Scarred Consumption*

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Abstract

We show that prior lifetime experiences can "scar" consumers. Consumers who have lived through times of high unemployment exhibit persistent pessimism about their future financial situation and spend significantly less, controlling for the standard life-cycle consumption factors, even though their actual future income is uncorrelated with past experiences. Due to their experience-induced frugality, scarred consumers build up more wealth. We use a stochastic life-cycle model to show that the negative relationship between past experiences and consumption cannot be generated by financial constraints, income scarring, and unemployment scarring, but is consistent with experience-based learning.

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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

More than a decade after the Great Recession, consumers have been slow to return to prior consumption levels (Petev et al. 2011, De Nardi et al. 2012). As the quote above suggests, the crisis appears to have "scarred" consumers. Consumption has 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]

What, then, explains such long-term effects of a macroeconomic crisis on consumption? 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-based learning*. We show that consumers' past lifetime experiences of economic conditions have a long-lasting effect on beliefs and consumption, 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 receive extra weight when individuals form expectations about future realizations of the same variables.[2] Here, we ask whether a similar mechanism is at work when individuals experience high unemployment rates. We apply the linearly declining weights estimated in prior work to both national and

[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).

[2] Theoretical papers on the macro effects of learning-from-experience in OLG models include Ehling, Graniero, and Heyerdahl-Larsen (2018), Malmendier, Pouzo, and Vanasco (2018), Collin-Dufresne, Johannes, and Lochstoer (2016), and Schraeder (2015). The empirical literature starts from Kaustia and Knüpfer (2008) and Malmendier and Nagel (2011, 2015).

local unemployment rates that individuals have experienced over their lives so far, and to their personal unemployment experiences. We show that past experiences predict both consumer pessimism (beliefs) and consumption scarring (expenditures) as well as several other empirical regularities, including generational differences in consumption patterns, after controlling for wealth, income and other standard determinants. At the same time, past experiences do not predict future income, after including the same controls, and predict, if anything a positive wealth build up.

We start by presenting four baseline findings on the relation between past experiences and consumption, beliefs, future income, and wealth build-up. We first document the long-lasting effect of past experiences on consumption. Using the *Panel Study of Income Dynamics* (PSID) from 1999-2013, we find that past macro and personal unemployment experiences have significant predictive power for consumption, controlling for income, wealth, age fixed effects, a broad range of other demographic controls (including current unemployment), as well as state, year, and even household fixed effects. To the best of our knowledge, our analysis is the first to estimate experience effects also within household, i. e., controlling for any unspecified household characteristics. Without household dummies, the identification comes both from cross-household differences in consumption and unemployment histories, and from how these differences vary over time. With household dummies, the estimation relies solely on within-household variation in consumption in response to lifetime experiences.[3] In both cases, the effects are sizable. A one standard deviation increase in the macro-level measure is associated with a 3.3% ($279) decline in annual food consumption, and a 1.6% ($713) decline in total consumption. A one standard deviation increase in personal unemployment experiences is associated with 3.7% ($314) and 2.1% ($937) decreases in annual spending on food and total consumption, respectively. The results are robust to variations in accounting for the spouse's experience, excluding last year's experience, or using different weights, from

[3] We have also estimated a model with cohort fixed effects. 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. The unemployment experience measure of a given cohort varies over time depending on where the cohort members have resided over their prior lifetimes.

equal to steeper-than-linearly declining.[4]

Second, we document that consumers' past experiences significantly affect beliefs. Using the *Michigan Survey of Consumers* (MSC) from 1953 to 2013, we show that people who have experienced higher unemployment rates over their lifetimes so far have more pessimistic beliefs about their financial situation in the future, and are more likely to believe that it is not a good time to purchase major household items in general. Importantly, 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 actual future income, up to three PSID waves (six 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 there is a strong reaction to prior lifetime experiences in terms of beliefs and 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. Our fourth finding confirms this prediction in the data. Using a horizon of three to seven PSID waves (6 to 14 years), we find past lifetime experiences predict liquid and illiquid wealth build up, in particular for past personal unemployment experiences. Unobserved wealth effects, the main alternative hypothesis, do not predict wealth build up, or even predict the opposite.

These four baseline results—a lasting influence of economic experiences in the past on current expenditure decisions and on consumer optimism, but the lack of any effect on actual future income, plus positive wealth build-up—are consistent with our hypothesis: Consumers over-weigh experiences they made during their prior lifetimes when forming beliefs about future realizations and making consumption choices, as predicted by models of experience-based learning (EBL). Considered jointly, and given the controls included in the econometric models, the results so far already distinguish our hypothesis from several alternative explanations: The inclusion of age controls rules out certain life-cycle effects, such as an increase in

[4] We also included lagged consumption in the estimation model to capture habit formation but do not find a significant effect, while the experience proxy remains significant.

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 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 allows for the inclusion of household fixed effects and thus to control for time-invariant unobserved heterogeneity.

To further distinguish EBL 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 for financial constraints as well as "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. We further extend the Low et al. (2010) model to allow for "unemployment scarring"—the notion that job loss itself may induce a negative, permanent wage shock.[5] We contrast these explanations with EBL by simulating the model for both Bayesian and experience-based learners.

First, we utilize the simulations to 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.

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

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 consumption 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 both 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 the wealth, liquidity, and income controls, and using different data sets; (2) a study of the implications of EBL for the quality of consumption and of the heterogeneity in consumption patterns across cohorts, 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, and all estimations are replicated both with and without household fixed effects. 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]

[6] Our variants of wealth and income controls also address the concern that consumption may

As another, out-of-sample corroboration of our results, we replicate the PSID results in two additional data sets, the Consumer Expenditure Survey (CEX) and the Nielsen Homescan Data. The CEX contains a more comprehensive list of product categories, and sheds light on the impact of unemployment experience on durable and total consumption. 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 estimated magnitudes in the Nielsen and CEX data are very similar to those in the PSID.[7]

Next, we exploit the richness of the Nielsen data to show that prior experiences affect consumption also at the qualitative margin. 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^{th} than at the 10^{th} percentile of unemployment experiences.

We then test a unique prediction of EBL: Since a given macroeconomic shock 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

be a non-linear function of assets and earnings (Arellano, Blundell, and Bonhomme 2017).

[7] 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 is not suitable for the analysis.

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, the 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).[8] 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

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

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 element 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, 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 their 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 (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 replicate the results in the CEX and Nielsen data. Section VI shows further results on the quality margins of consumption and the cross-cohort heterogeneity in responses to shocks. Section VII discusses the aggregate implications of experience-based learning for consumer spending and concludes.

II Data and Variable Construction

II.A Measure of Consumption Scarring

Our 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

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.

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. Moreover, the experience-based learning model has a second implication: 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 raw time-series of household expenditures (from the Nielsen data) in Figure 1 illustrates the hypothesized effects. Expenditures are expressed as deviations from the cross-sectional mean in each month. In general, the spending of younger cohorts (below 40) is more volatile than that of older cohorts, consistent with younger cohorts exhibiting greater sensitivity. Zooming in on the Great Recession period, we also see that the spending of younger cohorts was significantly more negatively affected than those of the other age groups. Such patterns are consistent with consumers being scarred by recession experiences, and more so the younger they are.

To formally test the experience-effect hypothesis, we construct measures of past experiences that apply the weighting function estimated in prior work to the experience of high and low unemployment rates. We focus on experiences of unemployment rates following Coibion, Gorodnichenko, and Hong (2015), who single out unemployment as the most spending-relevant variable. We construct measures of past experiences on both the macro (national and local) level and the personal level. The macro measure captures the experience of living through various spells of unemployment rates. The personal measure captures personal situations experienced so far.

Specifically, unemployment experience accumulated by time t is measured as

$$E_t = \sum_{k=0}^{t-1} w\left(\lambda, t, k\right) W_{t-k}, \tag{1}$$

where W_{t-k} is the unemployment experience in year $t-k$, and k denotes the time lag.[9] 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

$$w(\lambda, t, k) = \frac{(t-k)^{\lambda}}{\sum_{k=0}^{t-1} (t-k)^{\lambda}}. \tag{2}$$

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

[9] In the empirical implementation, we utilize unemployment information from birth up to year $t-1$ while the theoretical p_t is constructed based on realizations of W_{t-k} for $k = 0, ..., t-1$, i. e., from the moment of birth to the realization at the beginning of the current period. It is somewhat ambiguous what corresponds best to the theoretical set-up, especially as, in practice, only backward looking (macro) information becomes available to every individual. However, since we do control for (macroeconomic and personal) contemporaneous unemployment in all regressions, the inclusion or exclusion of macro or personal unemployment at time t in the experience measure does not make a difference to the estimation results.

$\lambda > 0$, the weighting scheme emphasizes individuals' recent experiences, letting them carry higher weights, while still allowing for an 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 linearly declining weights ($\lambda = 1$), which approximate the weights estimated in Malmendier and Nagel (2011, 2015). For robustness, we also conduct the analysis using $\lambda = 3$. 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 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.[10]

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.[11] These data limitations

[10] 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.

[11] 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.

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, $t-5$ to $t-1$, either by themselves or combined with national unemployment rate data from birth to year $t-6$. 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 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 $t-5$ to $t-1$ and national unemployment rates from birth to $t-6$, with weights calculated as specified in (2).

II.B 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 will later replicate the results in Nielsen and CEX data. Compared to those data, the PSID has the advantage of containing rich information on household wealth, a key variable in consumption models.

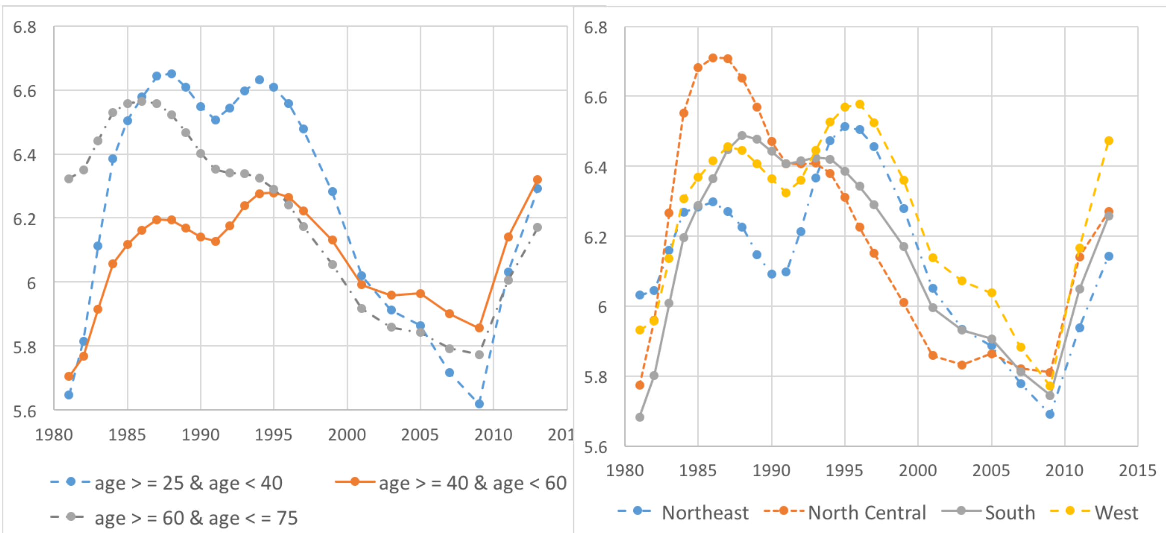
The PSID started its original survey in 1968 on a sample of $4,802$ family units. Along with their split-off families, these families were surveyed each year until 1997, when the PSID became biennial. We focus on data since 1999 when the PSID started to cover more consumption items (in addition to food), 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, and to further 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 most recent years state-level unemployment rates, $t-5$ to $t-1$, either by themselves or combined with national unemployment rate data from birth to year $t-6$. 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.[12] We employ the same approach as with the state-level data regarding the early-years data limitations.

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.

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. 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

[12] 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.

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.

