Employment Exits and the Evolution of Financial Literacy

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This paper studies how financial literacy evolves around exits from employment among adults in the United States. Using longitudinal data from the Understanding America Study and a matched event-study design, we examine financial literacy and five other cognitive outcomes around the first observed transition out of employment. Financial literacy declines relative to matched controls beginning in the year of the employment exit, with the relative decline reaching 0.11–0.13 standard deviations one to two years later. In contrast, we find little evidence of post-exit changes in numeracy, number series, picture vocabulary, verbal analogies, or serial sevens. Predicted trajectories help explain this relative decline: predicted financial literacy increases modestly among matched controls, while it declines among individuals leaving employment before partially recovering. The decline in financial literacy persists among individuals whose self-reported health remains stable around the exit, reducing concern that the result is driven by contemporaneous health deterioration. These findings point to a domain-specific decline in financially relevant knowledge around employment exits rather than broad cognitive deterioration.

I. Introduction

Financial literacy is an economically important form of human capital, shaping saving, investment, borrowing, mortgage performance, and retirement preparation (Lusardi and Mitchell, 2007; van Rooij, Lusardi and Alessie, 2011; Behrman et al., 2012; Hastings, Madrian and Skimmyhorn, 2013). Unlike many forms of human capital acquired primarily through formal education, financial knowledge can continue to evolve throughout adulthood as individuals encounter new economic decisions and environments. Given both its economic importance and its continued malleability throughout adulthood, understanding what builds, sustains, or erodes this knowledge is important for policies aimed at strengthening financial capability and for understanding how differences in economic experience may contribute to disparities in financial outcomes over the life cycle. This paper studies one such environment: employment.

Employment may shape financial knowledge by supporting the continued use and maintenance of existing knowledge while also providing opportunities for further learning. Work regularly exposes individuals to decisions involving income, benefits, saving, retirement contributions, and other financial choices, which may help maintain financial knowledge already acquired. At the same time, the workplace provides opportunities to acquire new financial knowledge through experience and interactions with coworkers. More broadly, cognitively demanding work has been linked to subsequent cognitive trajectories, suggesting that continued engagement in the workplace may contribute to the maintenance of human capital (Schooler, Mulatu and Oates, 1999; Fisher et al., 2014). Leaving employment changes this environment. Reduced use may contribute to the depreciation of existing knowledge and skills, consistent with the "use it or lose it" hypothesis (Salthouse, 2006; Rohwedder and Willis, 2010), while fewer opportunities for financial decision-making and workplace learning may slow further accumulation. A relative decline in financial literacy after an employment exit can therefore arise because existing knowledge deteriorates, because new knowledge accumulates more slowly, or both.

A growing literature has examined the accumulation and evolution of financial knowledge over adulthood, establishing that financial literacy responds to changes in incentives, experience, education, and information. Models of endogenous financial knowledge treat it as a form of human capital that individuals accumulate in response to expected returns (Lusardi, Michaud and Mitchell, 2017). Empirical evidence likewise shows that financial decision quality reflects both experience and cognitive resources, while substantial knowledge gaps are present among young adults and can widen again at older ages (Agarwal et al., 2009; Lusardi, Mitchell and Curto, 2010; Finke, Howe and Huston, 2017). Financial knowledge also

responds to education and information delivered through schools and workplaces, although effects vary across interventions, populations, and timing (Bernheim and Garrett, 2003; Cole, Paulson and Shastry, 2014; Fernandes, Lynch and Netemeyer, 2014; Brown et al., 2016; Kaiser and Menkhoff, 2017; Kaiser et al., 2022). Together, this evidence characterizes financial knowledge as a form of human capital that can continue to evolve after formal schooling in response to changes in individuals' economic environments, suggesting that employment experience may likewise shape its evolution.

More broadly, the idea that employment experience can shape human capital is well established in the labor literature. Skills accumulated on the job can be task- or occupation-specific, so their value depends in part on the continuity and portability of work experience (Kambourov and Manovskii, 2009; Gathmann and Schönberg, 2010). Time away from work may also erode skills: skill loss during unemployment has long been proposed as a mechanism through which employment shocks can have persistent effects (Pissarides, 1992), and direct evidence links periods of nonemployment to depreciation in general information-processing skills and subsequent wages (Edin and Gustavsson, 2008). Consistent with the broader economic costs of employment disruptions, worker-displacement studies document persistent earnings losses and adverse health consequences following involuntary job separations (Jacobson, LaLonde and Sullivan, 1993; Sullivan and von Wachter, 2009; Couch and Placzek, 2010; Brand, 2015). Although this literature does not speak directly to financial knowledge, it establishes employment history as an important dimension of experience through which human capital can be accumulated, maintained, or depreciated.

A smaller but more directly related literature examines how withdrawal from employment is associated with cognitive performance. Several studies examine cognitive outcomes around retirement and withdrawal from paid work at older ages (Bonsang, Adam and Perelman, 2012; Mazzonna and Peracchi, 2012; Coe et al., 2012; Celidoni, Bianco and Weber, 2017). Freise, Schmitz and Westphal (2022) find little deterioration in word recall, verbal fluency, or numeracy following late-career unemployment, while Kouchekinia, Neumark and Bruckner (2026), using plausibly exogenous variation in local labor demand, find adverse effects on cognitive scores among men aged 51–64. Cohen, Johnston and Lindner (2025), studying unemployment over a broader age range, find little decline in a broad set of cognitive and non-cognitive skills. Overall, the evidence on cognitive responses to employment exits is mixed, and existing research has largely focused on measures of general cognitive performance rather than financial knowledge.

More directly, Atalay and Staneva (2026) examine the effect of retirement on financial literacy using changes in Australian Age Pension eligibility as an instrument for retirement. They find that retirement reduces scores on a five-item financial literacy index ranging from 0 to 5 by approximately one point among women induced to retire by pension eligibility, while the estimated effect for men is not statistically significant. Their additional analyses suggest that reduced responsibility for household financial decision-making may contribute to this decline. Their study provides causal evidence on financial literacy at retirement, but less is known about how financial literacy evolves around employment exits more broadly, particularly those occurring earlier in adulthood, and how these changes unfold over time.

In this paper, we examine how financial literacy changes before and after employment exits relative to individuals who remain employed. We study financial literacy alongside five additional measures—numeracy, number series, picture vocabulary, verbal analogies, and serial sevens. These outcomes capture different forms of knowledge and cognitive performance and span dimensions of fluid and crystallized ability to varying degrees, providing a broader view of cognitive performance around employment exits. Fluid abilities, such as abstract reasoning and quantitative problem solving, tend to peak earlier and subsequently decline, whereas crystallized abilities, such as vocabulary, reflect accumulated knowledge and experience and often remain stable or continue to increase through much of adulthood (Horn and Cattell, 1967; Deary et al., 2009; Hartshorne and Germine, 2015). Some cognitive decline is already detectable in midlife, indicating that age-related changes in cognitive performance begin well before conventional retirement ages (Singh-Manoux et al., 2012). Comparing these outcomes allows us to assess whether changes around employment exits are concentrated in financial literacy or shared more broadly across the cognitive measures we observe. We also examine predicted trajectories separately for individuals leaving

employment and those who remain employed to clarify the movements underlying their relative divergence.

We study these questions using longitudinal data from the Understanding America Study (UAS), a nationally representative internet panel of U.S. adults that allows us to examine employment exits across a broad range of adult ages. We define an employment exit as the first observed transition from employment to nonemployment, including unemployment, retirement, leave, disability, and related mixed labor-force statuses. For each cognitive outcome, we match individuals experiencing an employment exit to individuals with the same age, gender, observation timing, and pre-exit employment history who remain employed over the relevant post-exit window. We then compare their cognitive trajectories around the employment exit using a matched event-study design with individual-by-matched-set and calendar-year fixed effects. We report both conventional event-study estimates and interaction-weighted estimates (Sun and Abraham, 2021) that allow treatment effects to vary across employment-exit cohorts.

We find that financial literacy begins to decline relative to matched controls in the year of the employment exit, with the relative decline reaching approximately 0.11–0.13 standard deviations one to two years after the exit across the conventional and interaction-weighted estimators. For perspective, Kaiser et al. (2022) report an average improvement in financial knowledge of approximately 0.20 standard deviations across randomized financial education interventions, making our estimated relative decline roughly half to two-thirds as large in absolute magnitude.[1] For the five other cognitive outcomes—numeracy, number series, picture vocabulary, verbal analogies, and serial sevens—we find little evidence of systematic post-exit divergence. The pre-exit estimates provide little indication of differential cognitive changes between treated individuals and their matched controls before the employment exit.

Predicted trajectories help clarify the evolution underlying this relative decline. Predicted financial literacy increases modestly among matched controls, while it falls among individuals leaving employment, reaches its lowest level two years after the exit, and subsequently recovers in part. These contrasting trajectories indicate that the relative decline reflects both modest gains among those who remain employed and a decline in predicted financial literacy among those who leave employment. These patterns are consistent with the possibility that continued employment supports further learning and accumulation of financial knowledge, while leaving employment reduces opportunities to use and reinforce knowledge already acquired. However, the predicted trajectories alone do not establish these mechanisms.

The financial literacy response is heterogeneous across individuals, with both its magnitude and timing differing across characteristics. The response in the exit year is more pronounced among respondents without a bachelor's degree and those without prior ownership of durable financial assets, while the relative decline is more persistent among unmarried respondents and appears larger among younger adults. These patterns do not appear to be explained simply by differences in time spent outside employment: groups with larger financial literacy responses often return to employment at least as quickly as their counterparts. The main financial literacy result is also robust to restricting the analysis to individuals whose self-reported health remains stable around the employment exit, reducing concern that the result is driven by contemporaneous health deterioration. Finally, although the timing differs across unemployment, retirement, and other nonemployment exits, similar financial literacy responses appear across different types of transitions.

This paper makes two contributions. First, it extends existing evidence on retirement and financial literacy by examining employment exits across a broader range of adult ages and transition types, including unemployment and other forms of nonemployment. Second, it provides evidence on how financial literacy evolves around employment exits and how these changes compare with those in other cognitive domains. We find that the post-exit relative decline is concentrated in financial literacy rather than shared across the five other cognitive measures. Predicted trajectories further show that this divergence reflects both a decline in predicted financial literacy among individuals leaving employment and modest gains among those who remain employed.

These findings suggest that employment exits may be an important point at which to support financial

[1] This comparison provides context for magnitude; outcome measures, populations, and follow-up horizons differ across studies.

literacy. Leaving employment can change individuals' financial circumstances and require them to reconsider decisions about saving, borrowing, and retirement planning, potentially making the relative decline in financial literacy around these transitions particularly consequential. Whether financial education or continued engagement with financial decisions can help preserve financial literacy and improve subsequent financial outcomes remains an open question.

The remainder of the paper is organized as follows. Section II describes the data and sample construction, Section III presents the empirical strategy, Section IV reports the main results, robustness analyses, and additional evidence, and Section V concludes.

II. Data

A. Understanding America Study

We use data from the Understanding America Study (UAS), a nationally representative internet panel of U.S. adults administered by the Center for Economic and Social Research at the University of Southern California (Kapteyn et al., 2024). The UAS repeatedly collects information on respondents' cognitive performance, labor-force status, demographic characteristics, and economic circumstances. Our cognitive measures come from the UAS Cognitive Comprehensive File (CogCF), which harmonizes cognitive assessments administered repeatedly since 2014. We use the version available as of July 2026. The assessments are generally administered about two years apart, and we use up to five assessment waves for each cognitive measure. Actual intervals between successive observed assessments vary across respondents because completion dates and participation across waves differ. We calculate these intervals in days using assessment completion dates and convert them to fractional years for estimation. For financial literacy, the mean interval in the retest-estimation sample is 788 days (SD = 184 days). The UAS is particularly well suited to our research question because repeated financial literacy assessments linked to employment information allow us to trace within-person changes around employment exits and compare these trajectories with those of individuals who remain employed. Its coverage of working-age as well as older adults enables us to examine employment exits across a broad range of adult ages, including transitions occurring well before conventional retirement ages.

