Elementary math in elementary school: The effect of interference on learning the multiplication table[°]

Dror Dotan and Sharon Zviran-Ginat

Mathematical Thinking Lab, School of Education and School of Neuroscience, Tel Aviv University

Memorizing the multiplication table is a major challenge for elementary school students: there are many facts to memorize, and they are often similar to each other. This similarity creates interference in memory. Here, we examined whether learning would improve if the degree of interference is reduced, and which memory processes are responsible for this improvement. In a series of 16 short training sessions over 4 weeks, first-grade children learned 16 multiplication facts – 4 facts per week. In 2 weeks the facts were dissimilar from each other (low interference), and in 2 control weeks the facts were similar (high interference). Learning in the low-similarity, low-interference weeks was better than in the high-similarity weeks. Critically, this similarity effect originated in the specific learning context, i.e., the grouping of facts to weeks, and could not be explained as an intrinsic advantage of certain facts over others. Moreover, the interference arose from the similarity between facts in a given week, not from similarity to the previously-learned facts. Similarity affected long-term memory – its effect persisted 7 weeks after training has ended; and it operated on long-term memory directly, not via mediation of working memory. Pedagogically, the effectiveness of the low-interference training method, which is dramatically different from currently-used pedagogical methods, seems to call for reconsideration of the way we teach the multiplication table in school.

1 Introduction

Learning the basic arithmetic facts, in particular the multiplication table, is a key part of the elementary school mathematics curriculum. Mastering the multiplication table is important not only by itself, but also for acquiring more advanced mathematical skills: even if not-memorized multiplication facts can be solved by various workarounds (strategies, external devices), automatic knowledge is still advantageous because it can free cognitive resources that can be used for other tasks (Bratina & Krudwig, 2003; Hasselbring, 1988). Sadly, learning the multiplication table is not only important, it is also hard. Children typically learn by rote the single-digit multiplication facts from 3×3 to 9×9 – a challenging quantity of 28 facts to memorize (other multiplications can be solved using rules, $N \times 0$, $N \times 1$, $N \times 10$; using twin addition, $N \times 2$; or using multi-stage procedures to solve multi-digit multiplication). Indeed, many children have difficulties learning the

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multiplication facts and show poor/abnormal performance patterns (Geary, 2004; Gross-Tsur et al., 1996; Noël & De Visscher, 2018; Räsänen & Ahonen, 1995).

Learning arithmetic facts, specifically the multiplication table, has at least two aspects. One aspect pertains to the mathematical meaning of the arithmetic facts. The mathematical meaning determines the result of each given fact, and has several consequences – for example, that addition facts are related with counting and with the idea of moving along a number line; that a multiplication fact is equivalent to a series of same-operand additions; and that for both additions and multiplications, larger operands are correlated with larger results. Learning such mathematical truths is critical in order to understand the meaning of arithmetic, to be able to use it properly, and to compute the result of arithmetic facts – e.g., if we need to solve a problem whose solution we did not yet learn or we already forgot. Moreover, several of these truths are not just mathematical, they may also affect the cognitive processing of the arithmetic facts. For example, the magnitude of an arithmetic fact affects the difficulty of solving it (Groen & Parkman, 1972; Zbrodoff & Logan, 2005), and solving addition and subtraction facts is associated with the activation of number-line representations (McCrink et al., 2007; Pinheiro-Chagas et al., 2017).

The present study focuses on the second aspect of learning arithmetic facts, in particular multiplication facts – rote memorization. This aspect is extremely important too: although multiplication has mathematical meaning, most educated adults eventually come to learn the multiplication table by heart, and they solve most single-digit multiplication problems not via mathematical computation but by retrieving a memorized response (Campbell & Beech, 2014). More specifically, multiplication facts are stored in verbal memory (Dehaene, 1992; Dehaene & Cohen, 1995; Dehaene et al., 2003). In line with this idea, several studies showed that phonological skills predict arithmetic abilities (Jordan et al., 2010; Korpipää et al., 2020; Simmons & Singleton, 2008). Thus, while understanding the mathematical aspects of multiplication certainly provides a way to solve it, and underlies the knowledge of how to use multiplication to serve particular goals, rote memorization is necessary to become proficient in arithmetic. Indeed, learning multiplication facts is not a purely “mathematical” operation – it depends on language skills and memory (LeFevre et al., 2010; Xu et al., 2021; this is true also for arithmetic facts other than multiplication).

Because multiplication facts are learned by rote memorization, their learning is subject to the limits of human memory, in particular to the interference induced by similarity between the to-be-memorized items: memorizing similar items is hard because they interfere with each other. This similarity effect is evident when learning arithmetic facts (Barrouillet et al., 1997; Campbell & Graham, 1985; De Visscher & Noël, 2013; Katzoff et al., 2020; Noël & De Visscher, 2018) as well as in other memory tasks (Baddeley, 1966a, 2003; Hall, 1971; Nelson et al., 1974; Oberauer & Kliegl, 2006; Oberauer & Lange, 2008; Stager & Werker, 1997; Vallar, 2006). Particularly for arithmetic facts, one explanation of the similarity effect is that the facts are represented in memory as a network of associations, in which each fact is associated not only with the correct solution but also with incorrect solutions (Campbell & Graham, 1985). The similarity between facts increases the likelihood of following these incorrect-solution associations and retrieving an incorrect answer. Some individuals have particularly high sensitivity to the similarity-induced interference (“hyper-

sensitivity to interference”), and consequently they find it extremely hard to learn the multiplication table (De Visscher & Noël, 2013, 2014a; De Visscher et al., 2018; Dotan & Friedmann, 2019), i.e., they show symptoms of dyscalculia (American Psychiatric Association, 2013; World Health Organization, 1992).