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

Variable	Mean	SD	p10	p50	p90	N
Age	47.61	12.06	32	47	65	33,164
Experience (Macro) [in %]	6.00	0.28	5.67	5.97	6.36	33,164
Experience (Personal) [in %]	4.55	14.27	0.00	0.00	18.92	33,164
Household Size	2.75	1.45	1	2	5	33,164
Household Food Consumption [in $]	8,452	5,153	2,931	7,608	14,999	33,164
Household Total Consumption [in $]	44,692	31,786	16,626	39,608	76,823	33,164
Household Total Income [in $]	80k	51k	22k	70k	155k	33,164
Household Liquid Wealth [in $]	38k	320k	-23k	0k	91k	33,164
Household Illiquid Wealth [in $]	222k	919k	1k	71k	513k	33,164
Household Total Wealth [in $]	260k	1,007k	-3k	72k	636k	33,164

Notes. Summary statistics for the estimation sample, which covers the 1999-2013 PSID waves and exclude observations with a total income below the 5^{th} or above the 95^{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.

Table 1 shows the summary statistics for our sample. We focus on household heads from age 25 to 75.[13] In the main analysis, we run the regressions excluding observations with total family income below the 5^{th} or above the 95^{th} percentile in each wave. The sample truncation addresses known measurement errors in the income variable.[14] After dropping the individuals for whom we cannot construct the experience measures (due to missing information about location or employment status in any year from t to $t-5$), and observations with missing demographic controls or that only appear once, we have 33,164 observations. The mean macro experience is 6.0%, and the mean personal experience is 4.6%. Average household

[13] 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.

[14] 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 5^{th} and 95^{th} percentiles, but there is less consensus in the upper and lower five percentiles. As a robustness check, we use the full sample, cf. Appendix-Table A.1.

food consumption and average household total consumption in our sample are \$8,452 and \$44,692, respectively (in 2013 dollars).

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. Prior lifetime experiences have been found to have long lasting effects on individual beliefs and decision-making in the realms of stock returns, bond returns, inflation, and mortgage choices. Here, we ask whether a similar mechanism might help to explain patterns in consumption expenditures. Specifically, 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 individual beliefs and consumption expenditures, 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 consumption, UE_{it} is i's macroeconomic and UEP_{it} her personal unemployment experience over her prior life, 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.[15] We conduct our empirical analysis both with food consumption, following the earlier literature, and with total consumption as dependent variables.[16] Standard errors are clustered at the cohort level. All the regression results are quantitatively and qualitatively similar when clustered by household, household-time, and cohort-time 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, based on the idea of experience effects, is that consumers who have experienced higher unemployment spend less on average and, hence, that both coefficients are negative.

Identification. We estimate the model both with and without household dummies. In the former case, we identify experience effects solely from time variation in the within-household co-movement of consumption and unemployment histories. In the latter case, identification also comes from time variation in cross-sectional differences in consumption and unemployment histories between households.

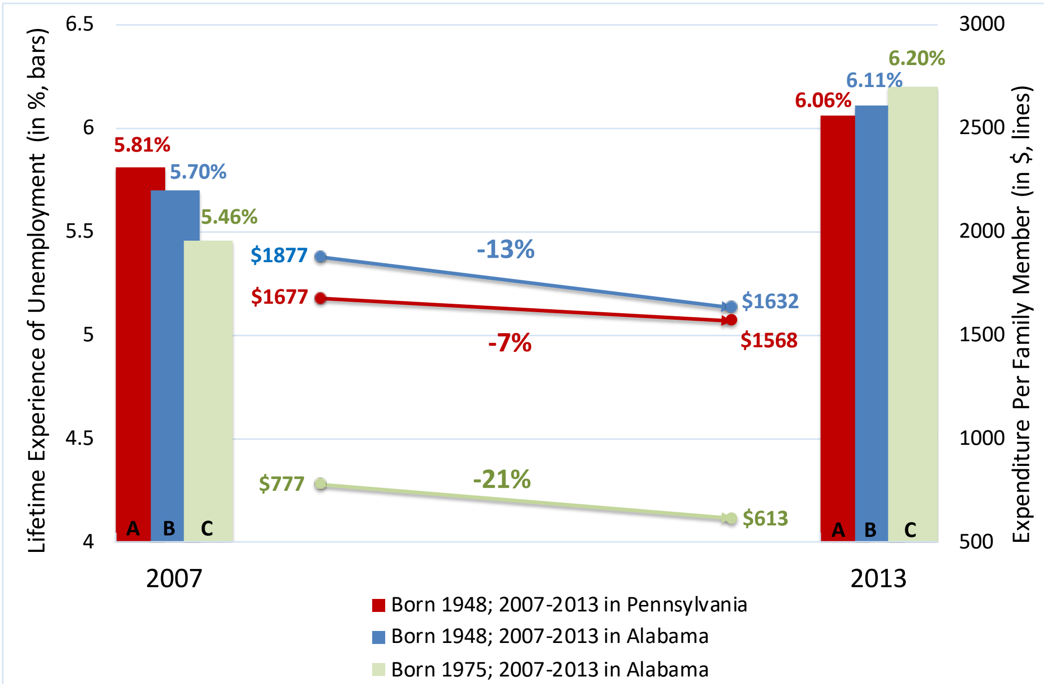
We illustrate the sources of identification with a simple example of the unemployment experiences and household consumption of three individuals in our PSID data over the course of the Great Recession. Consider two individuals (A and B) who have the same age (born in 1948) but live in different states (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

[15] 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.

[16] Food consumption had been widely used in the consumption literature largely because food spending used to be the only available consumption variable in the PSID before 1999. We are separating out the results on food consumption post-1999 partly for comparison, but also in case the data is more accurate as some researchers have argued. Food consumption and total consumption come directly from the PSID Consumption Data Package 1999-2013.

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.

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) with (log) food consumption as the dependent variable in the upper panel and with (log) total consumption in the lower panel. All regressions control for first- and second-order (logs of) income, lag income, liquid wealth, illiquid wealth, and for all other control variables listed above as well as the fixed effects indicated at the bottom of the table. Columns (1)-(3) show results without household fixed effects, and columns (4)-(6) with household fixed effects. All estimated coefficients on the control variables (not shown) have the expected sign, consistent with prior literature.

The estimated negative coefficients indicate that both macroeconomic and personal unemployment experiences predict reduced consumption expenditures in the long-run. In the estimations predicting food consumption, shown in the upper half of the table, we find a significantly negative effect of both macroeconomic and personal experiences, controlling for the current unemployment status. The economic magnitudes remain the same whether we include the two types of experience measures separately or jointly, though the statistical significance of the macro measure diminishes somewhat in the specifications without household fixed effects (columns 1-3) when we include both measures jointly (column 3). When we introduce household fixed effects (in columns 4-6), the estimated coefficient on macro experience becomes larger and more precisely estimated. Based on the column (6) estimates, a one standard-deviation increase in macroeconomic unemployment experience leads to a 3.3% decrease in food consumption, which translates to $279 less annual spending. Hence, the economic magnitude of the macro experience effect alone is large, particularly considering that the estimates reflect behavioral change due to fluctuation in the macro-economy, not direct income shocks.

As expected, the estimated personal experience effects become slightly smaller when we include household fixed effects. The decrease reflects that experience effects (also) predict cross-sectional differences in consumption between households with “mostly good” versus “mostly bad” lifetime experiences, and this component of experience effects is now differenced out. Nevertheless, the effect of personal experience is more than two times larger than macroeconomic experience in absolute value.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	-0.097**		-0.091*	-0.120**		-0.117**
	(0.047)		(0.047)	(0.054)		(0.055)
Experience (Personal)		-0.322***	-0.320***		-0.263**	-0.260**
		(0.097)	(0.097)		(0.119)	(0.119)
R-squared	0.192	0.193	0.193	0.541	0.542	0.542
Dependent Variable: Total Consumption						
Experience (Macro)	-0.022		-0.018	-0.059***		-0.057***
	(0.019)		(0.019)	(0.021)		(0.021)
Experience (Personal)		-0.178***	-0.177***		-0.148***	-0.147***
		(0.030)	(0.030)		(0.031)	(0.031)
R-squared	0.573	0.574	0.574	0.788	0.788	0.788
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	No	No	No	Yes	Yes	Yes
Observations	33,164	33,164	33,164	33,164	33,164	33,164

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. 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 5^{th} or above the 95^{th} percentile in each wave Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

The estimated effect of a one standard-deviation increase in personal unemployment experiences is similar to that of macro experiences. It predicts a 3.7% decrease in food consumption, which is approximately \$314 in annual spending.

When we use total consumption as the dependent variable, in the lower half of Table 2, the economic magnitude of the macro experience effect decreases in the specification without household fixed effects (columns 1-3) but is again as precise as in the case of food consumption when we include household fixed effects (columns 4-6). In terms of economic magnitude, a one standard-deviation increase in macro experience lowers total consumption by 1.6%, or \$713 annually, based on the estimated coefficient from column (6). A one standard-deviation increase in personal lifetime unemployment experience lowers total consumption by 2.1%, or \$937 annually.

We also re-estimate the results on the entire sample, without excluding observations in the top and bottom 5 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.

The results are also robust 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. 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), to excluding the experience of year $t-1$ from the measure (Appendix-Table A.5), and to using different weighting λ (Appendix-Table A.6). In terms of alternative approaches to calculating standard errors, we estimate regressions with standard errors clustered at different levels in Appendix-Table A.7. We also vary the weighting of observations by applying the PSID family weights, shown in Appendix-Table A.8. (We do not use PSID family weights in the main regression due to efficiency concerns.)

Overall, 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. To what extent do personal lifetime experiences color beliefs about future outcomes?

We relate past lifetime experiences of economic fluctuations to current beliefs about future economic prospects, using the Reuters/Michigan Survey of Consumers (MSC) microdata on expectations from 1953 to 2013. 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 includes a total of 292,708 observation. On average, 605 individuals are surveyed each month.

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.

We also extract income and all other available demographic variables, including education, marital status, gender, and age of the respondent.[17] The explanatory variable of interest is again our measure of 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)"

[17] 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.

variable for each of individual i from birth until year t, 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 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 present the estimates of the relation between prior unemployment-rate experiences and 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 -0.015 and -0.011. 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. This renders the estimates in columns (1) to (3) open to alternative interpretations, especially unobserved wealth effects. When we include income in columns (2) and (3), the estimation has the expected positive coefficient, and the same holds for demographics that might proxy for unobserved wealth (e. g., education) in column (3). 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 at the 10th percentile.

The estimations based on the second question, shown in columns (4) to (6), generate 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 -0.061 to -0.047. Respondents who have experienced unemployment rates at the 90th percentile of the sample are around

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)	-0.015***	-0.013***	-0.011***	-0.061***	-0.052***	-0.047***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Unemployment rate	-0.015***	-0.015***	-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***		0.050***	0.044***
		(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	221,115	207,742	206,216	214,695	201,909	200,437
R-squared	0.045	0.047	0.047	0.055	0.062	0.066

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 2012. Standard errors, shown in parentheses, are robust to heteroskedasticity. *, **, *** denote 10%, 5%, and 1% significance, respectively.

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 also addresses concerns unobserved wealth and other unobserved financial constraints even further, beyond the stability 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).

III.C Past Experiences and Future Income

Next we ask whether the long-term reduction in consumption after past unemployment experiences, as well as the ensuing consumer pessimism, might be 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.

To test whether past unemployment experiences are correlated with (unobserved) determinants of households' future income, we re-estimate our baseline model from equation (3) with the dependent variable changed to future income either one or two or three survey waves in the future, i. e., two, four, and six years ahead.

