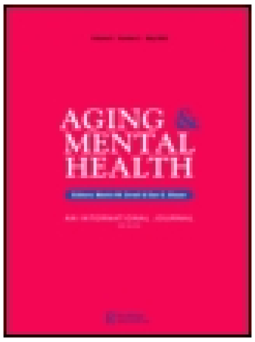

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One-year cognitive outcomes from a multiple real-world skill learning intervention with older adults

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ABSTRACT

Objectives: Novel skill learning has been shown to have cognitive benefits in the short-term (up to a few months). Two studies expanded on prior research by investigating whether learning multiple novel real-world skills simultaneously (e.g. Spanish, drawing, music composition), for a minimum of six hours a week, would yield 1-year cognitive gains.
Method: Following a 3-month multi-skill learning intervention, Study 1 ($N = 6$, $M_{age} = 66$ years, $SD_{age} = 6.41$) and Study 2 ($N = 27$, $M_{age} = 69$ years, $SD_{age} = 7.12$) participants completed follow-up cognitive assessments 3 months, 6 months, and one year after the intervention period. Cognitive assessments tested executive function (working memory and cognitive control) and verbal episodic memory.
Results: Linear mixed-effects models revealed improvements in multiple cognitive outcomes from before the intervention to the follow-up timepoints. Specifically, executive function increased from pre-test to the 1-year follow-up for both studies (an effect driven mostly by cognitive control scores).
Discussion: Our findings provide evidence that simultaneously learning real-world skills can lead to long-term improvements in cognition during older adulthood. Future work with diverse samples could investigate individual differences in gains. Overall, our findings promote the benefits of lifelong learning, namely, to improve cognitive abilities in older adulthood.

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Cognitive intervention; engagement; skill-learning; cognitive control; aging

Promising research over the past few decades has revealed that fluid cognitive abilities (e.g. working memory, cognitive control, episodic memory) can increase in older adulthood (see Hertzog et al., 2008; Nyberg & Pudas, 2019). In addition to cognitive training, which uses targeted computer tasks or strategy training for specific abilities (see Lampit et al., 2014 for a review), cognitive engagement interventions that use real-world skills such as photography (Noice & Noice, 2013; Park et al., 2014) have demonstrated cognitive gains in older adulthood. Cognitive training studies using computer tasks have demonstrated increases in trained abilities immediately following the intervention (i.e. Jaeggi et al., 2014 ; Kueider et al., 2012), although evidence of gains in non-trained abilities seems to be rare (see Simons et al., 2016). Only a handful of cognitive engagement interventions, where older adults are actively working with new materials and instructors, exist (e.g. Bugos et al., 2007; Chan et al., 2016; Leanos et al., 2020; Park et al., 2014; Stine-Morrow et al., 2008). Among these studies, the results generally support cognitive gains in fluid abilities as measured via computer tasks (although see Berggren et al., 2020), even though the engaged tasks were real-world skills, such as photography and piano playing.

Despite these encouraging findings, evidence of longer-term (at least one year) maintenance of, or improvements in, cognitive gains is rare. Only a small subset of cognitive training studies have investigated long-term intervention effects (Nguyen et al., 2019). The ACTIVE study is a landmark project that has demonstrated very long-term effects of cognitive training through two-, five-, and ten-year follow-ups (Ball et al., 2002; Rebok et al., 2014; Willis et al., 2006). The ACTIVE study placed participants into one of three cognitive training groups (memory, reasoning, and speed of processing); training was provided in ten sessions over a six-week period. Eleven months after completing the intervention, randomly selected participants received booster training consisting of four sessions over a three-week period. By the two-year follow-up, participants demonstrated overall maintenance of trained cognitive domains (Ball et al., 2002). By the ten-year follow-up, improvements in trained tasks were sustained in the reasoning and speed-of-processing groups, but not in the memory group (Rebok et al., 2014). Another cognitive training study (Chambon et al., 2014), which focused on episodic memory and attention, found that at the six-month follow-up, older adults maintained their episodic memory via free recall from post-test. However, maintenance of other trained abilities in this intervention (visual recognition, visuospatial recognition) was not observed. Chambon et al. (2014) posited that tasks with a high mental load (such as those for episodic memory) may be more likely to provide longer term benefits.

With the small number of real-world skill learning intervention studies compared to cognitive training studies, there are very few engagement interventions that have included follow-up periods. In one study, Bugos et al. (2007) found that three months after finishing personalized piano training, older adults

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continued to show increases in their working memory ability. Notably, the participants did not practice the exact memory tasks from the assessments in the skill learning intervention, and therefore these findings provide evidence for far transfer of cognitive abilities from a complex real-world skill to a pared-down task for an assessment. Additionally, a small subset of Synapse participants (Park et al., 2014) were selected to participate in fMRI scans at the intervention pre- and post-testing and for a one-year follow-up scan. Scans at the one-year follow-up timepoint revealed that the participants had maintained the improvements in the brain regions that had improved immediately after the skill learning intervention (McDonough et al., 2015).

Novel approach to cognitive engagement interventions

How might real-world skill learning promote long-term cognitive gains? A novel lifespan theory provides an approach for maximizing long-term cognitive gains in older adulthood, perhaps beyond what is currently known (Wu et al., 2017; Wu & Strickland-Hughes, 2019). This theory posits that providing older adults with rich learning environments akin to learning environments from childhood may yield considerable immediate and long-term cognitive gains. In contrast to practicing or training specific abilities using computer tasks or cognitive strategies, the theory proposes six key ingredients that allow learning experiences to promote cognitive growth: open-minded input-driven learning (e.g. learning completely new skills), individualized scaffolding (tailored help from instructors), growth mindset (belief that one's abilities can improve with effort), forgiving environment (being allowed to make mistakes, no negative stereotypes about novel learning), serious commitment to learning (e.g. spending several hours a week to learn difficult skills), and learning multiple skills simultaneously. These six factors may account for a portion of the considerable cognitive gains during infancy to young adulthood (i.e. the learning experiences themselves may be driving cognitive growth and development in these young learners). The rich learning environments from infancy to young adulthood including these factors typically dwindle after young adulthood (from the last formal year of education), perhaps making it more difficult for adults, especially older adults, to maintain or develop cognitive abilities.

