Role of verbal working memory

in rapid procedural acquisition of a choice response task†

Stephen Monsell* and Brontë Graham

Psychology, College of Life and Environmental Sciences,

University of Exeter, UK

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* Corresponding author at School of Psychology, University of Exeter, Washington Singer Labs, Exeter, EX4 4QG, UK. Email address: s.monsell@exeter.ac.uk.

Abstract

How quickly are instructions for a task translated into an effective task-set? If declarative working memory (dWM) is used to maintain a task's S-R rules until practice compiles them adequately into procedural memory, variables that affect retrieval from dWM should influence task performance while it is still dependent on dWM. Participants were trained on a series of 6-choice RT tasks, with a 1:1 mapping from object pictures to keys. In Experiments 1 and 2, an instruction phase — presentation of the S-R rules one by one —was followed by test trials. The phonological similarity of the objects' names significantly affected performance only during the first few encounters with the stimuli. Serial position effects were also consistent with retrieval from verbal dWM during those early trials. In Experiment 3, instruction in the S-R rules was omitted, so participants had to learn the S-R mappings by trial and error alone; the effect of phonological similarity lasted longer, but still disappeared after a dozen encounters with each stimulus. Experiment 4 showed that instructions and just four trials of practice per S-R rule were sufficient to form a persisting representation of the S-R rules robust enough to interfere with later acquisition of a competing S-R rule after several minutes spent acquiring other task-sets. An effective and lasting task-set is rapidly compiled into procedural memory through instruction and early feedback; verbal dWM plays little role thereafter.

Keywords: Skill acquisition, Task-set acquisition, Working memory, Procedural memory, Instructions.

To perform a new cognitive task we must find a way to link together and tune mental processes and representations so that appropriate internal operations and external actions are generated in response to relevant combinations of stimuli. This new organisation of the mind and the associations it enacts – a novel "task-set" – is initially acquired through some combination of instruction, imitation, and trial and error, then refined and optimised through practice and feedback. Somewhere along this trajectory, a representation of the task-set is constructed in procedural memory, from which it may be later elicited without having to relearn the task from scratch.

It is common experience in the laboratory that after brisk and simple instructions of the form "When you see stimulus X, press key A; when you see stimulus Y, press key B; ……; please respond as quickly as you can while avoiding errors.", participants respond fluently within just a few trials: errors are few and responses rapid. Of course, performance continues to improve with further practice, and the exact form of this slow improvement in speed over hundreds or even thousands of trials has been much debated: e.g. the classic power law (Fitts & Posner, 1967; Newell & Rosenbloom, 1981) versus some variant of an exponential function (Evans, Brown, Mewhort, & Heathcote, 2018, Heathcote, Brown & Mewhort, 2000). Our concern, however, is with the initial transition between no knowledge of the task, via instruction (if available) and trial and error, to early fluency, over just the first few trials. In this article we report four experiments exploring the initial acquisition of a choice reaction time (CRT) task.

When instructed in a new task, we presumably start by encoding into *declarative* memory as much as we can of what the instructions tell us: a description of the stimuli to expect, possible responses, the rules of the task, when and where to expect the stimuli, whether to strive for speed or accuracy, and so on. Somehow, this declarative representation generates an executable representation of the task-set in procedural memory, and performance becomes fluent. As we continue to practice the task, and receive feedback, the procedural representation is refined and strengthened, and fluency continues to improve incrementally. The component of task-set we are particularly interested in here is S-R rules: the stimuli to be ready for and the responses to be made to each.

To examine this early transition from instruction to fluency we need to accumulate data over multiple early acquisitions of such a task. To do this we used a rapid instructed task learning (RITL) paradigm similar to that invented by Ruge and Wolfensteller (2010). They took participants through 20 task-acquisition cycles in an experimental session. In each cycle participants were shown for ten seconds a display showing four novel visual stimuli, two of which were to receive a left hand response, and two a right hand response. Stimuli from the set were then presented in a random order, eight times each overall; participants responded as quickly they could while avoiding errors and received feedback. The next cycle began with introduction of a new set of four stimuli, mapped to the same two responses. Thus, although the structure of the task and the response alternatives remain the same throughout the session, the participant has to acquire a new set of S-R associations in each cycle. By averaging over several cycles we can obtain a fine-grained *acquisition function* indicating how performance on the task changes over the first few exercises of an S-R rule. In Ruge and Wolfensteller's study, mean RT on the first trial per item was ~610 ms, with an error rate of ~6%, and improved rapidly over the next three or four trials, reaching a relatively stable level (~575 ms, ~3% errors) for the remainder.

What happens during the presentation of instructions and the first few trials to enable that fluency? Classical accounts of the improvement in performance of more complex motor or cognitive tasks through extended practice identify at least two phases: (1) a "cognitive" (Fitts, 1964; Fitts & Posner, 1967) or "declarative" (Anderson, 1982) phase in which the participant interprets or understands the task, representing its rules relatively abstractly, perhaps verbally, followed by (2) a learning-through-doing compilation of an effective task-specific procedure which is then gradually refined and strengthened until, after large amounts of practice, it becomes relatively automatic. (See also Chein and Schneider, 2012, who distinguish three phases). Brass, Liefooghe, Braem and De Houwer (2017), focusing on the rapid acquisition of simple S-R (or condition-action) rules in CRT tasks, also distinguish between an instruction phase, in which the participant builds a "task model" which integrates and structures the declarative information conveyed by the task-instructions, and an "implementation phase" through which a procedural representation becomes highly accessible so that the S-R rules are ready to be applied when a stimulus is presented – as captured by Exner's (1879) concept of the "prepared reflex" (Hommel, 2000). Ramamoorthy and Verguts (2012) model acquisition as a transition between fast Hebbian learning of abstract verbal rules in prefrontal cortex (which execute relatively slowly) and slower Hebbian learning of direct S-R links in basal ganglia (which execute faster).

These two phases in the life-history of a task-set map onto its representation in declarative and procedural memory and the transfer of control between them.

The distinction between declarative and procedural memory is firmly grounded in neuropsychological dissociations (Cohen & Squire, 1980). Failure of transfer between them can be seen in the phenomenon of "goal neglect" in both frontal patients and in normal participants under high memory load (Duncan, Elmslie, Williams, Johnson, & Freer, 1996): the declarative retention of instructions for a task, evidenced by the ability to reproduce them later, accompanied by failure to perform the task when appropriate cues and stimuli appear. Some authors assume, particularly in the context of task-switching, that working memory (WM) has distinct declarative and procedural components (e.g. Rubinstein, Meyer, & Evans, 2001; Vandierendonck, 2012; Van 't Wout, Lavric, & Monsell, 2013, 2015). The most explicit theory of such a separation between declarative and procedural memory, each with similarly structured working and long-term components, has been proposed by Oberauer (2009, 2010; Oberauer, Souza, Druey, & Gade, 2013; Souza, Oberauer, Gade, & Druey, 2012). We will return to this account later, but start with a less detailed working hypothesis: a specification of the task derived from instructions is, if simple enough[1], held initially in dWM; from this a compiled task-set is created in (some form of) procedural memory by a process of "task-set formation" (Cole, Bagic, Cass, & Schneider, 2010) or proceduralisation. Representation in either format can control execution of the task, but the procedural representation soon takes over control from the declarative representation, which no longer needs to be actively maintained. How rapidly does this transition happen? For more complex tasks, like changing gear or using a mobile phone app, it would appear that many trials of practice and feedback are required for the transition to control by the procedural representation alone. But for a very simple CRT task, an acquisition function like that of Ruge and Wolfensteller (2010) might suggest that of the order of three or four trials per S-R rule are enough.

More dramatically, it has been claimed that, after instruction, S-R rules may be fully proceduralised without having been exercised even once (see Meiran, Liefooghe, & De Houwer, 2017, for review). Evidence for such "automatic effects of instruction" or “intention based reflexivity” comes from a family of paradigms that look for effects of instructing the participant for one task on how they perform another. In one such paradigm (e.g. Liefooghe, Wenke, & De Houwer, 2012; Wenke, Gaschler, & Nattkemper, 2007) the participant experiences repeated cycles in which they (i) see an instruction for a two-choice RT task with the rules for a new pair of stimuli (e.g. left for "X", right for "T"), (ii) perform for a few trials a different "diagnostic" task (the same on all cycles) afforded by the same class of stimuli

[1]A declarative representation of a task-set that exceeds the capacity of working memory presumably requires use of long-term declarative memory as well.

