expense shocks drive fewer of these tradeoffs than income shocks do, even though they are larger and more common, because expense shocks are temporary. But because expense shocks are temporary, agents are more willing to use liquidity to smooth them. Partly as a result, we find that expense shocks are substantially more important than income shocks for other critical topics including consumption smoothing, precautionary wealth, and consumers' marginal propensity to consume.

Our main dataset, the "Making Ends Meet" ("MEM") survey, is explicitly designed to study the causes and consequences of consumer financial distress.[1] Importantly for this paper, the MEM survey asked both whether the household experienced a variety of "significant unexpected expenses" in the past year and the amount of each expense. While it is impossible to distinguish between expected and unexpected expenses in typical data, our survey wording allows us to measure only unexpected expenses. For example, buying a new car could be a planned upgrade or an unexpected shock following an accident, and we study only the latter. Validating this survey measure, we show that (1) our estimates of unexpected expenses are just a fraction of total spending on comparable categories in the Consumer Expenditure survey and (2) very few people anticipated these expenses beforehand. Similar questions on income drops allow us to compare expense shocks with income shocks, which have been widely studied and so provide a useful benchmark.

Unexpected expenses are about twice as common as income drops: 68 percent of households experience a significant expense shock they can put a cost on in a year, while only 30 percent experience a significant income drop they can put a cost on. When they do occur, the average share of income lost to unexpected expenses (16 percent) is similar to the average share lost to income drops (18 percent). As a result, in our MEM data the average household spends about twice as much on expense shocks (10.3 percent of income, after dropping outliers) than it loses to income shocks (5.6 percent of income). Our data come from a period when the labor market was tight and so income drops may have been unusually rare, but even measuring income shocks using Panel Survey of Income Dynamics (PSID) data spanning multiple business cycles, we still find that expense shocks are on average larger than income shocks.

Especially large expense shocks costing 80 percent or more of income are five times more likely than similarly-sized income drops. Expense shocks are often extreme because multiple expense shocks often occur in the same year. Hence, while typical research neglecting expense shocks understates liquidity shocks by about half on average, it understates the probability of particularly large shocks even more. This neglect could be concerning because very large shocks are especially important for household welfare and behavior (Carroll, 1992).

To make it easy for future research to account for expense shocks, Section 4 studies them

[1] See https://www.consumerfinance.gov/data-research/making-ends-meet-survey-data/public-data/.

in detail. We first show that, when they occur, expense shocks are approximately lognormal. Researchers willing to assume that expense shocks are exogenous can therefore account for them with a simple process with three parameters: the probability of an expense shock, and the mean and variance of their log size.[2] Lower-income households spend a larger share of their income on expense shocks, which may help explain why lower-income consumers are more precautionary (Fulford, 2020). Expense and income shocks are slightly correlated, which is partly explained by specific shocks that affect both income and expenses such as illness or childcare. Expense shocks are much less persistent than income shocks. We show that consumers with limited liquidity are much more likely to report dealing with unexpected expenses by not paying for them, which suggests that the expense shock spending we observe is partly determined by available liquidity.

To study the implications of our results and directly address this endogeneity, we extend a standard consumption-savings model with liquidity constraints and medical spending shocks (De Nardi, French, and Jones, 2010) to account for all expense shocks. In the model, the marginal utility of a composite "expense shock good" varies stochastically. These exogenous expense preference shocks are calibrated so that, in the model, endogenous spending on expense shocks matches our data. Income shocks are calibrated in a standard way to match PSID data, allowing for persistent and left-skewed income shocks (Guvenen, McKay, and Ryan, 2023). Agents have rational expectations and so plan for unexpected expenses.

We use the model to explore the implications of expense shocks for several major topics in economics. Expense shocks increase the marginal value of liquidity, just like income shocks do. But one important difference is that expense shocks are larger, which increases their importance. Another important difference is that expense shocks are less persistent than income shocks; hence (conditional on size) they have milder implications for consumer financial distress, but greater implications for many spending and savings decisions.

First, we consider agents' willingness to pay a utility cost in exchange for immediate extra liquidity. Many distinct literatures consider specific versions of this topic, including default on rent (Abramson, 2024), mortgages (Foote and Willen, 2018; Ganong and Noel, 2023; Low, 2026) or payday loans (Saldain, 2025; Huang, 2023), bankruptcy (Exler and Tertilt, 2020), home sales (Han and Strange, 2015), life insurance lapsation (Koijen, Lee, and van Nieuwerburgh, 2024), retirement account withdrawals (Goodman, 2025), and small business investment and child education (Agte et al., 2024). One question common to these different literatures is the extent to which these costly tradeoffs are driven by "strategic" considerations—offsetting benefits such as discharged debt or higher-interest investment opportunities—versus liquidity shocks.

[2]Because expense shocks costing more than 100 percent of income are common, it may be challenging for models with exogenous expense shocks to fully account for them; later we illustrate one way to endogenize them using our model.

In our model, expense shocks contribute to most of these costly tradeoffs, so standard models that study these tradeoffs are missing an important force that drives them. What are the implications? If we calibrate a version of the model without expense shocks and force it to generate the same tradeoff rate, it compensates for the missing expense shocks by underestimating the costs (net of any strategic benefits) of the tradeoff by more than half. Hence models without expense shocks are likely to understate the financial distress and exaggerate the strategic motives driving these tradeoffs by a significant margin. This result generalizes similar findings from the mortgage default (Ganong and Noel, 2023; Low, 2026) and bankruptcy (Livshits, MacGee, and Tertilt, 2007) literatures to a wide range of other contexts in which households pay costs to obtain liquidity.

Expense shocks are even more important for precautionary wealth. Our model predicts that roughly 70 percent of precautionary wealth is driven by expense shocks, in part because they are transitory and precautionary wealth is especially useful for managing transitory shocks (Deaton, 1991; Carroll and Samwick, 1997). While consistent with empirical evidence (Fulford, 2015; Guiso and Jappelli, 2025), this theoretical result is at odds with the common practice of neglecting expense shocks in models of precautionary wealth.

Similarly, our model predicts that expense shocks are more important than income shocks for consumption smoothing. Agents accumulate precautionary wealth to smooth the marginal utility of spending over time (Bewley, 1977). Most models predict this means smoothing spending over time, because they assume the utility function is time-invariant. Our model follows Bewley (1977) in allowing for time-varying utility, and so agents spend some liquidity on raising consumption in periods they value it most. Indeed, in our model this force dominates; if we exogenously increase liquidity in our model, agents spend more of it dealing with expense shocks than income shocks, and so more liquidity in fact leads to more volatile consumption. While at odds with standard views on consumption smoothing, this finding is consistent with survey (Miranda-Pinto et al., 2025) and administrative (Bergholt, Furlanetto, and Mori, 2025) data showing that consumption is volatile but disconnected from income.

Next, we use our model to study the marginal propensity to consume (MPC) out of additional liquidity, which is a central focus of modern macroeconomics (Kaplan and Violante, 2018; Kaplan, Moll, and Violante, 2018; Kaplan and Violante, 2022; Crawley and Theloudis, 2024; Auclert, 2025; Auclert, Rognlie, and Straub, 2025). One major puzzle relative to standard theory is the surprisingly weak empirical relationship between liquidity and MPCs (Auclert, 2025). In our model, poor agents have surprisingly low MPCs because, in the face of expense shock risk, they are especially eager to avoid the borrowing constraint (Miranda-Pinto et al., 2025). Meanwhile, wealthy agents have surprisingly high MPCs because agents must be impatient to explain why they accumulate so little wealth in the face of expense shock risk, and impatient agents have high MPCs (Kaplan and Violante, 2022). Hence, in our model the relationship between liquidity and MPCs is much weaker than in standard models that neglect expense shocks, and so it is more in line with the data. Moreover, because expense shocks are larger, more common, and more temporary than income shocks, they generate much more additional variation in MPCs than income shocks do. These findings raise questions about why macroeconomic models focused on the MPC account for income shocks but neglect expense shocks.

We then briefly consider the implications of our results for other puzzles in macroeconomics. If we suddenly and unexpectedly double the probability of entering a very low-income state, accounting for expense shocks reduces the drop in total spending by 26 percent; if instead we unexpectedly double the real interest rate, expense shocks reduce the drop in spending by 60 percent. These findings suggest that accounting for expense shocks may help address known shortcomings of current macroeconomic models, such as the excessively large spending responses they predict after changes in income risk (Auclert, Rognlie, and Straub, 2025) or the real interest rate (del Negro, Giannoni, and Patterson, 2023; Crawley and Gamber, 2026).

We conclude by studying the empirical relationship between expense shocks and financial outcomes. When directly asked, households are much more likely to report that expense shocks caused financial difficulty than income shocks. But more conservative estimates from regressions suggest that (conditional on size) income shocks are more strongly correlated with financial distress than expense shocks are, which is consistent with our model. However, expense shocks predict significant increases in credit card debt while income shocks do not. This result is consistent with our model's prediction that agents should be more willing to use liquidity for expense shocks than income shocks, which in turn supports our model's conclusion that expense shocks are more important than income shocks for many other topics.

Although our emphasis on expense shocks contrasts with much recent research, several foundational papers accounted for expense shocks. As he did with income, Friedman (1957) also separated consumption into temporary and permanent components, with "temporary consumption" including what we call expense shocks. So did Lucas (1976), in a seminal argument that macroeconomic models should be based on real household spending behavior. Bewley (1977) allowed for endogenous expense shocks in the form of a utility function with stochastic components,[3] in the first of a highly influential class of models that would come to be called "Bewley models" (Ljungqvist and Sargent, 2004). Our findings strongly support the effort taken by these researchers to account for expense shocks.

However, our findings raise concerns about the neglect of expense shocks in subsequent models, including the next generation of "Bewley" models (Aiyagari, 1994) and other highly influential work (Zeldes, 1989; Deaton, 1991; Carroll, 1997).[4] The literature does not clearly discuss the rea-

[3] Indeed, allowing for expense shocks was a key contribution of Bewley (1977) who notes: "most of the results of this paper are proved by Schechtman in a special case [without expense shocks]."

[4] Some models accounted for specific expense shocks, especially medical expenses or childcare (Heathcote,

sons for this shift away from the methods Friedman (1957), Lucas (1976), and Bewley (1977) used to account for expense shocks; see Ljungqvist and Sargent (2004), Heathcote, Storesletten, and Violante (2009) and Cherrier, Duarte, and Saidi (2023) for literature reviews.[5] But likely contributing factors include the theoretical and empirical difficulty of accounting for expense shocks, intuition that income shocks matter more, and the influence of other foundational papers that only considered income shocks (Leland, 1968). Whatever the reasons for it, this shift endured and the tradition of accounting only for income shocks continues in most models in household finance (Campbell and Ramadorai, 2025) and in Heterogeneous Agent New Keynesian ("HANK") models at the frontier of macroeconomics (Kaplan and Violante, 2018; Kaplan, Moll, and Violante, 2018; Kaplan and Violante, 2022; Auclert, 2025; Auclert, Rognlie, and Straub, 2025). A major exception is Miranda-Pinto et al. (2025), who model expense shocks as stochastic consumption thresholds that yield large utility costs if violated. They discipline their model with PSID data and obtain results that complement ours.

Empirically, important work with experimental data (Sussman and Alter, 2012; Berman, Tran, and Zauberman, 2016; Peetz et al., 2016; Howard et al., 2022), survey data (Livshits, MacGee, and Tertilt, 2007; Pew Charitable Trusts, 2015; Sabat and Gallagher, 2019; Fulford and Rush, 2020; Bufe et al., 2022; Low, 2026) and administrative data (Farrell and Greig, 2017; Bergholt, Furlanetto, and Mori, 2025) establishes that a wide variety of expense shocks are important for households. Fulford (2015) and Fulford (2020) find that households do not appear to save or want to save because of income shocks, but do seem to save for expense shocks. Guiso and Jappelli (2025) elicit consumers' beliefs about the future and find that households believe themselves to be subject to large stochastic expense shocks.

Our main contribution is to provide the first quantitative estimate of all expense shocks, just as other papers (Carroll and Samwick, 1997; Crawley, Holm, and Tretvoll, 2025; Guvenen, McKay, and Ryan, 2023; Ganong et al., 2025) estimate the income shock process. Empirically, we find that expense shocks are larger and more frequent than income shocks. These results can be used in structural models to account for expense shocks. In one such model, we find that expense shocks matter in every context we consider, and they matter more than income shocks in most contexts.

2 Data

Our primary data sources are a rotating panel of surveys mailed in January 2022, January 2023, January 2024, and January 2025. Each year, versions of the survey were sent to respondents to

Storesletten, and Violante, 2009).

[5]The clearest discussion we have been able to find is in Carroll and Samwick (1997), who argue that it is safe to abstract from medical expense shocks because Hubbard, Skinner, and Zeldes (1994) find their role to be minor.

the previous wave, as well as to an entirely new sample. Across all of the surveys we have 14,202 respondents with expense shock information, but some analyses use only a subset as described in table or figure notes. These survey data are now publicly available.[6]

A key advantage of the MEM surveys is that they are sampled from and matched to the CFPB's Consumer Credit Panel (CCP). The CCP is a comprehensive 1-in-48 sample of credit records maintained by one of the three nationwide consumer reporting agencies. Because of this CCP match, we have considerable information on survey non-respondents, allowing us to adjust for survey non-response more comprehensively than is possible in most other surveys. The combined surveys are weighted to be representative of the CCP and so are representative of consumers with a credit record. Because nearly all U.S. adults have a credit record (Kambara and Luce, 2025), our data are very nearly nationally representative. The survey sampling, protocol, response rates, weighting, and results are described in detail in the documentation for the public use data.

The surveys asked: "In the past 12 months, has your household experienced a significant unexpected expense from any of the following?" and listed nine possible expense categories including a catchall "other" option. If the respondent reported experiencing a significant unexpected expense, the survey followed up with: "About how much was the cost?" Appendix Figure A-2 shows the primary questions we use as they appeared on the 2023 survey.

These survey questions are important because we derive most of our results from them. We asked respondents only about "significant" expenses to reduce cognitive burden, so it is likely our results exclude unexpected expenses that households regarded, at least individually, as insignificant. We asked households about "unexpected" expenses to correspond most closely to what economists think of as "shocks." Households plan for many significant expenses, such as durable good purchases and medical procedures, and the question wording was designed to exclude these planned expenses. Qualitative testing for this question suggested that survey respondents indeed excluded planned expenses.

Our qualitative testing suggests that people can put values on the shocks they experience. Our testing suggests that expenses are often less cognitively demanding than calculating income changes because expense shocks often have a clear cost. We ask about distinct kinds of expense shocks and have an "other" expense category to allow for types of expenses beyond those we explicitly asked about. In previous surveys, we included a write-in option and modified our categories to include common options, so our options include the most common expense shocks.

To validate our expense shock estimates, Appendix B compares them to spending in the closest comparable categories in the Consumer Expenditure (CE) survey, which measures all spending,

[6]The public use files are available at: https://www.consumerfinance.gov/data-research/making-ends-meet-survey-data/. For privacy reasons, an income drop question on "Loss of income due to a natural disaster" was suppressed in the public use file. Our working paper using the non-public data shows that 1.7 percent of people experienced such a shock.

both expected and unexpected. Our measure of unexpected spending in each category is smaller than the comparable CE estimates. Moreover, this relationship also holds for different income groups, which is a stricter test because there is substantial variation in both MEM and CE spending across income groups. These results provide validation that the MEM expense shock estimates are reasonable and that respondents exclude planned expenses.

To further validate our survey measure of expense shocks, Appendix C studies expectations about future expenses using the 2025 survey. Compared to the number of people who report significant unexpected medical, auto, or home expenses, the number who "expect" (attribute probability greater than 80 percent to) these expenses is much smaller. This finding further confirms that the unexpected expenses our survey measures are indeed unanticipated shocks.

Income shocks have been extensively studied by previous research and so are not our primary focus, but we also study them so we can directly compare the size and implications of income and expense shocks for the same people at the same time. The surveys asked: "In the past 12 months, have you or someone in your household experienced a significant drop in income from any of the following?," and provided a list of 12 possibilities shown in Appendix Figure A-2, and followed up by asking "About how much income did you lose because of this circumstance over the past 12 months?" Note that this question did not ask respondents to distinguish between expected and unexpected income drops.[7]

3 Expense shocks and income shocks

This section begins by quantifying expense shocks. We then compare expense shocks to income drops, which provide a useful benchmark because they have been widely studied by other papers. For comparability, we express both expense and income shocks as a percent of "pre-income drop income," which is reported household income plus reported income drops.

3.1 Significant unexpected expenses

Table 1 shows the frequency and size of unexpected expenses that households face. Overall, 67.7 percent of households faced at least one significant unexpected expense in the previous year that they could put a positive cost on. Individual expenses are also important: 27.7 percent faced a significant out-of-pocket medical expense, 32.6 percent faced a vehicle repair or replacement, 22.9

[7]The income shock and expense shock questions on the 2023, 2024, and 2025 surveys are structured similarly, making this comparison straightforward. The income shock questions had a somewhat different structure in 2022. Appendix A.1 discusses our approach to producing estimates from the 2022 survey, but we generally exclude the 2022 survey when estimating income shocks. A potential issue with asking about distinct income shocks is that respondents could reasonably include the same event in multiple categories. Appendix A describes how we identify and suppress potentially double-counted income drops.

Table 1: Frequency and size of significant unexpected expenses

	Experienced (%)	Mean cost (% of income)	Mean cost ($)	Cost percentile ($) 25	50	75
A major out-of-pocket medical or dental expense	27.7	8.5	4871	1000	2000	5000
	(0.51)	(0.75)	(298)			
An unplanned gift or loan to a family member or friend	11.9	6.5	5340	500	1200	4400
	(0.34)	(0.55)	(498)			
A major vehicle repair or replacement	32.6	9.5	6807	1000	2000	5000
	(0.53)	(0.35)	(249)			
A major house or appliance repair	22.9	7.2	6143	800	2000	5000
	(0.45)	(0.40)	(367)			
A computer or mobile phone repair or replacement	22.1	1.5	1013	300	700	1042
	(0.48)	(0.14)	(108)			
Legal expenses, taxes, or fines	12.8	8.2	5431	500	1900	5000
	(0.41)	(2.08)	(648)			
Increase in childcare or dependent care expenses	5.8	3.2	3375	350	1000	3000
	(0.25)	(0.24)	(398)			
Moving costs	5.2	4.4	3095	650	1500	3000
	(0.23)	(0.28)	(218)			
Some other major unexpected expense	11.1	10.8	6745	980	2000	5500
	(0.33)	(1.78)	(592)			
Total unexpected expenses	67.7	16.2	11404	1550	4500	11250
	(0.54)	(0.72)	(332)			

Notes: Standard errors in parentheses. Income is calculated based on pre-income drop income. The first column defines "experienced" as having (1) reported a positive cost for the event and (2) having non-missing income, to be consistent with later results. 73.4 percent (standard error 0.52) selected at least one expense event, including people who reported a zero or missing cost and people who did not report income. Each row is calculated only among people who experienced that type of expense and is the cost of that type, conditional on having it. The last row calculates the share who experience any expense they could put a positive cost on and the sum of all expenses among people with at least one. Source: Authors' calculations from the 2022, 2023, 2024, and 2025 Making Ends Meet surveys with a total sample of 14,202 respondents, except for moving costs which were added after 2022.

percent a house or appliance repair, and 22.1 percent a computer or mobile phone replacement or repair. We show how these shocks are related to each other in Appendix D and their relationship to all expenses in the same category in the Consumer Expenditure Survey in Appendix B.

Among the households that experienced the expense in each row, the mean share of (pre-income drop) income lost varies from 8.5 percent for an out-of-pocket medical expense and 9.5 percent for a vehicle repair or replacement to 1.5 percent for a computer or mobile phone replacement or repair whose median dollar cost is about the price of a new phone. Cost distributions are typically wide. Conditional on having an expense, the mean dollar cost is often about as large as the 75th percentile, indicating that a few households experience especially large expense shocks.

One important lesson from Table 1 is that many different kinds of expense shocks matter. Among unexpected expenses, unexpected medical expenses have received by far the most attention in previous work. But, as shown in Table 1, unexpected vehicle expenses—which have received almost no attention—are both more common and larger. Home expenses are also frequent and large, while several other smaller expense categories are still important. A major reason we obtain total expense shock estimates that are so large, even relative to previous work emphasizing the importance of expense shocks (Livshits, MacGee, and Tertilt, 2007), is that we are able to account for all of these categories. As we discuss in Subsection 3.4, households often experience more than one unexpected expense. That many households experience multiple shocks highlights the importance of accounting for multiple expense shocks simultaneously and is one reason we find such a thick tail of expense shock severity which we discuss more below.

3.2 Income drops

Table 2 shows the frequency with which households experienced different kinds of income drops and their costs. While research has sometimes assumed that the main income shock is unemployment, Table 2 shows that many different income shocks matter. Overall, 30 percent of respondents reported they experienced a significant income drop. Appendix Figure A-4 shows the distribution of distinct shocks and Appendix D studies the relationship between distinct income shocks.