As means to overcome the difficulty caused by the similarity between facts, we propose a simple teaching method based on two foundations. The first foundation is *similarity versus dissimilarity*: similar multiplication facts are hard to memorize because they interfere with each other, but it may still be easy enough to memorize a set of dissimilar multiplication facts (Campbell, 1987; De Visscher & Noël, 2013, 2014a, 2014b; Girelli et al., 1996; Katzoff et al., 2020). Thus, learning the full multiplication table is hard, but it should be possible to learn a subset of multiplication facts as long as they are dissimilar from each other (e.g., $9 \times 9 = 63$ and $7 \times 4 = 28$). The second foundation is *temporal distance*: similar facts interfere with each other when they are presented simultaneously, or within a short time from each other, but they should not interfere as much when they are presented with sufficient temporal delay between them (Campbell, 1987). Thus, we may be able to teach even similar facts (e.g., $8 \times 8 = 64$ and $8 \times 6 = 48$) if we present them with sufficient temporal delay between each other. These two foundations lead to the following simple teaching method: each lesson should include only dissimilar facts; and different lessons that include similar facts should be administered with sufficient temporal delay between them.

We examined whether this low-similarity training method would improve the learning of multiplication facts by first-grade children who did not yet start learning the multiplication table. The specific experimental design was as follows: during four weeks of training, each child learned four multiplication facts per week (16 facts overall). In two weeks, the 4 facts were dissimilar from each other. As control, in the two other weeks the facts were similar to each other. Namely, each child learned both similar and dissimilar facts. We predicted better learning in the weeks with dissimilar facts than in the control weeks. As we shall see, this was indeed the case.

In a previous single-case study (Dotan & Friedmann, 2019), this low-similarity training method was extremely successful for a single woman with hyper-sensitivity to interference. A small effort of about 4 minutes per multiplication fact, distributed over 4 weeks, sufficed for her to learn 12 facts and to remember them two months later with 80% accuracy – much better than her performance in a high-similarity control condition. However, Dotan and Friedmann’s study had several limitations, which prevent it from illuminating reliably on the real-life learning of the multiplication table. First, the study examined only a single participant, so its pedagogical conclusions are suggestive at best. Second, the participant was an adult woman, so the study does not inform directly about the memorization processes in children – the actual population which is typically learning the multiplication table. Third, this woman had a very specific memory disorder (hypersensitivity to interference), so her performance pattern may be different from that of individuals without cognitive disorders, or individuals with other types of cognitive disorders. Last, although Dotan and Friedmann showed that low-similarity training can improve memorization, they did not examine in detail why this improvement occurred. For example, they did not identify the specific memory mechanism responsible for the improvement.

The present study aimed to overcome these limitations. We had three goals, with impact on pedagogy as well as on cognitive theory. First, we aimed to find simple means to make it easier to learn the multiplication table by the most relevant population – children in early elementary school grades, who do not have learning disorders. Second, we aimed to show, for the first time, causal evidence for the effect of similarity between multiplication facts on their memorization, specifically in typically-developing elementary school children. Third, we asked *why* similarity improves memorization; in particular, we aimed to identify the specific memory mechanism responsible for the similarity effect.

2 Method

The study was approved by the Tel Aviv University Ethics Committee. The raw data and the analysis scripts are available in <http://osf.io/pqtjy>.

2.1 Participants

35 children, native Hebrew speakers, started the study (mean age 7;1, $SD=0;5$). Three children were excluded because the pre-experiment test showed they already knew some of the multiplication facts to be learned. Additional 15 children dropped along the way: 3 decided to quit; one was excluded for not following the study rules, which prohibited parental help; and 11 were excluded for being uncooperative or inattentive (detailed exclusion reasons in Table S1). This left 17 participants (mean age = 7;0, $SD = 0;5$): two in second grade, 14 in first grade, and one in kindergarten senior year. None of them knew any of the 16 multiplication facts taught in the study (Table S7), and none learned the meaning of multiplication at school. Importantly, no participant was excluded for reasons confounded with the magnitude of a putative similarity effect.

2.2 Stimuli

We taught 16 multiplication facts with both operands between 3 and 9, with no ties. The smaller operand appeared first. The 16 facts were grouped into 4 sets with 4 facts in each: two sets with low similarity among the facts, and two sets with high similarity (i.e., similarity was manipulated within participant). The degree of similarity between the facts in a given set was computed in accordance with previous studies (De Visscher & Noël, 2013; Dotan & Friedmann, 2019): first, the similarity between each two facts was defined as the number of digit pairs that appeared in both facts, irrespectively of the digits' position in the fact. For example, the facts $8 \times 7 = 56$ and $8 \times 3 = 24$ have no common digit pair (only the digit 8 appears in both) so their similarity is 0. The facts $3 \times 4 = 12$ and $3 \times 7 = 21$ have three common digit pairs (1-2, 2-3, and 1-3) so their similarity is 3. The similarity index for a set of 4 facts was computed by summing the pairwise similarities of all 6 fact pairs in the set.

All children learned the same 16 facts, but the grouping of the facts into 4 sets was different for each child (Table S1). This per-child grouping was random, and designed to maximize the difference between the low-similarity and the high-similarity sets (Table 1). We also ensured that similarity was not confounded by problem size – i.e., that the low-similarity sets were not

consistently easier due to smaller operands. In fact, the average operand size in the low-similarity sets was slightly *higher* than in the high-similarity sets (Table 1. The excluded participants showed a very small difference in problem size, Table S2). The participants were not told about the similarity manipulation. The experimenters knew about the manipulation, but were not told whether each specific week was high- or low-similarity.

Table 1. Statistical information about the stimuli. Each participant learned the same 16 multiplication facts, which were grouped into 2 low-similarity sets and 2 high-similarity sets (different grouping for each participant). Each set was learned in a separate week (4 weeks overall), in counterbalanced order (either low-high-low-high or high-low-high-low). We made sure that the low-similarity sets were not easier than the high-similarity sets in terms of problem size.

