# Replication Recipe (Brandt et al., 2013): Pre-Registration of replication of Shebani and Pulvermüller (2013) in *Cortex*

*NB: This document complements the manuscript "Does the motor system functionally contribute to keeping words in working memory? A pre-registered replication of Shebani and Pulvermüller (2013, Cortex)", submitted to* Cortex *as a Registered Report*.

## The Nature of the Effect

1. *Verbal description of the effect I am trying to replicate (optional)*

We try to replicate the finding of effector-specific semantic interference in working memory, as reported in Shebani and Pulvermüller (2013, henceforth SP13). SP13 report that performing a complex rhythmic pattern (a "paradiddle") with either the arms or legs differentially impairs working memory for arm- and leg-related action words. Specifically, participants had to memorize four sequentially shown words per trial, which were either all arm related (arm trials, e.g., *peel-bash-chop-clap*) or leg related (leg trials, e.g., *hop-stomp-limp-skip*). Both types of trials occurred within a block. The critical finding took the form of a cross-over interaction effect between effector (hand vs foot paradiddle) and word type (arm vs leg words). Specifically, participants made more errors remembering arm-related than leg-related words when they performed the hand paradiddle, whereas the opposite was true when they performed the foot paradiddle, in which case they made more errors memorizing leg-related than arm-related words.

2. *It is important to replicate this effect because (optional)*

This result was interpreted as evidence "that body movements and working memory for action-related words share processing resources" and further as supporting "the necessity of sensorimotor areas of the upper and lower extremities for arm- and leg-word processing" (SP13, p. 227). It can thus be seen as evidence supporting embodied semantic theories that claim "language understanding and memory depend on links between linguistic brain systems and sensorimotor domains" (SP13, p. 222).

The theoretical relevance of this study warrants a replication: Taken at face value, the results of this study's interference design constitute strong evidence for embodied meaning representations (Barsalou, 2008; Pulvermüller & Fadiga, 2010), which is a heatedly debated topic in cognitive science (Mahon & Caramazza, 2008; Mahon & Hickok, 2016). Furthermore, this is the only study to our knowledge that indicates that the motor system functionally contributes to action verb semantics in *working memory*.

On the one hand, SP13 stands out by its elegant yet simple behavioural design, and its clear-cut pattern of results. On the other hand, with only 15 participants, the original study is also one of those "underpowered studies with perfect results […] that should invite extra scrutiny" (Simmons, Nelson, & Simonsohn, 2011, p. 1363). More generally, the current replication is motivated by calls for more replication studies in the psychological sciences (Munafò et al., 2017; Zwaan, Etz, Lucas, & Donnellan, forthcoming) following a debate on how reproducibility in this discipline was generally low (Anderson et al., 2016; Open Science Collaboration, 2015).

3. *The effect size of the effect I am trying to replicate is (optional)*

In the original paper, the reported Cohen's *d* for the interaction of interest was *d* = 1.25 (SP13, p. 226). In terms of Cohen's (1988) rule of thumb for size effect interpretation, this would be considered an effect that is by some margin larger than "large" (*d* = .8); it would count as a "very large" effect according to Sawilowsky (2009).

There are, however, potential problems with this effect size estimate. We first reanalyzed the original data shared by the authors with the same analysis type, repeated measures ANOVAs.[1] Although we could precisely reproduce their summary statistics and the *F* and *p* values for their ANOVAs, we were unable to reproduce their effect size estimate of Cohen's *d* = 1.25. Second, the suboptimal analysis method (ANOVA) used in SP13 might have led to an inflated effect size estimate. A more appropriate analysis would treat the error counts as arising from a binomial distribution. When we re-analyzed the original data with this improved analysis method, the critical interaction effect was still robustly greater than zero. However, the effect size estimate became very small, namely 0.15 log-odds. Note that the unit for effect size in binomial models (log-odds) is not on the same scale as Cohen's *d*: the estimated effect (0.15 log-odds) is less than a third of what is considered a "small" effect, namely 0.5 log-odds (Chen, Cohen, & Chen, 2010).