Our primary outcome is financial literacy, scored as the total number of correct responses across 14 items. The UAS financial literacy assessment adopts a measure developed for the RAND American Life Panel, incorporating basic questions from earlier financial literacy surveys alongside additional questions on financial instruments and concepts (Alattar, Messel and Rogofsky, 2018). The 14-item assessment combines quantitatively oriented questions involving interest, inflation, purchasing power, and compound interest with knowledge-based questions about stocks, bonds, mutual funds, risk, and diversification. Angrisani and Kapteyn (2024) trace the origins of these questions and describe the composition of the full 14-item UAS battery. Appendix Table A1, Panel A, presents the 14 financial literacy question stems from the first-wave UAS questionnaire.

Alongside financial literacy, we examine five additional cognitive measures: numeracy, number series, picture vocabulary, verbal analogies, and serial sevens. Numeracy is an eight-item assessment of the ability to understand and use percentages, probabilities, and other numerical information. It uses the abbreviated numeracy scale developed by Weller et al. (2013), which combines items from earlier numeracy measures and the Cognitive Reflection Test. Number series, picture vocabulary, and verbal analogies are based on the Woodcock–Johnson Tests of Cognitive Abilities (Woodcock, McGrew and Mather, 2001). Number series requires respondents to identify missing values in numerical sequences, picture vocabulary asks them to name objects shown in pictures, and verbal analogies requires them to identify relationships between words. Serial sevens, a component of the HRS cognitive battery, asks respondents to perform five sequential subtractions beginning with 100 minus 7 (Ofstedal, Fisher and Herzog, 2005) and is scored as the number of correct responses. Numeracy, number series, picture vocabulary, and verbal analogies use item response

theory (IRT) scores provided by the UAS.[2] Appendix Table A1, Panels B–F, provides the questionnaire items or examples for these measures.

The six measures differ in their reliance on accumulated knowledge and fluid cognitive processes. Crystallized intelligence refers to knowledge and skills acquired through education and experience, whereas fluid intelligence refers more closely to the capacity to reason and solve problems independently of previously acquired knowledge (Horn and Cattell, 1967; Hartshorne and Germine, 2015). Picture vocabulary is more closely aligned with crystallized knowledge. Number series places greater weight on fluid reasoning, while numeracy and serial sevens draw on quantitative reasoning, attention, and working memory associated with fluid cognitive abilities. Verbal analogies combines lexical knowledge with relational reasoning. Financial literacy spans both dimensions: some items measure accumulated knowledge of financial concepts and institutions, while others require quantitative reasoning.

Repeated exposure to the assessments may affect observed performance independently of changes in the underlying knowledge or cognitive abilities being measured. Positive practice effects from repeated testing are well documented, but repeated assessment may also alter motivation or fatigue, which can affect performance in the opposite direction (Scharfen, Peters and Holling, 2018; Bell et al., 2018). To account for either type of effect, we estimate a constant, additive retest effect for each cognitive measure by regressing the change in the original score between successive observed assessments on the actual elapsed time between them, expressed in years. Under this specification, the intercept captures the score change associated with an additional assessment, while the slope captures the change associated with elapsed time. We estimate these regressions using the largest available sample of successive assessment pairs in the CogCF for which both scores and the elapsed time are observed, prior to applying the additional sample restrictions used in the main analysis. Appendix Table A2 reports the regression results for the six cognitive measures. We then adjust the original score for each measure by subtracting the estimated retest effect multiplied by the number of prior observed assessments of that measure.

B. Sample Selection

For each cognitive outcome, we first construct a longitudinal sample from the available UAS assessment waves. We exclude observations with missing cognitive scores, employment status, age, gender, education, marital status, number of other household members, or survey year. For financial literacy and numeracy, which are administered together, we require nonmissing scores on both measures. We also exclude assessments completed more than 30 days after they were started. Respondents must have participated in at least two consecutive assessment waves and must be observed in employment at least once. We impose no additional age restrictions on the UAS adult sample, allowing us to examine employment exits across a broad range of adult ages, including younger, middle-aged, and older adults. The cognitive measures are administered across different UAS studies, with varying assessment schedules and respondent participation histories. We therefore construct the analysis sample separately for each cognitive measure.

We classify respondents who are currently working as employed; those who are on sick or other leave, unemployed, retired, disabled, or otherwise not employed are classified as non-employed. We define treatment as the first observed transition from employment to nonemployment and refer to this transition as an employment exit.

To identify an employment exit, we require a pair of consecutive assessment waves in which the respondent is employed in the earlier wave and non-employed in the later wave, with valid cognitive scores and employment information in both waves. For the 1.9 percent of treated individuals with multiple observed employment exits, we use the first transition. The first wave in which the individual is observed outside employment defines the event time, and we retain the individual's available observations before and after that transition. Employment exits encompass several types of labor-force transitions: across the outcome-specific matched samples, approximately half involve unemployment through layoff or job search, about

[2]IRT scores summarize performance across items while accounting for differences in item difficulty and discrimination.

two-fifths involve retirement, and the remainder involve sick or other leave, disability, or mixed labor-force statuses. Our baseline analysis pools these transitions and treats them uniformly as exits from employment; in additional analyses, we examine whether the cognitive response differs across exit types.

We then construct a matched comparison group following a procedure similar to Freise, Schmitz and Westphal (2022). For each treated individual, we identify respondents with the same observed history up to the employment exit but who remain employed when the treated individual exits. Specifically, treated individuals and their matched controls must have the relevant cognitive measure observed in the same assessment waves and calendar years, the same gender and age in each corresponding wave, and the same employment-status history prior to the event. The two groups differ at the event time: the treated individual leaves employment, while the control remains employed throughout the remainder of the observed event window. This matching procedure aligns treated individuals and controls not only on demographic characteristics and observation timing, but also on their labor-market trajectories leading up to the employment exit. We do not require treated individuals and controls to have the same education, marital status, or household composition across assessment waves, as these characteristics may themselves be affected by an employment exit. Instead, we assess robustness by allowing age-related cognitive trajectories to vary with these characteristics measured before the exit.

Matching is conducted with replacement, allowing an eligible control to be matched to more than one treated individual. When a treated individual has multiple eligible controls, we retain all matches and assign the controls weights that sum to one within that matched set, while each treated individual receives a weight of one.

For each cognitive outcome, we standardize the retest-adjusted scores using the matching-weighted mean and standard deviation calculated over all observations in its matched sample, pooling treated individuals and matched controls across pre- and post-exit periods.

Importantly, matching on pre-exit employment histories does not require individuals to have been continuously employed throughout the pre-treatment period. Individuals may have entered employment from nonemployment before the transition that defines treatment, provided that treated individuals and their matched controls share the same pre-event employment trajectory. The identifying comparison therefore begins only when their employment paths diverge at the treated individual's employment exit.

We likewise impose no restrictions on treated individuals' employment status after the initial exit. Treated individuals may remain outside employment, return to work, or experience subsequent employment transitions. The estimates should therefore be interpreted as the response to an initial employment exit and the labor-market trajectory that follows, rather than as the effect of remaining continuously outside employment.

C. Descriptive statistics

We begin by documenting within-person variation in cognitive performance in the matched samples. Table 1 summarizes changes in the six cognitive measures across repeated assessments. Panel A reports annualized changes between successive assessments. For every outcome, mean absolute changes are substantially larger than mean signed changes. For financial literacy, for example, the mean annualized change is only -0.003 standard deviations per year, compared with a mean absolute annualized change of 0.227 standard deviations. Thus, the near-zero average change masks substantial movements in individual financial literacy, with positive and negative changes largely offsetting one another in the aggregate.

Panel B compares each respondent's first and last assessments. The same pattern remains over these longer intervals: mean absolute annualized changes are substantially larger than mean signed changes. For financial literacy, for example, the mean annualized change is -0.003 standard deviations per year, compared with a mean absolute annualized change of 0.128 standard deviations. Moreover, 55.8 percent of respondents score higher at their final assessment and 44.2 percent score lower. The mean span between the first and last assessments is 5.54 years.

Table 1: Within-Person Changes in Cognitive Performance

	Financial literacy	Numeracy	Number series	Picture vocabulary	Verbal analogies	Serial sevens
Panel A. Successive-assessment changes						
Mean annualized change (SD/year)	-0.003	-0.012	-0.029	0.101	0.019	-0.047
Mean absolute annualized change (SD/year)	0.227	0.246	0.272	0.274	0.333	0.326
Mean interval between successive assessments (years)	2.08	2.08	2.04	2.04	2.04	2.04
Assessment pairs	4,401	4,401	6,320	6,022	5,973	3,676
Respondent-year observations	6,320	6,320	8,733	8,328	8,242	5,205
Respondents	1,919	1,919	2,413	2,306	2,269	1,529
Panel B. First-to-last changes						
Mean annualized first-to-last change (SD/year)	-0.003	-0.013	-0.030	0.101	0.020	-0.047
Mean absolute annualized first-to-last change (SD/year)	0.128	0.131	0.133	0.149	0.147	0.179
Share with higher final score (%)	55.8	42.0	37.4	79.1	45.1	15.9
Share with lower final score (%)	44.2	58.0	62.6	20.9	54.9	84.1
Mean observation span (years)	5.54	5.54	5.94	5.82	5.86	5.55
Respondents	1,919	1,919	2,413	2,306	2,269	1,529

Notes: The table reports within-person changes in standardized cognitive scores in the outcome-specific matched samples. Controls appearing in multiple matched sets are counted once, with their weights summed across sets. Panel A reports annualized changes between successive assessments, averaged first within respondent and then across respondents. Respondent-year observations are unweighted counts of distinct respondent–calendar-year observations. Panel B reports annualized first-to-last changes. Annualization uses the actual elapsed time between assessments.

The data therefore contain substantial within-person variation in cognitive performance even when average changes are limited. We therefore ask whether employment exits account for a systematic component of this individual-level variation.

Table 2 further describes the outcome-specific matched samples summarized in Table 1. Panel A reports mean standardized cognitive scores for treated individuals and matched controls before and after the employment exit. Sample sizes vary across outcomes due to differences in assessment schedules and participation histories. All reported means and standard deviations use the matching weights.

Treated individuals have lower average cognitive scores than their matched controls even before the employment exit. For financial literacy, for example, the pre-exit means are -0.11 standard deviations for treated individuals and 0.10 for controls. Similar differences in levels are present for the other cognitive outcomes. These differences do not by themselves pose a problem for the event-study design. Identification does not require treated individuals and matched controls to have the same level of cognitive performance before the exit; rather, it requires their cognitive outcomes to follow similar trajectories in the absence of treatment. The matched-record fixed effects absorb persistent differences in levels, while the pre-exit event-study estimates allow us to assess whether the two groups were already experiencing differential changes before the employment exit.

Panel B reports characteristics measured at the last pre-exit observation for the matched sample used to analyze financial literacy and numeracy.[3] Age and gender are identical between treated individuals and matched controls by construction. The two groups are also similar in marital status and the number of other household members. More noticeable differences remain in characteristics that are not included in the exact-matching criteria. In particular, 41 percent of treated individuals have a bachelor's degree or higher, compared with 53 percent of matched controls. Treated individuals are also somewhat less likely to be White and more likely to be Black.

These remaining differences do not, by themselves, invalidate the matched comparison. The event-study

[3]The cognitive measures are drawn from separate UAS studies and therefore differ in sample coverage. We construct the analysis sample separately for each outcome, retaining all available observations for that measure before applying the matching procedure. Financial literacy and numeracy are both collected in Study P and therefore share the same underlying respondent-wave sample; the remaining cognitive outcomes are drawn from separate studies and have their own analysis samples. Importantly, although these study-specific samples differ in size, their distributions of observed baseline characteristics are very similar.