Participant	Similarity in...		Similarity of the set trained in week 1	Average operands size in...	
	Low-similarity sets	High-similarity sets		Low-similarity sets	High-similarity sets
1	2, 2	11, 11	High	6.75	6.00
2	2, 2	11, 11	Low	6.25	6.50
4	2, 2	11, 11	Low	6.69	6.06
6	1, 1	11, 12	High	6.50	6.25
7	1, 2	11, 11	Low	6.44	6.31
9	0, 1	11, 11	Low	6.44	6.31
11	1, 1	10, 11	High	6.13	6.63
12	2, 2	11, 11	Low	6.44	6.31
13	2, 2	10, 10	High	6.56	6.19
16	2, 2	10, 10	High	6.63	6.13
19	2, 2	10, 10	Low	6.88	5.88
23	1, 1	10, 11	High	6.13	6.63
25	2, 2	12, 12	High	6.44	6.31
27	1, 1	10, 10	Low	6.81	5.94
31	2, 2	11, 11	High	6.56	6.19
33	0, 1	10, 11	Low	6.56	6.19
34	2, 2	11, 11	High	6.00	6.75
Average	1.56	10.76		6.48	6.27

2.3 Procedure

Experiment sessions were held during the Covid-19 pandemic period, which prevented face-to-face meetings, so they were done in one-on-one online video meetings with voice over a phone call. The design was fully within-participant, i.e. all children underwent the same procedure. The experiment started with one week of pre-experiment tests (week 1). At this time the children did not yet start learning, so obviously they almost invariably answered “I don’t know”; the pre-experiment testing was aimed mainly to verify that the participants had no prior knowledge of multiplication. Four training weeks followed immediately (weeks 2-5). After a delay of one week

there was a post-experiment test (week 7), and another post-experiment test after an additional delay of 4 weeks (week 12). The experiment weeks were not aligned with calendar weekdays. Figure 1 illustrates the overall procedure.

Figure 1. The experiment protocol. The experiment included a pre-experiment testing week (week 1), four training weeks (weeks 2-5), and two post-experiment testing weeks (weeks 7 and 12). Each testing week included 3 sessions, each of which involved testing the child on each multiplication fact as an open question. The post-experiment testing weeks included an additional 4th session with a forced-choice test. Each training week included 4 training sessions in which that week's 4 facts were rehearsed, followed by a session that tested all facts trained in the present and previous weeks. A training session consisted of a pre-session test (each of the weeks' 4 fact tested once; a week's first training session did not include a pre-session test), 4 training rounds, and a post-session test (each fact tested once). In the training rounds, the child first heard the 4 facts and repeated them, and was then tested on them.

Training period (weeks 2-5). Each training week included 5 sessions: 4 training sessions (approximately 20 minutes each) in which that week's 4 facts were rehearsed, followed – with a gap of at least one day – by a weekly test session. Each training session consisted of a pre-session test, 4 training rounds, and a post-session test.

Training rounds. Each training session included 4 training rounds, which were the core of learning. Each round started with the experimenter saying each fact and the participant repeating it. Errors were corrected immediately. The participants then retrieved all facts they remembered (exercise and result), and the experimenter corrected any errors. Finally, the experimenter asked about any fact not yet retrieved, with immediate error correction. Whenever an error was corrected, the participant repeated the correct fact (exercise and result) until saying it correctly. The order of presenting the facts was random, and different for each presentation of the 4 facts (counterbalanced within participant).

Pre- and post-session tests in the training sessions. These tests were held in the beginning and end of each training session. A week's first training session did not include a pre-session test, as the child did not yet start learning that week's facts. Both tests had the same structure: the child was tested once on each of the weeks' 4 facts by asking "how much is X times Y?" After the child responded to all 4 facts, errors were corrected: for each fact in which the child erred or did not

know the answer, the experimenter said the exercise and the solution, and the child repeated it. If the child repeated incorrectly, the experimenter asked to repeat the fact over and over again, until it was repeated correctly.

Weekly test session during the training period. In the last session in each week, the child was tested on each of the facts learned so far in the current and previous weeks – i.e., on 4 facts in the end of the first training week, and on all 16 facts in the end of the 4th training week. This weekly test included 2 rounds, each presenting all tested facts in random order (different order in each round), with the limitation that the 4 facts of the current week were the first in each round. No feedback was given except general encouragement. In these weekly test, and in the pre-experiment and post-experiment tests (described below), low-similarity and high-similarity exercises were presented together in the same session. Namely, we manipulated the similarity level only in the training sessions, not in the testing sessions.

From these weekly tests, we analyzed only the most reliable responses. Two main factors affect the reliability of a particular response. First, in each week the participants were tested on the facts learned in the current week and in the preceding weeks. We analyzed only the responses to the current week's facts, to avoid confounding with the time elapsed since learning. Second, in each weekly test session the participants were tested on each fact twice, in two rounds. From these two rounds, we deemed the first round as more reliable, because it presumably reflects better the participant's long-term knowledge. The responses in the second round may be confounded by local effects – e.g., the participant may simply repeat their response from the first round. Thus, throughout the results section, the weekly-test analyses refer only to the first-round responses, unless explicitly said otherwise.

Pre-experiment testing (week 1). Week 1 included 4 testing sessions, held in 4 separate days. In the first session, the child was tested on 10 single-digit addition facts and on 7 subtraction facts with single-digit subtrahend and result. The results of these addition/subtraction tests were eventually not used; they are provided in Supplementary Material (Tables S5, S6). In each of the next 3 sessions, the child was tested on 43 multiplication facts (each fact once per session, i.e. 3 times during the whole week): all operand pairs between 2×2 and 9×9 (36 facts, the larger operand always appeared second), and 7 facts with 0 or 1 as operands. The question was “how much is X times Y?” No feedback was given except general encouragement.

Post-experiment testing (weeks 7 and 12). Each of these two weeks included 4 testing sessions, held in 4 different days. The first 3 sessions tested the multiplication fact knowledge like in the pre-experiment tests. The 4th session was a two-alternative forced-choice test. The distracter for a fact was the result of another fact from the same 4-fact set. In week 7, the forced-choice test included a single round with 16 questions – each trained facts once. In week 12, the test included two rounds, each round asking once about each of the 16 trained facts. Untrained facts were not presented in the forced-choice tests. No feedback was given except general encouragement.