(e.g. left for italic, right for normal font), until (iii) they see a cue telling them to perform the instructed task on subsequent stimuli. During performance of the diagnostic task (phase ii), it is found that RT is shorter when the response specified by the instructed task matches that specified by the diagnostic task (*X* in this example) than when it conflicts (*T*). This response congruence effect indicates that the newly instructed S-R association is already involuntarily active during the diagnostic phase (ii), when the instructed task has not been performed even once. In another variant, the "NEXT" paradigm of Meiran, Pereg, Kessler, Cole, & Braver (2015), participants are similarly instructed in the S-R rules for a new two-choice task in phase (i), but in phase (ii) the participant must dismiss a series of stimuli from the same set by pressing one of the two response keys to move on to the "next" until (iii) the colour changes to indicate it is time to start performing the instructed task; RTs in phase (iii) are longer when the key used for "next" responses is incongruent rather than congruent with the response specified by the instructed task on that cycle. These demonstrations certainly indicate that there is cross-talk between the instructed and the diagnostic S-R rules for a newly instructed 2-choice task. But the congruence effects in these experiments are typically small[2] compared to what we see when participants must switch between two well-acquired CRT tasks using the same response set (e.g. Monsell, Sumner, & Waters, 2003) – so may imply less than complete procedural acquisition. Nor is it quite clear yet what implication the observed cross talk has with respect to the representation of the S-R rule: does an S-R rule held only in dWM have no impact on performance? Liefooghe et al. (2012) initially found evidence suggesting that the instruction had no effect if the participant was instructed merely to remember it for recognition or recall, rather than to be ready to perform the task, or did not need to remember it because the instruction would be repeated before execution was required (Liefooghe, De Houwer, & Wenke, 2013). Wenke, Gaschler, Nattkemper, and Frensch

[2] At least in adults; see Verbruggen et al (2018) for large effects in children.

(2009) found that the instruction-based congruency effect disappeared when it was frequently indicated that the inducer task would not proceed, and Whitehead and Egner (2018) found that the effect scaled with the expected frequency of use. But further observations led Liefooghe and De Houwer (2018) to qualify their earlier conclusion: conditions in which participants had to remember the instructed rules only for a recognition test, but with a manipulation ensuring maintenance of both rules and a response deadline, led to small but reliable instruction-based congruence effects even though participants never had to execute the instructed two-choice task. The limitation of almost all these demonstrations to two-choice inducer tasks (requiring only two S-R rules to be encoded, or perhaps just one, with the other response selected by default) also raises the issue of how such claims of "intention based reflexivity" would generalise to somewhat more complex tasks.

In the experiments reported in this article, we tracked and explored the transition between a dWM representation of a novel set of S-R rules to control by a procedural representation, using a RITL paradigm inspired by that of Ruge and Wolfensteller (2010), but with a different instruction phase. Participants repeatedly acquired CRT tasks specified by a set of S-R rules mapping 1:1 between six stimuli (novel for each new task) and responses (the same six fingers for every task). So far RITL paradigms have been used largely to examine changes in brain activity over the first few trials of instruction and acquisition (e.g. Cole, Laurant, & Stocco, 2013; Demanet, Liefooghe, Hartstra, Wenke, De Houwer, & Brass, 2016; Dumontheil, Thompson, & Duncan, 2011; Ruge & Wolfensteller, 2010, 2016). But what change in the functional representation of the task rules do such changes in brain activity represent? We need a behavioural index that tracks the transition from declarative to procedural control of performance. Our strategy was to manipulate a variable expected to influence performance when it is (still) mediated by dWM. Finding an effect of this variable on performance of the task over the first few trials would suggest dWM is still playing a role in mediating retrieval of the response; the later disappearance of the effect would index the transition to responses mediated only by a procedural representation.

The variable we manipulated was phonological similarity among the names of the stimuli in a set. It is well known that we have the capacity to represent in WM a phonological sequence equivalent to five or six monosyllabic words (Baddeley 2012; Baddeley and Hitch, 1974). The use of such a representation is indicated by marked effects of phonological similarity and complexity in immediate serial recall of word lists, even with written presentation and recall (Conrad, 1964; Baddeley, 1966), as well as by effects of concurrent irrelevant articulation (Baddeley, Thompson, & Buchanan, 1975). Of course, representation in dWM is not limited to phonological codes; visuo-spatial and other codes may be represented, whether in a visuo-spatial sub-store (Baddeley, 2012), distinct visual and spatial sub-stores (Klauer & Zhao, 2004; Logie, 1995), or in some sort of domain-general work-space (Morey, 2018). We therefore chose stimuli for the CRT task such that it was likely that participants would initially exploit their phonological dWM capacity to encode the instructed S-R rules.

Our starting point was an incidental observation in a task-cuing study, in which the stimuli were four object pictures, or four letters, and participants had to switch between responding to their identity or their colour (Van 't Wout, Lavric, & Monsell, 2013). The phonological similarity of the objects' names was manipulated: they rhymed or were distinct. In the task-switching phase of the experiment, there was no detectable effect of this variable on measures of performance, or switch cost, or reduction of switch cost with preparation. But at the very beginning of the experiment, when participants practiced each task alone for just 16 trials, performance on the identity task was significantly worse for the stimulus sets with rhyming names. This is consistent with the idea that verbal dWM was used to acquire the task-set very early in practice, but played no role in reactivation of a fluent task-set later.

However, this observation was based on the acquisition of just one task per participant. We therefore embarked on manipulating phonological similarity in the same way in a RITL paradigm.

Preliminary experiments

We first mention briefly two preliminary RITL experiments which constrained the choice of stimuli and the number of stimuli per set for the main experiments we report here. Both used a CRT task with 1:1 mappings between just four stimuli and a row of four keys pressed with index and middle fingers of the two hands. In each cycle there was (i) an instruction phase, in which the four items were displayed serially accompanied by an indicator of which in the row of four response keys was the correct response to that item, and then (ii) a test phase, with one item presented per trial in a random order, with no immediate repetitions, and the participant responding with the appropriate key as quickly as possible while trying to avoid errors; if an error was made, the correct key was shown. Participants practiced acquiring a set with non-rhyming names, then a set with rhyming names, and then alternated between rhyming and non-rhyming sets.

In the first experiment[3] the stimuli in each set were four line-drawings of objects with rhyming or dissimilar-sounding monosyllabic names, as in Van 't Wout et al. (2013). S-R rules for 16 sets of stimuli were acquired, in turn, alternating between similar and dissimilar sets, with each set practiced for 12 presentations per item. Acquisition functions similar to those in Ruge and Wolfensteller (2010) were obtained but, although the experiment was adequately powered (40 participants), no disadvantage could be detected in acquisition of a set with rhyming names even early in practice. Over all 12 encounters, the mean correct RT difference (similar minus dissimilar) was -6 ms [95% CI: ±7.3]; and the difference in errors was 0.05% [CI: ±0.46]. Even over just the first four encounters with the stimuli, the

[3] conducted as an undergraduate final-year research project by Kamila Korzekwa and Kathryn Ashmore

differences were similarly negligible: for RT, -6.4 ms [CI: ± 11.7]; for errors, 0.06% [CI: ±0.81].

In retrospect, the lack of a disadvantage for a set with phonologically similar names under these conditions should not have been surprising. The 4 stimuli per set used by Ruge and Wolfensteller (2010) were not easily nameable, yet their participants evidently acquired the S-R rules rapidly. Either our participants completely proceduralised the S-R rules during the instruction phase, or they did not need to use phonological dWM. It seems plausible that they avoided use of phonology by using only visual WM (perhaps after experiencing the disadvantage of verbally encoding rhyming stimulus items in the practice set). Evidence from change-detection experiments suggests that the functional capacity of visual WM is three to four distinct objects (Luck & Vogel, 2007).

A second experiment[4] therefore sought to limit usage of visual WM by using as stimuli visually presented abstract monosyllabic nouns (abstract to minimise the ease of imaging their referents), instead of object pictures. 32 participants completed two practice cycles (one with a rhyming set) and then twenty-four cycles alternating between rhyming and non-rhyming sets, with 8 test trials per item. Now there was a large disadvantage for phonologically similar sets, but although it reduced for errors over the first 4 encounters per stimulus, there was still a substantial and apparently asymptotic effect over the last four encounters, and the effect did not significantly diminish with practice for RT. Of course, it is possible that participants were not tested for long enough for the use of phonological WM to disappear. But there is another probable reason for the persistence of the phonological similarity effect into fluent performance. Practiced CRT performance is surely influenced by the discriminability of the stimuli, and in languages with alphabetic scripts, the phonological and orthographic similarity of words is highly correlated. So the effect of stimulus similarity

[4] conducted as an undergraduate final-year research project by Bradley Wooster

in this experiment, even after fluency had developed, probably reflected a greater difficulty of discriminating the stimuli when they came from an orthographically similar set.

Main experiments

The preliminary experiments supplied two constraints on the design of the main experiments. To capture use of dWM early in practice through a manipulation of phonological similarity, we need to be sure (a) that the initial declarative representation of the task-set is likely to require at least some use of phonological dWM, and (b) that the discriminability of the stimuli (as processed after acquisition of a fluent task-set) is not confounded with phonological similarity. Hence in the experiments we now report, we returned to using object pictures as stimuli, but increased the set size to six, to exceed the estimated capacity of visual WM and encourage at least some dependence on phonological WM. We also now mixed two kinds of phonological similarity in each set: some items shared their rime[5] e.g. *jar-car*; others shared their head[5] e.g. *shed-shell*). This was partly because it is hard to find sets of 6 picturable objects whose names all share a rime, or all share a head. Also, mixing head and rime similarity discourages a strategy that some participants in the preliminary experiments identified as possible with sets sharing just a rime: recoding items' names as just their onsets.