3.3 The relative importance of income and expense shocks

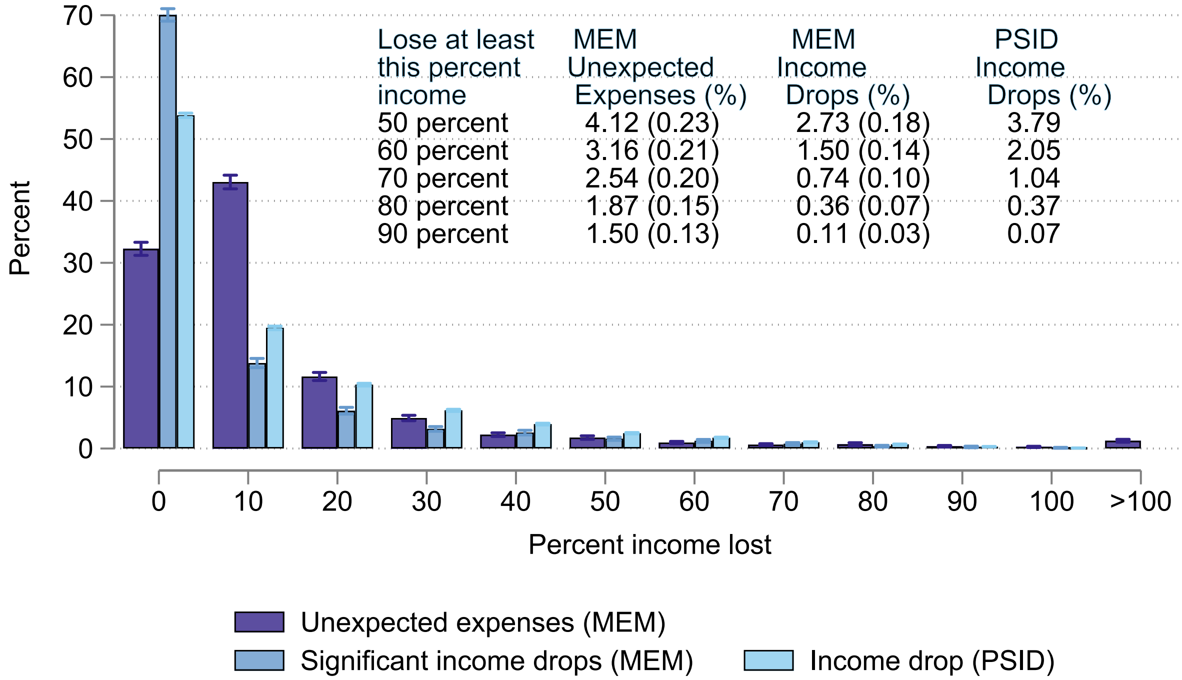
Comparing Table 1 and Table 2, households with an income drop lost on average 18.2 percent of their income to it, but only 30 percent of households experienced any income drop. Meanwhile, 68 percent of households experienced a significant unexpected expense, and among these households the mean share of income lost to all unexpected expenses was 16.2 percent. Including households who did not experience an expense or income drop as zeros, the average household lost 5.6 percent of its pre-drop income to income drops and 10.3 percent to significant unexpected expenses (after

Table 2: Frequency and size of significant income drops

	Experienced (%)	Mean cost (% of income)	Mean cost ($)	Cost percentile ($) 25	50	75
Period of unemployment or furlough	10.8 (0.35)	18.9 (0.71)	19130 (1323)	3000	8000	20000
Reduction in work hours	10.8 (0.35)	10.7 (0.48)	9186 (855)	1000	3000	10000
Reduction in wages at your job	4.1 (0.22)	11.6 (0.87)	11271 (999)	1000	4000	12000
Changed to a lower-paying job	4.7 (0.24)	14.0 (0.79)	14615 (940)	1920	8000	20000
Loss of government benefits	3.3 (0.20)	6.3 (1.09)	3636 (469)	400	1000	3600
Worked less because of illness or injury	8.1 (0.32)	11.1 (0.57)	10242 (1371)	1000	3000	10000
Worked less to care for others who were sick or injured	4.4 (0.24)	9.7 (0.85)	7620 (695)	500	2000	8000
Worked less or stopped working to take care of children	4.5 (0.24)	14.3 (1.05)	11124 (799)	1000	4500	15000
Loss of revenue from a business you own	3.0 (0.19)	16.4 (0.95)	26076 (2995)	3000	10000	25000
Other significant drop in income	3.7 (0.20)	17.0 (1.37)	16543 (1395)	2000	6000	20000
Any significant drop in income	30.0 (0.51)	18.2 (0.39)	19456 (857)	2000	7000	20000

Notes: Standard errors in parentheses. Income is calculated based on pre-income drop income. "Any significant drop in income" excludes income drops from the same event that may fall in more than category (see Appendix A) but all responses are included in specific income drops. The first column defines "experienced" as (1) having reported a positive cost for the event and (2) having non-missing income, to be consistent with later results. 36.3 percent (standard error 0.54) selected at least one event, including people who reported zero or missing cost and people had missing income. The table does not include "Loss of income due to a natural disaster" which is not reported in the public data. "Lost rental income from a property you own" was not included in 2025 so is added to "Other" for previous years. Source: Authors' calculations from the 2023, 2024, and 2025 Making Ends Meet surveys with a total sample of 12,108 respondents.

Figure 1: Income and expense shocks compared to pre-income-drop income



Notes: The share with zero expense includes respondents who reported experiencing an income or expense shock but did not report a cost (which we include as a zero) or reported a zero cost. The income for each respondent is the income before income drops. The bars show 95 percent confidence intervals and the table parentheses contain standard errors. Source: Authors' calculations from 2022, 2023, 2024, and 2025 Making Ends Meet surveys (expense shocks), the 2023, 2024, and 2025 surveys (income drops), and 1969-1997 PSID.

dropping households who reported total expense shocks greater than 10 times income). Hence, on average in our MEM data the average household lost about twice as much to expense shocks as income shocks.

During the time period our data are from the labor market was tight, so income drops may have been milder than usual. For a comparison with data covering recessions, we calculate similar income drops from the Panel Study of Income Dynamics (PSID) from 1969 to 1997 when it was an annual survey. This time period included several recessions which might produce larger income drops (Guvenen, McKay, and Ryan, 2023). 45 percent of households had an income drop in the PSID data; among households with an income drop, the average amount lost was 19.1 percent. Treating not having an income drop as zero (in analogy to our MEM data), the average household lost 8.8 percent to income drops in the PSID. Hence, even when using PSID data to measure income shocks we still find that expense shocks are larger on average.

Figure 1 provides histograms for income and expense shocks by income share. Small expense shocks—on the order of 10 percent of income or less—were substantially more common than small income drops. Even these relatively small shocks may present a significant problem for liquidity-constrained households (Mello, 2024).

Very large expense shocks were also more common than very large income shocks. The table in Figure 1 examines events costing 50 percent or more of income; 4.1 percent of households experienced unexpected expenses of this size, while a similar 3.8 and 2.7 percent of households experienced income drops of this size in the PSID and MEM, respectively. However, very large expense shocks are much more common than very large income shocks: 1.87 percent of households experienced unexpected expenses worth at least 80 percent of income, which is more than five times the share that suffered a comparable income drop in either MEM or PSID data. 1.50 percent of households experienced unexpected expenses worth at least 90 percent of income, more than 10 times the share that suffered a comparable income drop in either MEM or PSID data.

The thickness of the tail of the expense shock distribution is worth highlighting. Even though drops in income of 100 percent are very rare, their major theoretical implications led early researchers to devote considerable effort to precisely estimating their low probability (Carroll, 1992); we estimate expense shocks of this size are many times more common. These estimates are quite stable across survey years.[8] Previous researchers have noted the importance of expense shocks of this size; for example, a small chance of a very large medical expense may substantially influence consumers' retirement and savings decisions (De Nardi, French, and Jones, 2010). But we estimate that shocks of this size are especially frequent because we account for non-medical expense shocks. For example, our estimate of the probability of an expense shock costing more than 80 percent of income is over three times higher than the frequently-used estimates in Livshits, MacGee, and Tertilt (2007), who—because of the data available to them—only accounted for medical shocks when studying expense shocks of this size.

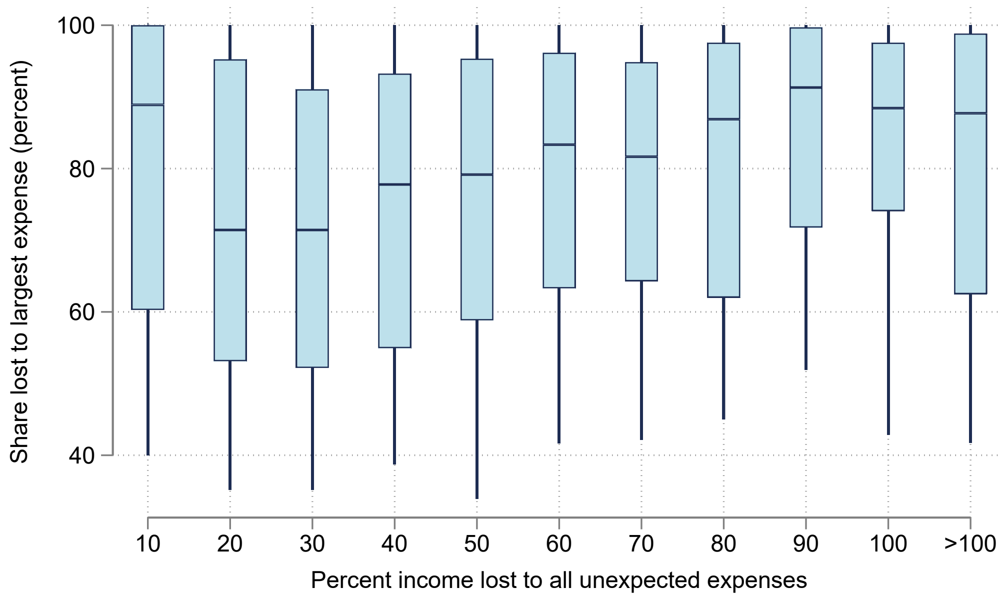
3.4 Shocks stack

One reason we find that expense shocks are so large in total is that different expense shocks often occur in the same year. Among households with unexpected expenses, over two-thirds experienced more than one (see Appendix Figure A-3).

To provide more detail on how expense shocks combine, Figure 2 provides a box-whisker plot of the size of the largest unexpected expense expressed as a percent of all unexpected expenses. For example, if a household experienced only one unexpected expense, then that expense constitutes 100 percent of all unexpected expenses for that household that year. Figure 2 shows that for many

[8] While extremes are difficult to estimate, the stability of our estimates even across surveys with different oversamples and weights suggests our estimates are precise. The percent of people with an expense shock greater than 80 percent of income is (with standard errors in parentheses): 1.5 (0.37) in sample 3 wave 1 (2022), 1.8 (0.64) in sample 3 wave 2 (2023), 1.6 (0.39) in sample 4 wave 1 (2023), 2.3 (0.43) in sample 4 wave 2 (2024), 2.4 in sample 5 wave 1 (2024), 1.6 (0.35) in sample 5 wave 2, and 1.7 (0.29) in sample 6 wave 1 (2025).

Figure 2: Distribution of the largest expense by percent income lost to all unexpected expenses



Notes: Conditional on total expense shock size (x-axis), the figure shows the distribution of the size of the largest expense expressed as a percentage of total unexpected expenses. Vertical lines show the 5th and 95th percentiles, boxes show the 25 and 75th percentiles, and horizontal lines denote the median. Sources: 2022, 2023, 2024, and 2025 Making Ends Meet surveys.

households a combination of shocks, not just the largest, were important. Across the distribution of total expense shock size (the x-axis of the figure), at the 25th percentile (the bottom of the boxes) the largest unexpected expense accounted for only about 60 percent of all expense shocks. Even at the median (the lines in the boxes' middle), the largest unexpected expense accounted for only about 80 percent of total unexpected expenses. Hence aggregating across many, sometimes small, unexpected expenses shifts mass into the right tail in Figure 1 and helps explain why our estimates for unexpected expenses are so large.

Among expense shocks, medical expense shocks have received by far the most previous attention. While medical expenses can be substantial, they constituted a minority of total expense shocks. On average, unexpected out-of-pocket medical expenses were 21 percent of all unexpected expenses for households with at least some (not necessarily medical) unexpected expenses. For households with a medical expense, the average medical expense was 50.3 percent of all unexpected expenses. Appendix Figure A-5 shows that these shares are relatively constant across the share of income lost to all unexpected expenses, and so (perhaps surprisingly) extreme shocks are not disproportionately medical expenses. These findings complement those of Miranda-Pinto et al. (2025), who find that the share of expenditures spent on health care varies little between normal and high-expenditure episodes.

Although we emphasize the importance of expense shocks, expense shocks and income shocks are complementary and not competing explanations for household financial distress. Instead, the expense shocks we document provide an additional layer of risk on top of the income shocks that have been widely documented. To illustrate, Appendix Figure A-6 shows that expense shocks push households with income shocks to have much larger total liquidity shocks than income shocks alone do.

That expense shocks combine, with each other and with income shocks, helps interpret previous empirical findings. Early surveys of defaulters (Gardner and Mills, 1989; Cutts and Merrill, 2008) and households declaring bankruptcy (Chakravarty and Rhee, 1999) typically asked respondents to identify one specific cause of their default or bankruptcy. While many respondents listed income or expense shocks, many others could not identify a specific cause, which was often taken as evidence of strategic default or bankruptcy. The MEM surveys allowed respondents to list multiple shocks, and we find many households experience shocks that together are much more severe than any specific shock they experience. This suggests that future surveys on similar topics should allow respondents to list multiple causes of financial distress. Indeed, in a survey of mortgage defaulters (Low, 2026) that allowed respondents to list multiple reasons for their default, 38–58 percent of respondents reported at least three different liquidity shocks.

4 How economic models can account for expense shocks

In this section, we provide more information on expense shocks so that future researchers can fully account for them. We expect that different models will account for expense shocks in different ways, depending on the application, so we summarize the expense shock distribution in several ways to give other researchers flexibility. For example, our estimates could be treated as the outcome of an exogenous expense shock process, or as moments to discipline the parameters of an endogenous expense shock process such as the one we introduce in Section 5.

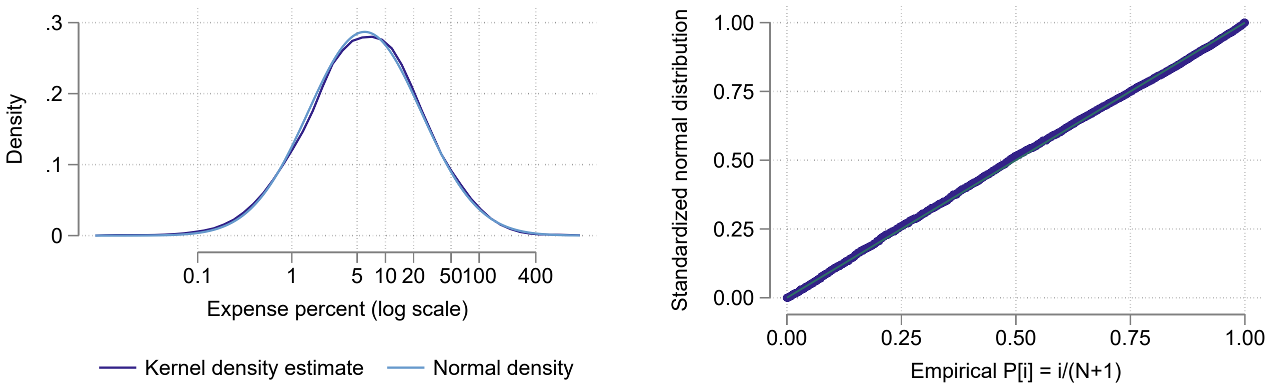
4.1 The expense shock distribution

We begin by showing that expense shocks are approximately log normal so expense shocks can be well approximated with only three parameters: the probability that an expense shock occurs, and the mean and variance of the log-normal distribution. Figure 3 compares the log fraction of income lost to expense shocks, conditional on an expense shock, to the normal distribution. Conditional on occurring, the fraction of income lost to expense shocks is very nearly lognormally distributed. Indeed, in the P-P plot it is difficult to see differences from the 45-degree line.

Table 3 shows the distribution of expense shocks overall and for different demographic groups while Appendix Table A-4 provides averages for several specific expense shocks across demo-

Figure 3: Unexpected expenses compared to normal distribution

(A) Kernel density plot (B) P-P plot



Notes: Kernel density is weighted using survey weights. The P-P plot is not weighted. Source: Authors' calculations from 2022, 2023, 2024, and 2025 Making Ends Meet surveys.

graphic groups. The first column of Table 3 shows the average amount of income lost to unexpected expenses. To be conservative, the table treats respondents who report experiencing a shock but do not report its cost or report a zero cost as experiencing no shock. On average, expense shocks decrease with income. The second column shows that income drops also decrease on average with income. The last three columns show the three parameters summarizing the distribution for each group with a lognormal approximation: the fraction of households with a non-zero unexpected expense and, conditional on a positive expense shock, the mean log expense share of income lost and the variance of the log.

Lower income households are much more exposed to expense shocks when they have them in Table 3. Both the mean and the variance of the log fraction lost to unexpected expenses fall with income. These two shifts mean that low-income households face much higher probability of large shocks, as shown in Appendix Figure A-7.

People aged 60 and over are less likely to experience an expense shock. But the expense shocks they do experience are larger and have greater variance, so their shock distribution has much more weight in the right tail. Meanwhile, the elderly lose less than half as much of their income to income shocks than younger people. Liquidity risk for older Americans is thus mostly from expense shocks. While a rich literature emphasizes the importance of medical expenses for older Americans (De Nardi, French, and Jones, 2010; Blundell et al., 2024), Appendix Table A-4 shows that, even for older Americans, medical expenses are just a fraction of total expense shocks.

Table 3 also shows that expense shocks do not vary much across racial and ethnic groups. However, many of the attributes in Table 3 are correlated, so Appendix Table A-3 studies how income and other demographics relate to shocks in a series of regressions. The regressions confirm the pattern in Table 3; income is much more predictive of expense shock size than other demographics.

Table 3: Income and expense shocks across demographic groups

	Income lost to unexpected expenses (%)	Income lost to income drops (%)	Fraction with non-zero unexp. exp.	Mean ln(fraction unexp. exp.)	Variance ln(fraction unexp. exp.)
Overall	10.3	5.6	0.677	-2.81	1.97
	(0.29)	(0.15)	(0.0054)	(0.020)	(0.042)
Income					
$20,000 or less	16.2	11.1	0.590	-2.23	2.15
	(1.35)	(0.70)	(0.0156)	(0.061)	(0.134)
$20,001 to $50,000	12.5	7.0	0.684	-2.56	1.84
	(0.57)	(0.31)	(0.0098)	(0.032)	(0.074)
$50,001 to $80,000	10.0	5.3	0.727	-2.80	1.76
	(0.53)	(0.31)	(0.0112)	(0.048)	(0.097)
$80,001 to $125,000	9.0	4.0	0.724	-2.99	1.84
	(0.55)	(0.25)	(0.0109)	(0.038)	(0.073)
More than $125,001	5.7	2.5	0.681	-3.29	1.84
	(0.28)	(0.19)	(0.0135)	(0.041)	(0.076)
Education					
Less than H.S.	10.9	6.6	0.634	-2.70	1.97
	(0.73)	(0.33)	(0.0120)	(0.051)	(0.095)
High School	10.9	6.7	0.706	-2.73	1.93
	(0.59)	(0.38)	(0.0112)	(0.040)	(0.102)
Some college	11.7	5.6	0.689	-2.73	2.04
	(0.76)	(0.41)	(0.0122)	(0.049)	(0.113)
College or post-grad.	9.2	4.5	0.696	-2.93	1.95
	(0.40)	(0.21)	(0.0087)	(0.028)	(0.060)
Race and ethnicity					
Non-Hisp. white	10.5	5.0	0.681	-2.81	2.01
	(0.40)	(0.18)	(0.0072)	(0.027)	(0.055)
Black	9.6	6.9	0.661	-2.87	1.98
	(0.71)	(0.44)	(0.0140)	(0.045)	(0.100)
Hispanic	10.8	7.5	0.703	-2.72	1.85
	(0.68)	(0.47)	(0.0115)	(0.046)	(0.113)
Other	8.8	4.9	0.642	-2.88	1.89
	(0.74)	(0.48)	(0.0168)	(0.059)	(0.117)
Age					
<40	9.7	7.1	0.717	-2.90	1.93
	(0.48)	(0.31)	(0.0095)	(0.038)	(0.077)
40-60	9.8	6.8	0.726	-2.90	1.90
	(0.40)	(0.29)	(0.0077)	(0.029)	(0.065)
>=60	11.6	3.0	0.601	-2.60	2.02
	(0.61)	(0.19)	(0.0090)	(0.033)	(0.066)
Observations	14233	12222	14239	9681	9681

Notes: All results are survey weighted; standard errors in parentheses. Observations are for the Overall result. Expense shocks greater than ten times income are dropped. Non-zero unexpected expenses include only respondents who reported a positive non-missing cost for the shock. Source: 2022, 2023, 2024, and 2025 Making Ends Meet surveys for expenses; 2023, 2024, and 2025 surveys for income drops.

Table 4: Relationship between income and expense shocks

	Income lost to unexpected exp. (%)			Income lost to income drops (%)		
Income lost to income drops (%)	0.142***	0.108***	0.0668***			
	(0.0181)	(0.0184)	(0.0244)			
Income lost to unexpected expenses (%)				0.0756***	0.0564***	0.0287**
				(0.0108)	(0.0105)	(0.0121)
Constant	8.492***	11.98***	10.96***	4.906***	10.30***	7.840***
	(0.225)	(0.905)	(0.876)	(0.156)	(0.675)	(0.672)
Observations	12,104	11,963	9,880	12,104	11,963	7,578
R-squared	0.011	0.024	0.015	0.011	0.045	0.029
Controls	None	Income	Income	None	Income	Income
Sample excludes medical and childcare	No	No	Yes	No	No	Yes

Notes: The sample in column 3 excludes respondents with income drops relating to injury, caring for others who are sick, or working less to care for children. The sample in column 6 excludes respondents with unexpected medical expenses or childcare expenses. Source: Authors' calculations from the 2023, 2024, and 2025 Making Ends Meet surveys. *** p<0.01, ** p<0.05, * p<0.1

There is somewhat more variation in income drops across demographic groups.

4.2 The correlation between income shocks and expense shocks

The raw correlation between the income share lost to expenses and income drops is 0.10, including households without income or expense shocks as losing zero. Among only households with positive shocks, the correlation is 0.09.

This small correlation is partly explained by specific shocks that affect both income and expenses. Figure A-10 in Appendix D provides the correlations between specific income and expense shocks. Households experiencing an unexpected increase in childcare expenses are more likely to report working less to care for children, and vice versa. Households that experience a medical expense are slightly more likely to work less because of illness.

Table 4 examines the correlation between income and expense shocks in regression form. The dependent variable in the first three columns is the share of income lost to expense shocks (including zeros) while the dependent variable in the next three columns is the share of income lost to income drops. Without any controls, the two shocks are modestly predictive of each other. Controlling for income weakens this correlation. Excluding income shocks related to illness or childcare (in column 3), or expense shocks related to illness or childcare (in column 6), the modest correlation falls further.[9]

[9] Appendix Table A-5 shows two stage regressions of a similar form. First, we estimate a probit model for whether

4.3 The persistence of income and expense shocks

Table 5 shows regressions that examine shocks' persistence. We use the panel dimension of our survey, so our sample size is smaller. Panel (A) shows the average marginal effect from a series of probit regressions. Households that experienced an expense shock in one year are about 25 percentage points more likely to experience an expense shock the next year and about 9 percentage points more likely if they experienced an income drop in the previous year across regressions. Households that experienced an income drop were around 30 percentage points more likely to experience an income drop the next year, depending on the specification, and 12 percentage points more likely if they had an expense shock. These average marginal effects are mostly unaffected by controlling for income the year before.