2.4 Statistical analyses

For each fact, we defined two measures reflecting its similarity to the 3 other facts in the same set. *Numeric Similarity* is the fact’s average similarity to the 3 other facts in the set, and *Similarity Level* is the set’s classification as low-similarity or high-similarity. The grouping of facts into sets was different for each child, so these parameters were computed for each child.

To examine the effect of similarity on accuracy, we submitted the per-fact accuracy (correct/incorrect) to logistic linear mixed model (LLMM) with the Participant and Fact as random factors. The critical within-subject factor was similarity; the specific similarity predictor was Numeric Similarity (numeric factor) or Similarity Level (categorical factor). These LLMMs also included 2 within-participant covariates accounting for problem size: the average and product of the operands (but in all the critical analyses, the results were essentially the same when removing the 2 problem-size factors, and also when additionally removing the Fact random factor). Other LLMM configurations are detailed below.

In the few cases wherein an LLMM reached a singular fit, which may result from over-fitting, or did not converge, we first tried z-scoring each predictor. If that did not help, we removed the Fact random factor (similar results were obtained when keeping this factor). To examine the significance of a particular factor or interaction, we used log-likelihood ratio test and compared the full LLMM to a model in which that factor/interaction was removed. For these comparisons, we report the test statistic $2(LL_1 - LL_0)$, which follows a χ^2 distribution (LL_0 and LL_1 denote the log-likelihoods of the reduced model and the full model), and the corresponding p value. The degrees of freedom are not reported as they were always 1.

3 Results

3.1 Learning dissimilar facts is easier

In the end-of-week tests, as predicted, accuracy was higher in the low-similarity weeks than in the weeks with similar facts (Figure 2a). To examine the similarity effect statistically, we used the logistic linear mixed model described in Methods. The dependent variable was the per-fact accuracy, the participant and fact were random factors, and there were 3 within-subject factors: similarity (either Numeric Similarity or Similarity Level, in two separate analyses), and the two problem-size factors as covariates (sum and product of the operands). The effect of similarity was significant (with Similarity Level factor: $\chi^2(1) = 7.34$, $p = .007$, odds ratio = 0.41, Figure 2a; with Numeric Similarity factor: $\chi^2(1) = 10.47$, $p = .001$, odds ratio = 0.43, Figure 2b; detailed LLMM results in Table S9a,b). Namely, as predicted, it was hard for the children to learn facts that were similar to each other, and it was easier for them to learn dissimilar facts.

The results above refer to the participants’ first round of responses in each weekly test session, as we considered the responses in this round to reflect the participants’ long-term knowledge more reliably. Still, the similarity effect was observed also in the second round of responses (same LLMM, Numeric Similarity effect: $\chi^2(1) = 6.72$, $p = .01$, odds ratio = 0.49; but not when using the Similarity Level factor, $\chi^2(1) = 2.48$, $p = .12$; Figure S2, Table S10). The

similarity effect in the second-round responses was smaller than in the first-round responses, in line with our assumption that the first round is a better reflection of the learning and the similarity effect.

Critically, the similarity effect originated in the learning that occurred during the experiment, and cannot be attributed to pre-existing knowledge. In the pre-experiment tests on multiplication facts (in week 1), the average accuracy on the to-be-trained facts was virtually zero, leaving no room for a similarity effect: 2.7% in the low-similarity sets and 2.5% in the high-similarity sets. The 0.2% difference between the low- and high-similarity sets was not significant (a logistic linear mixed model did not converge, so we used paired t-test: $t(16) = 0.32$, one-tailed $p = .37$).

Figure 2. Results of the end-of-week tests show better learning of the dissimilar facts than of the similar ones. (a) The children were more accurate in the low-similarity weeks than in the high-similarity weeks. The thin bars show individual results of each participant. (b) Accuracy decreases as a function of the fact's average similarity to the 3 other facts learned in the same week. This effect is observed even within the low-similarity sets and the high-similarity sets, i.e., the fact-specific similarity is important.

Contrary to findings with older children (De Visscher & Noël, 2014a), the participant's individual sensitivity to similarity (hereby StS, Figure 2a) did not predict their overall accuracy. To examine this, we entered the per-participant overall accuracy as the dependent variable in a multiple linear regression. The critical predictor was the participant's StS, defined as $\Delta\text{accuracy}$ between low- and high-similarity sets ($\Delta\text{accuracy} > 0$ denotes high sensitivity). A second predictor controlled for the fact that each child learned the multiplication facts in different grouping, with different specific levels of similarity. This predictor, $\Delta\text{similarity}$, was defined as the difference between the participant's average within-set similarity of the two high-similarity sets minus the average within-set similarity of the two low-similarity sets. If the participant's StS affects their overall accuracy, the StS predictor should have a significant negative effect. This was not the case: neither predictor had a significant effect (StS: $b = -0.55$, one-tailed $p = .21$; $\Delta\text{similarity}$: $b = -0.12$, one-tailed $p = .30$). Still, crucially, high sensitivity to similarity predicted low performance in the high-similarity sets: in a similar regression, in which the dependent variable was changed into accuracy in the

high-similarity sets, the StS predictor had a significant negative effect ($b = -0.61$, one-tailed $p = .006$; Δ similarity: $b = -0.57$, one-tailed $p = .20$). Such effect was not found for accuracy in the low-similarity sets (StS: $b = 0.39$, i.e., opposite to the predicted direction; Δ similarity: $b = -0.57$, one-tailed $p = .20$). Namely, sensitivity to similarity disrupted learning specifically in high-similarity conditions.

The findings above clearly show that it is easier to learn dissimilar facts than similar ones. In the subsequent sections we examine why this is so.

