Scarred Consumption*

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Abstract

We show that economic downturns can "scar" consumers in the long-run. Consumers who have personally experienced unemployment spells or have lived through times of high unemployment remain pessimistic about their future financial situation and spend significantly less for years to come, controlling for income, wealth, and employment. Their actual future income is uncorrelated with past lifetime experiences after controls. Due to their experience-induced frugality, scarred consumers accumulate more wealth. We use a stochastic life-cycle model to show that the estimated negative relationship between past downturns and consumption cannot reflect financial constraints, income scarring, or other notions of unemployment scarring, but is predicted by our model of consumption scarring. As also predicted by the model, the estimated effects of unemployment shocks are stronger for younger cohorts. Our results provide a novel micro-foundation for fluctuations in aggregate demand and imply long-run effects of macroeconomic shocks.

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

I Introduction

Soon after its onset in the spring of 2020, the COVID-19-induced recession prompted debates about its long-run implications. With unprecedented numbers of initial claims for unemployment and unemployment rates rivaling those of the Great Depression, would the recovery be as painful and slow as in the 1930s? Looking back at the more recent experience of the Great Recession of 2008, why were consumers so slow to return to prior consumption levels then (Petev et al. 2011, De Nardi et al. 2012), and would the pattern repeat itself? Do crises "scar" consumers?

The lingering, long-term effects of macroeconomic crises have been hard to capture in existing workhorse consumption models. For example, consumption after the Great Recession remained low not only in absolute levels, but also relative to the growth of income, net worth, and employment. This pattern has challenged standard life-cycle consumption explanations, such as time-varying financial constraints. It is 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]

Our research hypothesis starts from the observation in Pistaferri (2016) that these long-lasting crisis effects are accompanied by consumer confidence remaining low for longer periods than standard models imply. We argue that past economic conditions scar consumer beliefs and expenditure decisions for years to come. We show that both personal experiences of unemployment and exposure to high local and national unemployment predict spending behavior years later, controlling for current and recent income, employment, wealth, liquidity, and other life-cycle determinants. They also predict lingering pessimism about financial conditions. At the same time, these past lifetime experiences do *not* predict actual future income, after including

[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).

the standard controls (including recent unemployment), and predict, if anything, increases in future wealth.

Taken together, these results challenge explanations that rely on unobserved low income and wealth. They also point to "what is missing" in the long line of work on adaptive expectations and learning in macroeconomics (e. g., Hansen and Sargent 2008; Kozlowski, Veldkamp, and Venkateswaran 2020): Existing learning models fail to predict that individual differences in personal lifetime experiences of unemployment, both on the individual and on the local and national level, strongly predict cross-sectional differences in beliefs and consumption decisions. As we show, past economic conditions, and heterogeneity in consumers' exposure to these conditions, give rise to significant heterogeneity in consumption choices, both over time and in the cross section, and explain generational differences in consumption and savings.

The notion of scarring effects of past economic conditions on beliefs and consumption expenditures is in line with prior research in labor and political economy. Papers have documented longterm scars from, for example, graduating into a recession (Kahn 2010, Oreopoulos, von Wachter, and Heisz 2012) or, in the political-economy realm, 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)). We are the first to explore long-term scarring effects in the realm of consumption decisions.

We operationalize the notion of scarring effects building on the proxies proposed in the macro-finance literature on the longlasting effects of stock-market and inflation experiences (Malmendier and Nagel 2011, 2015).[2] This approach amounts to forming weighted averages over past economic conditions during an individual's lifetime so far. Our approach differs in that we do not rely solely on nationwide macroeconomic conditions but also on individual-specific outcomes—in our case personal experiences of unemployment—and on local variation in macroeconomic conditions. As a result, the identifying variation is not absorbed by cohort fixed effects, as it was the case in the prior literature. Relatedly, our paper is the first to estimate within-household effects, rather than only differences across cohorts, which ameliorates concerns about cross-sectional confounds. Moreover, we alter previously used measures of experience

[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).

effects to distinguish our estimation results from known earnings implications of job loss (see, e. g., Jacobson, LaLonde, and Sullivan (1993), Couch and Placzek (2010)) by restricting the scarring analysis to unemployment experiences further in the past. That is, we exclude 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 start by presenting four baseline findings on the relation between past experiences and (1) current consumption, (2) current beliefs, (3) future income, and (4) future wealth build-up.

First, we document the long-lasting scars arising from past exposure to personal unemployment and to local and national unemployment conditions on consumption 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 have significant predictive power for consumption expenditures, 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. To the best of our knowledge, our analysis is the first to estimate experience effects within household, i. e., controlling for any unspecified household characteristics.[3]

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 1.65%-1.90% ($615-$708) decrease. The results are robust to a myriad of robustness checks. For example, we assign more or less weight to unemployment conditions further in the past; 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 data; we explore several units of clustering and double-clustering in the calculation of standard errors; we do or do not include the spouse when measuring past experiences; and we vary the weighting of sample observations, including the use of PSID family weights. All of our results are robust.

In addition, we are able to both expand and replicate our results out-of-sample,

[3] For completeness, we have also estimated a model with only cohort fixed effects, paralleling prior literature. In that case, the identification controls for cohort-specific differences in consumption. The results are very similar to estimations without cohort fixed effects.

employing two additional data sets, the Nielsen Homescan Data and the Consumer Expenditure Survey (CEX). The Nielsen data contains details about the products that households purchase at the Universal Product Code (UPC) level for each shopping trip, which allows us to control more finely for time (year-month) effects. The CEX contains a more comprehensive list of product categories, and thereby more extensively sheds light on the impact of unemployment experience on total, durable, and non-durable consumption. The estimated effects across all three datasets are not only consistent but in fact very similar in economic magnitude.[4]

Second, we document that consumers' past exposure to unemployment significantly affects beliefs years later in their lives. Using the *Michigan Survey of Consumers* (MSC) from 1953 to 2018, we show that people who have experienced higher unemployment rates over their lifetimes report more pessimistic beliefs about their financial situation in the future. They are more likely to believe that it is not a good time to purchase major household items in general. 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. Again, we control 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 appear to 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 consumers with higher past exposure to adverse unemployment conditions become more frugal, we would expect their savings and ultimately their wealth to increase. We confirm this prediction in the data. Using a horizon of three to six PSID waves (6 to 12 years) into the future, we find that a one-standard-deviation increase in personal lifetime exposure to unemployment leads to additional precautionary savings and resulting wealth build-up of about 1.6% or $3,500 ten years later. Similarly, a one-standard-deviation increase in exposure to macroeconomic unemployment in the past results in wealth increases of about 1.5% or $3,100 ten years later. Unob-

[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.

served wealth effects, the main alternative hypothesis of our findings on consumption expenditures, do not predict wealth build up, or predict the opposite.

These four baseline results—a lasting influence of past exposure to unemployment on current expenditures and on consumer optimism, but the lack of any predictive effect on actual future income, plus positive wealth build-up—are consistent with the proposed scarring mechanism: consumers over-weigh past experiences when predicting future realizations. The results are instead jointly inconsistent with several alternative consumption theories, given the controls included: 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. (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.) Moreover, the household fixed effects control for any time-invariant unobserved heterogeneity.

To further distinguish the proposed scarring 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 consumption (and other outcomes) through two types of channels: (a) a "rational" channel of lost income, lower wealth, and worse employment prospects, and (b) an "experience-based channel," which affects only 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 scarring effects through channel (b) from channel (a). We now utilize the Low, 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 it takes time to obtain an offer of the same job-match quality as before unemployment. We extend the Low et al. (2010) model to allow for "unemployment scarring" in the sense that job loss itself may induce a negative, permanent wage shock.[5] We simulate the original and the extended Low et al. (2010) model for both Bayesian and experience-based learners, and estimating the relationship between past unemployment and current consumptions on the simulated data.

The simulations show that, even 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 add unemployment scarring. When consumers are, instead, simulated to overweight their own past experiences, higher unemployment experiences in the past predict lower consumption, consistent with the empirical estimates. 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 helps to alleviate concerns about imperfect wealth controls. When we leave out the wealth control in the estimation, we continue to estimate a positive rather than negative relation between past experiences and consumption in the case of rational consumers, and a negative relation in the case of experience-base learners.

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

Guided by these simulation results, we perform three more empirical steps: (1) a broad range of robustness checks and replications using variations in wealth, income, and liquidity controls, (2) a study of the implications of experience-based scarring for the heterogeneity in consumption across cohorts and for the quality of consumption, and (3) a discussion of aggregate scarring effects on consumption and savings.

First, we replicate the PSID results 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 dummies of income and lagged income, 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 address the concern about measurement error in income by incorporating estimates of the extent of measurement error in income in our regressions and assessing whether they affect our coefficients of interest. 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 a significant effect of past exposure to unemployment on consumption.

We then show that prior exposure to unemployment also affects consumption at the qualitative margin. Exploiting the richness of the Nielsen data, we estimate a significant increase in (i) the use of coupons, (ii) the purchase of lower-quality items (as ranked by their unit price, within product module, market, and month), and (iii) the purchases of on-sale products. For example, households buy 9% more sale items at the 90^{th} than at the 10^{th} percentile of past exposure to unemployment.

