Scarred Consumption*

Ulrike Malmendier†
UC Berkeley, NBER and CEPR

Leslie Sheng Shen‡
Federal Reserve Bank of Boston

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Abstract

We show that prior lifetime experiences can "scar" consumers. Consumers who have lived through times of unemployment exhibit persistent pessimism about their future financial situation and spend significantly less years later, controlling for income, employment, and other life-cycle consumption factors. Due to their experience-induced frugality, scarred consumers build up more wealth. We use a stochastic life-cycle model to show that financial constraints and traditional models of income and unemployment scarring fail to generate the negative relationship between past experiences and consumption, while it is consistent with experience-based learning.

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†Department of Economics and Haas School of Business, University of California, 501 Evans Hall, Berkeley, CA 94720-3880, ulrike@econ.berkeley.edu

‡Federal Reserve Bank of Boston, 600 Atlantic Ave Boston, MA 02210, lesliesheng.shen@bos.frb.org

The crisis has left deep scars, which will affect both supply and demand for many years to come. — Blanchard (2012)

I Introduction

Macroeconomic crises often come with lingering, long-term consequences. A well-known example is the slow recovery after the Great Depression, which took a decade despite extraordinary measures such as the New Deal. Another example is the Great Recession: After 2009, consumption was slow to return to prior levels even relative to the growth of income, net worth, and employment (Petev et al. 2011, De Nardi et al. 2012). The longer-run effects of economic crises have been hard to capture in existing workhorse consumption models, including standard life-cycle consumption explanations such as time-varying financial constraints. They are also at odds with "secular stagnation" and "hysteresis" arguments, which rely on low employment due to the loss of worker skills or low private investment.[1]

In this paper, we offer a novel explanation for the lingering crisis effects. Our research hypothesis starts from the observation in Pistaferri (2016) that these long-term effects are accompanied by consumer confidence remaining low for longer periods than standard models imply. We relate this observation to the notion of *experience effects*: Personal experiences of economic crises can scar consumer beliefs and suppress spending for years to come. Both personal unemployment and episodes of high local and national unemployment predict spending years later, controlling for income, employment, wealth, liquidity, and other life-cycle determinants.

These past experiences also predict persistent pessimism about future financial conditions—even though they do *not* predict actual future income after including the standard controls (including recent unemployment), and predict, if anything, increases in future wealth. As a result, heterogeneity in consumers' exposure to past economic conditions gives rise to heterogeneity in consumption, both over time

[1] The literature on secular stagnation conjectured protracted times of low growth after the Great Depression (Hansen 1939). Researchers have applied the concept to explain scarring effects of the Great Recession (Delong and Summers 2012, Summers 2014a, 2014b). Blanchard and Summers (1986) introduce the term "hysteresis effects" to characterize the high and rising unemployment in Europe. Cf. Cerra and Saxena (2008), Reinhart and Rogoff (2009), Ball (2014), Haltmaier (2012), and Reifschneider, Wascher, and Wilcox (2015).

and in the cross section, and explains generational differences in consumption and savings.

The notion of scars from past experiences builds on the macro-finance literature on the longlasting effects of stock-market and inflation experiences (Malmendier and Nagel 2011, 2016).[2] It is also in line with prior work in labor and political economy. Research has documented longterm scars from, for example, graduating into a recession (Kahn 2010, Oreopoulos, von Wachter, and Heisz 2012), or in those who used to live under communism, its surveillance system, and propaganda (Alesina and Fuchs-Schündeln (2007), Lichter, Löffler, and Siegloch (2016), Fuchs-Schündeln and Schündeln (2015), Laudenbach et al. (2018)).

To operationalize the concept of experience effects in the realm of consumption, we form weighted averages over past unemployment conditions during an individual's lifetime so far as proxies for experience effects. Differently from prior literature, we construct such proxies not only based on nationwide economic conditions, but also using local and individual-specific outcomes. As a result, the identifying variation is not absorbed by cohort fixed effects. Relatedly, our paper is the first to estimate within-household effects, which further ameliorates concerns about cross-sectional confounds. To distinguish longlasting experience effects from known earnings implications of job loss (see, e. g., Jacobson, LaLonde, and Sullivan (1993), Couch and Placzek (2010)), we exclude unemployment in the recent past from the scarring measure, in addition to controlling for recent unemployment, income in the recent past, current income, wealth, and other demographics.

We present four baseline results on the relation between past experiences and (1) current consumption, (2) current beliefs, (3) future income, and (4) future wealth.

First, we document the long-lasting scars arising from past exposure to unemployment using the *Panel Study of Income Dynamics* (PSID) from 1999-2017. Both the experience of personal unemployment and the exposure to macroeconomic unemployment conditions earlier in life predict significantly reduced consumption expenditures years later, after controlling for past and current income, wealth, age, a broad range of other demographic controls (including current unemployment), as well as state, year, and household fixed effects. That is, these estimates capture

[2] Theoretical papers on the macro effects of learning-from-experience in OLG models include Ehling, Graniero, and Heyerdahl-Larsen (2018), Malmendier, Pouzo, and Vanasco (2018), Collin-Dufresne, Johannes, and Lochstoer (2016), and Schraeder (2015).

long-lasting experience effects above and beyond known income scarring effects.

The estimated effects are sizable. A one-standard-deviation increase in personal unemployment experiences is associated with a 0.92%-1% (\$344-\$370) decline in total annual consumption spending and a one-standard-deviation increase in the macro-level measure with a 1.60%-1.85% (\$595-\$687) decrease, depending on the experience-weighting function and the number of experience proxies included. The results are robust to a myriad of robustness checks.[3] In addition, we are able to both expand and replicate our results out-of-sample, employing two additional data sets, the Nielsen Homescan Data and the Consumer Expenditure Survey (CEX), which provide for finer product controls and time effects. The estimated effects across all three datasets are very similar in economic magnitude.[4]

Second, we document that past exposure to unemployment also has a lasting effect on beliefs. Using 1953-2018 *Michigan Survey of Consumers* (MSC) data, we show that people who have experienced higher unemployment rates over their lifetimes report more pessimistic beliefs about their financial situation in the future and are more likely to believe that, in general, it is not a good time to purchase major household items. These estimations control for income, age, time effects, and a host of demographic and market characteristics.

Third, we relate consumers' past exposure to unemployment to their actual future income, up to five PSID waves (ten years) in the future. We account for known income dynamics by controlling for current income, wealth, demographics, as well as age, state, year, and also household fixed effects. We fail to identify any robust relation. In other words, actual future income does not explain the strong influence of past unemployment experiences on beliefs and on consumption expenditures.

Our fourth baseline result concerns the wealth implications of consumption scarring. If higher past exposure to adverse employment conditions makes consumers more frugal, their savings and ultimately their wealth should increase. We confirm

[3] For example, we do (or do not) trim the sample to exclude extreme values (in income); we vary the approach to filling "gap years" in the biennial PSID; we vary the units of clustering and double-clustering of standard errors; we do (or do not) include the spouse when measuring past experiences; and we vary the sample weights, including the use of PSID family weights.

[4] We have also explored the Health and Retirement Survey (HRS), which contains information on consumption (from the Consumption and Activities Mail Survey) and wealth on a biennial basis since 2001. However, given that cross-cohort variation is central to our identification, the lack of cohorts below 50 makes the HRS unsuitable for the analysis.

this prediction in the data. A one-standard-deviation increase in personal lifetime unemployment experiences predicts an additional wealth build-up of 1.7% or $3,555 ten years later. Similarly, a one-standard-deviation increase in past exposure to macroeconomic unemployment results in 1.5% (or $3,159) higher wealth ten years later. Unobserved wealth effects, the main alternative hypothesis of our first finding on consumption, do not predict wealth build up, or predict the opposite.

These four baseline results—a lasting influence of past unemployment experiences on current expenditures and on consumer optimism, but the lack of any effect on actual future income, plus positive wealth build-up—are consistent with the proposed mechanism: consumers over-weigh past experiences when predicting future realizations. In contrast, the results are jointly inconsistent with several alternative consumption theories: The inclusion of age controls rules out life-cycle variation, such as an increase in precautionary motives and risk aversion with age (cf. Caballero 1990, Carroll 1994), or declining income and tighter liquidity constraints during retirement (cf. Deaton 1991, Gourinchas and Parker 2002). The controls for labor market status and demographics account for intertemporal expenditure allocation as in Blundell, Browning, and Meghir (1994) or in Attanasio and Browning (1995). The time fixed effects control for common shocks and available information such as the current and past national unemployment rates. The PSID also has the advantage of containing information on wealth, a key variable in consumption models.[5] Moreover, the household fixed effects control for any time-invariant unobserved heterogeneity.

To further distinguish the proposed mechanism from determinants that can be embedded in a life-cycle permanent-income model with rational agents, we turn to a simulate-and-estimate exercise that contrasts Bayesian and experience-based (quasi-Bayesian) learners, and clarifies the orthogonality conditions underlying our identification. The estimations so far account for past unemployment affecting consumers through two channels: (a) a "rational" channel of lost income, lower wealth, and worse employment prospects, and (b) an "experience-based channel" of consumers whose beliefs are scarred by their past exposure to unemployment. We have argued that the controls for current and lagged income, wealth, employment status and other variables differentiate channel (b) from channel (a). We now utilize the Low,

[5] We present a battery of robustness checks to address noise and measurement errors in the wealth and income variable, including estimates of the extent of measurement error which we incorporate in our regressions.

Meghir, and Pistaferri (2010) model of consumption and labor supply to illustrate that this claim holds even when we allow for additional variants of channel (a).

The Low et al. (2010) model accounts for various types of shocks, including productivity and job arrival, and incorporates financial constraints as well as (Bayesian) "income scarring," i. e., the notion that job loss may have long-lasting effects on future income because the job-match quality declines after unemployment. We simulate the model for both Bayesian and experience-based learners, and estimate the relation between past unemployment and current consumption on the simulated data.

The simulations show that, after accounting for all life-cycle determinants and frictions built into the Low et al. model, there is no negative correlation between past unemployment and consumption when consumers are rational. This holds both when we allow for financial constraints and income scarring, as in the original Low et al. model, and when we extend the model to allow for "unemployment scarring" in the sense that job loss itself may induce a negative, permanent wage shock.[6] When consumers are, instead, simulated to overweight their own past experiences, higher past unemployment experiences predict lower consumption, consistent with the empirical findings. The simulate-and-estimate exercise illustrates that it is hard to generate our empirical findings in a rich consumption model with Bayesian learners, and disentangles experience-based learning from potential confounds such as financial constraints, income scarring, and unemployment scarring.

It is noteworthy that, when consumers are simulated to be Bayesian learners, the simulate-and-estimate exercise often predicts a *positive* relation between unemployment experiences and consumption. This reflects that a consumer who earns the same income as another consumer despite worse unemployment experiences likely has a higher permanent income component, and rationally consumes more. Thus, the simulation exercise reveals that our set of control variables "over-controls" and might soak up part of the consumption scarring effect. We conclude that the estimates in the paper are a lower bound on the true underlying scarring effect.

The model also speaks to the role of (imperfect) wealth controls. When we leave out the wealth control in the estimation, we continue to estimate a positive relation between past experiences and consumption in the case of rational consumers and, in most cases, a negative relation in the case of experience-base learners. When

[6] We thank the audience at the University of Minnesota macro seminar for this useful suggestion.

weighting past experiences with a strong recency bias, though, and leaving out the wealth control, the effect becomes positive, suggesting that imperfect wealth controls might, if anything, bias the estimates in the opposite direction.

Guided by these simulation results, we perform a broad range of robustness checks and replications using four variants of wealth controls: third- and fourth-order liquid and illiquid wealth, decile dummies of liquid and illiquid wealth, separate controls for housing and other wealth, and controls for positive wealth and debt. Similarly, we check the robustness to four variants of the income controls: third- and fourth-order income and lagged income, quintile or decile dummies of income and lagged income, and five separate dummies for two-percentile steps in the bottom and in the top 10% of income and lagged income. All variants are included in addition to first- and second-order liquid and illiquid wealth and first- and second-order income and lagged income. Furthermore, we incorporate estimates of the extent of measurement error in income in our regressions. Our results remain robust. We also subsample households with low versus high liquid wealth (relative to the sample median in a given year) and find experience effects in both subsamples. Our variants of wealth and income controls also address the concern that consumption may be a non-linear function of assets and earnings (Arellano, Blundell, and Bonhomme 2017). In summary, a battery of robustness checks and alternative estimation methods that assess the roles of wealth, income, and liquidity confirms the significant effect of past exposure to unemployment on consumption.

Related literature. Our work contributes to the rich literature on consumption since Modigliani and Brumberg (1954) and Friedman (1957). Modern life-cycle permanent-income models account for uncertainty, time-separability, and the curvature of the utility function (cf. overviews in Deaton 1992; Attanasio 1999). Our approach is complementary as it documents lingering effects of past unemployment episodes beyond known life-cycle channels. It predicts that two individuals with similar income profiles and demographics still make different consumption choices if they have lived through different personal and macroeconomic employment histories.

Our approach explains empirical facts that remain hard to reconcile with existing life-cycle consumption models. Campbell and Deaton (1989) point out that consumption underreacts to unanticipated innovation to the permanent component of income (excess smoothness). Consumption also overreacts to anticipated income increases (excess sensitivity; cf. West 1989, Flavin 1993). Proposed explanations range from traditional factors such as liquidity constraints (Gourinchas and Parker 2002; Kaplan et al. 2014; Deaton 1991; Aguiar and Hurst 2015) to behavioral approaches such as hyperbolic discounting (Harris and Laibson 2001), expectations-based reference dependence (Pagel 2017; Olafsson and Pagel 2018), and myopia (Gabaix and Laibson 2017). Experience effects offer a unifying explanation for both puzzles.

Our predictions are reminiscent of models with intertemporal non-separability such as habit formation (Meghir and Weber 1996, Dynan 2000, Fuhrer 2000), since current consumption also predicts long-term effects. However, under habit formation, households suffer disutility from not attaining their habitual consumption. Under consumption scarring, households adjust their consumption based on inferences drawn from past experiences, without direct implications for utility gains or losses.

Another strand of related literature analyzes the long-term effects of macroeconomic shocks. In Kozlowski, Veldkamp, and Venkateswaran (2020), tail events generate persistent drops in economic activity through the channel of belief updating. While its most apparent difference to our paper is that we take the concept of scarred beliefs to the data, there are also conceptual differences. Our approach builds on a large neuroscience literature on synaptic tagging (e. g., Dolan 2002; LaBar and Cabeza 2006; Richter-Levin and Akirav 2003; Talarico, LaBar, and Rubin 2004; Bergado, Lucas, and Richter-Levin 2011), which provide strong evidence that personal experiences alter how we think about the world. Such neuropsychological "rewiring" is separate from rational information updating. The neuroscience-based explanation implies that not only macroeconomic conditions, but also personal experiences have a strong impact on consumption expenditures. Further, experience effects imply generational differences in consumers' response: Younger generations react more strongly to a recent shock than older generations, and the size of the generational difference varies over time depending on the differences in "average" generational experiences at that point in time. Finally, the psychological underpinning of our hypothesis (cf. Kahneman and Tversky 1974, Tversky and Kahneman 1974) imposes structure on the belief formation. For example, people tend to estimate event likelihoods by the ease with which past occurrences come to mind (availability bias) and assign particular weight to the most recent events (recency bias).

Our paper relates to work on adaptive expectations, which also relates belief formation to distributed lags of past realizations (Hansen and Sargent 2008). We are the first to embed this type of expectations formation into a precautionary savings model. In addition, the conceptual differences mentioned above apply: Different lengths of past exposure to unemployment generate different beliefs and consumption choices across different generations (e. g., older versus younger). At the same time, our model is *consistent* with this prior literature, which typically only considers a representative agent: If we collapse the different generations into one (median) agent, the predictions for this representative agent match.

On the empirical side, a growing literature documents that macroeconomic shocks scar individuals. In addition to the labor and political economy literature cited earlier, evidence comes from investors and mutual fund managers who experienced stock-market booms and crashes (Vissing-Jorgensen 2003, Greenwood and Nagel 2009, Malmendier and Nagel 2011), CEOs who grew up in the Great Depression (Malmendier and Tate 2005, Malmendier, Tate, and Yan 2011), and households who experience periods of high inflation (Malmendier and Nagel 2016). Our analysis is the first to examine consumption spending, and to identify effects within-household. Earlier work relied on time variation in cross-cohort differences, making it hard to disentangle time, age, and cohort effects from experience effects. Moreover, we are the first to contrast personal experiences with exposure to macro conditions, and show that personal experiences play a significant role.

II Measures and Data

Our empirical analysis tests whether individuals who have lived through difficult economic times, in terms of both personal and macro-level unemployment, spend less than other consumers with the same income, wealth, employment situation, and other demographics. The opposite holds for extended exposure to prosperous times: Consumers who have mostly lived through good times in the past will tend to spend more than others with the same income, wealth, and demographics.

Measuring past experiences. We focus on exposure to unemployment following Coibion, Gorodnichenko, and Hong (2015), who single out unemployment as the most spending-relevant variable. We consider experiences of both spells of high local and national unemployment, and personal unemployment. The latter distinguishes the proposed mechanism most clearly from existing learning models.

The ideal experiment would exogenously change the experience of unemployment some time in the past for a random sample of households and examine the effect on consumption today, without affecting other household characteristics including income and wealth. The challenge is that unemployment shocks also generate persistent earnings losses (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010). Indeed, we replicate the result on earning losses around displacement in the PSID, as shown in Appendix Figure A.1. To distinguish long-term scarring effects from these known earnings implications of job loss, we measure experience effects excluding unemployment in the recent past, and we control for earnings in the recent past in the analysis. This means that, if the experience-effect hypothesis is correct, we are estimating a lower bound since recent experiences also scar consumption and, in fact, are predicted to have the largest effects due to recency bias.

Specifically, exposure to unemployment accumulated by time t is measured as

$$E_t = \sum_{k=2}^{t-1} w\left(\lambda, t, k\right) W_{t-k}, \tag{1}$$

where W_{t-k} is the unemployment experience in year $t-k$. We start the summation with lag $k = 2$ to ensure that we do not confound experience effects with the known, shorter-term earnings implications of job loss. Weights w are a function of time t, lag k, and a shape parameter λ, using the parameterization

$$w(\lambda, t, k) = \frac{(t-k)^{\lambda}}{\sum_{k=2}^{t-1} (t-k)^{\lambda}}, \tag{2}$$

with either linearly declining weights ($\lambda = 1$) or a sharper decline ($\lambda = 3$) to approximate the weights estimated in Malmendier and Nagel (2011, 2016). We construct personal and macro measures of unemployment experiences as defined in (1) and (2).

Data and variables. Our main source of data is the PSID. It contains comprehensive household-level data on consumption and has long time-series coverage, which allows us to calculate experience-effect measures for each household. In Appendix-

Sections A.2 and A.3, we replicate the results in the Nielsen and CEX data. Compared to those data, the PSID has the advantage of rich information on household wealth, a key variable in consumption models.