4. *The confidence interval of the original effect is (optional)*

Confidence intervals were not reported in the original. In our re-analysis of the data (see point 3), the 95% Bayesian credible interval of the interaction effect in log-odds was [0.07, 0.23].

5. *The sample size of the original effect is (optional)*

N = 15 (SP13, p. 223).

6. *Where was the original study conducted? (e.g., lab, in the field, online) (optional)*

This is not explicitly mentioned in the article, but most likely in the lab judging from the description of the procedure.

7. *What country/region was the original study conducted in? (optional)*

This is not explicitly mentioned in the article, but probably in Cambridge, UK, since a) the first author was affiliated to the Medical Research Council, Cognition and Brain Sciences Unit, Cambridge, UK, and b) ethics permission for the study was obtained from the Cambridge Psychology Research Ethics Committee.

8. *What kind of sample did the original study use? (e.g. student, Mturk, representative) (optional)*

The authors provide the following description (SP13, p. 223):

> Fifteen monolingual, native speakers of English (8 males) aged 18–30 (mean = 20.4, standard deviation SD = 3.2) took part in the experiment. All reported normal vision and hearing and

[1] The original data are available at https://github.com/zshebani/LMB/tree/1.0 (DOI: 10.5281/zenodo.3402035).

had no history of neurological or psychiatric illness. All participants were also right-handed with an average laterality quotient (Oldfield, 1971) of 80.5% (SD = 23.6) and reported no immediate left-handed family members (parents or siblings). All gave their informed, written consent prior to their participation and were reimbursed for their time.

9. *Was the original study conducted with paper-and-pencil surveys, on a computer, or something else? (optional)*

The critical task was administered on a computer. Participants' oral responses were recorded and later transcribed and coded (Z. Shebani, personal communication, October 3, 2018).

## Designing the Replication Study

10. *Are the original materials for the study available from the author? (optional)*

Yes, the original stimuli (72 English action verbs that are either arm [36 verbs] or leg related [36 verbs]) are provided as an appendix in the original study (SP13, p. 229). However, we will not use the original materials, as our replication will be conducted in Sweden with native speakers of Swedish.

11. *I know that assumptions (e.g., about the meaning of the stimuli) in the original study will also hold in my replication because (optional)*

We will use Swedish verbs in our replication with native speakers of Swedish: 52 arm-related and 52 leg-related verbs. Arm- and leg-relatedness was assessed by semantic ratings on a 7-point scale obtained from 12 Swedish native speakers. The two lists differ significantly on arm-relatedness (arm words: 6.59 [SE=0.03]; leg words = 1.80 [0.07]) and leg-relatedness (arm words: 1.34 [0.03]; leg words = 6.46 [0.08]). We have matched the two lists of verbs along the same psycholinguistic variables as in the original: Number of letters, number of phonemes, word frequency, grammatical ambiguity, lemma frequency, bigram frequency, trigram frequency, valence, arousal, and imageability (see Table 1). Since the original study did not explain how some of these measures were obtained, we contacted the authors and operationalized the variables based on this correspondence. We omitted three of the original variables (visual relatedness, body relatedness, and general action relatedness) that were redundant with other collected measures according to the authors (F. Pulvermüller, personal communication, May 30, 2019).

Because the stimuli are controlled along the same variables as in the original, the only difference is that the study will be carried out in a different language (Swedish instead of English). Since there is no theoretical reason for why the effect reported in SP13 should be language-specific, all the assumptions about the meaning of the stimuli will also hold in our replication.

Table 1. Means, standard errors and *p* values (from unpaired t-tests) comparing psycholinguistic features of the 52 arm and 52 leg words used in this study.