Evidence for this theory thus far has largely been circumstantial. For example, learning experiences earlier in the lifespan in terms of education are one of the strongest predictors of cognitive outcomes in late life (e.g. Park et al., 2014; Ritchie & Tucker-Drob, 2018; Vemuri et al., 2014; although see Nyberg et al., 2021). Real-world skill-learning interventions with older adults typically include learning only one skill at a time (Bugos et al., 2007; Chan et al., 2016; Park et al., 2014), but studies that include some of the six factors have provided promising evidence to support the novel theory, although mostly in terms of short-term effects. If older adults are provided with aspects of the rich learning environment for skill learning afforded to children, would we observe cognitive gains over the long term?

Leanos et al. (2020) reported one of the first skill-learning interventions that included learning at least three new real-world skills simultaneously. As one of the first tests of the novel theory, Leanos et al. (2020) taught new skills, such as Spanish, drawing, and music composition, to community-dwelling healthy older adults (aged 55+) for several hours, multiple days a week, over a period of three months. Immediately following the end of the intervention, participants exhibited significant improvements in cognitive abilities: older adult participants' post-test cognitive scores were similar to a cross-sectional sample of middle-aged adults' baseline cognitive scores (M_{age} = 42.36, SD_{age} = 5.79). Although learning multiple skills concurrently promoted robust gains in cognitive abilities by post-test, it is unclear whether these improvements would be sustained over the long term (one year later). If engaging in intervention activities is important for maintaining intervention outcomes, the considerable time commitment needed to do so (Leanos et al. 2020 reported approximately 15 h per week) may not be sustainable.

The present study

The present study investigated whether learning multiple new real-world skills simultaneously would lead to long-term (one-year) gains in cognitive abilities. These include executive functions of cognitive control (one's ability to adapt behaviors to continuously changing environments or information; considered in this manuscript via inhibition and flexibility tasks explained below), working memory, and verbal episodic memory. Specifically, we predicted that overall cognitive composite scores, as well as the sub-components of the cognitive battery measuring working memory and cognitive control, would significantly improve compared to pre-test assessments for Study 1 and baseline assessments for Study 2, as described in Leanos et al. (2020). Regarding verbal episodic memory, we predicted that both studies would demonstrate improvements in the immediate list recall (RAVLT) measure for all three follow-ups compared to pre-test (Study 1) and baseline (Study 2) assessments. We predicted that Study 2 also would reveal improvements for the digit span task across the three follow-up timepoints compared to baseline. Cognitive abilities in two studies with older adults were assessed up to one year after completing the intense multi-skill learning intervention reported in Leanos et al. (2020). The first study included a feasibility sample, and the second study included a larger sample aimed to replicate the pattern of findings from the feasibility sample. Long-term gains would indicate the potential for cognitive growth in older adulthood, perhaps in some ways like cognitive growth observed earlier in the lifespan within rich learning environments.

Method

Participants

This study received ethical approval by the Institutional Review Board at the University of California, Riverside (IRB protocol number HS-17-211). All participants provided written informed consent prior to their participation at the first assessment timepoint. This consent process was conducted with a trained member of the research team and explained voluntary participation, confidentiality and privacy, risks and benefits, results communication, and general procedures of the study. Participants were provided with a copy of their signed consent form.

We conducted two separate studies with older adults: Intervention Study 1 included six participants (67% female, M_{age} = 66.33 years, SD_{age} = 6.41, Mdn_{age} = 68.5, range = 58–74 years old at pre-test), and Intervention Study 2 included 27 participants (67% female, M_{age} = 69.44 years, SD_{age}= 7.12, Mdn_{age} = 69, range = 58–86 years old at baseline) (see Figure 1 for recruitment and attrition). Table 1 details the demographic