The PSID started its original survey in 1968 on a sample of 4,802 family units. Along with their split-off families, these families were surveyed each year until 1997, when the PSID became biennial. We focus on data since 1999 when the PSID started to cover more consumption items (in addition to food) as well as household wealth. The additional consumption variables include spending on childcare, clothing, education, health care, transportation, and housing, and approximately 70% of the items in the CEX survey (cf. Andreski et al. 2014). Regarding household wealth, the survey asks about checking and saving balances, home equity, and stock holdings. Those variables allow us to control for consumption responses to wealth shocks and to tease out the experience effects for different wealth groups. Compared to the Survey of Consumer Finances (SCF), which is often regarded as the gold standard for survey data on wealth, Pfeffer et al. (2016) assess the quality of the PSID wealth data to be quite similar. We construct separate controls for liquid and illiquid wealth, using the definitions of Kaplan, Violante, and Weidner (2014).[7]

The PSID also records income and other demographics, including years of education (ranging from 0 to 17), age, gender, race (White, African American, or Other), marital status, and family size. The information is significantly more complete for the household head than other family members. Hence, while the family is our unit of analysis, our baseline estimations focus on the experiences and demographics of the heads. We then show the robustness to including the spouse's experiences.

The key explanatory variable is each household's cumulative exposure to past unemployment realizations, calculated as in (1) and (2). The PSID allows us to construct measures of both personal and macroeconomic exposure to unemployment.

To measure personal past exposure to unemployment, we first create a set of dummy variables indicating whether the respondent is unemployed at the time of each survey.[8] However, as family units from the later sample have longer histories available

[7] Liquid wealth includes checking and savings accounts, money market funds, certificates of deposit, savings bonds, treasury bills, stock in public companies, mutual funds, and investment trusts. Illiquid wealth is private annuities, IRAs, investments in trusts or estates, bond funds, and life insurance policies as well as the net values of home equity, other real estate, and vehicles.

[8] The PSID reports eight categories of employment status: "*working now*," "*only temporarily laid off*," "*looking for work, unemployed*," "*retired*," "*permanently disabled*," "*housewife; keeping*

than those from the earlier sample, their experience measures would be systematically more precise if we were to work with "all available" data. To avoid biasing the estimates, but also ensure sufficient history to construct a reliable experience measure for a sufficient sample, we restrict the use of the personal-experience indicator to range from year $t-6$ to $t-2$, and combine it with national unemployment rates from birth to $t-7$ into one personal experience measure, with weights as in (2).

For national unemployment rates, we combine several historical time series: a) the data from Romer (1986) for the period 1890-1930; b) data from Coen (1973) for the period 1930-1939; c) the BLS series that counts persons aged 14 and over in the civilian labor force for the period 1940-1946; and d) the BLS series that counts persons aged 16 and over in the civilian labor force for the period 1947-present.

For the more local measure of unemployment experiences, we combine information on where a family has been living (since the birth year of the household head) with information about local historical unemployment rates. Ideally, both sets of information would be available since the birth year of the oldest generation in our data. The oldest heads of household in our sample are born in the 1920s, but the PSID provides information about the region (state) where a family resides only since the start of the PSID in 1968, and the Bureau of Labor Statistics (BLS) provides state-level unemployment rates only since 1976.[9] Mirroring our approach in constructing the personal measure, we use the five most recent years state-level unemployment rates, $t-6$ to $t-2$, either by themselves or combined with national unemployment rate data from birth to year $t-7$. In the former case, we weight past experiences as specified in (2) for $k = 1, ..., 5$, and then renormalized the weights to 1. In the latter case, we use weights exactly as delineated in (2). We find that the estimation results are very similar under all three macro measures: national, regional, and combined. In the following, we will focus on results using the combined macro measure.

houses," "*student*," and "*other*." We treat "*other*" as missing, "*looking for work, unemployed*" as "unemployed," and all other categories as "not unemployed." For each gap years t (in the biennial data), we assume the same employment status, living in the same state as in year $t-1$. Alternatively, we average the values of $t-1$ and $t+1$, shown in Appendix A.

[9] The BLS provides model-based estimates, controlled in "real time" to sum to national monthly (un)employment estimates from the Current Population Survey (CPS). Pre-1976, we could estimate state-level rates from the CPS; we do not do so to avoid inconsistencies and measurement error.

Summary statistics. Table 1 shows the summary statistics for our sample. We focus on household heads from age 25 to 75.[10] In the main analysis, we run the regressions excluding observations with total family income below the 10$^{\text{th}}$ or above the 90$^{\text{th}}$ percentile in each wave. The sample truncation addresses known measurement errors in the income variable.[11] After dropping the individuals for whom we cannot construct the experience measures (due to missing information about location or employment status in any year from $t-2$ to $t-6$) and observations with missing demographic controls or that only appear once, we have 33,263 observations.

Table 1: **Summary Statistics (PSID)**

Variable	Mean	SD	p10	p50	p90	N
Age	49.48	11.49	35.00	48.00	66.00	33,263
Household Size	2.74	1.44	1	2	5	33,263
Household Total Consumption [$]	37,195	25,072	15,055	33,110	63,204	33,263
Household Total Income [$]	65k	35k	23k	58k	116k	33,263
Household Liquid Wealth [$]	27k	184k	-22k	0.1k	70k	33,263
Household Illiquid Wealth [$]	185k	758k	2k	64k	420k	33,263
Household Total Wealth [$]	212k	800k	-2k	64k	510k	33,263
Unempl. Exp. (Personal), λ=1 [%]	5.94	3.36	4.38	4.95	9.84	33,263
Unempl. Exp. (Personal), λ=3 [%]	5.75	5.89	3.02	3.95	13.14	33,263
Unempl. Exp. (Macro), λ=1 [%]	6.06	0.29	5.72	6.04	6.46	33,263
Unempl. Exp. (Macro), λ=3 [%]	5.99	0.56	5.34	5.93	6.75	33,263

Notes. Summary statistics for the estimation sample, which covers the 1999-2017 PSID waves and excludes observations with a total income below the 10^{th} or above the 90^{th} percentile in each sample wave, as well as in the pre-sample 1997 wave (since we control for lagged income). Age, Experience (Personal), and Experience (Macro) are calculated for the heads of households. Household Total Income includes transfers and taxable income of all household members from the last year. Liquid and Illiquid Wealth are defined following Kaplan, Violante, and Weidner (2014). Values are in 2017 dollars (using the PCE), annual, and not weighted.

The average age of the head of household is 49, with an interdecile range (IDR) of 35-66 years, capturing various life-cycle stages. Average household consumption is $37,195 (in 2017 dollars), and of the average wealth of $212k about 13% is liquid.

[10] Controlling for lagged income, the actual minimum age becomes 27. We also use a subsample that excludes retirees (households over age 65) since they likely earn a fixed income, which should not be affected by beliefs about future economic fluctuations. The results are similar.

[11] Gouskova and Schoeni (2007) evaluate the quality of the family income variable in the PSID by comparing it to family income reported in the CPS. The income distributions from the two surveys closely match between the 10$^{\text{th}}$ and 90$^{\text{th}}$ percentiles, but there is less consensus in the upper and lower ten percentiles. As a robustness check, we use the full sample, cf. Appendix-Table A.1.

The age, cohort, and location characteristics translate into lifetime experiences of unemployment around 6%, with significant cross-sectional and time-series variation. For personal experiences of unemployment, past exposure averages at 5.94% when we use linearly declining weights, and a slightly lower value of 5.75% when we impose higher recency bias ($\lambda = 3$). However, the IDR increases, from 4.38-9.84 to 3.02-13.14, when we increase the steepness of the weighting function from λ=1 to λ=3. The values are similar for the macro measures of past exposure to unemployment rates, with means of 6.06% and 5.99%, respectively. Naturally, the standard deviation of the latter measure is much lower, about one tenth of the personal-exposure measures, and the IDR is compressed to less than 1.5%.

III Baseline Results

Our analysis starts from the observation that macro shocks appear depress consumer expenditures and consumer confidence for longer than standard models would suggest (Pistaferri 2016). We test whether we can better predict consumer behavior and beliefs if we allow for a longer-lasting influence of past exposure to adverse economic conditions, as implied by the concept of experience effects.

III.A Consumption

We first relate consumption expenditures to traditional consumption-model determinants, an array of fixed effects, and measures of past exposure to unemployment:

$$C_{it} = \alpha + \psi UEP_{it} + \beta UE_{it} + \gamma^{'} x_{it} + \eta_t + \delta U_{st} + \varsigma_s + \upsilon_i + \varepsilon_{it}. \tag{3}$$

C_{it} is total consumption; UEP_{it} and UE_{it} are respectively i's exposure to personal and macro-level unemployment over her life so far, excluding the present and recent past as shown in equation (1); x_{it} is a vector of controls including wealth (first- and second-order logarithms of liquid and illiquid wealth), income (first- and second-order logarithms of income and lagged income), age dummies, employment status (indicator whether the household head is currently unemployed), family size, gender, years of education, marital status, and race; U_{st} denotes the local unemployment rate, η_t are time dummies, ς_s state dummies, and υ_i household dummies.[12] Standard errors are clustered at the cohort level, and results are similar when clustered by household or two-way clustered at the cohort and time level.

Our main coefficients of interest are ψ and β. The null hypothesis is that both coefficients are zero. The alternative hypothesis is that consumers who have experienced higher unemployment in the past spend less on average and, hence, both coefficients are negative. This interpretation of the coefficient estimates $\widehat{\psi}$ and $\widehat{\beta}$ rests on the assumption that the model accounts for the traditional life-cycle channels, e. g., for the channels of lost income, lower wealth, and worse employment prospects, via the controls for current and lagged income, wealth, employment status and other variables. Furthermore, age fixed effects soak up any age-specific effect, and year fixed effects soak up all variations from the current aggregate unemployment rates. The household fixed-effects fully control for any unspecified time-invariant household characteristics and imply that we identify scarring effects solely from time variation in the within-household co-movement of consumption and unemployment histories.

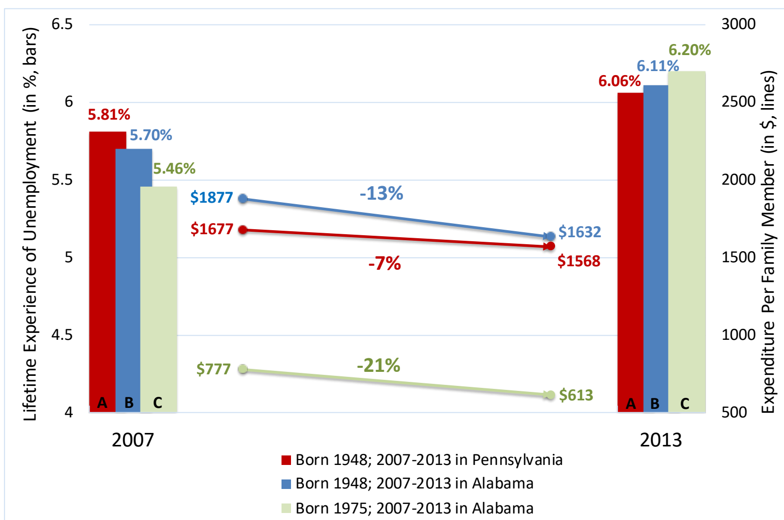
Identifying Variation. Our identification relies on several margins of variation. First, personal unemployment experiences vary at the household level, and these cross-sectional differences evolve over time. Second, macro unemployment experiences vary along three dimensions: People differ in their experiences by cohort and location, and these cross-sectional differences also evolve over time.

To illustrate the three sources of identifying variation exploited with the macro measure, we present a simple example of the unemployment exposure and consumption of three individuals in our PSID data over the course of the Great Recession. Two individuals (A and B) have the same age (born in 1948) but live in different states (Pennsylvania and Alabama) during the 2007-2013 period, and a third (C) lives in the same state as B (Alabama) but differs in age (born in 1975). The two

[12] The results are very similar when including region∗year fixed effects. When including state∗year fixed effects, however, we absorb one key margin of variation in macroeconomic unemployment experience (UE_{it}) at the state∗year level, resulting in insufficient statistical power to precisely estimate coefficients. While the latter approach fully saturates the model and controls for unspecified determinants that affect consumers differently over time and by state, we note that those alternative determinants present a potential confound only if they predicted cross-sectional differences in consumption exactly in the direction of the experience-effect hypothesis, including the different strength of scarring among younger and older people. Instead, we control for current state-level unemployment rates as a sufficient statistic.

sets of bars in Figure 1 illustrate their lifetime exposure to unemployment at the beginning and at the end of the 2007-2013 period, based on the weighting scheme in equation (2) and on their states of residence. Person A enters the crisis period with a history of higher lifetime unemployment rates than Person B (5.81% versus 5.70%), but her lifetime exposure worsens less over the course of the financial crisis and becomes relatively more favorable by 2013 (6.06% versus 6.11%) because unemployment rates were lower in Pennsylvania than in Alabama during the crisis period. Person C has even lower macroeconomic unemployment experiences before the crisis period than Person B (5.46%), but, being the younger person, C is more affected by the crisis which leads to a reversal of the lifetime unemployment experience between the old and the young by the end of the crisis (6.11% versus 6.20%).

Figure 1: **Examples of Experience Shocks from the Recession (PSID)**



Notes. The red (dark) bars depict the 2007 and 2013 unemployment experiences of person A and the red (dark) line the corresponding change of total consumption per member of A's family. Similarly, the blue (medium dark) bars and line show person B's unemployment experiences and consumption and the green (light) bars and line person C's unemployment experiences and consumption. All consumption expenditures are measured in 2013 dollars, adjusted using PCE. Person A's ID in the PSID is 45249; person B's ID in the PSID is 53472; person C's ID in the PSID is 54014.

Figure 1 relates these differences-in-differences of lifetime experience over the crisis period to consumption. The increase in unemployment experiences of A, B, and C by 0.25%, 0.41%, and 0.74%, respectively, were accompanied by decreases in consumption in the same relative ordering, by 7%, 13%, and 21%. Thus the figure illustrates the three sources of identifying variation: variation across cohorts, variation across geographic location, and variation in these differences across cohorts and location over time. The example also shows that two people with the same age can have very different experiences. This is important given that, with age fixed effects included in equation (3), our estimation does not identify off of age effects.

Results. Table 2 shows the estimation results from model (3). In columns (1)-(3), the scarring proxies based on linearly declining weights (λ=1), and columns (4) to (6) use the proxies that shift more weight to recent observations (λ=3).

All control variables have the expected coefficient sign, consistent with prior literature. All coefficients of interest on personal and aggregate unemployment experiences are negative, whether included separately (columns 1–2 and 4–5) or jointly (columns 3 and 6). In other words, both personal unemployment experiences and the exposure to high aggregate unemployment rates suppress spending years later, controlling for current unemployment status, current and lagged income, wealth, and other demographics, as well as age, state, year, and household fixed effects.

The coefficient estimates imply large effects. Using experience measures based on linearly declining weights (column 3), our estimates show that a one-standard-deviation increase in personal unemployment experience predicts a 0.92% decrease in consumption, which is approximately \$344 less annual spending. Similarly, a one-standard-deviation increase in macroeconomic unemployment experience leads to a 1.60% decrease in consumption, which translates to \$595 less annual spending. The estimates using experience measures based on $\lambda = 3$ weights are slightly smaller. As shown in column (6), a one-standard-deviation increase in personal and macroeconomic unemployment experience leads to a 0.6% and 1% decrease in consumption, respectively, which translates to \$211 and \$354 less annual spending.[13] The impact of personal experience and macro experience on consumption is quantitatively similar. The magnitude of the macro experience is particularly remarkable considering that it reflects behavioral change due to consumers witnessing and living through periods of unemployment, locally and nationally, but controlling for personal income shocks.

The results are robust to re-estimation on the entire sample, without excluding observations in the top and bottom 10 percentiles of income. As shown in

[13] In absolute terms, a 1 pp increase in personal and macroeconomic unemployment experience lowers consumption by 0.3% (0.2%) and 6% (3%), respectively, which translate to \$102 (\$63) and \$1,990 (\$1,207) less annual consumption, based on the coefficients in column (3) (column (6)).

Table 2: **Experience Effects and Consumption (PSID)**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.280**		-0.275**	-0.172***		-0.169**
	(0.112)		(0.114)	(0.064)		(0.065)
Experience (Macro)		-0.056***	-0.055***		-0.033***	-0.033***
		(0.018)	(0.018)		(0.011)	(0.011)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	33,263	33,263	33,263	33,263	33,263	33,263
R-squared	0.771	0.771	0.771	0.771	0.771	0.771

Notes. The consumption variables come from the 1999-2017 PSID Consumption Expenditure Data package. We take the logarithm of consumption, income, and wealth; non-positive values are adjusted by adding the absolute value of the minimum plus 0.1 before being logarithmized. "Experience (Macro)" is the macroeconomic experience measure of unemployment, and "Experience (Personal)" is the personal experience measure, as defined in the text. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). Demographic controls include family size, heads' gender, race, marital status, education level, and a dummy variable indicating whether the respondent is unemployed at the time of the survey. Income controls include the first and second order of the logarithm of income and lagged income. Wealth controls include the first and second order of the logarithm of liquid and illiquid wealth. We exclude from the sample observations with total family income below the 10^{th} or above the 90^{th} percentile in each wave from 1999 to 2017, as well as the pre-sample 1997 wave (because we control for lagged income). Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Appendix-Table A.2, the coefficients on personal and macroeconomic unemployment experiences become both larger (in absolute value) and more statistically significant.

We also confirm robustness to several variations in the construction of key variables. First, as discussed above, our baseline specification fills the gap years of the (biennial) PSID by assuming that families stay in the same state and have the same employment status as in the prior year. Alternatively, we average the values of the prior and the subsequent year, $t-1$ and $t+1$. This variation affects both the experience proxy and several control variables, but, as shown in Appendix-Table A.3, the results are robust. Second, our results are robust to including both the head of the household and the spouse in the construction of the experience measure (Appendix-Table A.4). We re-calculate standard errors clustered at different levels in Appendix-Table A.5. We also vary the weighting of observations by applying the PSID family weights, shown in Appendix-Table A.6.[14]

Moreover, we replicate the estimations using two alternative consumption data sets, Nielsen and the CEX. The Nielsen data contain detailed micro-level information on household purchases at the UPC level for each shopping trip. The CEX contains additional categories of consumption including durable and nondurable goods, as well as total consumption that encompasses additional categories of expenditures. Since neither Nielsen nor the CEX provides information on where households resided prior to the sample period, nor their prior employment status, we cannot construct the same personal and (local) macro experience measures as in the PSID. Instead, we construct a macro-level measure based on national unemployment rates, at monthly frequency for the Nielsen data and at quarterly frequency for the CEX data. We find that adverse macro experience strongly predicts not only total consumption but also food, durable, and non-durable consumption (Appendix-Table A.14 and Appendix-Table A.16). Appendix-Sections A.2 and A.3 present more details about the alternative data and the corresponding results.

Overall, the results robustly show that consumers with more exposure to adverse unemployment experiences in the past, both in terms of personal unemployment and in terms of unemployment rates, tend to spend significantly less, controlling for wealth, income, employment, family structures, and demographics.

III.B Beliefs

Turning to the channels through which past unemployment experiences might scar consumers, we ask to what extent they might beliefs about future outcomes.

We first utilize the Reuters/Michigan Survey of Consumers (MSC) microdata on expectations from 1953-2019. The MSC is conducted by the Survey Research Center at the University of Michigan, quarterly until 1977 and monthly since 1978. The data is a repeated cross-section, with 605 individuals surveyed each month on average.