Third, we test a prediction that further distinguishes the scarring hypothesis from other learning models, such as overextrapolation or adaptive learning: macroeconomic shocks in the recent past have particularly strong effects on younger cohorts since they make up a larger fraction of the lifetime experiences of younger than older people. As a result, younger cohorts increase their consumption more than older cohorts during economic booms, and lower their spending more during busts. We confirm the prediction for both national and local unemployment experiences, and in both the positive and the negative direction.

Our results imply that longterm scarring effects constitute a novel micro-foundation of fluctuations in aggregate demand and long-run effects of macro shocks. We provide suggestive evidence of these aggregate implications by estimating a strong negative correlation between aggregate lifetime experiences of past national unemployment among the U.S. population and real personal consumption expenditure (PCE) from the U.S. Bureau of Economic Analysis (BEA) from 1965 to 2018. In other words, our results indicate that changes in aggregate consumption may reflect not only responses to recent labor-market adjustments, but also the lingering effects of labor-market crises further in the past. Overall, our findings imply that the long-term consequences of macroeconomic fluctuations can be significant, thus calling for more discussion on optimal monetary and fiscal stabilization policy to control unemployment and inflation (Woodford 2003, 2010).

Related Literature Our work contributes to a rich literature on consumption. Since the seminal work of Modigliani and Brumberg (1954) and Friedman (1957), subsequent variants of the life-cycle permanent-income model have provided more rigorous treatments of uncertainty, time-separability, and the curvature of the utility function (cf. overviews in Deaton 1992; Attanasio 1999). Our approach is complementary to this existing literature as it describes the lingering effects of past unemployment episodes after taking into account other channels captured in the life-cycle framework. It explains why two individuals with similar income profiles and demographics make different consumption choices if they have lived through different personal and macroeconomic employment histories.

Our approach also helps to explain a number of empirical facts that have remained hard to reconcile with the 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). And consumption overreacts to anticipated income increases (excess sensitivity; cf. West 1989, Flavin

1993). Proposed explanations for these puzzles 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).[6] Consumption scarring offers a unifying explanation for both puzzles as it predicts that personal exposure to economic realizations rather than (only) information induces changes in behavior. The lasting impact of lifetime income histories can explain both consumers' lack of response to permanent shocks and their overreaction to anticipated changes.

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, the channel is distinct. Under habit formation, utility is directly linked to past consumption as households suffer a loss of utility if they do not attain their habitual consumption. Under consumption scarring, households adjust their consumption based on inferences drawn from their past experiences, without direct implications for utility gains or losses.

A second strand of related literature analyzes the long-term effects of macroeconomic shocks. One example is the theoretical model of Kozlowski, Veldkamp, and Venkateswaran (2020), in which consumers update beliefs about the probability of crises from macro realizations, and tail events generate persistent drops in economic activity through the channel of belief updating. While the most apparent difference to our paper is that we take the concept of scarred beliefs to the data and estimate statistically and economically significant effects, the conceptual differences are deeper, with important empirical implications. Differently from the channel of information acquisition in (Kozlowski et al. 2020), 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. For example, personal exposure to high inflation earlier in life affects beliefs about future inflation not only among consumers (Malmendier and Nagel 2015), but also among FOMC members (Malmendier, Nagel,

[6] See also Dynan (2000) and Fuhrer (2000) on habit formation.

and Yan 2021), who are presumably very well informed about all inflation-relevant data. The neuroscience-based explanation implies that not only macroeconomic conditions, as highlighted in Kozlowski et al. (2020), but also personal experiences, have a strong impact on consumption expenditures. We confirm this prediction in the data. The experience-based explanation for scarring also implies generational differences in consumers' response to a given macroeconomic shock. We show that on average, younger generations react more strongly to a recent macro shock thank 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). Those constraints are accounted for and tested in existing work Malmendier and Nagel 2011, and we incorporate them here as well.

Our paper relates to a long line of work on adaptive expectations, which also proposes expectation formation based on distributed lags of past realizations (Hansen and Sargent 2008). To the best of our knowledge, we are the first to explicitly embed this type of expectations formation into a precautionary savings model. However, the conceptual differences mentioned above still apply. For example, under consumption scarring, different lengths of past exposure to unemployment generate different beliefs and consumption choices across different generations (e. g., older vs. younger). At the same time, our model is fully *consistent* with this prior literature, which typically only considers a representative agent: If we collapse the different generations into one (median) agent, our predictions for this representative agent tend to match.

On the empirical side, a growing literature documents that macroeconomic shocks scar those individuals who personally live through these conditions. In addition to the labor and political economy literature cited earlier, evidence in line with scarring is found in investors and mutual fund managers who experienced stock-market booms and crashes (Vissing-Jorgensen 2003, Greenwood and Nagel 2009, Malmendier and Nagel (2011)), in CEOs who grew up in the Great Depression (Malmendier and Tate 2005, Malmendier, Tate, and Yan 2011), and in households who experience periods of high inflation (Malmendier and Nagel 2015). Our analysis is the first to examine consumption spending, revealing a novel link between consumption, life-cycle, and the state of the economy. Relative to the existing findings, our paper makes significant progress in terms of identification. The detailed panel data allow us to identify effects using within-household variation. Earlier works such as Malmendier and Nagel (2011, 2015) relied solely on time variation in cross-sectional differences between cohorts, making it hard to disentangle time, age, and cohort effects from experience-based learning. Moreover, we are the first to contrast effects of personal experiences from exposure to macro conditions, which had been the focus of the prior literature, and show personal experiences play a significant role.

In the rest of the paper, we first present the data and measures of consumption scarring (Section II), followed by the four baseline findings on consumption, beliefs, future income, and wealth build-up (Section III). The stochastic life-cycle model in Section IV illustrates the differences between the consumption of Bayesian and (quasi-Bayesian) experience-based learners. Guided by the simulation results, we present additional wealth and income robustness tests in Section V, and show further implications on the cross-cohort heterogeneity in responses to shocks and the quality margins of consumption. Section VI discusses the aggregate implications for consumer spending and concludes.

II Measures and Data

Our empirical analysis aims to test the conjecture that individuals who have lived through difficult economic times, both in terms of personal and in terms of macro-level unemployment, have more pessimistic beliefs about future job loss and income and 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.

The scarring hypothesis also implies that younger cohorts react more strongly to a given shock than older cohorts since this shock makes up a larger fraction of their life histories. Thus, the cross-sectional differences vary over time as households accumulate different histories. Indeed, as shown in Appendix-Figure A.1, the consumption spending of younger cohorts is significantly more volatile and more sensitive to crises such as the Great Recession. We test if this sensitivity reflects stronger scarring effects among the young, rather than only conventional channels such as liquidity constraints.

Measures. We focus on exposure to unemployment as the source of scarring effects, following Coibion, Gorodnichenko, and Hong (2015), who single out unemployment as the most spending-relevant variable. We consider both macro experiences of living through spells of high local and national unemployment, and personal unemployment experiences, controlling for the macro environment. The latter will distinguish the proposed scarring effects most clearly from existing learning models.

An 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 can generate persistent earnings losses for displaced workers (Jacobson, LaLonde, and Sullivan 1993; Couch and Placzek 2010). Indeed, we replicate this result when we estimate earning losses around displacement in the PSID, as shown in Appendix Figure A.2. To distinguish scarring effects from these known earnings implications of job loss, our analyses will control for earnings in the recent past, and we construct all measures of past experiences such that they exclude the recent past. This means that, if the scarring 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 a lag of $k = 2$ 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 t, k, and λ, where λ is a shape parameter for the weighting function. Following the psychology literature on recency bias, we impose higher weights on the more recent past compared to the earlier past. Specifically, using the parsimonious parameterization

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

we linearly declining weights ($\lambda = 1$) and a sharper decline ($\lambda = 3$) to approximate the weights estimated in Malmendier and Nagel (2011, 2015). Economically speaking, this range of λ parameters allows us to capture that, say, in the early 1980s, when the national unemployment rate exceeded 10%, a then 30-year-old was still affected by the experience of living through low unemployment in the early 1970s (around 5-6%), but that this influence was likely smaller than more recent experiences.

We construct personal, national, and local measures of unemployment scarring as defined in (1) and (2), depending on the data set and individual information available.

Data. 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 each household's past exposure to personal and macroeconomic unemployment spells. We replicate the results in the Nielsen and CEX data in Appendix-Sections A.2 and A.3. Compared to those data, the PSID has the advantage of containing 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 information on 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 effects of experiences on consumption for different wealth groups. Indeed, 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 wealth variables in the PSID to be quite similar. The exceptions are "business assets" and "other assets," for which the PSID tends to have lower values. We construct separate controls for liquid and illiquid wealth, using the definitions of Kaplan, Violante, and Weidner (2014). 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 includes 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.

