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Scarred Consumption

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Scarred Consumption*

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Abstract

We show that economic downturns can "scar" consumers in the long-run. Having lived through times of high unemployment consumers remain pessimistic about the future financial situation and spend significantly less years later, controlling for income, wealth, and employment. Their actual future income is uncorrelated with past experiences. Due to experience-induced frugality, scarred consumers accumulate more wealth. Using a stochastic life-cycle model we show that the negative relationship between past downturns and consumption cannot arise from financial constraints, income scarring, or unemployment scarring. Our results suggest a novel micro-foundation of 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

The onset of the COVID-19-induced recession in the Spring of 2020 immediately prompted a debate about its long-run implications: With unemployment numbers rivaling those of the Great Depression, would the recovery be as painful and slow as in the 1930s, or could we hope for a V-shaped reversal? Looking back at 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, as the quote above suggests?

Many of the lingering, long-term effects of macroeconomic crises have been hard to capture in existing workhorse consumption models. For example, after the 2008 Great Recession, consumption remained low not only in absolute levels, but also relative to the growth of income, net worth, and employment—a pattern that challenges standard life-cycle consumption explanations, such as time-varying financial constraints. For the same reason, low employment due to the loss of worker skills or low private investment, as put forward in the literature on "secular stagnation" and "hysteresis,"cannot account for the empirical pattern either.[1]

Our hypothesis starts from the observation in Pistaferri (2016) that the long-lasting crisis effects are accompanied by consumer confidence remaining low for longer periods than standard models imply. We relate this observation to the notion of *experience effects*: We show that consumers' past lifetime experiences of economic conditions have a long-lasting effect on their beliefs and on their consumption decisions, which is not explained by income, wealth, liquidity, and other life-cycle determinants. Prior research on experience effects has shown that personally experienced stock-market and inflation realizations strongly influence individual beliefs about fu-

[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).

ture realizations of the same variables and the corresponding investment decisions.[2] Here, we show that a similar mechanism is at work when individuals experience periods of economic downturn, and the reverse for boom times: Past experiences of both national and local unemployment rates, as well as personal unemployment experiences, exert a lasting influence on consumer optimism or pessimism, on their expenditures, and several other empirical regularities, such as generational differences in consumption patterns. Our paper is the first to establish *experience effects* even within household, which ameliorates concerns about cross-sectional confounds. Moreover, to distinguish these experience effects from known earnings implications of job loss (see, e. g., Jacobson, LaLonde, and Sullivan (1993), Couch and Placzek (2010)), all estimations employ measures of experience effects that exclude the recent past and, in addition, control for income in the recent past, current income, wealth, unemployment, and other demographics. Moreover, we show that the same experience measures do *not* predict actual future income, after including the same standard controls, and predict, if anything, a positive wealth build up.

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 effect of past experiences on consumption using the *Panel Study of Income Dynamics* (PSID) from 1999-2013. We show that past macroeconomic and personal unemployment experiences both 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]

[2] The empirical literature starts from Kaustia and Knüpfer (2008) and Malmendier and Nagel (2011, 2015). 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).

[3] 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. Note that, differently from most of the prior literature on experience effects (Malmendier and Nagel 2011, 2015), the experience measure is not absorbed by cohort fixed effects as the consumption data sets contain substantial within-cohort variation in experiences depending on where the cohort members have resided over their

The estimated effects are sizable. A one-standard-deviation increase in the macro-level measure is associated with a 1.74%-1.78% ($1,099-$1,127) decline in total annual consumption spending, and a one-standard-deviation increase in personal unemployment experiences with a 0.83%-0.97% ($520-$609) decrease. The results are robust to numerous robustness checks: we vary the construction of the experience effect proxies to assign more or less weight to observations 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 expand our results and replicate them out-of-sample, employing two additional data sets, the Nielsen Homescan Data and the Consumer Expenditure Survey (CEX). The Nielsen data is a panel of consumption purchases by representative U.S. households. It contains detailed data on 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 similar in magnitude.[4]

Second, we document that consumers' past experiences significantly affect beliefs. Using the *Michigan Survey of Consumers* (MSC) from 1953 to 2018, we show that people who have experienced higher unemployment rates over their lifetimes further in the past have 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 controls.

Third, we relate the same measure of lifetime unemployment experiences to ac-

prior lifetimes as well as their personal unemployment experiences.

[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.

tual future income, up to four PSID waves (eight years) in the future. Again, we control for current income, wealth, demographics, as well as age, state, year, and even household fixed effects. We fail to identify any robust relation. In other words, while prior lifetime experiences of unemployment further in the past exert a strong influence on beliefs about the future and on actual consumption expenditures, actual future income does not appear to explain these adjustments.

Our fourth baseline result captures the wealth implications of consumption scarring. If consumers become more frugal in their spending after negative past experiences, even though they do not earn a reduced income, 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 macroeconomic lifetime experiences of unemployment leads to additional precautionary savings and resulting wealth build-up of about 0.7% or $1,400 ten years later, and a one-standard-deviation increase in personal unemployment experiences in the past results in wealth increases of about 1.1% or $2,300 ten years later. Unobserved wealth effects, the main alternative hypothesis, do not predict wealth build up, or predict the opposite.

These four baseline results—a lasting influence of economic experiences in the past 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 our hypothesis: Consumers over-weigh past experiences when predicting future realizations and making consumption choices. Considered jointly, and given the controls included in the econometric model, the results so far already distinguish our hypothesis from several alternative explanations: The inclusion of age controls rules out important life-cycle effects, 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. Moreover, the panel structure of the PSID data allowed for the inclusion of household fixed effects and thus for us to control for time-invariant unobserved heterogeneity.

To further distinguish scarring effects due to past experiences from other determinants that can be embedded in a life-cycle permanent-income model, we simulate the Low, Meghir, and Pistaferri (2010) model of consumption and labor supply and use estimations on the simulated data to illustrate directional differences and other distinctive effects. The Low et al. (2010) model accounts for various types of shocks, including productivity and job arrival, and allows not only for financial constraints but also for "income scarring"—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. Moreover, we can extend the Low et al. (2010) model to allow for yet another type of "scarring" that is sometimes discussed in the literature and in practice: "unemployment scarring" in the sense that job loss itself may induce a negative, permanent wage shock.[5] We contrast these explanations with the predictions of experience-based learning (EBL) by simulating the model for both Bayesian and experience-based learners.

The simulations show that, even with all of the life-cycle determinants and frictions built into the Low et al. model, it is hard to generate a negative correlation between past unemployment experiences and consumption when consumers are rational, after controlling for income and wealth. This holds both when we allow for financial constraints and income scarring, as in Low et al., and when we further add unemployment scarring. In fact, given the income control, the simulate-and-estimate exercise often predicts a *positive* relation between unemployment experiences and consumption. Intuitively, a consumer who has the same income as another consumer despite worse unemployment experiences likely has a higher permanent income component, and rationally consumes more.

We then turn to consumers who overweight their own past experiences when forming beliefs. Here, we find the opposite effect: Higher life-time unemployment experiences predict lower consumption among EBL agents, controlling for income and wealth. Thus, the simulate-and-estimate exercise disentangles EBL from potential confounds such as financial constraints, income scarring, and unemployment scarring. There is a robust negative relation between past experiences and consump-

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

tion under EBL, consistent with the empirical estimates, but not under Bayesian learning. Moreover, Bayesian learning is inconsistent with the estimated relation between past experiences and downward biased beliefs.

The model also helps to alleviate concerns about imperfect wealth controls. We conduct all simulate-and-estimate exercises leaving out the wealth control in the estimation. In the case of rational consumers we continue to estimate a positive rather than negative relationship between past experiences and consumption; in the case of experience-base learners, we continue to estimate a negative relationship.

Guided by these simulation results, we perform three more empirical steps: (1) a broad range of robustness checks and replications using variations in wealth, liquidity, and income controls, (2) a study of the implications of EBL for the heterogeneity in consumption patterns across cohorts and the quality of consumption, and (3) a discussion of the potential aggregate effects of EBL for 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. 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.[6]

We then show that prior experiences affect consumption also at the qualitative margin, exploiting the richness of the Nielsen data. We estimate a significant increase in several measures of frugality: (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$^{\text{th}}$ than at the 10$^{\text{th}}$ percentile of unemployment experiences.

Next, we test a unique prediction of EBL: Since a given macroeconomic shock

[6] 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).

makes up a larger fraction of the lifetime experiences of younger than older people, macroeconomic shocks should have particularly strong effects on younger cohorts. That is, the EBL model predicts that 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 aggregate and personal unemployment experiences, and in both the positive and in the negative direction.

Overall, our results on the lasting effects of past experiences on consumption suggest that experience effects constitute a novel micro-foundation of fluctuations in aggregate demand and long-run effects of macro shocks. We provide suggestive evidence of this implication on the aggregate level by correlating aggregate lifetime experiences of past national unemployment among the U.S. population with real personal consumption expenditure (PCE) from the U.S. Bureau of Economic Analysis (BEA) from 1965 to 2013. The resulting plot shows that times of higher aggregate past-unemployment experience in the population coincide with lower aggregate consumer spending. This suggests that changes in aggregate consumption may reflect not only responses to recent labor-market adjustments, but also changes in belief formation due to personal lifetime experiences of economic shocks. 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 connects several strands of literature. First and foremost, this paper contributes to a rich literature on consumption. Since the seminal work of Modigliani and Brumberg (1954) and Friedman (1957), the life-cycle permanent-income model has been the workhorse to study consumption behavior. Consumption decisions are an intertemporal allocation problem, and agents smooth marginal utility of wealth across predictable income changes over their life-cycle. Subsequent variants provide more rigorous treatments of uncertainty, time-separability, and the curvature of the utility function (see Deaton (1992) and Attanasio (1999) for overviews). A number of empirical findings, however, remain hard to reconcile with the model predictions. Campbell and Deaton (1989) point out that consumption does not react sufficiently to unanticipated innovation to the permanent component of income (excess smoothness). Instead, consumption responds to anticipated income increases, over and above what is implied by standard models of consumption smoothing (excess sensitivity; cf. West 1989, Flavin 1993).

The empirical puzzles have given rise to a debate about additional determinants of consumption, ranging from traditional explanations such as liquidity constraints (Gourinchas and Parker 2002; see also Kaplan, Violante, and Weidner 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).[7] Experience-based learning offers a unifying explanation for both puzzles. The lasting impact of lifetime income histories can explain both consumers' lack of response to permanent shocks and their overreaction to anticipated changes.

Our approach is complementary to the existing life-cycle literature: Experience effects describe consumption after taking into account the established features of the life-cycle framework. EBL can explain why two individuals with similar income profiles, demographics, and household compositions make different consumption choices if they lived through different macroeconomic or personal employment histories.

Our predictions and findings are somewhat reminiscent of consumption models with intertemporal non-separability, such as habit formation models (Meghir and Weber 1996, Dynan 2000, Fuhrer 2000). In both cases, current consumption predicts long-term effects. However, the channel is distinct. Under habit formation, utility is directly linked to past consumption, and households suffer a loss of utility if they do not attain their habitual consumption level. Under EBL, households adjust consumption patterns based on inferences they draw from their past experiences, without direct implications for utility gains or losses.