Panel (B) shows expense shocks have a low persistence from year to year compared to income shocks. Including people who did not have a shock as zeros, the first column shows that a one percentage point higher expense shock in one year predicts a 0.095 percentage point larger expense shock the next year, lower than the corresponding income drop persistence in column 2. Including only people who experienced the shock in both years has a similar size prediction. In log terms, a one point larger expense shock in one year predicts a 0.19 point larger shock the next year. Column 4 shows that income drops are much more persistent, which is consistent with the large existing literature on income shocks. However, the sample size behind this result is small because income drops are rarer than expense shocks. Appendix Table A-6 shows that individual expense shocks' persistence differ. Medical expenses are the most persistent, although legal expenses and house repair are nearly as persistent, while the persistence of moving expenses is almost zero.

4.4 Expense shocks are partly endogenous

Both income and expense shocks are to some extent endogenous. Previous work using the MEM survey (Fulford and Rush, 2020) shows that households in financial difficulty often seek new income and cut back on expenses. Previous research has also shown that the endogeneity of income can have important policy implications (Low, Meghir, and Pistaferri, 2010). In this subsection we provide evidence on the endogeneity of expense shocks.

Table 6 shows how income and expense shocks vary by credit score. We measure the credit score one year before each survey to avoid a mechanical connection between income and expense shocks and credit score. While income drops fall substantially with credit score, unexpected expenses vary little with credit score.

the household experienced a shock, and then we estimate an OLS regression of the size of the shock, conditional on having one. The probit regressions suggest that households who experience income shocks are modestly more likely to also experience expense shocks even controlling for income or childcare, but the size of the shocks are uncorrelated.

Table 5: Persistence of income and expense shocks

(A) Probit dep. variable:	Had expense shock			Had income drop		
Had an expense shock year before	0.274*** (0.0201)	0.255*** (0.0209)	0.245*** (0.0213)		0.121*** (0.0183)	0.123*** (0.0187)
Had an income drop year before		0.0889*** (0.0189)	0.0945*** (0.0190)	0.341*** (0.0198)	0.309*** (0.0203)	0.299*** (0.0204)
Observations	4,307	4,183	4,121	4,241	4,200	4,133
Control for income	No	No	Yes	No	No	Yes

(B) OLS dep variable:	Share inc. lost (%) Expenses	Share inc. lost (%) Income	Share income lost (%) if had shock both years Expenses	Income	ln(fraction inc. lost unexpected expenses)	
Income lost to expense shocks year before (%)	0.0953*** (0.0219)		0.0824*** (0.0265)	0.124** (0.0523)		
Income lost to income drops year before (%)		0.262*** (0.0329)	0.129** (0.0630)	0.329*** (0.0579)		
Had an expense shock year before				-0.166 (2.851)		
Had an income drop year before			-1.038 (1.855)			
ln(fraction inc. unexp. exp. year before)					0.216*** (0.0275)	0.194*** (0.0270)
Observations	4,234	4,133	2,231	690	2,275	2,275
R-squared	0.017	0.121	0.021	0.189	0.048	0.059
Control for income	Yes	Yes	Yes	Yes	No	Yes

Notes: Probit results in Panel (A) show the average marginal effect. In Panel (B) we exclude the observations who lost more than 1000% of income to expense shocks in that year or the previous year from the share income lost columns. The first two columns include people who did not have a loss as zero loss. Columns 3-6 require people to have the shock in both years. Source: Authors' calculations from the 2022-2023, 2023-2024, and 2024-2025 Making Ends Meet survey panels. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Income and expense shocks across credit score groups

	Income lost to unexpected expenses (%)	Income lost to income drops (%)	Fraction with unexpected expense	Fraction difficulty paying bills	Fraction did not pay all expense
Credit score one year before survey					
300-540	7.8	10.8	0.748	0.713	0.503
	(0.63)	(1.18)	(0.0227)	(0.0318)	(0.0392)
540-599	11.1	8.2	0.742	0.724	0.463
	(1.51)	(0.77)	(0.0218)	(0.0285)	(0.0344)
600-659	9.9	6.8	0.724	0.632	0.397
	(0.95)	(0.71)	(0.0216)	(0.0278)	(0.0356)
660-719	11.9	4.9	0.657	0.424	0.301
	(1.81)	(0.55)	(0.0233)	(0.0282)	(0.0364)
720-779	10.3	4.0	0.715	0.295	0.203
	(0.93)	(0.37)	(0.0177)	(0.0204)	(0.0305)
780-850	9.5	3.4	0.666	0.124	0.193
	(0.71)	(0.30)	(0.0131)	(0.0103)	(0.0360)
Observations	9502	7498	9506	5067	2212

Notes: Credit score is as of the end of December, 12 months before the survey. Income drops and unexpected expenses include zeros for households without such a loss. Standard errors for means or coefficients in parentheses. All results are survey weighted. Source: 2022, 2023, and 2024 non-public Making Ends Meet surveys except for income drops which use only 2023 and 2024.

However, lower credit score consumers were much more likely to report having "difficulty paying a bill or expense." Among those with difficulty, lower credit score consumers were much more likely to say that they did not pay all of the bill or expense in response to their financial difficulty. These questions were in a different section of the survey than the expense shock questions, so we do not know whether the respondent did not pay all of a specific unexpected expense. We take these results as suggestive evidence that observed expense shock spending is limited by available liquidity. We develop this distinction between underlying exogenous spending needs and the resulting endogenous spending, which is limited by liquidity, more thoroughly in our model in Section 5.

5 A model of endogenous expense shocks

In this section, we explore the theoretical implications of our empirical results using a deliberately standard model from the medical expense shock literature (De Nardi, French, and Jones, 2010; White, 2021; Khwaja and White, 2025). Using a standard model ensures that our new results below are driven by the data we use to calibrate the model, not by any departures from standard theory. Indeed, to solve our model, we mostly use the code base from the publicly-available HARK repository, with only a few modifications.[10] Our main modeling contributions are: (1) to demonstrate how, with underlying expense preference shocks, a relatively standard model can match the expense shock distribution we document empirically and (2) to show that matching the distribution of expense shocks has major implications for many important topics.

5.1 Model specification

The core of the model is a standard infinite-horizon consumption-savings problem. Consumers discount the future exponentially at rate β and choose consumption c subject to standard transitory and persistent lognormal income shocks. They have access to one liquid, risk-free asset a that earns a constant rate of return R.

We introduce two main departures from this otherwise quite standard model. First, we allow for a small possibility of a large and persistent "crisis" income shock. Households occasionally experience worse income shocks than the standard lognormal income process will generate (Guvenen, Ozkan, and Madera, 2024), and it is important for our model to account for these severe shocks so that our comparison of expense shocks with income shocks is fair. Hence the exogenous and stochastic income vector $\vec{y_t}$ is given by:

$$\vec{y_t} = z_t + \delta_t + \epsilon_t,$$

where

$$z_t = \rho z_{t-1} + \psi_t,$$

ϵ_t is the lognormal temporary shock, ψ_t is the lognormal persistent shock with persistence ρ, and the binary Markov state δ_t is the crisis income shock.

The model's more significant departure from the standard consumption-savings framework is that agents experience stochastic utility shocks η. In response to these exogenous η shocks (such as their car breaking down), consumers choose how much to spend on unexpected expenses (whether to scrap the car, fix it, or replace it). We denote this endogenous spending on expense shocks x. Utility is additively separable in normal consumption c and shock consumption x. It is CRRA in both, with parameters γ and ν respectively. Utility is given by:

$$u(c, x) = \frac{c^{1-\gamma}}{1-\gamma} + \eta \frac{x^{1-\nu}}{1-\nu}. \tag{1}$$

[10] Readers can quickly access the base HARK code and some basic results at https://github.com/econ-ark/HARK/blob/master/examples/ConsumptionSaving/example_ConsMedModel.ipynb. The main modifications we made for our model are to allow for: (1) a large and persistent crisis income shock, and (2) slight persistence in expense preference shocks.

With this specification, consumer utility is not homothetic. If $\nu > \gamma$, consumers are more risk averse over x than c. Thus while consumers with higher income or wealth will choose to spend more on x given η, they will do so less than proportionally, and so the fraction of income spent on expense shocks will fall with income. Given our empirical finding that lower-income consumers spend proportionally more on expense shocks (see Table 3), this is an attractive feature of the model.

With utility given by Equation 1, the Bellman equation can be written:

$$V(a, \vec{y}, \eta) = \max_{c,x} u(c, x) + \beta E_{\vec{y}', \eta'} V(a', \vec{y}', \eta') \tag{2}$$

subject to:

$$a' \geq \underline{a}(\vec{y}) \tag{3}$$

$$R^{-1}a' + c + x = a + \vec{y} \tag{4}$$

where Equation 3 is the exogenous borrowing limit and Equation 4 is the budget constraint.

5.2 Calibration and model fit

We consider three versions of our model: one with income shocks but not expense shocks (INC, for "Income Shocks"), one with expense shocks but not income shocks ("EXP", for "Expense Shocks"), and our preferred specification with both ("BOTH"). Comparing INC with BOTH establishes that introducing expense shocks to a model that already has income shocks has major implications. Of course, this comes at the cost of additional complexity, so comparing INC with EXP helps to inform readers considering this tradeoff. This comparison demonstrates that in many cases it may be more important for models to account for expense shocks than income shocks.

Calibration We calibrate the income process using PSID data. In versions of the model with income shocks, we set the persistence of traditional lognormal income shocks ρ to a fairly standard value of 0.97. The Markov crisis income shock δ is characterized by three parameters: its size, and the probability of entering and exiting the state. We estimate the size and probability of exiting the state from the PSID. In the data we assume a household enters the crisis state from the non-crisis state if they experience an income change within the bottom 1 percent of all income changes. When this occurs, we estimate that log household income falls by 1.64 (or income falls by 80.5 percent) on average. We assume a household exits the crisis state if, the period after experiencing one, their log income increases by at least .5. With this definition, we estimate the annual probability of leaving the crisis income state to be 60.9 percent.

Numerically simulating the income process, we set (1) the probability of entering the crisis income state and (2) the variance of the temporary and persistent components of income shocks to match (1) the unconditional probability of an income shock large enough to be counted as a crisis shock and (2) the variance of three- and five-year changes in log income, using methodology inspired by Carroll and Samwick (1997). This yields that agents not in the crisis state have a 1.1 percent probability of entering it, and values for σ_ϵ^2 and σ_ψ^2 of 0.0397 and 0.0136 respectively.

In versions of the model with expense shocks, η follows a persistent Markov process to account for the mild persistence of expense shocks in our data. First, we assume that η_t is drawn from a mixture of three distributions: either zero, "mild", or "severe." These three states are persistent, with transition probabilities informed by our empirical results. Details are in Appendix Subsection F.1.

If an expense shock occurs, it is drawn from a discretized lognormal distribution. The shape and scale parameters of this distribution are calibrated, in part, so that the model matches the left (between 0 and 10 percent of income) and right (greater than 90 percent of income) tails (from Figure 1) of the expense shock distribution. The lognormal distribution is winsorized at the 99.5th percentile, so that especially large expense shocks beyond those we can reliably measure do not drive our results. A "mild" expense shock is drawn from the bottom 95 percent of this distribution, while a "severe" expense shock is drawn from the top 5 percent.

We set the coefficient of relative risk aversion γ on normal consumption c to a standard value of 2, the risk-free interest rate on liquid assets to 1 percent, and the borrowing constraint to 18.5 percent of permanent income following Kaplan and Violante (2022).

We calibrate consumer risk aversion over expense shocks ν to match the average expense shock size by income group in Table 3. Higher values of ν make expense shocks more of a necessity good, leading them to represent a higher fraction of income for lower-income consumers. The average value of expense shocks in Table 3 also helps to identify the shape and scale parameters of the lognormal η distribution.

In the INC version of the model, expense shocks are turned off by setting η_t to always be zero. This collapses the model to a standard consumption-savings model without expense shocks. In the EXP model, income shocks are turned off by setting the variance of temporary and persistent lognormal shocks, as well as δ_t, to 0. On its own, setting the variance of persistent income shocks to 0 would eliminate heterogeneity in persistent income z. But as just discussed ν is calibrated to match the relationship between expense shock size and income; so that we can continue to use this calibration strategy in EXP, we assign z in the simulation to have the same distribution as it does in INC, but this assignment is permanent and is not subject to shocks.

Finally, we set the discount factor β of each model to match an average ratio of liquid wealth to income of 0.6 (Kaplan and Violante, 2022). Because households hold much of their wealth in illiquid assets, one-asset models like ours typically must choose between matching consumers' liquid wealth or their total wealth including illiquid assets; see Carroll et al. (2017) and Kaplan and Violante (2022) for discussions of the advantages of each approach. We target liquid wealth because it is likely more relevant for financial distress, consumption smoothing, and other outcomes we focus on. For example, targeting liquid wealth allows the models to match average MPC levels (Kaplan and Violante, 2022), which is one important topic we study. Later, we briefly explore agents' demand for illiquid assets as a first step towards understanding models with expense shocks that could match both liquid and illiquid wealth levels.

Model fit All versions of the model fit the targeted normalized wealth level of .6 well. Normalized wealth in BOTH, INC, and EXP is 0.60, 0.60, and 0.61 respectively.

Agents in BOTH face more liquidity risk than agents in INC, so all else equal they have a stronger precautionary motive. To counteract this and achieve the same mean wealth-to-income ratio, agents in BOTH must have lower discount factors ($\beta = 0.905$) than in INC ($\beta = 0.948$). This illustrates an important point: incorporating expense shocks into calibrated quantitative models will affect other parameters which will, in turn, have other downstream effects that depend on the specific parameters that change. The discount factor in EXP is $\beta = 0.957$.

Figure 4 plots the distribution of normalized spending on expense shocks, $\frac{x}{z}$, in BOTH and EXP against the data. Both models fit the data well, including the right tail which (as we show) is important for many outcomes. The calibrated shape and scale of the winsorized lognormal η distribution in BOTH are, respectively, 4.16 and 7.76e-05. In EXP they are 4.45 and 0.000225.

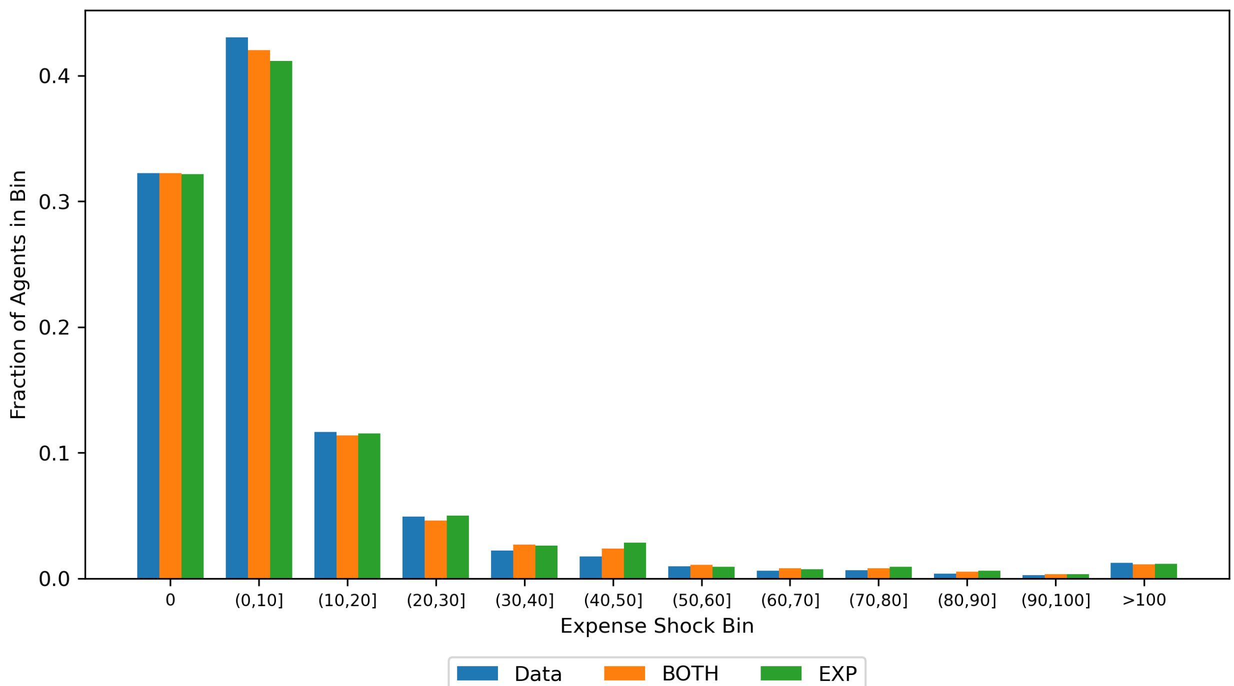
Figure 5 plots mean normalized expense shocks by income category, in BOTH, EXP, and the data. Recall that this information is used to calibrate ν, yielding values of $\nu = 3.40$ in BOTH and $\nu = 3.09$ in EXP. EXP needs a lower value of ν than BOTH because, without persistent income shocks, it generates a greater share of precautionary savings among low-income agents (see Figure A-11), which makes larger expense shocks among lower-income agents less surprising.

Finally, the model approximately replicates the persistence of expense shocks. By construction, the model matches the finding in Table 5 that an expense shock in period $t-1$ increases the probability of an expense shock in period t by about 27 percentage points. The model also closely replicates that the probability of an expense shock greater than 50 percent of income is 2.06 times as likely if there was such a large shock in the previous period; in BOTH the value is 2.07.

5.3 Model intuition: Income versus Expense Shocks

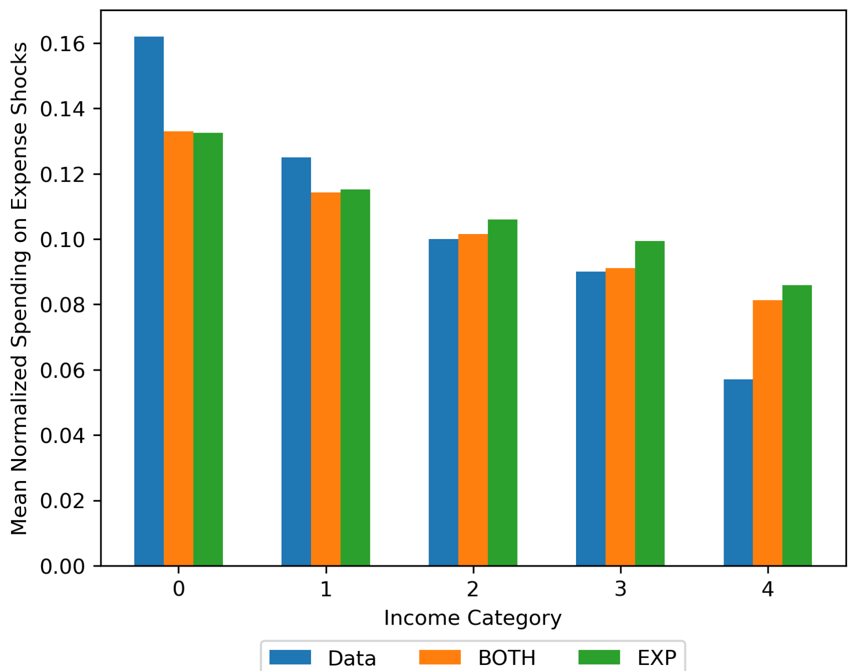
Below we contrast the models to explore the implications of expense shocks for household finance and macroeconomics. One theme is that expense shocks are like income shocks in that they gen-

Figure 4: Distribution of Expense Shocks in Models and Data



Notes: Figure shows the fraction of consumers in each expense shock bin in the data and in the two versions of the model with expense shocks. BOTH has both income and expense shocks. EXP only has expense shocks.

Figure 5: Normalized Spending on Expense Shocks in BOTH and EXP models compared to Data



Notes: MEM respondents are placed into income categories, based on whether they make less than \$20k, \$20-\$50k, \$50-\$80k, \$80k-\$125k, or more than \$125k. 12.0%, 21.3%, 18.9%, 20.0%, and 27.8% of MEM respondents are in each of these categories, respectively. In the models, we divide agents into comparable categories by placing the 12.0% of agents with the lowest permanent income in the first category, the next highest 21.3% in the second category, etc. Given these categories, the figure shows the average percent of permanent income spent on expense shocks in each of these categories. BOTH is the version of the model with both expense and income shocks. EXP only has expense shocks.

erate a high marginal value of liquidity. But income and expense shocks also differ in important respects.

One important difference is that expense shocks are much less persistent than crisis income shocks. Following standard intuition, this means that crisis income shocks have worse implications for households than comparably-sized expense shocks because they are more likely to persist. For the same reason, agents are more willing to use liquidity to smooth over expense shocks.

Another difference is that without expense shocks, households value spending only on c, so as spending grows the marginal value of spending falls relatively quickly at a rate determined by γ. But households experiencing expense shocks split their spending on both c and x, at a rate also determined by ν and η. Thus the marginal value of spending falls more slowly with spending for agents experiencing an expense shock, even when they are not borrowing constrained.