We identify two questions that capture expectations about economic conditions

[14] We do not use PSID family weights in the main regression due to the usual efficiency concerns.

and consumption. The first question elicits beliefs about one's future financial situation: "Now looking ahead – do you think that a year from now you will be better off financially, or worse off, or just about the same as now?" The second question is about attitudes towards durable consumption purchases: "About the big things people buy for their homes – such as furniture, refrigerator, stove, television, and things like that. Generally speaking, do you think now is a good or bad time for people to buy major household items?" Note that the first question elicits beliefs about respondents' personal situation, while the second elicits beliefs about the macroeconomic situation. For the empirical analysis, we construct two binary dependent variables. The first variable takes 1 if the respondent expects better or the same personal financial conditions over the next 12 months. The second takes 1 if the respondent assesses times to be good or the same for durable consumption purchases.

The explanatory variable of interest is again a measure of unemployment experiences. Since the MSC does not reveal geographic locations, we use national unemployment rates to construct "Experience (Macro)" variables as in equation (1) for each of individual i at time t. Mirroring the PSID measure, we apply weighting function (2) to each individual's vector of past experiences from birth until year $t-2$. We also extract income and all other available demographic variables, including education, marital status, gender, and age.[15] We regress the indicators on past unemployment experiences, controlling for current unemployment, income, demographics, and age and year fixed effects. Year fixed effects absorb all current macroeconomic conditions as well as all historical information available at the given time.

Table 3 shows the corresponding linear least-squares estimations. In columns (1) to (3), we find that people who have experienced times of higher unemployment in the past are significantly more pessimistic about their own future financial situation. The statistical and economic significance of the estimated effect is robust to variations in the controls: Whether we include only (age and time) fixed effects, control for income, or for all demographic variables, we always estimate a highly significant coefficient between -0.020 and -0.016. The robustness of the estimates to the income control is reassuring, since the controls for respondents' financial situation are more limited in the MSC data. Income has the expected positive coefficient, as do demographics

[15] The MSC does not make information about race available anymore via their Survey Documentation and Analysis (SDA) data access system, since it has been found to be unreliable. When we extract the variable from the full survey, all results are very similar with the additional control.

Table 3: **Experience Effects and Expectations**

	Expected financial condition coming year (1 = Better or Same, 0 = Worse)			Good/bad time to buy major household items (1 = Good or Same, 0 = Bad)		
	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)	-0.020***	-0.018***	-0.016***	-0.064***	-0.054***	-0.049***
	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.006)
Unemployment rate	-0.015***	-0.016***	-0.015***	-0.042***	-0.044***	-0.044***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Income		0.017***	0.020***		0.051***	0.042***
		(0.001)	(0.001)		(0.001)	(0.002)
Demographic controls	No	No	Yes	No	No	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	209,752	197,096	195,610	203,467	191,396	189,965
R-squared	0.047	0.048	0.048	0.057	0.065	0.068

Notes. All variables are from the Michigan Survey of Consumers (MSC). The dependent variable in columns (1)-(3) is the response to the question "Now looking ahead – do you think that a year from now you will be better off financially, or worse off, or just about the same as now?" (1 = Better off or about the same, 0 = Worse off) reported by individual respondents in the Michigan Survey of Consumers. Dependent variable in columns (4)-(6) is response to the question "About the big things people buy for their homes – such as furniture, refrigerator, stove, television, and things like that. Generally speaking, do you think now is a good or bad time for people to buy major household items?" (1 = Good (or Same), 0 = Bad) reported by individual respondents. Estimation is done with least squares, weighted with sample weights. "Experience (Macro)" is the macroeconomic experience measure of unemployment, constructed as a lifetime linearly-declining weighted national unemployment rate experienced by households. Demographic controls include education, marital status, and gender. Age controls are dummy variables for each age. The sample period runs from 1953 to 2019. Standard errors, shown in parentheses, are robust to heteroskedasticity. *, **, *** denote 10%, 5%, and 1% significance, respectively.

that might proxy for unobserved wealth (e. g., education). In terms of the economic magnitude, we consider the inter-decile range of lifetime experiences: Respondents at the 90th percentile are 2 pp more likely to say financial conditions will be worse in the next 12 months than those at the 10th percentile.

The estimation results for the second question, shown in columns (4) to (6), are very similar. We estimate a significantly negative effect of past exposure to unemployment on "buying attitude." The coefficient is again fairly stable across specifications, ranging from -0.064 to -0.049. Respondents who have have been exposed to unemployment rates at the 90th percentile of the sample are around 7 pp more likely to say now is a bad time to buy major household items than those at the 10th percentile. This second analysis further ameliorates concerns about unobserved wealth or financial constraints, beyond the stability of coefficients across specifications. Here, respondents are asked about "times in general," and the confounds should not affect their assessment of general economic conditions.

Our results suggest that the economic conditions individuals have experienced in the past have a lingering effect on their beliefs about the future, controlling for current economic conditions, individual unemployment, income, age and other demographics.

Habit formation. Before turning to consumers' actual future earnings, and the question whether these beliefs are overly pessimistic, we ask whether past exposure to unemployment might influence not only consumers' beliefs but also their preferences.

As there are many possible specifications, it is impossible to conclusively reject the instable-preferences explanation, and we can at best test specific formalizations. We explore one preference specification that has garnered significant support in prior literature: habit formation. We study whether the relation between consumption and unemployment experiences is correlated with persistent consumption habits.

We re-estimate (3) including lagged consumption on the right-hand side. This dynamic specification requires a correction for the correlation between the lagged dependent variable and the fixed effects ("dynamic panel bias," Nickell 1981). To obtain unbiased and consistent coefficients, we implement a dynamic GMM panel estimator following Holtz-Eakin, Newey, and Rosen (1988), Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998). That is, we use both level and differenced equations, and instrument the lagged dependent variable using lagged differences for the level equation and lagged levels for the differenced equation.[16] The goodness of fit is calculated as the square of the correlation coefficients between the actual and fitted values of the dependent variable.

The estimates are in Appendix-Table A.7. The effects of personal unemployment experience on consumption remain significant, while macro experience appears less significant. Overall, the estimation confirms the robustness of (personal) experience effects, and indicate that they do not operate through the channel of habit formation.

III.C Future Income

Having established a negative relation of past unemployment experiences with consumption and consumer optimism, we now ask whether such experiences also predict lower future earnings, which would merit reduced spending and pessimistic beliefs.

In Table 4, we re-estimate model (3) with the dependent variable changed to future income. Going from left to right, the columns show the estimation results for future income one, two, three, four, and five survey waves in the future, i. e., two, four, six, eight, or ten years ahead. As before, we weight past unemployment experiences with either linearly declining weights (λ=1), shown in the top half, and with more recency-biased weights (λ=3), shown in the bottom panel.

As the table reveals, neither personal nor past macro exposure to past unemployment predict future income over any horizon in any specification. After controlling for income, wealth, employment status, other demographics, and fixed effects,[17] the estimated experience effects are all statistically and economically small and insignificant.[18] In summary, past exposure to unemployment does not predict future earnings prospects, conditional on current income, wealth, and demographics. As noted earlier, this finding does not contradict evidence in prior literature on persistent earnings

[16] We test for first- and second-order autocorrelation in the first-differenced errors and find that they are first-order serially correlated, but not second-order serially correlated. This supports the validity of the moment conditions used by the system GMM estimators.

[17] All results are similar if we do not include time fixed effects in the regressions, which may more realistically capture how people form belief given information frictions.

[18] Based on our estimates, a one-standard-deviation increase in personal lifetime experiences of unemployment predicts an increase in future income of 0.0% in year $t+2$, 0.3% in $t+4$, 0.4% in $t+6$, -0.09% in $t+8$, and 0.18% in $t+10$ (with a 95% confidence interval of 0.4-0.8%), using the $\lambda = 1$ weighting scheme. A one-standard-deviation increase in macroeconomic lifetime experiences of unemployment predicts an increase in future income of -0.14% in year $t+2$, -0.26% in $t+4$, 0.03% in $t+6$, -0.23% in $t+8$, and -0.09% in $t+10$ (with a 95% confidence interval of 0.5-1.7%).

Table 4: **Experience Effects and Future Income**

	Income_{t+2}	Income_{t+4}	Income_{t+6}	Income_{t+8}	Income_{t+10}
Experience (Personal), λ=1	-0.000	0.098	0.129	-0.027	0.053
	(0.061)	(0.065)	(0.079)	(0.095)	(0.124)
Experience (Macro), λ=1	-0.005	-0.009	0.001	-0.008	-0.003
	(0.012)	(0.013)	(0.017)	(0.023)	(0.031)
Observations	18,736	14,396	10,809	8,019	5,688
R-squared	0.789	0.803	0.816	0.834	0.851
Experience (Personal), λ=3	-0.003	0.055	0.077	-0.014	0.020
	(0.035)	(0.038)	(0.047)	(0.055)	(0.071)
Experience (Macro), λ=3	-0.003	-0.006	-0.001	-0.004	0.000
	(0.007)	(0.008)	(0.009)	(0.013)	(0.018)
Observations	18,736	14,396	10,809	8,019	5,688
R-squared	0.789	0.803	0.816	0.834	0.851
Income controls	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes

Notes. The dependent variables are future income in two, four, six, eight, and ten years, respectively. "Experience (Macro)" is the macroeconomic experience measure of unemployment, and "Experience (Personal)" is the personal experience measure. In the top panel, we use experience measures based on linearly declining weights (λ=1), and in the bottom panel, we use experience measures that shift more weight to recent observations (λ=3). Demographic controls include family size, heads' gender, race, marital status, education level, and a dummy variable indicating whether the respondent is unemployed at the time of the survey. Income controls include the first and second order of the logarithm of income and lagged income. Wealth controls include the first and second order of the logarithm of liquid and illiquid wealth. We exclude from the sample observations with total family income below the 10^{th} or above the 90^{th} percentile in each wave from 1999 to 2017, as well as the pre-sample 1997 wave (because we control for lagged income). We take the logarithm of income, and wealth; non-positive values are adjusted by adding the absolute value of the minimum plus 0.1 before being logarithmized. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

losses after unemployment shocks(Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010). Indeed, we replicate the result on earning losses around displacement (Appendix Figure A.1). Instead, we show the lack of predictive power of past experiences for future earnings after controlling for current earnings and earnings in the recent past (and other demographics), which absorb those dynamic effects. This lack of predictive power after controls is interesting because past experiences do predict reduced consumption after the same control.

One may still wonder about the volatility of future income: After unemployment experiences, a consumer might (correctly) feel greater uncertainty about the stability of their future employment, save more to mitigate the risk, and thus consume less.

To assess the relationship between unemployment experience and income volatility we re-estimate model (3) using income volatility as the dependent variable. Following Meghir and Pistaferri (2004) and Jensen and Shore (2015), we construct volatility measures for both transitory and permanent income. The transitory income-variance measure is the squared two-year change in excess log income, where excess log income is the residual from an OLS regression of log income on our full slate of control variables. The permanent-income variance measure is the product of two-year and six-year changes in excess log income (from year $t-2$ to t and $t-4$ to $t+2$, respectively). Table 5 shows the results for income two years in the future (columns 1 and 2), four years in the future (columns 3 and 4), and six years ahead in the future (columns 5 and 6), and again for both weighting schemes (top half versus bottom half). We do not find any significant correlation between unemployment experiences and income volatility over any of the future time horizons. Hence, consumers' long-term reduction in consumption after past unemployment experiences does not appear to be a rational response to future income uncertainty.

III.D Wealth Build-up

The lingering effects of past unemployment on consumption, and the lack of any relation with future income, imply that past exposure to unemployment could affect the build-up of wealth. If consumers with high past exposure to unemployment restrain from spending more than rationally "required" by their income and wealth positions, their experience-induced frugality should generate higher future wealth. Vice versa, consumers who have lived through mostly good times (i. e., low unemployment) are predicted to be spenders and should thus end up with less wealth.

We test whether experience effects are detectable in long-run wealth accumula-

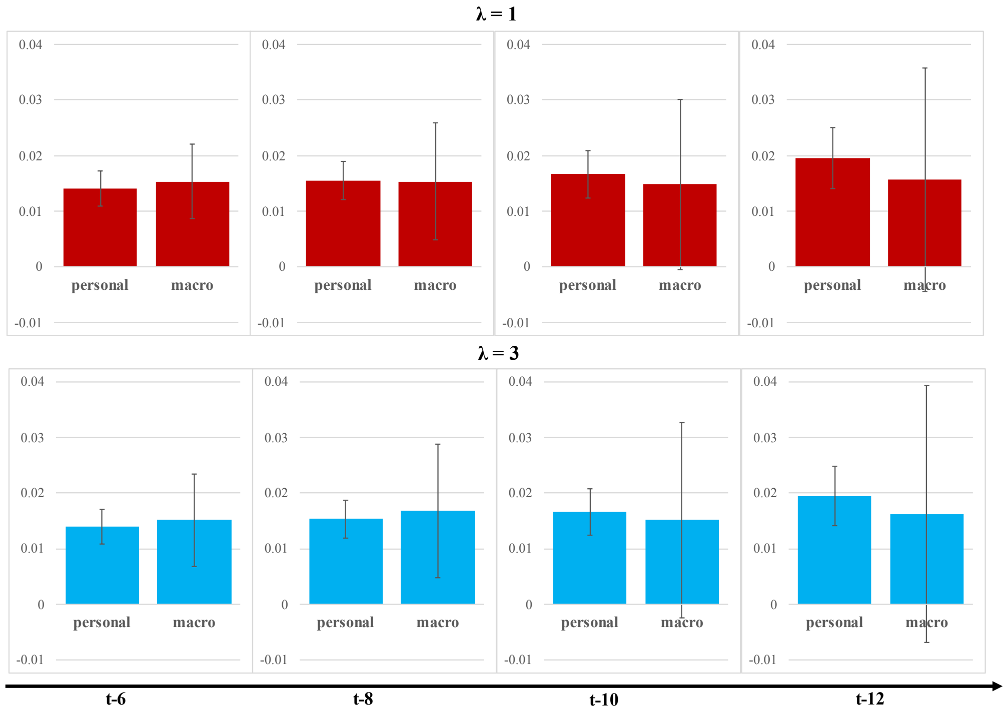
Table 5: **Experience Effects and Future Income Volatility**

	Dependent Variable: Variance of Income					
	(1)	(2)	(3)	(4)	(5)	(6)
	Permanent_{t+2}	Transitory_{t+2}	Permanent_{t+4}	Transitory_{t+4}	Permanent_{t+6}	Transitory_{t+6}
Experience (Personal), λ=1	0.224	0.189	-0.065	-0.177	0.492	-1.272
	(0.419)	(0.422)	(0.355)	(0.705)	(0.370)	(0.877)
Experience (Macro), λ=1	0.048	-0.014	-0.003	0.081	-0.019	0.032
	(0.053)	(0.062)	(0.056)	(0.075)	(0.065)	(0.106)
R-squared	0.335	0.446	0.337	0.410	0.374	0.419
Experience (Personal), λ=3	0.132	0.126	-0.022	-0.089	0.266	-0.738
	(0.236)	(0.248)	(0.210)	(0.402)	(0.215)	(0.537)
Experience (Macro), λ=3	0.030	-0.010	0.001	0.044	-0.011	0.017
	(0.030)	(0.036)	(0.032)	(0.042)	(0.037)	(0.059)
R-squared	0.335	0.446	0.337	0.410	0.374	0.419
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,967	24,127	12,966	17,672	9,212	13,494

Notes. The dependent variables are permanent and transitory income volatility in two, four, and six years, respectively. "Experience (Macro)" is the macroeconomic experience measure of unemployment, and "Experience (Personal)" is the personal experience measure. In the top panel, we use experience measures based on linearly declining weights (λ=1), and in the bottom panel, we use experience measures that shift more weight to recent observations (λ=3). Demographic controls include family size, heads' gender, race, marital status, education level, and a dummy variable indicating whether the respondent is unemployed at the time of the survey. Income controls include the first and second order of the logarithm of income and lagged income. Wealth controls include the first and second order of the logarithm of liquid and illiquid wealth. We take the logarithm of income and wealth; non-positive values are adjusted by adding the absolute value of the minimum plus 0.1 before being logarithmized. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

tion. We relate households' past unemployment experiences to their future wealth, using up to six survey waves (12 years) into the future. Note that this analysis also ameliorates potential concerns about the quality of the consumption data, as consumption does not enter the estimation, and about alternative life-cycle explanations, as a positive correlation of past unemployment experiences and future wealth directly contradicts unobserved illiquidity or other financial constraints as an explanation.

Figure 2: **Wealth Build-up: Effects of a One-Standard-Deviation Increase in Past Unemployment Experiences**



Notes. The upper four graphs (red bars) show the effects of a one-standard-deviation increase in past exposure to personal and macro-level unemployment on total wealth, constructed with linearly-declining weights ($\lambda = 1$). The bottom four graphs (blue bars) show the same effects, constructed with the $\lambda = 3$ weighting scheme, on total wealth. The four graphs in horizontal order show the estimated coefficients when we use 6-year lagged, 8-year lagged, 10-year lagged, and 12-year lagged experience measures respectively. Error bars show 90% confidence level.

Figure 2 summarizes graphically the coefficients of interest from eight regressions, predicting wealth at $t+6$, $t+8$, $t+10$, and $t+12$. The upper part shows the effects of a one-standard-deviation increase in experience on total wealth, constructed with the linearly declining ($\lambda = 1$) weighting scheme. The bottom part shows the same effects, with more weight shifted towards more recent periods ($\lambda = 3$). (The exact coefficient estimates and details of the estimation are in Appendix-Table A.12.) All coefficient estimates are positive and mostly statistically significant. The economic magnitude is large: a one-standard-deviation increase in personal experiences predicts additional wealth build-up of about 1.7% or $3,555 ten years later, using the $\lambda = 1$ weighting scheme, and a one-standard-deviation increase in macroeconomic experiences of unemployment predicts additional precautionary savings and resulting wealth build-up of about 1.5% or $3,159 ten years later. In other word, households who have experienced high unemployment tend to accumulate more wealth down the road.

Summarizing all four sets of results, we have found that past exposure to unemployment strongly predicts consumption expenditures and beliefs about future economic conditions. However, such beliefs do not seem consistent with actual income and wealth changes. In fact, we see evidence of a positive relationship between past experience and future wealth build-up.

IV Consumption Model with Experience Effects

Our four baseline results on expenditures, beliefs, future income, and wealth build-up are consistent with *experience effects* and the notion that past experiences can "scar" consumers, while they are hard to fully explain (jointly) in the traditional life-cycle consumption model. However, given the lack of exogenous, experimental variation in lifetime experiences, it is important to further explore potential confounds arising from unobserved determinants and frictions.

We consider a life-cycle consumption model that accounts for a broad array of standard life-cycle consumption factors and possible confounds, namely, the Low, Meghir, and Pistaferri (2010) model. Using this model, we perform a simulate-and-estimate exercise that shows that, for a wide range of parameterizations, we can distinguish experience effects even directionally from traditional life-cycle explanations as the negative relationship between past exposure to unemployment and consumption spending, which we saw in the data, does not emerge for rational learners (after controls). Our goal is not to implement a full structural estimation but to leverage the structural elements and richness of the model to investigate the mechanism of experience-based learning as well as possible confounds, thus providing guidance towards empirical robustness checks and additional tests.