Related research provides evidence on the quality margin of consumption. Nevo and Wong (2015) show that U.S. households lowered their expenditure 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 decrease in households' opportunity cost of time, we argue that experience effects are also at work. The key elements to identifying this additional, experience-based source of consumption adjustment are the inter-cohort differences and the differences in those differences over time. Relatedly, Coibion, Gorodnichenko,

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

and Hong (2015) show that consumers store-switch to reallocate expenditures toward lower-end retailers when economic conditions worsen.

The second strand of literature is research on experience effects. A growing literature in macro-finance, labor, and political economy documents that lifetime exposure to macroeconomic, cultural, or political environments strongly affects an individual's economic choices, attitudes, and beliefs. This line of work is motivated by the psychology literature on the availability heuristic and recency bias (Kahneman and Tversky 1974, Tversky and Kahneman 1974). The availability heuristic refers to peoples' tendency to estimate event likelihoods by the ease with which past occurrences come to mind, with recency bias assigning particular weight to the most recent events. Taking these insights to the data, Malmendier and Nagel (2011) show that lifetime stock-market experiences predict subsequent risk-taking in the stock market, and bond-market experiences explain risk-taking in the bond market. Malmendier and Nagel (2015) show that lifetime inflation experiences predict subjective inflation expectations. Evidence in line with experience effects is also found in college students who graduate into recessions (Kahn 2010, Oreopoulos, von Wachter, and Heisz 2012), retail investors and mutual fund managers who experienced the stock-market boom of the 1990s (Vissing-Jorgensen 2003, Greenwood and Nagel 2009), and CEOs who grew up in the Great Depression (Malmendier and Tate 2005, Malmendier, Tate, and Yan 2011). In the political realm, Alesina and Fuchs-Schündeln (2007), Lichter, Löffler, and Siegloch (2016), Fuchs-Schündeln and Schündeln (2015), and Laudenbach et al. (2018) reveal the long-term consequences of living under communism, its surveillance system, and propaganda on preferences, norms, and financial risk-taking.

Our findings on experience effects in consumption point to the relevance of EBL in a new context and reveal a novel link between consumption, life-cycle, and the state of the economy. A novelty of our empirical analysis, compared to the existing literature, is that the detailed panel data allow us to identify effects using within-household variation, whereas earlier works such as Malmendier and Nagel (2011, 2015) rely solely on time variation in cross-sectional differences between cohorts.

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 rational and 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 of experience effects for consumer spending and concludes.

II Measures and Data

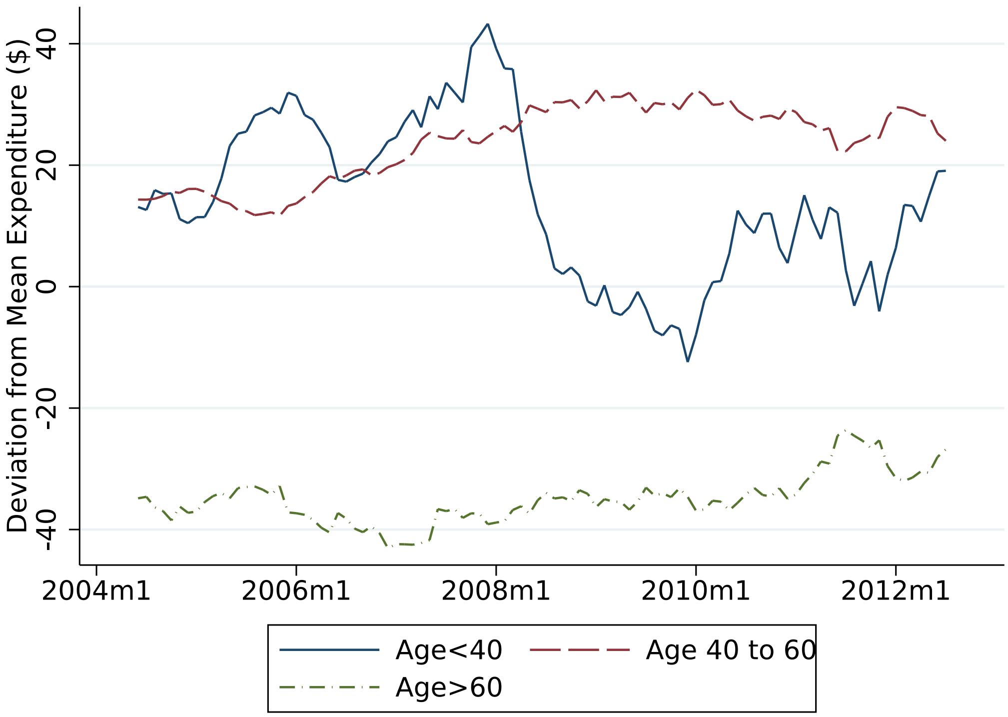
The experience-effect hypothesis is based on the idea that individuals overweight realizations that have occurred during their lifetimes. In the context of consumption, the conjecture is that individuals who have lived through difficult economic times have more pessimistic beliefs about future job loss and income, and thus 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.

In addition to these cross-sectional predictions, experience effects also imply that younger cohorts react more strongly to a shock than older cohorts since it makes up a larger fraction of their life histories so far. As a result, the cross-sectional differences vary over time as households accumulate different histories of experiences. The time-series of household expenditures in Figure 1 (expressed as deviations from the cross-sectional monthly means from the Nielsen data) reveals that the spending of younger cohorts is more volatile in general and was significantly more negatively affected by the Great Recession than those of other age groups.

Such patterns are consistent with consumers being scarred by recession experiences, and more so the younger they are.

Measuring Past Experiences. To formally test the experience-effect hypothesis, we construct measures of past experiences of national, local, and personal unemployment. We focus on experiences of unemployment rates following Coibion, Gorodnichenko, and Hong (2015), who single out unemployment as the most spending-relevant variable. The macro measure captures the past experiences of living through

Figure 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.

various spells of unemployment rates. The personal measure captures personal situations experienced so far.

The ideal experiment for testing the impact of past experiences on consumption would be to 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, as shown in a strand of well-established literature such as Jacobson, LaLonde, and Sullivan (1993) and 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.1. Our analyses below, instead, control for earnings in the recent past. To further ensure that we differentiate experience effects from these known earnings implications of job loss, we construct all measures of past experiences such that they exclude the recent past.

Specifically, unemployment experience 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 $t-k$, and k denotes the time lag. 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 Malmendier and Nagel (2011), we parametrize w as

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

This specification of experience weights is parsimonious in that it introduces only one parameter, λ, to capture different possible weighting schemes for past experiences. It simultaneously accounts for all experiences accumulated during an individual's lifetime and, for $\lambda > 0$, allows for experience effects to decay over time, e. g., as memory fades or structural change renders old experiences less relevant. That is, for $\lambda > 0$, the weighting scheme emphasizes individuals' recent experiences, letting them carry higher weights, while there is still a measurable impact of earlier life histories. As $\lambda \to \infty$, it converges towards the strongest form of recency bias. In our main empirical analyses, we will apply two weighting schemes, $\lambda = 1$ and $\lambda = 3$, to approximate the weights estimated in Malmendier and Nagel (2011, 2015). The former entails linearly declining weights, and the latter puts higher weights on consumers' recent experiences. With this range of λ parameters, we 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%) as a then-20-year-old, but that this influence was likely smaller than more recent experiences.

Empirically, we construct national, local, and individual measures of unemployment experiences, depending on the data set and individual information available. 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, region-specific 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 Bureau of Labor Statistics (BLS) provides state-level unemployment rates only since 1976, and there do not appear to be reliable sources of earlier historical unemployment data for all US states.[9] These data limitations imply that, if we were to work with "all available" data to construct region-specific measures, the values for family units from the later periods would be systematically more precise than those constructed for earlier periods, biasing the estimates. Hence, we have to trade off restricting the sample such that all family units in a given data set have sufficient location and employment-rate data and ensuring sufficient history to construct a reliable experience measure. We choose to 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 estimation results are very similar under all three macro measures, national, regional, and combined. We will show the combined measure in our main

[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] 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 errors.

regressions whenever geographic information on the individual level is available.

For the personal experience measure, we use the reported employment status of the respondent in the respective data set. We face the same data limitations as in the construction of the state-level macro measure regarding the earlier years in the lives of older cohorts. Mirroring our approach in constructing the local macro measure, we use 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).

Consumption 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 construct experience measures for each household. 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.

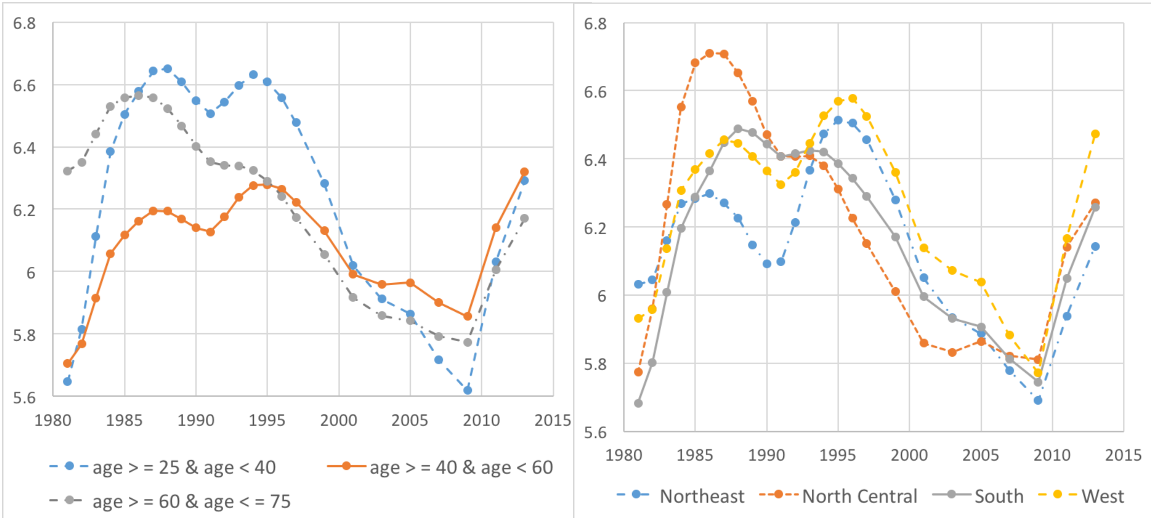
The key explanatory variable is the past experience of each household head at each point in time, calculated as the weighted average of past unemployment experiences as defined in (1) and (2). The PSID allows us to construct both macroeconomic and personal experience measures. Further, we can use both national and state-level rates for the macro measure. As discussed above, the more local measure has to account for several data limitations. 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. As specified above, we use the five 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 $t-7$. We will show the combined measure in our main regressions; the results for (pure) national and regional measures are very similar.