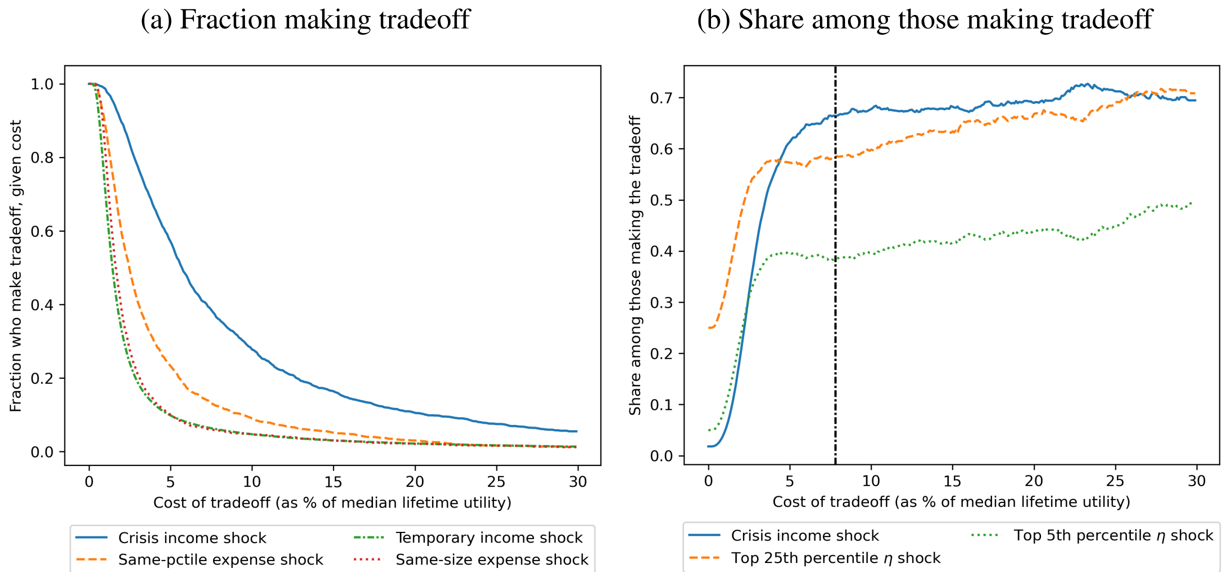
Both characteristics of expense shocks weaken the relationship between financial distress and spending and saving decisions relative to standard models. As detailed below, this helps to explain our potentially surprising result that even though income shocks generate more severe financial distress, expense shocks are more important for many other outcomes of interest. One reason it is important to understand this result is that intuition that income shocks are the worst shocks facing households appears to have been one reason consumption-savings models shifted towards neglecting expense shocks (see the literature review in Section 1.)

5.4 Model results: Shocks and financially costly tradeoffs

In this subsection, we consider the problem of an agent considering whether to pay a utility cost in exchange for immediate liquidity. This kind of tradeoff is ubiquitous in economics. Within the context of rent default (Abramson, 2024), loan default (Indarte, 2022), or bankruptcy (Exler and Tertilt, 2020), it can be viewed as risking default costs, such as foreclosure or homelessness, by not making scheduled loan or bill payments. Within the context of high-return but illiquid assets such as housing (Han and Strange, 2015), life insurance (Koijen, Lee, and van Nieuwerburgh, 2024), retirement accounts (Goodman, 2025), small businesses or child education (Agte et al., 2024), it can be viewed as the decision to withdraw from (or not to invest in) the illiquid asset. As discussed in these literatures, people might "strategically" choose to make these kinds of costly decisions because of offsetting benefits (such as debt discharge or higher-interest investment opportunities) or be forced to because of liquidity shocks.

Here we consider the implications of neglecting expense shocks when studying this kind of tradeoff. Figure 6a plots the fraction of agents in our BOTH model experiencing various shocks (y-axis) that are willing to pay a utility cost (x-axis) in order to increase their assets a by 10 percent of their persistent income z. Consistent with our empirical results shown later in Section 6, a much

Figure 6: Shocks and costly tradeoffs



Notes: In an experiment we allow agents to increase their assets a by 10 percent of their persistent income z, at the cost expressed as a percent of their value function given on the x-axis. The figure on the left shows the fraction of agents experiencing different shocks that make the tradeoff. The figure on the right shows, among those making the tradeoff, the share of agents experiencing different shocks. The temporary income shock is the same size as the crisis income shock. Unlike the other shocks in this figure, it does not actually occur in the model, so we calculate the fraction of all agents who would accept the tradeoff if they were subject to such a shock. The crisis income shock leads to a roughly 80% drop in income, and 1.8% of agents are in the crisis income state. To be comparable, the "same-size" expense shocks are defined as those more than 70% and less than 90% of persistent income. The same-percentile expense (or η) shock includes agents with unexpected expenses x (or preference shock η) in the top 1.8% of all values. The vertical line in the figure on the right denotes a utility cost of the tradeoff of 7.8%, which is the value the model needs to generate a tradeoff rate of 1%.

larger share of agents experiencing the crisis income shock (the solid blue line) accept the costly tradeoffs than agents experiencing expense shocks of the same size (dotted red line). To understand if this is because expense shocks are endogenous or because they are temporary, we consider the decisions of agents facing *temporary* income shocks of the same size as the crisis income shock.[11] The dashed-dotted green line shows that these large but temporary income shocks are no more predictive of costly tradeoffs than expense shocks. Hence, conditional on size, income shocks are much more predictive of these costly tradeoffs mainly because they are persistent.

However, there is a countervailing force. As in our data (see Section 3), expense shocks in the model are on average larger and have a much thicker right tail than income shocks even though we allow for the crisis income shock. When we consider expense shocks that are comparable to the crisis income shock in percentile rank rather than in size (dashed orange line), their ability to

[11] Unlike the other shocks studied in this figure, these very large temporary income shocks do not actually occur in the model.

predict tradeoffs increases.

Still, expense shocks drive many, and contribute to most, of these costly tradeoffs. Figure 6b plots, among agents making the tradeoff, the share experiencing the crisis income shock (solid blue line), an exogenous expense preference shock in the top 25th percentile (dashed orange line), and an exogenous expense preference shock in the top 5th percentile (dotted green line). Taking a standard approach of calibrating default costs (net of long-run default benefits) to match default rates, we find that to achieve a tradeoff rate of 1 percent (which is comparable to the serious delinquency rate on many credit products), the net utility cost of the tradeoff must be 7.8 percent of lifetime utility (shown with the dashed-dotted vertical black line in Figure 6b). The figure shows that, at this cost, roughly two-thirds of tradeoffs are driven at least in part by the crisis income shock, while 38 percent are driven in part by expense preference shocks in the top 5th percentile. Hence, most tradeoffs are driven primarily by crisis income shocks, but a large minority are driven by large expense shocks. Smaller expense preference shocks contribute to most tradeoffs, including many driven primarily by the crisis income shock, as shown by the dashed orange line.

What are the implications of expense shocks for literatures that study costly tradeoffs to increase liquidity? For empirical work, our findings show that researchers cannot assume they observe all liquidity shocks if they only observe income shocks. In the well-studied context of mortgage default, researchers used to assume that the roughly one-third of defaults not driven by income shocks are "strategic", that is, motivated by long-run benefits in the form of discharged negative home equity (Bradley, Cutts, and Liu, 2015; Gerardi et al., 2018; Foote and Willen, 2018). However, Ganong and Noel (2023) and Low (2026) show that almost all of the defaults that are not driven by income shocks are instead driven by expense shocks and are not strategic. Moreover, as in our model, even many defaults that are partly caused by income shocks are also partly caused by expense shocks (Low, 2026), so empirical researchers studying the motives for mortgage default must account for both income and expense shocks. Our results generalize this finding from the mortgage default literature to other literatures studying similar tradeoffs to increase liquidity.

To study the implications for structural models, suppose that the true data generating process is given by the model with both income and expense shocks, BOTH. Recall that the calibrated liquidity tradeoff cost is 7.8 percent of lifetime utility in BOTH. Now suppose that we use a structural model that only accounts for income shocks (INC) and calibrate the tradeoff cost in INC to match the same 1 percent tradeoff rate.[12] INC predicts that the net tradeoff cost is only 2.9 percent of lifetime utility, less than half the "true" value, because INC neglects expense shocks and so generates fewer liquidity-constrained agents willing to pay a utility cost in exchange for liquidity. One corol-

[12]Calibrating structural models missing some or all expense shocks to match tradeoff rates is standard in the bankruptcy literature (Exler and Tertilt, 2020) and very common in the mortgage default literature; see Low (2023) for a discussion. It also common in other literatures such as rent default (Abramson, 2024), pay-as-you-go financing (Gertler et al., 2025), and credit scores (Chatterjee et al., 2023).

lary is that INC significantly underpredicts the degree of financial distress among agents making the tradeoff. It predicts that the liquidity benefit of the tradeoff is worth on average 8.1 percent of median lifetime utility among those making the tradeoff, while the "true" value in BOTH is 22.3 percent of median lifetime utility. Hence, even though only a little over a third of agents willing to pay a tradeoff cost of this size are experiencing severe expense shocks, accounting for expense shocks more than doubles our estimates of the utility cost and liquidity benefits of the tradeoff. One major reason why is that less-severe expense shocks also contribute to many tradeoffs. In total, 74 percent (58 percent) of agents accepting the tradeoff are experiencing expense preference shocks above the 50th (75th) percentile. Subsection F.2 provides more information on the distribution of the liquidity benefits of the tradeoff in the three versions of the model. The combination of income and expense shocks generates many more cases of severe financial distress than either kind of shock alone, which is reminiscent of the result in Miranda-Pinto et al. (2025) that expense shocks significantly increase the welfare cost of income shocks.

Our results demonstrate that, by neglecting expense shocks, standard models on these kinds of topics are likely to substantially understate both the costs of these tradeoffs and the financial distress among households that make them, which has important policy implications. For example, Livshits, MacGee, and Tertilt (2007) show that as expense shocks get larger, so does the value of policies that lower bankruptcy costs, and so bankruptcy models must account for expense shocks. Our expense shock estimates are significantly larger than the baseline estimates in Livshits, MacGee, and Tertilt (2007) that have been widely used (Exler and Tertilt, 2020), so applying our estimates in their model implies that bankruptcy should be substantially less costly. More importantly, our results generalize this finding that it is important for models to account for expense shocks to other literatures studying tradeoffs to increase liquidity.

5.5 Model results: Spending and saving decisions

The previous section showed that expense shocks contribute substantially to agents' willingness to make financially costly tradeoffs, but income shocks contribute more. In this section we show that despite this result, expense shocks are more important than income shocks for many other outcomes of interest.

Precautionary Wealth To a substantial degree, the models of precautionary wealth that are now ubiquitous were first developed to provide a microfoundation for the hypothesis in Friedman (1957) that consumers should smooth over temporary shocks (Cherrier, Duarte, and Saidi, 2023). While agents in Friedman (1957) are subject to both income and expense shocks, in most precautionary wealth models agents are only subject to income risk (Cherrier, Duarte, and Saidi, 2023; Auclert, Rognlie, and Straub, 2025). Here, informed by our data, we consider the relative role of income and expense shocks in generating precautionary wealth.

To understand different shocks' importance for precautionary wealth, we take our BOTH model as calibrated and selectively turn off different shocks. This holds agents' preferences fixed, but allows them to re-optimize in the face of different amounts of risk. We simulate four versions of BOTH with: (1) both income and expense shocks, as usual; (2) expense shocks turned off; (3) income shocks turned off; (4) income and expense shocks turned off.[13] Precautionary wealth is the wealth in each version minus the wealth in version (4), in which agents borrow up to their borrowing constraint because they face no risk and so have no reason to save. We then compare the precautionary wealth in version (1) to the wealth in versions (2) and (3).

With this exercise, we find that most precautionary wealth is from expense shocks. Eliminating expense shocks but keeping income shocks reduces precautionary wealth by 70 percent. Eliminating income shocks but keeping expense shocks reduces precautionary wealth by 57 percent. One major reason these numbers add up to well over 100 percent is because expense and income shocks can occur at the same time, which widens the tail of total liquidity risk facing households and so encourages precautionary savings. This is reminiscent of theoretical findings in Miranda-Pinto et al. (2025) and our empirical results in Section 3.4. Though it contrasts with a modeling literature that assumes precautionary wealth is driven only by income risk, our findings are also consistent with evidence that households hold substantial precautionary wealth for expense shocks (Fulford, 2015; Guiso and Jappelli, 2025).

To further explore these results, we turn off specific income and expense shocks and examine how much precautionary wealth falls. For income shocks, we turn off (1) only transitory income shocks by setting their variance to 0, (2) only persistent income shocks by setting their variance to 0, and (3) only the crisis income shock by setting its probability to 0. We find that transitory income shocks explain 17 percent of precautionary wealth, persistent income shocks explain 31 percent, and the crisis shock explains 11 percent. Note that transitory income shocks explain more precautionary wealth than crisis income shocks, even though crisis income shocks are much more important for explaining severe financial distress, in line with previous research (Deaton, 1991; Carroll and Samwick, 1997). This resembles our finding that expense shocks explain more precautionary wealth than income shocks, even though they generate fewer cases of severe financial distress.

For expense shocks, we set the probability of transitions to the worst shock in the discretized distribution to 0. This exercise yields that these especially severe expense shocks, which occur with 0.4 percent probability, are responsible for 18 percent of precautionary wealth. Instead turning off

[13] We turn off expense shocks by setting the exogenous preference shock η to always equal its median value. We turn off income shocks by setting the variance of all three kinds of income shocks to zero.

all three of the worst shocks in the discretized expense shock distribution (which together occur with 4.1 percent probability) reduces precautionary wealth by 59 percent. While researchers have long known that unlikely but extreme income shocks can induce significant precautionary saving (Carroll and Samwick, 1997), expense shocks have a much thicker right tail in our data, which is a major reason they have a stronger impact on saving in our model.

Why does the source of precautionary wealth matter? One reason it that it determines who holds the wealth; Figure A-11 shows that the relationship between normalized precautionary savings and persistent income z is much flatter in MAIN than in INC, consistent with evidence that the precautionary motivate falls as income increases (Fulford, 2020). Another major implication is that, because income risk drives just some precautionary wealth, the relationship between income risk and wealth across countries (Carroll, Slacalek, and Tokuoka, 2014; Joyce and Singh, 2025) or income or demographic groups (Hubbard, Skinner, and Zeldes, 1995; Derenoncourt et al., 2024) is likely smaller than traditional models would predict.

Consumption Smoothing Although in modern models precautionary wealth is used to smooth consumption, when also facing expense shocks consumers should instead use it to smooth the marginal utility of consumption (Bewley, 1977). Here we show that, in a quantitative model disciplined with evidence on expense shocks, more liquidity leads to more volatile consumption, rather than less.

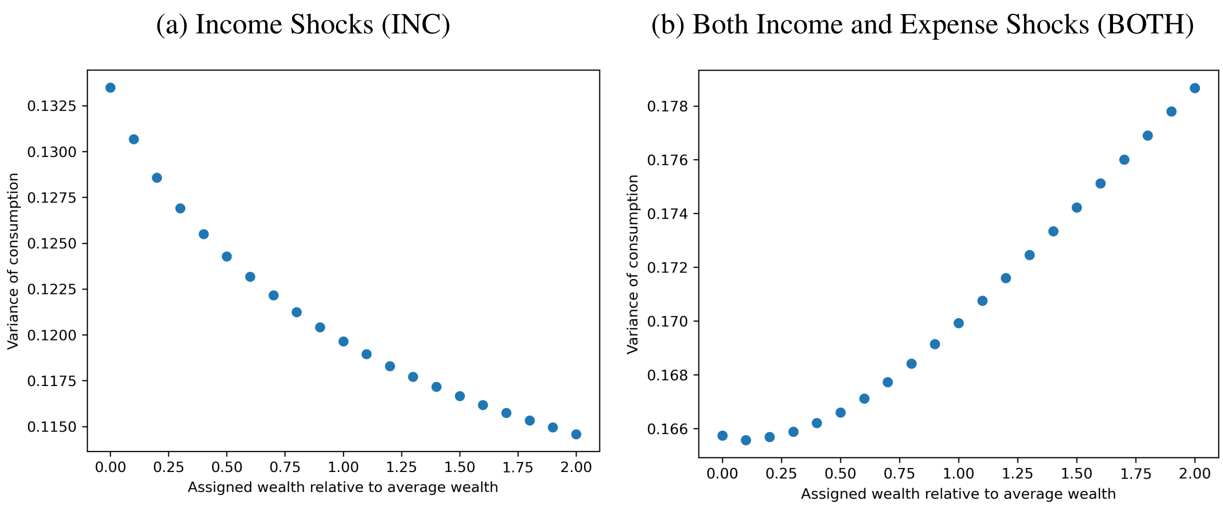
We generate exogenous variation in liquidity by assigning agents' a rather than using their simulated values. We hold their other state variables as given by the simulation. Then we study how the variance of total spending $c + x$ varies with the assigned values of a, in INC (Figure 7a) and BOTH (Figure 7b).

INC reproduces the standard prediction that, if agents only face income shocks, consumption volatility should fall with liquidity as agents use the liquidity to smooth over income shocks. But in BOTH, which accounts for both income and expense shocks, expense shocks turn out to be a stronger force and so (except for very poor agents) consumption volatility increases with liquidity. These findings are consistent with Miranda-Pinto et al. (2025) and Bergholt, Furlanetto, and Mori (2025), who find empirically that consumption is volatile yet disconnected from income.

Marginal Propensity to Consume The marginal propensity to consume (MPC) out of additional liquidity is a central focus of modern macroeconomics (Kaplan and Violante, 2018; Kaplan, Moll, and Violante, 2018; Kaplan and Violante, 2022; Crawley and Theloudis, 2024; Auclert, 2025; Auclert, Rognlie, and Straub, 2025). Here, we study the implications of our results for MPCs.

One enduring puzzle is that the correlation between MPCs and liquid wealth, while significant, appears to be much weaker in the data than in standard models (Auclert, 2025). In particular, there

Figure 7: Liquidity and Consumption Smoothing



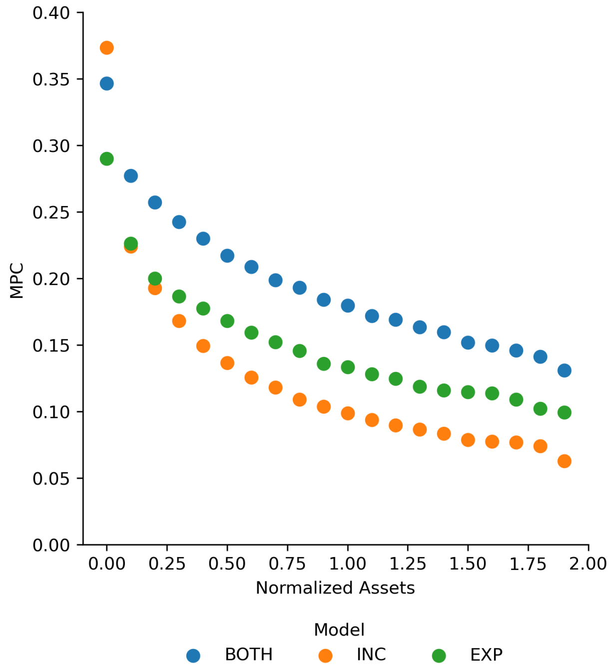
Notes: In an experiment we set all agents' wealth equal to a given multiple (on the x-axis) of average wealth in the model, but hold everything else as simulated. The figure on the left shows the variance of agents' consumption as a function of assigned wealth in the model with income shocks only, INC. The figure on the right shows the same but in the model with both income and expense shocks, BOTH.

is considerable evidence that many low-wealth households have low MPCs (Koşar et al., 2025; Miranda-Pinto et al., 2025) and that many households with significant liquid wealth have high MPCs (Parker et al., 2013; Fagereng, Holm, and Natvik, 2021; Kueng, 2018; Boehm, Fize, and Jaravel, 2025). See Crawley and Theloudis (2024) for a more thorough review.

Expense shocks substantially weaken the relationship between liquidity and MPCs, as shown in Figure 8, which plots MPCs against liquid wealth (normalized by permanent income) in the three versions of our model. One major reason why is that—because lower-income agents are more exposed to expense shocks—expense shocks shift the distribution of precautionary wealth towards lower-income agents (see Figure A-11), consistent with evidence on the precautionary motive (Fulford, 2020). Moreover, recall that to generate the same wealth levels with more liquidity risk, BOTH has a lower discount factor than INC. This raises the MPC across the wealth distribution (Kaplan and Violante, 2022). But the additional risk also makes agents more averse to being near the borrowing constraint, which lowers MPCs for low-wealth agents (Miranda-Pinto et al., 2025). As a result, the relationship between liquidity and MPCs is much weaker in BOTH than in INC. The relationship in EXP is also weaker than in INC, although it is stronger than in BOTH. This finding demonstrates that, while both income and expense shocks help determine how MPCs relate to liquidity, expense shocks are more important.

Another puzzle in the literature is that there is significant heterogeneity in MPCs that cannot be explained by observables, including income (Aguiar, Bils, and Boar, 2025; Lewis, Melcangi, and

Figure 8: MPCs as a function of wealth



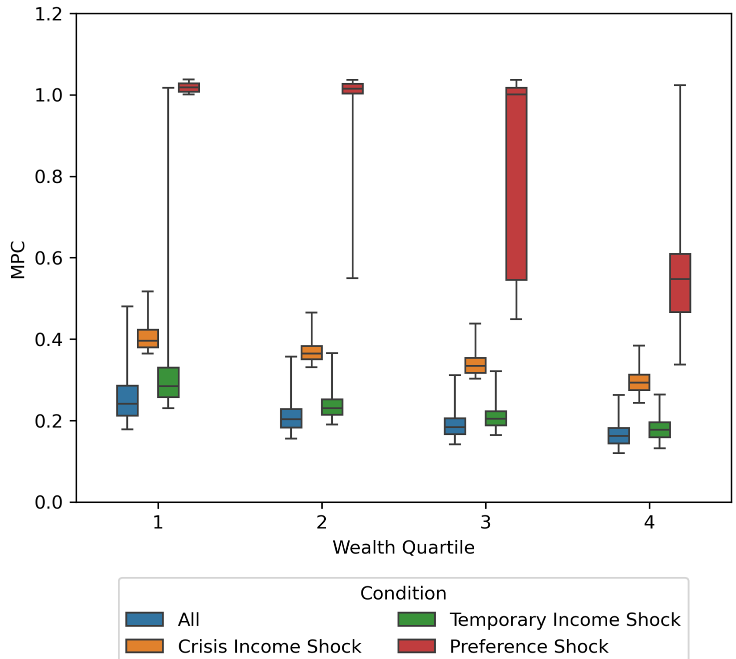
Notes: This figure plots average MPC (y-axis) by bins of assets normalized by permanent income (x-axis) in the three versions of the model. MPCs are calculated by giving agents a very small amount of extra liquidity and calculating the fraction of it that they spend immediately on either normal consumption c or expense shocks x.