The Low et al. model embeds the established determinants of consumption and includes interaction of different types of risk (productivity shocks, employment risk) with social insurance (unemployment insurance, food stamps, and disability insurance).[19] We simulate the model both for standard rational agents, as in the original model, and for agents whose beliefs are scarred by past unemployment experiences. We then estimate the relation between consumption spending and past unemployment experiences on the two sets of simulated data. The aim is to further disentangle the two types of channels we have accounted for in our empirical estimations: (a) a "rational" channel of lost income, lower wealth, and worse employment prospects, which predict lower consumption spending among Bayesian learners, and (b) an "experience-based channel," which affects only consumers whose beliefs are scarred by their past exposure to unemployment.

Model setup. Consumers in the Low et al. model can work for 40 years, until age 62 (starting at age 23), then have mandatory 10 years of retirement with social-security benefits, and die at the end of retirement. Periods are quarters, amounting to $L = 200$ periods of consumption and labor decisions. Their utility function is

$$U(c, P) = \frac{\left(c \times e^{\eta P}\right)^{1-\gamma}}{1-\gamma}, \tag{4}$$

where c is consumption, and P an indicator equal to 1 if a person works. In each t, consumer i chooses consumption $c_{i,t}$ and, when applicable, labor supply $P_{i,t}$ to maximize lifetime expected utility

$$\max_{\substack{c_{i,t} \\ P_{i,t}}} V_{i,t} = U(c_{i,t}, P_{i,t}) + \mathrm{E}_t \left[\sum_{s=t+1}^{L} \beta^{s-t} U(c_{i,s}, P_{i,s}) \right]. \tag{5}$$

We impose $c_{i,t} < A_{i,t}$, where $A_{i,t}$ is beginning-of-period-t assets and which rules out borrowing. As we will see below, by maximizing the financial constraints we are able to derive the sharpest distinction between the role of experience effects and financial constraints.[20] We assume that flow utility takes a near CRRA form, which induces

[19] It does not capture assets with different term structures and different levels of liquidity.

[20] The reason is that (unobserved) financial constraints are a potential confound of the empirical relation between prior experiences and consumption: Younger cohort tend to be more constrained *and* are predicted to react more strongly to a shock than older cohorts under the experience-effect hypothesis. By eliminating borrowing altogether, we maximize the impact of financial constraints.

a precautionary savings motive. (A detailed description of the intertemporal budget constraint and the social-insurance programs is in Appendix C.)

Income process. The wage in this model is determined by the following formula

$$\ln w_{i,t} = d_t + x'_{i,t}\psi + u_{i,t} + a_{i,j,t_0}, \tag{6}$$

where d_t is the log-price of human capital at time t, $x'_{i,t}\psi$ the component determined by i's age at time t, $u_{i,t}$ the stochastic component, and $a_{i,,j,t_0}$ the job-fit component of i's wage at firm j for a job offered (and accepted) in period t_0. Gross quarterly income is $w_{i,t}h$, where h is the number of hours worked in a quarter. The three social-insurance programs Low et al. include in their model are detailed in Appendix C.

Agents have the ability to make decisions about whether or not to work. For example, agents need not work if an offer is too low. They can also retire early. Note that this implies that experience-based learners may make different labor supply choices depending on their concern about future employment and desire to save.

The *deterministic component* of the wage $d_t + x'_{it}\psi$ is the same for all individuals of a given age at time t. Its size is estimated in Low et al. via the regression[21]

$$d_t + x'_{i,t}\psi = \alpha + \beta_1 \cdot \text{age} + \beta_2 \cdot \text{age}^2. \tag{7}$$

The *stochastic (permanent) component* of the wage $u_{i,t}$ is determined by a random walk. Consumers receive a shock to this component on average once a year. If consumer i has an income shock in period t, then $u_{i,t}$ is

$$u_{i,t} = u_{i,t-1} + \zeta_{i,t}, \tag{8}$$

where $\zeta_{i,t}$ is i. i. d. normal with mean 0 and variance σ^2_ζ.

A key element of the Low et al. model is its job-match process. The *job-match component* a_{i,j,t_0} is drawn from a normal distribution with mean 0 and variance σ^2_a. It is indexed by the period t_0 in which the consumer joined firm j, and not by t, since it is constant throughout the duration of the consumer-firm interaction.

In each period, the probability of job destruction is δ, the probability of a job offer is $(1-\delta)\lambda^e$ for an employed worker, and λ^n for an unemployed worker. Agents receive job offers with varying job matches. By construction, they accept all offers with a higher job match and reject all offers with a lower job match.

The job match component, in combination with the processes of job destruction

[21] While $x'_{i,t}$ includes a larger set of control variables in the empirical portion of Low et al., only age and age squared are used to fit a general lifetime income profile to the model.

and job generation, is at the core of the "income scarring" result of Low et al. (2010). While employed, people successively trade up for jobs that are a better match. They thus gain higher incomes over their life-cycle. In turn, if they experience job destruction, they lose their job match and must (re-)start getting better and better job offers. Hence, agents typically earn a lower income after an unemployment spell, and job loss leads to a long-lasting reduction in earnings. By accounting for rational "income scarring," we impose a high bar on our hypothesis. We test whether experience-based learners adjust their consumption beyond this bar.

Model extension (unemployment scarring). As a further step, we extend the model to allow for "unemployment scarring" in the sense that job loss itself may induce a negative, permanent wage shock. The motivation comes from research in labor economics that has found a persistent negative effect of being unemployed on future income, especially during a recession (Davis and Von Wachter 2011, Huckfeldt 2016, Jarosch 2015). While those findings might actually be evidence for experience effects, the existing literature proposes more traditional explanations. The model of "unemployment scars" in Jarosch (2015), for example, features a job-security component that resembles the "job-match component" of wages in Low et al., albeit with the difference that wage gains lost due to "income scarring" can be regained by working for an extended period.[22] The additional negative correlation between unemployment and future income resulting from "unemployment scars" could be an alternative explanation for the estimated experience effect.

To add "unemployment scarring" to our simulation, we reduce a consumer's permanent wage component every time she experiences job destruction by the average size of a permanent income shock, σ_ζ.

Belief formation. Both types of consumers, rational and experience-based learners, know the model, but differ in their beliefs about the probability of job loss δ. We denote consumer i's believed probability of job destruction at time t as $\delta^b_{i,t}$. Rational consumers use all available data on unemployment to update their beliefs. If they have lived long enough, they know (or closely approximate) the true value of δ, $\delta^b_{i,t} = \delta \; \forall t$. Experience-based learners form their belief based on the history of re-

[22] See, in contrast, the θ_y component of the firm-type vector in Section 2.1 of Jarosch (2015).

Table 6: **Key Simulation Parameters**

Parameter		Benchmark value(s)	
Preference parameters			
Relative risk aversion coefficient	ρ	1.5	
Interest rate	r	1.5%	
Discount factor	β	$1/(1+r)$	
Lifetime parameters			
Working years		40	
Retirement years		10	
Income process		High education	Low education
Standard deviation of job matches	σ_a	0.226	0.229
Standard deviation of permanent shocks	σ_ζ	0.095	0.106

alizations in their prior lives, lagging one period to be consistent with our empirical specification. Applying specification (1), with weighting scheme (2), we obtain

$$\delta_{i,t}^{b} = \sum_{k=2}^{t-1} w\left(\lambda, t, k\right) P_{i,t} D_{i,t-k}, \tag{9}$$

where $D_{i,t}$ is an indicator of i experiencing job destruction in t, and

$$w(\lambda, t, k) = \frac{\left(t-k\right)^{\lambda}}{\sum_{k=2}^{t-1} P_{i,t}\left(t-k\right)^{\lambda}}. \tag{10}$$

is the weight assigned to realizations D at k periods before period t.

Model estimates. We simulate the consumption-saving decisions for both rational and behavioral consumers using the parameters in Table 6. (The full list of parameters is in Appendix-Table C.1.) The values are identical to those in Low et al. (2010) whenever possible. Following Low et al., we distinguish between high- and low-education individuals by varying the corresponding parameters.

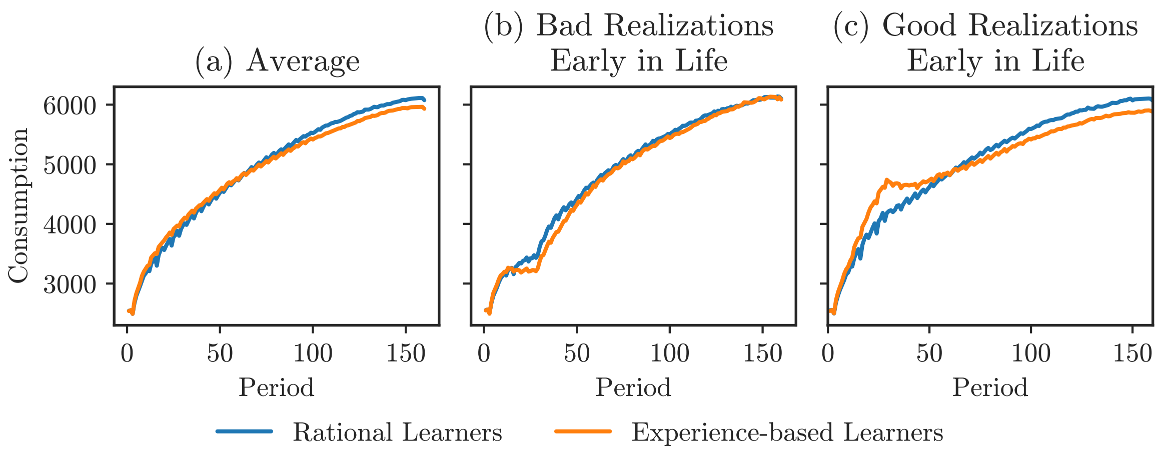
Figure 3 illustrates the resulting consumption paths for rational and experience-based learners in the low-education group under "income scarring" as in the original Low et al. model. Panel (a) depicts their average consumption during working years (the years used in the regressions). It provides a first hint as to the differences in consumption patterns. Relative to the darker (blue) plot of the rational benchmark, the lighter (orange) plot for experience-based learners indicates higher consumption earlier in life, and lower consumption later in life. That is, early in life, experience-based learners tend to underestimate the probability of job loss, spend more, and must then save more towards the end of their working life.

Panels (b) and (c) amplify the illustration of these differences by zooming in on consumers who were "unlucky" and "lucky" early in life in terms of their earnings.

Panel (b) considers the subset of consumers who had rather bad employment realizations early in life—to the extent that, by period 30 of the simulation, the experienced-based learners believe that their probability of job loss δ_i is 0.1 or greater, even though the true probability of job destruction is 0.049. Within this "unlucky" group, experience-based learners consistently consume less than their rational counterparts for almost the entire lifecycle. This plot illustrates the prediction of wealth build-up due to excess frugality, for which we found evidence in Section III.D.

Panel (c) illustrates the opposite scenario. Here, we consider the subset of consumers who had particularly good employment realizations early in life—so much so that, by period 30 of the simulation, experienced-based learners believe that their probability of job loss δ_i is 0.025 or less, while the true probability of job destruction is 0.049. For these experience-based consumers, the over-consumption early in life and financial constraints later is much more pronounced.

Figure 3: **Life-Cycle Consumption for Agents**



Notes. The plots show average consumption for rational learners and experience-based learners (with $\lambda = 1$) in the low-education group, based on 10,000 lifetime simulations for each type. In panel (b), the simulations are restricted to those where agents have, or in the rational case would have, a believed delta of 0.1 or greater at period 30, and in panel (c) to those where agents have, or in the rational case would have, a believed delta of 0.025 or less at period 30.

Table 7: **Estimations with Model-Simulated Data**

	Baseline Model (Income Scarring)				Extended Model (Income and Umempl. Scarring)			
	(1) Rational	(2) Rational	(3) EBL	(4) EBL	(5) Rational	(6) Rational	(7) EBL	(8) EBL
$\lambda = 1$:								
Income	0.569	0.383	0.604	0.396	0.417	0.287	0.465	0.348
	(224.41)	(64.91)	(197.99)	(56.76)	(85.82)	(85.82)	(116.24)	(68.09)
Wealth		0.264		0.265		0.315		0.261
		(51.67)		(58.79)		(55.26)		(31.48)
Unempl. Exp.	0.350	0.692	-0.071	-0.466	-0.293	0.419	-1.522	-1.733
	(9.56)	(5.21)	(-1.70)	(-6.43)	(-4.47)	(5.44)	(-17.46)	(-21.81)
$\lambda = 3$:								
Income	0.578	0.387	0.619	0.400	0.423	0.288	0.484	0.358
	(212.06)	(62.44)	(163.54)	(53.40)	(94.37)	(124.22)	(129.11)	(63.42)
Wealth		0.261		0.271		0.310		0.267
		(52.44)		(66.27)		(57.67)		(36.27)
Unempl. Exp.	0.565	0.575	0.133	-0.274	0.129	0.330	-1.197	-1.427
	(8.50)	(5.80)	(6.49)	(-6.62)	(9.26)	(6.39)	(-20.64)	(-27.83)

Notes. Estimations with simulated consumption as the dependent variable and simulated same-period income and wealth as well as past unemployment experiences as regressors, for rational consumers in columns (1)-(2) and (5)-(6) and experience-based learning (EBL) consumers in columns (3)-(4) and (7)-(8). Simulations account for income scarring and, in columns (5)-(8), for unemployment scarring. Estimations are for $\lambda = 1$ in the top panel and $\lambda = 3$ in the bottom panel. Consumption, income, and wealth are in log terms. All estimations include period and education fixed effects and use period-clustered standard errors. Simulations are based on the working periods of 10,000 simulated consumers and thus 1,600,000 observations. t-statistics in parentheses.

Using the simulated values, we estimate the relationship between consumers' past unemployment experiences and consumption behavior, controlling for income and wealth and including education and time fixed effects. The corresponding OLS regressions are in Table 7. We use the simulated data from the baseline model (with income scarring) in columns (1)-(4), and from the extended model (with income and unemployment scarring) in columns (5)-(8), separately for Bayesian learners and for experience-based learners. Note that, in the case of Bayesian agents, prior experiences do not actually enter their belief formation. The purpose of including the experience measure in the regression is to detect possible confounds of the significantly negative effect we have estimated in the PSID data. One such possible confound is unobserved wealth. In order to probe the role of unobserved wealth effects, we estimate the model first without wealth as a control (columns 1, 3, 5, 7) and then with wealth (columns 2, 4, 6, 8). We include the experience-effect proxy in both cases to test whether it might pick up unobserved wealth effects. We conduct the estimations both on the data simulated with linearly declining weights on experiences (λ=1), shown in the upper table, and on the data simulated wtih recency-shifted weights (λ=3), shown in the lower half.

Baseline model (with income scarring). Starting from the baseline estimation with linearly declining weights, we find that income has the expected positive sign and is significant, as does wealth when it is included. In the simulations with rational agents (columns 1 and 2), we estimate a *positive* coefficient of the experience measure. This is the opposite of what we find empirically, and a first step to addressing confounds: It appears to be hard to (falsely) estimate a negative experience effect if agents are rational, whether or not we include perfect wealth controls.

When we alter the belief-formation process to experience-based learning, we estimate a significant *negative* coefficient, both with and without wealth control (columns 3 and 4). That is, lifetime experiences strongly predict consumption behavior of experience-based learners, after taking into account their income and wealth. Compared to the results obtained empirically, the coefficients on unemployment experience in columns 3 and 4 are greater in magnitude, which may be attributed to the lack of other control variables in the simulation exercises.

The contrast between the positive experience-effect estimates for rational agents, and the negative estimates for experience-based learners is helpful in that it addresses concerns about possible confounds in our empirical estimates. But the positive sign in the data simulated for rational agents seems to contradict the basic intuition of "income scarring" that job destruction lowers lifetime income, and thus consumption. Why do higher unemployment experiences predict *higher* spending of rational agents?

To understand this result, consider two consumers, A and B, with the same income. A has experienced unexpected job loss in the past, while B has not. All else held equal, "income scarring" predicts that A earns less. However, by assumption, A and B have the same income, implying that A's wage is driven by her permanent-income component rather than her job-match component. As a result, A is less worried about unexpected job destruction and rationally consumes more. In other words, if one introduces a proxy for experience effects into a world with rational agents, it can act as a proxy for the permanent-income component when predicting consumption controlling for current income.

The estimation result, then, clarifies two things. First, there is little concern about confounding experience effects with traditional life-cycle determinants, including (unobserved) wealth effects and income scarring. The model exercise is helpful because it implies the reverse result in the absence of experience-based learning. Second, the income and wealth controls are, in fact, "overcontrolling" for the rational channels through which past unemployment might affect current consumption among Bayesian learner (lost income, lower wealth, worse employment prospects).

The results are similar when we put higher weights on consumers' recent experiences ($\lambda = 3$), as shown in the bottom panel of Table 7. For rational consumers, we continue to estimate a positive coefficient on unemployment experiences, contrasting our empirical findings. For experience-based learners, instead, we estimate a negative coefficient in the specification that also controls for wealth. The one difference to the estimations with $\lambda = 1$ is the positive coefficient in column (3). It indicates that, even if consumers *are* scarred by past experiences, one might fail to detect experience effects empirically when not properly controlling for wealth. The reason is the same as for rational learners: If a person has experienced unemployment but earns the same income as another person without such a personal unemployment history, it suggests a high permanent component. This effect can override experience-based learning when the recency bias is high (high λ), at least when wealth is not controlled for. Empirically, we do estimate a negative coefficient also when allowing for high recency bias, as shown in the previous section. This implies that, if anything, the true experience effect might be stronger under perfect wealth controls.

Extended model (with income and unemployment scarring). In columns (5)-(8), we display the estimation results for the extended model, that allows for additional negative correlation between unemployment and future income ("unemployment scarring"). The specification with rational learners and without wealth controls (column 5) shows a negative correlation between unemployment experience and consumption for λ=1, which becomes positive for λ=3. Once we control for wealth (column 6) the experience coefficient is positive for both λ=1 and λ=3. For simulations with experience-based learners, instead, all four estimates are significantly negative. That is, in contrast to Table 7, we now estimate a negative experience effect for behavioral learners whether or not we control for wealth.

Note that, compared to columns (1)-(4), the size of the coefficients becomes (mechanically) lower. Intuitively, the experience measure still acts as an indirect proxy for a high permanent component, but now for a subgroup where the permanent component has been systematically reduced compared to the baseline model. That is, observing two people A and B with the same income today, where only A has experienced unemployment, still suggests that A has a higher permanent component. However, A's distribution of the permanent component will be shifted down by one-standard-deviation due to the "unemployment scarring".

Taken together, the results from the simulate-and-estimate exercises show that financial constraints, unobserved wealth, income scarring, and unemployment scarring fail to generate a negative relation between past unemployment experiences and current consumption when agents are Bayesian learners.[23] Thus, the negative coefficient likely indicates scarring effects, as we show using empirically validated parameterizations of experience effects. Only if we fail to appropriately control for wealth effects *and* allow for only weak recency bias *and* introduce fairly potent unemployment scarring, a confound materializes. Since all of our estimations explore the results for a high λ parameter and control for wealth, and since we found no relation between unemployment experiences in the past and future incomes (after controls), this scenario is unlikely to apply. Still, to address remaining concerns, we conduct robustness checks with a variety of alternative wealth specifications, including varying proxies for liquid versus illiquid wealth, higher-order terms, decile dummies, separate dummies for housing wealth or for positive wealth versus debt, and, for completeness, a similar battery of variations of the income controls.