To measure personal experiences, we first create a set of dummy variables indicating whether the respondent is unemployed at the time of each survey.[10] We employ the same approach as with the state-level data regarding the early-years data limitations.

Figure 2 illustrates the heterogeneity in lifetime experiences, both in the cross-section and over time, for our PSID sample using the (combined) macro measure.

[10] 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.

Figure 2: **Unemployment Experience by Age Group and by Region**



Notes. The graphs show the unweighted means of local unemployment experiences, in the left panel for different age groups and in the right panel for different regions.

The left panel plots the unweighted mean experiences of young (below 40), middle-aged (between 40 and 60), and old individuals (above 60), while the right panel plots the measures for individuals in the Northeast, North Central, South, and West. The plots highlight the three margins of variation that are central to our identification strategy: At a given point in time, people differ in their prior experiences depending on their cohort and location, and these differences evolve over time.

Summary Statistics. Table 1 shows the summary statistics for our sample. We focus on household heads from age 25 to 75.[11] In the main analysis, we run the regressions excluding observations with total family income below the 10th or above the 90th percentile in each wave. The sample truncation addresses known measurement errors in the income variable.[12] After dropping the individuals for whom we

[11] 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.

[12] 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 10th and 90th 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.

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 25,578 observations. The average values for macro experiences based on weights of $\lambda = 1$ and $\lambda = 3$ are 6.00% and 5.64%, respectively, and the corresponding mean personal experience values are 5.84% and 5.19%. Naturally, the standard deviation of the latter measure is much higher, about ten times as large as for the macro measures. Average household total consumption in our sample is $63,074 (in 2013 dollars).

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

Variable	Mean	SD	p10	p50	p90	N
Age	49.15	11.24	35.00	48.00	65.00	25,578
Experience (Macro), λ=1 [%]	6.00	0.26	5.69	5.97	6.34	25,578
Experience (Macro), λ=3 [%]	5.84	0.47	5.28	5.81	6.46	25,578
Experience (Personal), λ=1 [%]	5.64	2.75	4.48	4.96	5.46	25,578
Experience (Personal), λ=3 [%]	5.19	4.83	3.04	3.95	4.80	25,578
Household Size	2.79	1.44	1	2	5	25,578
Household Total Consumption [$]	63,074	25,905	14,584	31,930	61,381	25,578
Household Total Income [$]	63k	34k	23k	57k	112k	25,578
Household Liquid Wealth [$]	29k	151k	-18k	0.3k	72k	25,578
Household Illiquid Wealth [$]	180k	798k	2k	65k	405k	25,578
Household Total Wealth [$]	209k	837k	-0.1k	68k	500k	25,578

Notes. Summary statistics for the estimation sample, which covers the 1999-2013 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 (Macro), and Experience (Personal) 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 2013 dollars (using the PCE), annual, and not weighted.

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 confidence and consumer behavior if we allow for a role of consumers' prior experiences of economic conditions. We measure past experiences of spending-relevant macro conditions in terms of higher or lower unemployment rates as in Coibion et al. (2015), both on the aggregate level (unemployment rates) and on the personal level. We then show that past experiences of unemployment have a measurable, lasting effect on consumption expenditures and on individual beliefs but fail to predict (lower) future income or future wealth.

III.A Past Experiences and Consumption

We relate expenditures to prior experiences of economic conditions by estimating

$$C_{it} = \alpha + \beta UE_{it} + \psi UEP_{it} + \gamma' x_{it} + \eta_t + \varsigma_s + \upsilon_i + \varepsilon_{it}, \tag{3}$$

where C_{it} is total consumption, UE_{it} and UEP_{it} are respectively i's macroeconomic and personal unemployment experience over her prior life (excluding the present and very recent past experience based on Equation 1), x_{it} is a vector of controls including wealth (first- and second-order logarithm of liquid and illiquid wealth), income (first- and second-order logarithm of income and lagged income), age dummies, household characteristics (dummy indicating if 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.[13] Standard errors are clustered at the cohort level and are similar when clustered by household or two-way clustered at the cohort and time level.

Our main coefficients of interest are β and ψ. The rational null hypothesis is that both coefficients are zero. The alternative hypothesis is that consumers who have

[13] We have also included region∗year fixed effects, and the results remain very similar. 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 determinants would need affect consumption exactly in the direction of the experience-effect hypothesis, including the different effects experiences have on 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.

experienced higher unemployment some time in the past spend less on average and, hence, that both coefficients are negative.

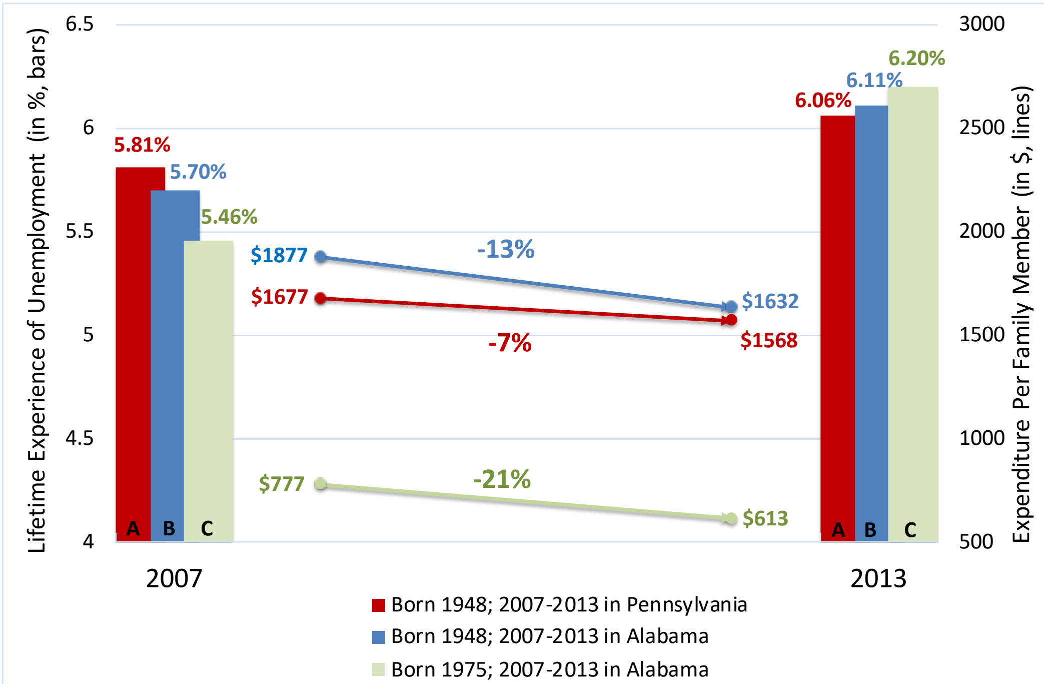
Identification. We estimate the model fully controlling any unspecified time-invariant household characteristics. The inclusion of household fixed-effects implies that we identify experience effects solely from time variation in the within-household co-movement of consumption and unemployment histories.

We illustrate the sources of identification with a simple example of the unemployment experiences and household consumption of three individuals in our PSID data over the course of the Great Recession. Consider two individuals (A and B) who have the same age (born in 1948) but live in different states (Pennsylvania and Alabama) during the 2007-2013 period and a third (C) who lives in the same state as B (Alabama) but differs in age (born in 1975).

The two sets of bars in Figure 3 illustrate their lifetime experiences of unemployment at the beginning and at the end of the 2007-2013 period, based on the weighting scheme in (2) and their states of residence. Person A enters the crisis period with a higher macroeconomic unemployment experience than Person B (5.81% versus 5.70%), but her lifetime experience worsens less over the course of the financial crises 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 3 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.

Results Table 2 shows the estimation results from model (3). All regressions control for first- and second-order (logs of) income, lag income, liquid wealth, illiquid wealth, all other control variables listed above as well as the fixed effects indicated at the bottom of the table. Columns (1)-(3) show results using experience measures

Figure 3: **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.

based on linearly declining weights (λ=1), and columns (4) to (6) use experience measures that shift more weight to recent observations (λ=3). All estimated coefficients on the control variables have the expected sign, consistent with prior literature.

All coefficients of interest on aggregate and personal 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 high aggregate employment rates and personal unemployment experiences have a 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.

The coefficient estimates imply large effects. Using experience measures based

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

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)	-0.069***		-0.067***	-0.038***		-0.038***
	(0.023)		(0.024)	(0.013)		(0.013)
Experience (Personal)		-0.003*	-0.003*		-0.002**	-0.002**
		(0.002)	(0.002)		(0.001)	(0.001)
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	25,578	25,578	25,578	25,578	25,578	25,578
R-squared	0.776	0.775	0.776	0.776	0.775	0.776

Notes. The consumption variables come from the 1999-2013 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 2013, 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.

on linearly declining weights (column (3)), our estimates show that a one-standard-deviation increase in macroeconomic unemployment experience predicts a 1.74% decrease in consumption, which is approximately $1,099 in annual spending. Similarly, a one-standard-deviation increase in personal unemployment experience leads to a 0.83% decrease in consumption, which translates to $520 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 macroeconomic and personal unemployment experience leads to a 1.78% and 0.97% decrease in consumption, respectively, which translates to $1,227 and $609 less annual spending. The magnitude of the macro experience coefficients is particularly remarkable considering that it reflects behavioral change due to fluctuation in the macro-economy and not to 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.13 and Appendix-Table A.15). Appendix-Section A.2 and Appendix-Section A.3 present more details about the alternative data and the corresponding results.

Overall, all the results robustly show that consumers with more adverse macroeconomic and personal unemployment experience tend to spend less, controlling for wealth, income, employment, family structures, and demographics.

III.B Past Experiences and Beliefs

Given the robust findings of a negative and significant relationship between people's lifetime experiences of economic conditions and their consumption behavior, we turn to explore the channels through which past experiences affect consumption. We start from consumer expectations: To what extent do personal lifetime experiences 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.

The explanatory variable of interest is again lifetime unemployment experiences. 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 equation (2) to calculate the weighted average of past unemployment 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.[14]

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 prior unemployment-rate experiences to respondents' forecasts of their own future situation. We find that people who have experienced times of greater unemployment during their lives so far are significantly more pessimistic about their future financial situation. The statistical and economic significance of the estimated effect is robust to variations in the controls: Whether we include only (age and time) fixed effects, control for income, or for all demographic variables, we always estimate a highly significant coefficient between -0.020 and -0.016. The robustness of the estimates to the income control is reassuring, since the controls for respondents' financial situation are more limited in the MSC data. Income has the expected positive coefficient, and the same holds for demographics that might proxy for unobserved wealth (e. g., education). The coefficient of past experiences of national unemployment rates remains highly significant and negative.

In terms of the economic magnitude, 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

[14] 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.

at the 10th percentile.

The estimations based on the second question, shown in columns (4) to (6), produce very similar results. We estimate a significantly negative effect of lifetime experiences of unemployment on "buying attitude." The coefficient is again fairly stable across specifications, ranging from -0.064 to -0.049. Respondents who have experienced 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 addresses 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. Yet, they strongly rely on their personal experiences to draw conclusions about economic conditions more broadly.