3.2 The similarity effect arises specifically from the grouping of facts in the training sessions

What is the origin of the similarity effect? Our approach, presented in the Introduction, assumes that the similarity effect originates in the specific grouping of facts into weekly sets during the learning time. In particular, we assumed that high similarity between the facts in a given week (hereby, “within-set similarity”) would disrupt learning. Correspondingly, the similarity index we defined captures the similarity between each fact and the 3 other facts in the same week.

An alternative view is that learning is disrupted by the similarity between the current week’s facts and the facts already learned in previous weeks. This resembles the idea proposed by de Visscher and Noël (2014b): they assumed that the multiplication facts are learned in a certain order, and that learning is modulated by the similarity of each fact versus all earlier facts in this “learning list”. A similarity index reflecting this view should capture the similarity between each fact and all the facts in the previous weeks.

To arbitrate between these alternatives, we ran an LLMM on the weekly tests data with Numeric Similarity as the dependent variable, the participant as a random factor, and with four within-participant factors. Two similarity factors represented the two hypotheses above: *Within-set Numeric Similarity*, defined as in the previous section, and *Previous-weeks Numeric Similarity*: the average pairwise similarity of each fact in the current week versus each fact learned in previous weeks. For facts learned in the first week, for which there are no preceding weeks, this factor was defined as 0. Additionally, the LLMM included the two problem-size factors – the sum and product of the two operands. This analysis showed a significant effect of Within-set Similarity, but not of Previous-weeks Similarity (Table 2). Namely, the critical factor that affects learning is the similarity of a fact versus the other facts learned in the same week.

The effect of the per-fact Numeric Similarity was marginally significant even when controlling for the per-set Similarity Level. We showed this using LLMM with 4 predictors – Numeric Similarity, Similarity Level, and the sum and product of the two operands. The dependent variable was the per-fact accuracy, and the Fact and Participant were random factors. Numeric Similarity had a marginally significant effect ($\chi^2(1) = 3.14$, $p = 0.08$, odds ratio = 0.8, Table S11). i.e., it is really the fact-specific similarity that counts. Figure 2b clearly shows how the fact-specific similarity affects performance on top of the per-set similarity level.

Table 2. The results of the logistic linear mixed model that examined how accuracy in the end-of-week tests was affected by the similarity between the facts in each week (within-set), and by the similarity of each set of facts versus the previously-learned facts. A significant effect was found only for the within-set similarity.

Factor	Odds ratio	95% confidence interval	Significance
Intercept	41.14	0.04 – 46,680	
Within-set numeric similarity	0.44	0.25 – 0.77	$\chi^2 = 9.53, p = .002$
Similarity versus previous facts	1.16	0.55 – 2.43	$\chi^2 = 0.14, p = .71$
Sum of operands	0.77	0.29 – 2.0	
Product of operands	1.03	0.89 – 1.18	
<i>Random effects</i>			
σ^2 within participant & exercise	3.29		
σ^2 between participants	1.47		
σ^2 between exercises	0.35		

3.3 The similarity effect arises from long-term memory

Once learned, the multiplication facts are stored in long-term memory. Similarity clearly affected these long-term memory representations, as its effect was observed in the end-of-week tests, which were conducted 2 days after learning has ended. But how did this effect take place?

One interpretation of the similarity effect could be actually in terms of working memory (WM): if two items have some identical features, this impairs their representations in WM due to interference (Baddeley, 1966a; Bunting, 2006; Oberauer & Lange, 2008). It could be that in high-similarity weeks, the children created poor WM representations of the facts, and as a result they had less opportunity to construct appropriate long-term memory (LTM) representations. According to this view, the similarity effect did not arise from LTM per-se, but “leaked” from WM. An alternative view is that similarity affected LTM directly. Finally, it is also possible that both LTM and WM give rise to similarity effects. Indeed, similarity can affect both WM and LTM (Baddeley, 1966b).

Two aspects of our data may arbitrate between these two views: (1) *Performance in the beginning versus the end of each training session*. If the similarity effect originates in WM, it should be evident in the post-session tests, when WM representations are stronger, and may be weaker or completely absent in the pre-session tests. In contrast, if the similarity effect originates in LTM, it should be observed in the pre-session tests: these tests are affected by long-term representation, but they should be less affected by the momentary WM activation by the current training session, which has not yet started. (2) *Day-by-day progress*. If the similarity effect originates in WM, it should be observed from the very first session, because each session recruits the WM. In contrast, if the similarity effect originates in LTM, its effect may kick in only later, after few days, when sufficiently-strong LTM representations have been created.

Figure 3. Accuracy in the short tests conducted in the start and end of each training session. The overall performance improves from day to day. Critically, a similarity effect was observed only in the pre-session test of the last training day – the test that was most sensitive to long-term memory representations. In all other pre- and post-session tests, the similarity effect was not significant.

The results (Figure 3) confirmed the predictions of the LTM-originated-interference view. We used multiple LLMMs to examine separately the pre-session test and the post-session test of each day. The dependent variable was accuracy, the participant and fact were random factors, and there were with 3 within-participant factor: Numeric Similarity, and the sum and product of the operands. A significant effect of similarity was observed in the pre-session test of the 4th training day ($\chi^2 = 6.5, p = .01$, odds ratio = 0.83, Table S12), but not in the pre-session tests of days 2 and 3 (both $p > .90$). The similarity effect in day 4 was stronger than in the pre-session test of the preceding days: a similar LLMM on all 3 days, with Day (#4 vs. others) and Day $\times$ Similarity interaction as additional factors, showed a significant interaction ($\chi^2 = 4.31, p = .04$, odds ratio = 0.68, Table S13). Namely, the predictions of the LTM-originated-similarity hypothesis were fully corroborated: there was a significant effect of similarity in the pre-session test, and this effect was not observed from the first day but appeared only in the last day of training. In contrast, the prediction of the WM-originated-similarity hypothesis was refuted: no significant effect of similarity was found in any of the post-session tests (all $p > .11$).

