**Shape of SNARC: How task-dependent are Spatial-Numerical Associations?**

**A highly powered online experiment**

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## Abstract

Spatial-Numerical Associations (SNAs) are fundamental to numerical cognition. They are essential for number representation and mathematics learning. However, SNAs are highly dependent on the experimental situation and task. Understanding this dependency is crucial to understanding SNAs and their impact on mathematical cognition. The hallmark SNA is the Spatial-Numerical Association of Response Codes (SNARC) effect, which denotes faster responses to small/large magnitude numbers on the left/right side, respectively (Dehaene et al., 1993). It is typically measured in *magnitude classification* (MC), where participants decide whether numbers from 1 to 9 (excluding 5) are smaller or larger than 5, or in *parity judgment* (PJ), where participants decide whether these numbers are odd or even. Despite their similarity, these tasks differ in the necessity of magnitude processing, compatibility effects being present, and other ~~phenomena~~aspects. Interestingly, the MC-SNARC seems to be categorical (i.e., same left-hand advantage between 1 and 4, and same right-hand advantage between 6 and 9), whereas the PJ-SNARC is continuous (i.e., increasing right-hand advantage with increasing magnitude). Strikingly, no matter the task, the standard analysis is a continuous linear regression, even though the MC-SNARC data are usually categorical. Only few studies systematically investigate similarities and differences between MC-SNARC and PJ-SNARC, and they often lack statistical power. In this registered report, we propose a highly powered online experiment to thoroughly investigate the shape of the MC-SNARC and the PJ-SNARC as well as task differences in a within-subjects design with up to 1700 participants.


*Keywords*: spatial-numerical associations, SNARC effect, magnitude classification, parity judgment, task dependency

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Spatial-numerical associations (SNAs) belong to the fundaments of numerical cognition (Fischer & Shaki, 2014; Toomarian & Hubbard, 2018). They have been implicated as an important representation (e.g., Dehaene et al., 2003) and as means to foster numerical and arithmetic learning (Booth et al., 2008; Dackermann et al., 2017, for an overview of~~n~~ embodied spatial-numerical learning). SNAs can be divided in spatial-extensional SNAs, where a particular number or magnitude is related to a physical extension (i.e., larger number to larger extensions), and directional SNAs, where a particular number is associated with a particular location in space (Patro et al., 2014). Both SNAs are important and seem to be highly dependent on the experimental situation or task (Cipora, Patro & Nuerk, 2018). Understanding such situational dependencies is key to understanding SNAs and their relation to mathematics as such (Cipora, He, & Nuerk, 2020).

SNAs can refer to explicit or implicit associations of different characteristics of numbers (e.g., cardinality, ordinality, parity) with different aspects of space, namely directions or extensions (Cipora, Haman, et al., 2020; Cipora, Schroeder, et al., 2018; Patro et al., 2014). For instance, the MARC effect (Linguistic Markedness of Response Codes; Nuerk et al., 2004) reflects the association between parity (odd/even numbers) and direction (the left/right side), respectively. The hallmark directional SNA, however, is the Spatial-Numerical Association of Response Codes (SNARC) effect, which denotes that – at least in left-to-right reading cultures – participants respond faster to small/large magnitude numbers on the left/right side, respectively (Dehaene et al., 1993). SNAs are claimed to reflect implicit and explicit mental representations of numbers and processes operating on them (Cipora, Haman, et al., 2020). The tendency to map characteristics of numbers onto space is considered one of the basic traits of human cognition (Cipora, Patro, & Nuerk, 2018). The SNARC effect has been replicated with stimuli in different modalities and notations (e.g., visual Arabic numerals, visual number words, auditory number words, visual dice patterns; Nuerk et al., 2004; Nuerk, Wood, & Willmes, 2005) and in different response setups (e.g., manual responses, pedal responses, saccadic eye movements; Schwarz & Keus, 2004; Schwarz & Müller, 2006), offline as well as online (Cipora et al., 2019; Roth, Jordan, et al., 2024). The association of number magnitude and space therefore seems to be highly robust and generalizable across many settings, even though many situational modulations have been described (Cipora et al., 2018).

Importantly, the SNARC effect arises in several tasks, which – as we will outline in detail below – have major conceptual differences in the underlying semantic features of the numbers that need~~s~~ to be processed or that are automatically processed. Two tasks that inquire about semantic numerical attributes of the digits/numbers themselves are by far the most frequently used to investigate the SNARC effect: (i) the magnitude classification (MC) task and (ii) the parity judgment (PJ) task (see Table 2 in Wood et al., 2008). In MC, participants judge whether numbers are smaller or larger than a reference number[1]. In PJ, participants judge whether numbers are odd or even. Although some studies have used other kinds of stimuli (e.g., dice patterns or number words in Nuerk et al., 2005, and multi-digit numbers in Tlauka, 2002; Weis et al., 2018), single-digit (i.e., Arabic or Hindu-Arabic) numbers are most used in this task. In both tasks, the instruction is to respond as quickly and as accurately as possible with a left- or right-hand key to numbers presented centrally on ~~the~~ a computer screen. Typically, symbolic[2] numbers from 1 to 9 (excluding 5) are used as the stimulus set, with number 5 serving as the reference number in MC. In both tasks, the response-to-key assignment is flipped in the middle of the experiment, so that both left- and right-hand responses are given for each number.

## Conceptual differences between MC and PJ

[1] In the current manuscript, tasks ~~where~~ in which presented numbers are to be compared with a fixed reference number (e.g., comparing whether a presented number between 1 and 9 excluding 5 is smaller or larger than 5) are referred to as magnitude *classification*. In contrast, tasks ~~where~~ in which the reference number is not fixed but varies between trials are referred to as magnitude *comparison*.

[2] Symbolic numbers consist of symbols, such as digits, whereas non-symbolic numbers do not consist of symbols but instead quantities, such as dot arrays.

At first sight, apart from the instructions, the MC and PJ tasks seem to be similar: A semantic feature of numbers (i.e., magnitude or parity) is to be categorized. In the current study, we will use the most common bimanual computerized setup with symbolic numbers from 1 to 9 (excluding 5) described above. One might assume that the required cognitive processes, the responses given by participants, and the arising spatial mapping of number magnitude are similar in both tasks. However, as we shall see, the SNARC effects in MC and PJ (called *MC-SNARC* and *PJ-SNARC* in the ~~following~~remainder of this article) differ, and the relation between them remains unclear. In the following, we describe conceptual differences between MC and PJ to shed light on reasons for the different SNARC effects.

***Relevance of number magnitude and number parity***

Most obviously, number magnitude is directly task-relevant to MC but not to PJ. This leads to an important difference: Tzelgov et al. (2015) distinguish between intentional and automatic processing, where the latter means processing without conscious monitoring according to Bargh (1992). Automatic processing can be measured in tasks where the process in question is not part of the task requirements (Tzelgov, 1997). In contrast, intentional processing is supposed to reflect the task requirements.

For participants to show the SNARC effect, two representations have to be activated, namely the *magnitude* (e.g., two) or *ordinality* (e.g., the second) of a number and its directional association with space (Cipora, He, & Nuerk, 2020). Importantly, when judging parity, neither the processing of magnitude nor of its directional association with space is task-relevant and intentional. As a result, the PJ-SNARC effect is commonly regarded as an indicator of automatic number magnitude processing in humans. It also supports the notion that single-digit numbers function as primitives in Western cultures, such that their meanings can be effortlessly and holistically retrieved from memory without additional processing (Tzelgov et al., 2015).~~Therefore, the *PJ-SNARC* is often referred to as a marker for automatic number magnitude processing in humans and for single-digit Arabic numbers to be primitives (i.e., their~~

~~meaning can be holistically retrieved from memory without further processing) in Western cultures (Tzelgov et al., 2015).~~ This is fundamentally different for MC, where the processing of magnitude is intentional, as it is task-relevant. Therefore, the MC-SNARC only shows that the directional association with space is automatic (as it is not needed for the response), but not that ~~automatic~~ number-magnitude processing is automatic.

Importantly, the two tasks differ not only regarding task-relevance of number magnitude but also in the task-relevance of number parity. ~~More precisely,~~ P~~p~~arity is task-relevant in PJ while being task-irrelevant in MC. Apart from the SNARC effect, a phenomenon to be considered in the current study is the MARC effect (Linguistic Markedness of Response Codes~~)~~ ~~(~~; Nuerk et al., 2004), which is typically observed in PJ but not in MC (see Replication Check 2; see ordered list of replication checks in the Introduction section "The current study"). The MARC effect reflects faster responses to odd/even numbers on the left/right side, respectively. In a similar vein as for the SNARC effect, there are two prerequisites for the MARC effect: the processing of parity and its directional association with space. Crucially, the processing of number parity is less automatic than the processing of number magnitude (Roth, Caffier, Cipora, et al., in press) and is more consistently found when using number words rather than Arabic digits (Nuerk et al., 2004; Roettger & Domahs, 2015). The processing of number parity seems not to be automatic, as typically no MC-MARC is found (Cipora, 2014; Deng et al., 2018). In contrast, the directional association of parity with space can be considered automatic, because it is not required in PJ, yet a PJ-MARC can typically be observed.

Further, RTs increase when numerical magnitude increases, which is referred to as the Numerical Size Effect (Moyer & Landauer, 1967). We expect the effect to arise in both tasks (Hypothesis 3a; see ordered list of hypotheses in the Introduction section "The current study"), although it has mainly been demonstrated for two-digit numbers (Brysbaert, 1995) and single-digit numbers will be used in the present study. Moreover, we expect it to be stronger in MC than in PJ (Hypothesis 3b) because numerical size (i.e., number magnitude) is only task-relevant in MC. Moreover, RTs increase with increasing numerical distance between the stimulus and the reference number in MC, which is referred to as the Numerical Distance Effect (Gevers, Verguts, et al., 2006). We expect the ~~might~~ effect to arise in MC (Replication Check 4), but it cannot arise in PJ because there is no criterion that numbers are compared to~~)~~.

***SNARC and MARC compatibility***

Importantly, the SNARC and MARC effects can also be considered to be *compatibility effects* (e.g., see cognitive-control account for the SNARC effect by Zhang et al., 2022) Crucially, MC and PJ differ with respect to such compatibility effects. In a typical PJ task with two blocks, one block is MARC-compatible (i.e., when the instruction is to respond to odd/even numbers with the left-/right-hand key, respectively) and the other block is MARC-incompatible. At the same time, within each block, half of the trials are SNARC-compatible (i.e., when the response to the parity of small/large numbers is assigned to the left/right, respectively) and half of the trials are SNARC-incompatible. On the contrary, in a typical MC task with two blocks, one block is SNARC-compatible (i.e., when participants are asked to respond to numbers that are smaller/larger than a reference number with the left-/right-hand key, respectively) and the other is SNARC-incompatible. SNARC-compatible and -incompatible trials alternating within blocks, as is the case in PJ, can elicit Gratton effects (Gratton et al., 1992). In line with this, Pfister et al. (2013) reported reduced SNARC effects after SNARC-incompatible than after SNARC-compatible trials in PJ. In MC, where SNARC-compatibility is grouped by block, such trial-to-trial effects cannot occur.