| | Arm words | | Leg words | | |
|---|---|---|---|---|---|
| Feature | Mean | SE | Mean | SE | *p* value (t-test) |
| Number of letters | 5.13 | 0.13 | 5.37 | 0.18 | 0.3 |
| Number of phonemes | 4.69 | 0.1 | 5.02 | 0.16 | 0.1 |
| Word log frequency | 2.56 | 0.09 | 2.28 | 0.13 | 0.1 |
| Lemma log frequency | 2.79 | 0.09 | 2.62 | 0.13 | 0.3 |
| Bigram log frequency | 6.02 | 0.04 | 6.03 | 0.05 | 0.8 |
| Trigram log frequency | 4.82 | 0.07 | 4.84 | 0.07 | 0.8 |
| Grammatical ambiguity | 0.2 | 0.02 | 0.16 | 0.02 | 0.2 |
| Valence | 3.67 | 0.1 | 3.79 | 0.11 | 0.4 |
| Arousal | 2.49 | 0.09 | 2.32 | 0.09 | 0.2 |
| Imageability | 5.54 | 0.06 | 5.33 | 0.1 | 0.1 |
| Arm-relatedness | 6.59 | 0.03 | 1.8 | 0.07 | <.001 |
| Leg-relatedness | 1.34 | 0.03 | 6.46 | 0.08 | <.001 |

*12. Location of the experimenter during data collection (optional)*

The experimenter will be in the room during data collection, since he/she will have to show the participants how to carry out the rhythmic patterns with hands/feet, and monitor that participants actually carry out the interference task correctly.

*13. Experimenter knowledge of participant experimental condition (optional)*

All critical manipulations are within participants. The effector manipulation (arm vs leg paradiddle) is between blocks and the experimenter necessarily knows if a participant is in the arm- or leg-interference condition, since correct realization of the paradiddle needs to be monitored by the experimenter. The word type manipulation (arm- vs leg-related words) is between trial (within each block) and the experimenter does not know in advance which word type a given trial belongs to, as trials are randomly ordered.

*14. Experimenter knowledge of overall hypotheses (optional)*

The experimenters are aware of the overall hypothesis.

*15. My target sample size is (optional)*

We will adopt a sequential Bayes factor design (Schönbrodt & Wagenmakers, 2018) with a minimum sample size of 60 and a maximum sample of size 96 participants with step sizes of 12 participants. The exact design is shown in Figure 1. Participants excluded from the statistical analysis due to pre-specified exclusion criteria will be replaced by new participants and the number of exclusions will be reported.

1. Collect data from minN=60 participants (four times the sample size of the original).
2. Compute the BF with a weakly informative prior (see Analyses below).
3. If $BF_{10} > 6$ or $BF_{01} > 6$, stop data collection and report results. Else:
4. If $N < 96$, collect another batch of 12 participants and go to step 2. Else:
5. When we reach maxN=96, stop data collection, compute BFs and report results.

Figure 1. Sequential design of the replication.

*16. The rationale for my sample size is (optional)*

We adopted a prospective Bayes factor design analysis to plan sample size (Schönbrodt & Wagenmakers, 2018). In contrast to *p* value-based inference, using Bayes factors allows for a 3-way decision once the data are collected, based on pre-specified evidence thresholds: The data may a) support the alternative hypothesis that there is an effect, b) support the null hypothesis that no effect exists, or c) remain inconclusive (Dienes, 2014; Wagenmakers, 2007). The goal then is to design a study that jointly yields a high probability of obtaining strong evidence (i.e., data that do not remain inconclusive) and a low probability of misleading evidence (i.e., data that do not lead to accepting the wrong hypothesis) (Schönbrodt & Wagenmakers, 2018). This framework makes it possible to implement a sequential design that pre-specifies a minimum sample size (minN), a plan to test additional batches of participants if the required degree of evidence is not reached at a given sample size, and a maximum sample size (maxN) at which for practical considerations data collection stops, irrespective of the degree of evidence reached.

To determine minN and maxN for our study, we used the state-of-the-art *R* package *BFDA* (Schönbrodt & Stefan, 2018), which allows researchers to simulate studies under different statistical assumptions and summarize the results. Following Cortex guidelines, in all simulations we set the threshold for accepting the alternative over the null hypothesis (or vice versa) at a Bayes factor of 6 ($BF_{10} > 6$ or $BF_{01} > 6$). We used non-informative default priors so as to make our estimates more conservative (Stefan, Gronau, Schönbrodt, & Wagenmakers, 2019). We next synthesize the outcome of our simulations and how they justify the current design.