The PSID also records income and a range of 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 head of household 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, including our key explanatory variable of unemployment experiences. 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 at each point in time, calculated as the weighted average as defined in (1) and (2). The PSID allows us to construct measures of both personal and macroeconomic exposure to unemployment, and we can use both national and state-level rates for the macro measure.

To measure personal past exposure to unemployment, we use the reported employment status of the respondent. We first create a set of dummy variables indicating whether the respondent is unemployed at the time of each survey.[7] However, if we were to work with "all available" data to construct personal-specific measures, the values for family units from the later periods would be systematically more precise than those constructed for earlier periods, which only shorter histories recorded in the data. To avoid biasing the estimates, we have to trade off restricting the analysis to later periods such that all family units included in the analysis have sufficient

[7] The PSID reports eight categories of employment status: "*working now*," "*only temporarily laid off*," "*looking for work, unemployed*," "*retired*," "*permanently disabled*," "*housewife; keeping houses*," "*student*," and "*other*." We treat "*other*" as missing, "*looking for work, unemployed*" as "unemployed," and all other categories as "not unemployed." One caveat is that the PSID is biennial during our sample period. For all gap years t, we assume that the families stay in the same state and have the same employment status as in year $t-1$. Alternatively, we average the values of $t-1$ and $t+1$, shown in Appendix A.

personal employment status data and ensuring sufficient history to construct a reliable experience measure. Given this limitation, we combine the personal-experience indicator variables from year $t-6$ to $t-2$ and national unemployment rates from birth to $t-7$, with weights calculated as specified 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.[8]

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. However, the oldest heads of household in the survey waves we employ 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] We thus face the same data limitations as in the construction of the personal experience measure regarding the earlier years in the lives of older cohorts. 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). As we will see, the

[8] An alternative, widely cited source of 1890-1940 data is Lebergott (1957, 1964). Later research has identified multiple issues in Lebergott's calculations and has sought to modify the estimates to better match the modern BLS series. Romer (1986) singles out two of Lebergott's assumptions as invalid and generating an excessively volatile time series: (1) that employment and output move one-to-one in some sectors and (2) that the labor force does not vary with the business cycle. Coen (1973) finds that both armed forces and cyclical variations in average hours/worker have been ignored in previous studies, and these variables appear to have significant effects on measures of labor participation.

[9] There do not appear to be reliable sources of earlier historical unemployment data for all US states. The state-level BLS rates are model-based estimates, controlled in "real time" to sum to national monthly (un)employment estimates from the Current Population Survey (CPS). While it is possible to construct estimates of state-level unemployment using the pre-1976 CPS, we do not do so to avoid inconsistencies and measurement error.

estimation results are very similar under all three macro measures, national, regional, and combined. We will show the combined macro measure in our main regressions whenever geographic information on the individual level is available.

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.

The average age of the head of household is 49, with an interdecile range (IDR) of 35 to 66 years of age, capturing various life-cycle stages. Average household consumption amounts to \$37,195 (in 2017 dollars), and of the average wealth of \$212k about 13% is liquid. 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 $\lambda = 1$ to $\lambda = 3$. The values are somewhat higher 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%.

[10] Controlling for lagged income, the actual minimum age becomes 27. We also conduct the analysis on 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.

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

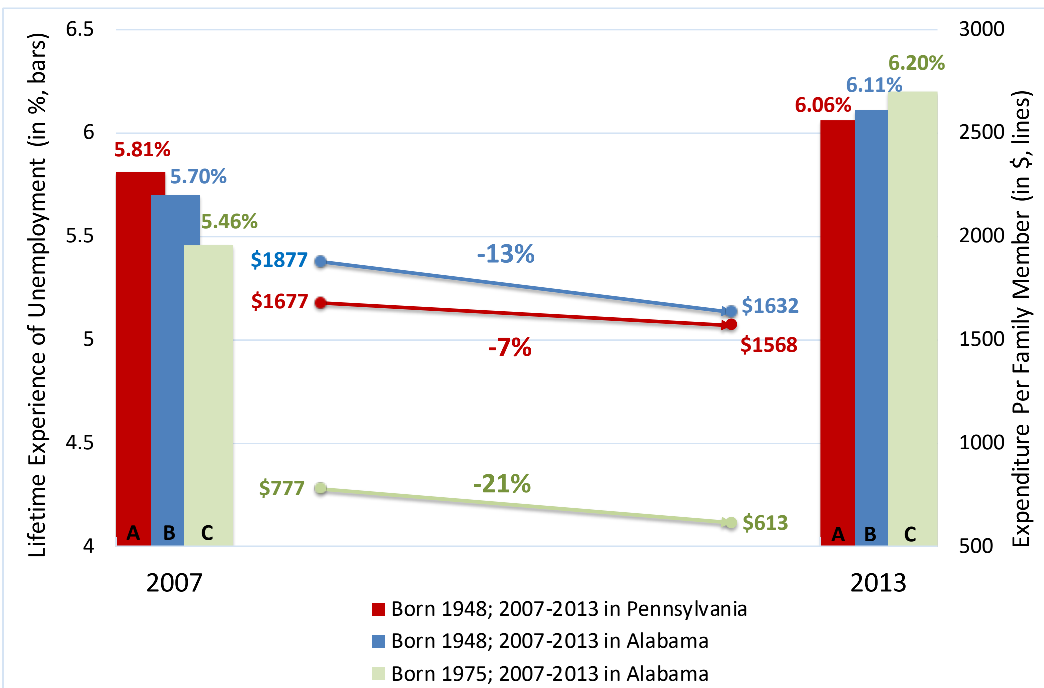
Variable	Mean	SD	p10	p50	p90	N
Age	49.48	11.49	35.00	48.00	66.00	33,263
Household Size	2.74	1.44	1	2	5	33,263
Household Total Consumption [$]	37,195	25,072	15,055	33,110	63,204	33,263
Household Total Income [$]	65k	35k	23k	58k	116k	33,263
Household Liquid Wealth [$]	27k	184k	-22k	0.1k	70k	33,263
Household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

Identifying Variation. We illustrate the key sources of identifying variation that we exploit in our empirical analyses with a simple example of the unemployment exposure and its relation to household 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 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 relates these differences-in-differences of lifetime experience over the crisis period to consumption behavior. The increase in unemployment experiences of Person 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%, respectively. 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.

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.

III Baseline Results

Our analysis starts from the observation that macro shocks appear to have a long-lasting impact on consumer behavior and that the puzzling persistence of reduced consumer expenditures correlates with consumer confidence remaining low for longer than standard models would suggest (Pistaferri 2016). We test whether we can better predict consumer beliefs and consumer behavior if we allow for a lingering influence of past exposure to adverse economic conditions, as implied by the cognitive science literature on the formation of neural pathways in the brain, and consistent with existing macro, labor, and finance literature on the longterm scarring effects of recessions. We focus on unemployment as the most spending-relevant economic condition (cf. Coibion et al. (2015)), both on the personal level and on the aggregate level (unemployment rates). We then show that individuals' past exposure to unemployment has a measurable, lasting effect on their consumption expenditures and on their beliefs, but fails to predict (lower) future income or future wealth.

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:

$$C_{it} = \alpha + \psi UEP_{it} + \beta UE_{it} + \gamma^{'} x_{it} + \eta_t + \varsigma_s + \upsilon_i + \varepsilon_{it}. \quad (3)$$

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 very recent past experience and calculated as shown in equation (1); x_{it} is a vector of controls including wealth (first- and second-order logarithm sof 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 (ranging from 0 to 17), marital status, and race (White, African American, Other); η_t are time (year) dummies, ς_s state dummies, and υ_i household dummies.[12]

[12] The results are very similar when we include region$*$year fixed effects. One may consider fully saturating the model with state$*$year fixed effects, to control for unspecified determinants of consumption that affect consumers differently over time and by state. (Note that those alternative

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 for some time periods 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 and year effects soak up all variation from the current aggregate unemployment rates. The inclusion of household fixed-effects fully controls for any unspecified time-invariant household characteristics and implies that we identify scarring effects solely from time variation in the within-household co-movement of consumption and unemployment histories. Thus, our identification relies on three margins of variation: People differ in their prior exposure to unemployment depending on their cohort and location at each given point in time, and these cross-sectional differences evolve over time.

In Sections IV and V, we will probe the identifying assumption by addressing further variants of the neoclassical model, such as "income scarring" and "unemployment scarring," as well as a myriad of robustness checks to address noise and measurement errors in the wealth and income variable, including estimates of the extent of measurement error. We will also discuss how the inclusion of income and wealth controls is, in fact, "overcontrolling" for the rational channels.

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.

Results Table 2 shows the estimation results from model (3). Columns (1)-(3) show results using 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 estimated coefficients on the control variables have the expected sign, con-

determinants would need to affect consumption exactly in the direction of the scarring hypothesis, including the different strength of scarring among younger and older people.) Since one of the key margins of variation in macroeconomic unemployment experience (UE_{it}) is at the state∗year level, state∗year fixed effects would absorb much of the variation, resulting in insufficient statistical power to precisely estimate coefficients. Instead, we have estimated the model controlling for current state-level unemployment rates as a sufficient statistic. The results are similar.

sistent with prior literature. All coefficients of interest on personal and aggregate unemployment experiences are negative whether included separately (in columns 1–2 and 4–5) or jointly (in columns 3 and 6). In other words, both the exposure to periods of personal unemployment experiences and high aggregate employment rates have a significant and lasting impact on spending behavior 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.