Our results suggest that the economic conditions individuals have experienced in the past have a lingering effect on their 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 all historical data, current unemployment, and other macro conditions. This evidence on the beliefs channel is consistent with prior literature on experience effects, including Malmendier and Nagel (2011, 2015), who have documented a strong effect of (stock-market and inflation) experiences on the corresponding expectations.

Before turning to analyze whether consumer beliefs might be overly pessimistic, in light of actual future earnings, we explore potential alternative explanations of consumers' response to past experiences. In particular, lifetime experiences might influence not only consumers' beliefs but also their preferences. That is, the evidence on experience-based learning (beliefs channel) does not rule out that experience-based taste changes (preference channel) are also at work.

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. 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 lifetime experience may be correlated with persistent habits in consumption.

To that end, we estimate an alternative version of the empirical model 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.[15] 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 unemployment experience on consumption remain 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.

III.C Past Experiences and Future Income

Having established a negative relationship of past exposure to unemployment and consumption, as well as a negative relationship with consumer optimism, we now ask whether past unemployment experiences *actually* 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? As we will show, the answer is no.

We re-estimate our baseline model from equation (3) with the dependent variable

[15] 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.

changed to future income either one or two or three or four or five survey waves in the future, i. e., two, four, six, eight, and ten years ahead, using experience measures based on the two weighting schemes.

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

	Income_{t+2}	Income_{t+4}	Income_{t+6}	Income_{t+8}	Income_{t+10}
Experience (Macro), λ=1	-0.028*	-0.017	0.007	-0.016	-0.020
	(0.014)	(0.017)	(0.022)	(0.028)	(0.063)
Experience (Personal), λ=1	-0.000	0.000	0.001	-0.001	0.005**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	13,206	9,762	6,846	4,483	2,374
R-squared	0.800	0.822	0.839	0.870	0.908
Experience (Macro), λ=3	-0.016*	-0.011	0.002	-0.009	-0.015
	(0.008)	(0.009)	(0.012)	(0.016)	(0.037)
Experience (Personal), λ=3	-0.000	0.000	0.001	-0.000	0.003**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Observations	13,206	9,762	6,846	4,483	2,374
R-squared	0.800	0.822	0.839	0.870	0.908
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 2013, 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.

The estimation results are in Table 4. They suggest that unemployment experiences do not play a significant role in explaining future income. After controlling for income, wealth, employment status, the other demographics, and fixed effects,[16] the estimated coefficients of personal unemployment experiences are all small and mostly positive and insignificant. For macroeconomic experiences, we estimate small negative coefficients, which are also insignificant with the exception of the estimation predicting income two years ahead, where it is marginally significant. In summary, our results imply that past experiences do not predict future earnings prospects.

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) using income volatility as the dependent variable. Following Meghir and Pistaferri (2004) and Jensen and Shore (2015), we construct volatility measures both for 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 either measure, two, four, or six years ahead (i.e., $t+2$, $t+4$, or $t+6$) using experience measures based on the two weighting schemes. We do not find a strong correlation between unemployment experiences and income volatility, other than one marginally significant coefficient on macroeconomic experience for the variance of permanent income in $t+2$. Hence, consumers' long-term reduction in consumption after past unemployment experiences does not appear to be a rational response to future income uncertainty.

[16] 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 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 (Macro), λ=1	0.027	0.059	-0.024	0.067	-0.070	0.001
	(0.078)	(0.058)	(0.092)	(0.079)	(0.099)	(0.117)
Experience (Personal), λ=1	0.005	0.012*	0.000	0.004	0.001	0.001
	(0.006)	(0.006)	(0.004)	(0.007)	(0.006)	(0.006)
R-squared	0.357	0.438	0.359	0.432	0.405	0.456
Experience (Macro), λ=3	0.017	0.024	-0.008	0.037	-0.038	-0.002
	(0.043)	(0.034)	(0.051)	(0.045)	(0.055)	(0.066)
Experience (Personal), λ=3	0.003	0.007*	0.000	0.002	-0.000	0.001
	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
R-squared	0.358	0.438	0.359	0.432	0.405	0.456
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	8,778	17,880	8,776	12,463	5,921	9,161

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.

III.D Past Experience and Wealth Build-up

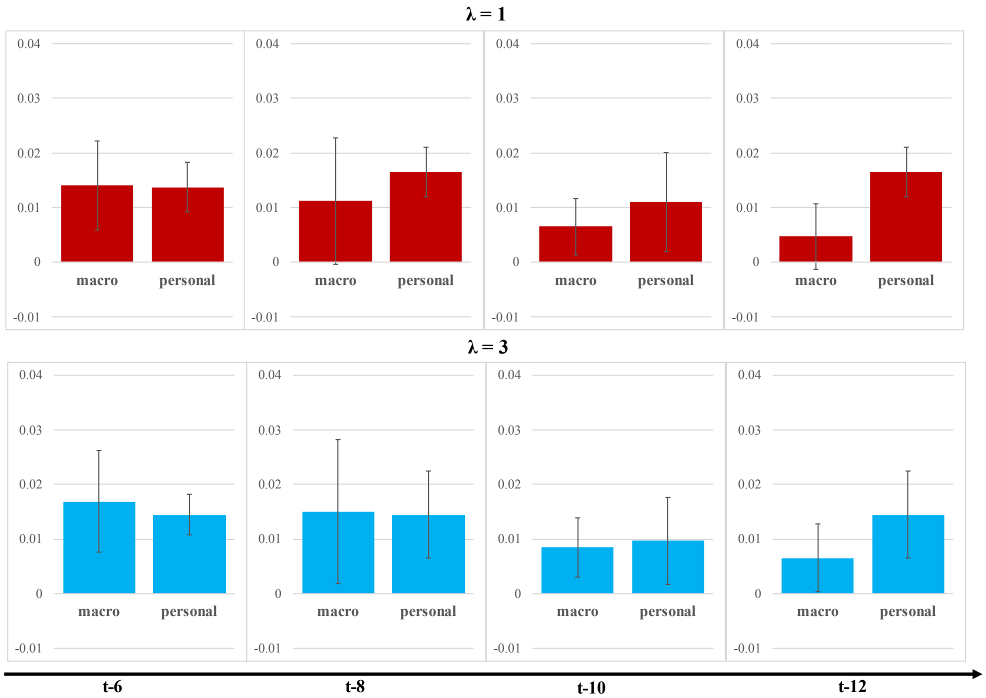
The significant effect of past unemployment experiences on consumption, and the lack of a relation with future income, imply that household experiences could affect the build-up of wealth. In the case of negative lifetime experiences, consumers appear to restrain from consumption expenditures more than rationally "required" by their income and wealth positions. This experience-induced frugality, in turn, predicts more future wealth. Vice versa, consumers who have lived through mostly good times 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' lifetime experiences to their future wealth, using up to six survey waves (12 years) in the future. We note that this analysis also ameliorates potential concerns about the quality of the consumption data and alternative life-cycle interpretations of our findings.

Figure 4 summarizes graphically the coefficients of interest from eight regressions, namely, the cases of 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, constructed with the $\lambda = 1$ weighting scheme, on total wealth. The bottom part shows the effects of a one-standard-deviation increase in experience, constructed with the $\lambda = 3$ weighting, on total wealth. Appendix-Table A.11 provides the details on the coefficient estimates of both experience measures. All coefficient estimates are positive and mostly statistically significant. Using the $\lambda = 1$ weighting scheme, the estimates of macro experiences imply that a one-standard-deviation increase in macroeconomic lifetime experiences of unemployment will lead to additional precautionary savings and resulting wealth build-up of about 0.7% or $1,400 ten years later. The estimates of the role of personal lifetime experiences imply a large economic magnitude: a one-standard-deviation increase in personal lifetime experiences of unemployment will lead to additional precautionary savings and resulting wealth build-up of about 1.1% or $2,300 ten years later. In other word, households who have experienced high unemployment tend to accumulate more wealth down the road.

In summary, individuals' lifetime experiences strongly predict consumption expenditure, 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

Figure 4: **Wealth Build-up: Effects of a One-Standard-Deviation Increase in Experience**



Notes. The upper four graphs (red bars) show the effects of a one-standard-deviation increase in experience, constructed with the $\lambda = 1$ weighting scheme, on total wealth. The bottom four graphs (blue bars) show the effects of a one-standard-deviation increase in experience, 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.

between past experience and future wealth build-up.

IV Consumption with Experience-based Learning

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 a broad array of standard life-cycle consumption factors, frictions, and possible confounds, including financial constraints, social-insurance programs, and "income scarring," i. e., the notion that job loss reduces income flows because of lower match quality in future jobs. 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. Moreover, we extend the Low et al. model to also capture "unemployment scarring," i. e., the notion that unemployment, once experienced, makes individuals inherently less employable. The extended Low et al. framework allows us to distinguish both income scarring and unemployment scarring as well as other life-cycle features from scars due to longlasting experience effects, and to illustrate that, for a wide range of parameterizations, we can distinguish experience effects, even directionally.

Towards that end, we introduce two classes of consumers into the model: standard rational agents, as in the original Low et al. model, and experience-based learners. Rational consumers use all available historical data to update their beliefs about the probability of being unemployed next period. Experience-based consumers overweight their own experiences when forming beliefs. We simulate intertemporal consumption and labor decisions for both types of consumers and estimate the relation between experience measures and consumption in both settings, i. e., also for rational consumers, for whom they should not have a significantly negative relation. The simulate-and-estimate exercise illustrates the basic mechanism of experience-based learning and distinguishes it from features of the standard consumption model, such as wealth or liquidity constraints. It provides guidance towards empirical robustness checks and additional tests.

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 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

[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.

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 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.

[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.

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

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

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.

We show several plots of the resulting consumption paths for both rational and experience-based learners in Appendix B. In particular, it is instructive to separate consumers who were "lucky" and "unlucky" early in life, in terms of their earnings. The graphs in Figures B.2 and B.3 illustrate the corresponding over- and under-consumption of experience-based learners during their early lifetimes, relative to rational consumers, as well as the need to then curtail consumption later in the first case (good experiences) and the excess wealth build-up in the second case (bad experiences). This mirrors the empirical relationship we found in Section III.D.

Using the simulated values, we estimate the relationship between consumers'

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

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

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.

unemployment experience and consumption behavior, controlling for income and wealth. The corresponding OLS regressions are in Table 7 columns (1) and (2), for rational consumers, and columns (3) and (4), for experience-based learners. In the case of rational agents, prior experiences do not actually enter their belief formation. The purpose of including the experience measure here is to identify possible confounds of the significantly negative effect we have estimated in the PSID data. Specifically, as we are concerned about 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 also included.

Income scarring. We first conduct the simulation using linearly declining weights ($\lambda = 1$) for the measure of prior experiences, as we did in our empirical analysis. As shown in the top panel of Table 7, income has the expected positive sign and significance across 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 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.