Table 2: Summary Statistics by Treatment Status

Panel A. Standardized cognitive outcomes

	Treated		Control		Overall statistics				
	Pre	Post	Pre	Post	Mean	SD	Min	Max	*N*
Financial Literacy	-0.11	-0.14	0.10	0.15	0.00	1.00	-2.97	1.44	8,583
Numeracy	-0.11	-0.19	0.18	0.12	0.00	1.00	-2.16	2.15	8,583
Number Series	0.02	-0.11	0.12	0.00	0.00	1.00	-4.56	1.59	15,924
Picture Vocabulary	-0.18	0.12	-0.17	0.16	0.00	1.00	-4.27	2.49	15,351
Verbal Analogies	-0.01	-0.09	0.06	0.05	0.00	1.00	-4.93	2.08	14,773
Serial Sevens	0.10	-0.16	0.13	-0.03	0.00	1.00	-4.55	0.53	7,524

Panel B. Baseline characteristics

	Treated		Control		Overall statistics				
	Mean	SD	Mean	SD	Mean	SD	Min	Max	*N*
Age	49.74	13.90	49.74	13.89	49.74	13.89	19.00	82.00	2,718
Male	0.38	0.49	0.38	0.49	0.38	0.49	0.00	1.00	2,718
Education									
Less than high school	0.05	0.22	0.01	0.11	0.03	0.18	0.00	1.00	2,718
High school	0.18	0.39	0.14	0.34	0.16	0.37	0.00	1.00	2,718
Some college	0.36	0.48	0.32	0.47	0.34	0.47	0.00	1.00	2,718
Bachelor's or higher	0.41	0.49	0.53	0.50	0.47	0.50	0.00	1.00	2,718
Race									
White	0.74	0.44	0.78	0.41	0.76	0.43	0.00	1.00	2,718
Black	0.12	0.32	0.08	0.27	0.10	0.30	0.00	1.00	2,718
Other	0.14	0.35	0.14	0.35	0.14	0.35	0.00	1.00	2,718
Married	0.56	0.50	0.56	0.50	0.56	0.50	0.00	1.00	2,718
Other household members	1.59	1.34	1.54	1.32	1.56	1.33	0.00	7.00	2,718

Notes: Means and standard deviations use the matching weights. In Panel A, Pre includes observations before the employment exit and Post includes the exit year and subsequent observations. Cognitive outcomes are standardized scores. Panel B reports characteristics at the last pre-exit observation, one to two years before the employment exit, using the financial-literacy/numeracy matched sample. Indicator means are weighted proportions. *N* denotes the unweighted number of observations with positive matching weight.

design absorbs time-invariant differences between matched records and relies on parallel changes rather than balance in baseline levels. Nevertheless, baseline characteristics may be associated with different cognitive trajectories over the life cycle. As described in Section III, we therefore assess robustness to allowing age-related cognitive profiles to vary with characteristics measured before the employment exit.

III. Empirical Strategy

Our empirical strategy compares changes in cognitive performance around an employment exit between individuals leaving employment and matched controls who remain employed. Treated individuals and their matched controls share the same age, gender, panel participation, and pre-exit employment histories. We implement this approach separately for each of the six cognitive outcomes using its corresponding matched sample.

Specifically, we estimate

$$Y_{it} = \sum_{k \neq -2} \left(\beta_k \cdot \text{Treated}_i \cdot 1[\text{Event time}_{it} = k] + \mu_k 1[\text{Event time}_{it} = k]\right) + \gamma_i + \delta_t + \varepsilon_{it}. \tag{1}$$

Here, i indexes an individual within a matched set, and t denotes calendar year. Matching with replacement allows a control respondent to appear in multiple matched sets, with a distinct index i in each set. Y_{it} is the standardized cognitive score for that individual in year t. Event time is defined relative to the treated individual's employment exit and is assigned to both the treated individual and the controls in the same matched set. Thus, treated individuals and their matched controls are compared at the same relative and calendar times.

We group event time into two-year intervals, keeping the employment-exit year as a separate period labeled $k = 0$. We label each interval by its earlier year before the exit and its later year after the exit. For example, the interval covering one and two years after the exit is labeled $k = 2$. We omit $k = -2$, corresponding to one to two years before the employment exit, as the reference period. The coefficients of interest, β_k, measure the change in the treated-control difference at each event time relative to the difference during the two years immediately preceding the employment exit. The coefficients μ_k absorb event-time movements common to treated individuals and their matched controls.

The specification includes individual-by-matched-set fixed effects (hereafter, matched-record fixed effects), γ_i, and calendar-year fixed effects, δ_t. Each individual within each matched set has a separate fixed effect, which absorbs time-invariant differences in cognitive performance. Calendar-year fixed effects account for aggregate changes common to respondents observed in the same year. Standard errors are clustered by the original respondent identifier, allowing observations from the same respondent to be correlated across both survey waves and matched sets.

We do not control for contemporaneous education, marital status, or household composition in the baseline regressions. These characteristics may themselves change following an employment exit, so controlling for their post-exit values could absorb part of the response to the employment transition. Race is time invariant and is absorbed by the matched-record fixed effects. As a robustness check, we instead allow cognitive trajectories to vary with characteristics measured before the employment exit by interacting these pre-exit characteristics with a linear age trend. This adjustment addresses differences in age-related cognitive profiles associated with baseline characteristics without conditioning on their potentially endogenous post-exit realizations.

The baseline specification pools event-time effects across employment-exit cohorts. This pooling can be problematic if the effects of employment exits differ across cohorts, because conventional two-way fixed-effects event-study coefficients need not recover a simple average of cohort-specific effects under treatment-effect heterogeneity. We therefore complement the baseline specification with the interaction-weighted estimator of Sun and Abraham (2021). We define treatment cohorts by the calendar year in which an

individual is first observed leaving employment. The interaction-weighted estimator first estimates cohort-specific event-time effects and then aggregates them using cohort shares, thereby allowing the effects at a given event time to vary across employment-exit cohorts. In our matched design, controls remain employed at and after the treated individual's event time, so the comparison within a matched set does not use already-treated observations as controls.

For both estimators, identification relies on the counterfactual evolution of cognitive performance among treated individuals relative to their matched controls. The key assumption is that, absent the employment exit, treated individuals would have experienced the same changes in cognitive performance as their matched controls. Exact matching on age, gender, observation timing, and pre-exit employment histories makes the comparison groups similar along dimensions closely related to both cognitive trajectories and labor-market histories, while the matched-record fixed effects absorb remaining time-invariant differences. The event-study coefficients before the exit provide a direct diagnostic for whether the two groups were already following different cognitive trajectories prior to treatment.

This design does not, however, eliminate the possibility that time-varying factors jointly affect employment exits and cognition. Changes in health, household circumstances, or financial conditions, for example, may contribute both to leaving employment and to subsequent cognitive outcomes. A causal interpretation therefore requires the additional assumption that, conditional on the matching design and included fixed effects, no unobserved time-varying factor generates a differential change in cognition coinciding with the employment exit. Absent this assumption, the coefficients should be interpreted as changes in cognition relative to matched controls around the employment exit rather than as causal effects of the exit itself. One particularly relevant concern is deteriorating health, which could contribute to both an employment exit and a decline in cognitive performance. The concentration of the post-exit response in financial literacy rather than across cognitive domains makes a broad health-related deterioration in cognition a less natural explanation for the results; we examine this possibility more directly in the robustness analysis by restricting the sample to individuals whose health remains stable around the employment exit. The main finding remains robust under these restrictions, reducing concern that contemporaneous deterioration in self-reported health drives the result.

IV. Results

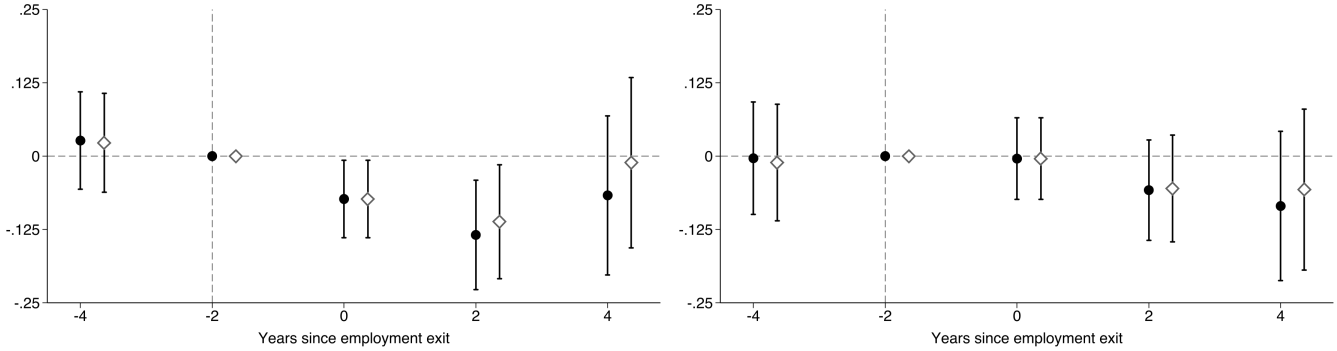
A. Baseline results

Figure 1 presents the event-study estimates for financial literacy and five additional cognitive outcomes. For each outcome, we report estimates from both the standard two-way fixed-effects specification and the interaction-weighted (IW) estimator. The two approaches produce broadly similar estimates, suggesting that the main pattern is not sensitive to allowing treatment effects to vary across employment-exit cohorts.

The central finding is a decline in financial literacy relative to matched controls following an employment exit. In the year of the exit, the standard event-study estimate is -0.073 standard deviations. The estimate becomes more negative, reaching -0.134 standard deviations two years after the exit. The IW estimates show a similar pattern, with effects of -0.073 standard deviations in the exit year and -0.112 standard deviations two years later. Both estimates are statistically significant at the 5 percent level at these horizons. Thus, relative to the reference period two years before the exit, financial literacy begins to diverge from that of matched controls around the employment exit, with the largest difference emerging over the following two years.

To put the magnitude of the financial literacy response in perspective, we compare our estimates with the effects of financial education interventions. Kaiser et al. (2022), in a meta-analysis of 50 randomized experiments reporting financial knowledge outcomes, estimate an average improvement of approximately 0.20 standard deviations. Our estimated relative decline of 0.11–0.13 standard deviations is therefore roughly half to two-thirds as large in absolute magnitude. In a separate randomized experiment conducted through the UAS, Clark et al. (2025) find that an online risk-diversification intervention increased the