Apart from trial-to-trial compatibility effects, there can be compatibility effects depending on block order. Van Galen and Reitsma (2008) observed a stronger MC-SNARC in participants who completed the SNARC-compatible followed by a SNARC-incompatible block than in participants who were administered the reverse order, whereas Bulut, Roth, et al. (~~in press~~2025) observed the opposite effect in one of the three tested samples, but no effect in the two remaining samples. No effect of block order on the PJ-SNARC has been found (Bulut, Roth, et al., ~~in press~~2025; Cipora, van Dijck, et al., 2019; Roth, Jordan, et al., 2024), where each block consists of half SNARC-compatible and half SNARC-incompatible trials. For the MARC effect, influences of block order have been found in both directions. In a previous study, we found a stronger MARC effect in PJ with the MARC-incompatible-compatible order compared to the compatible-incompatible order (between-subjects design, Cipora, van Dijck, et al., 2020). In another previous study, we found the reversed pattern (within-subjects design, Roth, Jordan, et al., 2024). The difference might be attributable to the design, and as the current study will be run between-subjects like the study by Cipora, van Dijck, et al. (2020), we expect the same pattern here. A stronger PJ-MARC in the MARC-incompatible-compatible order than in the MARC-compatible-incompatible order seems plausible: Participants need to familiarize themselves with PJ and overcome their natural odd-left and even-right association in the first block, while they are already familiar with PJ and can respond in line with their natural odd-left and even-right association in the second block. Participants therefore have two reasons to be slower in the first block, and the difference (i.e., the MARC effect) between blocks is therefore especially strong in this block order. We will exploratorily investigate compatibility-order effects in both tasks in the current study (Exploratory 2~~3~~; see ordered list of exploratory tests in the Introduction section "The current study").

Bae et al. (2013) and Bulut, Çetinkaya, et al. (2024) demonstrated that the response-to-key assignment in MC influences the SNARC effect in subsequently measured PJ. Specifically, they found a regular left-to-right number mapping in PJ after a SNARC-compatible MC block (i.e., small-left and large-right), but a reversed right-to-left number mapping in PJ after a SNARC-incompatible MC block (large-left and small-right). However, in these studies, participants were assigned to only one of two possible response-to-key assignments for MC. Hence, habituation or practice that spilled over from MC to PJ was unidirectional, and furthermore, no MC-SNARC could be determined.

### *Strength of the SNARC effect in MC and PJ*

As outlined above, the processing of number magnitude is highly automatized and single-digit ~~Arabic~~ numbers can therefore be considered as primitives in Western cultures' numerical cognition (Tzelgov et al., 2015). In contrast, the processing of number parity might be less automatized when only a semantic number feature other than parity is task-relevant (no MC-MARC found by Cipora, 2014, and Deng et al., 2018). Note that evidence has been found for both the MARC effect and the Odd effect (i.e., faster responses to odd than to even numbers, Hines, 1990) when only non-semantic features of numbers were judged (i.e., font color; Roth, Caffier, Cipora, et al., in press), reflecting automatic parity processing to some extent, but the evidence was only weak. Also, number magnitude is more often relevant in daily life than number parity. Hence, the processing of magnitude seems to be more straightforward than the processing of parity. In line with this, average responses are typically faster in MC than in PJ (Kiesel et al., 2007; Saeki & Saito, 2009; descriptively also observed by Fattorini et al., 2015; Fitousi et al., 2009; Gevers, Verguts, et al., 2006; Ito & Hatta, 2004, see also Wood et al., 2008, for a meta-analysis), which we expect to find in the current study as well (see Replication Check 3).

The processing of magnitude being explicitly required in MC, but not in PJ, might elicit a stronger spatial mapping in MC than in PJ. In line with this assumption, the MC-SNARC has been found to be stronger than the PJ-SNARC (Bae et al., 2009; Cheung et al., 2015; Fitousi et al., 2009; van Dijck et al., 2009). On the other hand, judgments of number magnitude are automatic (Tzelgov et al., 2015) and therefore naturally faster than judgments of number parity. At the same time, the SNARC effect is typically stronger in slower responses in both MC and PJ, both within participants and on the sample level (Cipora, Soltanlou, et al., 2019, see their Supplementary Materials, Table ST4; Didino et al., 2019, Table 3; Gevers, Verguts, et al., 2006, Figure 6). In contrast to the reasoning above, this would lead to the opposite prediction of the PJ-SNARC being stronger than the MC-SNARC, which has been observed by Georges et al. (2017), Gevers, Verguts, et al. (2006), and Ito and Hatta (2004). No difference between the

MC-SNARC and the PJ-SNARC was found by Didino et al. (2019) in an independent-samples *t*-test. Taken together, ~~we have two opposing mechanisms:~~ (i) easier and possibly stronger processing of magnitude in MC than in PJ~~, which~~ should lead to a greater magnitude-space association in MC, and (ii) longer response times in PJ than in MC~~, which~~ should lead to a greater magnitude-space association in PJ. Both opposing processes seem to be valid and there is no clear picture in the literature. It remains unclear whether the SNARC effect differs in size between tasks, and we will therefore look at this in an exploratory analysis (Exploratory 6).

***Further differences between MC and PJ***

Several more differences may exist between MC and PJ. First, the MC-SNARC seems to more strongly involve visuospatial working memory, the PJ-SNARC seems to rely more on verbal working memory (Deng et al., 2017; Herrera et al., 2008; van Dijck et al., 2009). Second, the MC-SNARC and the PJ-SNARC might arise at different processing stages (Basso Moro et al., 2018; Xiang et al., 2022). Third, cognitive mechanisms underlying the MC-SNARC and the PJ-SNARC might differ. Namely, Prpic et al. (2016) claim that ordinality drives the SNARC effect in *direct* tasks (e.g., MC, where magnitude is response-relevant), whereas cardinality underlies in *indirect* tasks (e.g., PJ, where magnitude is response-irrelevant). Note that Casasanto and Pitt (2019) claim that only ordinality is crucial for both direct and indirect tasks, and that Koch et al. (2023) show that order- and magnitude-related mechanisms are not mutually exclusive. Looking into these differences between the MC-SNARC and the PJ-SNARC is beyond the scope of the current study; however, the current study will provide a better understanding of the two tasks and thereby lay the groundwork for further investigations.

In summary, several conceptual differences exist between MC and PJ, concerning the task-relevance of number magnitude and number parity, the compatibility of the response-to-key assignment with the SNARC and MARC effects, arising numerical-cognition effects, and underlying cognitive mechanisms. However, both tasks elicit a SNARC effect, and the presence and strength of the MC- and PJ-SNARC in the same participants might be related to one another, as will be discussed in the next section.

**Correlation between the MC- and PJ-SNARC**

After having described the similarities and differences of MC and PJ, the question arises whether the MC-SNARC and the PJ-SNARC are correlated. However, both factors at the construct level of SNAs and at the operational level of the two tasks might lead to a null correlation. First, there seem to be high fluctuations in the SNARC effect over time (Roth, Jordan, et al., 2024) that limit the maximum correlation that can be detected. Second, the test-retest reliability of the SNARC effect has been found to be poor for both MC and PJ (correlations .22 < r < .41; Cipora & Göbel, 2013; Georges et al., 2013; Hedge et al., 2018; Viarouge et al., 2014). The lower the test-retest reliabilities of the MC-SNARC and PJ-SNARC, the lower is also the maximally observable correlation between the two effects. Third, the split-half reliability of the SNARC effect has been found to be poor for PJ at least in some studies (correlations .43 < r < .96; for an overview, see Cipora, van Dijck, et al., 2019, Table 1 there). To conclude, both properties of the SNA construct and its operationalization in experimental tasks influence whether a correlation between the MC-SNARC and the PJ-SNARC will be found. Possible reasons for a null finding could be low intraindividual stability, low reliability, or low internal consistency, whereas a high correlation between the MC-SNARC and the PJ-SNARC would lead to the conclusion that both MC and PJ reliably measure the same underlying theoretical construct.

Several previous studies did not find any correlation (correlations with 95% confidence intervals and *p*-values: $r = -.02$ [-0.19, 0.15] and $p = .822$ for Germans; $r = -.08$ [-0.26, 0.10] and $p = .386$ for Turks; $r = .10$ [-0.13, 0.32] and $p = .402$ for Iranians, in Bulut, Roth, et al., ~~in press~~2025; $r = .09$ [-0.18, 0.35] and $p = .513$ in Cipora, 2014; $r = 0.06$ [-0.30, 0.40] and $p = .744$ in Didino et al., 2019; $r = .18$ [-0.07, 0.42] and $p = .18$ in Fattorini et al., 2015; $r = 0.20$ [-0.01, 0.39] and $p = .07$ in Georges et al., 2017). To our knowledge, a statistically significant correlation has only been reported by Cheung et al. (2015; $r = 0.25$) and by Cipora (2014; $r = .50$, but only in a unimanual setup). Note that an existing weak correlation between the MC-SNARC and the PJ-SNARC despite limiting factors such as low intraindividual stability, low reliability, or low internal consistency would only be detectable in large samples. To be able to detect a potential correlation between the MC-SNARC and the PJ-SNARC, we will administer both tasks to a large sample in a within-subjects design. This will enable us to test the correlation between the MC-SNARC and the PJ-SNARC in an exploratory analysis with high statistical power (Exploratory 7~~5~~).

**Different shapes of the SNARC effect**

After having outlined the differences between MC and PJ and after having discussed the potential correlation between the SNARC effect in these two tasks, it is important to note that the shape of the SNARC effect seems to differ systematically between MC and PJ (Wood et al., 2008). While the advantage of the right hand over the left hand increases with number magnitude in a continuous manner in PJ, it seems to be categorical in MC with the same left-hand advantage for all small numbers (i.e., smaller than the reference number 5) and the same right-hand advantage for all large numbers (i.e., larger than the reference number 5). However, the SNARC effect in MC is often modelled as a continuous phenomenon, just as in PJ, as described in the following. The main aim of the current study is to thoroughly investigate the SNARC effect in the two most widely used tasks to assess it and to find out how to best statistically model the SNARC effect.

The SNARC effect is usually calculated by subtracting the mean reaction times (RTs) with the left hand from those with the right hand for each number and regressing these differences (dRTs) on magnitude as a continuous predictor in both MC and PJ. A negative regression slope reflects the increasing right-hand advantage for larger numbers and therefore the SNARC effect. To investigate whether the effect is present on group level, regression slopes (one per participant) are then tested against zero in a one-sample *t*-test (repeated-measures regression, adapted by Fias et al., 1996, based on Lorch and Myers, 1990). Importantly, this analysis method is only suitable for a continuous SNARC effect, reflecting a constant increase in right-hand advantage (reflected by a constant decrease in dRT) per increase of magnitude. Hence, when participants judge whether numbers in the typically used stimulus set from 1 to 9 (excluding 5) are odd or even, the spatial mapping of extreme magnitudes such as 1 and 9 is stronger than for magnitudes closer to the mid of the stimulus set such as 4 and 6. In other words, the association with the left side is stronger for the very small number 1 than for the slightly small number 4. While the PJ-SNARC is linear, the MC-SNARC is typically categorical (e.g., Gevers, Verguts, et al., 2006), especially in adults (van Galen & Reitsma, 2008): In a typical MC task, when averaging across all participants, responses are *equally* faster with the left hand to numbers from 1 to 4 and equally faster with the right hand to numbers from 6 to 9. Therefore, a stepwise model reflects the MC-SNARC better than a continuous model (as reflected by a better model fit in terms of a higher proportion of explained variance). The use of a categorical instead of a linear function for quantifying the MC-SNARC would increase the model fit at the participant level and thereby likely also the precision of the effect size estimate at the sample level.