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,[18] the estimated coefficients of personal unemployment experiences are all positive, small, and insignificant. For macroeconomic experiences, we estimate small negative coefficients, which are also insignificant with the exception of the estimation predicting

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

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

	Income_{t+2}	Income_{t+4}	Income_{t+6}
Experience (Macro)	-0.030	-0.044*	-0.050
	(0.020)	(0.023)	(0.030)
Experience (Personal)	0.010	0.021	0.017
	(0.013)	(0.013)	(0.021)
Income controls	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes
Observations	15,710	11,258	7,641
R-squared	0.865	0.884	0.903

Notes. The dependent variables are future income in two, four, and six years, respectively. "Experience (Macro)" is the macroeconomic experience measure of unemployment, and "Experience (Personal)" is the personal experience measure. 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 5^{th} or above the 95^{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.

income four 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 volatiltiy 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). Appendix-Table A.12 shows the results for either measure, two, four or six years ahead (i.e., $t+2$, $t+4$, or $t+6$). 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.

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 even 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 seven survey waves (14 years) in the future. We consider both liquid wealth and total wealth. This analysis also ameliorates potential concerns about the quality of the consumption data and alternative life-cycle interpretations of our findings.

Figure 4 summarizes the coefficients of interest graphically for 10 regressions, namely, the cases of wealth at $t+6$, $t+8$, $t+10$, $t+12$, and $t+14$. The upper part shows the coefficient estimates when studying the impact on liquid wealth, and the lower part shows the estimates for total wealth. All coefficient estimates are positive. The impact of macro experiences is smaller and (marginally) significant only in a few cases, namely, for total wealth in the more recent years and for liquid wealth further in the future. The estimates of the role of personal lifetime experiences are much larger and typically significant, with coefficients ranging from 0.02 to 0.03 for liquid wealth and from 0.08 to 0.10 for total wealth. The estimates imply that a one-standard deviation increase in personal lifetime experiences of unemployment will lead to additional precautionary savings and resulting wealth build-up of about 1.3% or $4,500 ten years later. In other word, households who have experienced high unemployment tend to accumulate more wealth down the road. Appendix-Table A.13 provides the details on the coefficient estimates of both experience measures.

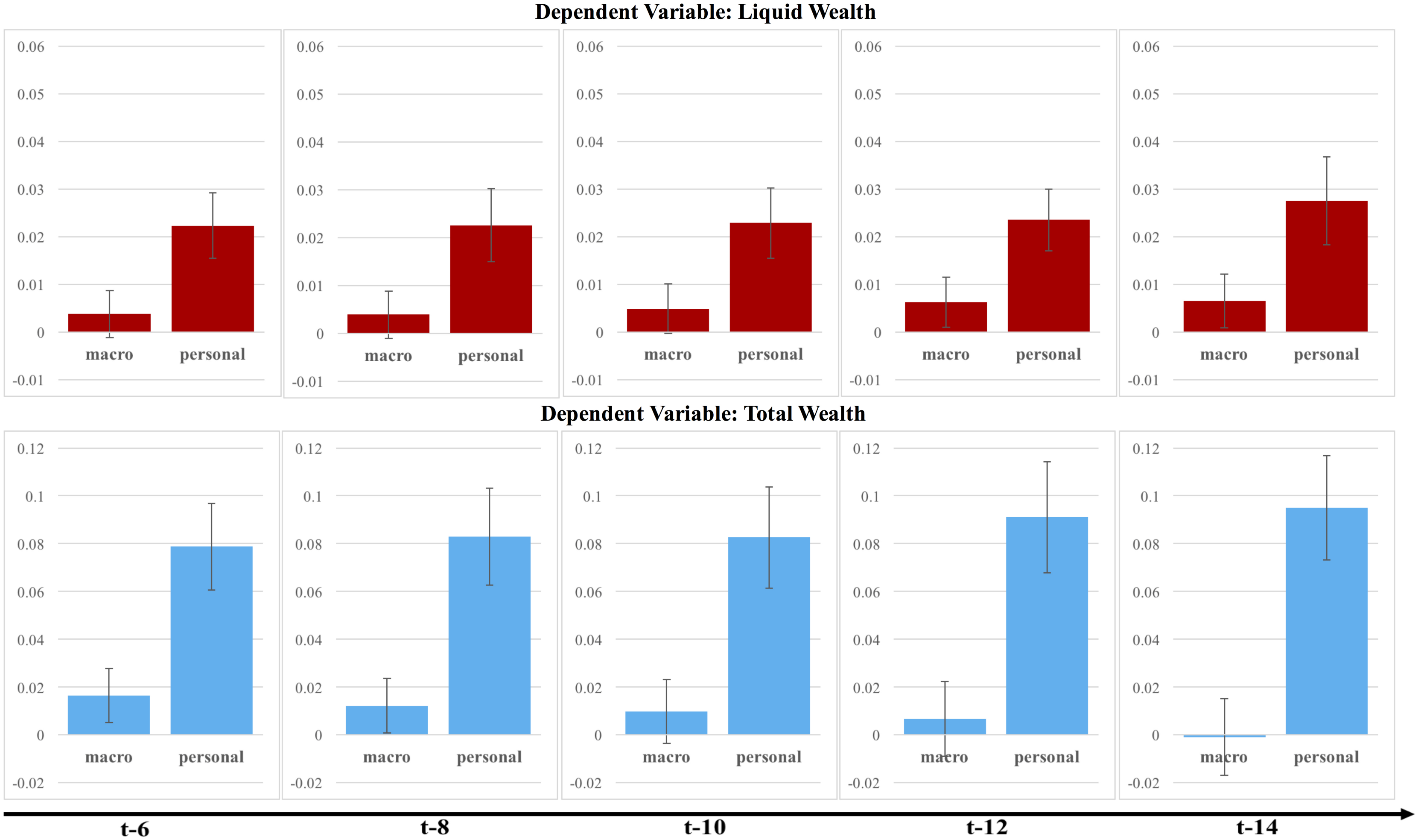
In summary, individuals' lifetime experiences strongly predict consumption expenditure, and beliefs about future economic conditions appear to play a role in explaining this result. However, such beliefs do not seem to be consistent with actual income and wealth changes. In fact, we see evidence of a positive relationship between past experience and future wealth build-up.

IV Consumption with Experience-based Learning

Our four baseline results suggest that past experiences can "scar" consumers. The combination of expenditure, belief, and future-wealth results 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.

We utilize the Low, Meghir, and Pistaferri (2010) model to account for several frictions and possible confounds, and to illustrate that, for a wide range of pa-

Figure 4: **Wealth Build-up: Estimated Coefficients and Confidence Intervals for Experience Measures**



Notes. The upper five graphs (red bars) present the estimates when we re-estimate our empirical model using liquid wealth as the dependent variable. The lower five graphs (blue bars) show the estimates when we use total wealth as the dependent variable. The five graphs in horizontal order show the estimated coefficients when we use 6-year lagged, 8-year lagged, 10-year lagged, 12-year lagged and 14-year lagged experience measures respectively. All the confidence intervals are at 90% confidence level.

rameterizations, we can distinguish experience effects, even directionally. The Low et al. framework captures a broad array of standard life-cycle consumption factors, 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. Moreover, we extend the Low et al. model to include "unemployment scarring," i. e., the notion that unemployment, once experienced, makes individuals inherently less employable. We distinguish both income scarring and unemployment scarring as well as other life-cycle features from scars due to longlasting experience effects. 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.

Towards that end, we introduce two classes of consumers into the model: standard rational agents 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 maximizing the financial constraints of consumers, we are able to derive the sharpest distinction between the role of experience effects and financial constraints.[19] 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 experienced-based learners may make different labor supply choices depending on their concern about future employment and desire to save.

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

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

this component is estimated via regression in Low et al. and of the form[20]

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

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 experienced-based learners reduce their consumption beyond this bar.

Belief Formation. Both types of consumers, rational and experience-based learners, know the model, but differ in their beliefs about the probability of job loss δ.

[20] 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.

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. Applying specification (1), with weighting scheme (2), we obtain

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

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

$$w(\lambda, t, k) = \frac{(t-k)^{\lambda}}{\sum_{k=0}^{t-1} P_{i,t}\left(t-k\right)^{\lambda}}. \tag{10}$$

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

Model Estimates on Experience Effects in Consumption. We simulate the consumption-saving decisions for both rational and behavioral consumers using the parameters in Table 5.[21] 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, we separate consumers who were "lucky" and "unlucky" early in life, in terms of their earnings in Figures B.2 and B.3. The graphs illustrate the corresponding over- and underconsumption of experience-based learners during their early lifetime, 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 corresponds to the empirical relationship we found in Section III.D.

Using the simulated values, we estimate the relationship between consumers' unemployment experience and consumption behavior, controlling for income and wealth. The corresponding OLS regressions are in Table 6, columns (1) and (2), for rational consumers, and in columns (3) and (4) for experience-based learners. In the case of rational agents, prior experiences do not actually enter their belief

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

Table 5: **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

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 in addition to the experience-effect proxy.

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 6, 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 without and with 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.

Table 6: **Estimations with Model-Simulated Data**

	(1) Rational	(2) Rational	(3) EBL	(4) EBL
$\lambda = 1$:				
Income	0.576	0.383	0.606	0.398
	(244.87)	(67.63)	(197.97)	(55.72)
Wealth		0.261		0.264
		(52.30)		(57.34)
Unemployment Experience	0.0975	0.398	-0.392	-0.762
	(2.08)	(3.24)	(-9.01)	(-9.49)
$\lambda = 3$:				
Income	0.567	0.379	0.619	0.401
	(137.52)	(50.58)	(163.45)	(52.05)
Wealth		0.265		0.270
		(40.46)		(63.31)
Unemployment Experience	0.280	0.286	-0.200	-0.576
	(4.85)	(3.39)	(-7.48)	(-9.97)
$\lambda = 0$:				
Income	0.569	0.382	0.598	0.397
	(209.47)	(236.72)	(198.14)	(55.39)
Wealth		0.260		0.259
		(52.52)		(50.97)
Unemployment Experience	-0.135	0.397	-0.496	-1.027
	(-3.18)	(3.20)	(-8.37)	(-8.39)

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 experienced-based learning (EBL) consumers in columns (3) and (4). Estimations are for with $\lambda = 1$ in the top panel, $\lambda = 3$ in the middle panel, and $\lambda = 0$ 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.

The results are qualitatively and quantitatively similar when we put higher weights on consumers' recent experiences ($\lambda = 3$), as shown in the middle panel of Table 6.

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.

To recover the intuition of income scarring and generate a negative relationship between the experience measure and consumption under rational learning, we can employ an experience measure that is more backward looking. In our model, this amounts to lowering λ. In the bottom panel of Table 6, we repeat the estimation setting $\lambda = 0$, so all prior experiences get equal weights. In this case, the specification with rational learners and without wealth control (in column 1) shows a negative correlation between unemployment experience and consumption. That is, for $\lambda = 0$, we see "income scarring" and its possible confound at work: If two people have the same income today, but one person got fired more in the past, then that person likely has earned less in the past, thus has lower assets today, and consumes less. However, once we control for asset accumulation (in column 2), we re-estimate a positive coefficient on unemployment experiences, with all coefficients being similar to the ones estimated for $\lambda = 1$ (column 2 of the upper panel). At the same time, the estimated experience effect is robustly negative for experience-based learners, both with and without wealth control, as shown in columns (3)-(4) of the lower panel. In other words, under a more backward-looking proxy for experience effects the potential wealth confound materializes: If we do not control for wealth, the experience-effect proxy might pick up those effects even though agents are not experience-based learners. If agents are experience-based learners, we expect to robustly identify their experience-induced consumption adjustment.

Table 7: **Estimations with Model-Simulated Data, Unemployment Scarring**

	(1) $\lambda = 1$ Rational	(2) $\lambda = 1$ EBL	(3) $\lambda = 3$ Rational	(4) $\lambda = 3$ EBL
Income	0.282	0.350	0.284	0.371
	(141.89)	(65.34)	(123.91)	(59.28)
Wealth	0.314	0.260	0.311	0.262
	(54.31)	(32.30)	(55.01)	(35.44)
Unemployment Experience	0.176	-1.757	0.138	-1.493
	(2.88)	(-25.31)	(3.06)	(-31.06)

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. The experience proxy is calculated with $\lambda = 1$ in columns (1) and (2) and $\lambda = 3$ in columns (3) and (4). All estimations include period and education fixed effects and use period-clustered standard errors. Simulations are based on the working periods of 10,000 simulated consumers and thus 1,600,000 observations. t statistics in parentheses.