Note that the positive sign of the experience-effect estimate in the data simulated for rational agents (columns 1 and 2) not only ameliorates concerns about wealth confounds, but also seems to contradict the basic intuition of "income scarring:" Unexpected job destruction lowers lifetime income, and thus consumption. Why do higher unemployment experiences predict higher consumption? 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, suggesting 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 and generate the opposite sign. Under this scenario, there is little concern about confounding experience effects with traditional determinants of lower consumption, including (unobserved) wealth effects and income scarring, as long as we control for current income.

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, constrasting our empirical findings. For experience-based learners, instead, we estimate a negative coefficient in the specificiation 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, at least 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 other people (without such personal unemployment histories), 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. Since prior research has estimated λ values greater than 1 (Malmendier and Nagel 2015; Malmendier and Nagel 2011), our estimation results might be affected by recency bias. Empirically, we do estimate a negative coefficient as shown in the previous section, implying that, if anything, the true experience effect might be stronger under perfect wealth controls.

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

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.

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

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

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: 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, we can conclude that, in most scenarios, a negative coefficient estimate for past unemployment experiences indicates actual experience-based scarring, even if consumers' income and consumption are also affected by income scarring in the sense of Low et al. (2010) and affected by 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 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."

Taken together, the results of both simulate-and-estimate exercises provide evidence that, for empirically validated parameterizations of experience effects (with linearly declining or steeper weighting functions), financial constraints, unobserved wealth factors, income scarring, and unemployment scarring individually fail to generate a negative relation between our proxy for unemployment experiences further in the past and consumption when agents are Bayesian learners. Instead, a negative coefficient estimate likely indicates experience-based learning. Only if we fail to appropriately control for wealth effects, allow for only weak recency bias, and introduce fairly potent unemployment scarring, the confound might materialize. Since all of our estimations explore the results for a high λ parameter and control for wealth, and since we found no relation between unemployement 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.[21]

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 results on the relation between past unemployment experiences and consumption, we study two further implications of experience effects.

[21] One prediction we did not pursue regards the hours worked. In general, EBL implies a positive relation between past unemployment experience and the likelihood of working because work generates greater income buffer. (Note that "work" is a binary decision in the 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.

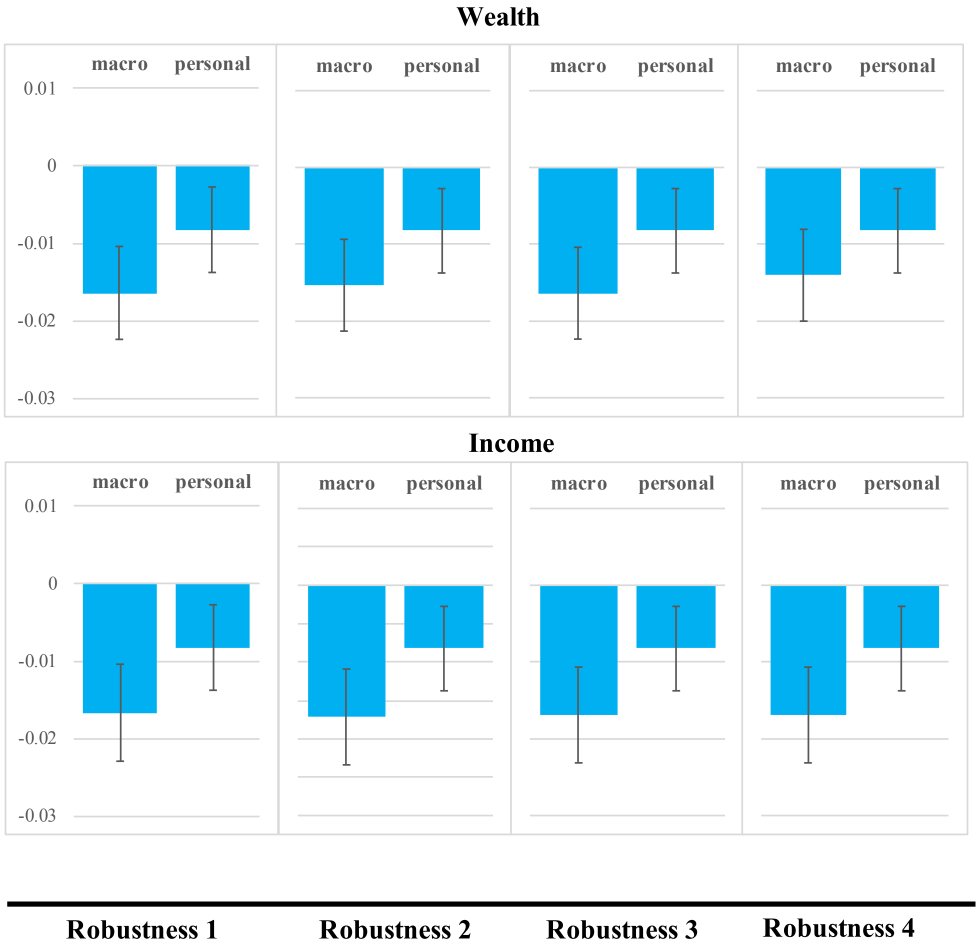
V.A Wealth, Income, and Liquidity

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 under our standard proxy 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: (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 are in Appendix-Table A.8. All coefficients of interest remain very similar, both in size and in statistical significance. We summarize the implied economic magnitudes of a one-standard-deviation increase in past macroeconomic and personal experiences on consumption in the top panel of Figure 5.

A related concern is measurement error in the income variable. As with wealth, we re-estimate our empirical model using varying constructs of income: (1) third and fourth order of (log) income and lagged income, (2) quintile dummies of income and lagged income, (3) decile dummies of income and lagged income, and (4) controls the bottom 2, 2^{nd}-4^{th}, 4^{th}-6^{th}, 6^{th}-8^{th}, 8^{th}-10^{th}, 90^{th}-92^{nd}, 92^{nd}-94^{th}, 94^{th}-96^{th}, 96^{th}-98^{th}, and top 2 percentile dummies of income and lagged income. All estimates, shown in Appendix-Table A.9, are again similar in both magnitude and significance. The implied economic magnitudes are shown in the bottom panel of Figure 5.

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? Our separate controls for liquid and illiquid wealth in the baseline estimations in Table 2 and in columns (2) and (6) of Appendix-Table A.8, 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. Closely following the practice in the consump-

Figure 5: **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.

tion literature, such as Johnson, Parker, and Souleles (2006) and Parker, Souleles, Johnson, and McClelland (2013), for each year we 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.10, 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 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 further test whether people's lifetime unemployment experiences affect also the quality of their consumption. To that end, we make use of the rich, micro-level information on purchases in the Nielsen data.

The Nielsen 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.[22]

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

[22] 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.

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.

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.[23]

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).[24] As before UE_{it} denotes the lifetime (macro) experience of unemployment rates based on a weighting scheme of $\lambda = 1$.[25] Note that we are not able to construct the same type of macro and personal unemployment experience proxies as in the PSID because Nielsen provides no information about households' prior residence or employment status (pre-sample period). We thus report the estimations employing only the macro experience measure, constructed based on national unemployment rates. The vector of controls x_{it} 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),

[23] 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.

[24] 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.

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

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.12.

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. Hence, a thorough study on the long-term impact of macroeconomics shocks on consumption calls for analyses not only of aggregate spending figures but also of product substitution and consumption reallocation—margins that entail important welfare implications.

V.C Heterogeneity Across Cohorts

Experience-based learning naturally gives rise to heterogeneity in consumption choices across cohorts. While all consumers overweight their personal experiences, in particular their more recent experiences, the experience-effect hypothesis also implies that younger cohorts do so more strongly than older cohorts. Experience-based beliefs, as defined in equations (1) and (2), assign weights to lifetime realizations, and the shorter a consumer's life is the more mass is assigned to the most recent realization.

One implication of our findings, then, is that a given unemployment shock should have a stronger effect on cohorts with shorter lifetime histories so far. 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 implication directly, regressing the change in log consumption in the Nielsen data 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.[26] We do so separately for positive and negative changes (in absolute value)

[26] It would be more difficult to estimate the effect of recent changes in unemployment experience on changes in consumption in the PSID. The low (biannual rather than monthly) frequency of

in unemployment rates 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. We interact age with the national-rate shock in columns (1)-(2), and with the local (county-level) rate shock in columns (3)-(4). We include all interactions in columns (5)-(6). The changes in log national unemployment rate are absorbed by the time (year-month) fixed effects, and we include the positive and negative changes in log local unemployment rate across all specifications.

The estimated age-unemployment interaction effects reveal that unemployment shocks, whether positive or negative, have a smaller effect on expenditures as age increases. The coefficients are always significantly negative. The effects are a bit stronger for increases in national unemployment and for decreases in local unemployment. When we include all four interaction effects, the coefficient sizes remain similar, with the exception of the interaction of age with lower national employment, where the estimated coefficient becomes smaller and insignificant. Overall, the results support our prediction of a significantly stronger response to recent experiences among the young than among the old.

This finding also helps further distinguish the experience-effect hypothesis from alternative theories such as liquidity constraints of 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, as they are more likely to hit liquidity constraints, but they do not easily predict a more positive reaction to positive shocks. To generate the latter prediction, these models need to rely on the argument that the young were previously constrained, and a positive shock allows them to adjust to their permanent-income optimum. However, our identification also exploits the differences in consumption of the young at better and worse economic times. Here, an adjustment to the PIH optimum would predict the opposite outcome relative to the experience effect hypothesis: the young with more negative prior experiences would exhibit a stronger reaction to recent good

survey waves makes it harder to define the "most recent" experience in a uniform way, and reduces statistical power as we have only eight waves. Hence we use the Nielsen data for this analysis.

Table 10: **Age-Heterogeneity in Reaction to Unemployment Fluctuation**

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

Notes. regression with dependent variable being the change in log monthly total consumption expenditure and the main regressors being the interaction term between age and the change in log national or local unemployment rate separated into two variables depending on whether the change is positive or negative (in absolute value), both from time t to $t-1$. Local unemployment controls are the change in log local unemployment rate separated into two variables depending on whether the change is positive or negative. 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.

outcomes according to the PIH.[27] Thus, our findings highlight experience effects as a distinct force in affecting people's consumption behavior.