Figure 1: Event-Study Estimates of Cognitive Responses to Employment Exits

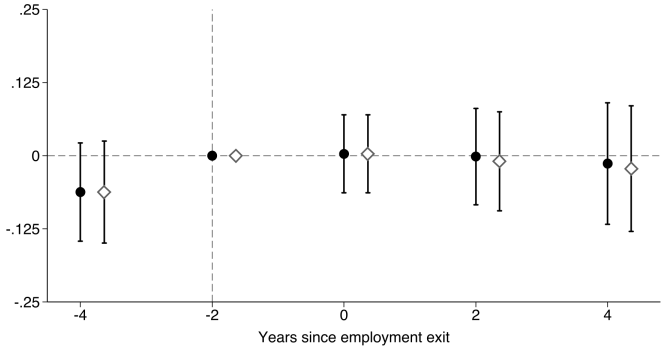


(a) Financial Literacy

Joint post-exit test: $p_{\text{TWFE}} = 0.021$; $p_{\text{IW}} = 0.029$

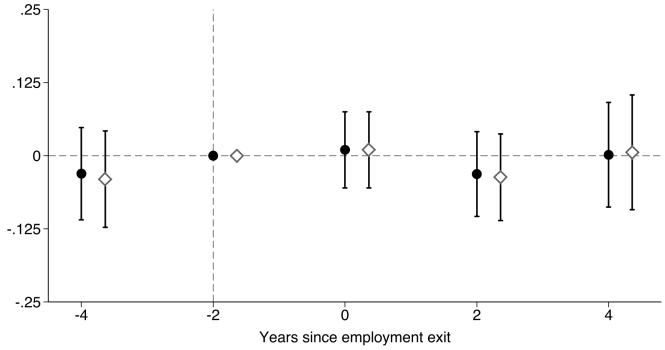
(b) Numeracy

Joint post-exit test: $p_{\text{TWFE}} = 0.449$; $p_{\text{IW}} = 0.647$



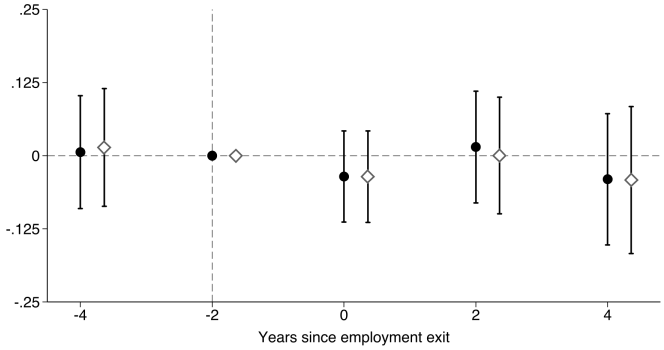
(c) Number Series

Joint post-exit test: $p_{\text{TWFE}} = 0.990$; $p_{\text{IW}} = 0.972$



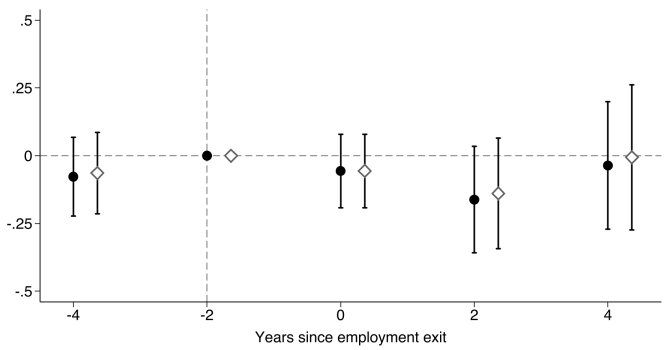
(d) Picture Vocabulary

Joint post-exit test: $p_{\text{TWFE}} = 0.752$; $p_{\text{IW}} = 0.659$



(e) Verbal Analogies

Joint post-exit test: $p_{\text{TWFE}} = 0.617$; $p_{\text{IW}} = 0.752$



(f) Serial Sevens

Joint post-exit test: $p_{\text{TWFE}} = 0.327$; $p_{\text{IW}} = 0.399$

● Two-way fixed-effects estimate ◇ Interaction-weighted estimate

Notes: Each panel reports estimates for one cognitive outcome using its outcome-specific matched sample. The omitted category is one to two years before the employment exit. All event-time intervals are included in estimation, but coefficients for the intervals covering five to six years before and after the exit are omitted from the figure because fewer observations yield less precise estimates. Treated individuals receive weight one, and control weights sum to one within each matched set. All specifications include matched-record and calendar-year fixed effects. Standard errors are clustered by respondent; vertical bars show 95 percent confidence intervals. Joint post-exit tests evaluate whether the coefficients at event times 0, 2, and 4 are jointly zero. Reported p-values use Wald F tests for TWFE and asymptotic Wald χ^2 tests for the aggregated IW estimates.

probability of correctly answering two intervention-specific knowledge questions by 7.8–8.5 percentage points eight months later, corresponding to approximately 0.16–0.18 item-level standard deviations.[4] At that horizon, the compound-interest and inflation interventions no longer had statistically significant effects on their respective knowledge questions. Although the outcome measures, populations, and follow-up horizons differ across studies, these comparisons suggest that the relative decline in financial literacy around employment exits is comparable in magnitude to the gains achieved through some financial education interventions.

For numeracy, number series, picture vocabulary, verbal analogies, and serial sevens, the post-exit estimates are generally small and statistically indistinguishable from zero under both estimators. Although some estimates become negative at longer horizons, they are imprecisely estimated and do not reveal a consistent pattern across outcomes. We therefore find little evidence that leaving employment is followed by a broad deterioration in cognitive performance. Rather, the post-exit divergence is concentrated in financial literacy.

The pre-exit estimates provide support for the identifying assumption underlying the event-study design. Across the six outcomes, the estimates four years before the employment exit are generally small and statistically indistinguishable from zero under both the standard and IW estimators. There is therefore little indication that the cognitive trajectories of treated individuals and their matched controls were already diverging prior to the employment exit.

We next examine two dimensions of the financial literacy response: whether it varies across individuals with different pre-exit characteristics, and what trajectories underlie the relative post-exit decline.

B. Heterogeneity by Pre-Exit Characteristics

We examine heterogeneity in the financial literacy response by age, gender, education, marital status, and ownership of durable financial assets, all measured before the employment exit. These characteristics capture differences in life-cycle stage, human capital, household circumstances, and prior financial experience that may shape how financial knowledge evolves after leaving employment.

For each analysis, subgroup membership is determined before the employment exit and held fixed throughout the event window. We divide the sample at age 50, distinguish men from women, respondents with and without a bachelor's degree, and married from unmarried respondents. Age and gender are exact matching variables in the baseline design, so these comparisons use the original matched sample. Education and marital status are not matching variables, so we reconstruct the matched samples within each subgroup to ensure that treated individuals and their controls belong to the same group. We follow the same approach for the asset analysis described below.

To estimate heterogeneity, we extend the baseline event-study specification to a dynamic difference-in-difference-in-differences design, interacting the treatment-by-event-time indicators with the relevant subgroup indicator and including the corresponding lower-order terms.[5] We report the two group-specific effects and their difference in Table 3.

The age results suggest that the financial literacy response is more pronounced among younger respondents, although the differences across age groups are not statistically precise. Among respondents aged 50 or younger, financial literacy falls by 0.111 standard deviations in the year of the employment exit and by

[4] We express the estimates in Clark et al. (2025, Table 2) in standard deviation units by dividing the reported treatment effects by the standard deviations of the corresponding binary outcomes, calculated as $\sqrt{p(1-p)}$, where p is the outcome mean reported in that table. These standardized effects are not directly comparable to our estimates based on the full 14-item financial literacy assessment.

[5] For subgroup indicator G_i, we estimate

$$Y_{it} = \sum_{k \neq -2} \Big[\beta_k \, \text{Treated}_i \times \mathbf{1}(\text{EventTime}_{it} = k) + \mu_k \mathbf{1}(\text{EventTime}_{it} = k) + \lambda_k G_i \times \mathbf{1}(\text{EventTime}_{it} = k) + \rho_k G_i \times \text{Treated}_i \times \mathbf{1}(\text{EventTime}_{it} = k) \Big] + \gamma_i + \delta_t + \varepsilon_{it}.$$

At event time k, the employment-exit effect is β_k for the reference group and $\beta_k + \rho_k$ for the other group, while ρ_k gives the between-group difference.

Table 3: Heterogeneity in Financial Literacy Responses to Employment Exits

Years since employment exit	Panel A. Age: Age ≤ 50	Panel A. Age: Age > 50	Panel A. Age: Age > 50 − Age ≤ 50	Panel B. Gender: Male	Panel B. Gender: Female	Panel B. Gender: Female − Male	Panel C. Education: Low education	Panel C. Education: High education	Panel C. Education: High − Low
−4	−0.001	0.047	0.048	0.061	0.004	−0.056	−0.046	0.118*	0.164*
	(0.088)	(0.047)	(0.101)	(0.062)	(0.057)	(0.084)	(0.071)	(0.069)	(0.099)
−2	—	—	—	—	—	—	—	—	—
0	−0.111**	−0.044	0.067	−0.022	−0.105**	−0.083	−0.117**	0.092	0.209***
	(0.054)	(0.042)	(0.068)	(0.046)	(0.046)	(0.065)	(0.056)	(0.057)	(0.080)
2	−0.210***	−0.077	0.133	−0.161**	−0.116*	0.045	−0.042	−0.134	−0.092
	(0.081)	(0.054)	(0.096)	(0.073)	(0.062)	(0.096)	(0.078)	(0.085)	(0.115)
4	−0.067	−0.073	−0.006	−0.132	−0.022	0.110	0.020	−0.128	−0.148
	(0.111)	(0.082)	(0.136)	(0.082)	(0.101)	(0.130)	(0.109)	(0.109)	(0.154)
Observations		8,583			8,583			4,674	
Individuals		2,718			2,718			1,456	
Clusters		1,919			1,919			1,244	
Equality test: $k = 0, 2$ (p-value)		0.342			0.241			0.003	
Equality test: $k = 0, 2, 4$ (p-value)		0.433			0.277			0.004	

Years since employment exit	Panel D. Marital Status: Unmarried	Panel D. Marital Status: Married	Panel D. Marital Status: Married − Unmarried	Panel E. Durable Assets: No assets	Panel E. Durable Assets: Has assets	Panel E. Durable Assets: Assets − No assets
−4	0.079	0.073	−0.006	−0.152*	0.044	0.196*
	(0.084)	(0.066)	(0.106)	(0.089)	(0.061)	(0.108)
−2	—	—	—	—	—	—
0	−0.122*	−0.039	0.082	−0.194***	−0.025	0.170**
	(0.067)	(0.050)	(0.084)	(0.068)	(0.048)	(0.084)
2	−0.222**	−0.129*	0.092	−0.161*	−0.124**	0.037
	(0.093)	(0.068)	(0.115)	(0.095)	(0.061)	(0.113)
4	−0.278**	0.027	0.305**	−0.128	−0.125	0.003
	(0.125)	(0.088)	(0.153)	(0.124)	(0.091)	(0.153)
Observations		4,940			4,559	
Individuals		1,524			1,392	
Clusters		1,265			1,144	
Equality test: $k = 0, 2$ (p-value)		0.565			0.113	
Equality test: $k = 0, 2, 4$ (p-value)		0.236			0.207	

Notes: The first two estimate columns in each panel report group-specific event-study estimates; the third reports the difference between groups. The omitted period is one to two years before the employment exit. Subgroup characteristics are measured before the exit and held fixed. Low education denotes less than a bachelor's degree. Durable assets include retirement accounts, stocks or mutual funds, certificates of deposit or Treasury bills, and bonds or bond funds. Education, marital-status, and asset analyses rematch treated individuals and controls within subgroup using the baseline matching criteria. Specifications include the relevant lower-order terms, matched-record and calendar-year fixed effects, and matching weights. Standard errors are clustered by respondent. The equality tests jointly test whether the between-group differences are zero at event times 0 and 2, and at event times 0, 2, and 4, respectively. Both use respondent-clustered Wald F tests. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

0.210 standard deviations two years later. The corresponding estimates for respondents older than 50 are smaller and statistically insignificant. However, the estimated differences between the two age groups are themselves insignificant. The evidence therefore points to a stronger response among younger adults, but does not establish statistically significant heterogeneity by age.

The gender results reveal a difference primarily in the timing of the response. For women, financial literacy falls by 0.105 standard deviations in the year of the employment exit, whereas the contemporaneous estimate for men is close to zero. Two years later, however, the estimates are negative for both men and women and are similar in magnitude. None of the differences between men and women is statistically significant. Thus, while the financial literacy response appears earlier among women, we find little evidence that its overall magnitude differs systematically by gender.

The education results provide clearer evidence of heterogeneity. Among respondents without a bachelor's degree, financial literacy falls by 0.117 standard deviations in the year of the employment exit. In contrast, the corresponding estimate for college graduates is positive and statistically indistinguishable from zero. The difference between the two groups is 0.209 standard deviations and statistically significant at the 1 percent level. Differences at subsequent horizons are smaller and statistically insignificant. The evidence therefore indicates that the financial literacy response to an employment exit is concentrated among respondents with less education, although the difference between education groups is not statistically significant at subsequent horizons. The marginally significant difference in the earlier pre-exit period also warrants some caution in interpreting this contrast.