V Model Validation

We start from concerns about imperfect measurement of wealth. Our simulate-and-estimate exercise in Section IV alleviates these concerns, as it appears hard to generate misattribution when employing empirically validated experience proxies and

[23] In addition to illustrating the influence of past experiences on consumption, we also apply the model to study the influence on future income, as described in Appendix C.5.

controlling for unemployment status and income – even in the presence of mismeasurement. Moreover, our prior results on future wealth build up, future income, and beliefs are hard to reconcile with the unobserved-wealth interpretation. Nevertheless, we use a battery of alternative wealth measures, which we include in addition to the first- and second-order liquid- and illiquid-wealth controls that are already included in Table 2. Specifically, we include (1) third- and fourth-order controls of (log) illiquid and illiquid wealth, (2) wealth decile dummies, separately for liquid and illiquid wealth, (3) log home equity value (home price minus mortgage) and log non-housing wealth, and (4) log total debt and log positive wealth separately. The detailed results for each of these estimations are in Appendix-Table A.8. We display all coefficients of interest graphically in the top panel of Appendix-Figure A.2. Robustness 1 to 4 refer to variations (1)-(4) above, and the bars shown below "personal" and "macro" refer to the estimated effects of personal and macroeconomic experiences, expressed as the implied economic magnitudes of a one-standard-deviation increase in past exposure to personal and macroeconomic unemployment. As the figure indicates, all estimates of interest remain very similar, both in size and in statistical significance.

A related concern is measurement error in the income variable. As with wealth, we re-estimate our empirical model using varying constructs of income: (1) third and fourth order of (log) income and lagged income, (2) quintile dummies of income and lagged income, (3) decile dummies of income and lagged income, and (4) controls the bottom 2, 2^{nd}-4^{th}, 4^{th}-6^{th}, 6^{th}-8^{th}, 8^{th}-10^{th}, 90^{th}-92^{nd}, 92^{nd}-94^{th}, 94^{th}-96^{th}, 96^{th}-98^{th}, and top 2 percentile dummies of income and lagged income. All estimates, shown in Appendix-Table A.9, are again similar in both magnitude and significance.

Next, we address the concern of measurement error in the income variable following Cogley, Sargent, and Surico (2015) and Romer (1986). Implementing their methodology, we directly estimate the extent of measurement error, incorporate the estimates into the income variable, and assess whether they affect our results. Specifically, we apply the estimates of the share of variance associated with measurement error in income from Bound, Brown, Duncan, and Rodgers (1994), who investigates properties of measurement error in earnings using a validation study for the PSID. While the validation study covers only a small fraction of the PSID sample, they extrapolate their findings to estimate the share of measurement errors in representative samples. We adopt their estimates for the share of measurement error in log earnings

$var(\epsilon^y) = 0.04var(y)$, and re-estimate our empirical model using the measurement-error-adjusted income. The results are shown in Appendix-Table A.10. They show that the estimates on personal and macroeconomic experience not only are similar in direction and significance to the baseline results but also increase in magnitude.

A more specific concern is that liquidity might be correlated with past exposure to unemployment, generating lower spending in response to bad experiences and higher spending in response to good experiences. Our separate controls for liquid and illiquid wealth, both in Table 2 and in the robustness checks in Appendix-Table A.8 (columns 2 and 6) ameliorate these concerns. As a further step, we test whether the consumption of households that are disproportionately likely to be liquidity constrained is more affected by their unemployment experience. Following prior consumption literature (cf. Johnson, Parker, and Souleles (2006); Parker, Souleles, Johnson, and McClelland (2013)), we sort households into two groups for each year, based on whether their liquid wealth lies above or below the sample median. Expanding equation (3), we interact an indicator for being below the median and the experience variables. As shown in Appendix-Table A.11, households in the bottom half of the liquid-wealth group tend to spend less relative to households in the top half. However, their consumption expenditure does not react more strongly to unemployment experience. All the coefficients are either insignificant or point in the opposite direction. This suggests that the negative effect of unemployment experiences on consumption is not explained by liquidity constraints.

Building on the robust estimation results, we also study two further implications of experience effects in Appendix B, regarding the quality of consumption and age heterogeneity.[24]

[24] One prediction we did not pursue regards the hours worked. In general, experience-based scarring implies a positive relation between past unemployment experience and the likelihood of working because work generates a greater income buffer. (Note that "work" is a binary decision in the Low et al. (2010) model.) However, this prediction does not hold if income or unemployment scarring is strong. In that case, the cost of working dominates the gain, and consumers are more likely to choose living off social welfare programs instead of working.

VI Conclusion

A better understanding of the long-term effects of economic shocks has proven to be of considerable importance, whether we consider the COVID-19 induced recession in 2020, the lingering effects of the Great Recession in 2008, or the Great Depression of the 1930s. In this paper, we have put forward the idea that past exposure to unemployment shocks play a significant role in shaping household attitudes towards consumption, and thereby generate long-term consequences for consumption choices.

Out estimation results show that households who have experienced times of higher local and national unemployment and more personal unemployment spend significantly less, after controlling for income, wealth, and demographics. We further show that consumers become more pessimistic, but that such beliefs do not seem to be consistent with actual future income and wealth changes. In fact, we see evidence of a positive relationship between past experience and future wealth build-up.

Consumption scarring effects constitute a novel micro-foundation underlying fluctuations in aggregate demand and long-run effects of macroeconomic shocks. For future research, our empirical methodology could be applied to a larger cross-section of countries, particularly countries that have undergone more drastic and volatile macroeconomic events such as the emerging market countries and some European countries. Such exercises would help to determine the extent to which past exposure to (good or bad) economic conditions exerts a lasting influence on household consumption—the key ingredient in all macro and macro-finance frameworks.

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Scarred Consumption

Ulrike Malmendier and Leslie Sheng Shen

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Appendix A Empirical Analysis

A.1 Robustness using PSID Data

We present a series of robustness tests of the estimations relating unemployment experiences to consumption, as well as of the estimations of the wealth build-up.

In Appendix-Figure A.1, we replicate the empirical exercise proposed in the job displacement literature, including Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010), which estimates income loss around displacement. It plots the coefficients δ_k from the regression $y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} D^k_{it}\delta_k + x_{it}\beta + \epsilon_{it}$, where y_{it} denotes earning of worker i in year t, D^k_{it} denotes dummy variables that take the value 1 if displacement occurred k years following the event and 0 otherwise; x_{it} denotes a set of controls including gender, martial status, race, education, and age; α_i denotes worker dummies; and γ_t denotes year dummies. The coefficients δ_k show the effect of displacement on a worker's earnings k years following its occurrence.

Our results show a persistent effect of displacement on earnings, which echoes the findings in the prior literature and supports the quality of our data on income. Our analyses differentiate experience effects from these known earnings implications of job loss in two ways: First, we control for earnings in the recent past. Second, we focus on the effects of unemployment experiences farther in the past, as we construct all measures of past experiences such that those from the recent past are excluded.

Appendix-Table A.1 presents the summary statistics of the full sample, i. e., including observations with total family income below the 10^{th} or above the 90^{th} percentile in each wave from 1999 to 2017, as well as the pre-sample 1997 wave (because we control for lagged income). Otherwise, we apply the same restrictions as in the construction of the main sample, namely, drop individuals for whom we cannot construct the experience measures (due to missing information about location or employment status in any year from t to $t-5$) and observations with missing demographic controls or that only appear once. The resulting sample has 42,167 observations, compared to 33,263 observations in the main sample. The sample statistics are very similar, with a mean personal experience of 6.21% and 6.23% based on weights of $\lambda = 1$ and $\lambda = 3$, respectively, a mean macroeconomic experience of 6.07% and 6.00% based on weights of $\lambda = 1$ and $\lambda = 3$, respectively, and average household total consumption of $38,904 (in 2017 dollars).

In Appendix-Table A.2, we re-estimate the regression model of Table 2 on the full sample. The results become even stronger. The estimated macroeconomic experience and personal experience effects are both larger and more significant than those estimated in Table 2.

In Appendix-Table A.3, we construct alternative scarring measures for the gap years (between the PSID biennial surveys). For the macroeconomic measure in the main text, we fill in the unemployment rate in a gap year t by assuming that the family lived in the same state as in year $t-1$. Here, we assume that respondents spend half of year t in the state in which they lived in year $t-1$, and the other half in the state in which they lived in year $t+1$. (This alternate construction does not change the value if respondents live in the same state in $t-1$ and $t+1$.) Similarly, for personal unemployment, we reconstruct respondents' employment status in year t as the average of their status in years $t-1$ and $t+1$, rather than applying the value from year $t-1$. For example, if a person is unemployed in $t-1$ and is employed in $t+1$, the personal experience in t will be denoted as 0.5. Re-estimating the model from equation (3), we find results very similar to those in Table 2 in the main text.

In Appendix-Table A.4, we present an alternative scarring measure that incorporates the unemployment experiences of the spouses. For married households, we use the average of the household heads' and spouses' past exposure to unemployment, controlling for a married-couples indicator. All other variables are defined as in Table 2. The coefficients of interest remain very stable.

Appendix-Table A.5 shows the results for different clustering units. Instead of clustering by cohort as in Table 2, we two-way cluster the standard errors by cohort and year (columns 1 and 3) and cluster by household (columns 2 and 4). In columns (1) to (2), we use experience measures based on linearly declining weights (λ=1), and in columns (3) to (4), we use experience measures that shift more weight to recent observations (λ=3). The statistical significance of our results are not affected in most cases.

In Appendix-Table A.6, we apply the PSID longitudinal family weights. Note that some families are given zero weight and are thus dropped from the estimation, which explains the lower number of observations in the weighted regressions. The results remain similar to the baseline results in Table 2 in direction and significance, though the magnitude of the coefficients are smaller.

In Appendix-Table A.7, we estimate an alternative version of the empirical model in equation (3) that includes a lagged consumption measure on the right-hand side, to take into account possible habit persistence in consumption. This dynamic specification, with the lagged dependent variable included, requires a correction for the correlation between the lagged dependent variable and the fixed effects in the error term, which gives rise to "dynamic panel bias" (Nickell 1981). To obtain unbiased and consistent coefficients, we estimate the specification using a dynamic GMM panel estimator, following Holtz-Eakin, Newey, and Rosen (1988), Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998). More details about the estimation are provided in Section III.B. The results show that the effects of prior unemployment experience on consumption remain mostly significant after taking into account possible habit persistence in consumption. The estimation results both confirm the robustness of experience effects and indicate that they do not operate through the channel of habit formation.

Appendix-Tables A.8, A.9, A.10 and A.11 address concerns about unobserved wealth, liquidity, or income components.

Appendix-Table A.8 presents results from estimations using alternative wealth controls, in addition to the measures of liquid and illiquid wealth in Table 2: third- and fourth-order liquid and illiquid wealth (columns 1 and 5); decile dummies of liquid and illiquid wealth (columns 2 and 6); housing wealth and other wealth (columns

3 and 7); positive wealth and debt (columns 4 and 8). In columns (1) to (4), we use experience measures based on linearly declining weights (λ=1), and in columns (5) to (8), we use experience measures that shift more weight to recent observations (λ=3). The coefficients of interest remain stable and are statistically significant.

Appendix-Table A.9 uses alternative income controls, in addition to the first- and second-order controls for income and lagged income: third- and fourth-order income and lagged income (columns 1 and 5); quintile dummies of income and lagged income (columns 2 and 6); decile dummies of income and lagged income (columns 3 and 7); bottom-2, 2^{nd}- 4^{th}, 4^{th}- 6^{th}, 6^{th}- 8^{th}, 8^{th}- 10^{th}, 90^{th}- 92^{nd}, 92^{nd}- 94^{th}, 94^{th}- 96^{th}, 96^{th}- 98^{th}, and top-2 percentile dummies of income and lagged income (columns 4 and 8). In columns (1) to (4), we use experience measures based on linearly declining weights (λ=1), and in columns (5) to (8), we use experience measures that shift more weight to recent observations (λ=3). The coefficients of interest remain stable. All of the estimates that were significantly negative before are still significant.

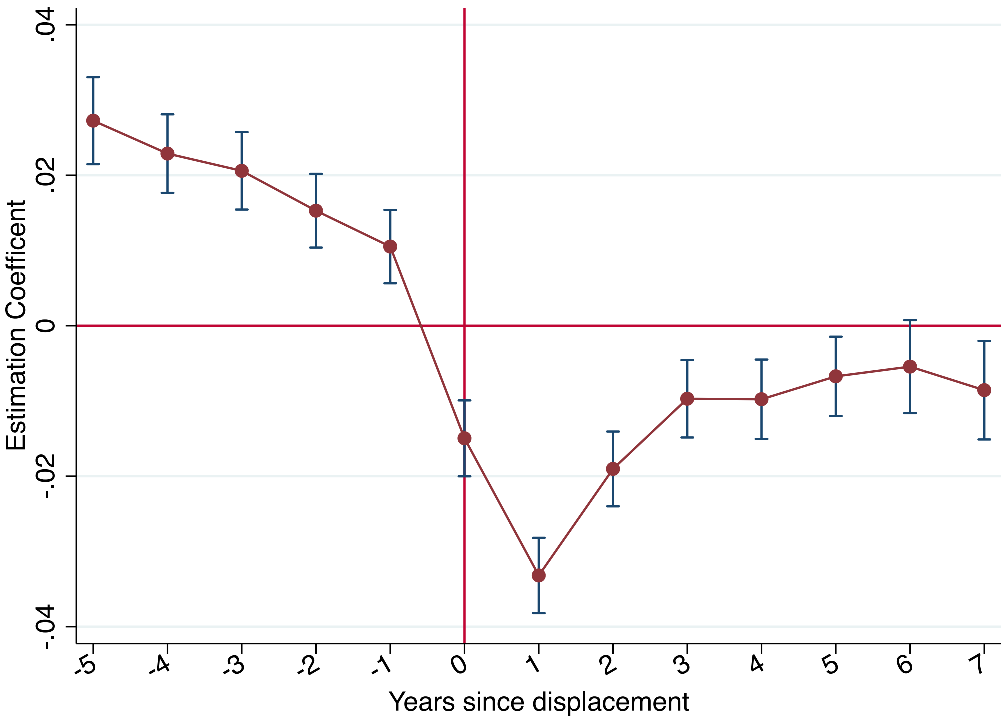
Table A.10 addresses the concern about measurement error in the income variable by incorporating estimates of the extent of measurement error into the income variable and assessing whether they affect our results, following the methodology in Romer (1986) and Cogley, Sargent, and Surico (2015). We apply the estimates of Bound, Brown, Duncan, and Rodgers (1994) on the share of variance associated with measurement error using a validation study for the PSID. While the validation study they use covers only a small fraction of the PSID sample, they extrapolate their findings to estimate the share of measurement errors in representative samples. We adopt their estimates for the share of measurement error in log earnings $var(\epsilon^y) = 0.04var(y)$. The results using the measurement-error-adjusted income are shown in Table A.10. They show that the coefficients of interest not only are similar in direction and significance but also increase in magnitude.

In Table (A.11), we test whether households that are more liquidity constrained are more affected by their unemployment experience. Closely following the practice in the consumption literature such as Johnson, Parker, and Souleles (2006) and Parker, Souleles, Johnson, and McClelland (2013), we sort households into two groups based on whether their liquid wealth is above or below the sample median in the respective year. We then add an indicator for below-median liquid wealth as well as its interactions with the experience variables to the estimating equation (3). As Appendix-Table A.11 shows, households in the bottom half of liquid wealth do not exhibit stronger reactions to unemployment experience. This suggests households' experiences affect consumption beyond potential liquidity constraints.

In Appendix-Table A.12, we study the effects of lifetime exposure to unemployment on wealth accumulation. This analysis tests whether, given the significant impact of unemployment experiences on consumption, we can also detect a role of unemployement experience effects on the build-up of wealth. The dependent variable is total wealth, and the main regressors are lagged experience measures. We lag the experience measures by six, eight, ten, and twelve years, instead of using the contemporary experience measures, recognizing that the effects of experience on wealth may take time to realize. We include the same set of control variables as in our main analyses, including controls for total wealth in the corresponding lagged year and income in years $t-1$ and $t-2$ while adding a control for the average family income between year $t-2$ and the year in which the lagged experience measures are based on (six, eight, ten, and twelve years ago, respectively). For example, when six-year lagged experience is the main regressor, we control for the average income between $t-2$ and $t-6$. This average-income control addresses the concern that previous experiences of economic booms or crises may have implications for future income (Oyer 2008; Kahn 2010; Oreopoulos, von Wachter, and Heisz 2012).[25] We find a significant role of past experiences for the build-up of wealth.

[25] The results are similar if, instead of having an average-income control, we include the incomes for all years between year $t-2$ and the year in which the lagged experience measures are based on.

Figure A.1: **Earnings Around Displacement**



Notes. The figure plots the coefficients δ_k from the regression $y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} D_{it}^k \delta_k + x_{it}\beta + \epsilon_{it}$, where y_{it} denotes earning of worker i in year t, D_{it}^k denotes dummy variables that take the value 1 if displacement occurred k years following the event and 0 otherwise, x_{it} denotes a set of controls including gender, martial status, race, education, and age, α_i denotes worker dummies, and γ_t denotes year dummies. The coefficients δ_k show the effect of displacement on a worker's earnings k years following its occurrence. Data source: PSID.

Table A.1: **Summary Statistics (PSID), Full Sample**

Variable	Mean	SD	p10	p50	p90	N
Age	49.63	11.40	35	49	66	42,167
Household Size	2.70	1.45	1	2	5	42,167
Household Total Consumption [$]	38,904	30,641	11,907	32,878	70,840	42,167
Household Total Income [$]	78k	105k	14k	58k	151k	42,167
Household Liquid Wealth [$]	53k	587k	-21k	0.6k	100k	42,167
Household Illiquid Wealth [$]	250k	1,077k	0k	65k	540k	42,167
Household Total Wealth [$]	303k	1,300k	-2k	65k	671k	42,167
Experience (Personal), λ=1 [%]	6.21	3.88	4.41	4.96	11.08	42,167
Experience (Personal), λ=3 [%]	6.23	6.81	3.06	3.98	15.19	42,167
Experience (Macro), λ=1 [%]	6.07	0.29	5.73	6.04	6.47	42,167
Experience (Macro), λ=3 [%]	6.00	0.55	5.35	5.94	6.76	42,167

Notes. Summary statistics for the estimation sample, which covers the 1999-2017 PSID waves, as well as the pre-sample 1997 wave (because we control for lagged income). Age, Experience (Personal), and Experience (Macro) are calculated for the heads of households. Household Total Income includes transfers and taxable income of all household members from the last year. Liquid and Illiquid Wealth are defined following Kaplan, Violante, and Weidner (2014). Values are in 2017 dollars (using the PCE), annual, and not weighted.