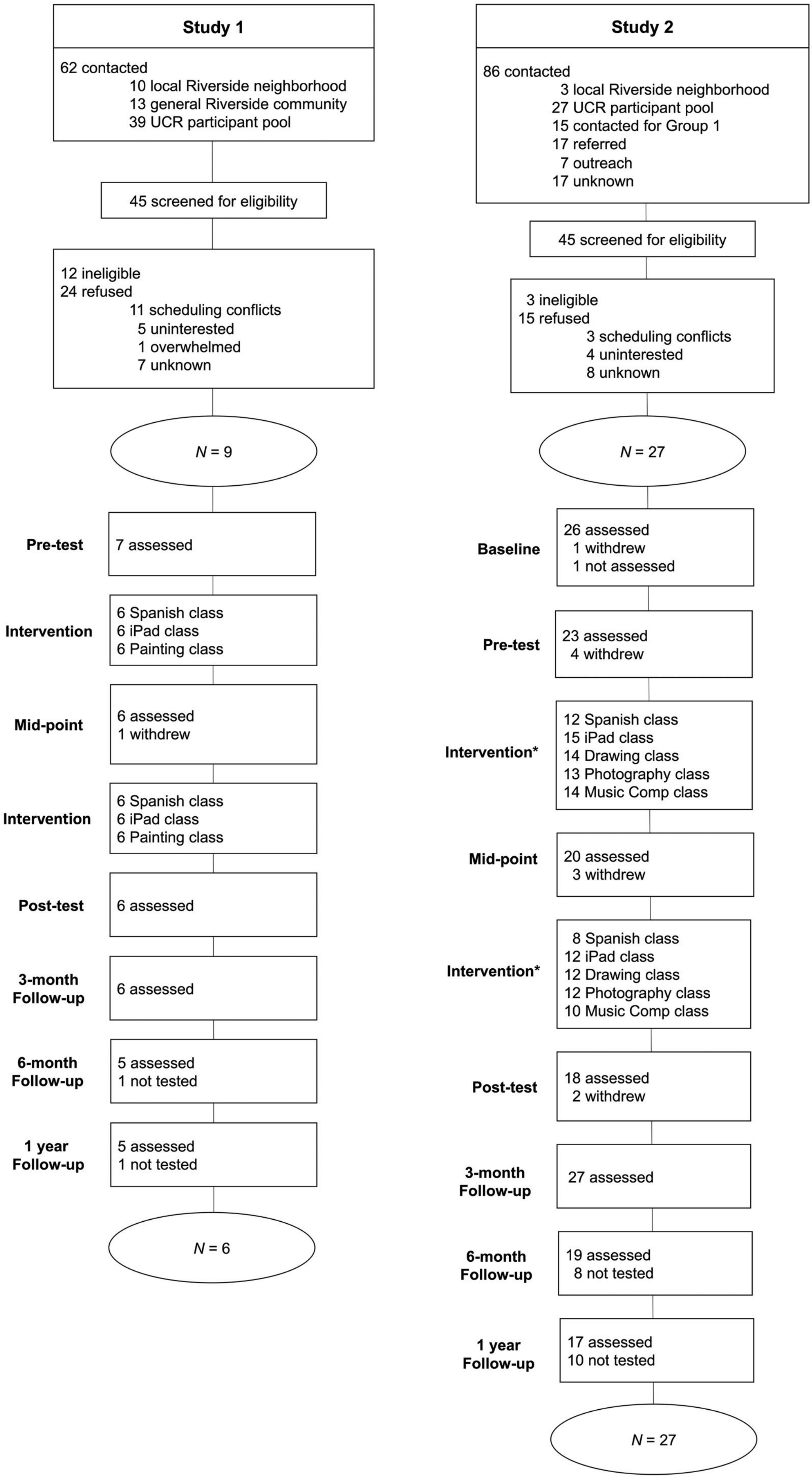

Figure 1. Recruitment and attrition flow chart for Study 1 and Study 2.

Table 1. Study 1 and Study 2 follow-up characteristics.

Characteristic	Group 1, $N=6$	Group 2, $N=27$
Age $M \pm SD$ (range)	66.3 ± 6.4 (58-74)	69.4 ± 7.1 (58-86)
Female, N (%)	4 (66.7)	18 (66.7)
Race, N (%)		
White	5 (83.3)	18 (66.7)
Black	1 (16.7)	4 (14.8)
Asian	0 (0.0)	1 (3.7)
Multiracial or Other	0 (0.0)	4 (14.8)
Ethnicity, N (%)		
Hispanic	0 (0.0)	3 (11.1)
Non-Hispanic	6 (100)	24 (88.9)
Years of education, $M \pm SD$ (range)	16.5 ± 3.6 (14-23)	15.6 ± 2.9 (12-20)
Retired, N (%)	5 (83.3)	22 (81.5)
Marital status (%)		
Married or have a partner	3 (50.0)	19 (70.4)
Widowed	1 (16.7)	5 (18.5)
Separated or divorced	1 (16.7)	1 (3.7)
Never married	1 (16.7)	1 (3.7)
Prefer not to answer	0 (0.0)	1 (3.7)
Living arrangement (%)		
Live alone	2 (33.3)	1 (3.7)
Live with spouse/partner	3 (50.0)	20 (74.07)
Live with other family	0 (0.0)	1 (3.7)
Live with someone else	1 (16.7)	1 (3.7)
Income (%)		
Less than $20,000	1 (16.7)	1 (3.7)
$20,000 to $29,999	1 (16.7)	3 (11.1)
$30,000 to $39,999	0 (0.0)	1 (3.7)
$40,000 to $49,999	0 (0.0)	1 (3.7)
$50,000 to $99,999	2 (33.3)	11 (40.7)
$100,000 to $199,999	2 (33.3)	4 (14.8)
$200,000 and over	0 (0.0)	1 (3.7)
MMSE score ± SD (range)	28.3 ± 2.3 (25-32)	26.5 ± 3.2 (19-30)

Note. MMSE = Mini-Mental State Exam. Minimum cut off score for MMSE was 19.

information from these two studies. Participants were recruited from the community via an existing aging database of potential participants, neighborhood online message boards, local community programs, and word of mouth. Inclusion criteria were as follows: 55+ years of age, fluent in English, had normal or corrected-to-normal vision, and self-reported no diagnostic history of a cognitive condition (e.g. mild cognitive impairment). All participants (Studies 1 and 2) were compensated $40 for each assessment session and were able to retain all supplies (apart from iPads, which were university property) provided to them from the classes, such as notebooks and writing utensils, sketchbooks, and art supplies.

Study design

Participants in Study 1 (feasibility sample) all learned the same three skills (Spanish, iPad operation, and painting) over a 15-week period. Weekly training included 2-hour classes for each skill and an additional 1-hour lecture/discussion session which covered topics such as motivation, growth mindset, barriers to learning, and successful aging. Attendance and hours involved in intervention-related activities (i.e. classes and homework) were tracked for analysis purposes. Cognitive assessments were administered at pre-test (start of the intervention, week 0), mid-point (week 7–8 of the intervention), post-test (after completion of the intervention classes—week 15), and 3-month, 6-month, and 1-year follow-ups.