The marital-status results exhibit a different pattern. Among unmarried respondents, the relative decline in financial literacy becomes larger following the employment exit, reaching 0.222 standard deviations two years afterward and 0.278 standard deviations four years afterward. For married respondents, the corresponding estimates are smaller and the relative decline largely disappears by four years after the exit. At that horizon, the difference between married and unmarried respondents is 0.305 standard deviations and statistically significant at the 5 percent level. These estimates suggest that the post-exit divergence in financial literacy may be more persistent among unmarried individuals.

Finally, we examine heterogeneity by ownership of durable financial assets. We classify respondents as asset owners if, prior to the employment exit, they report owning at least one IRA or Keogh account, stock or mutual fund, certificate of deposit or Treasury bill, or bond or bond fund. This measure provides an additional dimension of financial experience beyond formal education: ownership of these assets indicates prior participation in financial markets or institutions and exposure to financial decisions involving saving and investment. Asset status is measured using information available before the employment exit, and treated individuals are rematched to controls within asset-ownership groups.

The asset results reveal a sharp difference at the time of the employment exit. Financial literacy falls by 0.194 standard deviations among respondents without durable financial assets, compared with a small and statistically insignificant decline of 0.025 standard deviations among asset owners. The difference between the two groups is 0.170 standard deviations and statistically significant at the 5 percent level. Two years later, however, both groups exhibit negative estimates and the difference between them is small and statistically insignificant. As with education, the evidence therefore points to heterogeneity in the exit-year response rather than a persistent separation between the two groups. The marginally significant pre-exit difference at the earlier horizon further suggests that the asset comparison should be interpreted cautiously.

Taken together, the heterogeneity results reveal differences in the timing and persistence of financial literacy responses across groups. The age and gender estimates differ in their timing and magnitude, but the corresponding between-group differences are not statistically significant. Clearer differences emerge in the exit year for education and durable financial assets, with larger relative declines among respondents with less education and those without prior asset ownership. Marital status is associated with differences later in the post-exit period, with a more persistent relative decline among unmarried respondents. Joint tests reject equality across education groups when considering the exit year and two years afterward together.

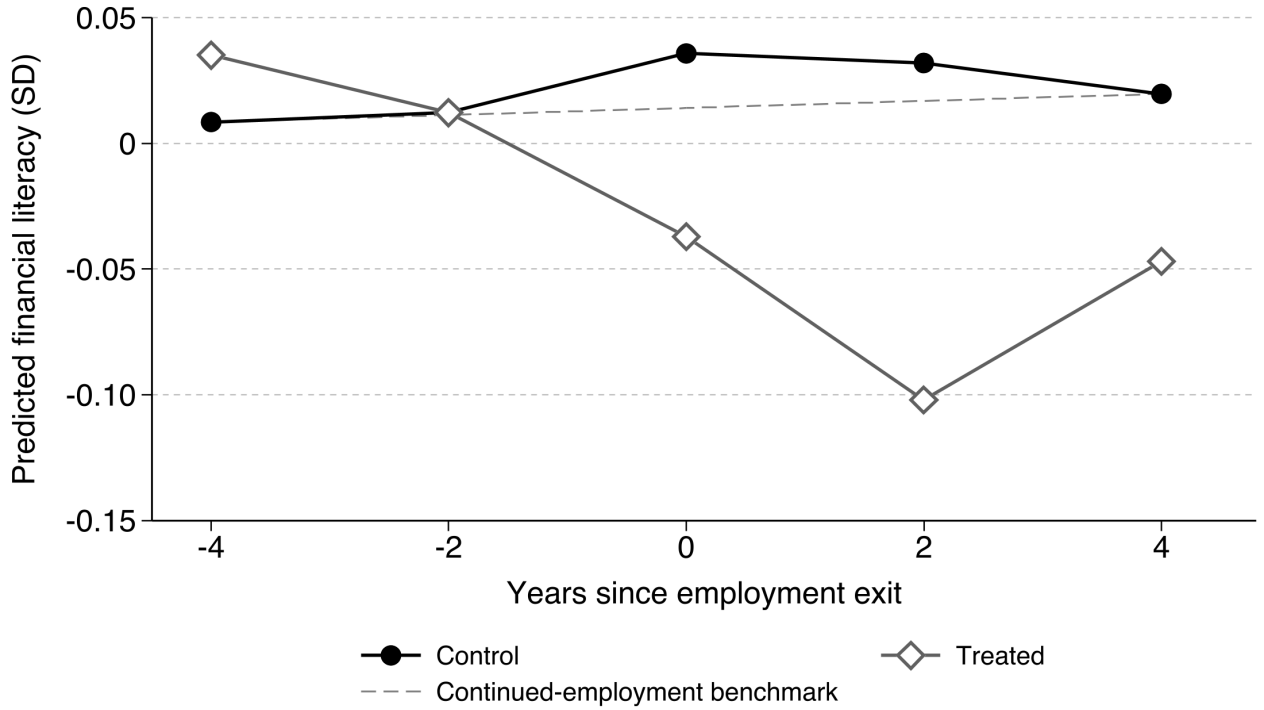
This conclusion also holds when the test includes four years after the exit. The corresponding tests do not reject equality by age, gender, marital status, or asset ownership at the 10 percent level.

C. Predicted Financial Literacy Trajectories

The event-study estimates establish that financial literacy declines relative to matched controls following an employment exit. This relative decline can arise from different underlying trajectories. Financial literacy among treated individuals may continue to increase, but more slowly than among controls, or it may decline in absolute terms following the employment exit. To distinguish between these possibilities, Figure 2 plots predicted financial literacy trajectories for treated individuals and their matched controls.

We construct these predictions from the baseline financial literacy event-study specification. For each observed matched record, we retain the estimated event-time terms and calendar-year fixed effects, while removing the matched-record fixed effects. We then average the predictions separately for treated individuals and matched controls at each event horizon using the matching weights. The resulting trajectories therefore abstract from persistent individual-level differences while preserving the estimated evolution of financial literacy around the employment exit and common calendar-time variation.

Figure 2: Predicted Financial Literacy Trajectories



Notes: Predictions use observed rows of the matched estimation sample and retain the event-study terms and actual calendar-year fixed effects, excluding matched-record fixed effects. Predictions are averaged within each event-time interval using matching weights. Because the panel is unbalanced, contributing respondents and calendar years may vary across intervals. The dashed line connects the first and last control-group predictions and is shown only as a descriptive reference.

Figure 2 shows that the relative decline identified by the event-study estimates reflects different predicted trajectories for the two groups. Predicted financial literacy among matched controls rises modestly, from 0.009 standard deviations four years before the employment exit to 0.020 four years afterward. By contrast, predicted financial literacy among individuals leaving employment falls from 0.012 standard deviations in the reference period to −0.102 two years after the exit, before partially recovering to −0.047 four years afterward. Two years after the exit, the corresponding prediction for matched controls is 0.032 standard

deviations, so the treated prediction is 0.134 standard deviations lower. Because the predicted treated-control difference is normalized to zero in the reference period, this gap corresponds to the conventional event-study estimate. The relative post-exit decline therefore reflects both modest gains among matched controls and a decline in predicted financial literacy among individuals leaving employment, rather than merely slower growth among those leaving employment.

One possible interpretation is that employment supports financial knowledge through two related margins: continued opportunities to acquire new knowledge and repeated use that helps maintain or reinforce knowledge already acquired. Models of financial knowledge as an endogenous form of human capital emphasize continued investment and accumulation in response to incentives (Lusardi, Michaud and Mitchell, 2017), while learning-by-doing mechanisms suggest that repeated use can help sustain acquired skills and knowledge (Arrow, 1962). Workplace financial education provides an additional channel through which employment may expose individuals to financially relevant information (Bernheim and Garrett, 2003). Leaving employment may weaken both the accumulation and reinforcement margins. The predicted decline among individuals leaving employment is consistent with reduced use or reinforcement of existing financial knowledge, while the divergence from matched controls may also reflect fewer opportunities for continued learning. The predicted trajectories are consistent with both channels but do not separately identify their contributions.

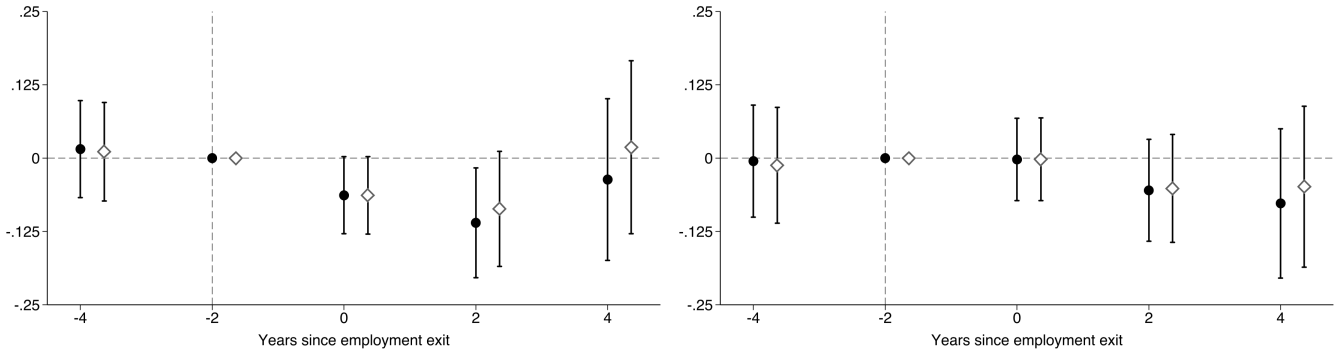
D. Robustness

D1. Robustness to Heterogeneous Age Profiles

The baseline design matches treated individuals and controls on age, but the two groups may still differ in pre-exit characteristics associated with different cognitive trajectories over the life cycle. We therefore allow age profiles to vary with education, race, marital status, and the number of other household members measured before the employment exit. Each characteristic is fixed at its value in event time -2, corresponding to two years before the exit, and interacted with age measured at each observation. This allows for heterogeneous age-related cognitive trends without conditioning on characteristics that may themselves respond to the employment exit. All other features of the baseline design remain unchanged.

Figure 3 shows that the financial literacy result remains robust after allowing cognitive age profiles to vary with pre-exit characteristics. Financial literacy remains lower relative to matched controls in the exit year and two years afterward, with both estimators statistically significant at the 10 percent level in the exit year. Two years after the exit, the conventional estimate is approximately -0.11 standard deviations and statistically significant at the 5 percent level, while the interaction-weighted estimate is somewhat smaller and significant at the 10 percent level. By contrast, the estimates for numeracy, number series, picture vocabulary, verbal analogies, and serial sevens continue to show little systematic post-exit movement. Allowing cognitive age profiles to vary with pre-exit characteristics attenuates the financial literacy estimates somewhat, but financial literacy remains the only outcome showing a clear post-exit divergence.