Unemployment scarring. As a last step, we consider an even higher hurdle to the identification of experience effects, and introduce additional negative correlation between unemployment and future income as an alternative explanation. Our motivation for introducing such "unemployment scarring" comes from research in labor economics that has found a persistent negative effect of being unemployed on future income, especially during a recession (Davis and Von Wachter 2011, Huckfeldt 2016, Jarosch 2015). While those findings might actually be evidence for experience effects, the existing literature proposes more traditional explanations. The model of "unemployment scars" in Jarosch (2015), for example, features a job-security component that resembles the "job-match component" of wages in Low, Meghir, and Pistaferri (2010), albeit with the difference is that wage gains lost due to "income scarring" can be regained by working for an extended period.[22]

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

To add "unemployment scarring" to our simulation, we reduce a consumer's permanent wage component by the average size of a permanent income shock, σ_ζ, every time she experiences job destruction. We re-simulate the model with this additional, permanent effect of job loss on income, and then re-estimate the specifications of Tables 6, i. e., analyze again which effects the experience proxy might pick up in estimations using simulated data with rational agents, and whether it identifies experience effects in estimations using simulated data with experience-based learners.

Table 7 shows the results for rational and for EBL agents, both for $\lambda = 1$ and for $\lambda = 3$, in the specifications controlling for wealth. We find that the signs and significant levels of all coefficients remain the same as in the simulation without unemployment scarring. For the simulations with rational agents, the coefficient on the experience-effect proxy remains significantly positive both for $\lambda = 1$ and for $\lambda = 3$,[23] though 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, these results provide evidence that our predictions are robust even when future income and unemployment are strongly negatively correlated due to both "income scarring" and "unemployment scarring." For empirically validated parameterizations of experience effects (with linearly declining or steeper weighting functions), financial constraints, unobserved wealth factors, income scarring, and unemployment scarring fail to generate a negative relation between our proxy for past unemployment experiences and consumption when agents are Bayesian learners. Instead, a negative coefficient estimate likely indicates experience-based learning. The same holds when varying the unemployment-experience proxy further to overweight past experiences even more (e. g., equal weighting all life-time experiences) as long as we control for wealth effects. However, if we fail to appropriately control for wealth effects under those latter flat weighting functions, the confound might materialize. We do not im-

[23] As before, we estimate a positive coefficient even when not controlling for wealth, unless we alter the experience proxy to further overweight experiences far in the past, i. e., for very low λ's.

plement the latter specifications empirically; nevertheless, the simulate-and-estimate exercise with those parameters indicates that it is important to 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.[24]

V Robustness using PSID, CEX, and Nielsen

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. We then turn to the CEX and the Nielsen data, both for replication and, in Section VI, to examine additional predictions of the experience-effect model.

V.A PSID: 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,

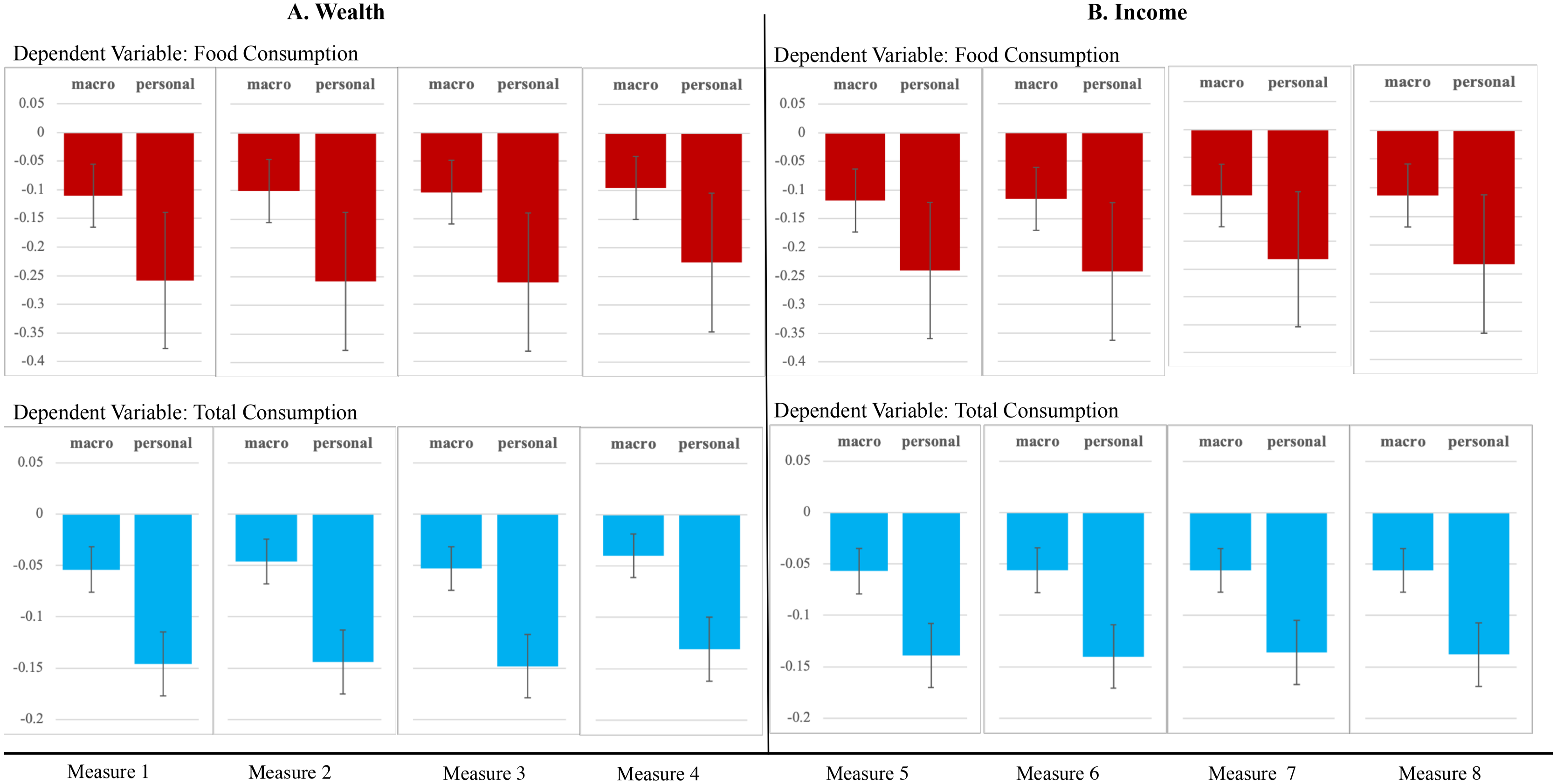
[24] One prediction we did not pursue regards the hours worked. In general, EBL implies a positive relation between past unemployment experience and the likelihood of working because work generates greater income buffer. (Note that "work" is a binary decision in the model.) However, this prediction does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

and (4) log total debt and log positive wealth separately. We summarize all estimated coefficients of the macro and personal experience measures in the left panel of Figure 5. (The detailed results are in Appendix-Table A.9.) All coefficients of interest remain very similar, both in size and in statistical significance.

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

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.9, 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, 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), 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.11, households in the bottom half of the liquid-wealth group tend to spend less relative to households in the top half on average. However, their consumption expenditure does not exhibit a significantly stronger reaction to unemployment experience. All 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.

Figure 5: **Wealth and Income Controls: Coefficients and Confidence Intervals for Experience Measures**



Notes. The red (dark) bars in the upper left quadrant present the estimates when we use food consumption as the dependent variable and 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 blue (light) bars in the lower left quadrant show the estimates when we use total consumption as the dependent variable and the same four sets of wealth controls. The red (dark) bars in the upper right quadrant present the estimates when we use food consumption as the dependent variable and 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. The blue (light) bars in the lower right quadrant show the estimates when we use total consumption as the dependent variable and the four income controls. All regressions include household fixed effects. Confidence intervals are at the 90% confidence level.

V.B CEX

Next, we turn to a second 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.[25]

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. As before, 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.

The top panel of Table 8 provides the summary statistics. The average income, \$47k, is in line with the average income at the national level. The sample period runs from 1980-2012. Note that durable and non-durable consumption do not add up to total consumption because of expenditures that are not considered durable or non-durable, such as healthcare and education expenses. The average non-durable and durable spending amount to 67.9% and 20.0% 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 9 shows the results for total, durable, and non-durable consumption.

[25] Estimations involving durable consumption can be affected by the timing of durable purchases. Prior research such as Bar-Ilan and Blinder (1992) and Berger and Vavra (2015) shows that durable purchases tend to be discontinuous and go down during recessions. However, these concerns do not apply to our estimates of experience effects on food and other non-durable consumption items.

Table 8: **Summary Statistics (CEX and Nielsen)**

Variable	Mean	SD	p10	p50	p90	N
CEX (Quarterly)						
Age of male head of HH	51	17	29	49	75	417,607
Income	47,220	48,925	8,634	33,728	100,000	417,607
Household size	2.7	1.5	1	2	5	417,607
Total expenditure	6,116	6,145	1,902	4,490	11,479	417,607
Non-durable expenditure	4,152	3,189	1,537	3,452	7,364	417,607
Durable expenditure	1,226	4,082	0	170	2,085	417,607
Experience (Macro)	6.1	0.3	5.8	6.0	6.5	417,607
Nielsen (Monthly)						
Age of male head of HH	50	12	33	49	67	3,171,833
Income	\$50-\$60k		\$20-\$25k	\$50-\$60k	\$100k+	3,171,833
Household size	2.8	1.5	1	2	5	3,171,833
Total expenditure	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
Experience (Macro)	6.0	0.2	5.8	5.9	6.3	3,171,833

Notes. The top panel shows quarterly CEX data from 1980-2012. The bottom panel shows monthly Nielsen data from 2004-2013. Nielsen reports income in 13 brackets. 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. Experience (Macro) is households' lifetime experience of national unemployment rates.

The results strongly confirm our prior findings, and reveal new quantitative implications for the different components of total consumption. All experience effect coefficients are negative and highly significant. Households who have experienced worse unemployment conditions during their lifetime spend significantly less in total, durable, and non-durable consumption. The economic magnitudes are large: A one standard-deviation increase in unemployment experience is associated with a decline in annual consumption of \$432 for non-durables and \$564 in total. The estimate on non-durable consumption is larger than in the PSID (\$276 decline in annual food consumption), while the estimate on total consumption is smaller than the PSID estimate (\$912 decline). This may reflect the fact that non-durable in the CEX include leisure, which tends to be elastic, while total expenditures in the CEX encompass healthcare and education, which tend to be more inelastic. The

Table 9: **Experience Effects and Quarterly Consumption (CEX)**

	Total	Durables	Non-durable
Experience (Macro)	-0.077***	-0.085***	-0.086***
	(0.010)	(0.027)	(0.005)
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	417,607	417,607	417,607
R-squared	0.390	0.126	0.409

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.

new estimate for durables indicates that a one standard-deviation increase in past unemployment experience predicts a $120 decline in annual durable consumption.

V.C Nielsen

As a final source of data on consumption choices, we turn to the Nielsen Homescan Dataset. The Nielsen data contains information on product purchases of a panel of more than 100,000 U.S. households from 54 geographically dispersed markets, each roughly corresponding to a Metropolitan Statistical Area (MSA), from 2004-2013. The households provide detailed information about their purchases, including price, quantity, date of purchase, identifier of the store, as well as product characteristics, including brand, size and packaging, at the Universal Product Code (UPC) level. Households record the dollar value of any coupons used and whether the purchase involved a deal from the retailer (sale item). The product categories are food and non-food grocery, health and beauty aids, and general merchandise, summing to approximately 3.2 million unique UPCs covering 125 general product categories.[26]

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

Our data sample consists of 3,171,833 observations of 105,061 households. The bottom panel of Table 8 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 covers mostly food products.