Table A.2: **Experience Effects and Consumption (PSID), Full Sample**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.687***		-0.684***	-0.422***		-0.419***
	(0.194)		(0.194)	(0.113)		(0.112)
Experience (Macro)		-0.073**	-0.071**		-0.048***	-0.047***
		(0.029)	(0.029)		(0.017)	(0.017)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	42,167	42,167	42,167	42,167	42,167	42,167
R-squared	0.776	0.775	0.776	0.776	0.775	0.776

Notes. The consumption variables come from the 1999-2017 PSID Consumption Expenditure Data package. We include all observations (i.e., also observations with total family income below the 10^{th} or above the 90^{th} percentile in each wave from 1999 to 2017), as well as the pre-sample 1997 wave (because we control for lagged income). We take the logarithm of consumption, income, and wealth; non-positive values are adjusted by adding the absolute value of the minimum plus 0.1 before being logarithmized. "Experience (Personal)" is the personal experience measure of unemployment and "Experience (Macro)" is the macroeconomic experience measure, as defined in the text. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). Demographic controls include family size, heads' gender, race, marital status, education level, and a dummy variable indicating whether the respondent is unemployed at the time of the survey. Income controls include the first and second order of the logarithm of income and lagged income. Wealth controls include the first and second order of the logarithm of liquid and illiquid wealth. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.3: **Consumption (PSID), Alternative Experience Measure: Gap Years**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.413***		-0.406***	-0.247***		-0.242***
	(0.123)		(0.124)	(0.069)		(0.070)
Experience (Macro)		-0.057***	-0.056***		-0.034***	-0.033***
		(0.018)	(0.018)		(0.011)	(0.011)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	33,263	33,263	33,263	33,263	33,263	33,263
R-squared	0.771	0.771	0.771	0.771	0.771	0.771

Notes. All variables other than the experience measures are defined as in Table 2. The construction of the experience measures differs as follows: For any gap year t (between PSID survey waves in $t-1$ and $t+1$), the baseline experience measures in the main text assume that families reside in the same state as in year $t-1$. The alternative construction used in this Appendix-Table assumes that families reside half of year t in their (t-1)-state of residence, and half of the year in their (t+1)-state of residence. (The different assumption does not matter when a family does not move between surveys.) Hence, the macro experience measure in this Appendix-Table uses the average of the year t unemployment rates of the (t-1)-state of residence and the (t+1)-state residence as gap year t's unemployment rate. Similarly, for the personal experience measure, we fill in the employment status of a household head in a gap year with the average of the years before and after. For example, if a person is unemployed in $t-1$ and is employed in $t+1$, then his personal experience in year t is denoted as 0.5. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.4: **Consumption (PSID), Alternative Experience Measure: Spousal Experience**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.414***		-0.421***	-0.255***		-0.257***
	(0.098)		(0.098)	(0.058)		(0.058)
Experience (Macro)		-0.050***	-0.047***		-0.033***	-0.030***
		(0.017)	(0.018)		(0.010)	(0.010)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	39,085	39,589	38,737	39,085	39,589	38,737
R-squared	0.763	0.764	0.764	0.763	0.764	0.764

Notes. All variables other than the couple indicator and experience measures are defined as in Table 2. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). Couple is an indicator equal to 1 for households who are married and is now included as a demographic control. The experience measures for the married households are constructed using an average of the household's head and the spouse. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.5: **Consumption (PSID), Alternative Clustering Units**

	(1)	(2)	(3)	(4)
Experience (Personal)	-0.275*	-0.275***	-0.169**	-0.169***
	(0.126)	(0.103)	(0.073)	(0.059)
Experience (Macro)	-0.057***	-0.057***	-0.034***	-0.034***
	(0.016)	(0.018)	(0.009)	(0.010)
Demographic controls	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=3	λ=3
Clustering Unit	Cohort&Year	HH	Cohort&Year	HH
Observations	33,263	33,263	33,263	33,263
R-squared	0.771	0.771	0.771	0.771

Notes. All variables are defined as in Table 2. In columns (1) to (2), we use experience measures based on linearly declining weights (λ=1), and in columns (3) to (4), we use experience measures that shift more weight to recent observations (λ=3). Standard errors are clustered by cohort and year (two-way clustering) in columns (1) and (3) and by household in columns (2) and (4). Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.6: **Consumption (PSID), Alternative Weights: PSID Weights**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.281**		-0.276**	-0.173***		-0.170**
	(0.113)		(0.114)	(0.065)		(0.065)
Experience (Macro)		-0.057***	-0.056***	-0.034***	-0.033***	
		(0.018)	(0.018)	(0.011)	(0.011)	
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	33,034	33,034	33,034	33,034	33,034	33,034
R-squared	0.770	0.770	0.770	0.770	0.770	0.770

Notes. All variables are defined as in Table 2, but observations are now weighted by the PSID family weights. The family with zero weights are dropped. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.7: **Experience Effects and Consumption, GMM regressions**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.476***		-0.459***	-0.275***		-0.185***
	(0.092)		(0.091)	(0.052)		(0.071)
Experience (Macro)		-0.013	-0.015		-0.018	-0.019**
		(0.017)	(0.017)		(0.011)	(0.009)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	25,464	25,464	25,464	25,464	25,464	25,464
R-squared	0.768	0.768	0.766	0.767	0.638	0.729

Notes. System GMM regressions with total consumption (in logarithm) as the dependent variable and lagged dependent variable as a regressor. All other variables are defined as in Table 2. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). Robust standard errors in parentheses are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.8: **Consumption (PSID), Additional Wealth Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Experience (Personal)	-0.273**	-0.255**	-0.274**	-0.166*	-0.158**	-0.149**	-0.159**	-0.106*
	(0.114)	(0.110)	(0.114)	(0.097)	(0.063)	(0.060)	(0.063)	(0.056)
Experience (Macro)	-0.056***	-0.053***	-0.058***	-0.049**	-0.031***	-0.031***	-0.032***	-0.030***
	(0.018)	(0.018)	(0.018)	(0.019)	(0.011)	(0.011)	(0.011)	(0.011)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3	λ=3
Observations	33,263	33,263	33,263	31,214	31,214	31,214	31,214	31,214
R-squared	0.771	0.774	0.771	0.789	0.775	0.778	0.775	0.789

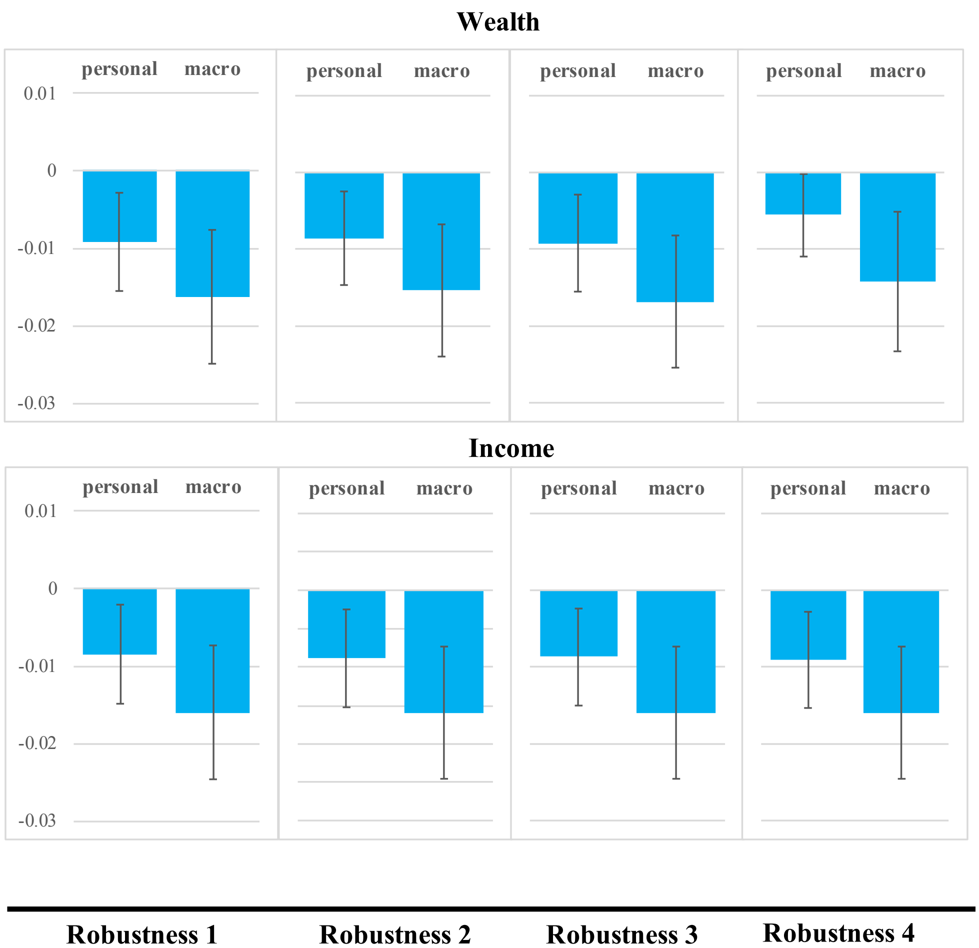
Notes. Regressions differ from those in Table 2 only in terms of the wealth controls. In columns (1) to (4), we use experience measures based on linearly declining weights (λ=1), and in columns (5) to (8), we use experience measures that shift more weight to recent observations (λ=3). Columns (1) and (5) control for third- and fourth-order liquid and illiquid wealth. Columns (2) and (6) include decile dummies of liquid wealth and illiquid wealth. Columns (3) and (7) control for housing wealth and other wealth (total wealth minus housing wealth). Columns (4) and (8) control for positive wealth and debt. All wealth controls are in addition to the controls of first- and second-order of liquid and illiquid wealth. Robust standard errors are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.9: **Consumption (PSID), Additional Income Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Experience (Personal)	-0.252**	-0.264**	-0.259**	-0.270**	-0.156**	-0.163**	-0.160**	-0.166**
	(0.115)	(0.115)	(0.114)	(0.113)	(0.066)	(0.066)	(0.065)	(0.065)
Experience (Macro)	-0.055***	-0.056***	-0.055***	-0.055***	-0.033***	-0.033***	-0.033***	-0.033***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.011)	(0.011)	(0.011)	(0.011)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3	λ=3
Observations	33,263	33,263	33,263	33,263	33,263	33,263	33,263	33,263
R-squared	0.771	0.771	0.771	0.771	0.771	0.771	0.771	0.771

Notes. Regressions differ from those in Table 2 only in terms of the income controls. In columns (1) to (4), we use experience measures based on linearly declining weights (λ=1), and in columns (5) to (8), we use experience measures that shift more weight to recent observations (λ=3). Columns (1) and (5) control for third- and fourth-order of income and lagged income. Columns (2) and (6) include quintile dummies of income and lagged income. Columns (3) and (7) include decile dummies of income and lagged income. Columns (4) and (8) include separately for the bottom-2, $2^{nd}-4^{th}$, $4^{th}-6^{th}$, $6^{th}-8^{th}$, $8^{th}-10^{th}$, $90^{th}-92^{nd}$, $92^{nd}-94^{th}$, $94^{th}-96^{th}$, $96^{th}-98^{th}$, and top-2 percentile dummies of income and lagged income. All income controls are in addition to the controls of first- and second-order of income and lagged income. Robust standard errors are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Figure A.2: **Wealth and Income Controls: Effects of a One-Standard-Deviation Increase in Experience**



Notes. The top panel show the effects of a one-standard-deviation increase in unemployment experience (constructed using λ=1 weighting) on total consumption when we include four alternative wealth controls: (1) third- and fourth-order liquid and illiquid wealth, (2) decile dummies for liquid wealth and illiquid wealth, (3) housing wealth and other wealth (total wealth minus housing wealth), and (4) positive wealth and debt. All wealth controls are in addition to first- and second-order liquid and illiquid wealth. The bottom panel show the effects of a one-standard-deviation increase in experience (constructed using λ=1 weighting) on total consumption when we include four alternative income controls: (1) third- and fourth-order income and lagged income, (2) quintile dummies for income and lagged income, (3) decile dummies for income and lagged income, and (4) separate dummies for the bottom 2, $2^{nd}-4^{th}$, $4^{th}-6^{th}$, $6^{th}-8^{th}$, $8^{th}-10^{th}$, $90^{th}-92^{nd}$, $92^{nd}-94^{th}$, $94^{th}-96^{th}$, $96^{th}-98^{th}$, and top 2 percentiles of income and lagged income. All income controls are in addition to first- and second-order income and lagged income. All regressions include household fixed effects. Error bars show 90% confidence level.

Table A.10: **Experience Effects and Consumption (PSID), Accounting for Measurement Error in Income**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.693***		-0.690***	-0.425***		-0.423***
	(0.195)		(0.194)	(0.113)		(0.113)
Experience (Macro)		-0.073**	-0.071**	-0.048***	-0.047***	
		(0.029)	(0.029)	(0.017)	(0.017)	
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	42,167	42,167	42,167	42,167	42,167	42,167
R-squared	0.752	0.751	0.752	0.752	0.752	0.752

Notes. Regressions differ from those in Table 2 only in terms of the income controls. As in Table 2, income controls include the first and second order of the logarithm of income and lagged income. In addition, we set a priori the amount of income variability that can be attributed to error, using the estimates of Bound, Brown, Duncan, and Rodgers (1994) based on the equation $var(\epsilon^y) = 0.04var(y)$. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.11: **Consumption (PSID), Additional Liquidity Controls**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)	-0.139		-0.132	-0.100		-0.096
	(0.134)		(0.134)	(0.076)		(0.077)
Experience (Personal) * LLW	-0.243		-0.248	-0.125		-0.126
	(0.149)		(0.149)	(0.085)		(0.086)
Experience (Macro)		-0.058***	-0.059***		-0.032***	-0.033***
		(0.020)	(0.020)		(0.011)	(0.011)
Experience (Macro) * LLW		0.001	0.005		-0.004	-0.002
		(0.014)	(0.014)		(0.007)	(0.007)
Low Liquid Wealth	0.020**	0.001	-0.008	0.013*	0.027	0.024
	(0.009)	(0.087)	(0.087)	(0.006)	(0.044)	(0.044)
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3
Observations	33,263	33,263	33,263	33,263	33,263	33,263
R-squared	0.771	0.771	0.771	0.771	0.771	0.771

Notes. Low Liquid Wealth (LLW) is an indicator variable equal to 1 for households with liquid wealth below the sample-year median. All other variables are defined as in Table 2. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.12: **Wealth Accumulation**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Personal)$_{t-6}$	0.426***		0.419***	0.241***		0.237***
	(0.057)		(0.057)	(0.032)		(0.032)
Experience (Macro)$_{t-6}$		0.055***	0.053***		0.029***	0.027***
		(0.014)	(0.014)		(0.009)	(0.009)
Observations	17,918	17,918	17,918	17,918	17,918	17,918
R-squared	0.320	0.319	0.321	0.320	0.319	0.321
Experience (Personal)$_{t-8}$	0.468***		0.462***	0.264***		0.260***
	(0.062)		(0.062)	(0.035)		(0.035)
Experience (Macro)$_{t-8}$		0.056**	0.053**		0.031**	0.030**
		(0.022)	(0.022)		(0.013)	(0.013)
Observations	13,754	13,754	13,754	13,754	13,754	13,754
R-squared	0.304	0.303	0.305	0.304	0.303	0.304
Experience (Personal)$_{t-10}$	0.498***		0.495***	0.283***		0.282***
	(0.078)		(0.077)	(0.043)		(0.043)
Experience (Macro)$_{t-10}$		0.053	0.051		0.028	0.027
		(0.032)	(0.032)		(0.018)	(0.019)
Observations	10,436	10,436	10,436	10,436	10,436	10,436
R-squared	0.286	0.284	0.286	0.286	0.284	0.286
Experience (Personal)$_{t-12}$	0.582***		0.582***	0.331***		0.331***
	(0.098)		(0.099)	(0.055)		(0.055)
Experience (Macro)$_{t-12}$		0.054	0.054		0.029	0.029
		(0.042)	(0.042)		(0.025)	(0.025)
Observations	7,525	7,525	7,525	7,525	7,525	7,525
R-squared	0.277	0.275	0.277	0.277	0.275	0.277
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Experience weighting	λ=1	λ=1	λ=1	λ=3	λ=3	λ=3

Notes. The dependent variable is total wealth, as defined in the main text. In columns (1) to (3), we use experience measures based on linearly declining weights (λ=1), and in columns (4) to (6), we use experience measures that shift more weight to recent observations (λ=3). The top panel uses the $t-6$ experience measures; the subsequent three panels use experience measures from $t-8$, $t-10$, $t-12$, respectively. Income controls include the $t-1$ family total income and the average family total income between $t-2$ and the year of the experience measures. Wealth controls include total wealth from the year of the experience measures. For gap years between PSID survey waves, we use prior-year income. Demographic controls include family size, heads' gender, race, marital status, education level, and employment status. We take the logarithm of all income and wealth variables. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

A.2 Robustness using Nielsen Data

Our second source of data on consumption choices is the Nielsen Homescan Dataset. This data contains detailed information on product purchases of a panel of more than 100,000 U.S. households from 54 geographically dispersed markets, including price, quantity, date of purchase, identifier of the store, as well as product characteristics, including brand, size and packaging, at the UPC level. Households record the dollar value of any coupons used and whether the purchase involved a deal from the retailer (sale item). The product categories are food and non-food grocery, health and beauty aids, and general merchandise, summing to approximately 3.2 million unique UPCs covering 125 general product categories.[26]

Households also report information on their demographics, including age, sex, race, education, occupation, employment status, family composition, household income, and location of residency up to the zip code level. Note that the geographic information is more precise than the state-level identification in the PSID, as it allows us to control for the local (county-level) unemployment rate U_{mt}. The information is updated annually, and the demographics of the households are representative of the population demographics at the national level. For our analysis, we drop households with heads below the age of 25 or above 75, as in the PSID sample.[27]

Our data sample consists of 3,171,833 observations of 105,061 households from 54 geographically dispersed markets, each roughly corresponding to a Metropolitan Statistical Area (MSA), from 2004-2013. Table A.13 provides the summary statistics. We note that the average consumption expenditure from Nielsen approximately corresponds to the food consumption expenditures in the PSID, which cross-validates the quality of the data sets as the Nielsen data cover mostly food products.

The high-frequency nature of the Nielsen data allows us to construct more fine-grained measures of consumption and unemployment exposure than the PSID. However, since Nielsen provides no information about households' prior residence or employment status (pre-sample period), we are not able to construct the same type

[26] Several studies have examined the quality of the data. For example, Einav, Leibtag, and Nevo (2010) compare the self-reported Nielsen data with data from cash registers. They conclude that the reporting error is of similar magnitude to that found in commonly used economic data sets.

[27] As in the PSID data, we also conduct the analysis on a subsample that excludes households over the age of 65 (retirees) whose expectation of their future income should be immune to beliefs about future economic fluctuations. The results from both sets of regressions are similar.

of macro and personal unemployment experience proxies as in the PSID. We thus construct the macro-level experience measure based on monthly national unemployment rates. For the personal experience measure, we can, at best, measure unemployment experiences since the beginning of the Nielsen data. Such a measure is biased as it is less precise at the beginning of the sample and less precise for households with shorter spells. We therefore report the estimations employing only the macro-experience measure, but re-estimate our model using a measure of personal unemployment experience that takes the value 1 at time t if the head of household has ever been unemployed since the beginning of the sample period up to time $t-1$, and 0 otherwise. (The coefficient of interest remains similar.)