Figure 3: Event-Study Estimates with Pre-Exit Characteristics × Age Trends

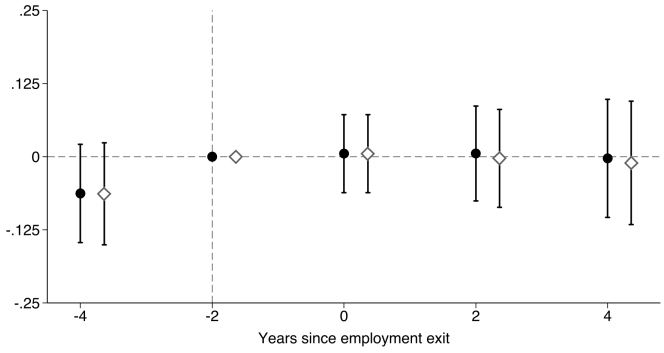


(a) Financial Literacy

Joint post-exit test: $p_{\text{TWFE}} = 0.062$; $p_{\text{IW}} = 0.062$

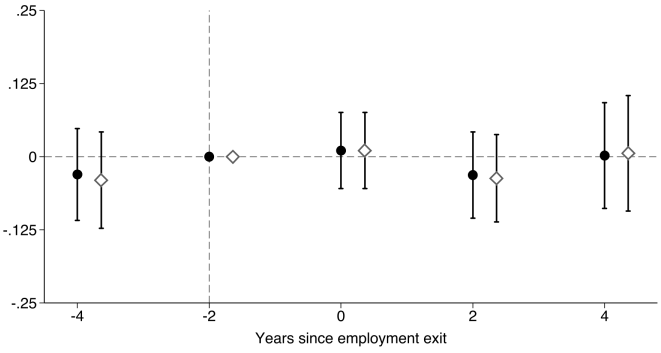
(b) Numeracy

Joint post-exit test: $p_{\text{TWFE}} = 0.511$; $p_{\text{IW}} = 0.699$



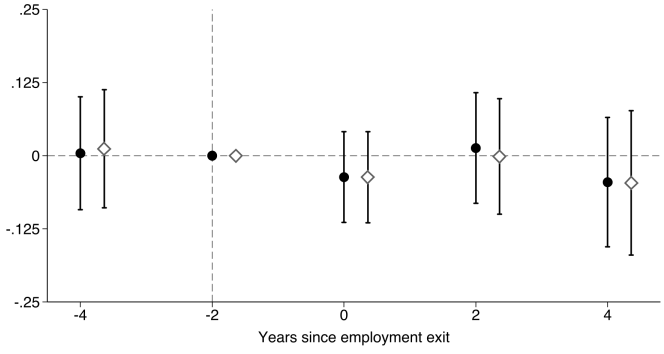
(c) Number Series

Joint post-exit test: $p_{\text{TWFE}} = 0.996$; $p_{\text{IW}} = 0.992$



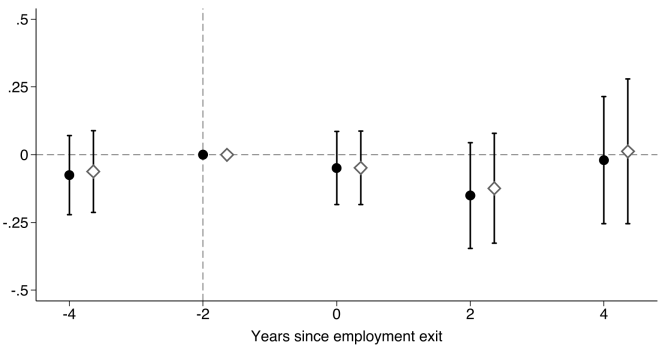
(d) Picture Vocabulary

Joint post-exit test: $p_{\text{TWFE}} = 0.752$; $p_{\text{IW}} = 0.656$



(e) Verbal Analogies

Joint post-exit test: $p_{\text{TWFE}} = 0.591$; $p_{\text{IW}} = 0.725$



(f) Serial Sevens

Joint post-exit test: $p_{\text{TWFE}} = 0.370$; $p_{\text{IW}} = 0.458$

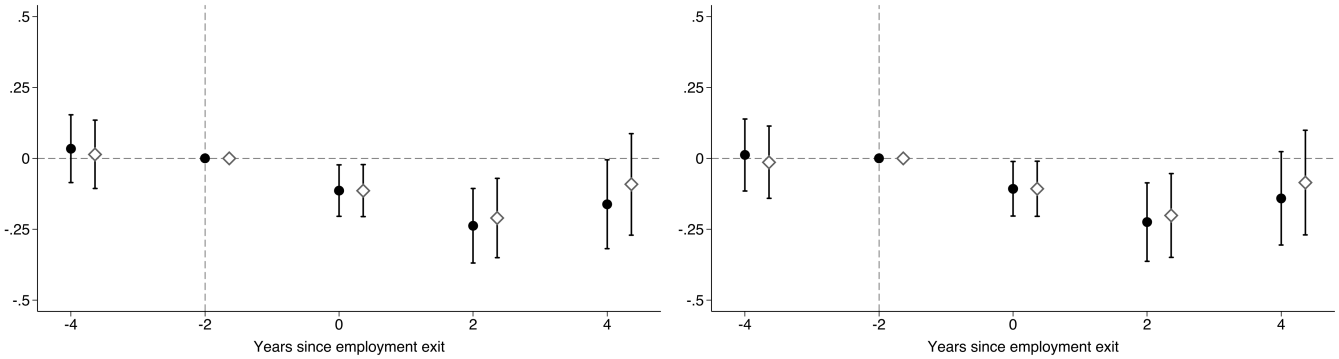
● Two-way fixed-effects estimate ◇ Interaction-weighted estimate

Notes: Specifications augment the baseline event study by interacting current age with education, race, marital status, and household size measured one to two years before the employment exit. All specifications include matched-record and calendar-year fixed effects and otherwise follow the weighting, clustering, normalization, and plotting conventions of Figure 1. Vertical bars show 95 percent confidence intervals. Joint post-exit tests evaluate whether the coefficients at event times 0, 2, and 4 are jointly zero. Reported p-values use Wald F tests for TWFE and asymptotic Wald χ^2 tests for IW; event time 6 is excluded.

D2. Robustness to Health-Related Confounding

A more direct identification concern is that deteriorating health may both contribute to an employment exit and independently affect cognitive performance. We address this possibility by restricting the analysis to matched observations for which self-rated health remains stable around the employment exit. Self-rated health is classified as either excellent, very good, or good, versus fair or poor. In the first specification, both treated individuals and controls must remain in the same health category from event time −2, corresponding to two years before the employment exit, through the exit year. In a stricter specification, we additionally require treated individuals and their matched controls to share the same stable health category. Matched sets and control weights are reconstructed after imposing each restriction.

Figure 4: Financial Literacy Estimates among Respondents with Stable Health



(a) Stable Health within Person

(b) Stable and Pair-Matched Health

Joint post-exit test: $p_{\text{TWFE}} = 0.003$; $p_{\text{IW}} = 0.011$

Joint post-exit test: $p_{\text{TWFE}} = 0.011$; $p_{\text{IW}} = 0.031$

● Two-way fixed-effects estimate ◇ Interaction-weighted estimate

Notes: Self-rated health is classified as excellent/very good/good versus fair/poor. Panel (a) requires each treated and control respondent to remain in the same health category from one to two years before the employment exit through the exit year. Panel (b) additionally requires treated and control respondents within a matched pair to share the same stable health category. Matched sets and weights are reconstructed after each restriction. The samples contain 4,383 observations on 338 treated respondents in panel (a) and 3,610 observations on 299 treated respondents in panel (b). All other estimation and plotting conventions follow Figure 1.

The financial literacy estimates remain negative and statistically significant in both samples. Among respondents whose health remains stable within person, the estimated relative decline is about 0.11 standard deviations in the exit year and 0.21–0.24 standard deviations two years later. Imposing the additional requirement that treated individuals and controls share the same health category yields very similar estimates, of approximately 0.11 and 0.20–0.22 standard deviations. These post-exit estimates are statistically significant, while the pre-exit estimates remain small and statistically indistinguishable from zero in both samples. The persistence of the result when health is held stable around the transition reduces concern that contemporaneous deterioration in self-rated health drives the main financial literacy finding.

E. Additional Analyses

E1. Heterogeneity by Employment-Exit Type

The baseline analysis pools several pathways out of employment. We next ask whether the financial literacy response is specific to a particular type of exit or instead appears across different transitions. We classify first exits into three mutually exclusive groups: unemployment, retirement, and other nonemployment. Unemployment includes layoffs, job search, and mixed statuses containing an unemployment component; retirement includes retirement and related nonworking statuses without an unemployment component; and other nonemployment includes the remaining exits, such as sick leave, other leave, and

disability. Each treated individual and the corresponding matched comparison are assigned to the treated individual's exit category.

We estimate a dynamic difference-in-difference-in-differences specification that allows the event-study effects to vary across the three exit types. Appendix Table A3 reports the exit-type-specific estimates together with formal pairwise differences.

Similar financial literacy responses appear across different types of employment exits, although their timing differs across groups. The estimate is -0.11 standard deviations in the exit year for unemployment exits, while the response emerges two years later for retirement (-0.15) and other nonemployment (-0.22). Although the timing differs across groups, the pairwise differences are not statistically significant, and a joint test does not reject equality of the effects across exit types at event times 0, 2, and 4 ($p = 0.376$). The financial literacy result therefore appears to reflect the broader transition out of employment rather than being driven solely by unemployment or retirement.

The remaining cognitive outcomes provide less systematic evidence of exit-type heterogeneity. Numeracy declines among retirees at longer horizons, including a decline of 0.28 standard deviations four years after exit, while picture vocabulary and verbal analogies show little evidence of systematic post-exit differences. Some differences also appear for number series and serial sevens, but these specifications show evidence of differential pre-trends and are therefore harder to interpret. Overall, the exit-type analysis leaves the central pattern unchanged: financial literacy is the outcome for which the post-exit response is most consistent across different pathways out of employment.

E2. Employment Dynamics

The heterogeneity results raise a natural question about whether differences in post-exit employment trajectories can account for the larger financial literacy responses among younger and less-educated respondents. If these groups remain outside employment for longer after the initial exit, differences in nonemployment exposure could contribute to the observed heterogeneity. To examine this possibility, Appendix Figure A1 uses the same event-study framework, with employment as the outcome, to trace post-exit employment trajectories. We report the full-sample employment trajectory and corresponding estimates by age, gender, and education.

By construction, employment falls to zero relative to controls in the exit year. Employment subsequently recovers, although the treated-control gap remains approximately 0.69 two years after the exit and 0.60 four years afterward. Importantly, the subgroup patterns do not mirror the heterogeneity in financial literacy. Respondents aged 50 or younger return to employment substantially faster than older respondents, even though the younger group exhibits the larger relative decline in financial literacy. Respondents without a bachelor's degree likewise return to employment more quickly than college graduates despite their larger exit-year financial literacy response. Women also recover somewhat faster than men, even though their financial literacy response emerges earlier.

These patterns indicate that the financial literacy heterogeneity is unlikely to be explained simply by differences in the duration of nonemployment. Groups with larger or earlier financial literacy responses do not systematically spend more time outside employment, suggesting that employment exits may affect opportunities to learn, use, or reinforce financial knowledge differently across individuals.

E3. Subjective Well-Being

Finally, we examine subjective well-being around employment exits to assess whether broader economic and subjective changes occur even when most cognitive outcomes remain stable. This analysis provides complementary evidence on the broader consequences of leaving employment and helps place the cognitive results in context. We consider overall life satisfaction, happiness, satisfaction with income, satisfaction with health, satisfaction with work or daily activities, and satisfaction with family life. Prior research documents substantial effects of unemployment and job loss on subjective well-being (Winkelmann and

Winkelmann, 1998; Paul and Moser, 2009; Freise, Schmitz and Westphal, 2022; Cohen, Johnston and Lindner, 2025), including through changes in daily activities and experienced well-being (Knabe et al., 2010; Krueger and Mueller, 2012).

Appendix Figure A2 shows statistically significant declines in several dimensions of well-being at the employment exit. The largest response is in satisfaction with income, which falls by approximately 0.42 standard deviations in the exit year and remains significantly lower through four years afterward. Satisfaction with health also remains lower two years after the exit. Overall life satisfaction, happiness, and satisfaction with work or daily activities decline around the exit but recover more quickly, while satisfaction with family life changes little.

These results show that employment exits are accompanied by substantial changes in economic and subjective well-being even though most cognitive outcomes remain stable. The contrast reinforces the domain-specific nature of the cognitive findings: the absence of broad cognitive deterioration does not reflect an absence of meaningful disruption around the employment transition.