Nevertheless, a linear predictor in the regression of dRTs on number magnitude remained a frequently used analysis of the MC-SNARC (Bachot et al., 2005; Bae et al., 2009; Bull et al., 2005; Cheung et al., 2015; Deng et al., 2017; Han et al., 2017; Herrera et al., 2008; Hoffmann et al., 2013; E. M. Hubbard et al., 2009; Ito & Hatta, 2004; Lohmann et al., 2018; Mourad & Leth-Steensen, 2017; Nathan et al., 2009; Pinto et al., 2021; Schiller et al., 2016; Shaki & Gevers, 2011; van Dijck & Doricchi, 2019; van Dijck et al., 2009; van Dijck et al., 2012; van Galen & Reitsma, 2008; Weis et al., 2018). As the correlation between the linear predictor (i.e., 1, 2, 3, 4, 6, 7, 8, 9) and ~~the~~ a categorical predictor (especially the most commonly assumed one, i.e., -0.5, -0.5, -0.5, -0.5, 0.5, 0.5, 0.5, 0.5) ~~magnitude predictor~~ is extremely high (*r* = .913), the model with a linear magnitude predictor fits relatively well both to the continuously and categorically distributed dRTs in MC and PJ (see top panel of Figure 1 in Bae et al., 2009, or Figure 1 in Nathan et al., 2009). In some studies, a two-way ANOVA including magnitude (small vs. large) and response side (left vs. right) as within-subjects factors has been used to quantify the MC-SNARC (Fattorini et al., 2015; Gevers, Verguts, et al., 2006; Herrera et al., 2008; Hoffmann et al., 2013; Nathan et al., 2009). However, compared to that approach, the repeated-measures regression approach has several advantages (Fias et al., 1996): First, the presence of the SNARC effect is judged by a main effect instead of an interaction effect, which allows a quantification of the size of the effect in milliseconds by the slope. Second, the presence or absence of a SNARC effect can be assessed for each participant individually. A repeated-measures regression with a categorical predictor for the MC-SNARC has only been used in few studies (Bulut, Roth, et al., ~~in press~~2025; Cipora, 2014; Didino et al., 2019; Fitousi et al., 2009; Georges et al., 2017; Gevers, Verguts, et al., 2006; Hohol et al., 2020; Nathan et al., 2009; Nuerk, Wood, & Willmes, 2005; Weis et al., 2018; Zorzi et al., 2012). For an overview of all mentioned studies including MC, see Table A1 in Appendix A. The categorical predictor was defined using 5 as a boundary (so that 1 to 4 are considered as “small” and 6 to 9 as “large”) in all the named studies, and, if not specified otherwise, the same categorical predictor is meant in the current article (for alternative considerations, see below in Exploratory 4).

Unfortunately, the suitability of the linear and categorical predictors for dRTs in MC with the stimulus set from 1 to 9 (excluding 5) was assessed by direct comparison in only a few studies. Fitousi et al. (2009) and Nathan et al. (2009) computed two separate regression models, one of which with a categorical and the other with a linear predictor, and in both studies the fit was higher with ~~the~~a categorical ($R^2$ = .904 in Fitousi et al., 2009; $R^2$ = .988 in Nathan et al., 2009) than with ~~the~~a linear predictor ($R^2$ = .775 in Fitousi et al., 2009; $R^2$ = .891 in Nathan et al., 2009). Similarly, Didino et al. (2019), Gevers, Verguts, et al. (2006) and Nuerk, Bauer, et al. (2005) ran regression analyses including both predictors, and only the categorical predictor turned out to be significant in all three studies. In a study with two-digit numbers where participants performed PJ and MC for the unit digit, Weis et al. (2018) also included both linear and categorical predictors for both unit and decade magnitude concurrently into one regression model. They found only ~~the~~a categorical predictor~~s~~ for units and decades to be significant in MC and only the linear predictors for units and decades to be significant in PJ, providing further evidence for a categorical MC-SNARC and a continuous PJ-SNARC. Importantly, because a linear model fits well even for the categorical MC-SNARC (e.g., see Fitousi et al., 2009; and Nathan et al., 2009), evidence for a better fit of a categorical model in MC can only be achieved with sufficient power by using a large sample and a sufficient number of repetitions per experimental cell (resulting from the combination of each stimulus with each response hand per task). However, as outlined above, although ~~the~~a linear model fit to the MC-SNARC might be high in some studies, ~~the~~a categorical model seems to be more adequate. We expect to find a better fit of ~~the~~a categorical model in MC (Hypothesis 1) and of ~~the~~a linear model in PJ (Hypothesis 2).

Importantly, the boundary between "small" and "large" numbers is not necessarily 5 for every individual and might instead vary between individuals. A split of the full number interval into two halves (i.e., from 1 to 4 and from 6 to 9) seems plausible in MC, where the boundary of 5 is explicitly defined in the task instructions. However, especially in PJ, but potentially even in MC, some individuals might classify numbers into the categories "small" and "large" with a different boundary.[3] Crucially, an overall continuous SNARC effect could result from continuous patterns in most individuals, but also from averaging across categorical patterns differing between individuals. In this study, we will therefore additionally determine the most likely categorical boundary for each participant separately. Subsequently, we will investigate

[3] We wish to thank the stage-1 PCI-RR reviewer Peter Wühr for bringing this conceptual alternative to our awareness.

the shape of the SNARC effect (Hypotheses 1 and 2) once more, this time comparing the best fitting categorical model for each individual with the continuous model (Exploratory 4).

**Explanations for the categorical MC-SNARC effect shape**

The literature provides several explanations for the different shapes of the SNARC effect, depending on the task. First, numbers are typically roughly classified into small and large numbers (Banks et al., 1976; Tzelgov et al., 1992), which is sufficiently precise to perform MC and might lead to the categorical MC-SNARC (Fitousi et al., 2009; Gevers, Verguts, et al., 2006). In contrast, participants are not instructed to process number magnitude at all in PJ, and thus number magnitude processing is not intentional (i.e., slow conditional route according to the dual-route model by Gevers, Ratinckx, et al., 2006) but rather automatic (i.e., fast unconditional route)[4]. Automatic number magnitude processing seems to be more exact and is continuously mapped onto space in PJ. This explanation is in line with the polarity-correspondence account of the SNARC effect by Proctor and Cho (2006), as well as with the application of the markedness principle to number magnitude (Nuerk & Schroeder, 2024; Schroeder et al., 2017). According to these two theories, the SNARC effect arises because both *large* and *right* are associated with the positive or unmarked polarity and both *small* and *left* with the negative or marked polarity. Similarly, this explanation is compatible with the verbal-spatial account of the SNARC effect proposed by Gevers, Verguts, et al. (2006; see also Gevers et al., 2010), stating that verbal categories such as *small* vs. *large* and *left* vs. *right* are responsible for the SNARC effect. These accounts argue for an intermediate classification into small or large numbers (Santens & Gevers, 2008), and the polarities, markedness, or verbal labels are categorical rather than continuous (Bae et al., 2009), which explains the categorical MC-SNARC. Note that it is possible that the PJ-SNARC is linear in the beginning of the task

[4] As outlined by the stage-1 PCI-RR reviewer Peter Wühr, predictions can be derived from the dual-route model. Importantly, both the automatic and the intentional route are activated in MC, whereas only the automatic route is activated in PJ. Thus, the SNARC effect should be stronger in MC than in PJ because it results from both routes instead of only one route. Moreover, a positive correlation of the MC- and PJ-SNARC can be assumed based on the dual-route model, since both effects are (at least partly) caused by the automatic route.

and becomes categorical over the course of the task, so that the continuous shape shifts to a stepwise one. That is, participants might start classifying the stimuli into the two categories "small" and "large" in PJ as soon as they become familiar with the stimulus set because they might notice that the stimulus set consists of two single-digit number sequences (i.e., 1 to 4 and 6 to 9) separated by the missing number 5. We will investigate this possibility in ~~the~~an exploratory analysis (Exploratory 5).

Second, the Numerical Distance Effect might play a role for the MC-SNARC (Gevers, Verguts, et al., 2006). For instance, if the reference number is 5 in MC with the stimulus set from 1 to 9, numbers 4 and 6 need to be processed more intensely than numbers 1 and 9 to discriminate them from number 5 (Wood et al., 2008). Hence, responses are slowest for numbers 4 and 6 and fastest for numbers 1 and 9 with this stimulus set. In combination with the finding that the SNARC effect becomes stronger with increasing RTs, the absolute values of dRTs for number magnitudes that are close to the reference are larger than dRT predictions by the linear SNARC regression slope, resulting in a categorical shape (Didino et al., 2019; Georges et al., 2017; Gevers, Verguts, et al., 2006). Importantly, the Numerical Distance Effect demonstrates automatic processing of number magnitude (i.e., fast unconditional route), because it reflects performance differences in the discrimination between numbers and arises although the task instructions do not favor or disfavor the performance for specific stimuli. It thus does not build on the gross classification into smaller and larger numbers described in the previous paragraph (i.e., slow conditional route), but rather on the exact number magnitudes.

In summary, the rationale for a categorical instead of linear MC-SNARC is twofold: First, the intentional classification into small and large numbers is categorical in MC, and second, the interaction between the Numerical Distance Effect and the positive correlation between the SNARC effect and overall RTs contributes to a step-wise shape. ~~Since~~ Because statistical models should correspond to scientific models as closely as possible (Westermann & Hager, 2017), the MC-SNARC should therefore be tested with a categorical predictor.

**Influence of task order on the SNARC effect**

The influence of task order (first MC and second PJ, or reversed) on the MC-SNARC and on the PJ-SNARC has been investigated in only a few studies. Didino et al. (2019) did not find any task-order effects on the SNARC effect. Fattorini et al. (2015) did not observe any task-order effect on the PJ-SNARC but found the MC-SNARC to be weaker after PJ than when MC was the first task. In most other studies that included~~ing~~ the two tasks, ~~the~~ effects of task order have either not been reported (Cheung et al., 2015; Gevers, Verguts, et al., 2006; Nuerk, Bauer, et al., 2005; Weis et al., 2018), or could not be calculated because task order was not counterbalanced (Bae et al., 2009; Cipora, 2014; Fitousi et al., 2009; Georges et al., 2017; Zorzi et al., 2012) or because different samples completed MC and PJ (Ito & Hatta, 2004; van Dijck et al., 2009). To our knowledge, only Bulut, Roth, et al. (~~in press~~2025; see their Supplementary Materials) have tested the influence of task order, and they did not find an influence on the SNARC effect in any of the two tasks in any of three samples (130 German, 112 Turkish, and 75 Iranian participants). In fact, two opposite theoretical predictions can be made. On the one hand, the SNARC effect might be stronger in each task if it is the second, because the processing of number magnitude and its spatial mapping should be stronger if they have already been activated in a previous task. On the other hand, the SNARC effect might be weaker in each task if it is the second, because RTs typically decrease with practice and faster RTs are typically associated with a weaker SNARC effect (note that both decreasing RT and a decreasing SNARC effect over time in PJ have been found by Roth, Jordan, et al., 2024). If both mechanisms were true, they might cancel out each other and make the influence of task order invisible. Hence, we cannot make any directional prediction and will investigate the potential influence of task order in an exploratory analysis (Exploratory 1).

**The current study**

In this large-scale online study, we wish to thoroughly investigate whether the MC-SNARC is truly categorical (i.e., better described by a categorical number magnitude predictor) and the PJ-SNARC continuous (i.e., better described by a continuous number magnitude predictor). Evidence for this systematic difference would suggest that we should not ~~talk about~~call it *the* SNARC effect, but instead acknowledge that different SNARC effects exist, which are elicited depending on the task. It is crucial to shed light on this issue, because wrong measurements and interpretations of the SNARC effect can lead to misconceptions of SNAs. Another goal of the present study is to investigate the relationship between the SNARC effect(s) that will be observed in the two tasks. In the following, all replication checks, hypotheses, and exploratory tests are listed (related ones follow one another).