VI Aggregate Implications and Conclusion

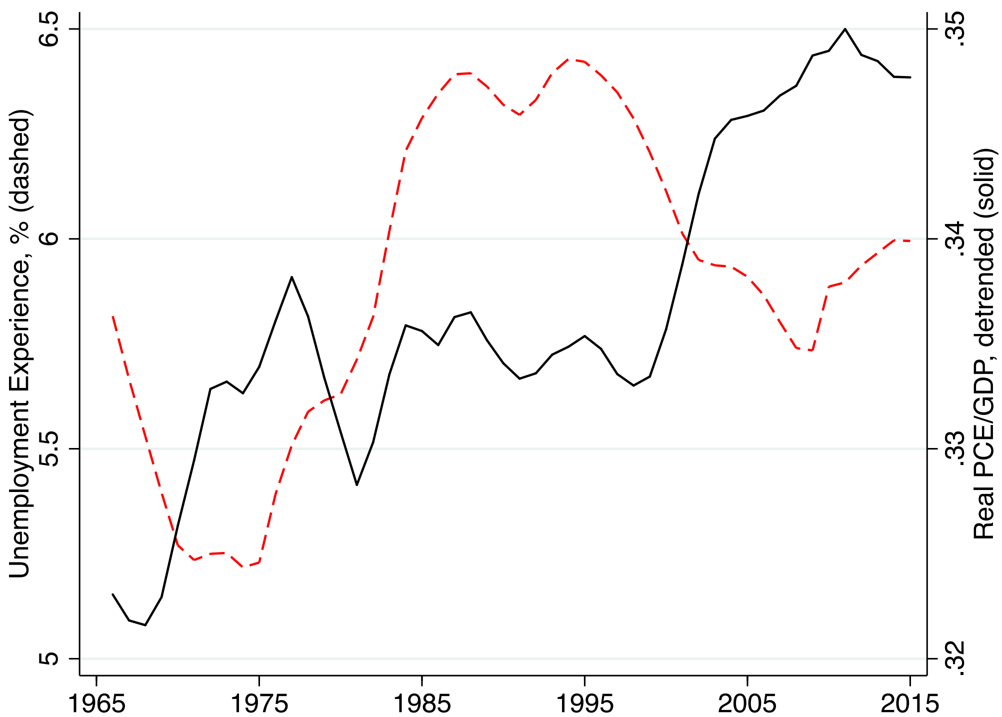
A better understanding of the long-term effects of economic shocks has proven to be of utmost importance for both academics and policy-makers, whether we consider the COVID-19 induced recession in 2020, the long-lingering effects of the Great Recession in 2008, or even the Great Depression of the 1930s.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

Estimation results from three different data sources confirm this conclusion. 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 income and wealth changes. In fact, we see evidence of a positive relationship between past experience and future wealth build-up.

Experience effects could even 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. Specifically, we relate an aggregate measure of lifetime experiences in the U.S. population to a measure of aggregate consumption expenditure in the U.S. from 1965 to 2013. For the former measure, we take a weighted average of national unemployment experience, as defined in Equation (1), using data on U.S. population broken down by age (age 25 to 75) from the Census as weights. For aggregate consumer

[27] 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 a dummy variable indicating above-median unemployment experience 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 EBL but not by a standard PIH framework.

Figure 6: **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.

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 6, there exists a 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 personally experienced labor market conditions may be a significant granular source of aggregate fluctuations.

The evidence on experience effects in consumption has potentially important policy implications. They 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 personal experiences affect 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 the estimations of the wealth build-up. The first figure and eleven tables use the PSID data.

In Appendix-Figure A.1, we replicate the empirical exercise proposed in the job displacement literature, including Jacobson, LaLonde, and Sullivan (1993) and Couch and Placzek (2010), which estimates income loss around displacement. It plots the coefficients δ_k from the regression $y_{it} = \alpha_i + \gamma_t + \sum_{k\geq -m} D_{it}^k \delta_k + x_{it}\beta + \epsilon_{it}$, 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 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 2013, 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 32,488 observations, compared to 25,578 observations in the main sample. The sample statistics are very similar, with a mean macroeconomic experience of 6.00% and 5.85% based on weights of $\lambda = 1$ and $\lambda = 3$, respectively, a mean personal experience of 5.89% and 5.63% based on weights of $\lambda = 1$ and $\lambda = 3$, respectively, and average household total consumption of \$37,843 (in 2013 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 an alternative experience measures for the gap years (between the PSID biennial surveys). For the macroeconomic experience 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 the personal experience measure, 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 in (3), we find results very similar to those in Table 2.

In Appendix-Table A.4, we present an alternative experience measure that incorporates the experiences of the spouses. For married households, we use the average of the household heads' and spouses' experiences, controlling for married-couples indicator. All variables other than the couple indicator and the experience measures 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 very similar to the baseline results in Table 2.

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, and A.10 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.

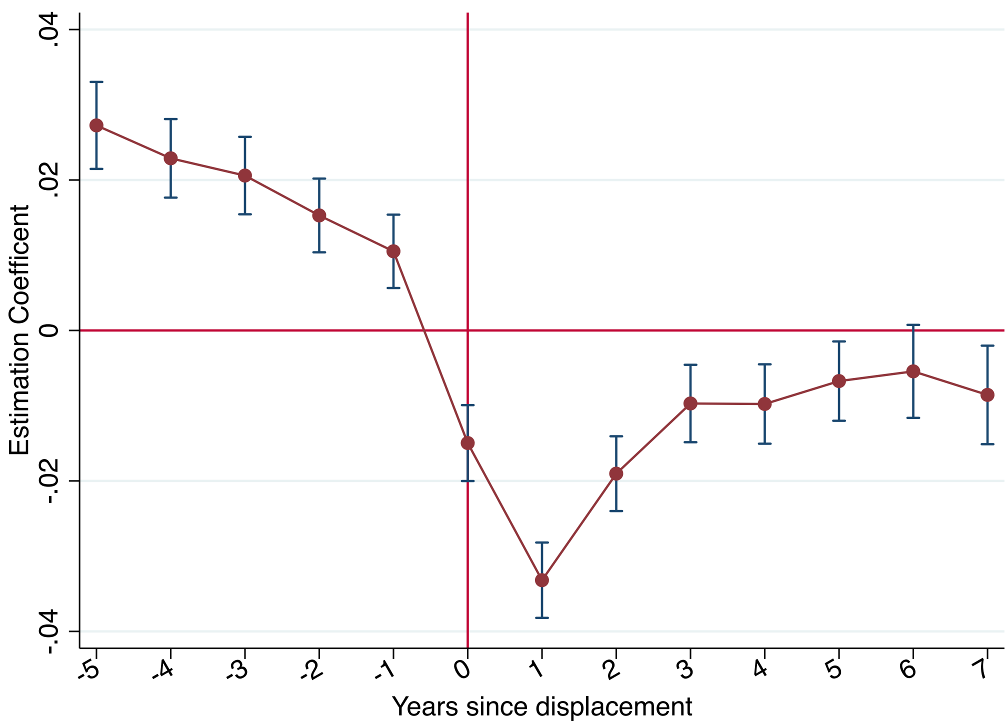
Appendix-Table A.9 uses alternative income controls, in addition to the controls of first and second order of income and lagged income: third- and fourth-order income and lagged income (columns (1) and (5)); quintile dummies of income and lagged income (columns (2) and (6)); decile dummies of income and lagged income (columns (3) and (7)); controls for 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.

In A.10, 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.10 shows, households in the bottom half of liquid wealth tend to spend less but do not exhibit stronger reactions to unemployment experience. This suggests households' experiences affect consumption beyond potential liquidity constraints.

In Appendix-Table A.11, we study the effects of lifetime experiences on wealth accumulation. This analysis tests whether, given the significant impact of unemployment experiences on consumption, we can also detect experience effects in 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)).[28] We find a significant role of past experiences for the build-up of wealth.

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

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



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

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

Variable	Mean	SD	p10	p50	p90	N
Age	49.35	11.19	35	48	66	32,488
Experience (Macro), λ=1 [%]	6.00	0.26	5.70	5.98	6.34	32,488
Experience (Macro), λ=3 [%]	5.85	0.47	5.29	5.82	6.47	32,488
Experience (Personal), λ=1 [%]	5.89	3.27	4.49	4.97	9.16	32,488
Experience (Personal), λ=3 [%]	5.63	5.76	3.07	3.97	11.67	32,488
Household Size	2.75	1.45	1	2	5	32,488
Household Total Consumption [$]	37,843	31,023	11,576	31,756	68,908	32,488
Household Total Income [$]	77k	109k	14k	57k	146k	32,488
Household Liquid Wealth [$]	56k	564k	-17k	0.2k	100k	32,488
Household Illiquid Wealth [$]	282k	1,268k	0k	72k	606k	32,488
Household Total Wealth [$]	346k	1,545k	-3k	73k	762k	32,488

Notes. Summary statistics for the estimation sample, which covers the 1999-2013 PSID waves, as well as the pre-sample 1997 wave (because we control for lagged income). Age, Experience (Macro), and Experience (Personal) are calculated for the heads of households. Household total income includes transfers and taxable income of all household members from the last year. Liquid wealth and illiquid wealth are defined following Kaplan, Violante and Weidner (2014). All values are in 2013 dollars using the PCE. Observations are annual and not weighted.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)	-0.086**		-0.082**	-0.049**		-0.046**
	(0.036)		(0.035)	(0.020)		(0.020)
Experience (Personal)		-0.009***	-0.009***		-0.006***	-0.006***
		(0.003)	(0.003)		(0.002)	(0.002)
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	32,486	32,486	32,486	32,486	32,486	32,486
R-squared	0.768	0.768	0.769	0.768	0.769	0.769

Notes. The consumption variables come from the 1999-2013 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 2013), 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 (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. 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 (Macro)	-0.068***		-0.067***	-0.038***		-0.038***
	(0.023)		(0.023)	(0.013)		(0.013)
Experience (Personal)		-0.003*	-0.003*		-0.002*	-0.002*
		(0.002)	(0.002)		(0.001)	(0.001)
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	25,578	25,578	25,578	25,578	25,578	25,578
R-squared	0.776	0.775	0.776	0.776	0.775	0.776

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 (Macro)	-0.069***		-0.067***	-0.038***		-0.038***
	(0.023)		(0.024)	(0.013)		(0.013)
Experience (Personal)		-0.003*	-0.003*		-0.002**	-0.002**
		(0.002)	(0.002)		(0.001)	(0.001)
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	25,578	25,578	25,578	25,578	25,578	25,578
R-squared	0.776	0.775	0.776	0.776	0.775	0.776

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 (Macro)	-0.067**	-0.067***	-0.038**	-0.038***
	(0.023)	(0.022)	(0.013)	(0.012)
Experience (Personal)	-0.003	-0.003**	-0.002	-0.002**
	(0.002)	(0.001)	(0.001)	(0.001)
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	25,578	25,578	25,578	25,578
R-squared	0.776	0.776	0.776	0.776

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 (Macro)	-0.068***		-0.066***	-0.038***		-0.037***
	(0.023)		(0.024)	(0.013)		(0.013)
Experience (Personal)		-0.003*	-0.003*		-0.002**	-0.002**
		(0.002)	(0.002)		(0.001)	(0.001)
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	25,349	25,349	25,349	25,349	25,349	25,349
R-squared	0.775	0.775	0.775	0.775	0.775	0.775

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 (Macro)	-0.017		-0.018	-0.022*		-0.021*
	(0.022)		(0.022)		(0.011)	(0.012)
Experience (Personal)		-0.006***	-0.006***		-0.004***	-0.004***
		(0.001)	(0.001)		(0.001)	(0.001)
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	19,111	19,111	19,111	19,111	19,111	19,111
R-squared	0.761	0.759	0.760	0.761	0.746	0.755