Pilossoph, 2024). To examine variation in MPCs generated by shocks, Figure 9 provides a box-whisker plot of MPCs by liquidity shock and wealth quartile. Temporary income shocks generate high MPCs among very poor households, but they are too small to induce large MPCs among even slightly richer households. The crisis income shock is large, but it does not induce large MPCs because it is persistent and so agents experiencing it are reluctant to spend. In contrast, expense shocks induce large MPCs even among cash-rich households, because they are large, temporary, and increase the return to immediate spending even for agents far from the borrowing constraint. This intuition helps to explain why, when we conduct a formal R^2 analysis in Appendix F.3, expense shocks explain over six times more variation in MPCs than income shocks do.

Addressing macro puzzles Modern macroeconomic models often overpredict the responsiveness of household consumption to unexpected events. Here, we briefly consider how accounting for expense shocks helps address this issue.

With nominal rigidities, countercyclical income risk amplifies business cycles in macroeconomic models; a recession increases income risk, which increases precautionary saving and so decreases spending, worsening the recession. This effect, though qualitatively plausible, is known

Figure 9: MPC distribution by shock as a function of wealth



Notes: This figure provides a box-whisker plot of MPCs (y-axis) against quartile of normalized wealth (x-axis). The bottom and top of the boxes correspond to 25th and 7th percentiles; the line represents the median. Bottom and top of whiskers denote the 5th and 9th percentiles. 1.8% of agents are in the crisis income state, so to be comparable, temporary income shocks and preference shocks are defined as those in the top 1.8% of all such shocks.

to be too strong in HANK models (Auclert, Rognlie, and Straub, 2025). If we suddenly and unexpectedly double the probability of entering the crisis income state, the immediate drop in consumption in BOTH is 24 percent smaller than it is in INC, because agents in BOTH respond less to income risk. Thus, accounting for expense shocks should help mitigate the overly powerful role of income risk in modern macroeconomic models.[14]

The response of household spending to changes in real interest rates also appears to be too strong in macroeconomic models. Most famous is the "forward guidance puzzle" (del Negro, Giannoni, and Patterson, 2023): models predict an excessive response of aggregate spending to forward guidance about the path of future real interest rates. More recently, Crawley and Gamber (2026) find that HANK models predict excessive spending responses to changes in real interest rates at any time horizon. If we suddenly and unexpectedly double the real interest rate R from 1% to 2%, the drop in aggregate spending is 60 percent smaller in BOTH than in INC. Therefore, accounting for expense shocks should also help macroeconomic models address this puzzle.

[14]It also seems likely that expense shock risk is less cyclical than income risk is. Since our data do not cover any recessions, we cannot investigate this possibility.

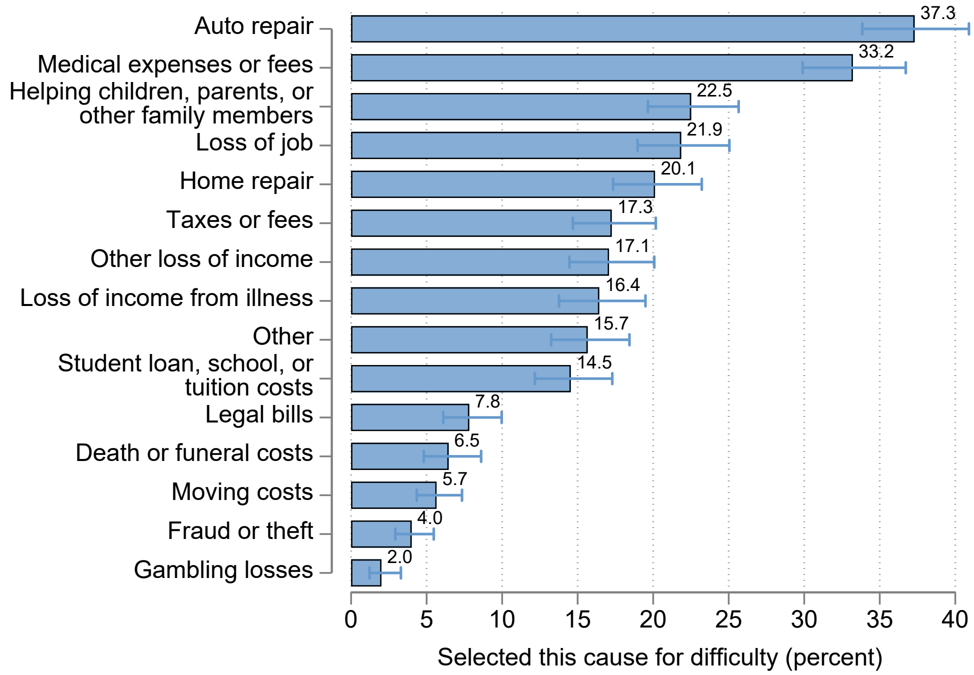
Exacerbating wealth puzzles Finally, we briefly consider the implications of expense shocks for liquid and illiquid wealth. Kaplan and Violante (2022) estimate that the median U.S. household has no liquid wealth, and roughly 15 percent of households have nonpositive *total* wealth (including illiquid assets). Standard one-asset models, including ours, typically do not match the percent of households with zero or negative liquid wealth unless they target it during calibration, because risk-averse optimizing agents avoid being near the liquidity constraint (Kaplan and Violante, 2022). Relative to the models studied in Kaplan and Violante (2022), this problem is particularly acute in our model because we also allow for the crisis income shock, which makes agents especially eager to avoid the borrowing constraint. Thus, INC does poorly replicating the tails of the liquid wealth distribution; only 17.6 percent of agents in INC have nonpositive wealth. Despite its lower discount factor, BOTH does even worse: only 3.3 percent of agents in BOTH have nonpositive wealth because adding expense shocks on top of income shocks makes agents even more averse to the borrowing constraint.

What are the implications for our results? We expect that the answer depends on the specific reasons our model fails to replicate the wealth distribution, which is a major question for future research. But as a first step, we exogenously create a mean-preserving spread in agents' wealth by multiplying a by 0.25 for half of agents, and by 1.75 for the other half. This experiment lowers the average endogenous expense shock x from 10.3 percent of income to 8.5 percent of income. x falls overall because it falls by more among those whose wealth decreased that it increases among those whose wealth increased. This result suggests that, were the model to better approximate the distribution of liquid wealth, it might need more severe preference shocks in order to still replicate the size of expense shocks which suggests that the results from our model may understate the effects of expense shocks.

Given the importance of illiquid assets for many topics, such as explaining homeownership, retirement savings, or the total amount of capital in the economy, another major puzzle is why consumers make long-run investments with short-run costs while also holding little liquid wealth. BOTH requires a low discount factor to explain why households have so little liquid wealth in the face of so much liquidity risk. But a lower discount factor means illiquid assets must have higher rates of return before agents purchase them, which exacerbates the puzzle.[15]

[15] For example, if we offer agents in the models the unexpected opportunity to purchase an annuity with a high per-period return of 10 percent, 92 percent of agents in INC purchase it. But despite its high return, less than 4 percent of agents in BOTH purchase the annuity because their discount factor is so low. Models have been developed that can match some important moments of the liquid and illiquid wealth distribution (Kaplan and Violante, 2014), but these models neglect expense shocks.

Figure 10: What caused difficulty among people who had difficulty and could point to a cause



Notes: Among the 41.1 percent (s.e. 0.9) who said yes to: "At any time in the past 12 months have you or your household had difficulty paying for a bill or expense?" 60.5 percent (s.e. 1.4) said there was an event or expense that caused the difficulty. The figure shows the non-exclusive options for what caused difficulty, among the N=1,175 respondents who said there was an event. Source: 2025 Making Ends Meet surveys.

6 Empirical implications of expense shocks

Section 5 studied the theoretical implications of expense shocks. Here we use our unique data to study the empirical support for these theoretical implications.

First, we consider causal evidence on the relationship between shocks and financial distress. In our 2025 survey, we asked respondents who had "difficulty paying a bill or expense" if there was an "event or expense" that caused their difficulty. 40 percent of consumers could not point to a specific cause, perhaps because a large number of individually minor shocks caused the difficulty (consistent with evidence in Section 3.4). But among the 60 percent who could point to one or more causes, the three most important were all expenses as shown in Figure 10: auto repair (37 percent), medical expenses (33 percent), and helping family or friends (23 percent). Only 22 percent of respondents point to job loss.[16] Interpreting "difficulty paying a bill or expense" as a measure of mild financial distress, these results are consistent with our model's prediction that expense shocks

[16]Some of these events may have been expected. In a 2019 Making Ends Meet survey, 77 percent of people who said an event caused the difficulty said the event was unexpected (Fulford and Rush, 2020).

contribute to many more cases of mild distress than income shocks.

However, among the many respondents who listed no or multiple causes for their payment difficulty, Figure 10 does not quantify the relative role of income and expense shocks in driving it. To study this issue, Table A-7 shows average marginal effects from logit regressions predicting "difficulty paying a bill or expense" that include indicators for expense and income shocks of different sizes. Small and moderate expense shocks (around 40 percent of income or less) are nearly as predictive of this measure of financial distress as comparably-sized income shocks. However—and perhaps surprisingly—the conditional correlation between expense shocks and financial distress varies little with their size, while income shocks are more predictive of financial distress if they are larger. As a result, the largest income shocks have average marginal effects that are roughly three times larger than those of comparably-sized expense shocks. These logit regressions are not causal, and the endogeneity of expense shocks explored in Subsection 4.4 may lead them to understate the role of expense shocks in driving financial distress. However, one possible explanation for our regression results is that, because large income shocks are more persistent than large expense shocks, they are more likely to drive financial distress, as in our model (see Figure 6a).

Another important prediction from the model is that, because expense shocks are temporary, consumers should be more willing to use liquidity to deal with them. To test this prediction, we study the correlation between shocks and changes in credit card debt. We also further study the role of shocks in driving financial distress by studying credit card delinquencies, which are much rarer (and likely costlier) than having "difficulty paying a bill or expense." Because delinquencies are rare, to more precisely estimate differences in them we pool the event effects into three time periods: (1) the year before the survey's reference period, (2) the year of the survey's reference period (during which observed shocks occurred), and (3) the year after the survey's reference period. We estimate individual fixed effects regressions.[17] When studying credit card debt, we limit the sample to people who were using less than half of their available credit card limit a year before the survey and so might have been able to increase their credit card debt after an income or expense shock.

The results, provided in Table 7, provide empirical support for key mechanisms in our model. Income shocks are significantly correlated with credit card delinquencies, but expense shocks are not. Again, this finding is consistent with the much stronger effect of income shocks on financial distress in our model, especially for more severe forms of financial distress, as shown in Section 5. However, expense shocks do predict increases in credit card debt, while income shocks do not.[18]

[17] Fixed effects are identified using the year before the time period of the survey. People without shocks identify common changes during and after the survey time period. Appendix E and appendix Figure A-12 provides event studies for credit card debt.

[18] Credit card debt from credit bureaus includes both payment debt accumulated over the previous month and unpaid revolving debt. However, our finding that the increase in credit card debt is persistent—and in particular lasts after

Table 7: Shocks and credit outcomes

	Panel A I(delinquent) x100		*Panel B* log(credit card debt+100)	
	Expense Shocks	Income Shocks	Expense Shocks	Income Shocks
I(During survey) X I(Shock [10, 40))	0.177	1.656***	0.0707***	0.0304
	(0.306)	(0.377)	(0.0167)	(0.0211)
I(During survey) X I(Shock [40, .))	-0.591	1.638***	0.0415	0.0329
	(0.526)	(0.525)	(0.0280)	(0.0298)
I(After survey) X I(Shock [10, 40))	-0.0476	0.961**	0.0490***	0.0258
	(0.307)	(0.381)	(0.0168)	(0.0213)
I(After survey) X I(Shock [40, .))	0.384	-0.539	0.0886***	0.0259
	(0.527)	(0.529)	(0.0281)	(0.0300)
Only not credit constrained?	No		Yes	
Observations	121,021		91,370	
Consumers	9,451		7,087	
R^2 overall	4.77e-06		0.0285	

Notes: $I(\cdot)$ is an indicator which is 1 if the condition is met. During the survey is the 12 months prior to survey during which shocks could have appeared. After the survey is the 12 months after the survey. Credit card debt is winsorized at the top 0.5 percent. We defined not credit constrained as using less than 50 percent of the credit card limit 12 months before the survey. All regressions include individual fixed effects, time effects, and an age quadratic. We limit the sample to 12 months before the survey and 12 months after. Source: Authors' calculations from the 2022, 2023, and 2024 Making Ends Meet Surveys and CCP.

The stronger correlation of expense shocks with liquidity use is consistent with our model, as discussed in Subsection 5.3.

7 Conclusion

Unlike foundational theory (Friedman, 1957; Lucas, 1976; Bewley, 1977), modern research in household finance and macroeconomics frequently neglects expense shocks. This paper uses unique survey data to provide the first empirical estimates of the total expense shocks that households face. Expense shocks are more common and larger on average than income shocks. Very large expense shocks are several times more common than very large income shocks. Hence modern economic research, though often focused on liquidity risk, typically fails to account for over half of the liquidity risk that households face. Our results provide the information necessary for future research to account for expense shocks.

We use a standard model of endogenous expense shocks to explore the theoretical implications of our empirical results. First, we consider the problem of an agent deciding whether to pay a utility cost in order to gain liquidity; this problem is a stylized version of similar problems considered in a very large number of literatures, from mortgage default, bankruptcy, and housing insecurity to the liquidation of illiquid assets such as life insurance, home equity, and retirement accounts. We show that neglecting expense shocks will lead researchers to significantly underestimate the financial distress experienced by agents making these tradeoffs, as well as the net benefits to them of doing so. Still, even though expense shocks are larger on average than income shocks, expense shocks are less important than income shocks in driving these tradeoffs because they are temporary. But because expense shocks are temporary, agents are more able and willing to use liquidity to deal with them. Partly as a result, we find that expense shocks are substantially more important than income shocks for major topics in macroeconomics and household finance, such as consumption smoothing, precautionary wealth, and the marginal propensity to consume. These findings are striking because to date very few of the many papers on these topics account for expense shocks.

While we follow one standard way of modeling endogenous expense shocks (De Nardi, French, and Jones, 2010) and show that this model can be calibrated to be consistent with our key empirical results, future research should study the empirical and theoretical strengths and weaknesses of different modeling approaches. For example, many of the most important expense shocks we document, including auto repair, home repair, and medical expenses can be viewed as the stochastic depreciation or replacement of a durable good or as a consumption commitment (Chetty and Szeidl, 2007; Miranda-Pinto et al., 2025). Alternatively, modeling expense shocks as simple exogenous

the expense shock—suggests that much of the increase comes from revolving behavior and so indeed reflects liquidity use.

shocks that mechanically reduce disposable income (Livshits, MacGee, and Tertilt, 2007; Fulford and Schuh, 2024) may allow models to easily incorporate much of the additional risk we document while focusing on other innovations.

Our findings raise important puzzles for future research. Precautionary savings models already have trouble explaining why so many households have so little liquid wealth; accounting for expense shocks exposes households to more risk and so exacerbates this puzzle. Our calibration strategy sets agents' discount factor so that, even when facing expense shock risk, the average level of liquid wealth is still correct. But this requires a low discount factor that makes investment in illiquid assets hard to explain. Models have been developed that can match important moments from the liquid and illiquid wealth distribution (Kaplan and Violante, 2014), but these models do not account for expense shock risk. Because empirical evidence (Fulford, 2015; Guiso and Jappelli, 2025) and our model suggest that expense shock risk drives most precautionary wealth, and because explaining both liquid and illiquid wealth holdings is a major goal of consumption-savings models, we think developing models that can explain households' wealth holdings while accounting for expense shock risk should be a priority for future work. One possibility is that it may be more important for models to incorporate realistic but non-standard preferences or beliefs to match the liquid and illiquid wealth distributions while accounting for expense shocks.

Our results demonstrate the importance of accounting for unexpected expenses, but in addition to income shocks and unexpected expenses there are other important risks households face. Persistent and likely anticipated expenses, such as increases in rental payments (Bhutta, 2023), mortgage and tax payments (Low, 2026), and wealth shocks and energy bills (Guiso and Jappelli, 2025) have been shown to be important for households. Future research should examine how to incorporate these risks into models along with income shocks and unexpected expenses to develop a more complete view of the risks households face.

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Appendix

For Online Publication

A Data construction

A.1 Income drops in the January 2022 Making Ends Meet survey

The January 2022 Making Ends Meet survey asked nearly identical expense questions to the later surveys,[19] but had a different block of income questions. This appendix discusses the income questions and how we calculate an approximate income fraction lost for comparison with the January 2023 follow up survey.

The January 2022 survey asked respondents: "In the past 12 months, have you or someone in your household experienced a significant drop in income from any of the following?" and provides a list of 12 possibilities. The survey gave one column for "You" and a separate column for "Someone else in your household," allowing us to break these out, but only asked for a check in the column if the respondent experienced the event, not an active no. The 2022 survey included a retired option. We exclude retirement from the reporting and discussion as a mostly voluntary and planned income drop.

Relative to the 2023 and 2024 surveys, asking about you and your household separately has a similar incidence of households experiencing an income drop, at 32.9 percent in 2022 (calculating incidence as either "You" or "Someone else in your household" experienced at least one income drop and defining the potential responses as respondents who answered the household income question).

A followup question asked respondents who experienced a significant income drop "about how much your monthly income dropped" and "how many months did this last?" for the largest income drop if there was more than one. We calculate a yearly income drop by multiplying the monthly income drop by the months (capped at 12) that the drop lasted. We have examined several methods for calculating an approximate total income drop given that the question asks only about the largest. All of them are imperfect and yield income drops that are larger as a fraction of income than the average experienced in 2023. We describe and implement an approach that gives a reasonable comparison to 2023 and 2024 but acknowledge it makes several assumptions.

[19] Relative to the later surveys, on the 2022 survey the "unplanned gift or loan" option read "giving a gift or loan to a family member or friend outside your household," the "medical or dental expense" option did not include "out-of-pocket" and the 2023 and 2024 surveys had a new option "moving costs."

In 2023, we calculate the ratio of subsequent income drops for households that experienced more than one. The second drop is, on weighted average, 50 percent of the first income drop. The third drop is 54.9 percent of the second drop and the fourth drop (of which there are not many) is 63.3 percent of the third drop. We apply these ratios to the "largest income drop" in the 2022 survey for households that experienced more than one type of drop (treating either you or someone else in the household experiencing a drop as one event). We do not calculate income drop for events more than four.

Using this approach, the weighted mean income lost to income drops is 24.2 percent, the 25th percentile is 5.2 percent, the median is 14.6 percent, and the 75th percentile is 35.1 percent each of which are several percentage points larger than their corresponding values in Table 2.

Other approaches produced larger falls. One approach was to calculate the average income lost for households that had only one income drop in 2022, then apply that income drop calculate smaller income drops for households that experienced more than one drop.

While we do not report the 2022 income drops directly, we use tThe 2022 income drop to calculate the pre-income drop income lost to expenses in 2022. However, because the correlation between expense drops and income drops is low, the impact is small. Indeed, the expenses shocks we calculate in 2022 are nearly identical to the 2023 shocks (with standard errors in parentheses): 68.5 percent (2.1) experienced an expense shock, with the mean income lost 14.9 percent (1.2) nearly the same as in Table 1. The 25th percentile lost to expense shocks is 2.5 percent, the median is 6.3 percent, and the 75th percentile is 15.7 percent, compared to 2.7 percent, 6.6 percent, and 15.9 percent in 2023.

A.2 Duplicate income drops and changing options

The 2025 survey removed one income drop on “Lost rental income from a property you own." In earlier years that included this question, we combined it with “Other significant drop in income” for consistency.

The public use files suppressed an income drop option “Loss of income due to natural disaster” for privacy reasons. Using the non-public data, we found that 1.7 percent of people experienced this income drop in 2023 and 2024.

Several income drop categories can reasonably overlap. For example, in Table 2 someone might "Work less because of illness or injury" and have a "Reduction in work hours" from the same event. When calculating the total income lost in a year, we attempt to remove extra costs that represent the same event where it was reasonable for the respondent to include both. For example, if the respondent selected both "illness or injury" and "reduction in work hours" and put the same value for "how much income did you lose" for both, we include only one drop in our calculation of total income drops. We give the complete table for which combinations we supprresed if they had the same value in our working paper.

The last column records whether we decided to suppress extra values. Our decision rule is that putting the exact same value for events that could plausibly overlap suggests that the events were the same to the respondent, so we suppress extra costs when calculating totals except where it is difficult to see an overlap between the events.

Because there is inherently a subjective element in the decision rule, we report income drops without any suppression. The number of consumers affected is not large, but experiencing multiple large events of the same size even for a few consumers increases the impact of income drops. Compared to the last row in Table 2, the income share lost if experienced is 2.25 percentage points higher at 20.4 percent. The mean cost is $24,433, the 25th percentile, $2,400, the median $8,000, and the 75th percentile $25,000.

B Comparison to consumer expenditure

This section compares MEM unexpected expenses to the categories in the Consumer Expenditure (CE) survey that most closely compare. Not all CE spending is unexpected, so this comparison helps understand how much of a given category is unexpected and provides validation that MEM respondents are giving reasonable values for their unexpected spending.

B.1 Consumer expenditure construction

We construct CE spending using the quarterly interviews conducted from 2021 q2 through 2024 q1 to most closely approximate the relevant time frame for MEM. Consumer Units (CU), CE's version of a family or household, are typically in the survey for four quarterly interview surveys. However, not all CUs are reachable for all four quarters because they might move. To maintain national representativeness, we use all survey quarters, rather than only CUs that are in all four surveys. CE also has a diary component which we do not use, so our shares may not match estimates by CE of consumer expenditure shares.

We use family income before taxes as imputed by CE. To calculate expenditure shares we divide consumption recorded in an interview by one fourth of annual family income. We divide families into annual income groups using annual income. We do not follow CE convention of dividing expenditures from a CU into current calendar quarter and previous calendar quarter but use CU expenditure calculated over the reference period.

We calculate expenditure categories that most closely match MEM unexpected expenses. For comparison with other work, we try to start with the broad CE category rather than build up from individual expenditures. This calculation is straightforward for some categories, but the categories do not always line up exactly. For example, MEM respondents might reasonably include purchases of new household furniture in unexpected moving expenses, but we do not include CE purchases of furniture in moving expenses.