V. Conclusion

This paper studies how financial literacy evolves around exits from employment. Using longitudinal data from the Understanding America Study and a matched event-study design, we find that financial literacy declines relative to matched controls beginning in the year of the employment exit, with the relative decline reaching approximately 0.11–0.13 standard deviations one to two years later. In contrast, we find little evidence of systematic post-exit changes in numeracy, number series, picture vocabulary, verbal analogies, or serial sevens. The response is therefore concentrated in financially relevant knowledge rather than reflecting a broad deterioration in cognitive performance.

Predicted trajectories help clarify the evolution underlying this relative decline. Predicted financial literacy rises modestly among matched controls over the observation window, while it declines among individuals leaving employment before partially recovering. These contrasting trajectories are consistent with two complementary margins through which employment may support financial knowledge. Continued employment may provide opportunities for further learning and accumulation, while repeated use may help maintain or reinforce knowledge already acquired. Leaving employment may weaken both margins.

The financial literacy response is not uniform across individuals. The response in the exit year is more pronounced among respondents without a bachelor's degree and those without prior ownership of durable financial assets, while the relative decline is more persistent among unmarried respondents and appears larger among younger adults. These differences are unlikely to be explained simply by time spent outside employment, since groups with larger or earlier responses do not systematically remain nonemployed for longer. The main finding also persists among individuals whose self-reported health remains stable around the exit, reducing concern that contemporaneous health deterioration drives the result. In addition, the financial literacy response is not confined to a single pathway out of employment: although the timing differs across unemployment, retirement, and other nonemployment exits, similar responses appear across different types of transitions.

At the same time, employment exits are not randomly assigned. Changes in health, household circumstances, financial conditions, or other shocks may affect both the transition out of employment and subsequent cognitive outcomes. Our matched design, pre-exit trajectory checks, and health robustness analysis address some of these concerns, but they cannot eliminate all time-varying confounding. The estimates should therefore be interpreted as changes in financial literacy around employment exits under the conditional parallel-trends assumption, rather than as the causal effect of an exogenously imposed separation from work.

More broadly, the findings suggest that employment may matter for the evolution of financial knowledge through both continued accumulation and maintenance of previously acquired knowledge. This perspective complements evidence on employment and general cognitive functioning by highlighting a domain of economically salient knowledge that may respond differently to changes in individuals' economic environment.

Future work could examine which specific features of employment—financial decisions, employer-provided information, workplace interactions, or other forms of learning and repeated use—contribute to the accumulation and reinforcement of financial knowledge, and whether alternative sources of engagement can substitute for them after employment ends. Designs based on plausibly exogenous employment separations could further distinguish the effect of leaving employment itself from the circumstances that precipitate the transition.

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APPENDIX

Appendix Table A1 presents the first-wave questionnaire items used to construct each cognitive outcome. For the Woodcock–Johnson measures, whose proprietary scored items are not reproduced in the UAS documentation, the table reports an example provided in the first-wave questionnaire.

Table A1: Questionnaire Items for Cognitive Outcomes

Item	Questionnaire item or example from the first wave
Panel A. Financial Literacy (Study P; 14 scored items)	
L001	Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?
L002	Suppose you had $100 in a savings account and the interest rate was 20% per year and you never withdraw money or interest payments. After 5 years, how much would you have in this account in total?
L003	Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, would you be able to buy more than, exactly the same as, or less than today with the money in this account?
L004	Assume a friend inherits $10,000 today and his sibling inherits $10,000 but 3 years from now. Who is richer today because of the inheritance?
L005	Suppose that in the year 2020, your income has doubled and prices of all goods have doubled too. In 2020, will you be able to buy more, the same, or less than today with your income?
D001	Which of the following statements describes the main function of the stock market?
D002	Which of the following statements is correct about mutual funds?
P001	If interest rates rise (fall), what should happen to bond prices?
P002	Is the following statement true? Buying a single-company stock (stock mutual fund) usually provides a safer return than a stock mutual fund (single-company stock).
P003	Is the following statement true or false? Stocks (bonds) are normally riskier than bonds (stocks).
P004	Considering a long period, for example 10 or 20 years, what normally gives the highest return?
P005	Normally, which asset displays the highest fluctuations over time: savings accounts, bonds, or stocks?
P006	When an investor spreads his or her money among different assets, does the risk of losing a lot of money increase, decrease, or stay the same?
P007	Is the following statement true? Housing prices in the United States can never go down.
Panel B. Numeracy (Study P; 8 scored items)	
LIP001	Imagine that we roll a fair, six-sided die 1,000 times. Out of 1,000 rolls, how many times do you think the die would come up as an even number?
LIP002	In the BIG BUCKS LOTTERY, the chances of winning a $10.00 prize are 1%. What is your best guess about how many people would win a $10.00 prize if 1,000 people each buy a single ticket?
LIP003	In the ACME PUBLISHING SWEEPSTAKES, the chance of winning a car is 1 in 1,000. What percent of tickets win a car?
LIP008	If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 1,000?
LIP009	If the chance of getting a disease is 20 out of 100, this would be the same as having what percent chance of getting the disease?
LIP012	Suppose a close friend has a breast lump and must have a mammogram. Of 100 women like her, 10 have a malignant tumor and 90 do not. Of the 10 women with a tumor, the mammogram correctly identifies 9; of the 90 women without a tumor, it incorrectly identifies 10 as having a tumor. If your friend tests positive, what is the likelihood that she actually has a tumor?
LIP015	A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?
LIP017	In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take to cover half of the lake?
Panel C. Number Series (Study N)	
Public questionnaire example	Please enter the number that belongs in the empty box: 2, 4, 6, [blank]. The correct answer is 8 because the numbers increase by 2.
Panel D. Picture Vocabulary (Study V)	
Public questionnaire example	A picture of a car is shown, and the respondent is instructed to type the word "car."
Panel E. Verbal Analogies (Study A)	
Public questionnaire example	Please type the word that best completes the phrase: "Mother is to daughter as father is to [blank]." The correct answer is "son."
Panel F. Serial Sevens (UAS HRS; 5 scored items)	
D142	Now let us try some subtraction of numbers. One hundred minus 7 equals what?

Continued on next page

Appendix Table A1 continued

Item	Questionnaire item or example from the first wave
D143	This is the second subtraction. And 7 from that?
D144	This is the third subtraction. And 7 from that?
D145	This is the fourth subtraction. And 7 from that?
D146	This is the fifth subtraction. And 7 from that?

Notes: Items and examples are taken from the first-wave UAS questionnaires; response options are omitted. Financial literacy is scored as the number correct across 14 items and serial sevens as the number correct across five subtractions. Numeracy uses an eight-item IRT score. Number series, picture vocabulary, and verbal analogies use IRT scores based on alternating Woodcock–Johnson Forms A and B. Panels C–E report questionnaire examples rather than the proprietary scored Woodcock–Johnson items, which are not publicly released. The substantive financial literacy items are unchanged across waves aside from the updated calendar year in L005 and randomized alternatives in P001–P003. Sources: UAS questionnaires and CogCF documentation.

Table A2: Retest Effects across Cognitive Measures

	(1) Financial literacy	(2) Numeracy	(3) Number series	(4) Picture vocabulary	(5) Verbal analogies	(6) Serial sevens
Retest effect (constant)	-0.221^{***}	0.454^{***}	0.508^{**}	-1.172^{***}	0.664^{**}	0.035
	(0.059)	(0.157)	(0.224)	(0.261)	(0.283)	(0.041)
Elapsed time (years)	0.005	-0.184^{***}	-0.276^{**}	0.798^{***}	0.084	-0.044^{**}
	(0.027)	(0.071)	(0.107)	(0.126)	(0.135)	(0.020)
Observations (adjacent pairs)	31,677	30,926	32,417	31,804	30,276	31,304
Respondents (clusters)	13,616	13,235	14,579	14,226	12,878	12,812

Notes: Each column reports a separate OLS regression of the change in the original score between successive assessments on elapsed time in years. The constant is interpreted as the retest effect. Regressions use all available nonmissing assessment pairs before the main analysis-sample restrictions, with standard errors clustered by respondent. Coefficients are in original score units. Downstream adjustment subtracts the estimated constant times the number of prior valid assessments of the same outcome. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Cognitive Responses to Employment Exits by Exit Type

Outcome	Years since employment exit	Exit-type effects: Unemployment	Retirement	Other nonemployment	Pairwise differences: Retirement − unemployment	Other − unemployment	Retirement − other
Financial Literacy	−4	−0.021	0.055	0.078	0.076	0.099	−0.023
		(0.071)	(0.056)	(0.148)	(0.091)	(0.164)	(0.158)
	−2	—	—	—	—	—	—
	0	−0.109**	−0.045	0.006	0.064	0.115	−0.051
		(0.051)	(0.048)	(0.094)	(0.069)	(0.107)	(0.106)
	2	−0.100	−0.147**	−0.223*	−0.047	−0.123	0.077
		(0.072)	(0.066)	(0.126)	(0.095)	(0.144)	(0.142)
	4	−0.121	−0.031	0.154	0.089	0.275	−0.185
		(0.080)	(0.115)	(0.200)	(0.127)	(0.210)	(0.230)
Observations / Individuals / Clusters		8,583 / 2,718 / 1,919					
Post-exit equality test (p-value)		0.376					
Numeracy	−4	0.052	−0.028	−0.134	−0.079	−0.186	0.107
		(0.079)	(0.068)	(0.142)	(0.103)	(0.163)	(0.158)
	−2	—	—	—	—	—	—
	0	−0.021	0.006	0.042	0.027	0.063	−0.036
		(0.053)	(0.054)	(0.087)	(0.075)	(0.101)	(0.102)
	2	0.001	−0.126*	−0.085	−0.127	−0.086	−0.041
		(0.065)	(0.065)	(0.116)	(0.090)	(0.133)	(0.133)
	4	0.013	−0.277***	0.064	−0.290**	0.052	−0.341
		(0.084)	(0.094)	(0.225)	(0.120)	(0.237)	(0.241)
Observations / Individuals / Clusters		8,583 / 2,718 / 1,919					
Post-exit equality test (p-value)		0.168					
Number Series	−4	0.033	−0.123**	−0.214	−0.155*	−0.247	0.092
		(0.065)	(0.061)	(0.141)	(0.089)	(0.155)	(0.153)
	−2	—	—	—	—	—	—
	0	−0.046	0.021	0.183*	0.067	0.229**	−0.163
		(0.046)	(0.056)	(0.105)	(0.072)	(0.115)	(0.119)
	2	−0.019	−0.003	0.103	0.015	0.121	−0.106
		(0.054)	(0.069)	(0.163)	(0.087)	(0.171)	(0.176)
	4	−0.033	−0.014	0.116	0.019	0.149	−0.130
		(0.065)	(0.099)	(0.131)	(0.116)	(0.145)	(0.164)
Observations / Individuals / Clusters		15,924 / 4,211 / 2,413					
Post-exit equality test (p-value)		0.630					