First, we will test the following expectations as ~~the following~~ replication checks ~~in the current study~~:

1. a SNARC effect in both MC and PJ with the standard analysis of a continuous linear regression (this positive control will be used as a basis for all further analyses, i.e., finding a ~~the~~ SNARC effect with the standard analysis in both tasks is a prerequisite for testing the three hypotheses in this study);
2. a MARC effect in PJ, but not in MC, because the activation of parity seems not to be automatic when only a semantic number feature other than parity is task-relevant;
3. shorter RTs in MC than in PJ, because processing magnitude is more straightforward and automatized than processing parity;
4. a Numerical Distance Effect in MC, which is typically found.

To summarize our three hypotheses derived above, we expect:

1. a categorical MC-SNARC, i.e., a better fit of ~~the~~a categorical than continuous MC-SNARC model
2. a continuous PJ-SNARC, i.e., a better fit of ~~the~~a continuous than categorical PJ-SNARC model;

3. (a) a Numerical Size Effect in both tasks, (b) which is stronger in MC than in PJ, because processing magnitude is task-relevant in MC but task-irrelevant in PJ.

Moreover, we will explore whether the following observations can be made~~ (without directional predictions)~~:

1. task-order effects on both (a) the MC-SNARC and (b) the PJ-SNARC;

~~2. a good model fit when including both continuous and categorical magnitude predictors for (a) the MC-SNARC or for (b) the PJ-SNARC, indicating a mixed shape of the SNARC effect (see Panel C in Figure 2);~~

~~3.~~2. compatibility-order effects on (a) the MC-SNARC (SNARC slopes in Conditions 1 and 3 versus Conditions 2 and 4) or on (b) the PJ-MARC (MARC slopes in Conditions 1 and 3 versus Conditions 2 and 4);

3. a good model fit when including both continuous and categorical magnitude predictors for (a) the MC-SNARC or for (b) the PJ-SNARC, indicating a mixed shape of the SNARC effect (see Panel C in Figure 2);

4. number 5 as the most likely boundary between "small" and "large" numbers for most individuals both in MC and PJ; and, relatedly, a categorical MC-SNARC (Hypothesis 1) and a continuous PJ-SNARC (Hypothesis 2), even when comparing the favored categorical model for each participant with the continuous model;

~~4.~~5. a shape difference of (a) the MC-SNARC or for (b) the PJ-SNARC between earlier and later phases within each task;

6. a stronger MC-SNARC than PJ-SNARC when the continuous magnitude predictor is used;

~~5.~~7. a positive correlation between ~~the~~ categorical MC-SNARC slopes and ~~the~~ continuous PJ-SNARC slopes.

We will collect data for MC and PJ with the numbers from 1 to 9 (excluding 5) in a within-subjects design using the typical bimanual response setup. Participants will be assigned to one of four conditions differing in block order, and 30 repetitions will provide reliable estimates per experimental cell (number magnitude * response side * task; see Cipora & Wood, 2017). Conducting this study online offers the possibility to test much larger samples than in most previous studies and thus reach high statistical power (Reips, 2000, 2002). The SNARC effect has been successfully replicated in online settings (Bulut, Roth, et al., ~~in press~~2025; Cipora, Soltanlou, et al., 2019; Gökaydin et al., 2018; Koch et al., 2023; Roth, Caffier, Cipora, et al., in press; Roth, Caffier, Reips, et al., ~~in press~~2025; Roth, Huber, et al., ~~2024~~in press). The measurement in the online setup showed reliability and a similar magnitude compared to the SNARC effect that is typically observed in lab studies. Further, it seems to be valid regarding correlations with mean RT and standard deviations of RT. We will calculate Bayes Factors ($BF_{10}$) to be able to quantify evidence both for differences between MC and PJ as well as for the relationship between the SNARC effects in the two tasks, and lack of such differences or such a relationship. This way, we hope to shed more light on the SNARC effect and specifically its shape in the two popular and widely used tasks.

## Method

The ethics committee of the University of Tübingen's Department of Psychology has approved of this study.

### Sample size considerations

The "Sequential Bayes Factor with maximal n" (SBF+maxN) approach described by Schönbrodt and Wagenmakers (2018)[5] will be applied to make our data collection efficient.

[5] Note that, apart from a maximum sample size, a minimum sample size needs to be defined as well, which is why it might be more reasonable to term the approach "sequential Bayes Factor with a minimum and maximum N" as done by Witt (2019).

This means that we will run the data analysis with a total of 500 participants in the first round and recruit further participants sequentially in steps of 50 until the stopping criterion or maximal sample size is reached. Our stopping criterion will be moderate evidence regarding all three hypotheses, so that the data either provides evidence in favor of ($BF_{10} > 3$) or against ($BF_{10} < 1/3$) each of them.

For the SBF+maxN approach, we need to define a maximal sample size. Thus, we will determine the sample size that is necessary to detect evidence for a true underlying effect or against a truly absent effect with a high probability (similar to power analyses in the frequentist framework). This will be done for each of the three hypothese~~i~~s and the largest required sample size will be chosen as maximal sample size for the SBF+maxN approach. Our main aim of the current study is to determine the shape of the SNARC effect in the two most common tasks. For this, we will compare the fit of a continuous and a categorical statistical model in MC and PJ separately (to test Hypotheses 1 and 2). We therefore chose the effect size of interest (ESOI) in a standardized unit, namely Cohen's $d = 0.2$ (although this is not recommended for power simulations, see Correll et al., 2020). The sample size considerations were based on this ESOI for all three hypotheses, because smaller effect sizes are not practically meaningful. Specifically, $d = 0.2$ reflects a small effect and corresponds to around 1% of explained variance (calculated according to Ruscio, 2008, using the conversion formula assuming equal-sized groups, see their Table 2). Regarding the detection of a SNARC effect while assuming similar standard deviations as reported in the literature, $d = 0.2$ corresponds to -4 in the continuous MC-SNARC (with $SD = 20$), -10 in the categorical MC-SNARC (with $SD = 50$), and -2 in the continuous PJ-SNARC (with $SD = 10$) in their measured unit (i.e., increase of right-hand advantage per continuous magnitude or categorically for large compared to small numbers in milliseconds). These SNARC slopes are of a typically observed or even small size.

Analogously to statistical power simulations in the frequentist framework, we randomly drew 5000 samples from a distribution around the ESOI ($d = 0.2$) and simulated the probability to obtain at least moderate evidence (i.e., $BF_{10} > 3$) for that effect size by looking at the proportion of Bayesian tests revealing at least moderate evidence for the alternative hypothesis (for a similar approach, see Kelter, 2021; Roth, Caffier, Reips, et al., ~~in press~~2025). Similarly, we randomly drew 5000 samples from a distribution with the respective SD around a truly absent effect ($d = 0$) and simulated the probability to obtain at least moderate evidence for the null hypothesis (see Kelter, 2021). We thereby determined the sample size that is required for a probability of .90 to obtain moderate evidence for and against the ~~six~~ three hypotheses with two-sided Bayesian paired or one-sample *t*-tests. ~~Paired or one-sample *t*-tests will be used for Hypotheses 1, 2, 3a, and 3b. Paired or one-sample *t*-tests as well as independent-samples *t*-tests and a Pearson correlation *t*-test will also be used for all replication checks and exploratory analyses.~~ The required sample size was largest for finding at least moderate Bayesian evidence for a true underlying effect of $d = 0.2$ with a probability of .90 in a two-sided Bayesian independent-samples *t*-test ($n = 2 * 850 = 1700$). The required sample sizes for finding evidence against a truly absent effect in an independent-samples *t*-test ($n = 2 * 340 = 680$), for evidence for a true underlying effect in a one-sample or paired *t*-test ($n = 440$), or for evidence against a truly absent effect in a one-sample or paired *t*-test ($n = 160$) were much smaller. We will therefore target $n = 1700$ as a maximal sample size for the SBF+maxN approach. The exact calculations and results for all tests can be found here: https://osf.io/4wpv6/.

**Participants**

We will sequentially recruit adults aged between 18 and 40 years via the recruiting platform Prolific, which checks participants' demographic variables via objective criteria rather than self-report during signup – an important issue in recruitment for Web-based research (Reips, 2021). As the study will be conducted in English, participation is only possible for native English speakers (as per Prolific's screening based on self-reports). Complete participation will be compensated with £5 (Prolific users receive their payment in Great British Pounds~~this currency~~), and incomplete participation will be compensated partially.

## Design and experimental task

The present study follows a 2 (task: MC vs. PJ) * 2 (compatibility: incompatible vs. compatible) within-subjects design, resulting in four experimental blocks per participant. Participants will be randomly assigned to one of four block orders. In Conditions 1 and 2, participants complete MC in the first and PJ in the second half of the experiment, while the task order is reversed in Conditions 3 and 4. Both blocks of each task will be kept together and presented one after the other to avoid mixing up instructions. Within each task, participants are assigned to the SNARC-/MARC-incompatible block first and to the SNARC-/MARC-compatible block second in Conditions 1 and 3, while the compatibility order is reversed in Conditions 2 and 4 (cf. Figure 1). Given the planned number of trials (see below), each of the two tasks is expected to take 15 minutes, so that the full participation including both tasks and some demographic questions will take approximately 35 minutes.

**Figure 1**

*Within-subjects manipulations and resulting block orders counterbalanced between-subjects*

| | Condition 1 | Condition 2 | Condition 3 | Condition 4 |
|---|---|---|---|---|
| Block 1 | Magnitude classification: SNARC incompatible | Magnitude classification: SNARC compatible | Parity judgment: MARC incompatible | Parity judgment: MARC compatible |
| Block 2 | Magnitude classification: SNARC compatible | Magnitude classification: SNARC incompatible | Parity judgment: MARC compatible | Parity judgment: MARC incompatible |
| Block 3 | Parity judgment: MARC incompatible | Parity judgment: MARC compatible | Magnitude classification: SNARC incompatible | Magnitude classification: SNARC compatible |
| Block 4 | Parity judgment: MARC compatible | Parity judgment: MARC incompatible | Magnitude classification: SNARC compatible | Magnitude classification: SNARC incompatible |

*Note.* We will randomly assign participants to one of the four conditions resulting from the combination of task order and compatibility order in the 2 (task: MC vs. PJ) * 2 (compatibility: incompatible vs. compatible) within-subjects design.

A binary response-key setup will be employed, requiring participants to respond as quickly and accurately as possible using a left or right key (defaults: D or K – can be adjusted individually by participants due to large technical variance on the Internet; Reips, 2000, 2021) depending on whether the number presented on the screen is smaller or larger than 5 (MC) or whether it is odd or even (PJ). In each of the experimental conditions resulting from the two within-subjects factors *task* and *compatibility*, number magnitude (1, 2, 3, 4, 6, 7, 8 vs. 9) will be manipulated. Thirty repetitions per experimental cell will lead to 240 SNARC-incompatible and 240 SNARC-compatible trials in MC, as well as 240 MARC-incompatible and 240 MARC-compatible trials in PJ per participant. Participants must take a break of a minimum of 30 seconds between blocks. The order of stimulus presentation within blocks will be randomized, with the restriction that within each block, each stimulus will be presented for the 1$^{st}$ throughout 15$^{th}$ time before each stimulus will be presented for the 16$^{th}$ throughout 30$^{th}$ time (i.e., each block is divided in two subblocks indistinguishable to the participant, in which each stimulus will be presented 15 times; note that this is necessary to investigate Exploratory 5). Each trial will start with a square (extended ASCII 254, size 72px), serving as the eye fixation point (300 ms), presented in the center of the screen. Then the number (Open Sans font, size 72px) will replace the square and remain on the screen until a response is given. A blank screen (500 ms) will conclude the trial. Stimuli as well as fixation squares will be presented in black color (0, 0, 0 in RGB notation), while the background remains gray (150, 150, 150 in RGB notation) throughout the experiment. A practice session with 16 trials will precede each block, in which each number will be presented twice. Accuracy feedback will appear during practice sessions only.