We construct the comparable CE categories as:

- Medical and dental: Medical Services, Medical Supplies, Drugs ((MEDSRVCQ + MEDSRVPQ) + (MEDSUPCQ + MEDSUPPQ) +(PREDRGCQ + PREDRGPQ)).

- Cash gifts: CONTEXPX if CONTCODE is 100 (cash gifts to college students away from home) and 170 (cash gifts to "Any and all other persons not in your CU")

- Vehicle purchase, repair, or replacement: Purchase of new or used cars and trucks (CARTKNCQ + CARTKNPQ + CARTKUCQ + CARTKUPQ), maintenance and repairs (MAINRPCQ +MAINRPPQ), and Towing (UCC 520550).

- House or appliance repair or replacement: Housing repair, maintenance, and insurance (MRPINSCQ + MRPINSPQ) minus the non repair categories (UCC 210901; Ground rent, UCC 220111; Fire and extended coverage, UCC 220121; Homeowners insurance, UCC 220901; Parking UCC 220901); appliance purchases (MAJAPPCQ + MAJAPPPQ), and appliance repairs (UCC 340620).

- Computer or phone repair or replacement: Telephones and accessories (UCC 320232); and Computers and computer hardware for nonbusiness use (UCC 690111).

- Legal and accounting expenses and fees: Legal fees (UCC 680110); Accounting fees (UCC 680902); Parking fees in home city, excluding residence (UCC 520531); Property taxes (UCC 220211); vehicle registration, inspection, and license fees (UCC 520110, 520111, 520112, 520310, 520410).

- Dependent care expenses: Babysitting and child care (UCC 340210); Care for elderly, invalids, handicapped, etc. (UCC 340906); Adult day care centers (UCC 340910); Babysitting, childcare, daycare, preschool (UCC 670320); Day care centers, nursery, and preschools (UCC 670310). CE changed its expenditure aggregation over the period, so these codes are not used for the entire period, see CE documentation.

- Moving, freight, and storage: Moving, storage, freight (UCC 340510); car and truck rental in and out of town (UCC 520516, 520517).

B.2 Comparison to consumer expenditure

Table A-2 shows each category of MEM expenses (except other) as a share of income, the comparable category of CE spending, and breaks both of them down by broad income group.

MEM respondents reported spending shares that are a fraction of the relevant CE category. One important conclusion from Table A-2 is that MEM results provide reasonable estimates of spending that respondents consider unexpected. Unexpected spending should be a fraction of the relevant spending category which includes both expected spending and unexpected spending. Table A-2 shows that MEM estimates of unexpected spending are less than the CE estimates of both unexpected and expected spending. Of course, MEM and CE categories are not exactly the same and different surveys with different sampling population will produce slightly different estimates of even the same survey question, so the confidence intervals cannot always exclude MEM being greater than CE. Yet MEM point estimates are less than CE point estimates almost universally even within income groups. For example, MEM respondents spend a fraction of the CE on medical in each income group, even though the share varies substantially across income groups.

MEM unexpected spending is often a large share of overall spending in the CE category. The medical, vehicle, house and appliance, computer or phone, and dependent care expenses are most comparable between the two surveys. Unexpected spending varies across these spending categories and by income groups: 73 percent of medical spending is unexpected; 52 percent of both vehicle and housing expense are unexpected; 60 percent of phone and computer expenses are unexpected; and 43 percent of dependent care expenses are unexpected.

C Expectations about expenses

In the 2025 Making Ends Meet survey we asked respondents what probability they placed on experiencing a (possibly expected) significant home, auto, or medical expense in the next year. The survey also asked for the probability of a significant income drop from a period of unemployment or furlough.[20]

The first column of Table A-1 shows the average subjective probability of an expense in each

[20] The questions wording followed the same structure as for unexpected expenses but did not include "unexpected" except for house repairs which did ask for "unexpected major house or appliance repair or replacement in the next year?" Cognitive testing confirmed that people were able to put probabilities on events that were reasonable based on their circumstances. The question instructions followed similar instructions in the Survey of Consumer Expectations. The full text for medical was: "In the following question, we will ask you to think about the percent chance of something happening in the future. Your answers can range from 0 to 100, where 0 means there is absolutely no chance, and 100 means that it is absolutely certain. For example, numbers like: 2 and 5 percent may indicate "almost no chance"; 18 percent or so may mean "not much chance" ; 47 or 52 percent chance may be a "pretty even chance"; 83 percent or so may mean a "very good chance"; 95 or 98 percent chance may be "almost certain." How likely is it that you will experience a major out-of-pocket medical or dental expense in the next year?"

Table A-1: Expectations

	Mean prob. will experience next year (%)	Share experienced unexpected last year (%)	Share expect will experience with prob. $\geq 80\%$	Mean prob. will exp next year if p<80 (%)	Mean prob. will exp next year if p<50 (%)	Mean prob. will exp. next year if exp. last year (%)
A major out-of-pocket medical or dental expense	27.89***	29.40***	8.767***	21.69***	12.07***	39.73***
	(0.772)	(1.206)	(0.737)	(0.625)	(0.420)	(1.563)
A major vehicle repair or replacement	31.83***	37.23***	11.67***	24.18***	13.93***	42.67***
	(0.773)	(1.254)	(0.856)	(0.608)	(0.477)	(1.287)
A major unexpected house or appliance repair	28.40***	26.14***	9.153***	22.16***	13.16***	39.55***
	(0.769)	(1.149)	(0.811)	(0.578)	(0.410)	(1.584)
Significant inc. drop from a period of unemp. or furlough	15.53***	14.84***	4.714***	11.65***	7.035***	27.83***
	(0.621)	(0.922)	(0.525)	(0.460)	(0.315)	(1.919)
Observations	2,206	2,300	2,206	2,206	1,543	649

Notes: Observations are for the first row for medical expenses. Source: 2025 Making Ends Meet survey.

category. The second column reports the share who actually experienced an unexpected expense in the previous year. The remaining columns report the share who predict they will experience an expense with a probability greater than 80 percent, and several conditional expectations. We draw several conclusions.

First, for these spending categories, the realization of a major spending event is mostly a shock in the sense of new information based on the realization of an ex ante uncertain outcome.[21] Few people predict they will experience significant medical, vehicle, or home expenses with a probability greater than 80 percent, so a large fraction of the events in these spending categories we measure are "unexpected" in the sense that they were not predicted before hand with a high probability. These results confirm our interpretation that the unexpected expenses we measure are shocks.

Second, we find evidence that people underestimate their likelihood of experiencing these expenses. While people expected they would experience significant expenses at nearly the same rate that they actually experienced significant *unexpected* expenses (column 2), the expectations include some people who put a high probability they will experience the event and so should not report it as "unexpected." If we exclude people who place more than 80 percent chance of the event occurring and so placed a "very good" or "almost certain" likelihood of it occurring according to the survey instructions, the mean probability of occurring falls (column 4) to below the frequency of unexpected events. The mean probability falls even further if we consider the threshold for "expected" to be 50 percent or more (column 5).

[21] Mathematically, one definition of a "shock" is the difference between expectation and realization: $E_{it}[x_{i,t+1}] - x_{i,t+1}$, which may be dependent on $i's$ private information or beliefs.

Third, some people substantially underestimate their risks across all categories. Even for healthy people with insurance, the risk of an out-of-pocket medical expense is not zero, yet 16 percent incorrectly place zero probability that they will experience a major out-of-pocket medical expense in the next year. Moreover, underestimating risk appears to be a type; among the people who incorrectly believe they face zero risk of medical expenses and own their home, 42 percent say they have zero risk of home or appliance repair expenses, compared to only 2.5 percent of people who own their home but did not say they face zero health expense risks. Similarly, 85 percent of the zero-health-risk people say they face zero unemployment risk, compared to 23 percent of people who put positive value on health risks.

Finally, we find evidence of private information or beliefs. Some people place a very high probability on events occurring, suggesting they have private information or are planning purchases in advance (column 3). As our comparison to the Consumer Expenditure Survey (Appendix B) shows, not all spending in these categories is unexpected, reinforcing that our approach to measuring unexpected expenses is necessary. In column 6, we show that people who have recently experienced the event put a higher probability they will experience it next year, either because they have individual information or they have updated their unconditional prior based on new information.

D Conditional expectation of shocks

This section examines the share of people who had one shock who also had another. These matrices help understand whether some types of shocks often accompany others. Because some of the cell sample sizes are small, the standard errors for these estimates are relatively large (and are not shown for space), so we report them because they help identify interesting patterns but do not place large weight on any particular estimate.

Figure A-8 shows unexpected expenses conditional on experiencing other unexpected expenses. In Figure A-8, 45 percent of people who had an unexpected significant moving cost also had an unexpected increase in childcare expenses, while only 16 percent of people with an unexpected medical expense also had an increase in childcare expenses.

Figure A-9 shows income drops conditional on experiencing other income drops.

Finally, Figure A-10 shows conditional means for unexpected expenses and income drops. The top panel shows how often a household that had experience the unexpected expense on the left also experienced the income drop on the bottom. The bottom panel switches the conditional, showing how often a household that had experienced the income drop on the bottom also experienced the expense drop on the left.

E Event study following shocks

We estimate event studies of the form:

$$y_{i,\tau} = \theta_i + \theta_t + f(a_{it}) + \theta_\tau + \epsilon_{i,t} \tag{5}$$

were $f(\cdot)$ is a quadratic in age, θ_t are common time effects which are separately identified from θ_τ because we have multiple surveys at different times and a long panel. Figure A-12 shows how log credit card debt evolves around the survey, with each graph estimated separately for people who experience expense shocks and no sizable income shocks, income shocks and no sizable expense shocks, and combined income and expense shocks. Figure A-12 limits the sample in all cases to people who were using 50 percent or less of their credit card limit a year before the survey and so might have been able to increase their credit card spending.

For people who experience an expense shock and have credit card liquidity, Figure A-12 shows credit card debt increases during and immediately after the survey relative to their personal mean. We do not know when in the year before the survey the expenses took place and expenses may be spread out over the year, so the comparison is most sharp 12 months before the survey and in the year after. For both medium (10 to 40 percent) and large (greater than 40 percent) expense shocks, credit card debt increases by close to 25 percent 12 months after the survey.

F Additional Model Information

F.1 Details on the expense shock transition matrix

The expense preference shock η is first drawn from one of three persistent states: zero, "small", or "large." This sections describes how the 3x3 transition array is calibrated to match our data.

Denote the zero, small, and large expense preference states A,B,C respectively. Denote by p_{ij} the probability of transition from state i to state j.

We make two symmetry assumptions: (1) $p_{BA} = p_{CA}$ and (2) $p_{AC} = p_{BC}$. Assumption 1 is that the probability of transitioning to the zero state in period $t+1$ can only depend on whether or not the agent is in the zero state in period t; conditional on the state at t not being zero, it does not depend on whether the state is small or large. Similarly, Assumption 2 is that the probability of transitioning to the large state in period $t+1$ can only depend on whether or not the agent is in the large state in period t; conditional on not being large, it does not depend on whether the state is zero or small state. Let $\delta_A = p_{AA} - pBA$ and $\delta_C = p_{CC} - p_{BC}$.

Together these two assumptions means the 3x3 transition matrix can be written as a function of two parameters, p_{CB} and p_{CC}:

$$P = \begin{pmatrix} 1+\delta_A - p_{CB} - p_{CC} & p_{CB} + \delta_C - \delta_A & p_{CC} - \delta_C \\ 1 - p_{CB} - p_{CC} & p_{CB} + \delta_C & p_{CC} - \delta_C \\ 1 - p_{CB} - p_{CC} & p_{CB} & p_{CC} \end{pmatrix} \tag{6}$$

To recover p_{CB} and p_{CC}, we assume that agents are in the steady state. Denote by π_A, π_B and π_C the fraction of agents in states A,B, and C, respectively. Then by stationarity, $P^T\pi = \pi$. This yields equations that we can rewrite as:

$$p_{CB} = \pi_A(\delta_A - \delta_C) + \pi_B(1 - \delta_C) \tag{7}$$

$$p_{CC} = \delta_C + (1 - \delta_C)\pi_C \tag{8}$$

These equations imply that we can obtain the 3x3 transition matrix P by specifying four parameters: π_A, π_C, δ_A and δ_C.

We set π_A to equal to the share of households with no expense shocks in our data. We set π_C to equal the share of households with expense shocks greater than 50% of income.

Agents endogenously spend 0 on expense shocks if and only if their expense preference shock $\eta = 0$. Therefore we can exactly set $\delta_A = Prob(x_{t+1} = 0|x_t = 0) - Prob(x_{t+1} = 0|x_t \neq 0)$ from our data. However, there is not a perfect mapping between large (exogenous) expense preference shocks and large (endogenous) expense shocks, so we set $\delta_C \approx Prob(x_{t+1} > .5|x_t \geq .5) - Prob(x_{t+1} > .5|x_t < .5)$.

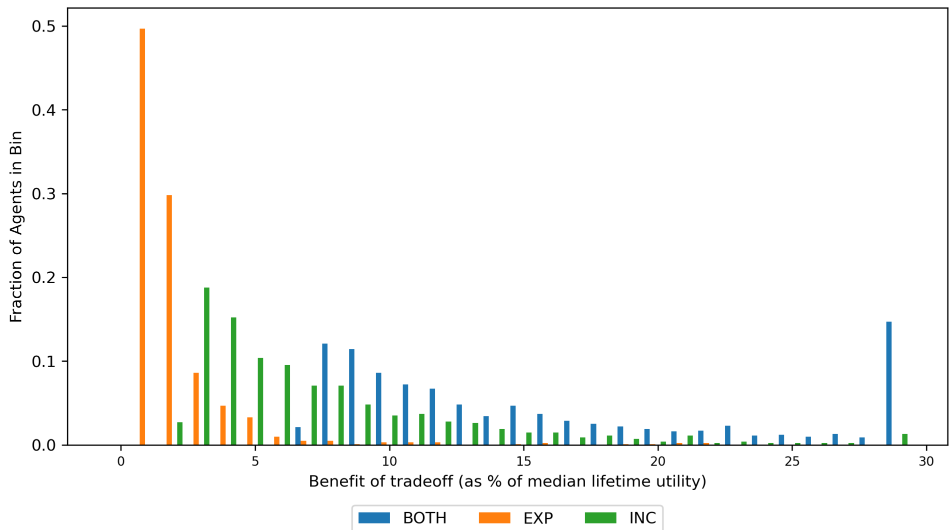
F.2 Tradeoff benefits

We calibrate the tradeoff costs so that, in each version of the model, 1 percent of agents accept it. Figure A-1 plots the distribution of the benefits of the tradeoff among those who accept it in the three versions of the model.

The mass of tradeoff benefits in INC is to the right of those in EXP, which is consistent with our results in Section 5 that income shocks drive more financial distress than expense shocks.

However, among those who accept the tradeoff, the distribution of tradeoff benefits in BOTH is significantly to the right of the distributions in both INC and EXP. In particular, the right tail of the distribution is much thicker. One reason why is that, among financially-distressed agents, it is common for income and expense shocks to occur at the same time. Thus, even though income shocks drive more instances of severe financial distress than expense shocks do, models that do not account for expense shocks are likely to substantially understate tradeoff costs, or the benefits of

Figure A-1: Distribution of tradeoff benefits among those accepting the tradeoff



Notes: Figure shows the distribution of the top 1% of benefits, expressed as a percent of median lifetime utility, of receiving 10 percent of persistent income. In the figure, benefits are winsorized at 30% of lifetime utility. The BOTH model accounts for both income and expense shocks. The EXP model accounts only for expense shocks. The INC model accounts only for income shocks.

the tradeoff among those who accept it.

F.3 Heterogeneity in MPCs

We begin by calculating MPCs for consumers with wealth given by the model, but assuming they are not experiencing any shock, i.e. they do not have the crisis income shock, the permanent and transitory components of income are set to their average level, and the preference shock η is set to its median value. Call agents' MPC under this scenario MPC_{base}. We then calculate consumers' actual MPCs, and we calculate the sum of squared deviations between MPC and MPC_{base}. This is the total variance in MPCs to be explained by shocks in the model.

To explain this variance, first we calculate MPCs for consumers with wealth and income states and shocks given by the model, but with the preference shock still set to its median value. Thus, this exercise accounts for variation in MPCs driven by income shocks, but not expense shocks. It explains 13% of the variation in MPCs.

Second, we calculate MPCs for consumers with income as in MPC_{base} but with η set to the value given by the simulation. Thus, this exercise accounts for variation in MPCs driven by expense shocks, but not income shocks. It explains 77% of the variation in MPCs in the model. Hence, expense shocks explain about six times more variation in MPCs than income shocks do.

Figure A-2: Unexpected expense and income drop questions in the 2023 Making Ends Meet survey

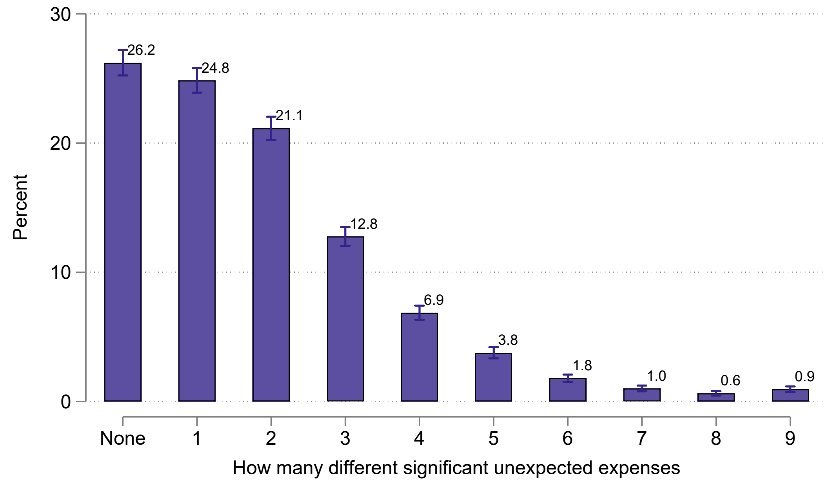
33. In the past 12 months, has your household experienced a significant unexpected expense from any of the following?

	No	Yes	If yes, about how much was the cost?
A major out-of-pocket medical or dental expense	☐	☐	$ ______ .00
An unplanned gift or loan to a family member or friend outside your household	☐	☐	$ ______ .00
A major vehicle repair or replacement	☐	☐	$ ______ .00
A major house or appliance repair	☐	☐	$ ______ .00
A computer or mobile phone repair or replacement	☐	☐	$ ______ .00
Legal expenses, taxes, or fines	☐	☐	$ ______ .00
Increase in childcare or dependent care expenses	☐	☐	$ ______ .00
Moving costs	☐	☐	$ ______ .00
Some other major unexpected expense	☐	☐	$ ______ .00

34. In the past 12 months, has your household experienced a significant drop in income from any of the following?

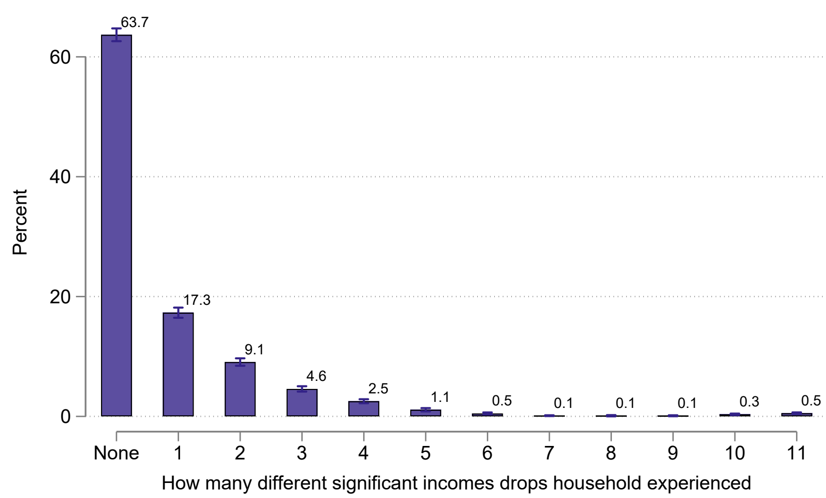
	No	Yes	If yes, about how much income did you lose because of this circumstance over the past 12 months?
Period of unemployment or furlough	☐	☐	$ ______ .00
Reduction in work hours	☐	☐	$ ______ .00
Reduction in wages at your job	☐	☐	$ ______ .00
Changed to a lower-paying job	☐	☐	$ ______ .00
Loss of government benefits	☐	☐	$ ______ .00
Worked less because of illness or injury	☐	☐	$ ______ .00
Worked less to care for others who were sick or injured	☐	☐	$ ______ .00
Worked less or stopped working to take care of children	☐	☐	$ ______ .00
Lost rental income from a property you own	☐	☐	$ ______ .00
Loss of revenue from a business you own	☐	☐	$ ______ .00
Loss of income due to a natural disaster	☐	☐	$ ______ .00
Other significant drop in income	☐	☐	$ ______ .00

Figure A-3: How many different significant unexpected expenses households experienced



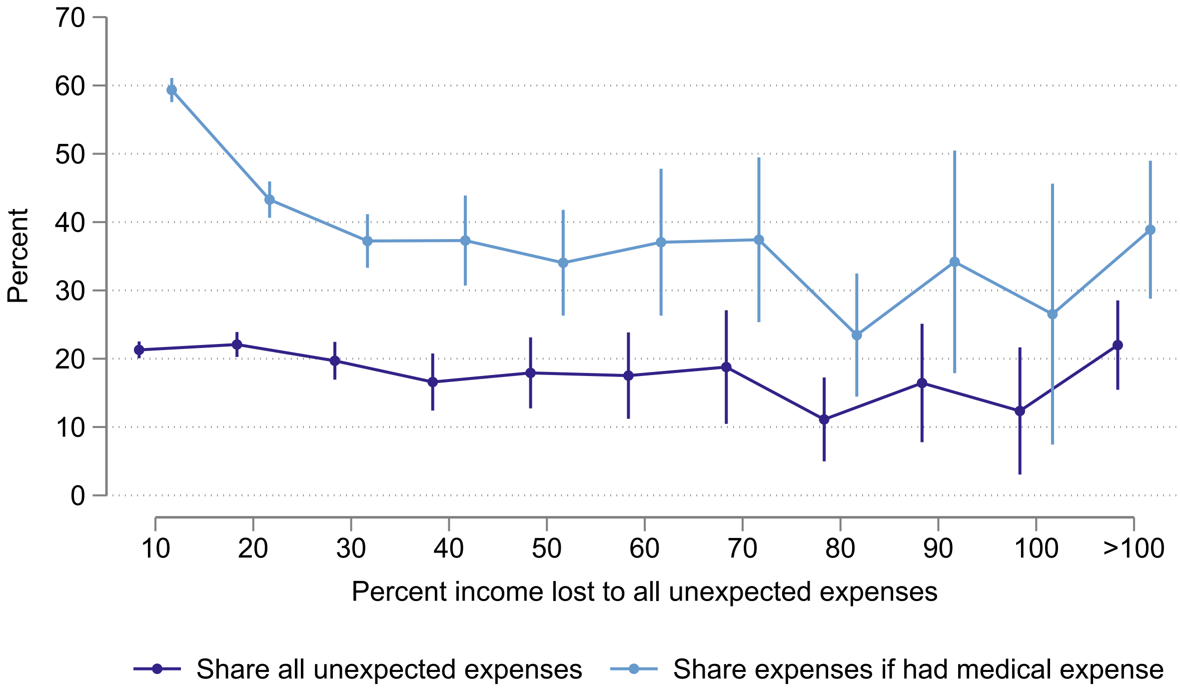
Notes: A household is counted as experiencing a shock even if it did not report a cost or reported zero cost for the shock. Source: Authors' calculations from the 2023, 2024, and 2025 Making Ends Meet surveys.