To anticipate our findings, in Experiment 1 we obtained the expected pattern — an effect of phonological similarity over just the first few encounters with a stimulus, then disappearing — but the effect was only marginally reliable. Experiment 2 increased the power, doubling both the number of participants and the number of sets learned. Experiment 3 explored the effect of removing the instruction phase so that participants had to learn the S-

[5] In linguistics, the "rime" of a syllable is the vowel nucleus + coda (i.e. following consonants, if any). We use "head" to indicate the onset (initial consonants, if any) + vowel nucleus.

R rules by trial and error. Experiment 4 explored the persistence of the S-R representations created by instruction and just a few practice trials.

Experiment 1

Method

Participants. Participants were drawn from the School of Psychology's volunteer participant panel, students or administrative staff, all young adult native speakers of English. Twenty four participants were tested[6], of whom 18 were women; their average age was 20.4 years with a range of 18 to 28. They were paid £7.50 per session with additional bonus payments for improving their performance through the session. The programme of experiments was approved by the Psychology Ethics Committee, and informed consent was obtained.

Materials. The stimuli were line drawings of familiar objects obtained from several sources (examples in Figure 1). The objects were chosen such that their names formed 12 sets of 6 phonologically similar names (see Appendix 1). Within each set, four names shared a rime, two assigned to each hand; the name of the third object assigned to each hand shared a head with the name of one of the two objects assigned to that hand. An example of a similar set is: "ring, witch, wing, spring, king, kilt". The 12 sets were split into two matrices of 6 sets. The response key for each item corresponded to its serial position in the set. To form sets with dissimilar names, names were reordered within the columns of each matrix so as to minimise the phonological similarity within each row: no pair of items within a row shared a rime, or a head. An example of a dissimilar set is: "wing, well, bow, star, hat, cave". Odd participants learned similar sets from the first matrix and dissimilar sets from the reordered second matrix; even participants learned similar sets from the second matrix and dissimilar sets from the reordered first matrix. Hence each stimulus was used in only one set per participant, while

[6] For an exploratory experiment of this kind, a priori power is hard to assess, but for the within-participants contrast of interest — the effect of phonological similarity in each 4-trial unit of practice (see Figure 2) —the overall number of trials per similarity condition (1728) exceeded Brysbaert and Stevens' (2018) rule-of-thumb of 1600 trials (trials x participants) for modest priming effects in psycholinguistics.

each stimulus and each stimulus-to-key assignment contributed equally, overall, to the phonologically similar and dissimilar sets.

Design and Procedure. Participants were given general instructions for CRT, shown the response keys (the adjacent row x,c,v,b,n,m) and the fingers they should use to press them (ring, middle and index on each hand), and told that they would learn a number of "tasks". For each task, which they initiated by pressing the space bar when they felt ready, there was first an instruction phase in which they were shown how to map the six new object pictures onto the six keys, and then a test phase. (See Figure 1).

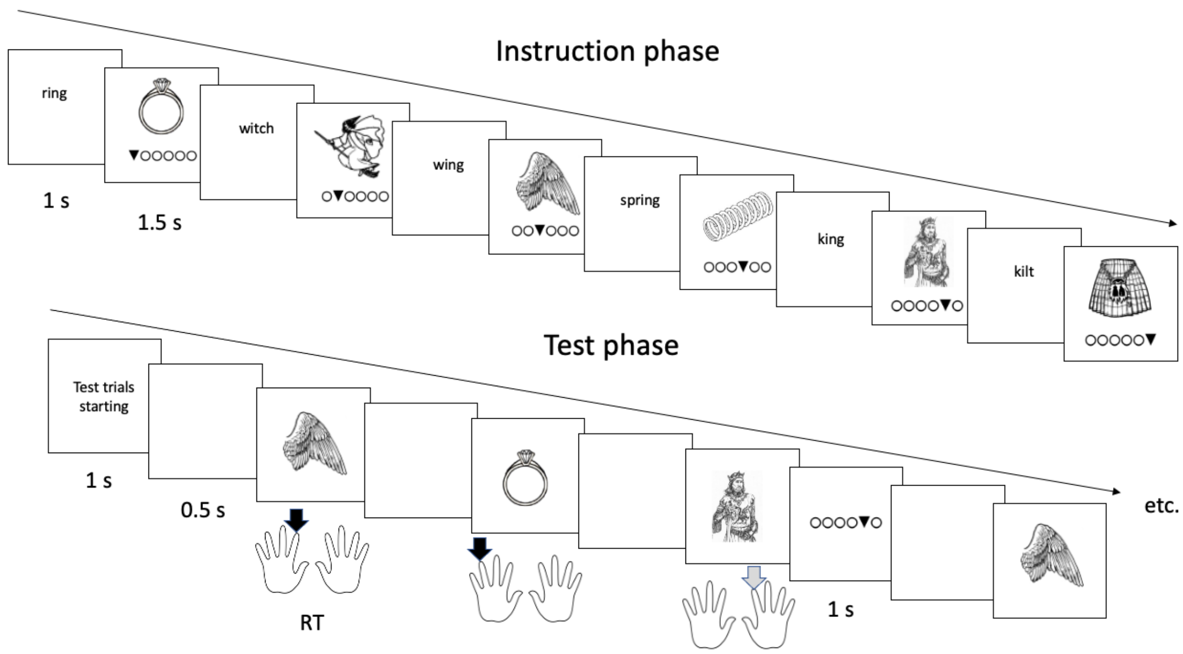

Figure 1. Instruction phase for a task, immediately followed by the test phase. Display durations in the instruction phase are as for Exp 1. Drawings are not to scale. The line drawing, presented centrally on the screen, fit within an area 8.0º x 7.6º; the key icon (4.5º x 1.3º) was presented 8.4º beneath the centre in the instruction displays, and centrally as feedback after an erroneous response.

The instruction phase comprised a presentation, for each stimulus in turn, of the name of the object (e.g. "ring"), shown for 1 s, followed by the line drawing of the object shown for 1.5 s accompanied by an icon indicating the associated key. The stimuli were displayed in the left to right order of the six response keys. There was then a 1 s display — "Test trials starting" — before the first test trial. Thus the instruction phase lasted 16 s. We preceded each picture with its name to maximise the likelihood that the participant, if using a name to represent the object in dWM, would use that name rather than some other.

The test phase for each task comprised 144 trials. On each trial, there was a blank interval of 0.5 s followed by display of the object in the centre of the screen until either the participant pressed a key or 3 s had elapsed. If they pressed the wrong key, they were shown the correct key for 1 s. (See Figure 1.) Each stimulus was presented 24 times, in a random sequence subject to two constraints: there were no immediate repetitions (as these permit participants to respond by detecting the repetition and repeating the previous response rather than by identifying the stimulus and retrieving the associated response – cf. Schneider & Anderson, 2011); each stimulus occurred four times in each sub-sequence of 24 trials. The test phase was split into three blocks of 48 trials. There was a timed rest break of 5 seconds between blocks, signalled by "Brief interval" for 4 s and "Block 2 [or 3] starting" for 1 s.

Between tasks, participants pressed the spacebar to start acquisition of the next set of S-R rules. The session began with two practice tasks, the first with a stimulus set with dissimilar names, the second with a set with similar names, each with 48 test trials (8 encounters per stimulus). Six experimental tasks were then acquired, 3 with stimulus sets selected from a similar matrix and 3 with stimulus sets selected from a dissimilar matrix constructed, with the selections rotated through the matrices so that overall each S-R rule was used equally often in the similar and dissimilar sets, and only once per participant. Similar and dissimilar sets alternated in a balanced order.

After the two practice tasks, participants were told that their performance in each block would thenceforward receive a score (5 per error plus RT in ms divided by 10) and that they would earn an extra 15p for each block in which their score was better than their average score in the equivalent block of all the tasks to date. The scores and bonuses per block were displayed at the end of each task.