The design and procedure for Study 2 was largely like Study 1 with a few differences. Study 2 participants had 12 weeks of classes (due to increased absences in the last three weeks of the intervention for Study 1). Additionally, to maintain small class sizes (under 20 students), Study 2 participants were assigned to three out of five possible classes (Spanish, photography, iPad operation, drawing, and music composition; class size range: 15–19 students, $M = 17.4$) based on experience level (i.e. assigned to classes to which they were naïve). To minimize attrition, participants were allowed to enroll in more than the three assigned classes if they were interested. Five participants enrolled in four classes, and three participants enrolled in all five classes. Classes were scheduled in the same 2-hour structure as Study 1 and included the 1-hour lecture/discussions on motivation and successful aging. Participants therefore completed a minimum of six hours of classes a week plus the 1-hour discussion. Study 2 included the same cognitive assessment periods as Study 1: pre-test (start of intervention—week 0), mid-point (week 6), post-test (after completion of the intervention—week 12), and 3-month, 6-month, 1-year follow-ups. In addition, to measure testing effects or changes in performance unrelated to the intervention, a baseline assessment was administered 6 weeks prior to the pre-test assessment. The procedure and assessments for Study 1 were pre-registered on ClinicalTrials.gov (Protocol Record 1320181), and for Study 2 the procedures were pre-registered on the Open Source Framework via aspredicted.org (https://osf.io/3ehtq).

Assessment measures

The cognitive assessments consisted of tasks that measured executive function (cognitive control/inhibition and working memory) and verbal episodic memory. The executive function tasks were from a standard battery (NIH Examiner; Kramer et al., 2014), and included flanker and set-shifting (cognitive control/inhibition), and n-back and dot counting (working memory; Study 1 completed 1-Back, Study 2 completed 1-Back and 2-Back). The tasks were presented on a 19-inch computer monitor and administered via PsychoPy (version 7.1). Overall composite and sub-component scores were compiled from reaction times and accuracy scores, apart from dot counting, which was a verbal task, and therefore only had accuracy scores. Overall composite and sub-component scores for working memory and cognitive control were standardized and computed using the R script provided by the EXAMINER development team (Kramer et al., 2014).

The verbal episodic memory tasks included the Rey Auditory Verbal Learning Task (RAVLT; Schmidt, 1996), and the WAIS-III (Ryan & Lopez, 2001) Digit Span forwards and backwards tasks. Participants were prompted five times with the same RAVLT word lists, at a rate of one word per second. Participants were given 60 s to recall as many words from the list as possible. After a distractor word list, there was an immediate sixth recall trial, in which participants were asked to recall as many words as they could remember without hearing the list again. Trial responses were scored for each correct word, excluding duplicates (i.e. a perfect score was 15 for a single trial). The six trial scores were summed and then averaged for the overall RAVLT score.

For the Digit Span task, experimenters verbally presented digits at a rate of one number per second. Participants were then asked to repeat the numbers back to the examiner in the correct order for the forward task, and in reverse sequence for the backward task. Sequence levels were in trial pairs. The length of the number sequences increased by one number with each successive trial pair until the participant incorrectly recalled two consecutive sequences of the same length. A perfect score for the forward version was 16, and 14 was a perfect score for the backward version. Forward and backward scores were summed for a total score (out of a possible 30).

For both tasks participants' responses were digitally recorded to code and score after the assessment. RAVLT and Digit Span scores were analyzed separately.

The full assessments lasted 1.5 to 2 h, depending on participants' pacing (breaks, practice blocks, etc.).

Engagement

Participants' hours of attendance (total class time in hours; experimenter recorded) and number of hours spent on homework (self-reported) during the intervention period were summed into an 'hours of engagement' measure (similar to Park et al., 2014) to measure engagement in the intervention activities.

Results

The primary goal of the follow-up assessments was to determine if gains in cognitive abilities (executive function and verbal episodic memory) would continue up to one year after the intervention. Analyses were conducted for overall executive function, as well as for the separate sub-components (cognitive control and working memory), in addition to verbal episodic memory.

To analyze performance on the cognitive assessments over multiple timepoints for each study, we employed separate linear mixed-effects models for each outcome variable: executive function cognitive composite scores (a combination of accuracy scores and reaction times from the four cognitive tasks), cognitive control scores (sub-component of the composite scores), and working memory scores (sub-component of the composite scores) from the EXAMINER battery, RAVLT mean scores, and Digit Span total scores. This analytic approach accounted for the dependence among repeated measurements for each participant. Demographic variables (sex, retirement status, age, years of education, race, marital status, and living arrangement) were entered as predictors to control for their potential effects. The models included fixed effects (i.e. population-level effects) and random effects (i.e. subject-level effects) to determine whether simultaneously learning multiple novel skills would increase cognitive outcomes from initial testing scores (i.e. baseline or pre-test) prior to starting the learning intervention. Time was included as a categorical variable to allow for inconstant changes in responses between timepoints. Starting with the model that had the highest level of interaction among all predictors (time, hours engaging in intervention, sex, retirement status, age, years of education, race, marital status, and living arrangement), predictors were systematically removed to identify the model with the smallest Akaike Information Criterion (AIC). The remaining predictors for each analysis are indicated in the appropriate sections that follow. Means and standard deviations of each outcome variable for all timepoints are reported in Table 2. Table 3 displays the results from the linear mixed-effects model for the cognitive composite scores for both Study 1 and Study 2 (for additional cognitive outcome results, see supplemental materials). The first cognitive assessment timepoint (pre-test for Study 1; baseline for Study 2) was the reference point for the subsequent timepoints in these models.

In addition to the linear mixed effect models, we report the estimated differences between each timepoint for each outcome variable, obtained by conducting a Wald test (an asymptotic Chi-square test with one degree of freedom). All values presented are based on estimated coefficients (Table 3). Only significant (α = .05) and marginally significant (α = .10; 90% CI) effects are reported for all outcome variables for brevity and transparency.