Figure A-4: How many different significant income drops households experienced



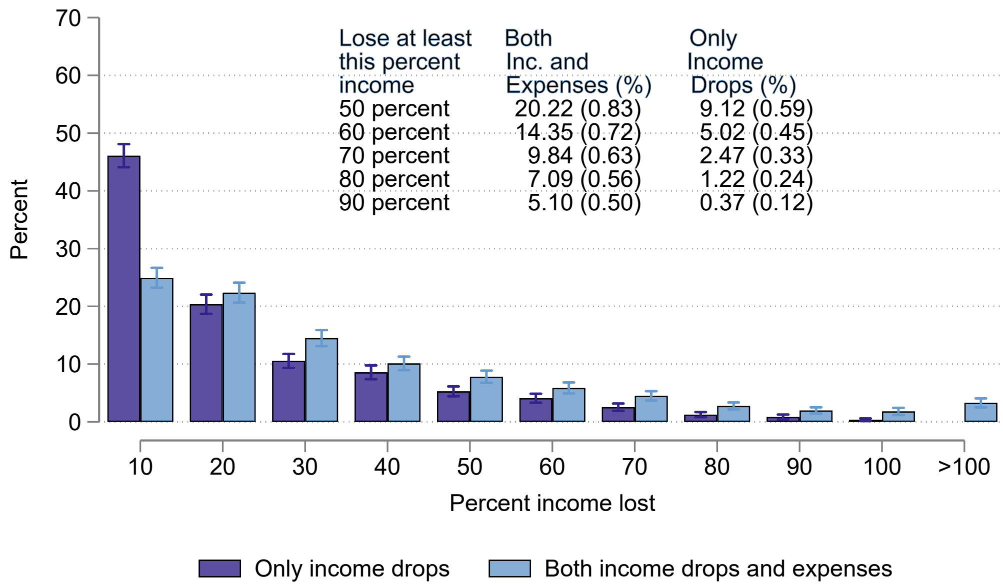
Notes: A household is counted as experiencing a shock even if it did not report a cost or reported zero cost for the shock. Source: Authors' calculations from the 2023, 2024, and 2025 Making Ends Meet surveys.

Figure A-5: Share of medical expenses in all unexpected expenses by share of income lost to all unexpected expenses



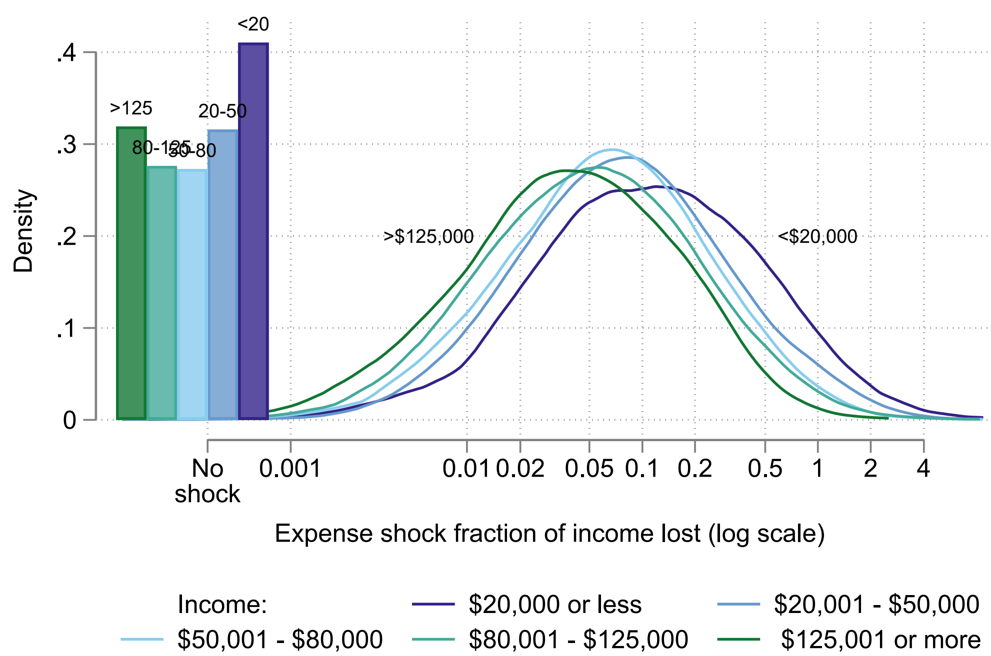
Notes: In the first (lower) line, the share of medical expenses in all unexpected expenses includes medical expenses as zero when the household did not have one. In the second (higher) line, medical expenses are never zero because only households with a medical expense are included. The second line therefore shows that, except for the lowest percent of income lost, for households with a medical expense the medical expense was only around 40 percent of all unexpected expenses. Drop lines show 95 percent confidence intervals. Source: 2022, 2023, 2024, and 2025 Making Ends Meet surveys.

Figure A-6: Among households with income drops, the share of income lost to income drops alone and both income and expense shocks



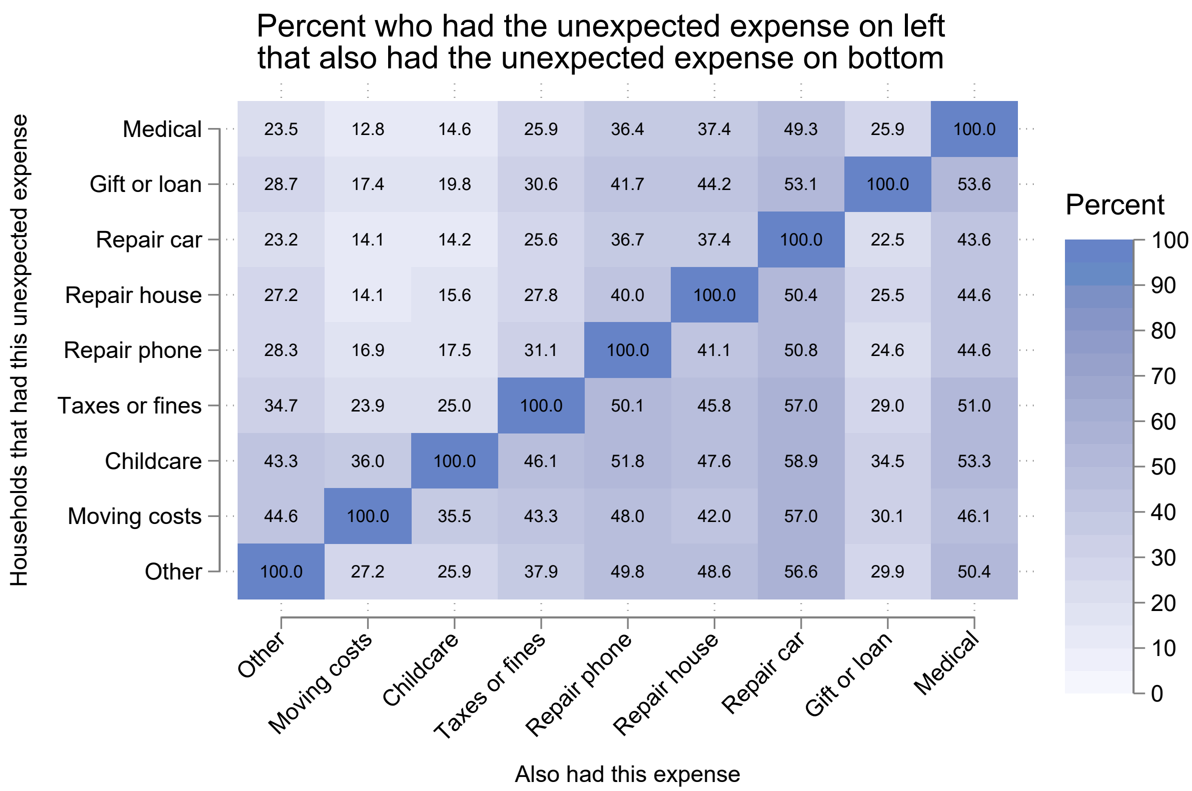
Notes: Among households who experienced an income drop, this figure shows the income lost to the income drop and the income lost to both income drops and unexpected expenses. Note that some consumers may also have income increases which are not identified by the survey. Sources: 2023, 2024, and 2025 Making Ends Meet surveys.

Figure A-7: Distribution of expense shocks across income groups



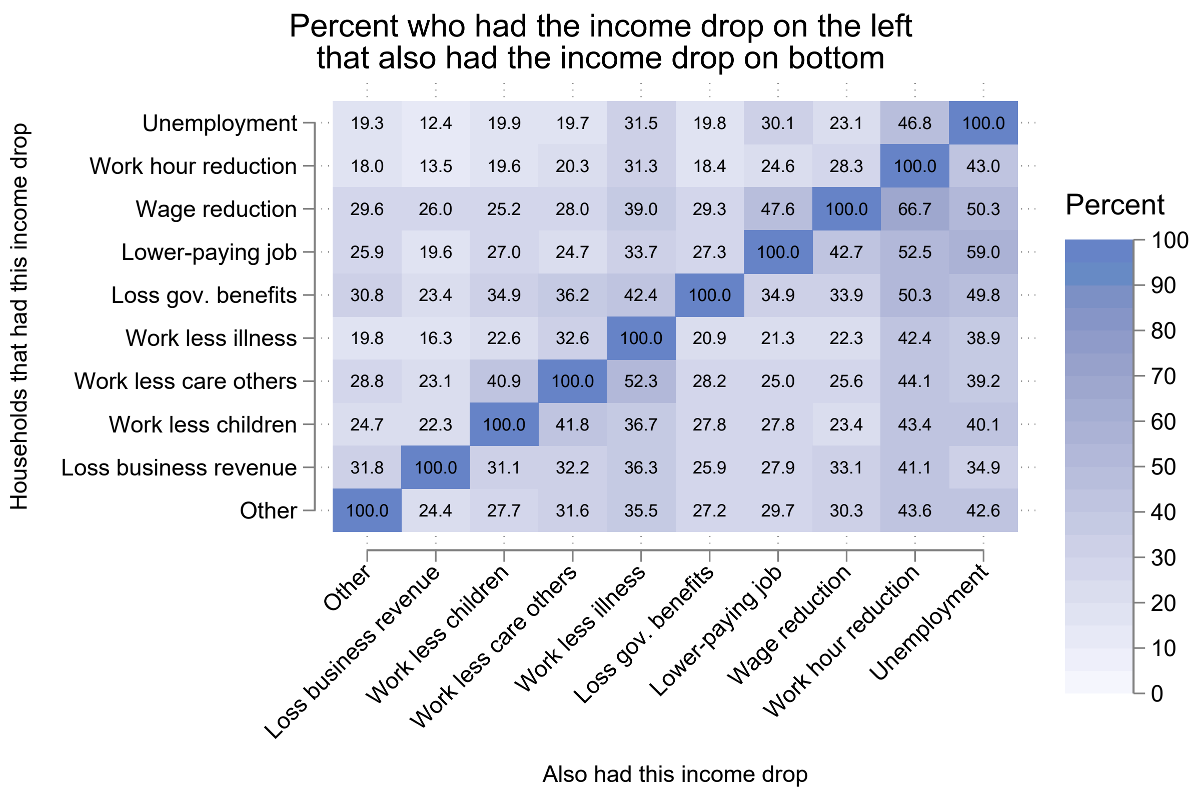
Notes: The figure shows the kernel density of the log expense fraction. The bars are the fraction of households with no expense shock or 0 expenses. Sources: 2022, 2023, 2024, and 2025 Making Ends Meet surveys.

Figure A-8: Individual expenses shocks conditional on expense shocks



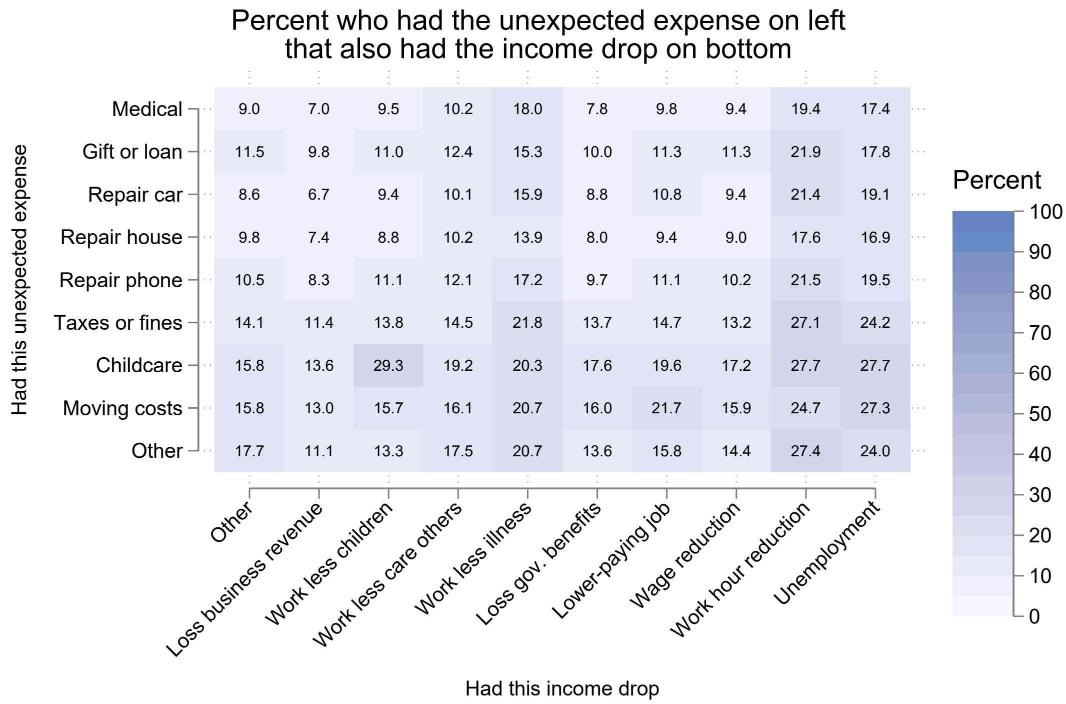
Notes: Each cell shows the conditional share of the population with the expense on the left that also had the expense on the bottom. Source: 2023, 2024, and 2025 Making Ends Meet surveys.

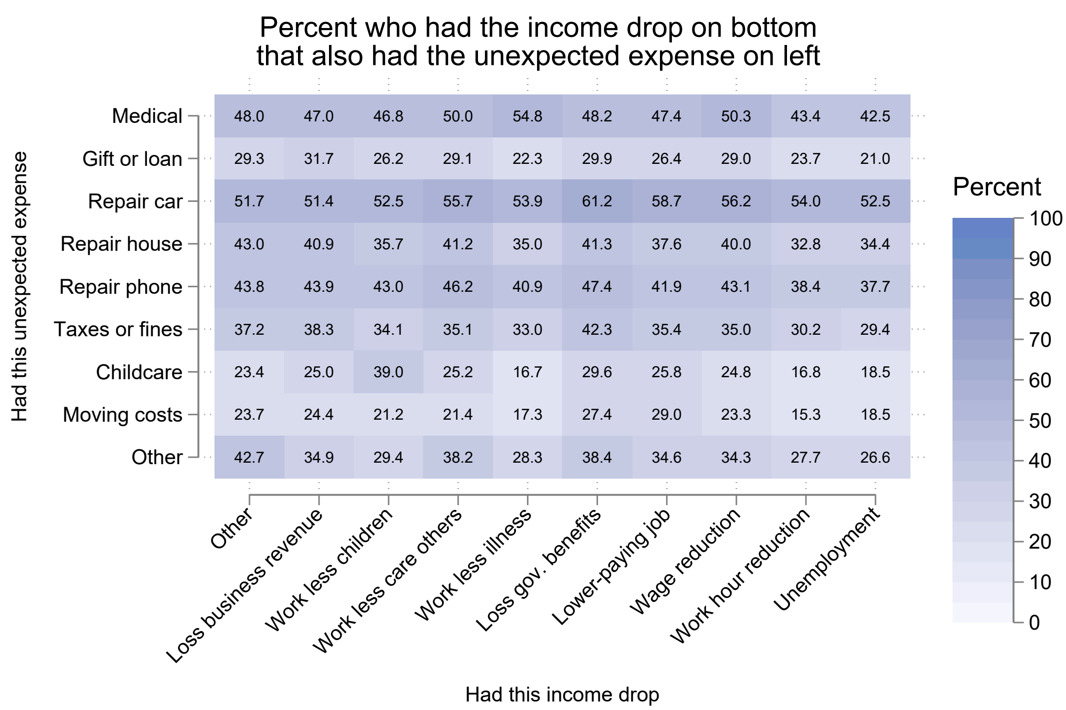
Figure A-9: Income drops conditional on income drops



Notes: Each cell shows the conditional share of the population with the income drop on the left that also had the income drop on the bottom. Source: 2023, 2024, and 2025 Making Ends Meet surveys.

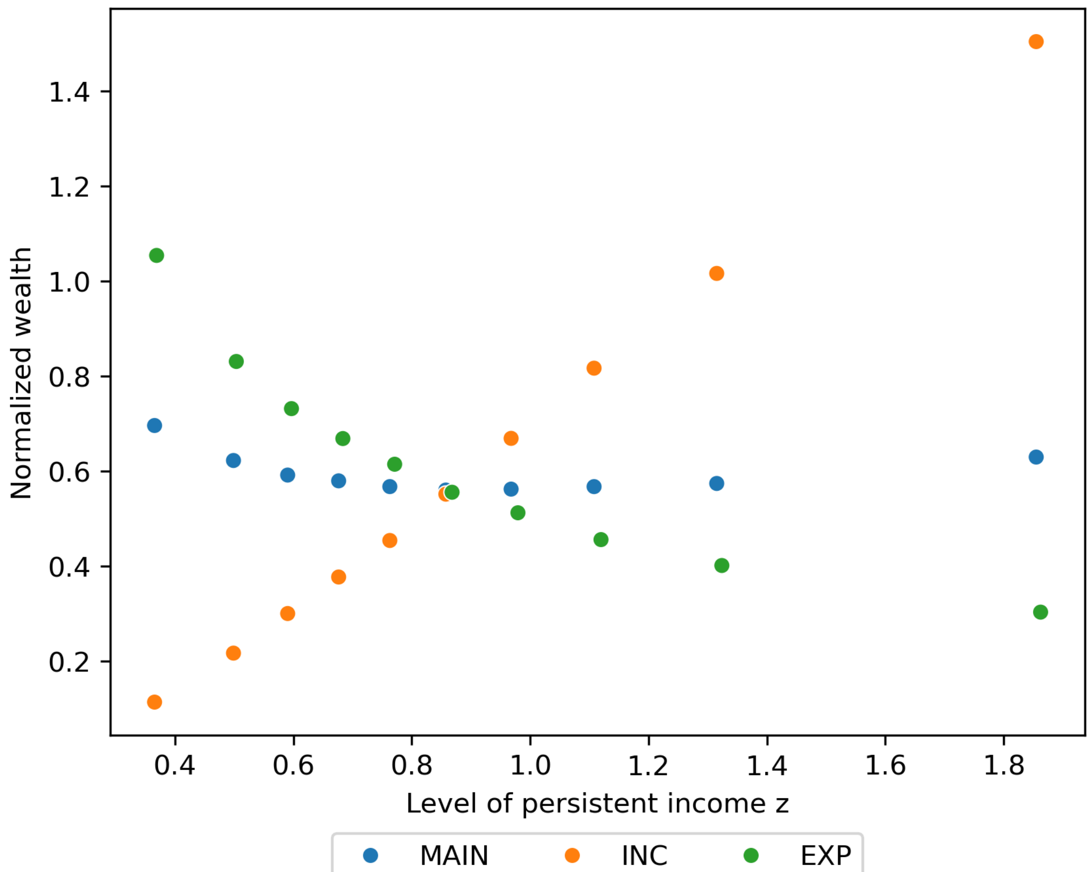
Figure A-10: Income drops conditional on expense shocks and expense shocks conditional on income drops





Notes: Each cell shows the conditional share of the population with the expense on the left that had the income drop on the bottom (top panel) or the share with the income drop that had the unexpected expense (bottom panel) Source: 2023, 2024, and 2025 Making Ends Meet surveys.

Figure A-11: Binscatter Plot of Normalized Wealth against Income



Notes: Figure provides a binscatter plot of wealth normalized by persistent income z (on the y-axis) against z (on the x-axis).