Results

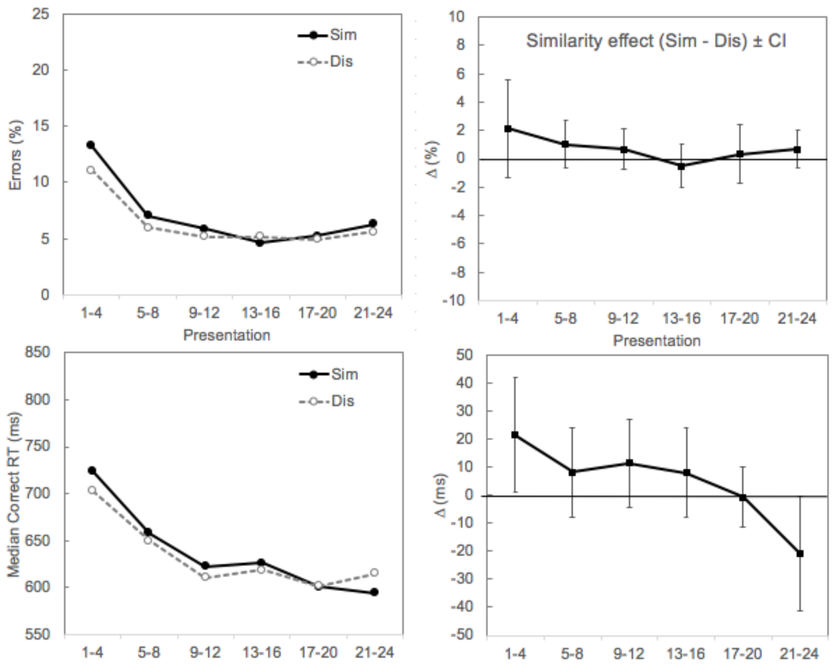

Figure 2. Left hand panels: acquisition functions: errors and median correct RT as a function of practice (per 4 presentations of the stimuli). Right hand panels: The effect of phonological similarity (similar minus dissimilar) and 95% CIs.

The left panels of Figure 2 show the acquisition functions (median correct RT[7] and error rate as a function of practice, averaged over the first 4 presentations per item, the second 4, and so on) for task sets with phonologically similar and dissimilar names. Performance improved rapidly over the first 8 trials or so, but then became more stable. The right panels of Figure 2 show the effect of phonological similarity at each stage of practice, with 95% confidence intervals. As the latter indicate, for trials 1-4, the effect reached a significant level for RT, $t(23) = 2.161$, $p = .041$ for RT , but not for errors. The effect then diminished, and by the last 8 trials of practice, there was no detectable disadvantage for phonologically similar stimulus sets. An ANOVA[8], with factors similarity and practice (6 levels), on the error rates, showed a reliable effect only of practice, $F(5,115) = 18.09$, $p < .001$, $\eta_p^2 = 0.45$; neither the effect of similarity $F(1, 23) = 2.03$, $p = 0.168$, nor the interaction

[7] We used median correct RTs as more robust to outliers; results were generally very similar for mean correct RTs, but slightly noisier.
[8] Huynh-Feldt corrected probabilities are shown, with uncorrected degrees of freedom.

with practice, F<1, was reliable. For the median correct RT, practice had a reliable effect $F(5,115) = 53.17$, $p < .001$, $\eta_p^2 = 0.70$; the main effect of similarity was not significant, F<1, but its reduction with practice was, $F(5,115) = 3.81$, p= .013, $\eta_p^2 = 0.14$. However, this appears in part to derive from the (possibly spurious) negative effect in the last four trials. If the data for the last four trials are excluded, the interaction is no longer reliable, $F(4,92) = 1.40$.

Discussion

The early effect of phonological similarity is consistent with performance relying, at least in part, on mediation by verbal representations in dWM during the first few trials after instruction. This effect diminished and disappeared with just a few more trials of practice, suggesting that verbal mediation was no longer needed. But evidently the effect is quite small, and of marginal reliability; more power is needed.

Experiment 2

This was similar to Experiment 1, but with twice the number of participants, each acquiring twice as many tasks. To keep the session length within bounds, and in the light of the acquisition function obtained in Experiment 1, we reduced the number of practice trials per task from 24 to 16 trials per item. We also added an exploratory manipulation of the rate at which the stimuli were presented during the instruction phase, motivated by the intuition that a more rapid presentation of the stimulus set would allow less time for anticipatory proceduralisation, hence delaying the disappearance of the phonological similarity effect.

Method

We tested 48 participants, of whom 35 were women, mean age 21.0 years (range 18-47), selected from the same population. For a one-tailed repeated-measures t-test of a d_v=0.4 effect (of phonological similarity) (p<.05, 0.8 power), G*Power (Faul et al, 2007) indicates a minimum of 41 participants (Brysbaert, 2019). In addition, we increased the power and precision by doubling the number of trials per participant per practice unit of 4 trials. Quadrupling the number of trials overall halves the expected confidence intervals. Each participant acquired one practice task, and 12 experimental tasks. During the instruction phase, the object's name was now presented for 0.75 s, and the picture and the response icon were presented for 0.75 s for half the tasks and 1.5 s for the other half. Thus in each similarity condition, the participant had either 10 s to encode the instructions or 14.5 s (including the 1 s display before the test trials started). These "fast" and "slow" instruction rates alternated every two tasks in a balanced order; one of the two tasks used a similar set of items, and one a dissimilar set, again in a balanced order.

For half the participants the similar sets were those in the six rows of Matrix 1 (see Appendix 1), and the dissimilar sets were those in the reordered Matrix 2; for the other participants, the similar sets were those in the rows of Matrix 2, and the dissimilar sets from the re-ordered Matrix 1. Hence, once again, each S-R pair occurred equally often in similar and dissimilar sets, while each stimulus object was used in only one task per participant. The single practice task used the slow instruction rate, and a set of objects with names of intermediate similarity ("tree, brick, key, tape, tail, fan").

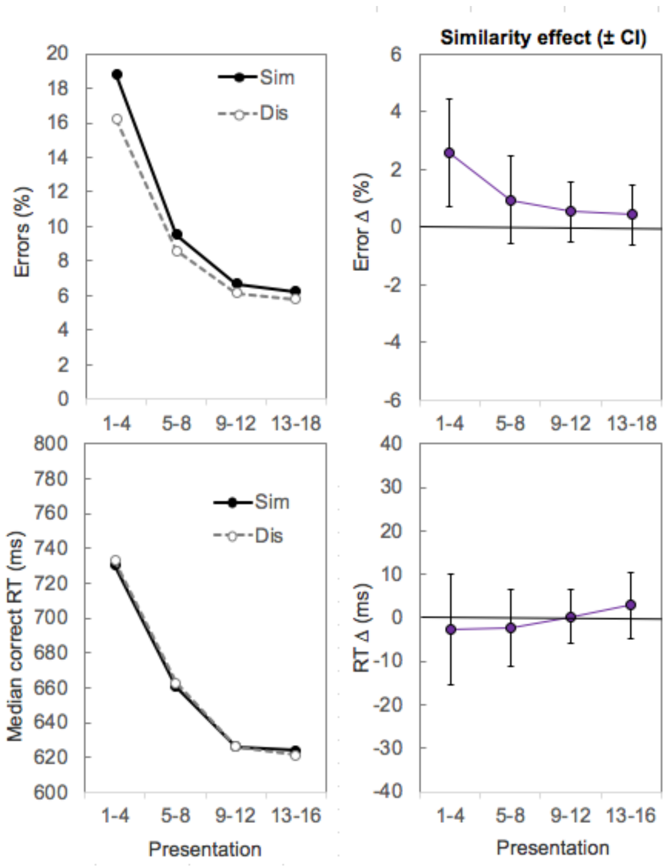

Figure 3. Left panels: Acquisition functions for sets with similar and dissimilar names. Right panels: effect of phonological similarity (similar minus dissimilar), with 95% Cis, as a function of practice.

Results

The left panels of Figure 3 show the acquisition functions for sets with phonologically similar and dissimilar names. Performance again improved rapidly, appearing near asymptotic after only about 8 trials on each S-R rule. An ANOVA with the factors practice, similarity, and instruction rate showed a large practice effect both for error rate, $F(3,141) = 186.8$, $p < .001$, $\eta_p^2 = 0.799$ and for correct median RT, $F(3,141) = 250$, $p < .001$, $\eta_p^2 = 0.842$. The top right panel of Figure 3 shows that error rates showed a reliable similarity effect over the first four trials, but the effect diminished rapidly. The overall effect of similarity on error rate was significant, $F(1,47) = 4.64$, $p = .036$, $\eta_p^2 = 0.090$, as was its reduction with increasing practice, $F(3,141) = 3.58$, $p = .022$, $\eta_p^2 = 0.071$. There was no detectable main effect (F<1) of similarity on RT, nor its interaction with practice, F<1.

As may be seen in Figure 4, these effects were somewhat modulated by the time participants had to encode the instructions, though these modulations were at best only marginally reliable. A slower presentation led to slightly more accurate performance, $F(1,47) = 3.58$, $p = .065$, $\eta_p^2 = 0.071$, and a smaller effect of phonological similarity, $F(1,47) = 2.94$, $p = .093$, $\eta_p^2 = 0.059$. The effect of similarity on errors appears limited to the first 4 presentations for the slow presentation of instructions, and more persistent for the fast presentation rate, but the three-way interaction was not significant, $F<1$. For RT, there were no reliable effects of presentation rate, all Fs<1.