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)		-0.058***	-0.057***		-0.034***	-0.034***
		(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.

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 in annual spending. Similarly, a one-standard-deviation increase in macroeconomic unemployment experience leads to a 1.65% decrease in consumption, which translates to \$615 less annual spending. The estimates using experience measures based on $\lambda = 3$ weights are slightly bigger. As shown in column (6), a one-standard-deviation increase in personal and macroeconomic unemployment experience leads to a 1% and 1.90% decrease in consumption, respectively, which translates to \$370 and \$708 less annual spending. The magnitude of the macro experience coefficients 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 Appendix-Table A.2, the coefficients on macroeconomic and personal unemployment experiences become both larger (in absolute value) and more statistically significant.

We also confirm robustness to several variations in the construction of the key explanatory variable. 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). In terms of alternative approaches to calculating standard errors, we estimate regressions with 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. (We do not use PSID family weights in the main regression due to the usual efficiency concerns.)

Moreover, we replicate the estimations using two alternative consumption data sets, the Nielsen and 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 goods, nondurable goods, as well as total consumption that encompasses additional categories of expenditures. Since neither the Nielsen nor the CEX provides information on where households resided prior to the sample period, nor on 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 the monthly frequency for the Nielsen data and at the 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-Section (A.2) and Appendix-Section (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

Given the robust findings of a negative and significant relationship between consumers' lifetime exposure to past unemployment conditions and their consumption behavior, we turn to explore the channels through which past exposure might scar consumers. We start from consumer expectations: To what extent do personal and macroeconomic unemployment conditions in the past color beliefs about future outcomes? And how do these changes in beliefs relate to actual future realizations?

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

Among the multitude of belief elicitations, we identify two questions that capture expectations about economic conditions 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 expenditures for (durable) consumption items and individuals' current attitudes towards buying such items: "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?" For the empirical analysis, we construct two binary dependent variables. The first indicator takes the value of 1 if the respondent expects better or the same personal financial conditions over the next 12 months and 0 otherwise. The second indicator is 1 if the respondent assesses times to be good or the same for durable consumption purchases and 0 otherwise. Note that the first question asks respondents about their beliefs regarding their personal situation, while the second question is about their beliefs regarding the macroeconomic situation.

The explanatory variable of interest is again a measure of past exposure to unemployment. Since the MSC does not reveal the geographic location of survey respondents, we apply equation (1) to the national unemployment rates to construct the "Experience (Macro)" variable for each of individual i at time t. Mirroring the construction of the PSID measure, we account for all experiences from birth until year $t-2$, and apply weighting function (2) to each individual's vector of past experiences. We construct the measure for each respondent at each point in time during the sample. We also extract income and all other available demographic variables, including education, marital status, gender, and age of the respondent.[13]

We regress the indicators of a positive assessment of one's future financial situation or a positive buying attitude on past unemployment experiences, controlling for current unemployment, income, demographics, age fixed effects, and year fixed effects. Year fixed effects, in particular, 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 relate our scarring measure of prior unemployment-rate experiences to respondents' forecasts of their own future situation. We find that people who have experienced times of higher unemployment in the past are significantly more pessimistic about their future financial situation. The statistical and economic sig-

[13] The MSC does not make information about race available anymore via their standard data access, the SDA system (Survey Documentation and Analysis), 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.

nificance 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, and the same holds for demographics that might proxy for unobserved wealth (e. g., education). In all cases, the coefficient of past unemployment experiences remains highly significant and negative.

In terms of the economic magnitude, we can consider the inter-decile range of lifetime experiences: Respondents at the 90th percentile are around 2 percentage points more likely to say financial conditions will be worse in the next 12 months than respondents at the 10th percentile.

The estimations based on the second question, which is about the economy more broadly rather than the consumers' personal situation, are shown in columns (4) to (6). The results 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 percentage points 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 and other unobserved 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 more broadly.

Our results suggest that the economic conditions individuals have experienced in the past have a lingering effect on their reported beliefs about the future. Individuals who have lived through worse times consider their own financial future to be less rosy and times to be generally bad for spending on durables, controlling for current economic conditions, individual unemployment, income, age and other demographics.

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

Evaluating preference-based mechanisms is tricky as there are many possible specifications. In fact, it is impossible to conclusively reject the instable-preferences explanation. As in the case of the beliefs-based channel, we can at best aim to provide evidence in favor of specific formalizations. Here, we explore one preference specification that has garnered significant support in prior empirical literature: habit formation. We study whether the significant relationship between consumption and past exposure to unemployment is correlated with persistent habits in consumption.

To that end, we estimate an alternative version of the empirical model from (3) that includes a lagged consumption measure on the right hand side. 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). Accordingly, both level and differenced equations are used, and the lagged dependent variable is instrumented using lagged differences for the level equation and lagged levels for the differenced equation.[14] The goodness of fit statistics for the system GMM estimators are calculated as the square of the correlation coefficients between the actual and the fitted values of the dependent variable.

We present the results in Appendix-Table A.7. The estimates show that the effects of prior personal unemployment experience on consumption remain significant after taking into account possible habit persistence in consumption, while macro experience appear less significant. Overall, the estimation results both confirm the robustness of experience effects, especially those attributed to personal unemployment, and indicate that they do not operate through the channel of habit formation.

[14] Note that 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.

III.C Future Income

Having established a negative relation of past exposure to unemployment with consumption, as well as a negative relation with consumer optimism, we now ask whether past unemployment experiences also predict lower future earnings or worse economic conditions that would merit reduced spending and pessimistic beliefs. Can we explain consumer behavior as the response to lower employment and earnings prospects? Might the consumer pessimism be explained by unobserved determinants of households' future income that are correlated with past unemployment experiences?

In Table 4, we re-estimate our baseline model from equation (3) with the dependent variable changed to future income. Going from left to right, the columns show the estimation results for predicting 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 weight shifted to recent observations (λ=3), shown in the bottom panel.

As the table reveals, neither the proxy for the scarring effects of past personal unemployment experiences nor the proxy for scarring effects due to past macro exposure are predictive of future income over any of the horizons and for any specification. After controlling for income, wealth, employment status, the other demographics, and fixed effects,[15] the estimated coefficients of personal and macroeconomic unemployment experiences are all small and insignificant. In summary, our results imply that past exposure to unemployment does not predict future earnings prospects, conditional on current income, wealth, and demographics.

Relatedly, one may ask whether past unemployment experiences affect the volatility of future income. Even if expected income is unaffected by past experiences, a consumer might (correctly) perceive the variance of income to be affected. If consumers feel greater uncertainty about the stability of their future employment, they will save more to mitigate risk and thus consume less as a result. To test if such a relationship between unemployment experience and income volatility exists, we re-estimate our baseline model (3) again, this time using income volatility as the dependent variable. Following Meghir and Pistaferri (2004) and Jensen and Shore

[15] 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 friction.

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	-0.001	0.098	0.128	-0.026	0.052
	(0.061)	(0.065)	(0.079)	(0.095)	(0.124)
Experience (Macro), λ=1	-0.007	-0.010	-0.001	-0.006	-0.000
	(0.013)	(0.014)	(0.016)	(0.022)	(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	0.076	-0.014	0.019
	(0.035)	(0.038)	(0.047)	(0.055)	(0.071)
Experience (Macro), λ=3	-0.004	-0.006	-0.002	-0.003	0.002
	(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.

(2015), we construct volatility measures for both the transitory and the permanent income. The transitory income-variance measure is the squared two-year change in excess log income, where excess log income is defined as 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 both measures, permanent and transitory, 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 estimate 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 long-term effects of past unemployment experiences on consumption, and the lack of any relation with future income, imply that past exposure to unemployment could also affect the build-up of wealth in households. If consumers with high past exposure to unemployment restrain from consumption expenditures 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.

In order to test whether experience effects are detectable in long-run wealth accumulation, we relate households' past unemployment experiences to their future wealth, using up to six survey waves (12 years) into the future. We note that this analysis also ameliorates potential concerns about the quality of the consumption data (as it is eliminated from the estimation) and about alternative life-cycle interpretations of our findings (as a positive correlation of past unemployment experiences and future wealth directly contradicts unobserved illiquidity or other financial constraints as an explanation).