The two critical parameters for the simulations are the estimated effect size and the type of analysis. The original study reported an effect size of Cohen's $d = 1.25$ for the critical interaction in a 2-by-2 within-subjects ANOVA (SP13, p. 226). This effect size is substantially larger than a "large" effect ($d = 0.8$) according to Cohen's (1988) practical guidelines. Using this effect size estimate with a paired t-test design in BFDA, a sample size of just N=12 would guarantee correctly accepting the alternative hypothesis 94% of the time, there would be 0% false-negative results, and 6% of the studies would remain inconclusive.

There are, however, potential problems with this effect size estimate. We first reanalyzed the original data shared by the authors with the same analysis type, repeated measures ANOVAs.

Although we could precisely reproduce their summary statistics and the F and p values for their ANOVAs, we were unable to reproduce their effect size estimate of Cohen's $d = 1.25$. Second, the suboptimal analysis method (ANOVA) used in SP13 might have led to an inflated effect size estimate. A more appropriate analysis would treat the error counts as arising from a binomial distribution. When we re-analyzed the original data with this improved analysis method, the critical interaction effect was still robustly greater than zero. However, the effect size estimate became very small, namely 0.15 log-odds. Note that the unit for effect size in binomial models (log-odds) is not on the same scale as Cohen's *d*: the estimated effect (0.15 log-odds) is less than a third of what is considered a "small" effect, namely 0.5 log-odds (Chen, Cohen, & Chen, 2010).

While the BFDA does not allow for fine-tuning the exact intended experimental design and analyses, simulations can be run with the more appropriate "AB-test" method, which implements Bayes factors for testing the equality of binomial proportions (Kass & Vaidyanathan, 1992; Schönbrodt & Stefan, 2018). If we use the posterior distribution of the critical interaction effect from our improved reanalysis as the input to the effect size estimate in our simulations, then not even a sample size as large as N=3000 participants would ensure enough power to detect the effect. Even with such an enormous sample size, only 48% of studies would lead to correctly accepting the alternative hypothesis, while 46% would lead to incorrectly accepting the null (only 5% would yield inconclusive evidence). In sum, based on these simulations, planning for compelling evidence with an effect size that small is practically unfeasible as it would require sample sizes in the high thousands.

To nevertheless test the claim put forward by SP13 that arm and leg movements selectively interfere with working memory for arm and leg words, respectively, we opted for a sequential design with a maximum N=96 that would send an important message to the field even if the results remained inconclusive, in line with this journal's guidelines. Our design ensures very high power to detect a "medium" effect size of 1.25 log-odds (equivalent to Cohen's $d = 0.5$, which is 2.5 times smaller than the effect size reported in SP13) and remains high-powered to detect a "small"-to-"medium" effect size of 0.9 log-odds (equivalent to Cohen's $d = 0.36$, i.e., 3.5 times smaller than that reported in SP13) (Chen et al., 2010). As illustrated in Figure 2, under a "medium" effect size of 1.25 log-odds our design has a power of 98% to correctly detect the effect of interest, a 0% rate of false-negatives, and yields 2% of inconclusive studies. Under a "small"-to-"medium" effect size (0.9 log-odds), the effect would be correctly detected in 83% of the cases, there would be 0% false negatives, and 17% of inconclusive results. Note that we deliberately used uninformative priors in the simulations, which therefore represent conservative power estimates compared to priors that would take into account the previous positive results.