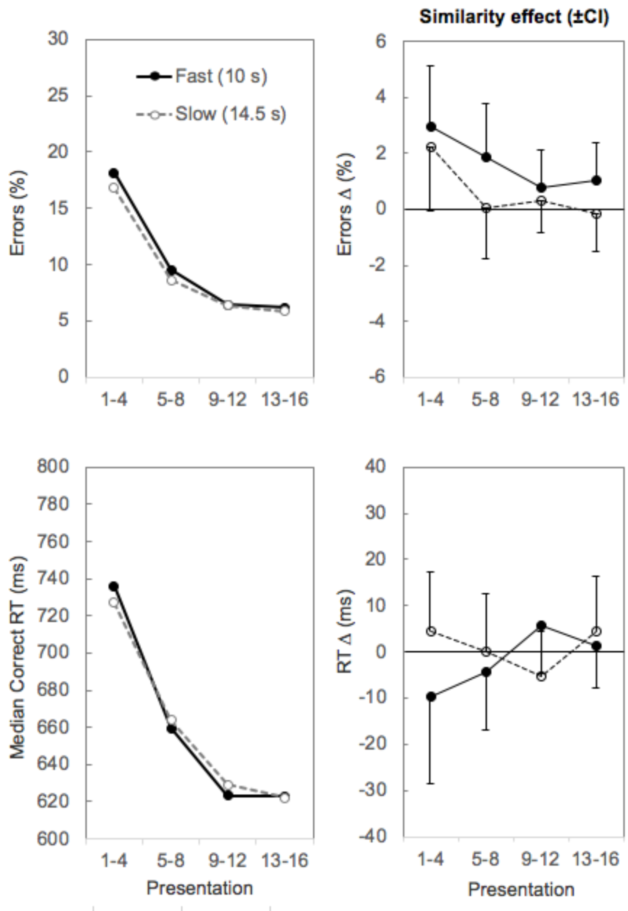

Figure 4. Left panels: Acquisition functions for fast and slow instruction phase. Right panels: the effect of phonological similarity (similar minus dissimilar), with 95% Cis, as a function of practice for fast and slow instruction phase.

As explained below, the effect of serial position in the stimulus set may provide clues to the representation(s) mediating performance. Figure 5 shows the effect of left to right serial position in the response set at successive stages of practice. During the first 4 encounters with the stimuli, the serial position functions for both errors and RT showed a marked primacy effect, monotonic up to the fifth position, but by the last four trials, the serial position effect has an approximately symmetrical inverted-U shape. In an ANOVA with the factors practice, similarity and serial position, the interaction of serial position and practice was highly significant for errors, $F(15,705) = 14.14$, $p < .001$, $\eta_p^2 = 0.231$, and for RT, $F(15,705) = 8.42$, p < .001, $\eta_p^2 = 0.152$. For the first four encounters with the stimuli, the error rates show a significant linear trend, $F(1,47) = 65.29$, p < .001, $\eta_p^2 = 0.581$, with a mean slope of 1.87 % [CI: ±0.47] ; for the last four encounters, the linear trend is only marginally reliable, $F(1,47) = 4.00$, $p = .051$, $\eta_p^2 = 0.078$, and its slope is only 0.2 % [CI: ±0.2]. Similarly, for the first four encounters the RTs show a significant linear trend, $F(1,47) = 24.50$, $p < .001$, $\eta_p^2 = 0.386$, with a mean slope of 11.6 ms [CI ±4.3] , while for the last four, the linear component is not reliable, $F(1,47) = 2.37$, p =.13, with a slope of 1.6 ms [CI ±2.0].

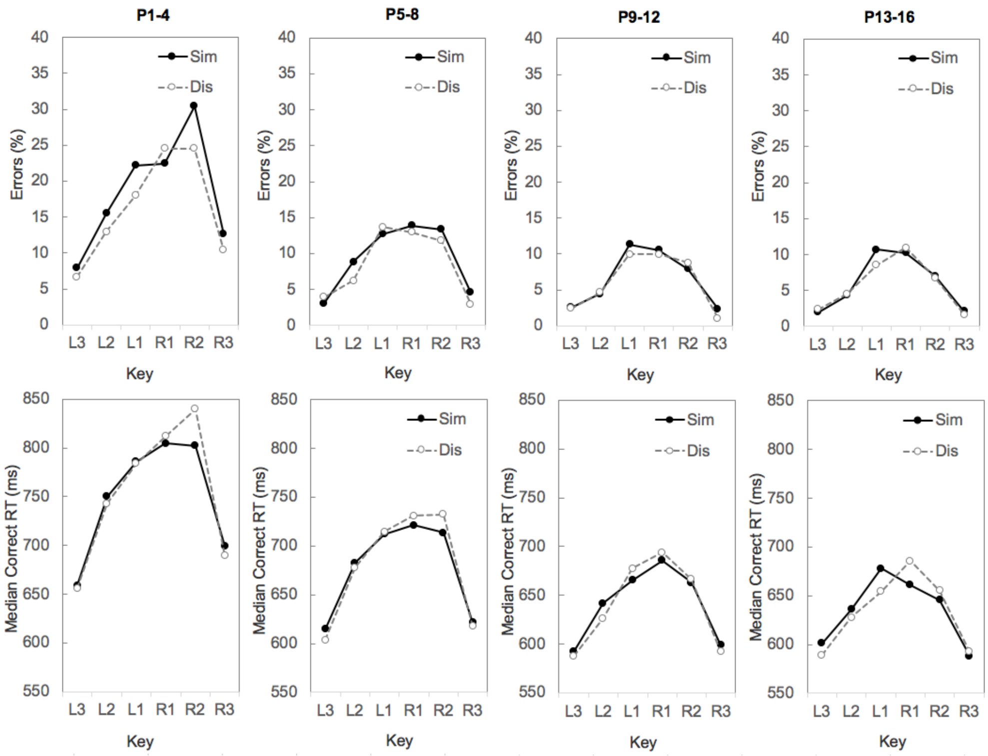

Figure 5. Effect on performance of the left-to-right serial position of the response associated with the stimulus, for the first four presentations of the items (P1-4), the next four (P5-8), etc.in the response set. L3, L2, L1 are ring middle and index fingers on the left hand, R1, R2, and R3 index middle and ring fingers on the right hand.

Discussion

Phonological similarity of the names of the items clearly increased the error rate over the first few test trials, implying that verbal representation of the stimulus set in dWM was contributing to retrieval of the responses. But this effect diminished rapidly with further practice so that it was no longer reliable after four test trials, suggesting that performance soon ceased to rely on verbal mediation. Our working hypothesis is that a non-verbal procedural representation had largely assumed control at this point. The effects of presentation rate during the instruction phase were consistent with the idea that the more time the participant had to encode the instructions, the more advanced the process of proceduralisation was. Perhaps proceduralisation happens faster because more encoding time leads to more robust (interference-resistant) WM representations (Barrouillet, Plancher, Guida, & Camos, 2013) that can drive faster proceduralisation. Better data are needed on this point.

The way we presented the S-R rules permitted participants to encode the S-R rules in dWM by representing the names of the stimuli as a list or lists in dWM, and thus take advantage of the sequential structure of phonological representations in WM; i.e. finding the position of the name in a list of the names specifies the position of the response key in the row of six. Hence the serial position effects are of particular interest. The transition between a function with a marked left-to-right increase during the first four trials of practice to a symmetrical inverted-U after about eight trials provides additional evidence for a transition to a different representation of the S-R rules.

There is evidence that when participants are presented with a sub-span list of words to retain for an immediate probe requiring the retrieval of order information, participants search forwards through the list (at a rate consistent with that of sub-vocal speech). Sternberg (1967,

1969) found that when the probe is an item from the list and its successor must be named, RT increased with set size by 124 ms/item on average, the serial position functions were monotonic increasing, and for some participants colinear, with a slope of 250 ms, suggesting a slow self-terminating forward search. Anders and Lillyquist (1971) argued from the rate of backward serial recall relative to the rate of forward serial recall that each item in backward recall had to be retrieved by starting again at the beginning, i.e. that verbal WM must be scanned in the order of entry (as proposed also by Conrad, 1965). In Experiment 2, the serial position functions for the first four presentations of the stimuli increased monotonically up to the fifth position, consistent with participants using a forward list search, at least some of the time, to retrieve the response position. What about the rapid responses on the sixth position? It is possible that the S-R rules in the extreme list positions are proceduralised first; or, possibly the final item gets a better representation in declarative LTM relative to the rest. We do not think that in our CRT task there was a pure list search process for n trials, and then something else. Rather we suggest that, over the first few trials, participants used a variable mixture of already proceduralised representations of the S-R rules and scanning through fragments of name sequences in dWM, until further practice rendered use of phonological WM redundant. Arguably there was enough reliance on a search of phonological WM for its characteristic forward search signature to be manifest in the effects of serial position on the first few test trials.

Experiment 3

The modest effect of phonological similarity in Experiments 1 and 2 during the first four encounters with the stimuli, and its rapid disappearance after exercising the S-R rules just a few times, suggest that much of the work of compiling the rules into procedural memory was accomplished during the instruction phase. If this is correct, then omitting the instruction phase, and requiring the participant to acquire the rules by trial and error alone, should substantially prolong the period during the test phase when performance is mediated (in part) by verbal representations in dWM, and hence the effect of phonological similarity should take longer to disappear. To test this, we ran an experiment just like Experiment 1, except that the instruction phase was replaced with a "familiarisation" phase, in which the 6 items of the next task's set were introduced as in that experiment, but in a random order and with no indication of which response was to be made to each item; this they had to discover by trial and error during the test trials.