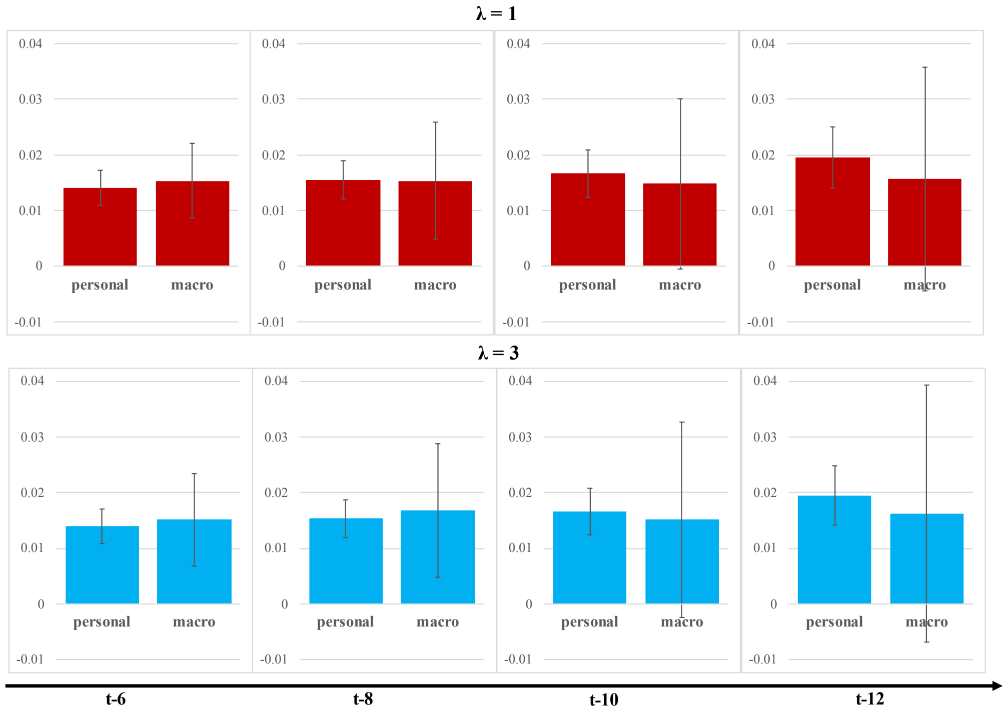
Figure 2 summarizes graphically the coefficients of interest from eight regressions, namely, the estimations predicting wealth at $t+6$, $t+8$, $t+10$, and $t+12$. The upper part shows the effects of a one-standard-deviation increase in experience on total wealth, constructed with the linearly declining ($\lambda = 1$) weighting scheme. The

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.

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.

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 estimates of the role of past exposure to personal unemployment imply a large economic magnitude: a one-standard-deviation increase in personal lifetime experiences of unemployment leads to additional precautionary savings and resulting wealth build-up of about 1.6% or $3,500 ten years later, using the $\lambda = 1$ weighting scheme. The estimates of macro experiences imply that a one-standard-deviation increase in macroeconomic lifetime experiences of unemployment leads to additional precautionary savings and resulting wealth build-up of about 1.5%

or \$3,100 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 shown that individuals' past exposure to unemployment strongly predicts their consumption expenditures, and beliefs about future economic conditions appear to play a role in explaining this result. However, such beliefs do not seem to be 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 Model with Simulate-and-Estimate Exercises

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 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.

In this section, we utilize the Low, Meghir, and Pistaferri (2010) model to account for an even broader array of standard life-cycle consumption factors and possible confounds. We simulate the Low et al. model both for standard rational agents, as in the original model, and for agents whose beliefs are scarred by past unemployment experiences. The simulations illustrate the basic mechanism of experience-based (quasi-Bayesian) learning and distinguishe it from standard Bayesian beliefs. We then re-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. The simulate-and-estimate exercise helps clarify the orthogonality conditions underlying our identification and illustrates that, even directionally, the standard model does not generate the negative relation between past unemployment and consumption expenditures that we saw in the data.[16]

The focus of Low et al. is on the interaction of different types of risk (productivity shocks, employment risk) with social insurance (unemployment insurance, food stamps, and disability insurance). While the social-insurance programs are not the focus of this paper, they add richness to our analysis and ensure that the experience-effect estimates are not confounded. Their model also allows for "income scarring," i. e., the notion that job loss reduces income flows because of lower match quality in future jobs. Moreover, we extend the Low et al. model to capture another form of rational scarring effects, which we dub "unemployment scarring." The latter captures the notion that unemployment, once experienced, makes individuals inherently less employable. Hence, the extended Low et al. framework allows us to distinguish both income and unemployment scarring as well as other life-cycle features from scars from past experiences. As we will see, it helps to illustrate that, for a wide range of parameterizations, we can distinguish experience effects even directionally as the negative relationship between past exposure to unemployment and consumption spending does not emerge for rational learners (after controls). It also provides guidance towards empirical robustness checks and additional tests. We follow Low et al. (2010) in restricting the analysis to personal unemployment experiences, which are the experiences most prone to confounds such as unobserved financial constraints, liquidity, or earnings prospects.

Low, Meghir, and Pistaferri (2010) Model Setup. Consumers can work for 40 years, until age 62 (starting at age 23), then have mandatory 10 years of retirement where they receive social-security benefits and die at the end of retirement. Periods are quarters, amounting to $L = 200$ periods of consumption and labor decisions in total. 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

[16] We are not implementing a full simulation with the goal of estimating structural parameters, but aim to investigate the mechanism of experience-based learning as well as possible confounds.

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 $c_{i,t} < A_t$, which rules out borrowing. As we will see below, by thus maximizing the financial constraints of consumers, we are able to derive the sharpest distinction between the role of experience effects and financial constraints.[17] We assume that flow utility takes a near CRRA form which induces a precautionary savings motive. (A detailed description of the intertemporal budget constraint and the social-insurance programs is in Appendix B.)

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 B.

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 Wage. The deterministic component of wage $d_t + x'_{it}\psi$ is the same for all individuals of a given age at time t. The size of

[17] 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 in their borrowing ability and are predicted to react more strongly to a shock than older cohorts under the experience-effect hypothesis. By eliminating borrowing altogether from the simulation, we maximize the impact of financial constraints.

this component is estimated via regression in Low et al. and of the form[18]

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

The Permanent Component of Wage. The stochastic 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.

The Job-Match Component of Wage. A key element of the Low et al. model is its job-match process. The consumer-firm 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.

Job Arrival. 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 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 reduce their consumption beyond this bar.

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.

[18] 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.

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

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

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

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

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

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

Model Estimates on Experience Effects in Consumption. We simulate the consumption-saving decisions for both rational and behavioral consumers using the parameters in Table 6.[19] 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.

[19] The full list of parameters is in Appendix-Table B.1.

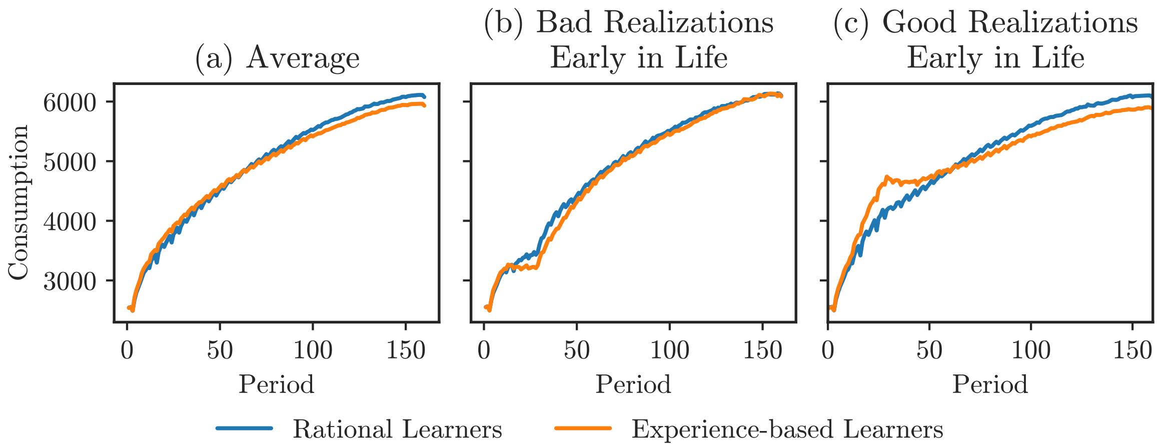
In Figure 3, we illustrate the resulting consumption paths for both rational and experience-based learners. Panel (a) depicts the average consumption trends of both types of agents during their working years (the years used in the regressions), and provides a first hint as to the differences in consumption patterns. Relative to the darker (blue) plot of the rational benchmark, the lighter (orange) plot of the consumption path of 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 destruction, spend more, and must then save more towards the end of their working life.

Panels (b) and (c) provide amplified illustrations of these differences by zooming in on consumers who were "lucky" and "unlucky" early in life in terms of their earnings. In panel (b), we consider the subset of rational and experience-based learners 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. We plot the consumption path of these experience-based consumers against that of rational agents with the same "lucky" earnings realizations early in life. For these experience-based consumers, the trend of over-consumption in the early periods and financial constraints later in life is much more pronounced.

Panel (c) illustrates the opposite scenario. Here, we consider 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 life-cycle. This plot illustrates the prediction of wealth build-up due to excess frugality, which mirrors the empirical relationship we found in Section III.D.