We also conduct a robustness analysis that keeps the advantages of the Nielsen panel structure but exploits the comprehensiveness of the CEX by creating a synthetic Nielsen-CEX panel. Details and estimations are in Appendix-Section A.2.

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, like the CEX, Nielsen provides no information about households' prior residence or employment status (pre-sample period). We thus construct the macro-level experience measure based on 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 in the main text.[28]

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

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

[28] We have re-estimated our model using a measure of personal unemployment experience that

Like the CEX, 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 comparable estimates across the PSID data (with wealth controls) and the CEX, and 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,[29] and merge it with the Nielsen data. 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.[30]

To re-estimate the sensitivity of consumption to experienced unemployment conditions in the Nielsen data, we use an estimation model that again closely mirrors the PSID model from equation (3), but accounts for the additional market-level information:

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

The 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).[31] As before UE_{it} denotes the lifetime (macro) experience of unemployment

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.

[29] 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).

[30] 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.

[31] DMAs are slightly bigger than a county but smaller than an MSA. We control for location at

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), and age dummies, and the time dummies η_t are now year-month-specific. Standard errors are clustered at the cohort level. All regression results are quantitatively and qualitatively similar when clustered by household, household-time, cohort-time, or two-way clustered at the cohort and time level.

Table 10 present results from regression specification (11). Columns (1)-(2) show estimates from pooled OLS regressions, and columns (3)-(4) from regressions with household fixed effects, thus controlling for time-invariant unobserved heterogeneity at the household level. 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: A one standard deviation increase in unemployment experiences is associated with a \$708 decline in annual consumption of non-durables, which amounts to around 8% of average spending for the households in our sample. When we introduce household fixed effects, the estimated experience effects become smaller – as expected given that we are differencing out the cross-sectional differences in consumption between households with "mostly good" versus "mostly bad" lifetime experiences. Now, a one standard deviation increase in unemployment experiences predicts a \$300 decline in annual non-durable consumption, comparable to the PSID estimates.

In Figure 6, we illustrate the economic magnitude of the estimates in the context of unemployment conditions during the Great Recession, which falls in the Nielsen sample period. The average monthly unemployment rate from 2008-2012 was 8.1%, with the maximum during the period being 10%. Comparing these numbers with historical averages, the average unemployment rate during the 60 years prior to 2008, from 1947-2007, was 5.6%. Now consider two individuals, a 25-year-old and a 60-year-old as of December 2007. Their lifetime unemployment experience, based on our experience weighting scheme, was 5.3% and 5.8%, respectively, when they entered the crisis in 2008. By the end of 2012, their lifetime unemployment experience was

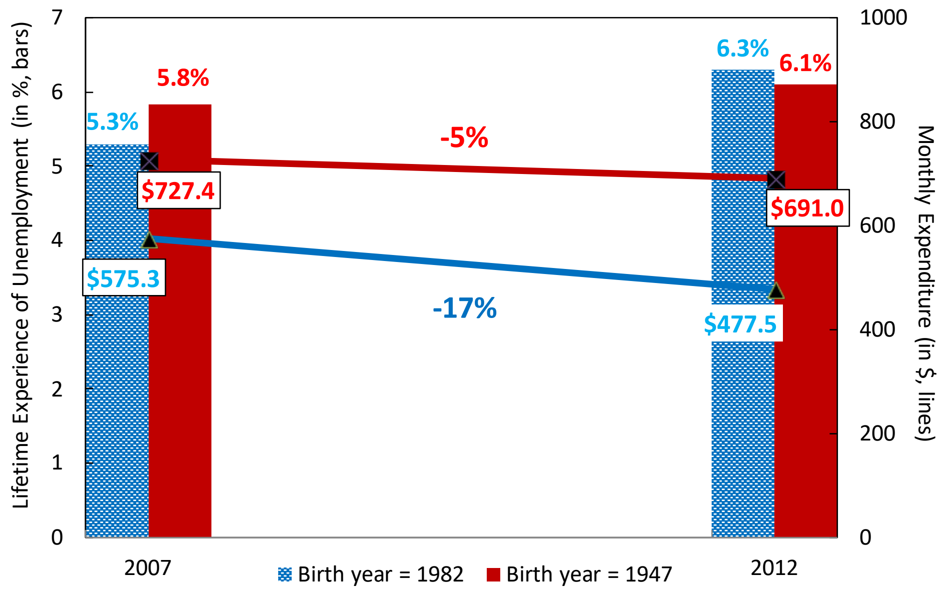
the local market level instead of the county level because people may travel outside of counties to purchase goods. The results are similar if we use county fixed effects instead.

Table 10: **Experience Effects and Monthly Consumption (Nielsen)**

	(1)	(2)	(3)	(4)
Experience (Macro)	-0.415***	-0.415***	-0.178***	-0.177***
	(0.044)	(0.044)	(0.034)	(0.034)
Unemployment rate (county)		-0.002		-0.005***
		(0.003)		(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	No	No	Yes	Yes
Observations	3,171,833	3,171,833	3,171,833	3,171,833
R-squared	0.116	0.116	0.526	0.526

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

Figure 6: **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.

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 0.3pp. Relating these experiences to consumption behavior, our model estimates imply that the monthly consumption expenditure of the 25-year-old decreased by approximately 18% while that of the 60-year-old decreased by approximately 5%.

VI Further Implications and Discussion

Building on the robust results on the relation between past unemployment experiences and consumption in the PSID, CEX, and Nielsen data, we study two further implications of experience effects and discuss alternative channels, besides belief.

VI.A 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, which also captures the qualitative margins. We construct three 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. The summary statistics are in Table 8.

The estimation model is exactly as delineated in equation (11), only with switched outcome variables. Table 11 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 coupon 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.

VI.B 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), assigns 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

Table 11: **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)	(0.003)	(0.003)
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
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. Other controls are as in Table 10. The sample period runs from 2004 to 2013. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

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 10. We do so separately for positive and negative changes (in absolute value) 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.[32]

The results are in Table 12. 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.

[32] 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 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 12: **Age-Heterogeneity in Reaction to Unemployment Fluctuation**

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

Notes. OLS 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.

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 outcomes according to the PIH.[33] Thus, our findings highlight experience effects as a distinct force in affecting people's consumption behavior.

VI.C Preference Channel

In Section III.B, we showed that individuals' past experiences significantly influence beliefs about their future financial situation. This evidence helped to distinguish experience-based learning from alternative explanations of consumers' response to past experiences. At the same time, lifetime experiences might influence not only consumers' beliefs but also their preferences. In other words, the evidence on experience-based learning (beliefs channel) does not rule out that experience-based taste changes (preference channel) are also at work.

There are many possible specifications of the preference-based interpretation, and it is thus 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: We study whether our findings on the significant relationship between consumption and lifetime experience

[33] We estimated a set of regressions that augments the specifications from Table 12 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.

may be correlated with habit persistence in consumption. To that end, we estimate an alternative version of the empirical model in equation (11) 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.[34] 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.

In Table 13, we present the results. The estimates show that the effects of prior unemployment experience on consumption remain highly 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.

VII Aggregate Implications and Conclusion

While it has been a decade since the start of the Great Recession, effects of the crisis still linger, and a better understanding of the long-term effects of economic shocks has proven to be of utmost importance for both academics and policy-makers. 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 consumption decisions and thereby the 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

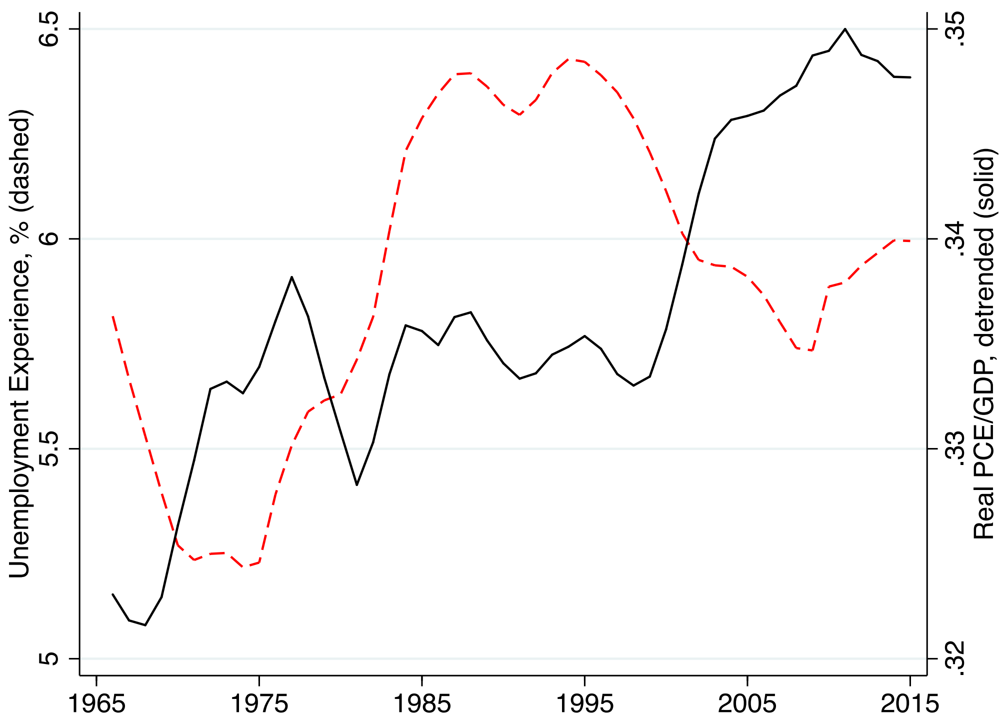
[34] Note that we test for first- and second-order autocorrelation in the first-differenced errors and find that they are first-order serially correlated, but not second-order serially correlated. This supports the validity of the moment conditions used by the system GMM estimators.

Table 13: **Experience Effects and Consumption, GMM regressions**

	PSID	Nielsen	CEX
Experience (Macro)	-0.181***	-0.266***	-0.045***
	(0.063)	(0.051)	(0.006)
Experience (Personal)	-0.635**	—	—
	(0.120)		
Income control	Yes	Yes	Yes
Wealth control	Yes	Yes	No
Household characteristics	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Location fixed effects	Yes	Yes	Yes
Observations	29,813	3,016,952	235,834
R-squared	0.45	0.41	0.64

Notes. System GMM regressions with food consumption (in logarithm) as the dependent variable. "Experience (Macro)" is the macroeconomic experience measure, "Experience (Personal)" is the personal experience measure, specified as described above for the respective datasets. Time fixed effects include year fixed effects for the PSID sample, year and month fixed effects for the Nielsen sample, and year and quarter fixed effects for the CEX sample. Location fixed effects include state fixed effects for the PSID sample, market area fixed effects for the Nielsen sample, and region fixed effects for the CEX sample. The sample period runs from 1999-2013 for the PSID, 2004 to 2013 for the Nielsen sample, and 1980 to 2012 for the CEX sample. Robust standard errors in parentheses are clustered on cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Figure 7: **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.

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.

In light of our results on the lasting effects of past experiences on consumption, experience effects could potentially 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 now 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 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 7, 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 twelve tables use the PSID data. Appendix-Table A.1 presents the summary statistics of the full sample, i. e., including observations with total family income below the 5^{th} or above the 95^{th} percentile in each wave. 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 37,156 observations, compared to 33,164 in the main sample. The sample statistics are very similar, with a mean macroeconomic experience measure of 6.0%, mean personal experience of 5.4%, average food consumption of \$8,559, and average total consumption of \$46,256 (both measured 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 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, with some of the personal experience effect estimates increasing in (absolute) magnitude.

Appendix-Table A.5 presents yet another alternative experience measure, which excludes unemployment experiences from year $t-1$ to further rule out concurrent factors. All other variables are defined as in Table 2. The coefficients of interest remain stable without households fixed effects. When including households fixed effects, the estimates are slightly smaller in magnitude but remain significant.