Table A-2: Comparison between unexpected expenses and Consumer Expenditures

	Overall	Household or family annual income				
	share of income (%)	$20,000 or less	$20,001 to $50,000	$50,001 to $80,000	$80,001 to $125,000	More than $125,001
MEM medical	2.47	6.57	3.05	2.14	1.28	0.70
or dental	(0.22)	(1.45)	(0.28)	(0.34)	(0.10)	(0.05)
CE medical	3.11	7.66	4.03	2.55	2.02	1.43
or dental	(0.11)	(0.87)	(0.20)	(0.15)	(0.08)	(0.05)
MEM gifts and loans	0.82	0.97	0.82	1.02	0.83	0.59
	(0.07)	(0.29)	(0.14)	(0.19)	(0.15)	(0.12)
CE cash gifts	1.07	1.71	1.54	0.86	0.65	0.76
	(0.07)	(0.25)	(0.21)	(0.06)	(0.06)	(0.12)
MEM vehicle repair	3.15	3.84	4.46	3.28	2.61	1.75
or replacement	(0.13)	(0.41)	(0.34)	(0.24)	(0.21)	(0.16)
CE vehicle purchase,	6.71	7.61	8.09	6.84	6.06	5.30
repair, or maintenance	(0.24)	(0.93)	(0.61)	(0.57)	(0.43)	(0.31)
MEM house or appliance	1.70	2.06	1.77	1.42	2.31	1.26
repair	(0.10)	(0.43)	(0.15)	(0.13)	(0.37)	(0.10)
CE house or appliance	3.42	7.15	4.57	2.99	2.10	1.90
repair or replacement	(0.13)	(0.81)	(0.29)	(0.21)	(0.13)	(0.12)
MEM computer or phone	0.34	0.57	0.46	0.28	0.32	0.14
repair or replacement	(0.03)	(0.06)	(0.09)	(0.02)	(0.11)	(0.01)
CE computer or phone	0.48	1.12	0.56	0.42	0.33	0.26
repair or replacement	(0.02)	(0.14)	(0.04)	(0.03)	(0.02)	(0.01)
MEM legal expenses,	1.09	1.48	2.02	0.73	0.63	0.54
taxes, or fines	(0.28)	(0.43)	(1.07)	(0.08)	(0.13)	(0.08)
CE legal and	1.13	2.74	1.30	1.14	0.70	0.57
accounting expenses	(0.08)	(0.41)	(0.11)	(0.31)	(0.04)	(0.02)
MEM increases in	0.19	0.32	0.16	0.18	0.19	0.18
dependent care	(0.02)	(0.08)	(0.03)	(0.04)	(0.03)	(0.03)
CE all dependent care	0.42	0.44	0.31	0.31	0.51	0.53
	(0.03)	(0.18)	(0.05)	(0.05)	(0.04)	(0.03)
MEM moving costs	0.28	0.53	0.39	0.23	0.21	0.13
	(0.02)	(0.10)	(0.05)	(0.03)	(0.03)	(0.02)
CE Moving, freight,	0.24	0.65	0.24	0.21	0.16	0.16
and storage	(0.03)	(0.25)	(0.02)	(0.02)	(0.01)	(0.01)

Notes: CE income groups use annual (imputed) before tax family income to most closely match MEM annual household income. We treat each survey as distinct even if CE respondents are included over multiple surveys. The overall CE observations are 23,322 for medical expenditures. The overall MEM observations are 13,800 for medical or dental expenses. Bot CE and MEM are weighted to be nationally representative. Sources: 2022, 2023, 2024, and 2025 Making Ends Meet surveys for unexpected expenses except moving which uses only 2023, 2024, and 2025; and 2021, 2022, and 2023 Consumer Expenditure surveys. See Appendix B for detailed data construction.

Table A-3: Income and expense shocks regressions across demographic groups

	Income lost to unexpected expenses (%)	Indicator have unexp. expense	ln(fraction unexp. exp.)	Income lost to income drops (%)	Indicator have income drop
	OLS	OLS	OLS	OLS	OLS
Inc. $20,001 to $50,000	-7.414***	0.0766***	-0.422***	-4.293***	0.00238
	(2.317)	(0.0187)	(0.0717)	(0.784)	(0.0180)
Inc. $50,001 to $80,000	-10.89***	0.0950***	-0.692***	-6.948***	-0.0636***
	(2.248)	(0.0211)	(0.0837)	(0.833)	(0.0201)
Inc. $80,001 to $125,000	-12.40***	0.0668***	-0.927***	-8.878***	-0.113***
	(2.247)	(0.0214)	(0.0816)	(0.853)	(0.0206)
More than $125,001	-16.13***	0.0110	-1.268***	-11.04***	-0.195***
	(2.165)	(0.0233)	(0.0869)	(0.891)	(0.0217)
High School	1.716	0.0656***	0.107*	1.745***	0.0227
	(1.422)	(0.0162)	(0.0615)	(0.514)	(0.0163)
Some college	2.953**	0.0414**	0.138**	0.731	-0.0118
	(1.427)	(0.0166)	(0.0667)	(0.550)	(0.0166)
College or post-grad.	2.484**	0.0539***	0.181***	1.522***	-0.0275*
	(1.135)	(0.0155)	(0.0599)	(0.465)	(0.0149)
Black	-3.167***	-0.0172	-0.228***	-0.475	0.0553***
	(1.021)	(0.0154)	(0.0535)	(0.492)	(0.0158)
Hispanic	-0.502	-0.00245	-0.0182	0.341	0.0574***
	(1.144)	(0.0140)	(0.0548)	(0.504)	(0.0157)
Other	1.306	-0.0246	0.00938	-0.243	-0.0456**
	(2.405)	(0.0188)	(0.0657)	(0.565)	(0.0178)
Age 40-60	0.740	-0.0144	0.102**	0.0420	-0.0538***
	(0.721)	(0.0123)	(0.0475)	(0.430)	(0.0134)
Age >=61	3.618***	-0.0867***	0.356***	-3.225***	-0.190***
	(1.150)	(0.0140)	(0.0500)	(0.371)	(0.0135)
Share finances with	0.581	0.0597***	0.0555	1.528***	0.0640***
spouse or partner	(0.912)	(0.0123)	(0.0434)	(0.380)	(0.0121)
Have financial	2.812***	0.103***	0.182***	2.774***	0.117***
dependents	(0.755)	(0.0109)	(0.0392)	(0.344)	(0.0119)
Indicator survey	0.303	0.0188	0.0662		
in 2023	(1.417)	(0.0200)	(0.0827)		
Indicator survey	1.146	0.0517***	0.0152	0.646*	0.0300**
in 2024	(1.324)	(0.0186)	(0.0789)	(0.392)	(0.0134)
Indicator survey	0.379	0.0413**	0.0209	0.681*	0.0215
in 2025	(1.408)	(0.0187)	(0.0781)	(0.389)	(0.0131)
Constant	15.82***	0.519***	-2.483***	9.626***	0.350***
	(2.662)	(0.0288)	(0.114)	(0.806)	(0.0220)
Observations	13,465	13,465	9,330	11,513	11,513
R-squared	0.013	0.041	0.073	0.075	0.093

Notes: Excluded group: income below $20,000, less than high school, white, age < 40, no financial spouse or partner, no financial dependents, indicator for survey in 2022. All results are survey weighted. Source: 2022, 2023, 2024, and 2025 Making Ends Meet surveys for unexpected expenses, 2023, 2024, and 2025 surveys for income drops. *** p<0.01, ** p<0.05, * p<0.1

Table A-4: Individual expense shocks across demographic groups

	Income lost to unexpected expenses (%)				
	Medical	Housing	Vehicle	Legal and fines	Unplanned gifts
Overall	2.47	1.70	3.15	1.09	0.82
	(0.22)	(0.10)	(0.13)	(0.28)	(0.07)
Income					
$20,000 or less	6.57	2.06	3.84	1.48	0.97
	(1.45)	(0.43)	(0.41)	(0.43)	(0.29)
$20,001 to $50,000	3.05	1.77	4.46	2.02	0.82
	(0.28)	(0.15)	(0.34)	(1.07)	(0.14)
$50,001 to $80,000	2.14	1.42	3.28	0.73	1.02
	(0.34)	(0.13)	(0.24)	(0.08)	(0.19)
$80,001 to $125,000	1.28	2.31	2.61	0.63	0.83
	(0.10)	(0.37)	(0.21)	(0.13)	(0.15)
More than $125,001	0.70	1.26	1.75	0.54	0.59
	(0.05)	(0.10)	(0.16)	(0.08)	(0.12)
Education					
Less than H.S.	2.99	1.45	3.44	0.75	0.81
	(0.50)	(0.22)	(0.28)	(0.08)	(0.18)
High School	3.04	1.78	3.51	0.91	0.55
	(0.74)	(0.22)	(0.30)	(0.13)	(0.11)
Some college	3.42	1.80	3.30	1.19	0.89
	(0.68)	(0.20)	(0.33)	(0.37)	(0.18)
College or post-grad.	1.58	1.83	2.82	1.34	0.94
	(0.17)	(0.18)	(0.18)	(0.68)	(0.12)
Race and ethnicity					
White	2.53	1.91	3.16	1.25	0.85
	(0.28)	(0.14)	(0.15)	(0.45)	(0.10)
Black	1.98	1.26	3.54	0.86	0.61
	(0.38)	(0.19)	(0.48)	(0.11)	(0.11)
Hispanic	3.31	1.30	3.51	0.80	0.90
	(0.84)	(0.26)	(0.34)	(0.08)	(0.22)
Other	1.40	1.62	2.03	0.79	0.74
	(0.18)	(0.29)	(0.27)	(0.22)	(0.24)
Age					
<40	2.23	1.22	3.34	0.93	0.44
	(0.42)	(0.13)	(0.26)	(0.18)	(0.06)
40-60	2.21	1.53	3.02	0.84	0.56
	(0.26)	(0.13)	(0.16)	(0.07)	(0.09)
>=60	3.01	2.39	3.13	0.69	1.41
	(0.45)	(0.25)	(0.23)	(0.09)	(0.19)
Observations	13800	13872	13882	13933	13964

Notes: People without the shock are included as zeros, so the calculation is the expected loss due to that expense. All results are survey weighted. Source: 2022, 2023, 2024, and 2025 Making Ends Meet surveys. Observations are for the Overall calculation.

Table A-5: Relationship between income and expense shocks

(A) Probit dependent variable: Had expense shock				
Had an income drop	0.245***	0.236***	0.223***	0.211***
	(0.00977)	(0.0132)	(0.0131)	(0.0167)
Share of income lost to		0.000548	0.00135**	0.000497
income drops (%)		(0.000564)	(0.000557)	(0.000870)
Observations	12,148	12,148	12,007	9,914
(B) OLS dep variable: Share of income lost to expenses (%) if had shock				
Had an income drop		-0.889	-1.167	-1.683**
		(0.732)	(0.718)	(0.727)
Share of income lost to	0.0834***	0.101***	0.0329	0.0173
income drops (%)	(0.0208)	(0.0257)	(0.0260)	(0.0259)
Observations	8,376	8,376	8,376	7,471
R-squared	0.003	0.004	0.044	0.041
(C) Probit dependent variable: Had income drop				
Had an expense shock	0.239***	0.240***	0.244***	0.198***
	(0.00956)	(0.00963)	(0.00967)	(0.0121)
Share of income lost to		-6.06e-05	-0.000234**	-0.000742**
expenses (%)		(9.00e-05)	(0.000102)	(0.000318)
Observations	12,148	12,148	12,007	7,594
(D) OLS dep. variable: Share of income lost to income drops (%) if had shock				
Had an expense shock		-0.138	1.764	1.181
		(1.133)	(1.112)	(1.200)
Share of income lost to	0.0838***	0.0844***	0.0324	0.0162
expenses (%)	(0.0227)	(0.0233)	(0.0218)	(0.0218)
Observations	3,772	3,772	3,772	3,193
R-squared	0.008	0.008	0.088	0.059
Control for income	No	No	Yes	Yes
Exclude childcare	No	No	No	Yes

Notes: Probit results show the average marginal effect. Source: Authors' calculations from the 2023, 2024, and 2025 Making Ends Meet surveys.

Table A-6: Persistence of specific expenditure shocks

	In second survey wave:				
Value in first survey wave:	Had this shock (Probit)	Had this shock (Probit)	Percent income lost (OLS)	Percent income lost if >0 (OLS)	ln(frac. income lost) (OLS)
A major out-of-pocket medical or dental expense	0.246***	0.245***	0.185***	0.108**	0.243***
	(0.0166)	(0.0168)	(0.0378)	(0.0502)	(0.0490)
Observations	4,187	4,074	3,773	593	593
An unplanned gift or loan to a family member or friend	0.154***	0.156***	0.139***	0.220**	0.458***
	(0.0154)	(0.0155)	(0.0426)	(0.0898)	(0.0726)
Observations	4,104	3,998	3,794	170	170
A major vehicle repair or replacement	0.284***	0.280***	0.0810***	0.0990**	0.220***
	(0.0159)	(0.0161)	(0.0272)	(0.0489)	(0.0602)
Observations	4,139	4,032	3,818	700	700
A major house or appliance repair	0.245***	0.233***	0.160***	0.318***	0.362***
	(0.0166)	(0.0170)	(0.0512)	(0.0897)	(0.0681)
	4,108	4,000	3,774	437	437
A computer or mobile phone repair or replacement	0.146***	0.141***	0.0888***	0.161	0.306***
	(0.0178)	(0.0181)	(0.0327)	(0.121)	(0.0863)
	4,083	3,976	3,742	302	302
Legal expenses, taxes, or fines	0.191***	0.188***	0.171***	0.231**	0.209**
	(0.0154)	(0.0157)	(0.0545)	(0.0908)	(0.101)
	4,034	3,929	3,712	192	192
Increase in childcare or dependent care expenses	0.147***	0.149***	0.127**	0.330*	0.523***
	(0.0138)	(0.0140)	(0.0568)	(0.180)	(0.109)
	3,968	3,862	3,664	81	81
Moving costs	0.0712***	0.0720***	0.0285	0.391**	0.414***
	(0.0154)	(0.0156)	(0.0219)	(0.166)	(0.128)
	3,014	2,941	2,816	42	42
Some other major unexpected expense	0.146***	0.143***	0.0824**	0.0478	0.281***
	(0.0179)	(0.0179)	(0.0329)	(0.0714)	(0.102)
	3,778	3,676	3,448	125	125
Control for income level	No	Yes	Yes	Yes	Yes

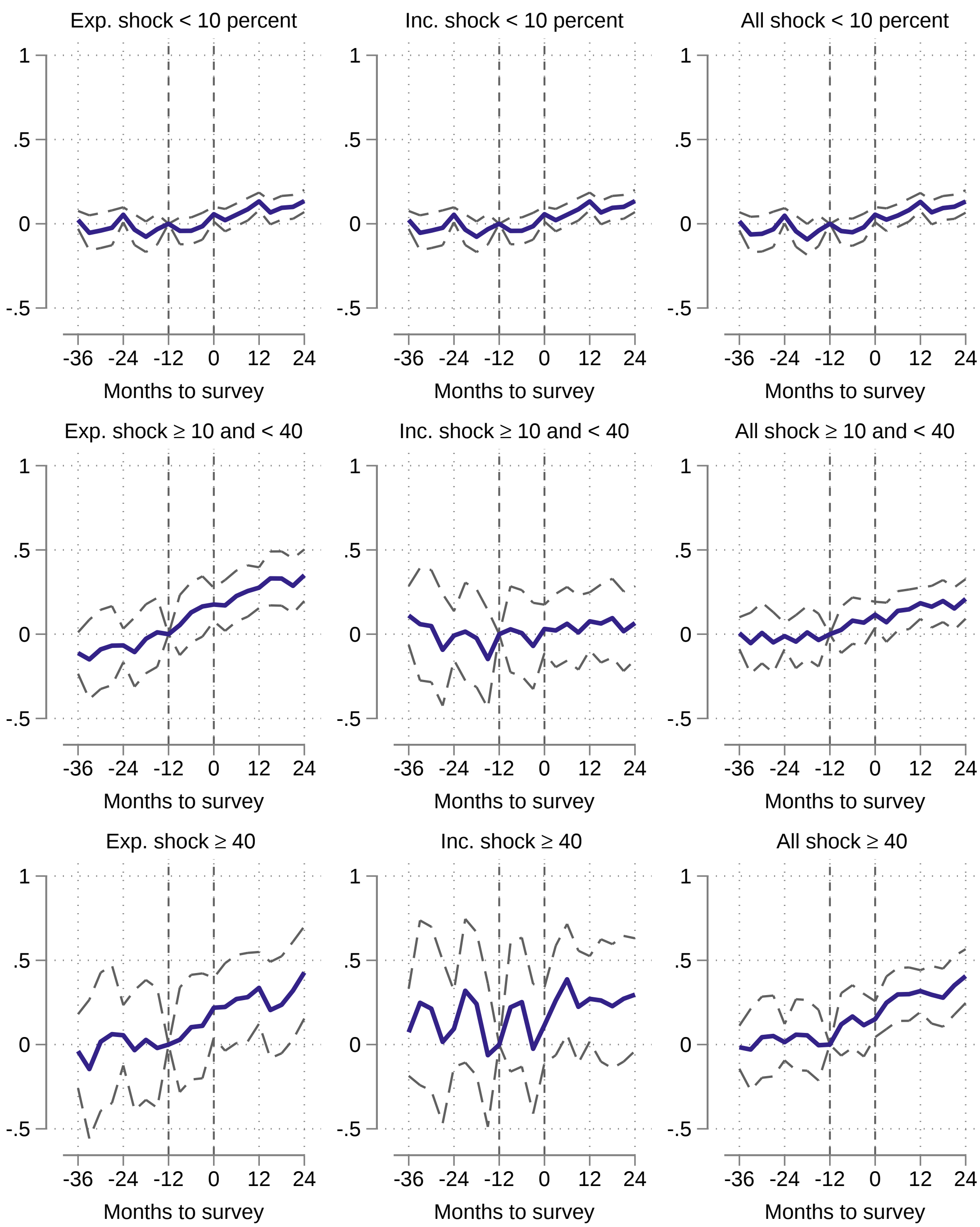
Notes: Each cell is from a different regression. Probit results show the average marginal effect. Moving costs were not in the 2022 survey, so they have a smaller sample size. Source: Authors' calculations from the 2022-2023, 2023-2024, and 2024-2025 Making Ends Meet survey panels.

Table A-7: Shocks and financial distress

	Had difficulty	Had difficulty with food	Had difficulty	Had difficulty with food	Had difficulty did not pay all
Expense (0, 20]	0.160***	0.0989***	0.136***	0.0804***	0.0710***
	(0.0100)	(0.00886)	(0.0110)	(0.00954)	(0.00650)
Expense (20,40]	0.200***	0.129***	0.199***	0.121***	0.0783***
	(0.0178)	(0.0162)	(0.0201)	(0.0179)	(0.0122)
Expense (40, 60]	0.215***	0.120***	0.215***	0.121***	0.0461***
	(0.0301)	(0.0273)	(0.0342)	(0.0305)	(0.0179)
Expense (60, 80]	0.118***	0.111***	0.145***	0.126***	0.0696***
	(0.0381)	(0.0343)	(0.0449)	(0.0405)	(0.0250)
Expense (80, .)	0.165***	0.148***	0.184***	0.169***	0.0441**
	(0.0325)	(0.0301)	(0.0376)	(0.0357)	(0.0195)
Income drop (0, 20]	0.157***	0.133***	0.238***	0.193***	0.117***
	(0.0118)	(0.0106)	(0.0129)	(0.0120)	(0.00828)
Income drop (20,40]	0.214***	0.153***	0.308***	0.230***	0.174***
	(0.0202)	(0.0182)	(0.0213)	(0.0215)	(0.0161)
Income drop (40, 60]	0.292***	0.209***	0.390***	0.302***	0.256***
	(0.0273)	(0.0250)	(0.0270)	(0.0291)	(0.0241)
Income drop (60, 80]	0.340***	0.317***	0.416***	0.366***	0.358***
	(0.0393)	(0.0388)	(0.0382)	(0.0438)	(0.0396)
Income drop (80, .)	0.394***	0.462***	0.385***	0.415***	0.428***
	(0.0479)	(0.0554)	(0.0538)	(0.0621)	(0.0711)
Observations	13,449	13,110	13,810	13,444	11,787
Controls	Yes	Yes	No	No	Yes

Notes: The table presents average marginal effects from logit regressions. The bins are 20 percent bins of expense and income drops as a percent of pre-drop income. The zero expense bin and zero income drop bin are excluded, so the coefficients are relative to zero income drop or expense shock. "Had difficulty" denotes that the respondent answered yes to "At any time in the past 12 months have your or your household had difficulty paying for a bill or expense?" "Had difficulty with food" denotes the respondent answered yes both to "had difficulty" and to a followup question for difficulty paying for food. "Had difficulty did not pay all" denotes the respondent had difficulty and in response to the difficulty selected they did not pay all of a bill or expense. Controls are: log household income (before income drops), education, and survey year. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: Authors' calculations from the 2022, 2023, 2025, and 2025 Making Ends Meet surveys.

Figure A-12: Shocks and changes in credit card debt around the survey



Notes: The dependent variable is ln (credit card debt +100) with credit card debt winsorized at the top 0.5 percent. The plot shows the θ_τ coefficients in Equation 5. Each plot is a separate regression for people that experienced in column 1: expense shocks of each size and no income shocks >10 percent; column 2: income shocks and no expense shocks > 10 percent; column 3: income + expense shocks. The first rows thus have almost the same sample and show almost the same results. All regression limit the sample to people who were using 50 percent or less of their credit card limit at $\tau = -12$. Source: Authors' calculations from the 2022, 2023, and 2024 Making Ends Meet Surveys and CCIP.

G Additional tables and figures

Table A-8: Distribution of individual expense shocks, conditional on experiencing

	Mean ln(fraction income lost)	Variance ln(fraction income lost)
A major out-of-pocket medical or dental expense	-3.51	1.61
	(0.030)	(0.060)
An unplanned gift or loan to a family member or friend	-3.89	2.07
	(0.040)	(0.100)
A major vehicle repair or replacement	-3.32	1.79
	(0.030)	(0.040)
A major house or appliance repair	-3.60	1.88
	(0.030)	(0.070)
A computer or mobile phone repair or replacement	-4.82	1.05
	(0.020)	(0.040)
Legal expenses, taxes, or fines	-3.74	2.02
	(0.060)	(0.110)
Increase in childcare or dependent care expenses	-4.28	1.96
	(0.060)	(0.130)
Moving costs	-3.75	1.29
	(0.050)	(0.070)
Some other major unexpected expense	-3.34	1.75
	(0.040)	(0.090)
Any unexpected expense	-2.81	1.97
	(0.020)	(0.040)

Notes: Standard errors in parentheses. Income is calculated based on pre-income drop income. Source: Authors' calculations from the 2022, 2023, 2024, and 2025 Making Ends Meet surveys with a total sample of 14,202 respondents, except for moving costs which were added as an option after 2022.