The similarity effect was observed not only in the tests, but also during the training rounds (Figure 4). Accuracy increased with training, and crucially, the effect of similarity emerged only in the last training day, again supporting the idea that similarity affects LTM. We ran an LLMM on the accuracy in each response attempt, with the participant and fact as random factors; and with several within-participant factors: Numeric Similarity, the sum and product of the operands (to account for problem size), the training round (1-4, numeric factor), and the serial position of the item in the specific round (1-4, numeric factor). Again, the effect of similarity was significant in

day 4 ($\chi^2(1) = 6.03$, $p = .01$, odds ratio = 0.70, Table S14) but not in the preceding days (all $p > .65$).

Altogether, these findings indicate that similarity affects the long-term memory representations directly, without mediation of working memory processes. Still, in the next section we will see evidence suggesting that working memory processes may be involved to some extent in the similarity effects.

Figure 4. The effect of similarity on accuracy in each response attempt during the training rounds. The improvement was nearly monotonous from round to round, with a significant effect of similarity only in the last training day – the day most sensitive to long-term memory representations.

3.4 Low-similarity training is beneficial also in the long run

Unsurprisingly given the relatively small amount of training, accuracy in the post-experiment tests was not high – about 20% in free recall (Figure 5a). Still, the post-experiment forced-choice accuracy was higher than chance (Figure 5b). Namely, although we provided relatively little training, the children had memory traces of the multiplication table several weeks after training has ended. The critical question is whether manipulating similarity during training affected the long-term representations. Figure 5b indicates that it did: in the second round of week 12 forced-choice test, performance was good in the low-similarity items (~70% success; comparing the per-participant success rate to the 50% chance level, paired $t(16) = 3.68$, one-tailed $p = .001$) but not in the high-similarity items. The difference between low- and high-similarity items was significant. We examined this using LLMM on the post-test accuracy of the week 12 second round of forced-choice, with Numeric Similarity and the operands' sum and product as within-subject factors, and with the participant as a random factor (the fact random factor was not added because this model did not converge). The Similarity effect was significant ($\chi^2(1) = 9.03$, $p = .003$, odds ratio = 0.67, Table S15). Such similarity effect was not found in the previous forced-choice tests (numerically, the performance was actually higher for high-similarity facts, as can be seen in Figure 5b, but this effect was not significant, $p > .22$). Still, note that the p -value obtained

here (0.003) can survive a Bonferroni correction for the multiple comparisons in 5 post-experiment tests.

Why was the similarity effect observed only in the last forced-choice test and not in the preceding tests? We propose that the similarity effect emerged only when the session included some “memory warmup”: in most tests, each fact was presented only once so there was no warmup; but in the week 12 forced-choice test, the first round served as warmup to the second. According to this view, the similarity effect originates in LTM, but it can be observed only after increasing the facts’ activation level – either by warmup (as in the week 12 forced-choice test) or if the activation was already high in the first place (as in the weekly tests). We acknowledge that while our findings in the previous sections showed a clear and robust long-term effect of similarity two days after learning has ended, the support for a weeks-long effect of similarity is not as reliable, as the long-term effect was observed only in one of the post-experiment tests, and the interpretation of this pattern is post-hoc. Still, note that the specific pattern observed here – an effect of similarity in week 7 but not in week 12 – does not appear to be random, because it precisely replicates a previous study that used the same paradigm (Dotan & Friedmann, 2019).

Figure 5. The effect of similarity on accuracy in the post-experiment tests – the recall tests (average of sessions #1-3 in each week) and the two-alternative forced-choice tests (session #4 in each week). Only the trained facts are shown here. The p values above the columns are the effect of the Numeric Similarity factor in the LLMM described in the text. The p values inside the forced-choice columns refer to comparing the per-participant mean accuracies to chance level (paired t-test against 50%, one-tailed p).

We ruled out an alternative, short-term-memory interpretation of the week 12 similarity effect. This interpretation emphasizes that the week 12 forced-choice test included two rounds, with different distracters in each round, and the children may have relied on this information: e.g., if $7 \times 6 = 42$ is presented with distracter 35 and then with distracter 63, in the second round they can

deduce that the correct answer is 42 – the only alternative that appeared in both rounds. Critically, the ability to remember the exercise and distracters could be affected by the within-set similarity. To examine this “strategic guessing” interpretation, we recruited another group of 20 first-grade children (mean age = 7;1, SD = 0;3) who performed only the two-round forced-choice test, with no preceding learning. Three children were excluded for stereotypical answers (consistently choosing the second alternative, Table S2). The strategic-guessing view predicts that here too the performance in the second round will be higher than chance, higher than in the first round, and affected by similarity. This was not the case: accuracy was similar in the two rounds (round 1: 53.8%, round 2: 47.0%), and round-2 accuracy was low (< 50%) and unaffected by similarity (low: 48.7%, high: 45.3%; using LLMM on accuracy with the participant as a random factor, and Numeric Similarity and the sum and product of the operands as within-participant factors, the effect of similarity was not significant: $\chi^2(1) = 0.03, p = .87$; similar results when using Similarity Level instead of Numeric Similarity, $\chi^2 = 0.38, p = .54$). This refutes the strategic-guessing interpretation and supports the idea that the similarity effect in week 12 indeed originated in long-term memory.

4 Discussion

4.1 Low similarity facilitates memorization

This study examined whether first-grade children, who do not yet know any multiplication facts, learn them better when dissimilar rather than similar facts are learned together. The children memorized sets of facts with low or high similarity, each set during one week. The results were clear: by the end of a learning week, accuracy in the low-similarity sets was higher by ~25% than in the high-similarity sets. This difference cannot be attributed to fact-specific characteristics such as problem size, because the facts were grouped in a different way for each child, and because our analyses controlled for problem size. Namely, the similarity effect did not originate in an intrinsic property of particular facts, but had to do with how we organized the learning. Moreover, the best predictor of performance was the fact’s precise similarity to the 3 other facts learned in the same week, not the facts learned during the previous weeks. Namely, the main source for similarity-induced interference is from the simultaneous learning of similar facts, not from learning a new fact that is similar to an already-learned one. Because similarity was manipulated, we can conclude about causality: low similarity not just correlates with multiplication fact knowledge; it improves knowledge.