Cognitive outcomes

Executive function cognitive composite score (NIH EXAMINER battery)

Figure 2 depicts the executive function cognitive composite scores for Studies 1 and 2. To facilitate interpretability of the intervention participant composite scores over time, the mean

Table 2. Mean and SE of the cognitive composite, cognitive control, working memory, RAVLT, and digit span scores from Study 1 and Study 2.

	Outcomes	Baseline	Pre-test	Midpoint	Post-test	3-month follow-up	6-month follow-up	1-year follow-up
Study 1	Cognitive composite	n/a	0.41 (0.18)	0.77 (0.17)	0.79 (0.20)	0.82 (0.27)	0.89 (0.12)	1.00 (0.20)
	Cognitive control	n/a	0.23 (0.17)	0.49 (0.21)	0.60 (0.24)	0.55 (0.27)	0.71 (0.20)	1.00 (0.22)
	Working memory	n/a	0.30 (0.09)	0.76 (0.23)	0.38 (0.12)	0.48 (0.20)	0.32 (0.13)	0.34 (0.22)
	RAVLT	n/a	8.63 (0.43)	9.40 (0.59)	9.50 (0.58)	11.10 (1.12)	12.13 (1.36)	9.58 (0.25)
	Digit Span	n/a	n/a	n/a	n/a	17.20 (1.28)	18.00 (0.77)	20 (1.73)
Study 2	Cognitive composite	0.61 (0.15)	0.80 (0.13)	1.06 (0.12)	0.90 (0.13)	0.97 (0.17)	1.15 (0.17)	1.33 (0.14)
	Cognitive control	0.48 (0.12)	0.53 (0.11)	0.81 (0.10)	0.69 (0.12)	0.83 (0.19)	0.93 (0.14)	1.12 (0.15)
	Working memory	0.04 (0.15)	0.34 (0.14)	0.72 (0.14)	0.61 (0.12)	0.55 (0.17)	0.71 (0.21)	0.90 (0.21)
	RAVLT	7.67 (0.40)	8.47 (0.39)	9.62 (0.47)	10.50 (0.45)	12.57 (0.50)	12.67 (0.62)	12.13 (0.54)
	Digit span	19.46 (0.79)	19.65 (1.01)	20.05 (0.85)	19.83 (0.88)	21.93 (1.21)	21.14 (1.24)	21.92 (0.73)

Note. n/a = not applicable; RAVLT = Rey Auditory Verbal Learning Test. *SE* reported in parentheses.

Table 3. Results of the mixed-effects model for the cognitive composite scores from Study 1 and Study 2.

Outcome	Predictor	Estimate	*SE*	95% CI	df	Unadjusted *p*-value
Study 1						
Cognitive composite scores	Midpoint	0.36	0.17	(0.01, 0.71)	21	.042*
	Post-test	0.39	0.19	(−0.00, 0.78) *90% CI: (0.07, 0.71)*	21	*.051*
	3-month follow-up	0.40	0.22	(−0.06, 0.86) *90% CI: (0.02, 0.79)*	21	*.085*
	6-month follow-up	0.47	0.22	(0.01, 0.94)	21	.046*
	1-year follow-up	0.40	0.18	(0.02, 0.78)	21	.041*
	Sex	−0.29	0.39	(−1.36, 0.79)	4	.499
Study 2						
Cognitive composite scores	Pre-test	0.19	0.11	(−0.02, 0.40) *90% CI: (0.01, 0.37)*	96	*.078*
	Midpoint	0.33	0.09	(0.15, 0.50)	96	<.001*
	Post-test	0.26	0.10	(0.05, 0.46)	96	.014*
	3-month follow-up	0.36	0.09	(0.18, 0.54)	96	<.001*
	6-month follow-up	0.51	0.11	(0.29, 0.73)	96	<.001*
	1-year follow-up	0.59	0.10	(0.39, 0.78)	96	<.001*
	Sex	−0.37	0.23	(−0.85, 0.11)	25	.126

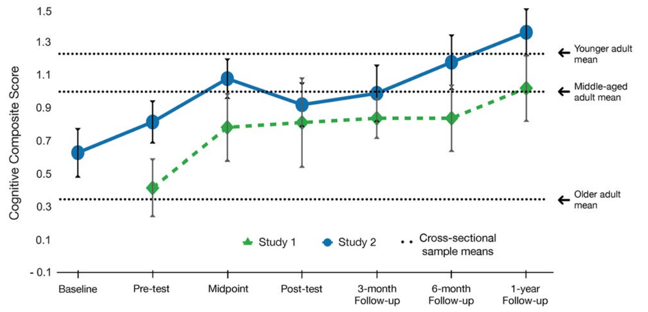

Figure 2. Cognitive composite scores from Study 1 and Study 2.
The dotted lines represent the means of cross-sectional convenience samples of younger ($n = 28$, $M_{age} = 19.07$ years, $SD_{age} = 1.05$, range: 18–22), middle-aged ($n = 22$, $M_{age} = 42.36$ years, $SD_{age} = 5.79$, range: 35–51), and older adults ($n = 43$, $M_{age} = 70.17$ years, $SD_{age} = 9.34$, range: 53–89). The younger adult mean score was 1.21, the middle-aged adult mean score was 0.98, and the older adult mean score was 0.33. Error bars represent ±1 SE.

composite scores of cross-sectional samples of younger (M_{age}= 19.07), middle-aged (M_{age}= 42.36), and older adults (M_{age}= 70.17) who did not participate in a learning intervention are included in this figure (for a detailed description, see Leanos et al., 2020).

Study 1. For the executive function cognitive composite scores for Study 1, the 6-month follow-up scores were significantly greater than the pre-test scores (increase of 0.47 units, p = .046, 95% CI [0.01, 0.94]). The cognitive composite scores for the 1-year follow-up were also significantly higher than the pre-test scores (increase of 0.40 units, p = .041, 95% CI [0.02, 0.78]). Three-month follow-up scores also demonstrated marginally significant improvement compared to pre-test scores (increase of .40 units, p = .085, 90% CI [0.02, 0.79]).