**Procedure**

At the very beginning of the experiment, a seriousness check (e.g., Reips, 2009) will be applied (i.e., participants will be asked whether they want to participate seriously). Participants will be asked to take part only if they wish to give their informed consent, if they use a computer (participation from mobile devices is not possible because a keyboard is required), and if they are ~~at least~~ between 18 and 40 years old. Then, participants will be asked to provide basic demographic data, namely age, gender, first native language, and handedness. They will also be asked about their finger counting habits, i.e., which hand they would usually start counting with (*right*, *left*, *do not know or no preferred hand*) and how stable their preference is (*always*, *mostly*, *slightly more often than with the other hand*, *do not know or no preferred hand*). In each question, participants will have the opportunity to click on “I prefer not to answer.” Next, participants may choose response keys for the experimental task that are located on the same height and about one hand width apart from each other on their keyboard, e.g., if this is not the case for the default response keys D and K. These default keys were chosen because they are located on the same height and about one hand width apart from each other on typical keyboards like QWERTZ, QWERTY, and AZERTY. Then, instructions will be displayed, and the first block of the experimental task will start with its practice trials.

After completion of both experimental tasks, data quality will be assessed by asking participants how they would describe their environment during participation (*silent*, *very quiet*, *fairly quiet*, *fairly noisy*, *very noisy*, or *extremely noisy)*, whether there were any major distractions during participation (*none*, *one*, or *multiple*), and whether there were any difficulties during participation (*yes* or *no*, text field for comments). Moreover, we asked participants whether they had used their left and right index fingers throughout the experiment, as asked for in the task instructions (*yes*, *partly*, or *no*). Participants will be provided with a completion code to be inserted in Prolific and with contact information of our research team.

The experiment has been set up with WEXTOR (https://wextor.eu; Reips & Neuhaus, 2002) in its HTML and JavaScript framework and adapted (see demo version at https://exp.wextor.eu/esnarc/task/?demo ~~https://luk.uni-konstanz.de/numcog-2/?demo~~). Our previous experiments have demonstrated that this software is suitable for detecting the SNARC effect in an online setup (Roth, Caffier, ~~Reips~~Cipora, et al., in press; Roth, Caffier, ~~Cipora~~Reips, et al., ~~in press~~2025). To prevent search engine bots (e.g., Googlebot) from submitting data ~~on~~ to our experiment, WEXTOR equips the experiment materials with a standardized “noindex, nofollow" meta tag, which prompts search engine bots not to index the experiment pages and also not to visit subsequent pages (see Reips, 2007, p. 379). Further, we will restrict participation to devices with a screen width of over 600 pixels. Additionally, to exclude multiple submissions from the same devices, we will perform checks based on User-Agents and IP addresses during data evaluation.

**Data preprocessing**

All data preprocessing steps and all statistical analyses will be performed in the ~~statistical~~ computing software R (R Core Team, 2022). As concerns data preprocessing, we will stay consistent with our previous studies and apply ~~the same~~similar inclusion criteria (Roth, Caffier, ~~Reips~~Cipora, et al., in press; Roth, Caffier, ~~Cipora~~Reips, et al., ~~in press~~2025). Only datasets of participants who complete both tasks, who indicate to be ~~at least~~ between 18 and 40 years old, and who state their intention is to seriously participate will be analyzed. Datasets will not be included for analyses if participants describe their environment as very/extremely noisy, ~~or~~ if they report multiple major distractions, or if participants do not use their left/right index finger for the left/right response key, respectively. Practice trials and incorrectly answered trials will not be analyzed. Only trials with RTs from 200 to 1500 ms will be included in the analysis. Further outliers will be removed in an iterative trimming procedure for each participant and task separately, such that only RTs that are a maximum 3 SDs above or below the individual mean RT of all remaining trials after these exclusions will be considered. Finally, only datasets of participants with at least 75% valid remaining trials per task and without any empty experimental cell (number magnitude * response side * task) will be considered.

**Data analysis**

***Confirmatory data analysis***

An overview of all replication checks and hypotheses, corresponding statistical tests, and interpretations of possible outcomes is given in the Study Design Table (see https://osf.io/4wpv6/). We will calculate Bayes Factors associated with the corresponding

Bayesian *t*-test to obtain evidence for both null and alternative hypotheses (using the R package *BayesFactor* by Morey et al., 2015, with a default *r*-scale of 0.707 as uninformed prior using Cauchy distribution). A resulting $BF_{10}$ greater than 3 or 10 will be treated as moderate or strong evidence for the alternative hypothesis compared to the null hypothesis, respectively, while a resulting $BF_{10}$ smaller than 1/3 or 1/10 will be treated as moderate or strong evidence for the null hypothesis compared to the alternative hypothesis, respectively (Dienes, 2021). Considering a $BF_{10}$ larger than 3 as evidence against the null hypothesis is more conservative than rejecting a null hypothesis in the frequentist framework with the typical significance level of $\alpha = .05$ (Wetzels et al., 2011). As explained above, we will apply the SBF+maxN approach for sequential data analysis with optional stopping in case of at least moderate evidence for or against ~~all~~ each of the three hypotheses.

Reaction times (RTs) will be measured as the time elapsing from the onset of the number presentation on the screen until a response key is pressed (within the limitations that apply in Internet-based research with consumer-grade equipment, see e.g., Garaizar & Reips, 2019). As the dependent variable, we will calculate the mean differences between reaction times (dRTs), which result from subtracting the average RT of the left hand from the average RT of the right hand for each number separately per participant and for each task separately.

Several regression models will be fit for each participant separately. In these regression models, number magnitude will be included as a predictor for dRTs to determine the shape of the SNARC effect in each task separately. First, magnitude will be included as a continuous predictor, which is equal to the actual stimulus that is displayed (e.g., 3 for number 3, and 8 for number 8). The resulting regression slopes for *continuous magnitude* represent the advantage of right-hand responses compared to left-hand responses in ms per increase by one in continuous magnitude (i.e., traditional repeated-measures regression in the SNARC effect analysis, as first proposed by Fias et al., 1996). Second, magnitude will be contrast-coded as a categorical predictor, using -0.5 for numbers from 1 to 4 and +0.5 for numbers from 6 to 9 (~~e.g.,~~

~~-0.5 for number 2, and +0.5 for number 7~~but see Exploratory 4 for a different approach). The resulting regression slopes for *categorical magnitude* represent the advantage of right-hand responses compared to left-hand responses in ms in large compared to small magnitude. Third, for the investigation of the MARC effect, contrast-coded number parity will be included as a predictor of dRTs, with -0.5 for odd and +0.5 for even numbers (as in Cipora, Soltanlou, et al., 2019). The regression slopes for *parity* represent the advantage of right-hand responses compared to left-hand responses in ms in even compared to odd numbers. An overview of all three predictors (i.e., continuous magnitude, categorical magnitude, and parity), along with their exact coding, can be found in Table 1. For each of the predictors, a more negative coefficient estimate β points towards a stronger SNARC/MARC effect.

**Table 1**

*Overview of dRT predictors*

| Continuous magnitude | 1 | 2 | 3 | 4 | 6 | 7 | 8 | 9 |
|---|---|---|---|---|---|---|---|---|
| Categorical magnitude: | | | | | | | | |
| Boundary 5 | -0.5 | -0.5 | -0.5 | -0.5 | +0.5 | +0.5 | +0.5 | +0.5 |
| Boundary 2.5 (Exploratory 4) | -0.5 | -0.5 | +0.5 | +0.5 | +0.5 | +0.5 | +0.5 | +0.5 |
| Boundary 3.5 (Exploratory 4) | -0.5 | -0.5 | -0.5 | +0.5 | +0.5 | +0.5 | +0.5 | +0.5 |
| Boundary 6.5 (Exploratory 4) | -0.5 | -0.5 | -0.5 | -0.5 | -0.5 | +0.5 | +0.5 | +0.5 |
| Boundary 7.5 (Exploratory 4) | -0.5 | -0.5 | -0.5 | -0.5 | -0.5 | -0.5 | +0.5 | +0.5 |
| Parity | -0.5 | +0.5 | -0.5 | +0.5 | +0.5 | -0.5 | +0.5 | -0.5 |

*Note.* This table gives an overview of the dRT predictors that will be used in the regression models summarized in Table 2. Continuous magnitude is equal to the actual presented stimulus. Categorical magnitude is contrast-coded with -0.5 for smaller vs.~~and~~ +0.5 for larger numbers (Boundary 5, i.e., a categorization into numbers 1 to 4 vs. 6 to 9, is used for most analyses, whereas different boundaries are used in Exploratory 4). Number parity is contrast-coded with -0.5 for odd and +0.5 for even numbers.

~~To test the~~ For Replication Checks 1 and 2, as well as Hypotheses 1 and 2, we will fit four regression models per participant and per task (for an overview, see Table 2). First, the presence of a SNARC effect in both tasks (Replication Check 1, which will be used as a positive control) will be tested in a repeated-measures regression as usually done in SNARC research (see Fias et al., 1996, adapted from Lorch & Myers, 1990). For this, dRTs will be regressed on continuous magnitude in models MC-1 and PJ-1 for each participant separately. The resulting slopes will be tested against zero in a two-sided Bayesian one-sample *t*-test, with Bayesian evidence for a difference from zero indicating a continuous SNARC effect, and negative value of the slope indicating the typical SNARC effect. ~~In an exploratory analysis, we will also test whether the MC-SNARC is stronger than the PJ-SNARC by comparing the slopes for continuous magnitude predictors resulting from models MC-1 and PJ-1 in a two-sided Bayesian paired *t*-test.~~

Then, the presence of the MARC effect (Replication Check 2) will be tested in both tasks with the same repeated-measures regression approach as the SNARC effect. For this, regression models MC-3 and PJ-3 will be computed. These models will contain a contrast-coded parity predictor and the magnitude predictor with the better fit in the previous test. Because the number-parity predictor and the number-magnitude predictor are orthogonal to each other (i.e., their correlation is zero), both can be concurrently included within one regression model without affecting the respective other parameter estimate. Then, the slopes will be tested against zero in a two-sided Bayesian one-sample *t*-test, with evidence for a difference from zero indicating a MARC effect, which is only expected in PJ but not MC.

Next, we will investigate whether the MC-SNARC and the PJ-SNARC are continuous or categorical (i.e., which number magnitude predictor fits the observed dRTs better;

Hypotheses 1 and 2). For this, besides regressing dRTs on continuous magnitude in models MC-1 and PJ-1 as previously described, they will be regressed on categorical magnitude (contrast-coded as in Table 1) in MC-2 and PJ-2. Then, we will logit-transform the $R^2$ for each model for each participant separately to approximate two normal distributions and compare the logit-transformed $R^2$ between the two models in a two-sided Bayesian paired *t*-test (as Koch et al., 2023, did this in a frequentist approach). A better fit of model MC-2 compared to MC-1 and of PJ-1 compared to PJ-2 as reflected by Bayesian evidence for a higher logit-transformed $R^2$ would indicate a categorical MC-SNARC (Hypothesis 1) and a continuous PJ-SNARC (Hypothesis 2). Additionally, we will confirm these findings via a Bayesian approach: dRTs will be regressed on continuous and categorical magnitude for both PJ and MC in four separate Bayesian models, and in each task, a leave-one-out cross validation will be performed to figure out, which of the two predictors better fits our data (using the R packages *brms* by Buerkner, 2017, and ~~the R package~~ *loo* by Vethari et al., 2017). An overview of possible SNARC effect shapes and the corresponding regression models tested in the current study can be found in Figure 2.