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 (Macro)	-0.063***	-0.059**	-0.063***	-0.054**	-0.033**	-0.030**	-0.031**	-0.031**
	(0.023)	(0.023)	(0.023)	(0.023)	(0.013)	(0.014)	(0.014)	(0.013)
Experience (Personal)	-0.003*	-0.003*	-0.003*	-0.003*	-0.002**	-0.002**	-0.002**	-0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
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	25,578	25,578	25,578	25,578	25,578	25,578	25,578	25,578
R-squared	0.776	0.778	0.776	0.794	0.781	0.783	0.780	0.794

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 (Macro)	-0.064***	-0.066***	-0.065***	-0.065***	-0.036***	-0.037***	-0.036***	-0.037***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.013)	(0.013)	(0.013)	(0.013)
Experience (Personal)	-0.003*	-0.003*	-0.003*	-0.003*	-0.002**	-0.002**	-0.002**	-0.002**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
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	25,578	25,578	25,578	25,578	25,578	25,578	25,578	25,578
R-squared	0.776	0.776	0.776	0.776	0.776	0.776	0.776	0.776

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: **Consumption (PSID), Additional Liquidity Controls**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)	-0.073***		-0.074***	-0.040***		-0.040***
	(0.025)		(0.025)	(0.014)		(0.014)
Experience (Macro) * LLW	0.010		0.014	0.003		0.005
	(0.018)		(0.018)	(0.010)		(0.010)
Low Liquid Wealth	-0.053	0.034**	-0.053	-0.011	0.030**	0.001
	(0.108)	(0.015)	(0.108)	(0.059)	(0.014)	(0.058)
Experience (Personal)		-0.001	-0.000		-0.001	-0.001
		(0.002)	(0.002)		(0.001)	(0.001)
Experience (Personal) * LLW		-0.005*	-0.005*		-0.004	-0.004
		(0.003)	(0.003)		(0.003)	(0.003)
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	25,578	25,578	25,578	25,578	25,578	25,578
R-squared	0.776	0.776	0.776	0.776	0.776	0.776

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.11: **Wealth Accumulation**

	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)$_{t-6}$	0.058***		0.054***	0.038***		0.036***
	(0.019)		(0.019)	(0.011)		(0.012)
Experience (Personal)$_{t-6}$		0.005***	0.005***		0.003***	0.003***
		(0.001)	(0.001)		(0.000)	(0.000)
Observations	12,788	12,788	12,788	12,788	12,788	12,788
R-squared	0.332	0.333	0.334	0.332	0.333	0.334
Experience (Macro)$_{t-8}$	0.051*		0.043	0.036**		0.032*
	(0.026)		(0.027)	(0.016)		(0.017)
Experience (Personal)$_{t-8}$		0.006***	0.006***		0.003***	0.003***
		(0.001)	(0.001)		(0.001)	(0.001)
Observations	9,288	9,288	9,288	9,288	9,288	9,288
R-squared	0.319	0.320	0.320	0.319	0.320	0.320
Experience (Macro)$_{t-10}$	0.029**		0.025**	0.020***		0.018**
	(0.011)		(0.012)	(0.007)		(0.007)
Experience (Personal)$_{t-10}$		0.004***	0.004***		0.002***	0.002**
		(0.002)	(0.002)		(0.001)	(0.001)
Observations	8,027	8,027	8,027	8,027	8,027	8,027
R-squared	0.294	0.295	0.295	0.295	0.295	0.295
Experience (Macro)$_{t-12}$	0.024*		0.018	0.018**		0.014*
	(0.014)		(0.014)	(0.008)		(0.008)
Experience (Personal)$_{t-12}$		0.006***	0.006***		0.003***	0.003***
		(0.001)	(0.001)		(0.001)	(0.001)
Observations	5,418	5,418	5,418	5,418	5,418	5,418
R-squared	0.450	0.452	0.452	0.450	0.452	0.453
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

As the second source of data on consumption choices, we turn to the Nielsen Homescan Dataset. Our data sample consists of 3,171,833 observations of 105,061 households. A detailed description of the dataset is provided in Section V.B. Table A.12 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 precise experience measures than the PSID. However, we are not able to construct the same type of macro and personal unemployment experience proxies as in the PSID because Nielsen provides no information about households' prior residence or employment status (pre-sample period). We thus 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 necessarily biased, as it is less precise at the beginning of the sample and for shorter household spells. We therefore report the estimations employing only the macro-experience measure.[29]

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 (see also Mian, Rao, and Sufi (2013) and Berger and Vavra (2015)). Empirical analyses can exploit this insight since better measures of housing prices have become available. Specifically, we extract Zillow's Home Value Index at the local ZIP code level as a proxy for local housing prices and merge it with the Nielsen

[29] We have re-estimated 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.

Table A.12: **Summary Statistics (Nielsen)**

Variable	Mean	SD	p10	p50	p90	N
Age	50	12	33	49	67	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
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

Notes. The table reports the summary statistics of the monthly Nielsen data from 2004-2013. Experience (Macro) is households' lifetime experience of national unemployment rates. Coupon 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.

data.[30] 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.[31]

The estimation model is exactly as delineated in equation (11). Table A.13 presents results from regression specification (11). 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 deviation increase in unemployment experiences is associated with a $255 decline in annual consumption of non-durables,

[30] 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).

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

Table A.13: **Experience Effects and Monthly Consumption (Nielsen)**

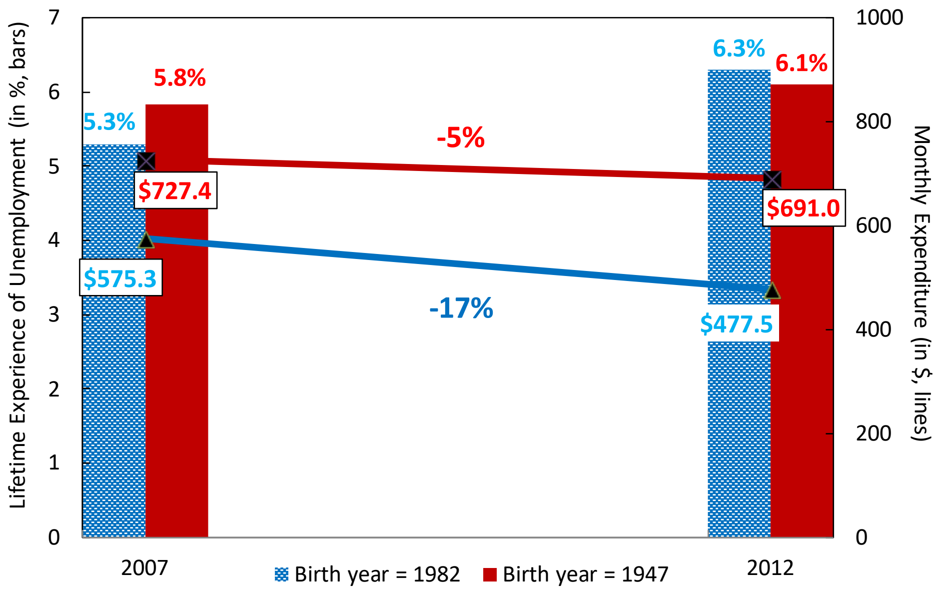
	(1)	(2)	(3)	(4)
Experience (Macro)	-0.166***	-0.165***	-0.172***	-0.172***
	(0.055)	(0.055)	(0.027)	(0.027)
Unemployment rate (county)		-0.005***		-0.005***
		(0.001)		(0.001)
Income control	Yes	Yes	Yes	Yes
Wealth control	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Age fixed effects	Yes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

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.2, we illustrate the economic magnitude of the estimates in the context of unemployment conditions during the Great Recession, which falls in the Nielsen sample period. The average monthly unemployment rate from 2008-2012 was 8.1%, with the maximum during the period being 10%. Comparing these numbers with historical averages, the average unemployment rate during the 60 years prior to

Figure A.2: **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.

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.13) 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. We construct lifetime experiences as the weighted average of experienced unemployment outcomes since birth, using linearly declining weights. 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.14 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.15 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:

Table A.14: **Summary Statistics (CEX)**

Variable	Mean	SD	p10	p50	p90	N
Age	51	17	29	49	75	439,315
Experience (Macro) [%]	6.1	0.31	5.80	6.1	6.6	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

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.15: **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.

A one standard-deviation increase in unemployment experience is associated with a decline of $701 in annual consumption and $460 in annual non-durable consumption. The estimate on annual total consumption is smaller than the PSID estimate ($1,099 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, $d_t+x'_{i,t}\psi=\alpha+\beta_1\cdot\mathrm{age}+\beta_2\cdot\mathrm{age}^2$, 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.[32]

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).

Figure B.1 depicts the resulting average consumption trends of rational and experience-based learners during their working years, which are the years used in the regressions. The graph hints at a pattern that, early in life, experience-based