Study 2. Scores at the 3-month follow-up for Study 2 were significantly higher than initial baseline scores (increase of 0.36 units, p < .001, 95% CI [0.18, 0.54]) and marginally higher than the pre-test assessment as well (increase of 0.17 units, p = .055, 90% CI [0.02, 0.31]). Scores at the 6-month follow-up were significantly higher than those from baseline (increase of 0.51 units, p < .001, 95% CI [0.29, 0.73]), pre-test (increase of 0.32 units, p = .003, 95% CI [0.11, 0.53]), midpoint (increase of 0.18 units, p = .043, 95% CI [0.01, 0.36]), and immediate post-test (increase of 0.21 units, p = .016, 95% CI [0.04, 0.37]). Furthermore, 6-month follow-up scores were marginally greater than those at the 6-month follow-up (increase of 0.15 units, p = .093, 90% CI [0.00, 0.30]). The 1-year follow-up scores were also significantly higher than the baseline assessment scores (increase of 0.59 units, p < .001, 95% CI [0.39, 0.78]), pre-test (increase of 0.40 units, p < .001, 95% CI [0.21, 0.58]), midpoint (increase of 0.26 units, p = .001, 95% CI [0.11, 0.40]), post-test (increase of 0.33 units, p < .001, 95% CI [0.16, 0.50]), and the 3-month follow-up (increase of 0.23 units, p = .002, 95% CI [0.08, 0.37]). These results suggest long-term improvement in cognitive scores (beyond maintenance), with a pattern of higher scores at 6-months and 1-year than most of the earlier timepoints.

Cognitive control (EXAMINER sub-component)

The executive function cognitive composite score was split into two sub-components to investigate the independent effects of cognitive control and working memory, following Leanos et al. (2020).

Study 1. For Study 1, cognitive control scores at 6-month follow up were significantly higher than pre-test (increase of 0.44 units, p = .002, 95% CI [0.19, 0.70]) and midpoint (increase of 0.18 units, p = .001, 95% CI [0.07, 0.29]). The 1-year follow-up was also significantly higher than pre-test (increase of 0.56 units, p < .001, 95% CI [0.31, 0.81]), midpoint (increase of 0.30 units, p < .001, 95% CI [0.19, 0.41]), and post-test (increase of 0.20 units, p = .025, 95% CI [0.02, 0.37]). Scores from the 3-month follow-up to the 1-year follow-up marginally increased by 0.27 units (p = .056, 90% CI [0.04, 0.51]).

Study 2. Cognitive control scores for Study 2 showed a significant increase for the 3-month follow-up from baseline (increase of 0.37 units, p < .001, 95% CI [0.21, 0.53]), pre-test (increase of 0.32 units, p < .001, 95% CI [0.14, 0.50]), and post-test (increase of 0.18 units, p = .036, 95% CI [0.01, 0.36]). Three-month follow-up scores were marginally greater than midpoint (increase of 0.14 units, p = .080, 90% CI [0.01, 0.28]). The 6-month follow-up scores increased significantly from baseline (increase of 0.44 units, p < .001, 95% CI [0.31, 0.57]), pre-test (increase of 0.40 units, p < .001, 95% CI [0.25, 0.55]), midpoint (increase of 0.21 units, p = .001, 95% CI [0.09, 0.34]), and post-test (increase of 0.26 units, p < .001, 95% CI [0.11, 0.40]). Scores for the 1-year follow-up were significantly greater than baseline (increase of 0.55 units, p < .001, 95% CI [0.38, 0.72]), pre-test (increase of 0.51 units, p < .001, 95% CI [0.32, 0.70]), midpoint (increase of 0.33 units, p < .001, 95% CI [0.16, 0.49]), post-test (increase of 0.37 units, p < .001, 95% CI [0.19, 0.55]), and 3-month follow-up (increase of 0.18 units, p = .030, 95% CI [0.02, 0.35]).

Working memory (EXAMINER sub-measure)

Study 1. No significant results were reported in the working memory component of the EXAMINER assessment for the follow-up time points compared to earlier timepoints for Study 1 (all ps > .10).

Study 2. Working memory scores for Study 2 increased significantly at the 3-month follow-up compared to baseline (increase of 0.49 units, p = .001, 95% CI [0.22, 0.75]) and pre-test scores (increase of 0.20 units, p = .048, 95% CI [0.00, 0.40]). Six-month follow-up scores were significantly higher than baseline (increase of 0.58 units, p = .001, 95% CI [0.24, 0.91]) and pre-test (increase of 0.29 units, p = .041, 95% CI [0.01, 0.57]). By the 1-year follow-up, working memory scores had increased significantly compared to baseline (increase of 0.75 units, p < .001, 95% CI [0.42, 1.09]), pre-test (increase of 0.47 units, p = .001, 95% CI [0.19, 0.75]), and post-test (increase of 0.28 units, p = .044, 95% CI [0.01, 0.55]). Additionally, scores from the 3-month follow-up to the 1-year follow-up were marginally higher (increase of 0.27 units, p = .064, 90% CI [0.03, 0.51]). Sex had a significant effect on working memory scores, such that females were estimated to be significantly less by 0.39 units compared to males (p = .044, 95% CI [−0.77, −0.02]). In addition, as the total hours of engagement for the intervention increased by one hour, the working memory score was estimated to significantly increase by 0.003 units (p = .001, 95% CI [0.00, 0.01]).