Using the simulated values, we estimate the relationship between consumers' past unemployment experiences and consumption behavior, controlling for income and wealth. The corresponding OLS regressions are in Table 7 columns (1) and (2), for Bayesian learners, and columns (3) and (4), for experience-based learners. In the case of Bayesian agents, prior experiences do not actually enter their belief formation. The purpose of including the experience measure here is to identify

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



Notes. Figure (a) shows 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. Figure (b) shows 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 and then restricted to those simulations where agents have, or in the rational case would have, a believed delta of 0.025 or less at period 30. Figure (c) shows 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 and then restricted to those simulations where agents have, or in the rational case would have, a believed delta of 0.1 or greater at period 30.

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 one model where we do not include wealth as a control (column 1) and one where we include wealth (column 2). In both cases the experience-effect proxy is included to test whether it might pick up unobserved wealth effects. We conduct the estimations both on the data simulated using linearly declining weights for the measure of prior experiences ($\lambda = 1$), shown in the upper half of the table, and on the data simulated using weights with more recency bias ($\lambda = 3$), shown in the lower half of the table.

Income scarring. Starting from the estimation with linearly declining weights, shown in the upper half of Table 7, we find that income has the expected positive sign and is significant across all specifications, as does wealth when it is included. More noteworthy is that, when using the simulations with rational agents (columns 1 and 2), we estimate a *positive* coefficient of the experience measure, indicating that higher unemployment experiences predict higher consumption. This is the opposite

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

	(1) Rational	(2) Rational	(3) EBL	(4) EBL
$\lambda = 1$:				
Income	0.569	0.383	0.604	0.396
	(224.41)	(64.91)	(197.99)	(56.76)
Wealth		0.264		0.265
		(51.67)		(58.79)
Unemployment Experience	0.350	0.692	-0.071	-0.466
	(9.56)	(5.21)	(-1.70)	(-6.43)
$\lambda = 3$:				
Income	0.578	0.387	0.619	0.400
	(212.06)	(62.44)	(163.54)	(53.40)
Wealth		0.261		0.271
		(52.44)		(66.27)
Unemployment Experience	0.565	0.575	0.133	-0.274
	(8.50)	(5.80)	(6.49)	(-6.62)

Notes. Estimations with simulated consumption values as the dependent variable and simulated same-period income and wealth as regressors, for rational consumers in columns (1) and (2), and experience-based learning (EBL) consumers in columns (3) and (4). 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.

of what we find empirically, and a first step towards ameliorating concerns about confounds: It appears to be hard to (falsely) estimate a negative experience effect when agents are rational, whether or not we include perfect wealth controls.

When we alter the belief-formation process to experience-based learning, instead, 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 to other control variables in the simulation exercises.

The contrast between the positive experience-effect estimates in the data simulated with rational agents, and the negative estimates in the data simulated with experience-based learners addresses concerns about possible confounds in our previous estimations. The positive sign in the data simulated for rational agents also 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 among 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, we learn that there is little concern about confounding experience effects with traditional life-cycle determinants, including (unobserved) wealth effects and income scarring, as long as we control for current income. Second, we learn that 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). If consumers are influenced by their past experiences, our estimates are a conservative lower bound on the true scarring effects.

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 $\lambda = 1$ in the upper half of the table is the positive coefficient in column (3). It indicates that, even if consumers are truly scarred by their past experiences, one might fail to detect the experience effect 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.

Table 8: **Estimations with Model-Simulated Data, Unemployment Scarring**

	(1) Rational	(2) Rational	(3) EBL	(4) EBL
$\lambda = 1$:				
Income	0.417	0.287	0.465	0.348
	(85.82)	(85.82)	(116.24)	(68.09)
Wealth		0.315		0.261
		(55.26)		(31.48)
Unemployment Experience	-0.293	0.419	-1.522	-1.733
	(-4.47)	(5.44)	(-17.46)	(-21.81)
$\lambda = 3$:				
Income	0.423	0.288	0.484	0.358
	(94.37)	(124.22)	(129.11)	(63.42)
Wealth		0.310		0.267
		(57.67)		(36.27)
Unemployment Experience	0.129	0.330	-1.197	-1.427
	(9.26)	(6.39)	(-20.64)	(-27.83)

Notes. Estimations with simulated consumption values as the dependent variable and the simulated same-period income and wealth as regressors for rational consumers. The simulations account for unemployment scarring. Consumption, income, and wealth are in log terms. Estimations are for $\lambda = 1$ in the top panel and $\lambda = 3$ in the bottom panel. 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.

Unemployment scarring. As a last step, we introduce additional negative correlation between unemployment and future income ("unemployment scarring") as a potential alternative explanation for the estimated experience effect. 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, Meghir, and Pistaferri (2010), albeit with the difference that wage gains lost due to "income scarring" can be regained by working for an extended period.[20] 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. We re-simulate the model with this additional, permanent effect of job loss on income and then re-estimate the specifications of Tables 7.

As shown in Table 8, the specification with rational learners and without wealth controls (in column 1) shows a negative correlation between unemployment experience and consumption for $\lambda = 1$, which becomes positive for $\lambda = 3$. Once we control for wealth (in column 2) the experience coefficient is positive in both cases, for $\lambda = 1$ and for $\lambda = 3$. For simulations with experience-based learners, instead, all four estimations produce a significantly negative coefficient, whether or not we control for wealth and whether we assume more or less recency bias (λ). That is, in contrast to Table 7, we now estimate a negative coefficient on unemployment experience for behavioral learners whether or not we control for wealth.

Note that, compared to Table 7, 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 ("unemployment scarring").

Overall, the estimation results in Table 8 reveal that, in most scenarios, a negative coefficient of past unemployment experiences indicates actual experience-based scarring, even if consumers' income and consumption are also affected by both "income scarring" in the sense of Low et al. (2010) and "unemployment scarring" as in Jarosch (2015). If consumers are rational learners, the resulting coefficient estimate is typically positive, which is the opposite of what we find empirically. The one exception is the scenario in which we construct the experience effect variable with

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

relatively low recency bias ($\lambda = 1$) and do not control for wealth effects. In that case, we might (mis-)estimate a negative experience effect for a rational learner since a person with more unemployment experiences in the past might accumulate less wealth and thus consumes less. Compared to Table 7, where we do not observe this confound, the addition of "unemployment scarring" drives the change in this result by reducing the probability of a high permanent component given recent unemployment experience. However, once we control for asset accumulation (in column 2), we re-estimate a positive coefficient on unemployment experiences, with coefficients similar to the case without "unemployment scarring."

Summarizing the insights from all 16 estimations in Tables 7 and 8, the simulate-and-estimate exercise provides evidence that financial constraints, unobserved wealth factors, income scarring, and unemployment scarring individually fail to generate a negative relation between past unemployment experiences further and current consumption when agents are Bayesian learners. Instead, a negative coefficient estimate likely indicates scarring effects, as we show using empirically validated parameterizations of experience effects (with linearly declining or steeper weighting functions). 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, this scenario is unlikely to apply. Still, to address remaining concerns, we will conduct exhaustive 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. We will also use the model to generate additional predictions of the experience-effect model that are not generated by alternative interpretations.

V Model Validation and Further Implications

Guided by the theoretical model, we re-estimate the consumption model with a battery of alternative and additional wealth, income, and liquidity controls using the PSID data. Then, building on the robust estimation results, we study two further implications of our model of scarring effects, regarding age heterogeneity and regarding the quality of consumption.[21]