In Appendix-Table A.6, we use weighting parameter $\lambda = 3$ instead of $\lambda = 1$ to construct experience measures, and re-estimate the fixed-effect models of Table 2. Higher λ means individuals put more emphasis on their more recent experiences. As shown in Table A.6, the results remain similar. Hence, the significant relation between experience and consumption is robust to the variation in weighting parameter.

Appendix-Table A.7 shows the results for different clustering units. Instead of clustering by cohort as in Table 2, we cluster the standard errors by cohort∗year, household, household∗year, and we two-way cluster by cohort and year. The pooled regressions in Appendix-Table A.7 correspond to the specification in column (3) of Table 2, and the specifications with household fixed-effects correspond to column (6) in Table 2. The statistical significance of our results are not affected in most cases. Once we included household fixed effects, both experience variables are significant.

In Appendix-Table A.8, 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. As before the results remain very similar in the specifications with household fixed effects.

Appendix-Tables A.9, A.10, and A.11 address concerns about unobserved wealth, liquidity, or income components. Appendix-Table A.9 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 (column 1); decile dummies of liquid and illiquid wealth (column 2); housing wealth and other wealth (column 3); positive wealth and debt (column 4). Columns (5)-(8) mirror columns (1)-(4) respectively but include household fixed effects. The coefficients of interest remain stable and (at least marginally) statistically significant.

Appendix-Table A.10 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 (column 1); quintile dummies of income and lagged income (column 2); decile dummies of income and lagged income (column 3); 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 (column 4). Columns (5)-(8) have the same income controls as columns (1)-(4) respectively but include household fixed effects. The coefficients of interest remain stable. All of the estimates that were significantly negative before are still significant.

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

In Appendix-Table A.13, 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 variables are either liquid wealth or total wealth, and the main regressors are lagged experience measures. We lag the experience measures by six, eight, ten, twelve, and 14 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 income in years $t-1$ and $t-2$, and add 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, twelve, and 14 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 boom or crisis may have implications for future income (Oyer (2008); Kahn (2010); Oreopoulos, von Wachter, and Heisz (2012)).[35] In Appendix-Figure A.1, we plot the estimated coefficients on the lagged experience measures. In Appendix-Table A.13, we show the estimates of the coefficients on the 10-year, 12-year, and 14-year lagged experience measures. We find a significant role of past experiences for the build-up of wealth and liquid wealth, especially in the context of personal experiences.

[35] 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.

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

Variable	Mean	SD	p10	p50	p90	N
Age	47.65	12.03	32	47	65	37,156
Experience (Macro) [in %]	6.00	0.28	5.67	5.97	6.37	37,156
Experience (Personal) [in %]	5.77	16.57	0.00	0.00	20.00	37,156
Household Size	2.73	1.45	1	2	5	37,156
Household Food Consumption [in $]	8,559	5,630	2,600	7,608	15,451	37,156
Household Total Consumption [in $]	46,256	36,497	14,733	39,559	82,765	37,156
Household Total Income [in $]	93k	133k	17k	69k	178k	37,156
Household Liquid Wealth [in $]	65k	718k	-22k	0k	117k	37,156
Household Illiquid Wealth [in $]	282k	1,268k	0k	72k	606k	37,156
Household Total Wealth [in $]	346k	1,545k	-3k	73k	762k	37,156

Notes. Summary statistics for the estimation sample, which covers the 1999-2013 PSID waves. 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: **Consumption (PSID), Full Sample**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	-0.181***		-0.165***	-0.171**		-0.163**
	(0.051)		(0.050)	(0.069)		(0.069)
Experience (Personal)		-0.756***	-0.752***		-0.426***	-0.422***
		(0.114)	(0.114)		(0.137)	(0.137)
R-squared	0.199	0.204	0.204	0.542	0.543	0.543
Dependent Variable: Total Consumption						
Experience (Macro)	-0.059*		-0.046	-0.079**		-0.073**
	(0.031)		(0.028)	(0.031)		(0.031)
Experience (Personal)		-0.603***	-0.602***		-0.328***	-0.326***
		(0.073)	(0.073)		(0.082)	(0.082)
R-squared	0.496	0.507	0.507	0.755	0.757	0.757
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	No	No	No	Yes	Yes	Yes
Observations	37,156	37,156	37,156	37,156	37,156	37,156

Notes. We include all observations i.e., also observations with total family income below the 5^{th} or above the 95^{th} percentile in each wave from 1999 to 2013, as well as the pre-sample 1997 wave (because we control for lagged income). All 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.3: **Consumption (PSID), Alternative Experience Measure: Gap Years**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	-0.099**		-0.093*	-0.124**		-0.120**
	(0.047)		(0.048)	(0.055)		(0.055)
Experience (Personal)		-0.337***	-0.335***		-0.267**	-0.264**
		(0.104)	(0.104)		(0.127)	(0.128)
R-squared	0.192	0.193	0.193	0.541	0.542	0.542
Dependent Variable: Total Consumption						
Experience (Macro)	-0.022		-0.018	-0.061***		-0.059***
	(0.020)		(0.019)	(0.022)		(0.022)
Experience (Personal)		-0.182***	-0.181***		-0.152***	-0.151***
		(0.031)	(0.031)		(0.033)	(0.033)
R-squared	0.573	0.574	0.574	0.788	0.788	0.788
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	No	No	No	Yes	Yes	Yes
Observations	33,164	33,164	33,164	33,164	33,164	33,164

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. We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	-0.079*		-0.071	-0.111**		-0.106*
	(0.047)		(0.047)	(0.054)		(0.055)
Experience (Personal)		-0.402***	-0.400***		-0.313**	-0.309**
		(0.111)	(0.111)		(0.130)	(0.130)
R-squared	0.192	0.193	0.193	0.541	0.542	0.542
Dependent Variable: Total Consumption						
Experience (Macro)	-0.021		-0.017	-0.059***		-0.056***
	(0.019)		(0.019)	(0.021)		(0.021)
Experience (Personal)		-0.213***	-0.212***		-0.161***	-0.159***
		(0.035)	(0.034)		(0.033)	(0.033)
R-squared	0.573	0.574	0.574	0.788	0.788	0.788
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
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
Observations	33,164	33,164	33,164	33,164	33,164	33,164

Notes. All variables other than the couple indicator, and experience measures are defined as in Table 2. Couple is an indicator equal to 1 for households who are married, and is now included as a demographic controle. The experience measures for the married households are constructed using an average of the household's head and the spouse. We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

Table A.5: **Consumption (PSID), Alternative Experience Measure: Lagged Experience**

Dependent Varariable:	Food Consumption			Total Consumption		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	-0.109*		-0.103*	-0.094**		-0.093**
	(0.056)		(0.057)	(0.046)		(0.046)
Experience (Personal)		-0.320**	-0.318**		-0.066	-0.064
		(0.134)	(0.134)		(0.136)	(0.136)
R-squared	0.204	0.205	0.205	0.587	0.587	0.587
Dependent Variable: Total Consumption						
Experience (Macro)	-0.013		-0.010	-0.047*		-0.046*
	(0.024)		(0.024)	(0.026)		(0.025)
Experience (Personal)		-0.179***	-0.178***		-0.120***	-0.119***
		(0.038)	(0.038)		(0.038)	(0.038)
R-squared	0.572	0.573	0.573	0.806	0.807	0.807
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
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
Observations	20,163	20,163	20,163	20,163	20,163	20,163

Notes. The experience measures (both macro and personal) does not contain unemployment experience from year $t-1$. All other variables are defined as in Table 2. We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

Table A.6: **Consumption (PSID), Alternative Experience Measure: Different Weights ($\lambda = 3$)**

Dependent Varariable:	Food Consumption			Total Consumption		
	(1)	(2)	(3)	(4)	(5)	(6)
Experience (Macro)	-0.021**		-0.019*	-0.033***		-0.032**
	(0.010)		(0.011)	(0.012)		(0.012)
Experience (Personal)		-0.175***	-0.174***		-0.150***	-0.149***
		(0.029)	(0.029)		(0.031)	(0.031)
R-squared	0.573	0.574	0.574	0.788	0.788	0.788
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
Observations	33,164	33,164	33,164	33,164	33,164	33,164

Notes. All variables other than the experience measures are defined as in Table 2. The experience measures are constructed using $\lambda = 0$ in the upper part of the table, and $\lambda = 3$ in the lower part. We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

Table A.7: **Consumption (PSID), Alternative Clustering Units**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Food Consumption								
Experience (Macro)	-0.091**	-0.091**	-0.091**	-0.091**	-0.117**	-0.117	-0.117**	-0.117**
	(0.041)	(0.033)	(0.046)	(0.042)	(0.053)	(0.062)	(0.050)	(0.049)
Experience (Personal)	-0.320***	-0.320**	-0.320***	-0.320***	-0.260**	-0.260*	-0.260***	-0.260**
	(0.086)	(0.113)	(0.095)	(0.085)	(0.109)	(0.132)	(0.099)	(0.101)
R-squared	0.193	0.193	0.193	0.193	0.542	0.542	0.542	0.542
Dependent Variable: Total Consumption								
Experience (Macro)	-0.018	-0.018	-0.018	-0.018	-0.057***	-0.057**	-0.057***	-0.057***
	(0.015)	(0.017)	(0.017)	(0.015)	(0.017)	(0.020)	(0.019)	(0.017)
Experience (Personal)	-0.177***	-0.177***	-0.177***	-0.177***	-0.147***	-0.147**	-0.147***	-0.147***
	(0.024)	(0.046)	(0.026)	(0.023)	(0.028)	(0.043)	(0.030)	(0.027)
R-squared	0.574	0.574	0.574	0.574	0.788	0.788	0.788	0.788
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	No	No	No	No	Yes	Yes	Yes	Yes
Observations	33,164	33,164	33,164	33,164	33,164	33,164	33,164	33,164

Notes. All variables are defined as in Table 2. Standard errors in columns (1) to (4) are clustered by cohort∗year, cohort and year (two-way clustering), household and household∗year, respectively, and the same for columns (5) to (8). We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

Table A.8: **Consumption (PSID), Alternative Weights: PSID Weights**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	0.016		-0.093*	-0.119**		-0.115**
	(0.056)		(0.047)	(0.054)		(0.054)
Experience (Personal)		-0.302***	-0.324***		-0.262**	-0.260**
		(0.112)	(0.098)		(0.120)	(0.120)
R-squared	0.225	0.226	0.193	0.541	0.541	0.541
Dependent Variable: Total Consumption						
Experience (Macro)	-0.003		-0.021	-0.058***		-0.056**
	(0.023)		(0.019)	(0.021)		(0.022)
Experience (Personal)		-0.162***	-0.176***		-0.150***	-0.149***
		(0.042)	(0.030)		(0.032)	(0.032)
R-squared	0.576	0.576	0.574	0.787	0.788	0.788
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	No	No	No	Yes	Yes	Yes
Observations	32,834	32,834	32,834	32,834	32,834	32,834

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. Robust standard errors (in parentheses) are clustered by cohort. We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Food Consumption								
Experience (Macro)	-0.088*	-0.079	-0.082*	-0.082	-0.110**	-0.101*	-0.104*	-0.096*
	(0.047)	(0.047)	(0.048)	(0.051)	(0.054)	(0.054)	(0.054)	(0.055)
Experience (Personal)	-0.318***	-0.272***	-0.321***	-0.231**	-0.258**	-0.258**	-0.261**	-0.226*
	(0.097)	(0.099)	(0.098)	(0.098)	(0.119)	(0.120)	(0.119)	(0.120)
R-squared	0.194	0.199	0.194	0.203	0.542	0.543	0.542	0.551
Dependent Variable: Total Consumption								
Experience (Macro)	-0.015	-0.005	-0.013	-0.003	-0.054**	-0.046**	-0.053**	-0.040*
	(0.019)	(0.019)	(0.019)	(0.020)	(0.021)	(0.021)	(0.022)	(0.020)
Experience (Personal)	-0.175***	-0.131***	-0.178***	-0.069***	-0.146***	-0.144***	-0.148***	-0.130***
	(0.029)	(0.026)	(0.029)	(0.024)	(0.031)	(0.030)	(0.031)	(0.030)
R-squared	0.577	0.596	0.575	0.633	0.788	0.791	0.788	0.805
Demographic 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	No	No	No	No	Yes	Yes	Yes	Yes
Observations	33,164	33,164	33,164	31,187	33,164	33,164	33,164	31,187