Verbal episodic memory: RAVLT

Study 1. For Study 1, the average RAVLT scores at the 3-month follow-up were significantly greater than those at pre-test (increase of 2.14 words, p = .042, 95% CI [0.08, 4.20]). By the

6-month follow-up, scores had significantly increased from pre-test (increase of 3.17 words, $p = .004$, 95% CI [1.11, 5.23]) and post-test (increase of 2.05 words, $p = .028$, 95% CI [0.23, 3.88]). RAVLT scores were marginally greater at the 6-month follow-up than at midpoint (increase of 1.62 words, $p = .082$, 90% CI [0.09, 3.15]). Sex had a marginal effect on RAVLT scores; the average RAVLT score for a female participant was estimated to be larger than that of a male participant by 2.17 words ($p = .065$, 90% CI [0.34, 3.99]).

Study 2. For Study 2, RAVLT scores were significantly greater at the 3-month follow-up than at baseline (increase of 3.73 words, $p < .001$, 95% CI [3.02, 4.45]), pre-test (increase of 3.30 words, $p < .001$, 95% CI [2.57, 4.03]), midpoint (increase of 2.14 words, $p < .001$, 95% CI [1.42, 2.86]), and post-test (increase of 1.17 words, $p < .001$, 95% CI [0.53, 1.80]). By the 6-month follow-up, average RAVLT scores had significantly increased from baseline (increase of 3.27 words, $p < .001$, 95% CI [2.44, 4.09]) and pre-test (increase of 2.83 words, $p < .001$, 95% CI [2.00, 3.66]), and midpoint (increase of 1.68 words, $p < .001$, 95% CI [0.86, 2.49]). In addition, 6-month follow-up scores were marginally greater than those at post-test (increase of 0.70 words, $p = .063$, 90% CI [0.08, 1.32]). Average RAVLT scores at the 1-year follow-up were significantly greater than baseline (increase of 2.76 words, $p < .001$, 95% CI [.95, 3.56]), pre-test (increase of 2.32 words, $p < .001$, 95% CI [1.50, 3.14]), and midpoint (increase of 1.17 words, $p = .004$, 95% CI [0.37, 1.97]). However, there was a significant decrease in average RAVLT scores from the 3-month follow-up to the 1-year time point (decrease of 0.98 words, $p = .022$, 95% CI [−1.82, −0.14]). There was a marginally significant relationship between hours spent on intervention activities and RAVLT scores (0.006 words per hour, $p = .078$, 90% CI [0.00, 0.01]).

Verbal episodic memory: Digit Span

Study 1. For Study 1, the Digit Span verbal episodic memory test was not introduced until the 3-month follow-up. Therefore, analyses were conducted comparing the subsequent follow-up assessments (6-month and 1-year) to the 3-month follow-up. Using the 3-month follow-up as the control, there were no significant differences in results for Study 1 on the Digit Span task.

Study 2. Scores for the Digit Span task for Study 2 were significantly higher at the 3-month follow-up than at baseline (increase of 2.17 digits, $p = .001$, 95% CI [0.94, 3.41]), pre-test (increase of 1.84 digits, $p = .009$, 95% CI [0.46, 3.22]), midpoint (increase of 1.43 digits, $p = .008$, 95% CI [0.37, 2.49]), and post-test (increase of 1.87 digits, $p < .001$, 95% CI [0.86, 2.88]). There was also a marginal increase at the 1-year follow-up compared to baseline (increase of 1.63 digits, $p = .075$, 95% CI [−0.17, 3.43]).

In addition, time spent on intervention-related work was a significant predictor of Digit Span scores, such that as intervention time increased by one hour, the Digit Span score was estimated to increase by .02 digits ($p = .028$, 95% CI [0.00, 0.03]).

Discussion

The present study investigated the long-term (one-year) cognitive effects in older adults after simultaneously learning at least three new real-world skills for three months in two separate intervention studies. Linear mixed-effects models for both Studies 1 and 2 revealed that older adults continued to increase their cognitive abilities even after one year from the end of the intervention. Compared to pre-test scores, Study 1 had significant increases in cognitive composite scores, driven by cognitive control, as well as increases in verbal episodic memory (RAVLT) by the 6-month follow-up. For Study 2, participants improved in all measures across all three follow-up periods compared to baseline assessments, apart from Digit Span scores. Overall, these findings supported most of our hypotheses and indicate that a multi-skill learning intervention has the potential to induce long-lasting cognitive improvements in older adults.

Our findings are atypical compared to prior research, although they were predicted based on our lifespan theoretical framework (Wu et al., 2017; Wu & Strickland-Hughes, 2019). Most cognitive interventions thus far with follow-up assessments have shown dissipation of effects over time, and often over short-term follow-ups, within 3 or 6 months of completing the interventions (Bugos et al., 2007; Kurita et al., 2019; Rahe et al., 2015). By contrast, the present study revealed significant improvements up to a year following the end of the learning intervention, which is similar to the fMRI follow-up reported by McDonough et al. (2015). Prior studies also found a significant relationship between time spent on activities and the level of cognitive improvements (Bugos et al., 2007; Kurita et al., 2019; Park et al., 2014; Rahe et al., 2015), which aligns with some of our present findings, namely with working memory and verbal episodic memory.

What might account for the long-term cognitive improvements that seem so rare in the cognitive intervention literature? One possibility is the fact that the participants learned at least three novel skills. The time and energy commitment to do so was similar to a full undergraduate course load. Comparing across studies (e.g. compared to Park et al., 2014), it may be the case that learning three skills may lead to greater cognitive gains than learning one skill. However, it remains to be tested whether frequency or variety of skill learning, or both, drive significant cognitive gains in older adulthood (see Bielak et al., 2019). Further investigation into the activities of participants following the conclusion of the learning intervention (and completion of post-test assessments) is also needed to understand if participants continued practicing skills learned in the study, learning new skills on their own, or took up other activities that could improve cognition. Anecdotally, some have reported learning for fun, picking up new skills that they always wanted to learn, such as playing the guitar, while others learned skills out of necessity, such as how to fix their own toilets or give themselves manicures during the pandemic when services were limited. One even reported gaining the confidence to enroll in classes to complete their undergraduate degree. However, this information was only provided casually and not measured. Future research should measure the activities participants engage in over the long term after the end of the intervention to increase their cognitive abilities.