These conclusions reaffirm De Visscher & Noël (2014b): they too showed that a similarity measure should consider the order of learning the facts. Our conclusions also extend De Visscher & Noël’s in two critical respects. First, whereas their similarity measure relied on a fixed learning order, using assumptions based on school curriculum, here we could precisely know the real learning order for each child. Second, here we could arbitrate between the effect of interference from previously-learned facts and the effect of interference from simultaneously-learned facts, and show that the latter is the critical one.

Our results join a growing number of studies showing that similarity disrupts memorization of arithmetic facts (Campbell, 1987; De Visscher & Noël, 2013, 2014a, 2014b; De Visscher et al., 2018; Katzoff et al., 2020; Polspoel et al., 2019). The exact definition of similarity, however, is still an open issue: we do not yet know what precisely makes two facts similar to each other (cf. Appendix of Dotan & Friedmann, 2019).

4.2 The specific memory mechanisms underlying the similarity effect

Our data clearly indicates that the similarity between facts specifically affected long-term memory representations. First, in the day-by-day learning curve, the effect of similarity was not observed right from the start, but emerged only in the last training day. Second, the effect was observed in the pre-session test, which reflects long-term memory representations, and not in the post-session test, which presumably reflects short-term effects from the just-ended session. Third, a strong effect of similarity was observed in the end-of-week tests, which were done 2 days after the last training session. Fourth, an effect of similarity, albeit smaller, was observed even in week 12 of the study, i.e., 7 weeks after training has ended.

In the week 12 forced-choice test, there was a similarity effect in the second round of testing but not in the first round – i.e., only after the representation of facts was activated in memory. This may suggest that although similarity affects long-term memory representations, its effect on behavior also depends on additional, short-term factors. Future studies may examine the ways in which long- and short-term effects interact to create the similarity effects (Katzoff et al., 2020).

We acknowledge that this weeks-long effect of similarity was observed only in one of the post-experiment tests, i.e., this specific effect is not as reliable as the previous effects observed in this study. It seems likely to assume that at least one of the reasons for the absence of a similarity effect in some of the post-experiment tests was that the amount of training provided in this study was not sufficient. Indeed, the overall performance level in the post-experiment tests was relatively low. We also note that while the discrepancy between the different post-experiment tests is not just random as one may think. Dotan and Friedmann (2019), who used a design similar to ours, showed a surprisingly similar pattern: their participant did not show a similarity effect in the post-experiment test administered shortly after the 4-week training period, but she did show a similarity effect in another test administered 2 months later.

Replicating the long-term similarity effect, preferably with more intensive learning than we provided here, may be the goal for future studies. An interesting question for such studies is whether the discrepancy between the different post-experiment tests would be replicated – specifically, that the similarity effect is not observed right after the training period but only several weeks later; and that the long-term similarity effect is stronger in the second testing round than in the first round. If these effects are replicated, we may ask which of the post-experiment test best reflects the pedagogical reality. Arguments could be made in either direction; but at least one argument supports the idea that a second testing round mimics real school scenarios better than the first testing round: at school, more often than not, children are not required to answer a single multiplication question, but to solve several mathematical problems one after another. The child

would therefore not encounter each fact once, as is the case in the first testing round, but several times in a row, as is the case in additional testing rounds.

Does similarity affect storage in memory or retrieval? The finding of a similarity effect only in the pre-session test, and only after several days, suggests a storage-related origin of similarity. In contrast, the discrepancy between the first and second testing rounds in week 12, which suggest that the long-term similarity effect may depend on some “memory warmup”, may indicate that similarity is also affected by retrieval-related processes. A retrieval-related similarity effect would be in line with Campbell (1987), who showed a similarity effect following a retrieval-time manipulation. It may be the case that similarity affects both storage and retrieval processes (cf. Bartko et al., 2010).

Eventually, although there may still be open questions with respect to the precise cognitive mechanisms responsible for the similarity effect, from a pedagogical perspective the picture is quite clear: it is better to learn arithmetic facts in low-similarity context than in high-similarity context.

4.3 Individual differences in sensitivity to similarity

Different children had different degrees of sensitivity to similarity-induced interference (StS), and StS correlated with their performance in the high-similarity sets. Previous studies too found that high StS correlates with poor knowledge of arithmetic facts (De Visscher & Noël, 2014a, 2014b). Interestingly, the precise kind of correlation between StS and performance was different in De Visscher and Noël’s studies and in ours. In our study, StS correlated specifically with the performance in high-similarity sets, i.e., the individual differences were reflected in the more-demanding situations (sets with high similarity). In contrast, in De Visscher and Noël (2014a), who examined 4th grade children, StS correlated with the overall performance. A possible interpretation of this discrepancy is that when children only start learning the multiplication table, the individual differences in StS affect only the difficult situations; and that as the children grow up, the gap between them is not closed but widened. Future studies may compare age groups in a better-controlled fashion to examine this possibility.

In extreme cases, an abnormal degree of sensitivity to the similarity-induced interference (“hypersensitivity to interference”) may cause abnormally low knowledge of arithmetic facts – dyscalculia (De Visscher & Noël, 2013; Dotan & Friedmann, 2019). The training program we used here seems to be effective also for such individuals, perhaps even more than for individuals without this disorder. Indeed, the similarity effect size observed here was considerably smaller than the effect size reported by Dotan and Friedmann (2019), who used the same method with a woman with hypersensitivity to interference. Still, our findings show that low-similarity training is effective also for children with a normal level of StS.