Notes. Regressions differ from those in Table 2 only in terms of the wealth controls. Column (1) controls for third- and fourth-order liquid and illiquid wealth. Column (2) includes decile dummies of liquid wealth and illiquid wealth. Column (3) controls for housing wealth and other wealth (total wealth minus housing wealth). Column (4) controls for positive wealth and debt. All wealth controls are in addition to the controls of first and second order of liquid and illiquid wealth. Columns (5) – (8) have the same wealth controls as columns (1) – (4) respectively. Robust standard errors are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

Table A.10: **Consumption (PSID), Additional Income Controls**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Food Consumption								
Experience (Macro)	-0.097**	-0.095**	-0.098**	-0.089*	-0.118**	-0.116**	-0.118**	-0.113**
	(0.047)	(0.047)	(0.048)	(0.048)	(0.055)	(0.055)	(0.056)	(0.055)
Experience (Personal)	-0.267***	-0.274***	-0.238**	-0.227**	-0.240**	-0.243**	-0.232*	-0.233*
	(0.099)	(0.098)	(0.101)	(0.105)	(0.119)	(0.120)	(0.121)	(0.121)
R-squared	0.200	0.200	0.204	0.206	0.543	0.543	0.544	0.544
Dependent Variable: Total Consumption								
Experience (Macro)	-0.020	-0.018	-0.017	-0.017	-0.057**	-0.056**	-0.056**	-0.056**
	(0.019)	(0.019)	(0.019)	(0.019)	(0.022)	(0.022)	(0.021)	(0.021)
Experience (Personal)	-0.153***	-0.154***	-0.142***	-0.141***	-0.139***	-0.140***	-0.136***	-0.138***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.031)	(0.031)	(0.031)	(0.031)
R-squared	0.579	0.579	0.581	0.581	0.789	0.788	0.789	0.789
Demographic 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	No	No	No	No	Yes	Yes	Yes	Yes
Observations	33,164	33,164	33,164	33,164	33,164	33,164	33,164	33,164

Notes. Regressions differ from those in Table 2 only in terms of the income controls. Column (1) controls for third and fourth order of income and lagged income. Column (2) includes quintile dummies of income and lagged income. Column (3) includes decile dummies of income and lagged income. Column (4) includes 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. Columns (5) – (8) have the same income controls as columns (1) – (4) respectively. Robust standard errors are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: Food Consumption						
Experience (Macro)	-0.147**		-0.144**	-0.143**		-0.143**
	(0.058)		(0.058)	(0.063)		(0.063)
Experience (Macro) * LLW	0.097		0.103*	0.048		0.055
	(0.060)		(0.059)	(0.056)		(0.055)
Low Liquid Wealth	-0.572	0.013	-0.602*	-0.316	-0.023	-0.352
	(0.358)	(0.013)	(0.355)	(0.335)	(0.014)	(0.331)
Experience (Personal)		-0.302**	-0.292**		-0.241	-0.236
		(0.142)	(0.142)		(0.149)	(0.149)
Experience (Personal) * LLW		-0.037	-0.053		-0.038	-0.046
		(0.177)	(0.176)		(0.156)	(0.155)
R-squared	0.192	0.193	0.193	0.542	0.542	0.542
Dependent Variable: Total Consumption						
Experience (Macro)	-0.021		-0.023	-0.054**		-0.055**
	(0.022)		(0.022)	(0.024)		(0.023)
Experience (Macro) * LLW	0.001		0.009	-0.012		-0.006
	(0.015)		(0.016)	(0.015)		(0.016)
Low Liquid Wealth	0.034	0.047***	-0.006	0.080	0.013***	0.046
	(0.093)	(0.006)	(0.094)	(0.093)	(0.004)	(0.095)
Experience (Personal)		-0.087**	-0.086**		-0.083**	-0.082**
		(0.036)	(0.036)		(0.033)	(0.033)
Experience (Personal) * LLW		-0.166***	-0.168***		-0.118***	-0.118***
		(0.046)	(0.046)		(0.044)	(0.044)
R-squared	0.573	0.575	0.575	0.788	0.788	0.788
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	No	No	No	Yes	Yes	Yes
Observations	33,164	33,164	33,164	33,164	33,164	33,164

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. We exclude from the sample observations with total family income below the 5^{th} or above the 95^{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.

Table A.12: **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)	0.084*	0.025	0.039	0.140	0.070	0.161
	(0.049)	(0.074)	(0.051)	(0.095)	(0.068)	(0.104)
Experience (Personal)	0.112	-0.031	0.033	-0.175	0.138	-0.062
	(0.164)	(0.159)	(0.134)	(0.157)	(0.110)	(0.187)
Observations	15,665	28,410	15,665	21,082	11,196	15,713
R-squared	0.410	0.364	0.389	0.379	0.437	0.248
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes
Wealth controls	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,201	24,060	12,201	17,072	8,416	12,573
R-squared	0.396	0.334	0.395	0.452	0.419	0.443

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. 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 5^{th} or above the 95^{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.

Table A.13: **Wealth Accumulation**

Dependent Var.:	Liquid Wealth at time t						Total Wealth at time t					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exp. (Macro)$_{t-10}$	0.006*		0.005	0.003		0.003	0.012		0.010	0.018***		0.019***
	(0.003)		(0.003)	(0.003)		(0.003)	(0.008)		(0.008)	(0.006)		(0.006)
Exp. (Personal)$_{t-10}$		0.023***	0.023***		-0.000	-0.001		0.083***	0.083***	-0.003	-0.005	
		(0.004)	(0.004)		(0.002)	(0.002)		(0.013)	(0.013)		(0.014)	(0.014)
R-squared	0.048	0.048	0.048	0.332	0.332	0.332	0.292	0.294	0.294	0.714	0.714	0.714
Observations	21,691	21,691	21,691	21,691	21,691	21,691	21,691	21,691	21,691	21,691	21,691	21,691
Exp. (Macro)$_{t-12}$	0.007**		0.006*	0.008**		0.007**	0.010		0.007	0.020***		0.020***
	(0.003)		(0.003)	(0.003)		(0.003)	(0.009)		(0.009)	(0.007)		(0.007)
Exp. (Personal)$_{t-12}$		0.026***	0.026***		0.002	0.001		0.092***	0.091***		0.003	0.001
		(0.005)	(0.005)		(0.002)	(0.003)		(0.014)	(0.014)		(0.014)	(0.014)
R-squared	0.049	0.050	0.050	0.333	0.333	0.333	0.294	0.296	0.296	0.730	0.730	0.730
Observations	19,427	19,427	19,427	19,427	19,427	19,427	19,427	19,427	19,427	19,427	19,427	19,427
Exp. (Macro)$_{t-14}$	0.008**		0.007*	0.008**		0.008**	0.002		-0.001	0.011*		0.010
	(0.003)		(0.003)	(0.003)		(0.003)	(0.009)		(0.010)	(0.006)		(0.006)
Exp. (Personal)$_{t-14}$		0.028***	0.028***		0.003	0.002		0.095***	0.095***		0.010	0.009
		(0.005)	(0.005)		(0.003)	(0.003)		(0.013)	(0.013)		(0.009)	(0.009)
R-squared	0.052	0.052	0.052	0.331	0.331	0.331	0.378	0.380	0.380	0.827	0.827	0.827
Observations	17,151	17,151	17,151	17,151	17,151	17,151	17,151	17,151	17,151	17,151	17,151	17,151
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Income controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes

Notes. "Exp. (Macro)" is the macroeconomic experience measure, and "Exp. (Personal)" is the personal experience measure. Liquid wealth and total wealth are defined as in the main draft. We separately use the $t-10$, $t-12$ experience measures, and $t-14$ experience measures. Income controls include the $t-1$ family total income and the average family total income between $t-2$ and the year we use the experience measures. For gap years between PSID survey waves, we use prior-year income. Demographic controls include family size, the household 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 CEX-Nielsen Synthetic Panel Data

In order to keep the advantages of panel analysis but also exploit the comprehensiveness of the CEX, we match the two datasets and create a synthetic panel. Specifically, we match a CEX household i with a Nielsen household j on a set of common covariates (characteristics) $x_i = (x_{i,1}, x_{i,2}, ..., x_{i,p})$ and $x_j = (x_{j,1}, x_{j,2}, ..., x_{j,p})$, which include age, income, marital status, household size, education, race, region of residency, employment status, as well as their consumption of non-durable items, using the nearest-neighbor matching estimator from Rosenbaum and Rubin (1983) and Abadie and Imbens (2011). The distance between x_i and x_j is the vector norm $\| x_i - x_j \|_S = ((x_i - x_j)' S^{-1} (x_i - x_j))^{1/2}$, where S is a symmetric, positive-definite matrix. We find the set of nearest-neighbor indices for observation i in the CEX as $\Omega_i = (j | t_j = 1 - t_i, \| x_i - x_j \|_S < \| x_i - x_l \|_S, t_l = 1 - t_i, l \neq j)$ in Nielsen. In words, the nearest-neighbor propensity-score matching chooses for each observation in the CEX an observation in Nielsen that has the closest estimated propensity score.

Table A.14 provides summary statistics for the matched sample. In the matched dataset, the distributions on total and durable consumption are comparable to those of the underlying CEX data, which is indicative of successful matching. For an average household, its share of durable consumption makes up 10% of total spending, while non-durable consumption amounts to 69% of total spending.

Table A.14: **Summary Statistics (Nielsen-CEX Matched Data)**

Variable	Mean	SD	p10	p50	p90	N
Total consumption expenditure	4,508	4,919	1,838	3,371	7,111	866,819
Durable consumption	1,078	4,466	0	117	1,460	866,819
Non-durable consumption	2,612	1,178	1,423	2,400	4,025	866,819
Non-durable consumption (Nielsen)	2,139	1,602	618	1,757	4,083	3,171,833
Experience (Macro)	5.9	0.2	5.8	5.9	6.2	866,819

Notes. The sample period runs from 2004 to 2012. Observations are quarterly and not weighted.

Table A.15 shows results from re-estimating specification (11) using the matched CEX-Nielsen sample. In columns (1) and (4), we use total expenditures as the outcome variable, in columns (2) and (5) durable consumption spending, and in columns (3) and (6) non-durables. As before we show the results both without household fixed effects (columns 1 to 3) and with fixed effects (columns 4 to 6).

Table A.15: **Consumption (Nielsen-CEX Matched Sample)**

	Total	Durables	Non-durables	Total	Durables	Non-durables
Experience (Macro)	-0.358***	-0.797***	-0.220**	-0.266***	-0.796***	-0.033
	(0.038)	(0.122)	(0.019)	(0.051)	(0.145)	(0.028)
Income control	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	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	No	No	Yes	Yes	Yes
Observations	866,819	866,819	866,819	866,819	866,819	866,819
R-squared	0.183	0.053	0.257	0.020	0.008	0.069

Notes. 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 (household's lifetime experience of national unemployment rates). Household characteristics include unemployment status, household size, education, and race. Time fixed effects include year-quarter fixed effects. Regressions are weighted by household sampling weights from Nielsen. The sample period runs from 2004 to 2012. Robust standard errors (in parentheses) are clustered by cohort. *, **, *** denote 10%, 5%, and 1% significance, respectively.

For all outcome variables – durable, non-durable, and total consumption – we continue to estimate highly significant negative experience effects. Households that have experienced worse unemployment conditions during their lifetime spend significantly less in total, and also specifically on durable and on non-durable items. One exception is non-durables in the case where we identify only within household; here the coefficient becomes small and insignificant. Otherwise, the coefficients are stable across specifications, and the economic magnitudes are large: a one standard deviation increase in lifetime unemployment experience is associated with a \$38 decline in monthly non-durable consumption and \$108 decline in monthly total consumption (using the estimates of columns 3 and 1 respectively). The new estimate for durable consumption is large and highly significant across specifications. A one standard deviation increase in lifetime unemployment experience is associated with a \$57 decline in monthly durable consumption.