Nielsen lacks information about consumers' wealth, which is an important component of consumption analyses. Our prior estimations alleviate concerns about unobserved wealth to some extent, given the robustness of the estimates across a broad range of wealth, income, and liquidity proxies. To further address the issue of the missing wealth control in the Nielsen data, we follow recent advancements in the literature, such as Stroebel and Vavra (2017) and Dube, Hitsch, and Rossi (2018), and use ZIP-code level house prices as a measure of housing wealth. According to these studies, consumption dynamics respond strongly to house price movements and housing wealth (cf. also Mian, Rao, and Sufi (2013) and Berger and Vavra (2015)). Empirical analyses can exploit this insight since better measures of housing prices have become available. Specifically, we extract Zillow's Home Value Index at the local ZIP code level as a proxy for local housing prices and merge it with the Nielsen data.[28] The match rate lies around 75%, and the resulting data set contains almost 3.2 million observations. We include the Home Value Index, an indicator for being a homeowner, and their interaction in all of our estimations.[29]

Table A.14 presents results from regression specification (B.1) in the main text. In columns (1) to (2), we use macroeconomic experience measures based on linearly declining weights (λ=1), and in columns (3) to (4), we use experience measures

[28] Zillow Inc. collects detailed data on home values across the U.S. and constructs monthly indices using the median value for a ZIP code. Zillow's estimates of home values ("Zestimates") aim to provide realistic market values given the size, rooms, and other known attributes of the house, recent appraisals, geographic location, and general market conditions. (The exact formula is proprietary.) For details about the data and Zillow's coverage across the U.S. see Dube, Hitsch, and Rossi (2018).

[29] We also conduct the analysis without including these wealth controls in the regressions, and the coefficient on unemployment experience remains significant and of very similar magnitude.

Table A.13: **Summary Statistics (Nielsen)**

Variable	Mean	SD	p10	p50	p90	N
Age	50	12	33	49	67	3,171,833
Household Size	2.8	1.5	1	2	5	3,171,833
Total Consumption [$]	714	537	205	586	1,366	3,171,833
Coupon Use [%]	0.03	0.05	0	0.01	0.09	3,171,833
Product Ranking	0.47	0.11	0.34	0.47	0.61	3,171,833
Purchase of Sale Items [%]	0.24	0.24	0	0.17	0.62	3,171,833
Household Income [$]	$50-$60k		$20-$25k	$50-$60k	$100k+	3,171,833
Experience (Macro), $\lambda = 1$ [%]	5.97	0.18	5.78	5.9 3	6.25	3,171,833
Experience (Macro), $\lambda = 3$ [%]	5.89	0.36	5.47	80	6.42	3,171,833

Notes. The table reports the summary statistics of the monthly Nielsen data from 2004-2013. Copon use is the value of coupons divided by total expenditures. Product ranking ranges from 0 to 1 based on the unit price of a good within its product module and market in a given month; lower-priced goods have lower values. Purchase of sale items is the number of sale items divided by the total number of items bought. Nielsen reports income in 13 brackets. Experience (Macro) is households' lifetime experience of national unemployment rates.

that shift more weight to recent observations (λ=3). We find that, exactly as in the PSID data, households who have experienced worse unemployment conditions during their lifetimes so far spend significantly less, controlling for contemporaneous macro conditions, local market conditions, and household controls. The economic magnitude is significant: based on the estimates in column (2), a one standard deviation increase in unemployment experiences is associated with a $255 decline in annual consumption of non-durables, which amounts to around 3% of average spending for the households in our sample. All regression results are quantitatively and qualitatively similar when clustered by household or two-way clustered at the cohort and time level.

In Figure A.3, we illustrate the economic magnitude of the estimates in the context of unemployment conditions during the Great Recession, which falls in the Nielsen sample period. The average monthly unemployment rate from 2008-2012 was 8.1%, with the maximum during the period being 10%. Comparing these numbers with historical averages, the average unemployment rate during the 60 years prior to 2008, from 1947-2007, was 5.6%. Now consider two individuals, a 25-year-old and a 60-year-old as of December 2007. Their lifetime unemployment experience, based on our experience weighting scheme of $\lambda = 1$, was 5.3% and 5.8%, respectively, when

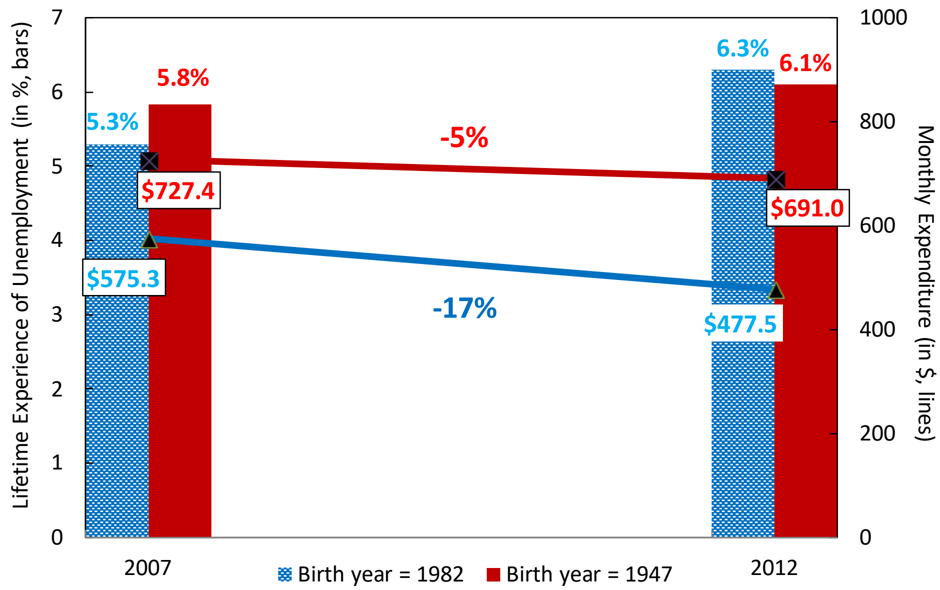
Table A.14: **Experience Effects and Monthly Consumption (Nielsen)**

	(1)	(2)	(3)	(4)
Experience (Macro)	-0.166***	-0.165***	-0.172***	-0.172***
	(0.055)	(0.055)	(0.027)	(0.027)
Unemployment rate (county)		-0.005***		-0.005***
		(0.001)		(0.001)
Income control	Yes	Yes	Yes	Yes
Wealth control	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Market-area fixed effects	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes
Experience weighting	$\lambda = 1$	$\lambda = 1$	$\lambda = 3$	$\lambda = 3$
Observations	3,171,833	3,171,833	3,171,833	3,171,833
R-squared	0.526	0.526	0.526	0.526

Notes. Fixed effects regression with (log) total consumption expenditure as the dependent variable. Experience (Macro) is the macroeconomic experience measure of unemployment. In columns (1) to (2), we use macroeconomic experience measures based on linearly declining weights (λ=1), and in columns (3) to (4), we use experience measures that shift more weight to recent observations (λ=3). Wealth controls include the ZIP-code level house-price index from Zillow, an indicator variable for households that own at least one house, and an interaction term between the house price index and the homeowner dummy. Household characteristics include unemployment status, household size, education, race, and a dummy variable indicating whether the respondent is unemployed at the time of the survey. Time fixed effects are year-month fixed effects. Regressions are weighted using the household sampling weights from Nielsen. The sample period runs from 2004 to 2013. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

they entered the crisis in 2008. By the end of 2012, their lifetime unemployment experience was 6.3% vs. 6.1%, respectively. In other words, the unemployment experience for the 25-year-old increased by 1 pp, whereas that for the 60-year-old increased by 0.3 pp. Relating these experiences to consumption behavior, our model estimates (from column (2) in Table A.14) imply that the monthly consumption expenditure of the 25-year-old decreased by approximately 17% while that of the 60-year-old decreased by approximately 5%.

Figure A.3: **Example of Unemployment Experience Shock from Recession, Nielsen**



Notes. Example of the impact of the Great Recession on weighted lifetime experiences of unemployment rates and monthly consumption expenditure of a 25- and a 60-year-old (as of 2007) from December 2007 to December 2012. The bars show the weighted lifetime experiences of unemployment rates based on linearly-declining weights. The lines show the monthly expenditures: the values for 2007 are from actual data, and the values for 2012 are calculated based on model estimates.

A.3 Robustness using CEX

In this section, we turn to a third source of consumption data, the Consumer Expenditure Survey (CEX). We now enlarge the set of consumption items to include durable goods as well as the CEX measure of total consumption, which is widely used in the literature. It encompasses further categories of expenditures, in addition to durables and non-durable items, including healthcare and education expenses.

The CEX is a repeated, cross-sectional survey of household spending across a comprehensive list of product categories at the quarterly frequency. It is considered the benchmark data in the consumption literature. Compared to the PSID, its two main disadvantages are the lack of wealth information and the lack of panel structure.

As in the analysis of the PSID, we link measures of consumption to households' lifetime unemployment experiences. In the CEX data, we are not able to construct the same type of macro and personal unemployment experience measures as in the

Table A.15: **Summary Statistics (CEX)**

Variable	Mean	SD	p10	p50	p90	N
Age	51	17	29	49	75	439,315
Household Size	2.7	1.5	1	2	5	439,315
Total Consumption [$]	6,280	6,234	1,997	4,626	11,747	439,315
Non-durable Consumption [$]	4,217	3,225	1,573	3,508	7,465	439,315
Durable Consumption [$]	2,064	4,517	128	810	4,159	439,315
Household Income [$]	48,180	49,409	9,000	34,490	100,000	461,390
Experience (Macro) [%]	6.1	0.31	5.80	6.1	6.6	439,315

Notes. The table reports the summary statistics of quarterly CEX data from 1980-2012. Experience (Macro) is households' lifetime experience of national unemployment rates.

PSID because the CEX does not provide information on where households resided prior to the sample period, nor on their prior employment status. We use the macro-level experience measure based on national unemployment rates at the quarterly frequency.

Table A.15 provides the summary statistics. The average income, $48k, is in line with the average income at the national level. The sample period runs from 1980-2012. The average non-durable and durable spending amount to 67% and 33% of the mean total expenditures, respectively. Non-durable spending and durable spending are weakly positively correlated, with durable spending being much more volatile than non-durable spending.

We re-estimate the sensitivity of consumption to experienced unemployment conditions, using an estimation model that closely mirrors the PSID model from equation (3). Table A.16 shows the results for total, durable, and non-durable consumption, using macroeconomic experience measures based on linearly declining weights (λ=1).

The results strongly confirm our prior findings and reveal new quantitative implications for the different components of total consumption. All experience effect coefficients are negative and highly significant. Households who have experienced worse unemployment conditions during their lifetime spend significantly less in total, durable, and non-durable consumption. The economic magnitudes are large: A one standard-deviation increase in unemployment experience is associated with a decline of $701 in annual consumption and $460 in annual non-durable consumption. The estimate on annual total consumption is smaller than the PSID estimate ($1,099

Table A.16: **Experience Effects and Quarterly Consumption (CEX)**

	Total	Durable	Nondurable
Experience (Macro)	-0.090***	-0.108***	-0.088***
	(0.008)	(0.007)	(0.020)
Income control	Yes	Yes	Yes
Household controls	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes
Observations	439,315	439,315	439,315
R-squared	0.436	0.462	0.243

Notes. Pooled regressions with (log) total consumption expenditure, durable consumption, and non-durable consumption as the dependent variables. Experience (Macro) is the macroeconomic experience measure of unemployment, constructed as a lifetime linearly-declining weighted national unemployment rate experienced by households. Household characteristics include unemployment status, household size, education, and race. Time fixed effects include year-quarter fixed effects. Region fixed effects include dummies for the Northeast, Midwest, South, and West region. Regressions are weighted by household sampling weights from CEX. The sample period runs from 1980 to 2012. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

decline), while the estimate on non-durable consumption is larger than that using the Nielsen data ($255 decline). This may reflect the fact that both total expenditures and non-durable expenditures in the CEX encompass more categories than the PSID and Nielsen. Compared to the PSID, total expenditures in the CEX include additional categories such as household furnishing and home repairs, which tend to be more inelastic. Compared to the Nielsen, non-durable consumption in the CEX includes categories such as clothing and entertainment, which tend to be elastic. The new estimate for durables indicates that a one standard-deviation increase in past unemployment experience predicts a $276 decline in annual durable consumption.

Appendix B Additional Implications

B.1 Consumption Quality

Motivated by the robust results on the quantity of consumption spending, we investigate whether people's lifetime exposure to unemployment also affects the quality of their consumption. Does the personal experience of harder economic times also induce more cautious spending in terms of bargain hunting, coupon use, and lower quality of items purchased?

To explore this question, we make use of the rich, micro-level information on purchases in the Nielsen data. To estimate the sensitivity of consumption quality to experienced unemployment conditions in the Nielsen data, we use an estimation model that mirrors the PSID model from equation (3) but accounts for the additional market-level information:

$$C_{it} = \alpha + \beta UE_{it} + \kappa U_{mt} + \gamma' x_{it} + \eta_t + \varsigma_m + \upsilon_i + \varepsilon_{it}. \tag{B.1}$$

where C_{it} denotes one of three monthly measures of consumption quality: (1) coupon use, normalized by total expenditures, (2) the ranking of products based on their unit price (within module, market, and month), normalized between 0 and 1, where lower value represents lower-priced goods, and (3) number of on-sale products purchased, normalized by the total number of products purchased. Other new variables are the current county-level unemployment rate U_{mt} and local-market dummies ς_m, where local markets denote Nielsen's designated market areas (DMAs).[30] As before UE_{it} denotes the lifetime (macro) experience of unemployment rates based on a weighting scheme of $\lambda = 1$.[31] Note that we are not able to construct the same type of macro and personal unemployment experience proxies as in the PSID because Nielsen provides no information about households' prior residence or employment status (pre-sample period). We thus report the estimations employing only the macro experience measure, constructed based on national unemployment rates. The vector of controls x_{it}

[30] DMAs are slightly bigger than a county but smaller than an MSA. We control for location at the local market level instead of the county level because people may travel outside of counties to purchase goods. The results are similar if we use county fixed effects instead.

[31] Results are quantitatively and qualitatively similar using experiences measures based on a weighting scheme of $\lambda = 3$.

includes income controls, wealth controls, household characteristics (unemployment status, household size, education, race, and a dummy variable indicating whether the respondent is unemployed at the time of the survey), age dummies, household dummies, and the time dummies η_t are now year-month-specific. While Nielsen lacks information about consumers' wealth, we follow recent advancements in the literature, such as Stroebel and Vavra (2017) and Dube, Hitsch, and Rossi (2018), and use ZIP-code level house prices as a measure of housing wealth. More details on the experience measures and income and wealth control variables are provided in Appendix-Section A.2. Standard errors are clustered at the cohort level for the regression. The summary statistics are in Table A.13.

Table B.17 displays the main coefficients of interest. We find that households who have lived through worse employment conditions are more likely to use coupons, purchase lower-end products, and allocate more expenditures toward sale items. For example, our estimates suggest that households who have experienced unemployment rates at the 90th percentile of the sample experiences use \$13 more in coupons and purchase 8% more sale items monthly than respondents at the 10th percentile. In other words, people who have lived through periods of high unemployment adjust the quality margins of their consumption accordingly.

Our results echo findings in the existing literature such as Nevo and Wong (2015), who show that U.S. households lowered expenditures during the Great Recession by increasing coupon usage, shopping at discount stores, and purchasing more goods on sale, larger sizes, and generic brands. While they explain this behavior with the decreased opportunity costs of time, we show that consumption scarring is also at work. Key to identifying this additional source of consumption adjustment are the inter-cohort differences and the differences in those differences over time.

B.2 Heterogeneity Across Cohorts

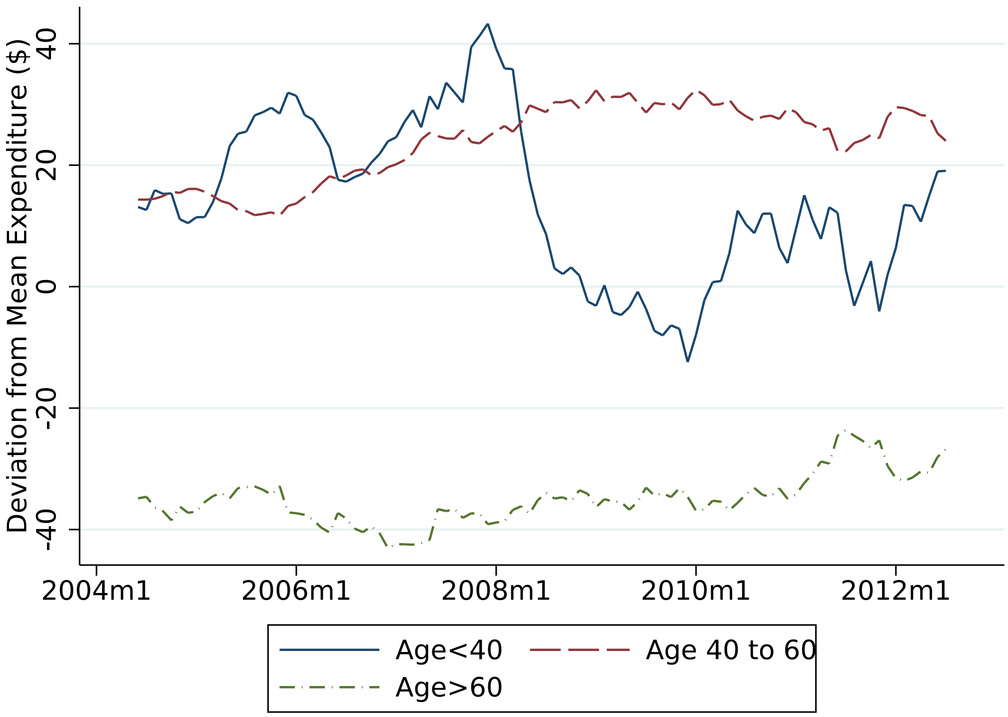
Scarring effects from past exposure to unemployment give rise to heterogeneity in consumption choices. The scarring hypothesis links this heterogeneity to different histories of past experiences. Another, more subtle implication of the scarring hypothesis is that there is heterogeneity in the response to the *same* recent experience: younger consumers will react more strongly to a new unemployment shock than older

Table B.17: **Experience Effects and Monthly Consumption Quality (Nielsen)**

	(1)	(2)	(3)	(4)
A: Coupons				
Experience (Macro)	0.036***	0.035***	0.005**	0.005**
	(0.005)	(0.005)	(0.002)	(0.002)
Unemployment rate (county)		0.001***		0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
R-squared	0.040	0.041	0.690	0.690
B: Product Ranking				
Experience (Macro)	-0.104***	-0.104***	0.004**	0.004**
	(0.0338)	(0.0338)	(0.002)	(0.002)
Unemployment rate (county)		-0.001**		-0.009***
		(0.001)		(0.002)
R-squared	0.083	0.083	0.680	0.680
C: On-sale Items				
Experience (Macro)	0.159***	0.156***	0.009**	0.009**
	(0.018)	(0.018)	(0.004)	(0.004)
Unemployment rate (county)		0.003***		0.005***
		(0.000)		(0.001)
R-squared	0.073	0.074	0.830	0.830
Income control	Yes	Yes	Yes	Yes
Wealth control	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Market area fixed effects	Yes	Yes	Yes	Yes
Household fixed effects	No	No	Yes	Yes
Observations	3,171,833	3,171,833	3,171,833	3,171,833

Notes. OLS regressions with the ratio of coupons used over total expenditure as the dependent variable in Panel A; the (transformed) ranking of goods, based on their unit price in their specific product modules, markets, and months in Panel B (where we use the logit transformation ln(y/(1-y)) to map the original ranking, which ranges from 0 to 1, to the real line); and with the ratio of on-sale items purchased over the total number of items purchased as the dependent variable in Panel C. Experience (Macro) is the macroeconomic experience measure of unemployment, constructed as a lifetime linearly-declining weighted national unemployment rate experienced by households. Column 2 and 4 include the regressor local unemployment. Wealth controls include the ZIP-code level house-price index from Zillow, an indicator variable for households that own at least one house, and an interaction term between the house price index and the homeowner dummy. Household characteristics include unemployment status, household size, education, race, and a dummy variable indicating whether the respondent is unemployed at the time of the survey. Time fixed effects are year-month fixed effects. Regressions are weighted using the household sampling weights from Nielsen. The sample period runs from 2004 to 2013. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Figure B.4: **Monthly Consumption Expenditure by Age Group**



Notes. Six-month moving averages of monthly consumption expenditures of young (below 40), mid-aged (between 40 and 60), and old individuals (above 60) in the Nielsen Homescan Panel, expressed as deviations from the cross-sectional mean expenditure in the respective month and deflated using the personal consumption expenditure (PCE) price index of the U.S. Bureau of Economic Analysis (BEA). Observations are weighted with Nielsen sample weights.

consumers. The reason is that an unemployment shock in the recent past alters the (weighted) lifetime average of a consumer more the shorter the history of past experiences is, i. e., the younger the consumer is. We can see this in the formula for experience-based beliefs, as defined in equations (1) and (2). The shorter a consumer's life is the more mass is assigned to the most recent realization. Hence, we predict that the young lower their consumption expenditure to a greater degree than older cohorts during economic busts and, vice-versa, increase it more during booms.