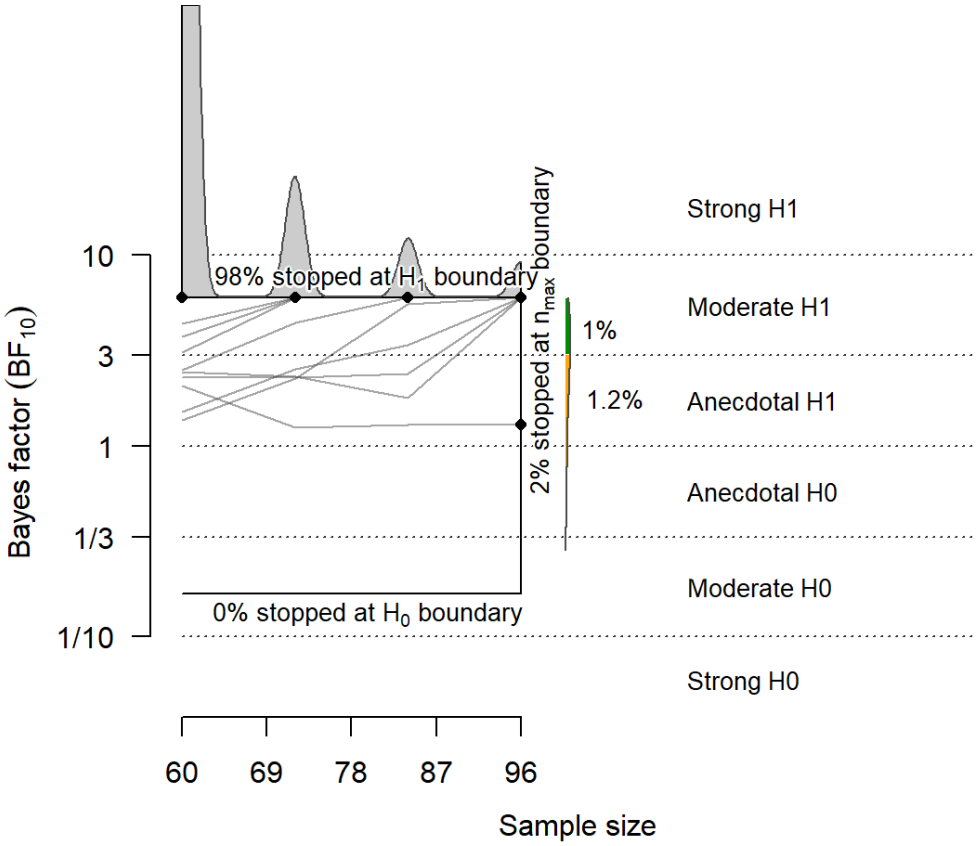


Figure 3. Sequential Bayes factor (BF) design with maximum N=96 assuming a "medium" effect size (1.25 log-odds) for the critical interaction. The figure shows a summary of 10000 simulated studies starting with a minN=60 and adding batches of 12 participants if the results do not reach the intended evidence threshold (BF>6), until reaching maxN=96. We used a default (non-informative) analysis prior and the "AB-test" as analysis method. Simulations were run with the BFDA package.

**Documenting Differences between the Original and Replication Study**

*17. The similarities/differences in the instruction are (optional)*

The instructions are as close to the original as we could make them based on the information provided. Exact instructions were not reported. We contacted the authors and asked them for the experimental protocol, but it was not available.

*18. The similarities/differences in the measures are (optional)*

We will collect the same raw data as SP13 (audio recordings of participants carrying out the task). However, the coding for our main dependent measure (Errors) will differ as follows. We will adopt a binary coding for the data: For each word within a 4-word memory trial, the dependent variable will be 1 if the verb is recalled and 0 if it is not. Our coding differs from that of SP13 in that it disregards shift errors, an error type that accounted for 12% of all errors in the original and whose removal did not affect the critical interaction effect (SP13, p. 226). To understand shift errors,

consider a trial that consists of the words *peel-bash-chop-clap*. If the participant response is *bash-peel-chop-clap*, this will be counted as zero errors according to our coding but it would be counted as one error (a shift error) in SP13's coding scheme, because the order of *peel* and *bash* is interchanged.

We opted for this divergence for several reasons. First, we did not obtain an algorithm from the authors that would allow us to unambiguously reproduce their error coding scheme from a written transcription of participant responses. SP13 report three types of errors: omissions, replacements, and shifts (they also mention that additions counted as errors [p. 225] but do not report the rate of this error type). Some coding decisions are inherently arbitrary; for example, a replacement (one error) could equally well be coded as an omission and an addition (two errors). For want of a principled protocol that can be implemented in a machine, we preferred to adopt our more transparent coding scheme. Second, counting shift errors just as any other error type makes the underlying assumption that all error types carry the same weight, which can lead to counterintuitive outcomes. For example, a participant response such as *bash-clap-peel-chop* for the trial above (where all words are correctly remembered) would count as three errors (three shifts), exactly the same as if the response had been *peel-potato-garden-I don't know* (two replacement errors and an omission). Intuitively, it would seem that the former response reflects superior memory than the latter, but this would not be captured by the coding. Third, as already mentioned, none of the critical results reported in SP13 hinged on shift errors: SP13 report that the critical interaction was still present if shift errors were removed and that it was not present if these errors were evaluated separately (SP13, p. 226). Finally, our coding scheme allows for improved inference on population-level effect estimates by letting us model item variability as a random effect. This is straightforward when each binary response can be linked to a specific verb (as in our coding), but it becomes difficult in the case of shift errors.