[32] 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**

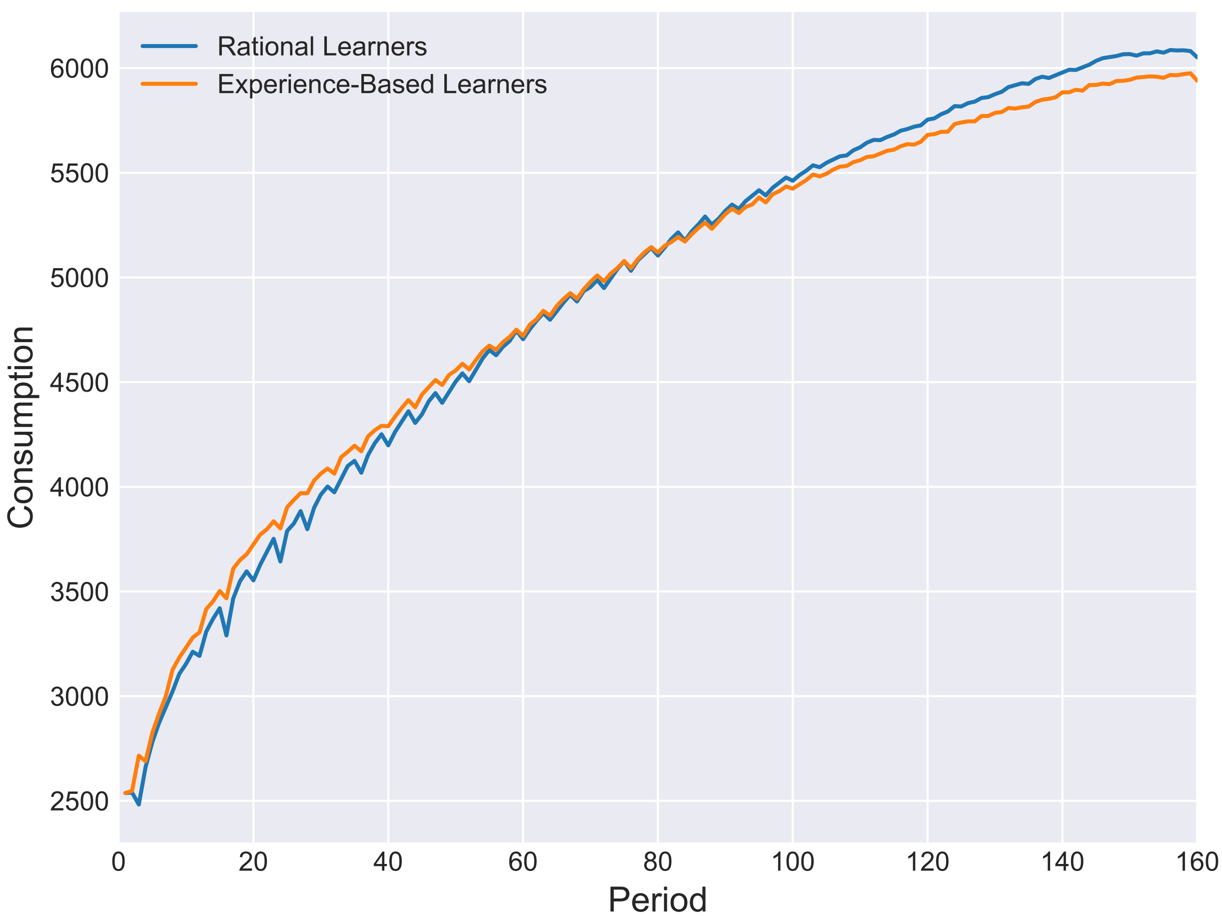
Parameter	Low Education	High Education	Source (from Low, Meghir, and Pistaferri (2010))
γ	1.5	1.5	Text
σ_a	0.226	0.229	Table 1
σ_ζ	0.095	0.106	Table 1
$P(\zeta)$	.25	.25	Text
δ	.049	.028	Table 2
λ^e	.67	.72	Table 2
λ^n	.76	.82	Table 2
b	.75	.75	Text
r (yearly)	.015	.015	Text
β	$1/(1+r)$	$1/(1+r)$	Text
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.

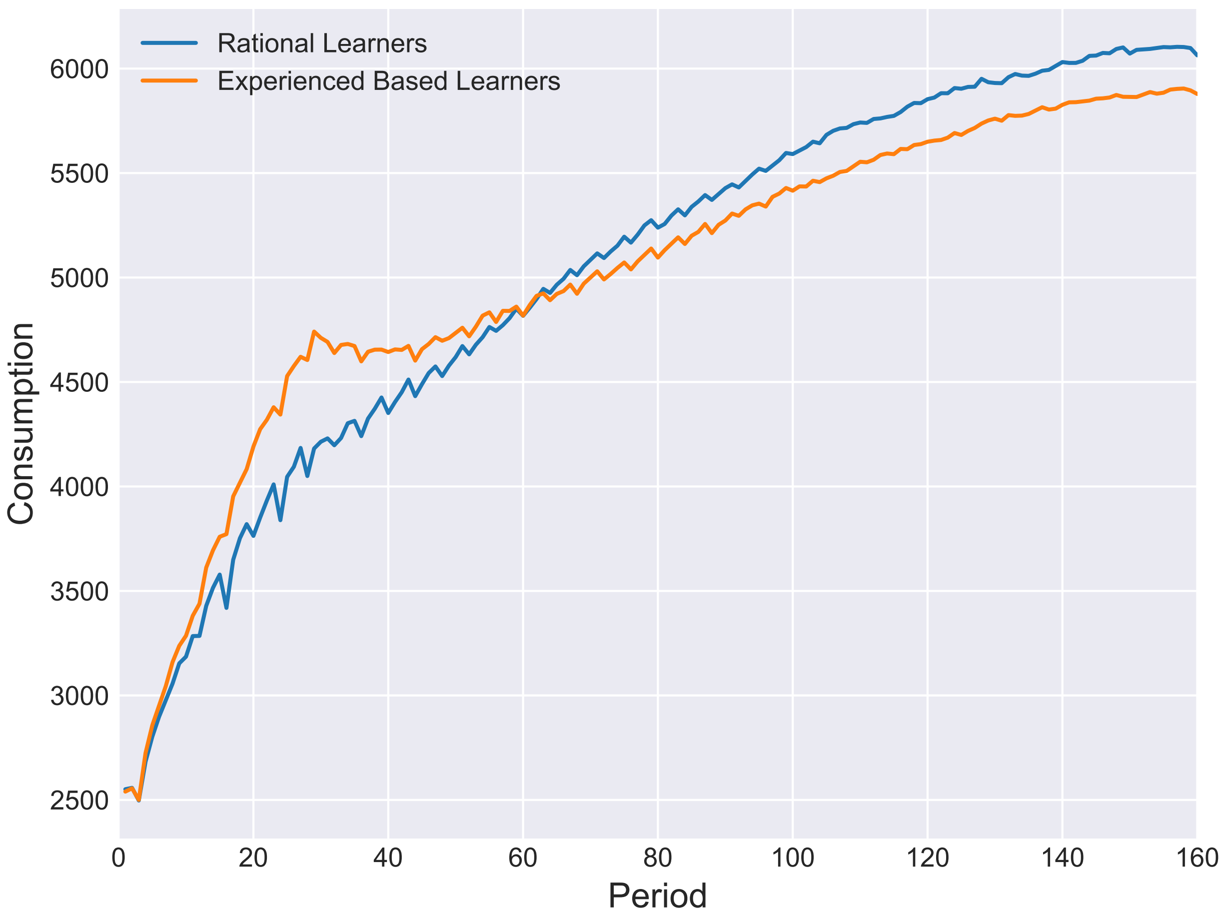
[c] See https://web.archive.org/web/20190228193653/https://fns-prod.azureedge.net/sites/default/files/Trends1999-2005.pdf. Accessed via https://web.archive.org/web/20190228195514/https://www.fns.usda.gov/snap/trends-food-stamp-program-participation-rates-1999-2005.

Figure B.1: **Average Life-Cycle Consumption**



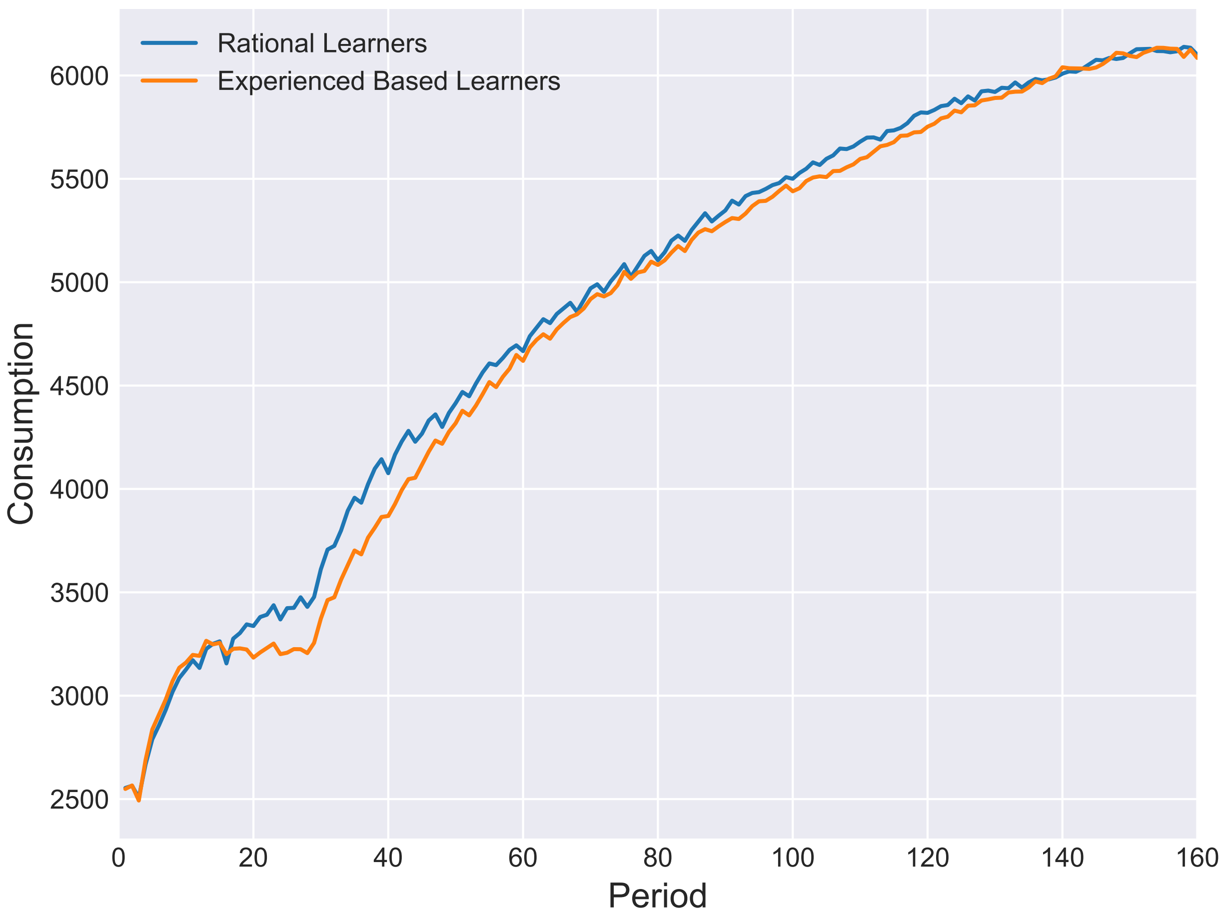
Notes. Average consumption for rational learners and experience-based learners (with $\lambda = 1$) in the low-education group, based on 10,000 lifetime simulations for each type.

Figure B.2: **Average Life-Cycle Consumption for Agents with Good Realizations Early in Life**



Notes. 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 B.3: **Average Life-Cycle Consumption Patterns for Agents with Bad Realizations Early in Life**



Notes. 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.

learners underestimate the probability of job destruction, spend more, and must then save more towards the end of their working life.

Figure B.2 provides an amplified illustration of the differences. In this figure, we only consider the subset of experience-based learners in the simulation who, at period 30, have a believed delta of 0.025 or less and, in the rational case, the subset of agents who would have a believed delta of 0.025 or less at period 30 if they were experience-based learners. Since the true probability of job destruction is 0.049, these agents were "lucky" early in life. For these consumers, the trend of over-consumption among experience-based learners in the early periods is much more pronounced.

Figure B.3 illustrates the opposite scenario. Here, we only consider the subset of experience-based learners in the simulation who, at period 30, have a believed delta of 0.1 or greater, as well as the corresponding rational agents. In light of the true probability of job destruction of 0.049, these agents have had bad luck early in life. This "unlucky" group of experience-based learners has a markedly different savings pattern. They consistently consume less than their rational counterparts for almost their entire lives. Moreover, the illustration hints at an additional prediction, wealth build-up due to excess frugality.