4.4 Pedagogical implications

Our study showed that to memorize randomly-grouped arithmetic facts, low-similarity context is superior to high-similarity context. From a pedagogical perspective, an important question is whether low-similarity training outperforms also the typical schooling method –

learning the multiplication table by columns (the times-3 table, then the times-4 table, etc). Our data cannot answer this question. An interesting possibility could be that both low-similarity learning and by-column learning are good methods, yet when considering the distinction between the two aspects of learning multiplication – conceptual learning and rote memorization – each method is best suited to teach a different aspect. By-column learning may be superior for conceptual learning of the multiplication table – e.g., to understand the meaning of multiplication, its relation with addition, and to learn calculation-based strategies (e.g., if you forgot how much 7×6 is, you can compute it as $7 \times 3 \times 2$). Low-similarity training may be superior for rote memorization, because the column-by-column organization tends to group similar facts together. If this is indeed the case, the best way to teach multiplication may be to start with column-by-column teaching in school, and then switch to low-similarity training for rehearsal in school, homework, computer games, etc.

Low-similarity training was tested here for multiplication, but it may be effective also to memorize addition facts, which can be retrieved from verbal memory too (Campbell, 1995; even if additions can be solved by other strategies too, Dehaene et al., 2003). The method may also be effective to memorize other types of information that may be subject to similarity-induced interference – for example, formulas in algebra ($(a + b)^2 = a^2 + b^2 + 2ab$), calculus ($\frac{dx^n}{dx} = nx^{n-1}$), or geometry ($triangle\ surface = \frac{base \times height}{2}$), and even for rote learning of non-mathematical content. This is not unlikely, given that the human memory is known to be affected by similarity and interference in a wide range of tasks, involving different memory processes and different types of content (Baddeley, 1966a; Farrell & Lewandowsky, 2003; Nelson et al., 1974; Oberauer et al., 2012; Pajak et al., 2016; Smith et al., 2021; Susic-Vasic et al., 2018).

4.5 Reliability and study limitations

The sample size in this study was not large (17 participants), but several factors support the reliability of the results even with this sample size. First, we examined the performance in multiple time points during training: before each training session, at the end of each training session, the individual response attempts during training, at the end of each training week, and after the experiment has ended. Similarity demonstrably affected performance in several of these time points, and the specific time points in which a similarity was found were generally predictable and consistent with previous studies. Thus, the results cannot be deemed unreliable due to a small dataset size or a statistical bias in a particular analysis. Second, the impact of similarity was observed in several different types of analyses: in the accuracy of the weekly and post-experiment tests, in the day-to-day progress patterns, and in the comparison between pre-session and post-session tests. Last, the effect of similarity was observed with high significance levels and large effect sizes.

Another issue that may raise a concern is the high dropout rate: about 50% of the participants were excluded. Critically, however, the effects of similarity were not experimenter-originated artefacts of our decision to exclude specific participants. No participant was excluded on grounds related with similarity: except 3 children who never started the study because they had pre-existing

knowledge of multiplication, and one child who quit for an unknown reason, all participant-exclusions were on grounds related with inattentiveness or being non-cooperative. The specific criteria were a high rate of fact-repetition errors (which do not index similarity); and the subjective impression of the experimenters (who were unaware of the similarity level in each particular week).

Still, several open questions remain regarding the generalizability of our conclusions to wider populations and to the classroom environment. We raise here what we view as central questions that still remain, which will hopefully be addressed by future studies. First, even if, as we explained above, our results are statistically reliable in spite of the sample size, the question still remains whether our participants represent the general population. This question seems particularly critical given our participant-recruitment procedure: we did not recruit whole classes; rather, we recruited individual participants via social networks on a voluntary basis, and this recruitment method may potentially be biased.

Second, although we never excluded participants on grounds related with similarity but only for being inattentive, this still leaves the possibility that the children who remained in the study had higher attentional abilities than the excluded children. It could therefore be that our results reflect what happens in children with high attentional abilities, and the situation is different for children with lower attentional abilities. Indeed, attention may certainly be related with similarity: a main aspect of attention, in particular executive attention, is coping with cognitive conflicts and interference, and executive attention is tightly related to (or even fully overlaps) working memory abilities (Engle, 2002; Petersen & Posner, 2012). However, note that a straightforward prediction from these theoretical relations between attention and similarity may be that the effect of similarity should be *higher* in children with low attentional abilities. In any case, it would clearly be incorrect to make the more-general assumption that similarity affects only children without cognitive difficulties – in fact, the situation seems to be the opposite: strong similarity effects were found specifically for individuals with mathematical or memory difficulties (De Visscher & Noël, 2013, 2014a; Dotan & Friedmann, 2019).

Third, we observed the effect of similarity when training children individually, in one-on-one sessions. An open question is whether the effect of similarity would be observed, and would be as strong, in real classroom settings.

Last, our study was intentionally designed to examine the memorization aspect of multiplication facts, and ignore the mathematical semantics of multiplication. For this reason, we taught the multiplication facts as arbitrary sequences of numbers, without explaining what they mean. While our decision was methodologically justified, given that adults solve multiplication facts using rote memorization (Campbell & Beech, 2014), it is still possible that the cognitive representation of the facts would be different if rote-learning is accompanied by understanding the mathematical semantics of each fact. Follow-up studies may examine the effect of similarity on learning the multiplication table in more ecological settings, i.e., in a classroom and when learning highlights both rote memorization and mathematical semantics.

5 Conclusion

This study shows quite clearly that the ability to learn the multiplication table is affected by the similarity between facts, and that this similarity effect arises from long-term memory processes. We further showed that a simple manipulation – careful grouping of specific arithmetic facts into lessons, based on their similarity to each other – may help memorizing the multiplication table. This method is quite different from the existing school practices, so we hope it may be used to improve how elementary schools teach the multiplication table – an excruciating challenge for many children.

Our study also leaves several open questions. Most importantly, it is still not fully clear which precise memory processes are responsible for the similarity effect in arithmetic fact learning; what precisely makes two facts similar to each other, and how similarity should be measured; and whether and how rote memorization and conceptual learning interact when learning arithmetic facts. Future investigations of these questions may lead to better understanding of how our brains learn mathematical facts, and what math teaching in elementary school should look like.

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