Method

Thirty six new participants were drawn from the same population; 25 were women, and the average age was 19.7 years (range: 18-27).[9] The details were otherwise identical to Experiment 1, except that the instruction phase was replaced with a familiarisation phase in which the stimuli were presented in the same way and with the same durations as in Experiment 1, but in a random order and without the response icon, so that no S-R rules could be inferred.

[9] Exp 3 was run before Exp 2. Relative to Exp 1, we expected a more substantial (and persistent) effect of phonological similarity than in Experiment 1; to achieve adequate power we increased the number of participants to 36.

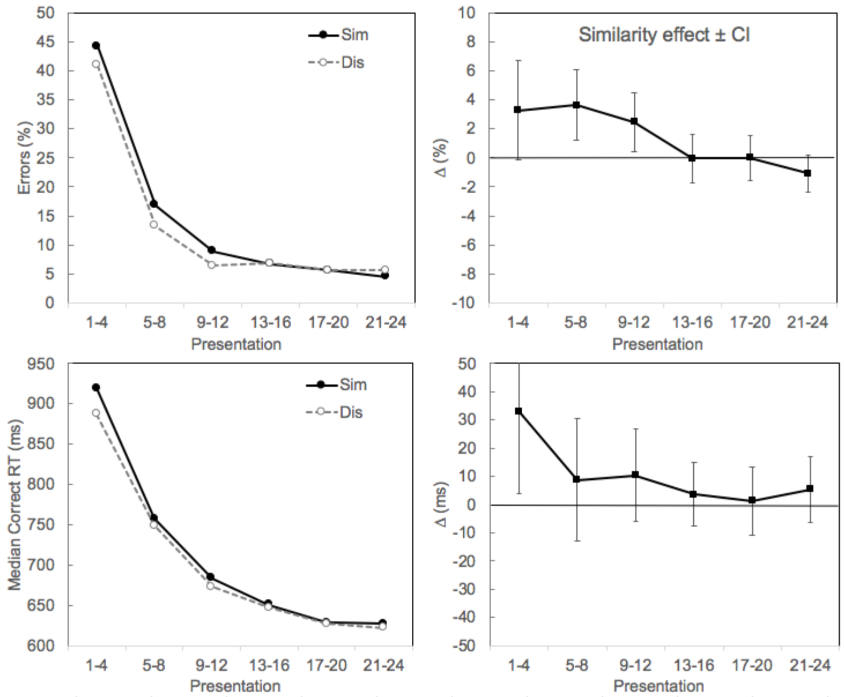

Figure 6. Left hand panel: acquisition functions for sets with similar and dissimilar names; right hand panel: effect of phonological similarity. [As for Experiment 1 in Figure 3.]

Results

The left-hand panels of Figure 6 show the acquisition functions. As one would expect, performance was error-prone over the first few encounters with the stimuli, while the S-R rules were discovered from error feedback. But after 20 or so encounters with the stimuli, performance appeared asymptotic, and as accurate as, and only slightly slower than, during the last few trials of Experiment 1, when participant had 16 s to learn each set of S-R rules before the same series of test trials began (see Figure 2). As the right-hand panels of Figure 6 show, phonological similarity of the names of the items lead to more errors and slower responses over the first 12 encounters with the stimuli, but this difference disappeared on the last 12. ANOVAs with the factors phonological similarity and practice showed significant effects of practice for errors, $F(5,115) = 497.0$, $p <. 001$, $\eta_p^2 = 0.934$, and for median correct RT, $F(1,115) = 125.5$, $p < .001$ $\eta_p^2 = 0.782$. The main effect of phonological similarity was marginally reliable for errors, $F(1,35) = 4.13$, $p = .050$, $\eta_p^2 = 0.108$, and RT, $F(1,35) = 3.49$, $p = .070$, $\eta_p^2 = 0.091$, but the reduction with practice was highly significant for errors, $F(5,115)$

= 4.71, p = .004, η_p^2 = 0.119 and marginally so for RT, $F(5,115)$ = 2.21, p = .082. η_p^2 = 0.083. The pattern can be captured by comparing the first and second halves of practice with a set: for the first 12 encounters, similarity increased the error rate by an average of 3.1 % [CI: ±2.1] and the RT by an average of 17.3 ms [CI: ±17.3]; for the last 12 encounters, the effect of similarity was -0.4 % [CI: ±1.2] for errors, and 3.5 ms [CI: ±7.6] for RT. The contrast was reliable for errors, $t(35)$ = 3.10, p = .004, but only marginally so for RT, $t(35)$ = 1.71, p = .097.

Discussion

The phonological similarity effect early in practice clearly indicates verbal mediation of performance, which could apparently be dispensed with after only a dozen or so encounters with each item, after which performance was accurate and relatively stable. Again this arguably reflects a transition from the partial use of dWM for representation of S-R rules and retrieval of responses to mediation of performance by compiled representations of these rules in procedural memory alone.

This pattern of results is highly consistent with effects of articulatory suppression reported by van 't Wout & Jarrold (2020). They conducted similar experiments in which sets of 5 picture-to-key associations were learned by trial and error, over 40 encounters with each item, with concurrent irrelevant articulation, a foot tapping control, or no concurrent task, in each half of practice. Articulatory suppression impaired performance if required during the first 20 encounters with the stimuli, but not if concurrent with the second 20, suggesting, again, that initially participants used verbal mediation to encode and exercise the S-R rules as they discovered them, but rapidly transitioned to using a non-verbal, presumably procedural, representation.

A comparison of the time course of the phonological similarity effect in Experiment 3 and the previous two experiments, in which the effect of similarity was largely limited to the first four encounters, suggests that, to a rough approximation, being explicitly instructed in the S-R rules brought forward the point at which performance was no longer verbally mediated by about 8 encounters per stimulus.

Experiment 4

If the initial effect of phonological similarity in Experiments 1-3, and that of articulatory suppression (van 't Wout & Jarrold, 2020), reflect some mediation of performance by verbal representations in dWM, then it would appear that the disappearance of the effect after only a small amount of practice reflects a transition to performance based on some other representation of the S-R rules. What other representation? Our working assumption has been that after a few trials the S-R rules are sufficiently compiled into an effective representation in procedural long-term memory (LTM) to drive performance. However, there are at least two other possibilities. One is that the S-R rules are encoded rapidly into some other non-verbal format in dWM – perhaps visual WM, or Baddeley's (2000) "episodic buffer", and that this other dWM representation is not only sufficient to control performance but more efficient than verbal rules, and so supersedes them. The other possibility is that the rules are rapidly encoded into a procedural component of WM, which then mediates performance.

In the model of Oberauer and colleagues, for example, procedural WM (the "bridge"), is distinct from both dWM and from active representations in procedural LTM. Even when a well-practiced task (i.e. well represented in procedural LTM) is performed, S-R rules must be held in the bridge —a central capacity-limited component for building task-set structures through temporary bindings (Oberauer et al, 2013) — to control performance. The model does not specify how quickly a representation would be created in procedural LTM in an experiment like ours, nor what role procedural WM plays in the creation of a representation in procedural LTM. One possible scenario is that, while instruction and a few trials of practice is sufficient to create an effective representation in procedural WM, it takes many more exercises of the S-R rule to build an effective representation in procedural LTM. If, after instruction and only a few trials, an effective representation has been established only in procedural WM or in non-verbal dWM, it should be displaced or overwritten when a new task has to be performed. But a representation in procedural LTM should persist over an interval during which one or more other tasks have to be performed.

In Experiment 4, to explore the persistence of the S-R rules formed by instruction and a small amount of practice, we adapted our RITL paradigm to look for associative interference. A stimulus for which an S-R rule was learned in one set could reappear later, after the learning of several other sets, in a new set and mapped to a different response. If a persistent and procedurally effective representation of the original S-R rule is created by instructions plus a few trials of practice, then we would expect slower acquisition of the new S-R rule than for an otherwise equivalent stimulus not previously encountered, due to competition with the previously learned association.[10] Given the evidence from Experiments 1 and 2 that verbal mediation was restricted largely to the first 4 practice trials after instruction, we compared S-R acquisition with novel stimuli to acquisition with stimuli that had previously received 4, 8, 16 or 32 trials of practice on a conflicting S-R rule. In the interim, participants were instructed and tested on between one and nine other task-sets — surely enough to eliminate any representation of the previous S-R rule in WM, whether declarative or procedural.

Method

Twenty-four new participants were tested, of whom 21 were women; their average age was 21.1 years (range 18 to 27). Each acquired 12 tasks, composed as follows: a *practice* task,

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[10] This is analogous to the congruence manipulation used to demonstrate "instruction-based reflexivity" (Meiran et al, 2017) but over a much greater time-span.

with 8 test trials per stimulus (48 trials per task); a series of four tasks we will call the *training* series; a *filler* task with 16 trials per stimulus (96 trials); and finally a series of six tasks we will call the *transfer* series.