Indeed, as shown in Appendix-Figure B.4 using the Nielsen data, the consumption spending of younger cohorts is significantly more volatile and more sensitive to crises such as the Great Recession.

We further test the age heterogeneity prediction.[32] We regress the change in log monthly consumption on the interaction of age with the change in log unemployment conditions from month t to $t-1$, controlling for the same battery of controls as in Table B.17. We do so separately for positive and negative changes (in absolute value) in unemployment in order to identify possible asymmetries in the reaction to improving versus tightening economic conditions. Since we know where a household resided in $t-1$, we can use changes in either the national unemployment rate or the local (county-level) unemployment rate as our proxy for a recently experienced unemployment shock, controlling for the respective other rate change.

The results are in Table B.18. Columns (1)-(2) show the estimates when we interact age with the national-rate shock, and columns (3)-(4) show the estimates when using the local (county-level) rate shock. We include both sets of interactions in columns (5)-(6). Note that the level effect of log national unemployment rate changes is absorbed by the time (year-month) fixed effects, and that we include the positive and negative changes in log local unemployment rate across all specifications.

The estimation results in columns (1) to (4) reveal that the coefficients of all age-unemployment interactions are significantly negative. That is, recent unemployment shocks, whether positive or negative, have a smaller effect on the consumption expenditures of older cohorts. The effects are a bit stronger for decreases in national unemployment and for increases in local unemployment. When we include all four interaction effects, in columns (5) and (6), the coefficient sizes remain similar, though the estimated coefficient of the interaction of age with higher national unemployment and with lower local unemployment become smaller and insignificant. Overall, the results support our prediction of a significantly stronger response to recent experiences among the young than among the old.

We note that a potential alternative explanation for some of the estimated interaction effects is the presence of stronger liquidity constraints among the young (e. g., Zeldes (1989), Gourinchas and Parker (2002)). Models with liquidity constraints

[32] We continue to use the Nielsen data rather than switching back to the PSID data since the low frequency of survey waves in the PSID (biannual rather than monthly) does not allow to define the "most recent" past experience in a uniform and consistent way, challenging the interpretation of the corresponding estimations. When we nevertheless estimate an approximative model in the PSID, relating the (log) change in total consumption to the interaction between the change in annual unemployment (from time $t-1$ to t) and a dummy variable for the young, we find qualitatively similar effects.

Table B.18: **Age-Heterogeneity in Reaction to Unemployment Fluctuation**

	(1) $\Delta\ln(C)$	(2) $\Delta\ln(C)$	(3) $\Delta\ln(C)$	(4) $\Delta\ln(C)$	(5) $\Delta\ln(C)$	(6) $\Delta\ln(C)$
Age * $\Delta\ln$(National unemp-down)	-0.023***	-0.023***			-0.021***	-0.021***
	(0.005)	(0.005)			(0.005)	(0.005)
Age * $\Delta\ln$(National unemp-up)	-0.006***	-0.007***			-0.001	-0.000
	(0.002)	(0.002)			(0.002)	(0.003)
Age * $\Delta\ln$(Local unemp-down)			-0.002*	-0.003**	-0.001	-0.002
			(0.001)	(0.001)	(0.001)	(0.001)
Age * $\Delta\ln$(Local unemp-up)			-0.008***	-0.008***	-0.008***	-0.009***
			(0.001)	(0.001)	(0.001)	(0.001)
Local unemployment control	Yes	Yes	Yes	Yes	Yes	Yes
Income control	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Market-area fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	No	Yes	No	Yes	No	Yes
Observations	3,171,833	3,171,833	3,171,833	3,171,833	3,171,833	3,171,833
R-squared	0.010	0.014	0.010	0.014	0.010	0.014

Notes. The dependent variable is the change in log monthly total consumption expenditures, and the main regressors are the interaction terms between age and the change in log national or local unemployment rate, separated into two variables for positive and negative changes (in absolute value), both from time t to $t-1$. Local unemployment controls are the change in log local unemployment rate, also separated into two variables for positive and negative changes. Household characteristics include household size, education, and race. Time fixed effects include year-month fixed effects. The sample period runs monthly from 2004 to 2013. Regressions are weighted by Nielsen household weights. Robust standard errors (in parentheses) are clustered by cohort and time. *, **, *** denote 10%, 5%, and 1% significance, respectively.

predict that the young react more strongly to negative unemployment shocks than the old because they are more likely to hit liquidity constraints. These models do not easily predict a more positive reaction to positive shocks, though. To generate the latter prediction, too, these models need to rely on the argument that the young were previously constrained, and that a particularly strong reaction to a positive shock allows the young to adjust to their permanent-income optimum. However, even with this additional argument, liquidity constraints are unlikely to explain our estimates since the identification exploits not only positive shocks at (previously) bad times, but also good shocks at (already) good times. For the latter instances, adjustments to the PIH optimum do not predict a stronger reaction among the young, and liquidity concerns point to the opposite outcome. In fact, young consumers with more positive prior experiences would exhibit a weaker reaction to recent good outcomes, and young consumers with more negative prior experiences would exhibit a stronger reaction to recent good outcomes according to the PIH.[33] Thus, our findings highlight experience effects as a distinct force in affecting people's consumption behavior.

Appendix C Model

We implement the empirical model of Low, Meghir, and Pistaferri (2010) with a few minor adjustments to our setting. All key equations are retained and, when possible, all parameters are set to the same values. As in Low et al., some parameters are set separately for high- and low-education groups, including the probability of job destruction and job offers.

[33] To show this directly, we estimated a set of regressions that augments the specifications from Table B.18 with triple interactions of age, positive and negative national or local unemployment shocks, and an indicator of above-median unemployment experiences for the respondent's age. The estimated effects of positive national and local unemployment shocks are weaker (given age) for respondents with worse unemployment experiences, as predicted by the scarring hypothesis, but not by a standard PIH framework.

C.1 Parameters governing the income process and utility maximization

The utility function and lifetime expected utility are defined in equations (4) and (5) in Section IV as $U(c, P) = \frac{\left(c \times e^{\eta P}\right)^{1-\gamma}}{1-\gamma}$ and $U(c_{i,t}, P_{i,t}) + \mathrm{E}_t \left[\sum_{s=t+1}^{L} \beta^{s-t} U(c_{i,s}, P_{i,s})\right]$, respectively. In the simulations, we follow Low et al. and take risk aversion parameter $\gamma = 1.5$ from Attanasio and Weber (1995), use the estimates for η from their Table 2, and set the discount factor $\beta = 1/R$ in the value function.

For the gross quarterly income $w_{i,t}h$, we also follow Low et al. in setting the number of hours worked per quarter to $h = 500$. In the wage process $\ln w_{i,t} = d_t + x'_{i,t}\psi + u_{i,t} + a_{i,j,t_0}$, we recover the parameters α, β_1, and β_2 governing the deterministic component, $d_t + x'_{i,t}\psi = \alpha + \beta_1 \cdot \text{age} + \beta_2 \cdot \text{age}^2$, from the parameters in the Fortran code published alongside Low et al. In the permanent component $u_{i,t} = u_{i,t-1} + \zeta_{i,t}$ where $\zeta_{i,t}$ is i. i. d. normal with mean 0 and variance σ_ζ^2. We use the value of σ_ζ given in Table 1 of Low et al.. The consumer-firm job match component, a_{i,j,t_0}, is drawn from a normal distribution with mean 0 and variance σ_a^2, and we use the value of σ_a given in Table 1 of Low et al..

We obtain the values for the probabilities of job destruction δ, of a job offer when employed $(1-\delta)\lambda^e$, and of a job offer when unemployed λ^n from Table 2 in Low et al. (2010). Note that, while the probability of job destruction is constant across time for a given household, the probability of receiving a job offer varies depending on whether or not an agent is employed.

C.2 Budget constraint

The intertemporal budget constraint for a working individual i in period t is given by

$$\begin{aligned} A_{i,t+1} & \\ &= R\left[A_{i,t} - c_{i,t}\right] \\ &\quad + \left(w_{i,t}h(1-\tau_w) - F_{i,t}\right) \\ &\quad P_{i,t} + \left(B_{i,t} I_{i,t}^{UI}(1 - I_{i,t}^{DI}) + D_{i,t} I_{i,t}^{DI}\right)(1 - P_{i,t}) + T_{i,t} I_{i,t}^{T} \end{aligned}$$

where $A_{i,t}$ is beginning-of-period-t assets, R is the interest factor, τ_w a tax, F the fixed cost of working, P an indicator for whether an individual is working, B are unemployment benefits, D disability benefits, T food stamp benefits, c is consumption, and the I variables are indicators of receiving the associated social insurance.

As in Low et al. (2010), we assume that individuals cannot borrow and thus $A_{i,t} \geq 0 \quad \forall t$. Also as in Low et al. (2010), we set $r = .15$ and define $R = 1 + r$. We use the estimates for F from their Table 2. In Low et al. (2010), τ_w is a variable of interest and solved for, albeit as fixed percentage (not progressive or regressive). As we do not focus on the value of social insurance programs, including the tax revenues to be raised to fund them and their relation with consumption, we normalize $\tau_w = 0$.

During retirement individuals receive social security equal to the value of disability, so the budget constraints simplifies to

$$A_{i,t+1} = R\left[A_{i,t} + D_{i,t} - c_{i,t}\right].$$

C.3 Social Insurance programs

As in Low et al. (2010), we implement three social insurance programs, unemployment insurance, food stamps, and disability insurance.

Unemployment Insurance. Unemployment Insurance is paid only during the quarter following job destruction. Unemployment benefits are given by

$$B_{i,t} = \begin{cases} bw_{i,t-1}h & \text{if } bw_{i,t-1}h < B_{\max}, \\ B_{\max} & \text{if } bw_{i,t-1}h \geq B_{\max}. \end{cases}$$

where b is the replacement ratio, and $B_{\max}$ is the cap on unemployment benefits. We set $b = .75$ as in Low et al. (2010) and $B_{\max}$ to the value used in the associated code.

Food Stamps (Means-Tested Social Insurance). Defining gross income as

$$y_{i,t}^{\text{gross}} = w_{i,t}hP_{i,t} + \left(B_{i,t}I_{i,t}^{UI}(1 - I_{i,t}^{DI}) + D_{i,t}I_{i,t}^{DI}\right)(1 - P_{i,t}),$$

and net income as

$$y = (1 - \tau_w)y^{gross} - d,$$

the amount of food stamps allocated to agent i in period t is

$$T_{i,t} = \begin{cases} \overline{T} - .3 \times y_{i,t} & \text{if } y_{i,t} \leq \underline{y} \\ 0 & \text{otherwise,} \end{cases}$$

where $\overline{T}$ is a maximum payment and $\underline{y}$ is a poverty line. One important implication of this definition is that there is no disincentive to hold assets. Adjusting to quarterly values, we set $\overline{T}$ to the maximum food stamp allotment for a couple in the US in 1993, $\underline{y}$ to the maximum food stamp allotment for the US in 1993, and d to the standard deduction for a couple in the US in 1993.

Disability. As in Low et al. (2010), individuals above 50 can apply for disability when they are unemployed and are accepted with a fixed probability of .5. If an application is successful, disability becomes an absorbing state for the remainder of the person's working life. If a person is not accepted, they can only reapply in a future bout of unemployment, after having worked again for at least one year. As a disincentive to applying, the individual must be unemployed in both the period they apply and the period after. We also impose that individuals must have a sufficiently low u and not be working or have a job offer at the time of application. The formula for disability benefits is

$$D_{i,t} = \begin{cases} .9 \times \overline{w}_i & \text{if } \overline{w}_i \leq a_1 \\ .9 \times a_1 + .32 \times (\overline{w}_i - a_1) & \text{if } a_1 < \overline{w}_i \leq a_2 \\ .9 \times a_1 + .32 \times (a_2 - a_1) + .15 \times (\overline{w}_i - a_2) & \text{if } a_2 < \overline{w}_i \leq a_3 \\ .9 \times a_1 + .32 \times (a_2 - a_1) + .15 \times (a_3 - a_2) & \text{if } \overline{w}_i > a_3 \end{cases}$$

where a_1, a_1, and a_3 are fixed thresholds from legislation, and $\overline{w}_i$ is the mean earnings prior to application. Similar to Low et al. (2010), we assume $\overline{w}_i$ can be approximated using the agent's value of $u_{i,t}$ at the time of application.

C.4 Implementation

Appendix-Table C.1 details all parameters referenced above and their sources. As discussed, most values are obtained directly from Low et al. (2010), and some are retrieved from examining the associated Fortran 90 code published with the paper. In cases where we were unable to ascertain values in either source, as is the case for several welfare values, we use actual values from 1993, the year in which the SIPP survey used in Low et al. for hourly wage data begins. This is also the closest year in the SIPP survey to the PSID data, and the values are consistent with the model values.

When we combine the high- and low-education data, we use 70% low- and 30% high-education observations, roughly corresponding to recent US census estimates of those without and with a bachelor's degree.[34]

Like Low et al. (2010), we solve the model numerically. In the last period, all agents consume the entirety of their assets. We then iteratively solve backwards for consumption and other relevant decisions that maximize the agents' value functions. Further details of the model solution can be found in Low et al. (2010).

C.5 Past Experiences and Income

In addition to illustrating the impact of past experiences on consumption, we also apply the model to study the relationship between unemployment experiences and future income. Appendix-Table C.2 replicates Table 4 for EBL consumers using the model accounting for unemployment scarring. Appendix Table C.2 shows the estimation results for predicting future income two, four, six, eight, and ten years ahead, corresponding to the setup in Table 4. As before, we weight past unemployment experiences with either linearly declining weights (λ=1), shown in the top half, and with more weight shifted to recent observations (λ=3), shown in the bottom panel.

The results on unemployment experiences with linearly declining weights show that past experiences do not significantly predict two-years-ahead future income, consistent with the empirical finding. All of the other coefficients in the table suggest a positive relationship between current unemployment experience and future income. The intuition for these results is similar to that for the positive relationship between unemployment experience and consumption for rational consumers: Conditional on current income, unemployment experience positively predicts a higher permanent

[34] The percent of the US population with at least a bachelor's degree has increased over the last three decades. It was closer to 25% in 2007 and 20% in 1995. We opted for the more recent estimates to err, if anything, on the side of a greater inclusion of high-education individuals.

Table C.1: **Model Parameters Used in Simulations**

Parameter	Low Education	High Education	Source (from Low, Meghir, and Pistaferri (2010))
γ	1.5	1.5	Text
σ_a	0.226	0.229	Table 1
σ_ζ	0.095	0.106	Table 1
$P(\zeta)$	.25	.25	Text
δ	.049	.028	Table 2
λ^e	.67	.72	Table 2
λ^n	.76	.82	Table 2
b	.75	.75	Text
r (yearly)	.015	.015	Text
β	$1/(1+r)$	$1/(1+r)$	Text
F	1088	1213	Table 2
η	-.55	-.62	Table 2
h	500	500	Text
b	.75	.75	Text
UI Cap	3178	3178	Code
P(Disability Acceptance)	.5	.5	Text
a_1	1203	1203	Code
a_2	7260	7260	Code
a_3	16638	16638	Code
α	1.0583	.642	Code
β_1	.0486	.0829	Code
β_2	-0.0004816	-.0007768	Code
Parameter	Low Education	High Education	Source
d	6200/4		Standard couple deduction in 1993[a]
$\underline{y}$	(6970+2460)/4		Actual poverty line in 1993 for couple[b]
$\overline{T}$	203×3		Actual max food stamp allotment for US 1993[c]

[a] See `https://web.archive.org/web/20190228193856/https://www.irs.gov/pub/irs-prior/f1040a--1993.pdf`.

[b] See `https://web.archive.org/web/20190228194017/https://aspe.hhs.gov/prior-hhs-poverty-guidelines-and-federal-register-references`.

[c] See `https://web.archive.org/web/20190228193653/https://fns-prod.azureedge.net/sites/default/files/Trends1999-2005.pdf`. Accessed via `https://web.archive.org/web/20190228195514/https://www.fns.usda.gov/snap/trends-food-stamp-program-participation-rates-1999-2005`.

component of income. If we do not control for income and wealth, unemployment experience and future income are negatively correlated.

Table C.2: **Experience Effects and Future Income: Model**

	Income_{t+2}	Income_{t+4}	Income_{t+6}	Income_{t+8}	Income_{t+10}
Experience , λ=1	0.004	0.195	0.277	0.330	0.351
	(0.10)	(6.07)	(8.85)	(11.87)	(13.63)
Experience, λ=3	0.257	0.495	0.558	0.592	0.582
	(5.87)	(11.75)	(13.69)	(16.60)	(17.17)
Income control	Yes	Yes	Yes	Yes	Yes
Wealth control	Yes	Yes	Yes	Yes	Yes
Period (age) fixed effects	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes

Notes. The dependent variables are simulated future income in two, four, six, eight, and ten years, respectively, where one year is modeled as 4 periods/quarters. The simulations are for behavioral agents and account for unemployment scarring. Estimations are for $\lambda = 1$ in the top panel and $\lambda = 3$ in the bottom panel. Log simulated income and wealth are controlled for. All estimations include period and education fixed effects and use period-clustered standard errors. Simulations are based on the working periods of 10,000 simulated consumers and thus 1,600,000 observations less 10,000 times each period in the future for which income is taken. t statistics in parentheses.