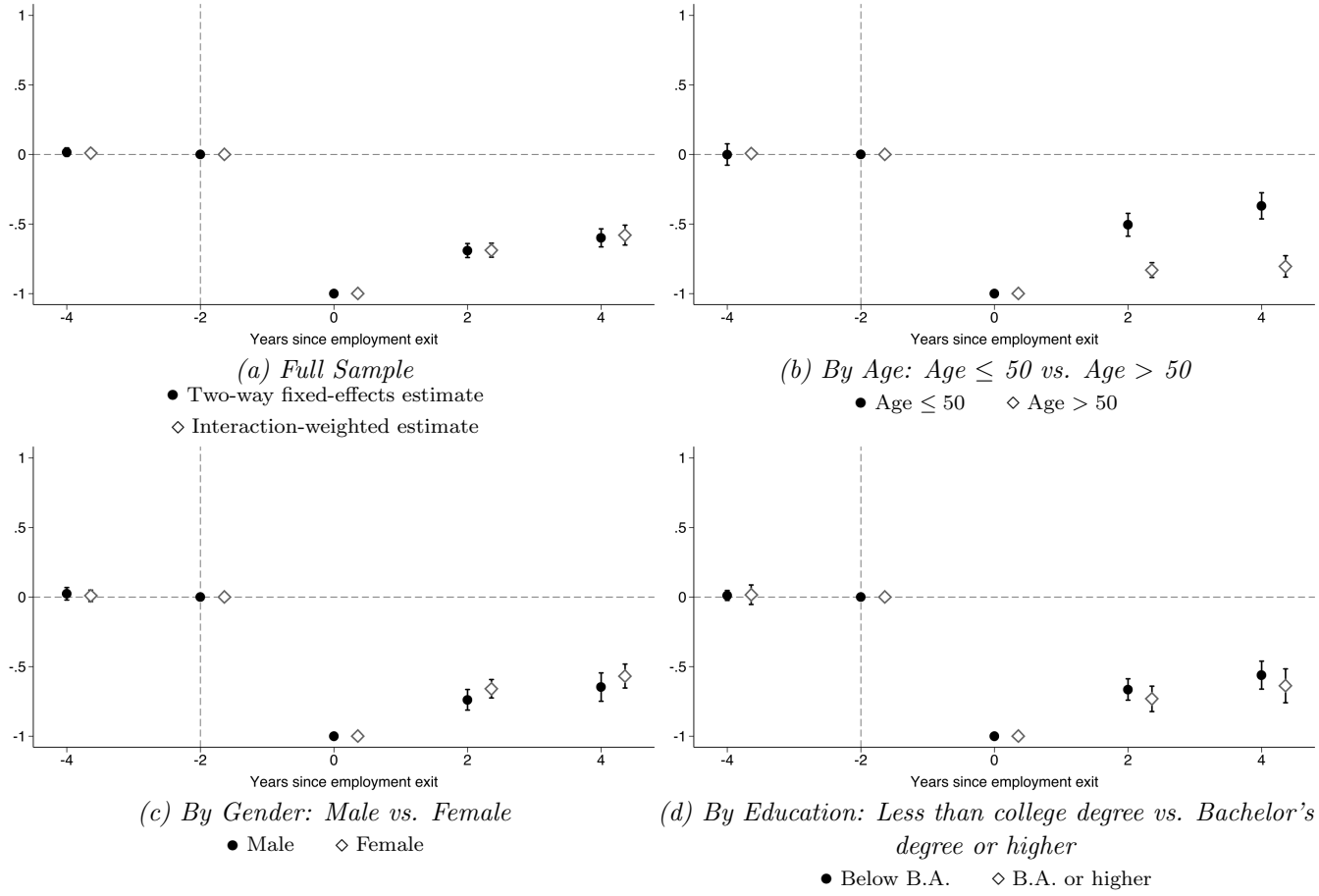
Continued on next page

Table A3 (continued)

Outcome	Years since employment exit	Exit-type effects: Unemployment	Retirement	Other nonemployment	Pairwise differences: Retirement − unemployment	Other − unemployment	Retirement − other
Picture Vocabulary	−4	−0.048	−0.034	0.102	0.015	0.150	−0.135
		(0.060)	(0.060)	(0.141)	(0.085)	(0.153)	(0.153)
	−2	—	—	—	—	—	—
	0	0.026	0.006	−0.052	−0.020	−0.077	0.057
		(0.046)	(0.055)	(0.100)	(0.072)	(0.110)	(0.114)
	2	−0.029	−0.016	−0.099	0.013	−0.070	0.083
		(0.053)	(0.055)	(0.111)	(0.075)	(0.122)	(0.123)
	4	0.004	0.015	−0.076	0.011	−0.080	0.091
		(0.062)	(0.069)	(0.140)	(0.090)	(0.152)	(0.155)
Observations / Individuals / Clusters		15,351 / 4,090 / 2,306					
Post-exit equality test (p-value)		0.990					
Verbal Analogies	−4	0.039	−0.062	0.229	−0.102	0.189	−0.291
		(0.075)	(0.073)	(0.184)	(0.106)	(0.201)	(0.198)
	−2	—	—	—	—	—	—
	0	−0.020	−0.063	−0.009	−0.043	0.010	−0.054
		(0.054)	(0.068)	(0.118)	(0.088)	(0.129)	(0.135)
	2	−0.007	0.034	0.059	0.041	0.065	−0.025
		(0.065)	(0.077)	(0.147)	(0.100)	(0.161)	(0.166)
	4	−0.086	−0.032	0.225	0.053	0.310*	−0.257
		(0.074)	(0.091)	(0.166)	(0.114)	(0.181)	(0.189)
Observations / Individuals / Clusters		14,773 / 3,932 / 2,269					
Post-exit equality test (p-value)		0.589					
Serial Sevens	−4	0.062	−0.239***	0.225	−0.301**	0.162	−0.463
		(0.114)	(0.092)	(0.333)	(0.146)	(0.352)	(0.342)
	−2	—	—	—	—	—	—
	0	0.038	−0.138	−0.238	−0.176	−0.276	0.100
		(0.099)	(0.100)	(0.245)	(0.142)	(0.264)	(0.260)
	2	−0.307**	−0.034	0.078	0.274	0.385	−0.112
		(0.135)	(0.150)	(0.293)	(0.200)	(0.324)	(0.294)
	4	−0.201	0.194	−0.101	0.396	0.100	0.296
		(0.152)	(0.193)	(0.431)	(0.243)	(0.452)	(0.468)
Observations / Individuals / Clusters		7,524 / 2,134 / 1,529					
Post-exit equality test (p-value)		0.187					

Notes: The first three columns report event-study estimates by exit type; the final three report pairwise differences. Unemployment includes layoffs, job search, and mixed statuses containing an unemployment component; retirement includes retirement and related nonworking statuses without an unemployment component; other nonemployment includes the remaining exits, such as leave and disability. The omitted period is one to two years before the employment exit. Specifications include the relevant lower-order terms, matched-record and calendar-year fixed effects, and matching weights; standard errors are clustered by respondent. The post-exit equality test jointly tests equality across the three exit types at event times 0, 2, and 4. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A1: Employment Dynamics following an Employment Exit



(a) Full Sample

● Two-way fixed-effects estimate
◇ Interaction-weighted estimate

(b) By Age: Age ≤ 50 vs. Age > 50

● Age ≤ 50 ◇ Age > 50

(c) By Gender: Male vs. Female

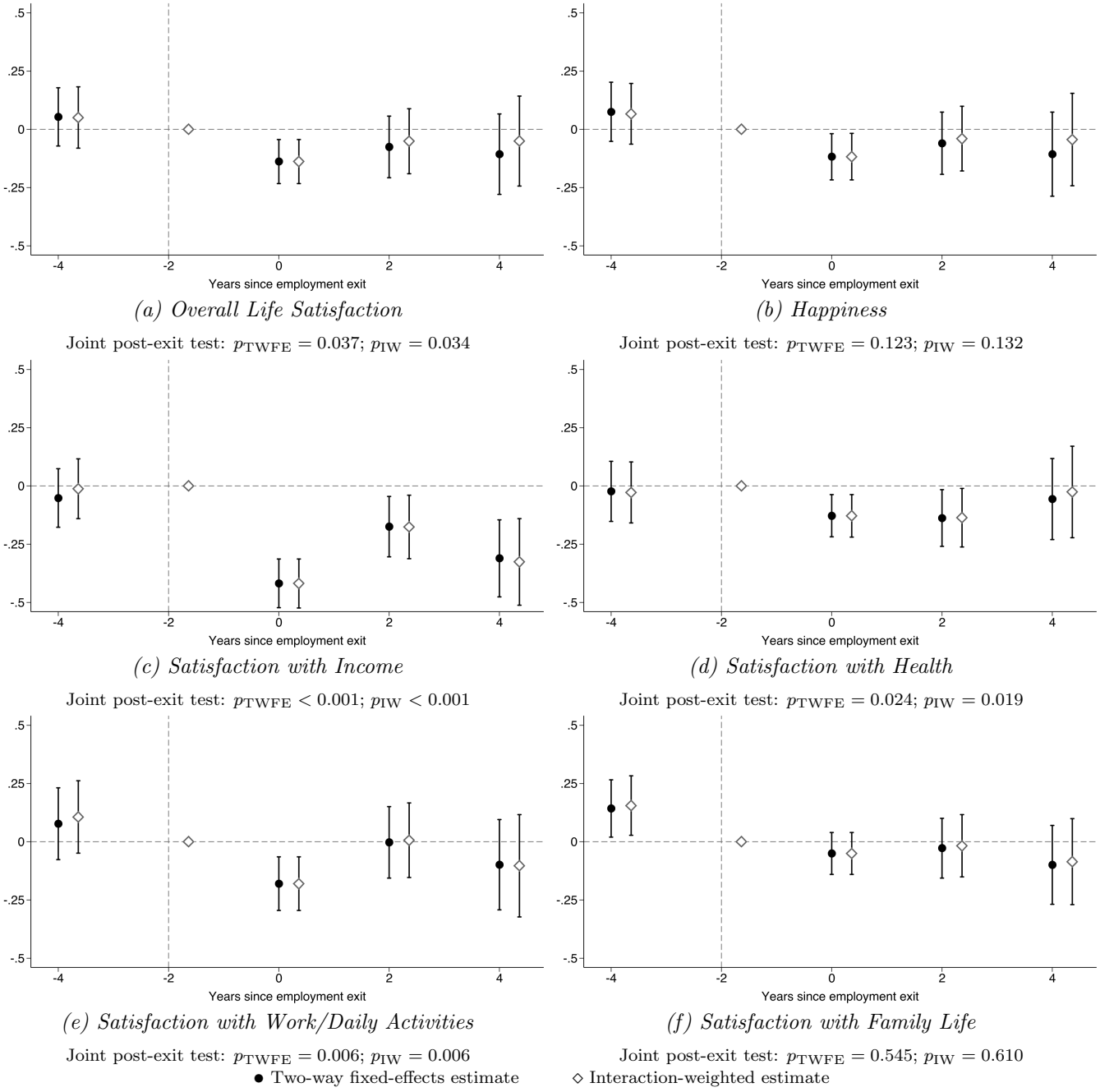
● Male ◇ Female

(d) By Education: Less than college degree vs. Bachelor's degree or higher

● Below B.A. ◇ B.A. or higher

Notes: The outcome is an indicator for employment. Panel (a) uses the financial-literacy matched sample; panels (b)–(d) report conventional estimates by age, gender, and education. The education panel uses the subgroup-rematched sample described in Table 3. The one-to-two-year pre-exit interval is omitted. All specifications use matching weights, matched-record and calendar-year fixed effects, and respondent-clustered standard errors. Vertical bars show 95 percent confidence intervals.

Figure A2: Subjective Well-Being around Employment Exits



(a) Overall Life Satisfaction

Joint post-exit test: $p_{\text{TWFE}} = 0.037$; $p_{\text{IW}} = 0.034$

(b) Happiness

Joint post-exit test: $p_{\text{TWFE}} = 0.123$; $p_{\text{IW}} = 0.132$

(c) Satisfaction with Income

Joint post-exit test: $p_{\text{TWFE}} < 0.001$; $p_{\text{IW}} < 0.001$

(d) Satisfaction with Health

Joint post-exit test: $p_{\text{TWFE}} = 0.024$; $p_{\text{IW}} = 0.019$

(e) Satisfaction with Work/Daily Activities

Joint post-exit test: $p_{\text{TWFE}} = 0.006$; $p_{\text{IW}} = 0.006$

(f) Satisfaction with Family Life

Joint post-exit test: $p_{\text{TWFE}} = 0.545$; $p_{\text{IW}} = 0.610$

● Two-way fixed-effects estimate ◇ Interaction-weighted estimate

Notes: Each panel reports estimates for one standardized subjective well-being outcome using the matched sample. The one-to-two-year pre-exit interval is omitted. Estimation, weighting, clustering, and plotting conventions follow Figure 1. Joint post-exit tests evaluate whether the coefficients at event times 0, 2, and 4 are jointly zero.