For each task participants were instructed in and practiced 6 S-R rules as in Experiment 1, except that the name of the object was not presented before the line drawing. The instruction phase lasted 10 s (1.5 s per image + 1 s) — the same as for the "fast" instruction rate of Experiment 2. Participants were initially given CRT acquisition instructions as in those experiments. Then, after the practice task, they were warned that the number of test trials per task would vary considerably, and that at the end of the test phase for each task, they should "forget the picture-key mappings because the same stimuli may be used again later in the session, assigned to different keys." Before the filler task they were told that in the rest of the session some of the pictures would indeed be used again, assigned to different keys, and that the next task would give them some practice on a set with a mixture of old and new stimuli. (The stimuli included some from the initial practice set, assigned to new responses.) After this filler task, participants were told that the target score they were now trying to beat was set by performance in that task.

The training series comprised, in a balanced order: one task with only 4 test trials per stimulus (24 in all), one with 8 (48 in all), one with 16 (96 in all), and one with 32 (192 in all). Each of the six task-sets of the transfer series was practiced for 24 trials per stimulus (144 per set). In each set of six stimuli in the transfer series, two stimuli were novel, and the other four had been encountered during the training series, associated with different responses; one had been practiced for 4, one for 8, one for 16 and one for 32 trials in training (and thus had not been practiced together in the same training set). Over the six tasks of the transfer series, each serial position in the transfer series had one instance of a stimulus previously practiced 4, 8, 16 and 32 times with a conflicting response and two novel stimuli.

Each serial position in the training series occurred once for each degree of prior practice. The combinations of serial position in the training series and transfer series were otherwise random. Within these constraints the lag between learning an S-R rule in the training series and learning a competing response in the transfer series was random, filled with between one intervening instruction cycle on another set + 96 test trials (the filler task), and up to nine intervening instruction cycles + up to 1152 test trials. The critical contrast in this study compares performance on transfer trials for novel items to performance on items that were previously practiced 4 times with a competing associated response. For the latter condition we have 144 trials per participant, 3456 in all (6 transfer items x 24 trials each x 24 participants), and twice that for the novel items.

Forty-eight object pictures, chosen for unrelated semantics and dissimilar names, were used, 36 for the training and transfer sets, 6 for the practice task, and 6 for the filler task (see Appendix 2 for a list).

Results

The left-hand panels of Figure 7 show the acquisition functions[11] for stimuli previously trained 4, 8, 16 or 32 times with a conflicting response, and for novel stimuli. Performance appears asymptotic by the last 8 presentations of the stimuli. Not surprisingly, a large amount of prior training on a conflicting response generated a large degree of conflict. More interesting for present purposes is that as little as 4 trials per stimulus of prior practice with a conflicting response was sufficient to impair acquisition of a new S-R rule.

[11] We analysed mean rather than median correct RTs for this experiment because there were twice as many trials for novel stimuli as for the stimuli with each level of prior practice; medians are biased with substantially unequal Ns (Miller, 1988).

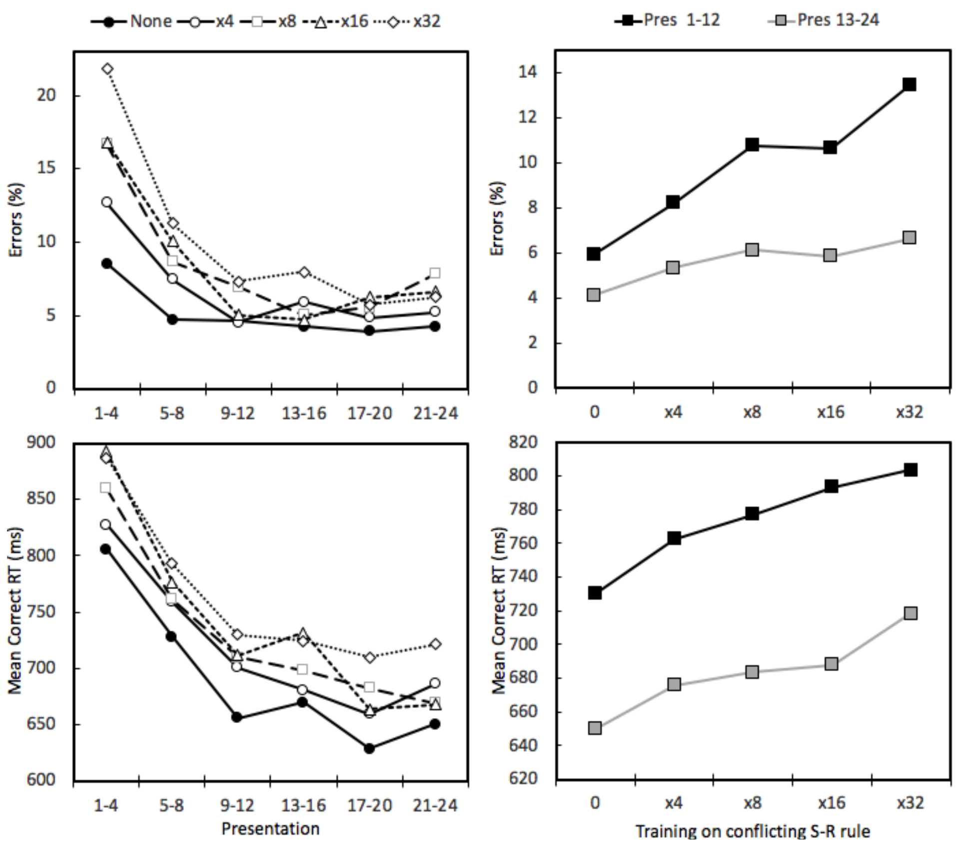

Figure 7. Left hand panels: acquisition functions during the transfer series for novel stimuli (the thick solid line) and stimuli previously trained on a conflicting response with varied amounts of practice. Right hand panels: average performance on these stimuli in the first 12 and last 12 encounters during the transfer series.

To summarise this pattern, the right-hand panels of Figure 7 average performance over the first 12 and the last 12 encounters in the transfer tasks, for stimuli subject to each degree of training on a conflicting S-R rule and none. ANOVAs with these factors showed significant increases in error rate, $F(4,92) = 11.85$, $p < .001$, $\eta_p^2 = 0.340$, and mean correct RT, $F(4,92) = 13.72$, $p < .001$, $\eta_p^2 = 0.374$, the more training there had been on the conflicting rule. Crucially, even stimuli with just 4 training trials on the conflicting rule reliably increased the error rate by 2.13 % [CI: 1.57] relative to novel stimuli, $F(1,23) = 8.29$, $p = .008$, $\eta_p^2 = 0.265$, and increased mean correct RT by 27 ms [CI: ±19.5], $F(1,23) =13.48$, $p < .001$, $\eta_p^2 = 0.370$. As one might expect, the impact of prior training on a competing association diminished in the second half of the test trials, after more practice on the new associations: for errors,

$F(4,92) = 4.20$, $p = .004$, , $\eta_p^2 = 0.154$; for RT, $F<1$. (However, when limited to stimuli with just 4 versus 0 training trials, this interaction was reliable for neither errors nor RT, *F*s<1.)

The lag between training and transfer on the conflicting associations varied between one intervening task (the filler task) and up to nine intervening tasks. To examine the effect of lag we divided the transfer series into first and second halves (three tasks in each). Figure 8 shows the conflict effect (subtracting performance on novel items), and its confidence interval, for each degree of prior training in the first and second halves of the transfer series. For stimuli with only 4 trials of training on a conflicting association, the conflict effect was substantial and robust at the shorter lag, but almost disappeared at the longer lag. The more prior training the conflicting association received, the more robust was the conflict effect at the longer lag.

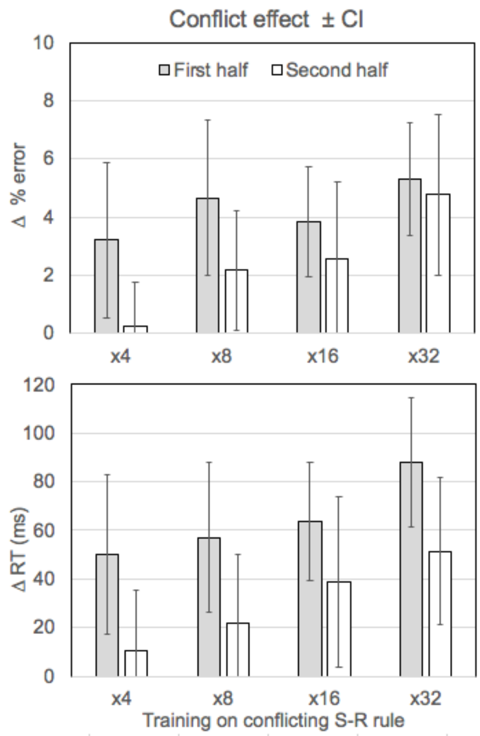

Figure 8. The effect of conflict from a competing S-R rule trained earlier in the session (error rate and RT subtracting those for novel stimuli), and 95 % confidence intervals, for the first and second half of the transfer series.