**Table 2**

*Overview of regression models that will be fit for each participant*

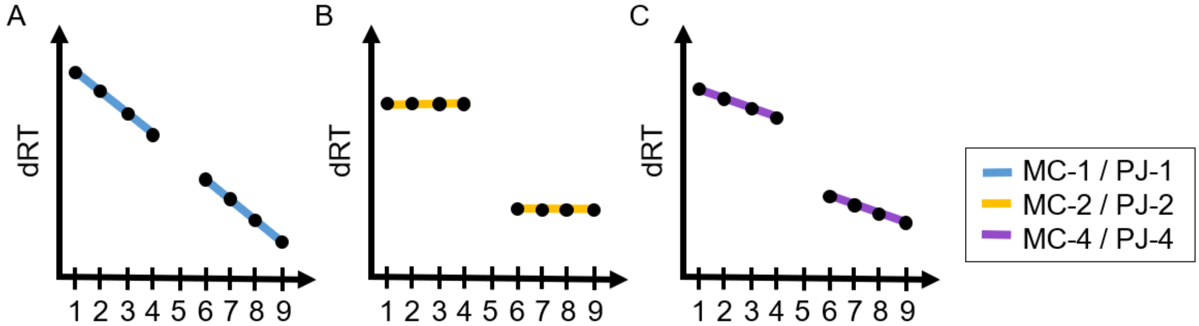
| | |
|---|---|
| **Magnitude classification** | |
| **MC-1** | $dRT \sim \beta_0 + \beta_1 * magnitude_{continuous}$ |
| **MC-2** | $dRT \sim \beta_0 + \beta_1 * magnitude_{categorical}$ |
| **MC-3**[a] | $dRT \sim \beta_0 + \beta_1 * magnitude_{continuous/categorical} + \beta_2 * parity$ |
| **MC-4** | $dRT \sim \beta_0 + \beta_1 * magnitude_{continuous} + \beta_2 * magnitude_{categorical}$ |
| **Parity judgment** | |

| | |
|---|---|
| **PJ-1** | dRT ~ $\beta_0 + \beta_1$ * magnitude$_{continuous}$ |
| **PJ-2** | dRT ~ $\beta_0 + \beta_1$ * magnitude$_{categorical}$ |
| **PJ-3**[b] | dRT ~ $\beta_0 + \beta_1$ * magnitude$_{continuous/categorical}$ + $\beta_2$ * parity |
| **PJ-4** | dRT ~ $\beta_0 + \beta_1$ * magnitude$_{continuous}$ + $\beta_2$ * magnitude$_{categorical}$ |

*Note.* Four regression models will be fit for each participant separately for MC (MC-1, MC-2, MC-3, and MC-4) and PJ (PJ-1, PJ-2, PJ-3, and PJ-4). The predictors used in the models are specified in Table 1. In each model, $\beta_1$ (and $\beta_2$) are the coefficients of the respective predictors for number magnitude or number parity. $\beta_0$ denotes the model intercept. [a] For MC-3, the better magnitude predictor from MC-1 and MC-2 will be used. [b] For PJ-3, the better magnitude predictor from PJ-1 and PJ-2 will be used.

**Figure 2**

*Different shapes of the SNARC effect*




*Note.* In Panel A, the SNARC effect is reflected by a linear regression line with a negative slope, that is, dRTs are best predicted by continuous magnitude (models MC-1 and PJ-1). In Panel B, the SNARC effect is reflected by a step-wise~~like~~ function, that is, dRTs are best predicted by categorical magnitude (models MC-2 and PJ-2), where all numbers smaller than 5 have the same dRT and all numbers larger than 5 have the same dRT. Panel C shows an intermediate shape of the SNARC effect, where both continuous and categorical magnitude predict dRTs (models MC-4 and PJ-4). The typically observed MC-SNARC appears as shown in Panel B (Hypothesis 1), and the typically observed PJ-SNARC appears as shown in Panel A (Hypothesis 2).

For Replication Checks 3 and 4 as well as for Hypotheses ~~2~~3a and ~~2~~3b, RTs will be examined in detail. First, to test whether reactions are on average shorter in MC than in PJ (Replication Check 3), we will compare mean RTs per participant between tasks in a two-sided Bayesian paired *t*-test. Next, the presence of a Numerical Distance Effect in MC (Replication Check 4) and of a Numerical Size Effect in both MC and PJ (Hypotheses 3a and 3b) will be investigated with the repeated-measures regression approach (as in Hohol et al., 2020). In MC, RTs will be regressed on numerical distance (i.e., difference between the number and the criterion number 5) and continuous magnitude (1, 2, 3, 4, 6, 7, 8, or 9) for each participant separately. Because numerical distance and magnitude are orthogonal (i.e., their correlation is zero), both can be concurrently included within one regression model without affecting the respective other parameter estimate. In PJ, RTs will only be regressed on continuous magnitude for each participant separately. Next, resulting regression slopes will be tested against zero in a two-sided Bayesian one-sample *t*-test for each task separately. Evidence for negative slopes for the numerical distance predictor indicates faster reactions for larger numerical distance, reflecting the Numerical Distance Effect (Replication Check 4). Evidence for positive slopes for the magnitude predictor indicates slower reactions for increasing number magnitude, reflecting the Numerical Size Effect (Hypothesis 3a). Last, we will test whether the magnitude of the Numerical Size Effect is stronger in MC than in PJ (Hypothesis 3b) by comparing resulting slopes between tasks in a two-sided Bayesian paired-samples *t*-test.

***Exploratory data analysis***

After the analyses for replication checks and hypotheses, we will investigate task-order effects on both the MC-SNARC and the PJ-SNARC (Exploratory 1). That is, we will test task-order effects by comparing SNARC slopes in Conditions 1 and 2 (first MC, second PJ) with Conditions 3 and 4 (first PJ, second MC) for each task separately (see Figure 1 for an overview of experimental conditions). The predictor that fits better in the respective task will be used (according to Hypotheses 1 and 2). For this, we will run two two-sided Bayesian independent-samples t-tests.

~~Next, a fourth model including both continuous and categorical magnitude will be fitted for both tasks (MC-4 and PJ-4). Both resulting slopes will be tested against zero in two-sided Bayesian one-sample *t*-tests, with Bayesian evidence for both slopes being different from zero indicating a mixed shape of the SNARC effect, as illustrated in Panel C in Figure 2 (Exploratory 2). This would mean that the dRT regression slope is negative within small and within large numbers, while there is a categorical step between numbers 4 and 6 (for an empirical observation of a such pattern, see Figure 2b in Nuerk, Bauer, et al., 2005).~~

Further, we will exploratorily test compatibility-order effects on the MC-SNARC by comparing SNARC slopes in Conditions 1 and 3 (first SNARC-incompatible, second SNARC-compatible) with SNARC slopes in Conditions 2 and 4 (first SNARC-compatible, second SNARC-incompatible; Exploratory 2~~3~~a). Note that we will use the categorical or continuous slope here depending on which of both describes the MC-SNARC better (i.e., depending on the outcome regarding Hypothesis 1). Similarly, we will test compatibility-order effects on the MARC effect in PJ by comparing MARC slopes in Conditions 1 and 3 (first MARC-incompatible, second MARC-compatible) with MARC slopes in Conditions 2 and 4 (first MARC-compatible, second MARC-incompatible; Exploratory 2~~3~~b). For this, we will run two two-sided Bayesian independent-samples *t*-tests. Evidence for a stronger SNARC/MARC effect in Conditions 2 and 4 compared to Conditions 1 and 3 would reflect larger compatibility effects when the response-key assignment is first compatible and then incompatible, and vice versa.

Next, a fourth model including both continuous and categorical magnitude will be fitted for both tasks (MC-4 and PJ-4). Both resulting slopes will be tested against zero in two-sided Bayesian one-sample *t*-tests, with Bayesian evidence for both slopes being different from zero indicating a mixed shape of the SNARC effect, as illustrated in Panel C in Figure 2 (Exploratory 3). This would mean that the dRT regression slope is negative within small and within large numbers, while there is a categorical step between numbers 4 and 6 (for an empirical observation of a such pattern, see Figure 2b in Nuerk, Bauer, et al., 2005).

Then, we will fit five categorical models for each participant per task. The models will be analogous to MC-2 and PJ-2. They will differ regarding the boundary between “small” and “large” numbers (contrast-coded with -0.5 and +0.5, respectively). Specifically, we will run five regression models while classifying 1 to 2 vs. 3 to 9, 1 to 3 vs. 4 to 9, 1 to 4 vs. 6 to 9, 1 to 6 vs. 7 to 9, and 1 to 7 vs. 8 to 9 as “small” and “large” numbers (see Table 1). Note that 1 to 4 vs. 6 to 9 corresponds to the model used to test Hypotheses 1 and 2. For each participant, we will determine the most likely underlying categorization by descriptively comparing which of the five models has the best fit to the data in terms of $R^2$. Next, we will logit-transform the $R^2$ for the favored categorical model and for the continuous model for each participant separately and compare the logit-transformed $R^2$ between the two models in a two-sided Bayesian paired *t*-test (Exploratory 4). Moreover, we will regress dRTs on continuous and categorical magnitude for both PJ and MC in four separate Bayesian models and perform a leave-one-out cross validation (as for Hypotheses 1 and 2). We will present the distribution of favored categorical models across the sample for each task.

Additionally, to check whether the determined boundary between “small” and “large” numbers is reliable for each participant and not only due to random measurement noise, we will determine its split-half reliability by splitting valid trials with the odd-even method based on presentation order (i.e., 1$^{st}$, 3$^{rd}$, 5$^{th}$, etc. trial vs. 2$^{nd}$, 4$^{th}$, 6$^{th}$, etc. trial). Subsequently, we will again compute five models per participant and per task, separately for each half of all valid trials, and determine which of the five models has the largest $R^2$ in each half of the experiment. For each half per participant within each task, we will code the boundary of the model with the highest $R^2$ with the cardinal boundary values 2.5, 3.5, 5, 6.5, or 7.5. We will then compute the Pearson product-moment correlation between the favored categorical models across halves within each task (note that coding the favorable model ordinally with 1, 2, 3, 4, and 5 to calculate the Spearman rank correlation would lead to the almost same results, as the correlation between the cardinal and ordinal values is $r$ = .997). Next, we will apply the Spearman-Brown correction to each correlation to adjust for task length and descriptively evaluate the correlations in terms of whether the boundaries between “small” and “large” numbers are related between both halves. As the boundaries can be also interpreted as categorical rather than continuous values, we will also create alluvial plots (using the R packages *ggplot2* by Wickham et al., 2024, and *ggalluvial* by Brunson & Read, 2023) showing the stability of the boundaries within participants across halves for each task separately.

Finally, we will compute the split-half reliability for the basic categorical MC-SNARC (using the classification of 1 to 4 vs. 6 to 9 as “small” vs. “large”) and continuous PJ-SNARC. These results will be taken into account when interpreting the split-half reliability of the favored categorical model (as a part of Exploratory 4). Note that, to our knowledge, the split-half reliability has never been reported for the categorical MC-SNARC. For the split-half reliability of the continuous PJ-SNARC, values between .43 and .82 have been reported in the literature (for an overview, see Cipora, Soltanlou, et al., 2019).