Appendix B Model

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

B.1 Parameters governing the income process and utility maximization

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

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

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

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.[36]

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).

[36] 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**

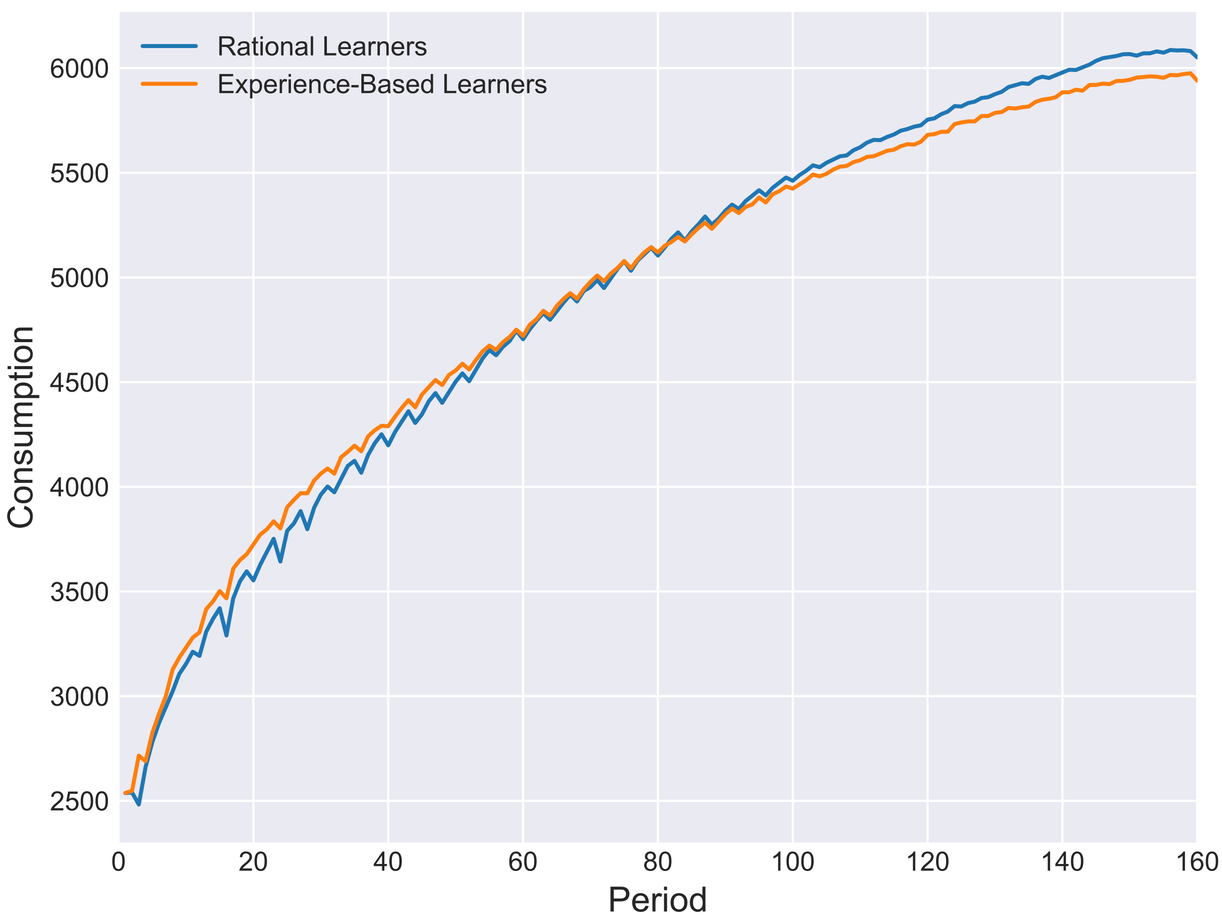
Parameter	Low Education	High Education	Source (from Low, Meghir, and Pistaferri (2010))
γ	1.5	1.5	Text
σ_a	0.226	0.229	Table 1
σ_ζ	0.095	0.106	Table 1
$P(\zeta)$	.25	.25	Text
δ	.049	.028	Table 2
λ^e	.67	.72	Table 2
λ^n	.76	.82	Table 2
b	.75	.75	Text
r (yearly)	.015	.015	Text
β	$1/(1+r)$	$1/(1+r)$	Text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

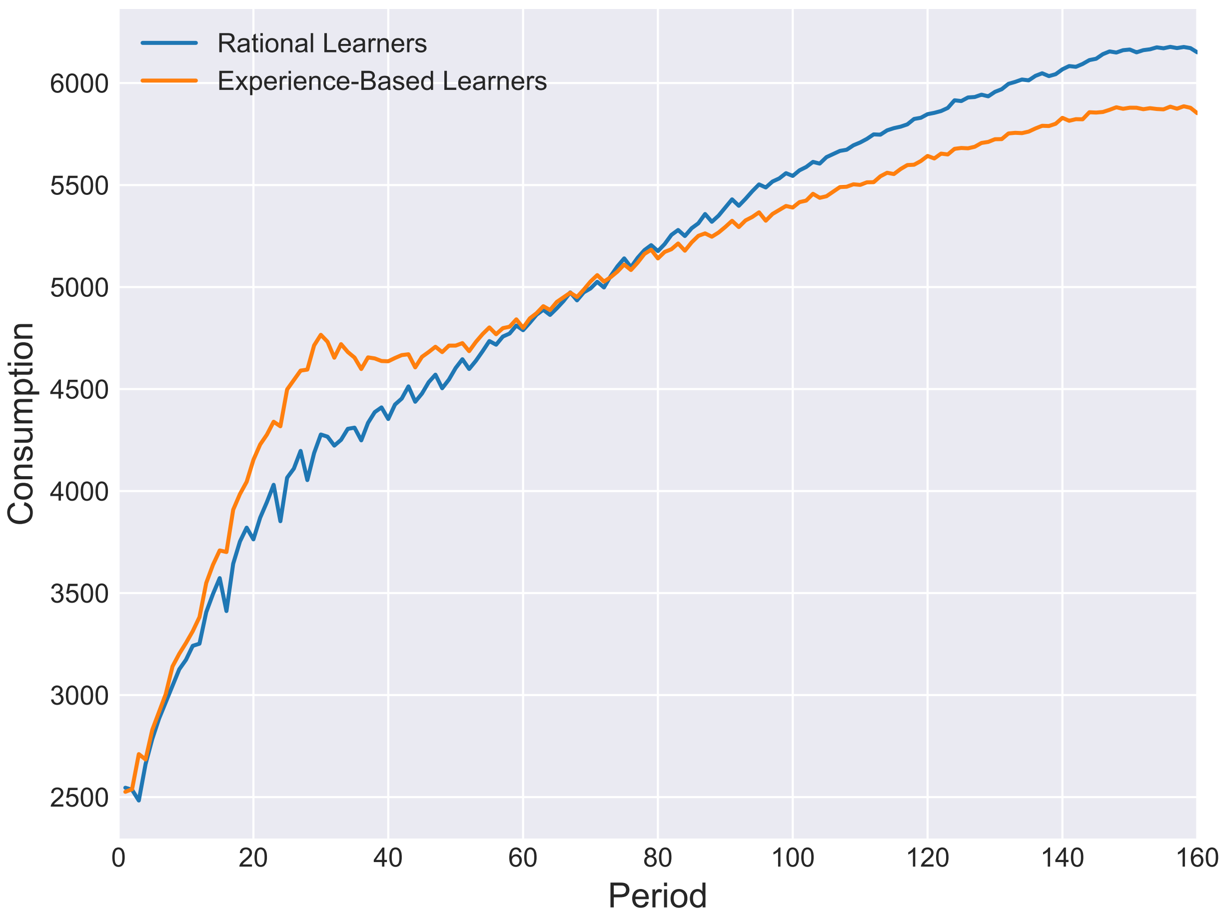
[c] See https://web.archive.org/web/20190228193653/https://fns-prod.azureedge.net/sites/default/files/Trends1999-2005.pdf. Accessed via https://web.archive.org/web/20190228195514/https://www.fns.usda.gov/snap/trends-food-stamp-program-participation-rates-1999-2005.

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



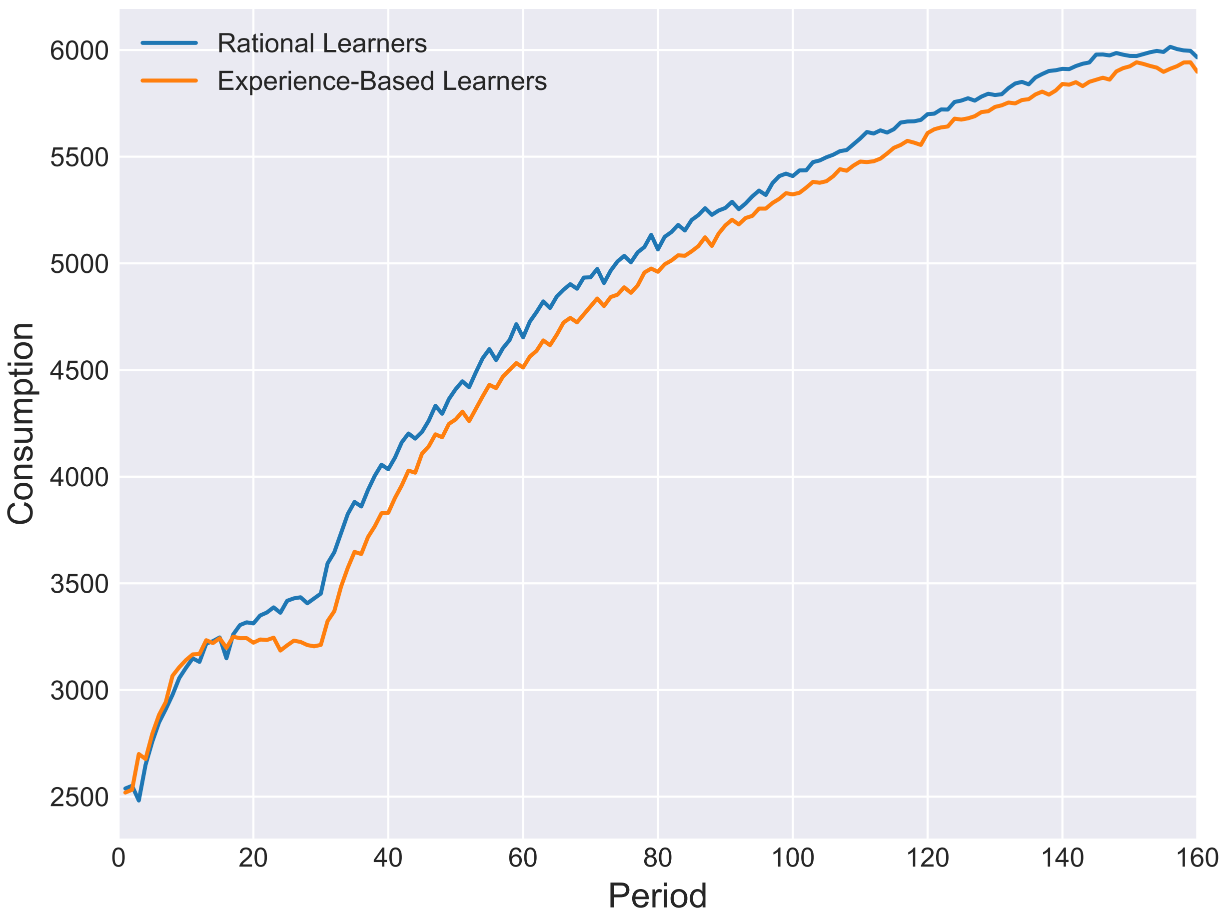
Notes. Average consumption for rational learners and experience-based learners (with $\lambda = 1$) in the low-education group, based on 10,000 lifetime simulations for each type.

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.

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 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 experienced 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 .