We note that an alternative coding scheme in which all error types used in SP13 are counted is easy to implement from the transcripts (e.g., by computing Levensthein distance from the string provided by the participant to the target string, where each word counts as a symbol). However, for the above reasons such a coding will not be the basis for our primary pre-registered analysis.

*19. The similarities/differences in the stimuli are (optional)*

Our stimuli are Swedish verbs instead of English verbs. They are otherwise similar with respect to all relevant dimensions, see point 11.

*20. The similarities/differences in the location (e.g., lab vs. online; alone vs. in groups) are (optional)*

The type of location is essentially the same based on the information provided in the article. Our sessions will take place in a quiet room with only the participant and the experimenter.

*21. The similarities/differences in remuneration are (optional)*

In both cases, participants were reimbursed for their time. The exact remuneration is not specified in SP13. We will pay participants a gift voucher with a value of 100 SEK (approx. 10 euro).

*22. The similarities/differences between participant populations are (optional)*

Our study will be conducted in Sweden and we will therefore recruit native speakers of Swedish (rather than native speakers of English). Because there is no theoretical motivation to suspect that the effect reported in SP13 is language-specific, this difference should not impact the results. In all other respects, the populations are as similar as possible based on the information provided by SP13, as we detail next.

Participants will be in the same age range as the original (18–30). As in the original, we will screen participants for right-handedness, normal vision and hearing, and lack of history of neurological or psychiatric illnesses. We will exclude musicians, operationalized as anybody who has at least five years of formal musical training or equivalent informal experience. We will also exclude participants who report having played the drums for more than one year.

Monolingual Swedish speakers are virtually impossible to find in the targeted age range and educational level, as English language instruction is compulsory in Swedish education and communicative English proficiency is generally high (Bylund & Athanasopoulos, 2015; Skolverket [Swedish National Agency for Education], 2011). We therefore adopted the following standard definition for who counts as a native speaker and may therefore participate in the study (cf. Abrahamsson & Hyltenstam, 2009; Bylund, Abrahamsson, Hyltenstam, & Norrman, 2019): Participants should a) be born in Sweden, b) be exposed to Swedish since birth and without significant interruption (i.e., not more than six months) throughout their lives; c) have grown up in a Swedish-speaking home; and d) have Swedish as their dominant language.

*23. What differences between the original study and your study might be expected to influence the size and/or direction of the effect? (optional)*

We do not expect any of the differences to influence the size and/or direction of the effect. Our increased power (due to substantially more participants and slightly more items) is only expected to increase precision of our estimates of the effect, but not to modulate the effect per se.

An additional difference not mentioned above is that we will only run the two critical conditions (movement interference with either arms or legs), and remove from the design the control and articulation conditions (SP13, p. 224-225). We choose to restrict the design to address the critical effect, namely the interaction between word type and movement. In the original study, it is this 2x2 subset of the design that is reported as the analysis "directly addressing the main hypothesis motivating this study" (SP13, p. 226). Because the order of the additional conditions was counterbalanced following a Latin-square design (SP13, p. 225), it should not be a confound in the reported results.

In sum, there is no theoretically motivated reason to expect that the true underlying effect should differ between the original study and our replication due to this or any of the above-mentioned differences.

*24. I have taken the following steps to test whether the differences listed in the previous question will influence the outcome of my replication attempt (optional)*

We have normed the stimuli (Swedish verbs) with respect to all variables that the original authors deemed important.