V.A Wealth, Income, and Liquidity

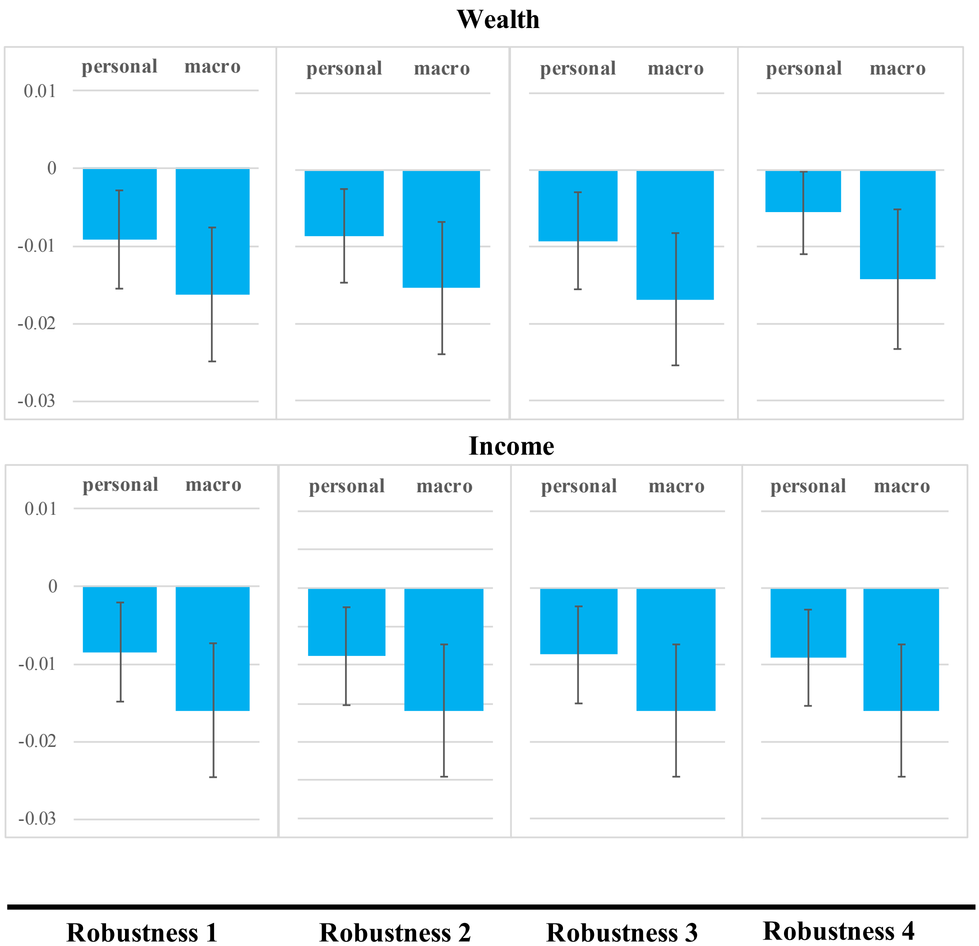
We start from concerns about imperfect measurement of individual wealth. Our simulate-and-estimate exercise in Section IV alleviates these concerns, as it appears hard to generate misattribution when employing empirically validated scarring proxies for experience effects and given the controls for unemployment status and income – even in the presence of such mismeasurement. Moreover, our prior results on future wealth build up, future income, and beliefs are also 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 in the top panel of Figure 4. The indications for "Robustness 1" to "Robustness 4" refer to variations (1)-(4) above, and the bars shown below "personal" and "macro" refer to the estimated effects of personal and macroeconomic scarring 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

[21] 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.

Figure 4: **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 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.

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. The implied economic magnitudes are shown in the bottom panel of Figure 4.

Next, we take an additional step in addressing the concern of measurement error in the income variable, following the approach of 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.[22] 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.[23] 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 related to the role of liquidity. Even though the results are robust to variations in wealth measures, might the estimated experience effect still be confounded with (unmeasured) liquidity constraints? The concern is that liquidity might be correlated with past exposure to unemployment, generating lower

[22] Romer (1986) and Cogley, Sargent, and Surico (2015) assess the significance of measurement error in pre-war unemployment and inflation data, respectively. They add noise to the higher-quality postwar unemployment and price-level series based on properties are consistent with lower-quality prewar data and assess whether the extend of measurement error affect results.

[23] The PSID validation study used in Bound, Brown, Duncan, and Rodgers (1994) is from a two-wave panel survey of a sample of workers from a large manufacturing company. The survey, which took place in 1983 and 1987, asked about annual earnings, hours, and tenure parallel those used annually for the PSID. With access to detailed company records, the authors were able to obtain virtually error-free validation of measures of earnings and hours worked. Subsequent studies, such as Meghir and Pistaferri (2004) and Blundell, Pistaferri, and Saporta-Eksten (2016), have used estimates from Bound, Brown, Duncan, and Rodgers (1994) to set a priori the amount of income variability that can be attributed to measurement error.

spending in response to bad experiences and higher spending in response to good experiences. Our separate controls for liquid and illiquid wealth, both in the baseline estimations 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, as proxied by their low liquid-assets position, is more affected by their unemployment experience. We closely follow the practice in the consumption literature, such as Johnson, Parker, and Souleles (2006) and Parker, Souleles, Johnson, and McClelland (2013), and, for each year, sort households into two groups based on whether their liquid wealth lies above or below the median liquid-wealth level in the sample. Expanding equation (3), we interact an indicator for being in the below-median group 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 on average. However, their consumption expenditure does not exhibit a significantly stronger reaction 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.

V.B 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. 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.[24]

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.[25]

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:

$$C_{it} = \alpha + \beta UE_{it} + \kappa U_{mt} + \gamma^{'} x_{it} + \eta_t + \varsigma_m + \upsilon_i + \varepsilon_{it}. \tag{11}$$

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).[26] As before UE_{it} denotes the lifetime (macro) experience of unemployment rates based on a weighting scheme of $\lambda = 1$.[27] 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

[24] 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.

[25] 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.

[26] 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.

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

Table 9: **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.

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} 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 9 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.

V.C 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 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.

We test this prediction using the Nielsen data set from the previous subsection.[28] 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 9. 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 10. 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 unemploy-

[28] 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 10: **Age-Heterogeneity in Reaction to Unemployment Fluctuation**

	(1) Δln(C)	(2) Δln(C)	(3) Δln(C)	(4) Δln(C)	(5) Δln(C)	(6) Δln(C)
Age * Δln(National unemp-down)	-0.023***	-0.023***			-0.021***	-0.021***
	(0.005)	(0.005)			(0.005)	(0.005)
Age * Δln(National unemp-up)	-0.006***	-0.007***			-0.001	-0.000
	(0.002)	(0.002)			(0.002)	(0.003)
Age * Δln(Local unemp-down)			-0.002*	-0.003**	-0.001	-0.002
			(0.001)	(0.001)	(0.001)	(0.001)
Age * Δln(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.

ment 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 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.[29] Thus, our findings highlight experience effects as a distinct force in affecting people's consumption behavior.

[29] To show this directly, we estimated a set of regressions that augments the specifications from Table 10 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.

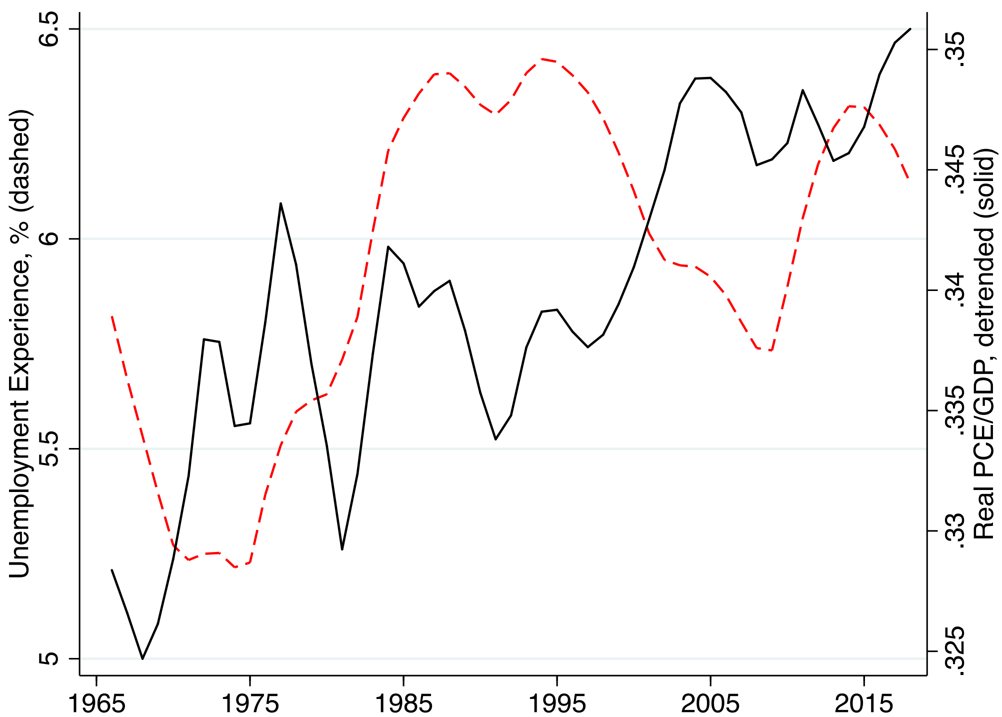
VI Aggregate Implications and Conclusion

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

Estimation results from three different data sources confirm this prediction. 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, and tend to choose lower-quality items. We further show that beliefs about one's future financial situation become pessimistic, consistent with the consumption behavior, 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. While a thorough investigation of the macroeconomic implications of experience effects is beyond the scope of this paper, we provide some suggestive evidence on the aggregate level. We relate an aggregate measure of the population-wide exposure to past unemployment spells in the U.S. to aggregate consumption expenditures from 1965 to 2018. For the former measure, we calculated a weighted average of past national unemployment, as defined in equation (1), for every age group (cohort) from age 25 to 75 and average by population weights (from the Census), separately for every sample year. For aggregate consumer spending, we use data on real personal consumption expenditure (PCE) from the U.S. Bureau of Economic Analysis (BEA) normalized by real gross domestic product (GDP). As shown in Figure 5, there is a clear negative relationship between the two measures: times of higher aggregate unemployment experience coincide with times of lower aggregate consumer spending. The strong negative correlation pattern not only adds credibility to our micro-level estimates but also suggests the possibility that past labor market conditions to which

Figure 5: **Aggregate Unemployment Experience and Consumer Spending**



Notes. Aggregate unemployment experience calculated as a weighted average of national unemployment experience, as defined in Equation (1), with the weights being U.S. population by age (restricted to age 25 to 75) from the Census. Aggregate consumer spending is measured as real personal consumption expenditure (PCE) from the U.S. Bureau of Economic Analysis (BEA) normalized by real gross domestic product (GDP), detrended by removing a linear time trend from the series.

the current population has been exposed are a significant granular source of aggregate fluctuations. Scarring effects appear to significantly dampen macroeconomic fluctuations, which in turn calls for considerations from policy-makers on optimal stabilization policy, monetary or fiscal.