Discussion

The evidence from Experiments 1 and 2 suggested that after instruction and just 4 trials of practice per stimulus, participants had largely dispensed with verbal mediation for performing a six-choice CRT task. The results of Experiment 4 indicate that even after so small an amount of practice, a long-term and effective procedural representation of the S-R rules had been created: long-term, because it survived the acquisition and extended practice of several other unrelated sets of S-R rules (on average 4 to 5 sets in the first half of the transfer series) — a persistence well beyond any plausible conception of the persistence of activation in WM after its contents have been overwritten by other content; effective, because it interfered substantially with the acquisition of a conflicting rule. But, although long-term and effective, the trace created by so little practice was evidently more fragile than it became after more practice: only after 16 or 32 training trials on the conflicting rule was conflict reliably detectable in the second half of the transfer series.

General Discussion and Conclusions

The experiments examined the early stages of acquisition of S-R rules for a six-choice reaction time task through practice with error feedback, with and without explicit instruction on the S-R rules before practice. The task was designed so that initial maintenance of the rules would be beyond the capacity of visual WM, but within the capacity of phonological WM, and so that the S-R rules were phonologically codable in WM by maintaining a list (or partial lists) of the stimuli as a sequence of names. Under these conditions, three effects indicate that verbal mediation – use of phonological WM – indeed contributes to performance during the first few test encounters with the stimuli: more errors when the names of the stimuli are phonologically similar (Experiments 1 and 2), a serial position function showing marked primacy (Experiment 2)[12], and interference from concurrent irrelevant articulation (van 't Wout & Jarrold, 2020). For our student participants, a six-word sequence of monosyllabic names sufficient to encode the S-R rules should generally have been within their WM capacity, but in a broader population, with substantial variation in capacity, including effects of age, pathology, and education, we might expect the progress of task-set proceduralisation to be dependent on the dWM capacity available for representing the task-set parameters and rules.

These three effects rapidly disappeared with further practice, suggesting that retrieval of responses from memory by then relied entirely on a non-verbal, arguably procedural, representation. In our Experiments 1 and 2, when instructions presented the S-R rules one by one before practice began, this transition occurred very rapidly (after only ~4 encounters per stimulus). When the S-R rules had to be inferred by trial and error without instruction (Experiment 3 and van 't Wout & Jarrold, 2020) — the transition occurred later, albeit still quite rapidly (after ~12 trials per stimulus in our experiment). This implies that when instructions were provided, considerable proceduralisation must have been accomplished during the instruction phase, before any overt exercise of the S-R rules. To this extent, the claims of Brass et al (2017), Liefhooghe et al. (2012), Meiran et al. (2015, 2017) and others, that for a two-choice task, instructions are sufficient to produce "intention based reflexivity" or "automatic" activation of the response by the stimulus, are extended by our data from a more complex task. With our six-choice task, and no more than 16 s to digest the instructed presentation of the rules, very little actual practice appears necessary to complete proceduralisation. In all these cases, it is entirely likely that, during instruction, participants

[12] The serial position functions in Experiment 1 and 4 (omitted for brevity) showed a similar transition from a marked to a negligible linear trend.

covertly simulated the response to each stimulus through motor imagery, facilitating proceduralisation (cf. Theeuwes, Liefooghe, Schryer, & De Houwer, 2018).

We know less, as yet, about the representation mediating performance after the transition to fluent performance. Experiment 4 established that instruction plus just 4 trials of practice is sufficient to create a robust representation of the S-R rules – one persistent enough to survive the acquisition and practice of several other sets (coupled with an instruction to forget), and still interfere with the acquisition of another task-set containing a competing S-R rule. One straightforward possibility is that not only is this S-R representation effectively established in (relatively) long-term procedural memory after a few trials practice, but it is this representation that directly mediates performance of the CRT task thereafter.

There are at least two other possible accounts. One is that, although a long-term procedural representation strong enough to cause interference many minutes later is rapidly created, performance after the first few trials is mediated by some other type of representation in dWM (such as a visuo-spatial code or transient bindings in an "episodic buffer"). This proposal seems unparsimonious and implausible as well as unspecific. If such a dWM representation exists and is an effective mediator of performance (even more effective than the developing procedural representation), why is it not relied on from the beginning of practice?

The other possible account relies on the distinction made, for example, by the theory of Oberauer and colleagues (Oberauer, 2009; Oberauer et al, 2013) between procedural WM and procedural LTM. According to this account, initial instructions and practice may indeed build representations of the S-R rules in procedural LTM, but to actually execute the task, they must be represented in procedural WM, also known as the "bridge". When a person switches between tasks, the rules for the new task must be loaded from procedural LTM into the bridge, displacing the rules for the previous task.[13] Activated rules in procedural LTM can to some extent activate responses without being in the bridge; this is how the theory interprets response congruence effects in many paradigms. Hence for the present situation, it would appear to follow that, once the participant stops relying on dWM (a few trials into practice) performance is mediated by S-R rules in procedural WM. But the S-R rules are presumably also represented, with gradually increasing strength, in procedural LTM, and it is this long-term representation that later causes interference when the participant must learn to perform another task containing a competing S-R rule. Debate on the Oberauer theory has so far focused largely on the issue of whether declarative and procedural WM are indeed independent capacities (e.g. Barrouillet, Corbin, Dagry, & Camos, 2015; Formica, González-García, & Brass, 2020; Gade, Druey, Souza, & Oberauer, 2014). As yet little direct evidence exists on whether procedural WM and procedural LTM are indeed separate stores – in contrast to the rich evidence available to the long-running argument about whether dWM (or "short-term memory" as classically conceived) is merely activated representations in declarative LTM or a separate store. (For a recent sample of the debate, see Cowan, 2019; Norris, 2017, 2019.) Some of the same *a priori* computational arguments apply: mere activation of pre-existing long-term traces cannot represent novel structures, multiple tokens of the same type, and variable bindings (Monsell, 1984; Norris, 2017; Oberauer, 2009, 2010). And if one accepts that dWM is distinct from declarative LTM, then the finding of parallel phenomena in, on the one hand, selection of or within subsets of items in dWM and, on the other, selection of or within task-sets in procedural memory (Gade et al., 2017; Oberauer et al., 2013; Souza et al., 2012), may be seen as supporting a parallel distinction between

[13] Van 't Wout et al. (2015) reported task-switching data suggesting that the time to retrieve one of several task-sets in play into (what they assume is) procedural WM is influenced by the recency and frequency with which the task has been performed, but not by the number of tasks in play; once a task-set is loaded, however, time to retrieve the response given the stimulus is a function of the number of S-R rules, as specified by Hick's law (see Schneider & Anderson, 2011; Van 't Wout, 2018).

working and long term procedural memory. Further investigation of the initial stages of task-set acquisition should illuminate these issues.

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Appendix 1: Object names for Experiments 1-3

Matrix 1: rows are sets with phonologically similar names

car,	jar,	cart,	star,	scarf,	scar
ring,	witch,	wing,	spring,	king,	kilt
web,	well,	cell,	shed,	bell,	shell
boat,	note,	bow,	goat,	coach,	coat
lake,	snail,	snake,	cake,	steak,	cave
map,	cat,	mat,	hand,	hat,	bat

Matrix 1: columns reordered to generate dissimilar sets

boat,	cat,	wing,	cake,	bell,	scar
lake,	witch,	mat,	shed,	scarf,	coat
map,	snail,	cart,	goat,	king,	shell
web,	jar,	snake,	hand,	coach,	kilt
ring,	well,	bow,	star,	hat,	cave
car,	note,	cell,	spring,	steak,	bat

Matrix 2: rows are sets with phonologically similar names

stamp,	rat,	ramp,	cap,	lamp,	camp
rake,	brain,	rain,	train,	tray,	drain
clog,	log,	clock,	cog,	dog,	doll
claw,	sword,	saw,	cork,	door,	core
bolt,	phone,	bone,	throne,	comb,	cone
eye,	tie,	tile,	pie,	fly,	pipe

Matrix 2: columns reordered to generate dissimilar sets

rake,	sword,	tile,	cog,	lamp,	cone
clog,	rat,	bone,	cork,	fly,	drain
claw,	log,	ramp,	throne,	tray,	pipe
eye,	brain,	saw,	cap,	comb,	doll
bolt,	tie,	clock,	train,	door,	camp
stamp,	phone,	rain,	pie,	dog,	core
tree,	brick,	key,	tape,	tail,	fan

Appendix 2: Names of the stimulus objects for Experiment 4

ant, belt, bench, book, box, bridge, butter, castle, crab, dice, drill, drum, egg, fish, flag, fork, ghost, globe, hat, hose, kite, leaf, letter, lion, mirror, mop, parrot, pear, puzzle, rocket, ruler, sink, tank, torch, window, worm

baby, cheese, mouse, nose, slide, wolf (practice set)
balloon, dress, guitar, nail, rug, key (filler set)