**Analysis and Replication Evaluation**

*25. My exclusion criteria are (e.g., handling outliers, removing participants from analysis) (optional)*

The experimenter will monitor the execution of the rhythmic task (hands/feet paradiddle) throughout the session using an error monitor form. The following will be considered execution errors: forgetting to execute the paradiddle during the memory phase or executing the paradiddle with the wrong effector. Trials with execution errors will be excluded from the analysis. At the participant level, we will exclude participants who fail to carry out the interference task in more than 30% of the trials across blocks or in more than 50% of trials in a single block. We will also exclude participants for which, due to technical failure (e.g., recording not working), data is missing for more than 30% of the trials across blocks or for more than 50% of trials in a single block. All exclusions will take place before the data is coded and analysed. Excluded participants will be replaced.

*26. My analysis plan is (justify differences from the original) (optional)*

Count data (in this case errors in the memory task) violate the statistical assumptions of ANOVAs and t-tests (Jaeger, 2008), which were the statistical analyses used in SP13. Instead, we will analyse the data using a Bayesian version of logistic mixed effects regression implemented in the package *brms* (Bürkner, 2017) in the R statistical environment (R Core Team, 2015). Logistic mixed effects regression is well suited to model binary outcomes and relies on the log of the odds as a link function (see Jaeger, 2008). The dependent binary variable Error (=1 if a word is missed, =0 if it is remembered; see Data coding) will be modelled as a function of the contrast-coded predictors Movement (1=arm movements, −1=leg movements), Word Type (1=arm-related words, −1=leg-related words), and their interaction. The model will include crossed random effects by participants and items. To determine the exact random effect structure of the model, we will follow the guidelines in Barr, Levy, Scheepers, and Tily (2013), fitting whenever possible the full random structure motivated by the design. Additionally we will include the following nuisance variables as fixed effect predictors in the model (centred and scaled): trial position within the experiment, error on any of the preceding words in trial (binary), word position within trial.

Statistically, our analysis plan represents an improvement over the original, as the underlying probabilistic model (logistic regression) is more appropriate to the phenomenon and the model allows for parsimoniously capturing by-participant and by-item random variability (Baayen, Davidson, & Bates, 2008; Clark, 1973).

*27. A successful replication is defined as (optional)*

To decide when to stop data collection (see point 15) and to make a decision as to whether our replication successfully detects the effect reported in SP13 or fails to do so, we will use Bayes factors (see Dienes, 2014; Verhagen & Wagenmakers, 2014, and references therein). We will compute the following two Bayes factors (see Verhagen & Wagenmakers, 2014):

1. BF1: Independent Jeffreys–Zellner–Siow (JZS) Bayes Factor to address the question if the effect is present or absent in the replication attempt.

2. BF2: Replication Bayes factor to address the question if the "effect from the replication attempt [is] comparable to what was found before, or [is] absent?" (Verhagen & Wagenmakers, 2014, p. 1458).

Our decision as to when to stop data collection will be based on the calculation of BF1 only. Once data collection has stopped – either because BF1>6 in favour of one of the competing hypotheses or because we have reached maxN=96 – BF2 will be computed.

Both BFs will be reported. A clear replication success will be an outcome in which both $BF1_{10} > 6$ and $BF2_{10} > 6$. Conversely, a clear failure to replicate will be an outcome in which $BF1_{01} > 6$ and $BF2_{01} > 6$. If only one of the two BFs reach the targeted threshold, our primary interpretation will be based on BF1, but it will be nuanced by the outcome of BF2. The value of BFs will be interpreted according to the heuristics in Table 2.

Table 2. Heuristic classification scheme for the interpretation of Bayes factors $BF_{10}$ (adjusted from Schönbrodt & Wagenmakers, 2018). The same scheme will be used to interpret $BF_{01}$.

| Bayes factor | Evidence category |
|---|---|
| > 100 | Extreme evidence for $H_1$ |
| 30 – 100 | Very strong evidence for $H_1$ |
| 10 – 30 | Strong evidence for $H_1$ |
| 6 – 10 | Evidence for $H_1$ |
| 3 – 6 | Anecdotal evidence for $H_1$ |
| 1 – 3 | Inconclusive evidence |

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