Moreover, we will explore whether the shape of the SNARC effect differs between earlier and later phases within each task (Exploratory 5~~4~~). Importantly, it is not possible to determine the SNARC effect in the first or second block of each task separately, because both blocks are needed in order to calculate the differences between left- and right-hand responses. Therefore, we will compute the models MC-4 and PJ-4 and test the resulting slopes for both ~~the~~a continuous predictor and ~~the~~a categorial predictor~~s~~ against zero in two-sided Bayesian one-sample *t*-tests, but instead of considering all 30 repetitions per block, we will only consider the first or second halves of both blocks within each task (i.e., first or second 15 repetitions of each number in one and in the other response-to-key assignment). This way, we can investigate whether early trials in each response-to-key assignment lead to a different SNARC shape than late trials.

Further, we will test whether the MC-SNARC is stronger than the PJ-SNARC by comparing the slopes for continuous magnitude predictors resulting from models MC-1 and PJ-1 in a two-sided Bayesian paired *t*-test (Exploratory 6).

Lastly, we will calculate Pearson's correlation between the ~~categorical~~ MC-SNARC ~~slopes~~ and ~~the continuous~~ PJ-SNARC slopes (Exploratory 7~~5~~). For this, the predictor leading to a better model fit will be used (categorical in MC according to Hypothesis 1 and linear in PJ according to Hypothesis 2). We will run a two-sided Bayesian Pearson correlation test to see whether the spatial mapping of number magnitude within participants is similar in both tasks.

**Data quality and positive controls**

To control the data quality in our study, we have implemented a seriousness check (Reips, 2009) as well as a self-assessment of noise, distractions, and other difficulties. To make sure that we will only analyze trials that reflect mental processes in correctly executed MC or PJ, we will only include correctly answered trials, trim RTs, and only include datasets with a minimum of 75% remaining valid trials (as described in the data preprocessing pipeline). Moreover, the test of the MC-SNARC and PJ-SNARC analyzed with the traditional linear regression (Replication Check 1) will serve as positive control. Importantly, we consider this positive control as a prerequisite for all further analyses and will only proceed with testing the other hypotheses if we can find at least moderate Bayesian evidence for both the continuous MC-SNARC and the continuous PJ-SNARC at the group level. Finally, in additional replication checks, ~~where~~ we aim to replicate results from previous studies to validate our investigation.

**Possible limitations and unexpected outcomes**

Importantly, including both continuous and categorical magnitude within one single regression model (as in MC-4 and PJ-4) is problematic because of collinearity. These two predictors correlate highly, namely with $r = .913$. However, we still decided to compute one such regression model for each task because the true shape of the SNARC effect might be determined by both continuous and categorical number magnitude simultaneously.

In the present study, we test the two most frequently used versions of MC and PJ (i.e., with symbolic single-digit numbers) in a sample in which the SNARC effect is not controversial (i.e., Western culture with left-to-right reading and writing direction). Future studies will show whether our results hold true for different types of stimuli and for different samples.

## Further procedure

Data collection will start after critical revisions of the current registered replication report according to peer review and is estimated to last one month. Data analysis is expected to be finished within three months after data collection.

## Data and code availability

Anonymized data and analysis scripts will be available via the Open Science Framework (https://osf.io/g48s2/).

## Author contributions

All the authors have full access to all the data and take responsibility for the integrity of the data and the accuracy of the data analysis. *Conceptualization*: K. Cipora, H.-C. Nuerk, U.-D. Reips; *Data Curation*: K. Cipora, H.-C. Nuerk, A. T. Overlander, U.-D. Reips, L. Roth; *Formal Analysis*: K. Cipora, H.-C. Nuerk, A. T. Overlander, U.-D. Reips, L. Roth; *Funding Acquisition*: K. Cipora, H.-C. Nuerk, U.-D. Reips.; *Investigation*: K. Cipora, H.-C. Nuerk, A. T. Overlander, U.-D. Reips, L. Roth; *Methodology*: K. Cipora, H.-C. Nuerk, U.-D. Reips, L. Roth; *Project*

*Administration*: H.-C. Nuerk, U.-D. Reips, L. Roth; *Resources*: H.-C. Nuerk, U.-D. Reips; *Software*: A. T. Overlander, U.-D. Reips; *Supervision*: K. Cipora, H.-C. Nuerk, U.-D. Reips; *Validation*: K. Cipora, H.-C. Nuerk, A. T. Overlander, U.-D. Reips, L. Roth; *Visualization*: L. Roth; *Writing - original draft*: L. Roth; *Writing - review and editing:* K. Cipora, H.-C. Nuerk, A. T. Overlander, U.-D. Reips.

## Competing interests

The authors declare no conflicts of interest with the content of this article.

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# Appendix A

## Table A1

*Overview of studies including magnitude classification tasks (MC) of visually presented numbers*

| **Study** | **Sample** | **Stimuli (and untypical instructions)** | **Repeated-measures regression** | | **Correlation between continuous predictors of MC and PJ** |
|---|---|---|---|---|---|
| | | | **Continuous predictor** | **Categorical predictor** | |
| Bachot et al. (2005): Control group | 16 visuospatially non-impaired children between 7 and 12 years | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -13.97 (*SD* = 26.35), $t(15) = -2.12$, $p < .05$ (one-sided) | Not reported | No PJ |
| Bae et al. (2009): Practice Task in Experiment 1 | 28 students | 2, 3, 4, 5, 6, 7, 8, 9 (classify small from 2 to 5 or large from 6 to 9) | *slope* = -31.675, $t(7) = -4.40$, $p < .01$, $R^2 = 0.7636$ | Not reported | No correlation reported |
| Bull et al. (2005): Experiment 1 | 20 deaf and 20 hearing students | 1, 2, 3, 4, 6, 7, 8, 9 (responses with mouse keys instead of keyboard keys) | Deaf: *slope* = -17.268, $t(7) = -3.21$, $p < .05$, $R^2 = 0.63$<br>Hearing: *slope* = -21.136, $t(7) = -4.17$, $p < .01$, $R^2 = 0.74$ | Not reported | No PJ |
| Bulut et al. (2024) | 130 German, 112 Turkish, and 75 Iranian adults | 1, 2, 3, 4, 6, 7, 8, 9 | Not reported | German: *slope* = -32.58 (*SD* = 63.38), $t(129) = -5.86$, $p < .001$ | German: $r = -0.02$, $p = .822$ |

| | | | | | |
|---|---|---|---|---|---|
| | | | | Turkish:<br>*slope* = -27.26 (*SD* = 73.84),<br>$t(111) = -3.91$, $p < .001$<br>Iranian:<br>*slope* = -19.42 (*SD* = 104.92),<br>$t(74) = -1.60$, $p = .113$ | Turkish: $r = -0.08$,<br>$p = .386$<br>Iranian: $r = 0.10$,<br>$p = .402$ |
| Cheung et al. (2015) | 125 students | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -5.89 (*SD* = 11.06),<br>$t(115)= -5.73$, $p < .001$ | Not reported | $r = 0.25$, $p < .05$ |
| Deng et al. (2017): basic 0-load task | 112 adults (Exp. 1: 30 spatial + 31 verbal; Exp. 2: 25 spatial + 26 verbal) | 1, 2, 3, 4, 6, 7, 8, 9 | basic 0-load task without spatial or verbal working memory load:<br>Exp. 1 spatial: *slope* = -6.08 (*SD* = 7.64),<br>$t(29) = -4.36$, $p < .01$<br>Exp. 1 verbal: *slope* = -7.09 (*SD* = 10.04),<br>$t(30) = -3.93$, $p < .01$<br>Exp. 2 spatial: *slope* = -7.59 (*SD* = 16.67),<br>$t(24) = -2.28$, $p < .05$<br>Exp. 2 verbal: *slope* = -7.33 (*SD* = 17.43),<br>$t(25) = -2.14$, $p < .05$ | Not reported | No correlation reported |
| Didino et al. (2019) | 32 adults | 1, 2, 3, 4, 6, 7, 8, 9 | Not retained in the stepwise linear regression analysis with forward and backward predictor selection based on the AIC | *slope* = -23.59 (*SE* = 7.39),<br>$t = -3.19$, $p = 0.019$, $R^2 = 0.57$ | $r = 0.06$, 95% confidence interval = [-0.30; 0.40] |
| Fattorini et al. (2015): Study 1, Supplementary material | 60 students | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -7.5 (*SD* = 11.3),<br>$t(59) = -5.1$, $p < .001$ | Not reported | $r = 0.18$, $p = .18$, 95% confidence interval = [-0.07; 0.42] |

| | | | | | |
|---|---|---|---|---|---|
| Fitousi et al. (2009): in Experiment 5 | 16 adults | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -11.652, $F(1,6) = 20.673$, $p < .01$, $R^2 = .775$ | *slope* = -68.951, $F(1,6) = 52.927$, $p < .01$, $R^2 = .904$ | No correlation reported |
| Georges et al. (2017) | 90 students | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -5.2 ($SD$ = 13.1), $t(80) = -3.57$, $p = .001$ | *slope* = -29.18, $t(1, 6) = -8.33$, $p < .001$, $R^2 = .92$ | $r = 0.2$, $p = .07$ |
| Gevers et al. (2005): Experiment 2 | 20 students | 1, 2, 3, 4, 6, 7, 8, 9 (presented on the left/right side of the screen but not centrally) | No repeated-measures regression, but instead main effect of SNARC compatibility (compatible: small-left, large-right, incompatible: small-right, large-left): $F(1, 19) = 5.97$, $p < .05$ | | No correlation reported |
| Gevers et al. (2006): Experiment 1 | 40 adults | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -3.52<br>Stepwise multiple regression reveals a better fit of the categorical predictor: $Z = 2.31$, $p < .05$ | *slope* = -17.64 | No correlation reported |
| Han et al. (2017): positive numbers | 16 students | +1, +2, +3, +4, +6, +7, +8, +9 (numbers presented with plus sign and compared to reference number +5) | Block with positive numbers: *slope* = -14.53, $t(28) = -3.51$, $p < .05$ | Not reported | No PJ |
| Herrera et al. (2008): basic task without working memory load | 18 students | 1, 2, 3, 4, 6, 7, 8, 9 | Basic tasks without phonological or visuospatial working memory load:<br>Exp. 1: *slope* = -7, $t(17) = -2.67$, $p < .05$<br>Exp. 2: *slope* = -6, $t(17) = -3.33$, $p < .01$ | Not reported | No PJ |
| Hoffmann et al. (2013) | 84 children | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -5.50, $t(69) = -0.50$, $p > .30$ (one-sided) | Not reported | No PJ |

| | | | | | |
|---|---|---|---|---|---|
| Cipora (2014) and Cipora et al. (2016) for PJ; Hohol et al. (2020, Supplementary Material 3) for MC | 100 adults (99 for PJ, and 98 for MC) | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -16.04 (*SD* = 50.57), $t(97) = -3.14$, $p = .001$ (one-sided; calculated from raw data shared by Hohol et al, 2020) | *slope* = -16.0 (*SD* = 50.6), $t(97) = -3.14$, $p = .001$ (one-sided; reported by Hohol et al, 2020) | $r = 0.06$, $p = .577$, 95% confidence interval = [-0.14; 0.25] (PJ-SNARC calculated from raw data shared by Cipora et al., 2019) |
| Hubbard et al. (2009): Experiment 5b | 8 adults (non-synaesthetic controls) | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -8.56, $F(1,6) = 19.97$, $p < .005$, $R^2 = 0.77$ | Not reported | No correlation reported |
| Ito and Hatta (2004): Experiment 3 | 28 students | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -0.08 (*SD* = 6.0), $t(27) = -0.07$, $p > .4$ (one-sided) | Not reported | MC and PJ in between-subjects design |
| Lohmann et al. (2018) | 16 students | 1, 2, 3, 4, 6, 7, 8, 9 (responses were given by clinching the left/right fist in virtual reality environment, while manipulating spatial displacement) | Close condition: *slope* = -19.91 (*SD* = 17.7), $t(24) = 1.50$, $p = .074$ (one-sided), $R^2 = 0.270$ Border condition: *slope* = -18.09 (*SD* = 19.6), $t(24) = 1.72$, $p = .049$ (one-sided), $R^2 = 0.282$ Hand condition: *slope* = -27.81 (*SD* = 28.9), $t(24) = 2.36$, $p = .014$ (one-sided), $R^2 = 0.472$ | Not reported | No PJ |