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

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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 plot the time-series of monthly household expenditures by age group, expressed as deviations from the cross-sectional monthly means, using the Nielsen data. The figure shows that the spending of younger cohorts is more volatile in general and was significantly more negatively affected by the Great Recessionthan those of other age groups.

In Appendix-Figure A.2, 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 $y_{it} = \alpha_i + \gamma_t + \sum_{k \geq -m} D_{it}^k \delta_k + x_{it}\beta + \epsilon_{it}$, 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.

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.

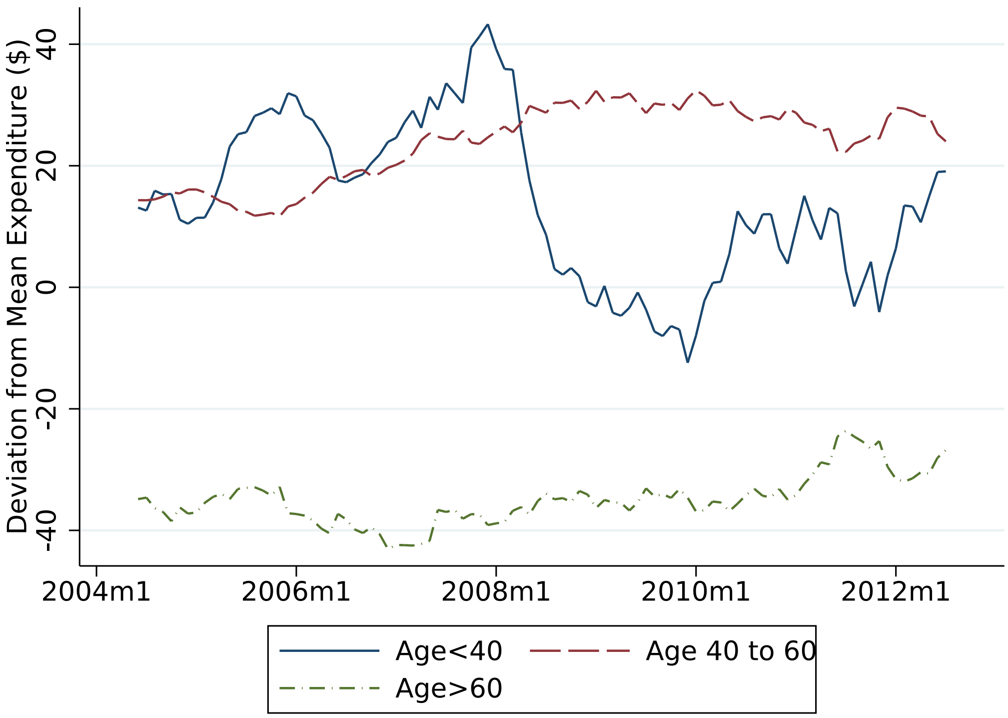
Table A.10 addresses the concern about measurement error in the income variable by incorporating estimates of the extent of measurement error into the income variable and assessing whether they affect our results, following the methodology in Romer (1986) and Cogley, Sargent, and Surico (2015). We apply 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).[30] We find a significant role of past experiences for the build-up of wealth.

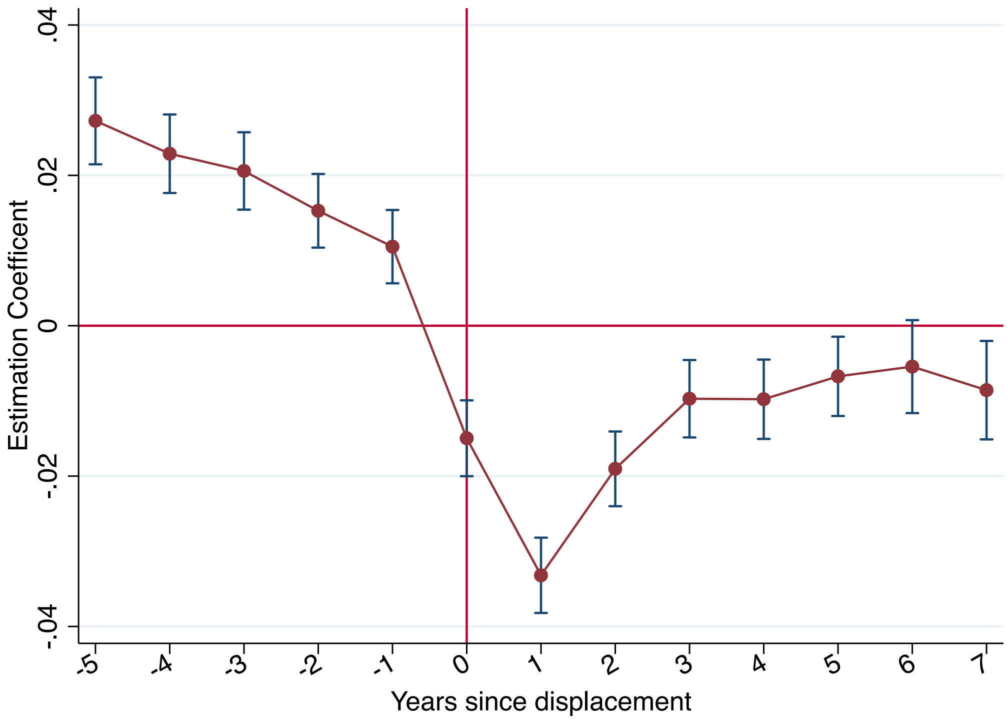
[30] The results are similar if, instead of having an average-income control, we include the incomes for all years between year $t-2$ and the year in which the lagged experience measures are based on.

Figure A.1: **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.

Figure A.2: **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.

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. 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. A detailed description of the dataset is provided in Section V.B. 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 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

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.

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.[31] 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.[32]

Table A.14 presents results from regression specification (11) 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 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

[31] 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).

[32] 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.14: **Experience Effects and Monthly Consumption (Nielsen)**

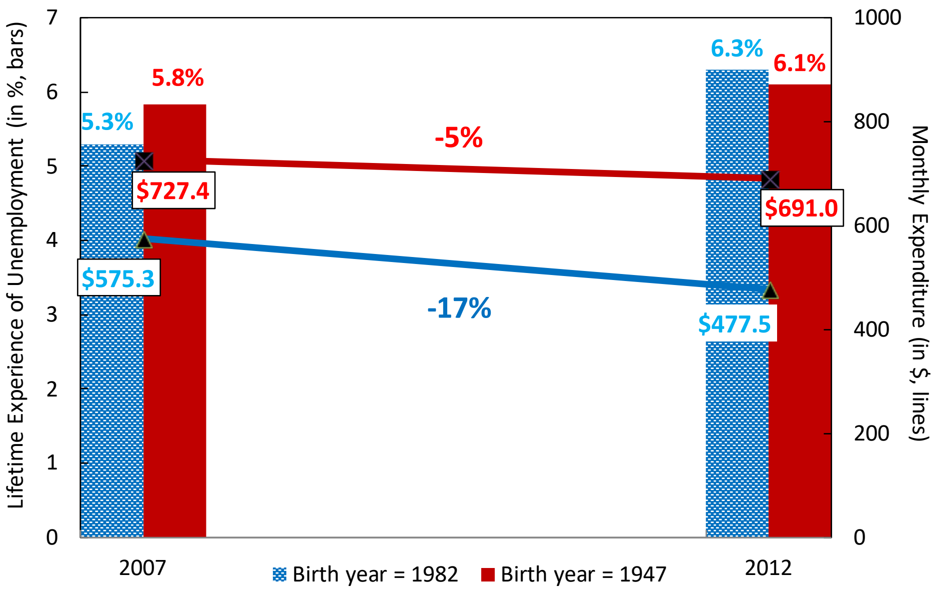
	(1)	(2)	(3)	(4)
Experience (Macro)	-0.166***	-0.165***	-0.172***	-0.172***
	(0.055)	(0.055)	(0.027)	(0.027)
Unemployment rate (county)		-0.005***		-0.005***
		(0.001)		(0.001)
Income control	Yes	Yes	Yes	Yes
Wealth control	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Age fixed effects	Yes	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.

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

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.

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 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%.

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 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

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.

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 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 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 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.

B.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.

B.2 Budget constraint

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

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

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

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

B.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.

B.4 Implementation

Appendix-Table B.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.[33]

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).

[33] 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 B.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.