In addition to the direct cognitive benefits of skill learning, our intervention also included a strong social component that could have contributed to our results, even if indirectly. The bonds and communication that continued during and outside of the study could have played an important role in participants' continued improvement, such as learning from peers and holding each other accountable (Bandura, 1986; Seeman et al., 2001; Sharifian et al., 2019; Zahodne et al., 2019). Prior studies that isolated social interaction to test its effect on cognitive abilities found that social interaction by itself did not change cognitive function (Park et al., 2014). Future research could investigate the enhancing role that social interaction plays in novel learning situations to help sustain cognitive growth in older adulthood. In turn, future work could also include measurements assessing changes in mood as a potential mediator of improved cognitive abilities (e.g. Baune et al., 2006).

Furthermore, our intervention included novel skill learning, motivational lectures, and peer social support, as we originally intended to closely mimic rich, encouraging learning environments provided to children, adolescents, and young adults. By including multiple factors for a rich learning environment, the exact cause (i.e. active ingredient) of the overall intervention effect is unclear from our multifactorial design. Given our present findings, future work can investigate these specific factors individually through control conditions and comparison studies that include some but not all the potential 'active ingredients' of the intervention (Rebok et al., 2007). However, doing so may fundamentally alter the learning experience and make the conditions difficult to compare, similar to comparing children who are homeschooled versus those who attend public school.

Although the results from the present study are encouraging, we note some important limitations. First, our samples were relatively small and generally lacked diversity (predominately non-Hispanic, White, female with more than a high school education for both studies). This issue leads to questions about the generalizability of our findings, specifically with regards to older adults in the US, as well as internationally. Future cognitive interventions could include a more diverse sample (see Tzuang et al., 2018), to account for individual differences, such as different levels of stress (e.g. based on racial stereotypes, income, etc.) and prior learning experiences. Our study did include participants reporting different income levels, and to some degree, different racial and ethnic backgrounds. However, our small sample sizes prevented us from disaggregating the sample to further investigate potential differences based on these factors. Despite our relatively small sample sizes, the findings from the second study were consistent with the pattern of results found in the first study, providing support for our overall findings (Rosenthal, 1990). More information related to individual differences in intervention outcomes would allow tailored intervention designs for targeted populations, such as for the most vulnerable older adults (e.g. disabled, cognitively impaired, low-income). Indeed, such a time and energy intensive intervention may not be feasible, especially for some vulnerable older adults. Therefore, future work with tailored interventions would be important for encouraging optimal cognitive growth with older adults with different needs.

Additionally, there are minor differences in the significance of the outcomes for the cognitive composite scores at different timepoints in the two studies (Table 3). There are several potential reasons for these differences, such as the design changes between Study 1 and Study 2, as well as the minor differences between study samples for age range, cognitive status (MMSE scores), and educational attainment. Future studies should include larger samples to stratify based on these factors to investigate how they may impact cognitive outcomes during learning interventions.

We also note that a portion of our effects may be due to testing or practice effects from repeated assessments. In the present study, Study 2 included a baseline measure prior to the start of the intervention to investigate any changes not related to the intervention after one time point. Ideally, we would have been able to include a no-contact control group (or a waitlist control group) for the long-term measures. Leanos et al. (2020) included a no-contact control group until post-test alongside Intervention Study 1. Given the promising results from Study 1, for ethical reasons, we provided the opportunity for those in that control group to participate in Study 2's intervention (without including their data). Half of the sample chose to do so, reducing our control group sample to only four participants, which would not yield reliable results. We tried to recruit another no-contact control group by the end of Intervention Study 2, but by that point word had spread throughout the community of the potential benefits of novel skill learning and recruiting for this group was extremely difficult. It was also difficult to recruit for a waitlist control group if the wait was for over a year. Future work can seek to overcome these recruitment difficulties by incorporating an attention control group rather than a no-contact control group (LaFave et al., 2019), which could help isolate the contribution of mere practice effects from the assessments.

In sum, our follow-up results from a multi-skill learning intervention are promising indicators that learning new skills could lead to improved cognitive abilities in older adulthood. We demonstrate that learning multiple new skills in older adulthood is not only possible, but also may lead to considerable long-term cognitive growth. Unlike most prior cognitive interventions, we found general sustained, significant improvements even after one year in multiple cognitive domains (cognitive control, working memory, and episodic memory) in one or both intervention samples. Importantly, performance on these specific cognitive assessments was not trained during the intervention (i.e. participants only saw the tasks during the 6 or 7 assessment periods). Future work can replicate and extend our learning intervention with a larger, more diversified sample to test the potential explanations for our results. Future research with intense learning interventions might also consider including neuroimaging techniques to better identify regions of the brain that are seeing the largest areas of growth (see McDonough et al., 2015 for an example). Research in this direction may provide additional support for the theory that promoting learning environments from earlier in the lifespan during older adulthood can lead to considerable cognitive gains (e.g. Wu et al., 2017; Wu & Strickland-Hughes, 2019). Such evidence could reduce negative stereotypes about cognitive abilities and learning in older adulthood and foster optimal cognitive and functional growth through late life.

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Data availability statement

Data, analytic methods, and study materials will be made available upon request from the corresponding author. Study 1 was pre-registered on ClinicalTrials.gov (Protocol Record 1320181), and Study 2 was pre-registered on the Open Source Framework via aspredicted.org (https://osf.io/3ehtq).

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