<table>
<tr><td></td><td></td><td></td><td>Extrapersonal condition: $slope = -25.03$ ($SD = 23.9$),<br>$t(24) = 2.05$, $p = .026$ (one-sided),<br>$R^2 = 0.403$</td><td></td><td></td></tr>
<tr><td>Mourad and Leth-Steensen (2017): HH and VH conditions</td><td>60 students</td><td>1, 2, 3, 4, 6, 7, 8, 9 (participants had to imagine a number line from left to right in the HH condition and from bottom to top in the VH condition before MC)</td><td>HH: $slope = -17.46$ ($SD = 34.22$),<br>$t(27) = -2.699$, $p < .012$<br>VH: $slope = -5.11$ ($SD = 16.53$),<br>$t(26) = -1.607$, $p < .120$</td><td>Not reported</td><td>No PJ</td></tr>
<tr><td>Nathan et al. (2009): fixed standard condition</td><td>18 students</td><td>1, 2, 3, 4, 6, 7, 8, 9</td><td>$slope = -34.8$,<br>$t(7) = 7.01$, $p < .001$, $R^2 = .891$</td><td>$slope = -200.7$,<br>$t(7) = -6.8$, $p < .001$, $R^2 = .988$</td><td>No PJ</td></tr>
<tr><td rowspan="2">Nuerk et al. (2005)</td><td rowspan="2">24 adults</td><td rowspan="2">1, 2, 3, 4, 6, 7, 8, 9</td><td>$slope = -3.65$,<br>$t(23) = -.89$, $p = .19$ (one-sided), $R^2 = 0.93$</td><td>$slope = -10.13$,<br>$t(23) = 1.84$, $p < .05$ (one-sided),<br>$R^2 = 0.96$</td><td rowspan="2">No correlation reported</td></tr>
<tr><td colspan="2">Repeated-measures regression with both predictors:<br>Categorical: $t(23) = -2.02$, $p < .05$, Continuous: $t(23) = -0.10$, $p = .46$</td></tr>
<tr><td>Pinto, Pellegrino, Lasaponara, et al. (2021)</td><td>15 right brain-damaged adults with left neglect (RBD N+), 17 right brain-damaged adults without left</td><td>1, 2, 3, 4, 6, 7, 8, 9</td><td>RBG N+: $slope = -42.35$ ($SD = 48.51$),<br>$t(14) = -3.27$, $p < .01$<br>RBG N-: $slope = -27.34$ ($SD = 32.15$),<br>$t(16) = -3.51$, $p < .01$<br>HC: $slope = -12.53$ ($SD = 22.09$),<br>$t(14) = -2.21$, $p < .05$</td><td>Not reported</td><td>No PJ</td></tr>
</table>

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| | neglect (RBD N-), 15 healthy controls (HC) | | | | |
| Schiller et al. (2016): Experiment 2 | 24 adults | 1, 2, 3, 4, 6, 7, 8, 9 (distance between response keys was manipulated: 6 cm vs. 57 cm vs. 108 cm) | 6 cm: *slope* = -11.62, $t(23) = 5.46$, $p < .001$<br>57 cm: *slope* = -11.37, t(23) = 4.37, p < .001<br>108 cm: *slope* = -8.34, t(23) = 3.64, p < .002 | Not reported | MC and PJ in between-subjects design |
| Shaki and Gevers (2011): Experiment 3 | 46 students | 1, 2, 3, 4, 6, 7, 8, 9 | *slope* = -5.43, $t(43) = -2.58$, $p < .01$ (one-sided), $R^2 = 0.41$ | Not reported | No PJ |
| van Dijck and Doricchi (2019) | 13 right brain-damaged adults with left neglect (RBD N+), 8 right brain-damaged adults without left neglect (RBD N-), 12 healthy controls (HC) | | RBD N+: *slope* = -63.68 (*SD* = 60.81), $t(11) = -3.63$, $p = 0.004$ (one-sided)<br>RBD N-: *slope* = -81.01 (*SD* = 52.24), $t(7) = -4.39$, $p = 0.003$ (one-sided)<br>HC: *slope* = -10.95 (*SD* = 21.85), $t(11) = -1.74$, $p = 0.055$ (one-sided) | Not reported | Spearman *r* between -0.09 and 0.13 with $p > .480$ in each group |
| van Dijck et al. (2009): Experiment 2 | 80 students | 1, 2, 3, 4, 6, 7, 8, 9 | verbal baseline: *slope* = -5.88, $t(34) = -3.34$, $p < .01$<br>spatial baseline: *slope* = -6.50, $t(35) = -4.00$, $p < .01$ | Not reported | Not reported |

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| without working memory load | | | | | |
| van Dijck et al. (2012) | 10 right brain-damaged adults with left neglect (RBD N+), 7 right brain-damaged adults without left neglect (RBD N-), 12 healthy controls (HC) | 1, 2, 3, 4, 6, 7, 8, 9 | RBD N+: *slope* = -70.87 (*SD* = 73.22), RBD N-: *slope* = -56.99 (*SD* = 39.02), HC: *slope* = -12.97 (*SD* = 23.61); all $t(8, 6, 12) < -1.90$, all $p < .05$ (one-sided) | Not reported | Not reported |
| van Galen and Reitsma (2008) | 33 7-year-olds, 29 8-year-olds, 27 9-year-olds, 18 adults | 1, 2, 3, 4, 6, 7, 8, 9 | 7-year-olds: *slope* = -47.5, $p < .001$, $R^2 = 0.13$ 8-year-olds: *slope* = -19.6, $p = .007$, $R^2 = 0.04$ 9-year-olds: *slope* = -33.6, $p < .001$, $R^2 = 0.12$ Adults: *slope* = -10.6, $p < .001$, $R^2 = 0.14$ | Not reported | No PJ |
| Weis et al. (2008): Experiment 1 | 22 students | 21, 22, 23, 24, 26, 27, 28, 29, 81, 82, 83, 84, 86, 87, 88, 89 (numbers had to be classified as smaller or larger than the reference number 50) | Unit: *slope* = -0.24, $t(18) = -0.06$, $p = .951$ | Decade: *slope* = -14.35, $t(18) = -3.37$, $p = .003$ Unit: *slope* = -66.61, $t(18) = -3.85$, $p = .001$ | Not reported |

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| Zorzi et al. (2012) | 12 right brain-damaged adults with left neglect (N+),<br>8 right brain-damaged adults without left neglect (N-) | 1, 2, 3, 4, 6, 7, 8, 9 | Not reported | N+: $t(5) = -3.33$, $p < 0.05$ (one-sided)<br>N-: $t(6) = -1.95$, $p < 0.05$ (one-sided) | Not reported |

*Note*. This table provides an overview of continuous and categorical number magnitude predictors within repeated-measures regressions for MC used in previous studies. If not specified differently, in all these studies, participants had to classify centrally presented Arabic digits as smaller or larger than the fixed reference of 5 by pressing a left or right response key. Magnitude comparison tasks where two numbers are presented next to each other are not included in this overview. For studies that additionally included parity judgment (PJ), the correlation between the continuous MC-SNARC and the PJ-SNARC slopes is reported (if available). Studies were not included if stimuli were presented in other modalities than visually (e.g., auditorily in Castronovo & Seron, 2007; Beecham et al., 2009; Weis et al., 2015), if responses were not given with the left and right index fingers using one left and one right response key (e.g., unimanual responses in Cipora, 2014; Riello & Rusconi, 2011; vocal responses in Leth-Steensen & Citta, 2016; four response keys in Santens & Gevers, 2008; fields with “left” and “right” labels on touch screen in Gevers et al., 2010), if the response rule was changing within blocks (e.g., response-to-key assignment announced before each trial in Basso Moro et al., 2018; response rule depending on stimulus in Notebaert et al., 2006; Pinto et al., 2021), if too few stimuli to differentiate the continuous from a categorical SNARC shape (e.g., 1, 2, 8, and 9 in Fischer et al., 2016; Imbo et al., 2012; Weis et al., 2018; 1, 4, 6, and 9 in Santens & Gevers, 2008) were used, if the stimulus set also included negative numbers (Fischer & Rottmann, 2005; Shaki & Petrusic, 2005), or if a distractor was included (e.g., Chinese character meaning “small” or “large” in the background in Nan et al., 2022). If not specified otherwise, all reported tests were two-sided.

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*PCI Study Design Table*

**Shape of SNARC: How task-dependent are Spatial-Numerical Associations? A highly powered online experiment**

(L. Roth, K. Cipora, A. T. Overlander, H.-C. Nuerk, and U.-D. Reips)

| Question | Hypothesis | Sampling plan[1] | Analysis Plan | Rationale for deciding the sensitivity of the test for confirming or disconfirming the hypothesis | Interpretation given different outcomes | Theory that could be shown wrong by the outcomes |
|---|---|---|---|---|---|---|
| Can a continuous SNARC effect be replicated in ~~the~~ magnitude ~~comparison~~ classification (MC) and in ~~the~~ parity~~-~~ judgment (PJ) ~~task~~? | **Replication Check 1:**<br><br>A significant SNARC effect will be observed in both MC and PJ when determined with the standard analysis of a continuous linear regression. | The Sequential Bayes Factor with maximal n" (SBF+maxN) approach (Schönbrodt & Wagenmakers, 2018) will be applied to collect data in an efficient way. The minimal sample size will be 500 participants, and more participants will be sequentially recruited in steps of 50 until the optional stopping criterion or the maximal sample size will be reached.<br><br>The maximal sample size was determined by drawing 5000 simulated datasets around the effect size of interest (Cohen's *d* = 0.2) to estimate the probability to obtain evidence for or against | 1. Regression of dRTs on continuous number magnitude (1, 2, 3, 4, 6, 7, 8, 9; see models MC-1 and PJ-1) for each task separately per participant (as in Fias et al., 1996)<br><br>2. Two two-sided Bayesian one-sample *t*-tests of SNARC slopes against zero for each task separately | This replication check aims at validating the experimental manipulation and method applied in the current study. That is, finding the SNARC effect in both tasks by using the typical analysis will be a positive control in this study, and will be considered as a prerequisite for all further hypothesis tests. The sensitivity of the tests, however, depends on the final sample size determined by the SBF+maxN approach used for the hypotheses. | If evidence is found for the continuous SNARC slopes to differ from zero and to be negative, the SNARC effect is detectable with the standard analysis, which would be in line with previous literature and lay the groundwork for further hypothesis tests.<br><br>If evidence is found against the SNARC slopes to differ from zero, no SNARC effect is observable, which is highly unlikely.<br><br>If evidence is found for the continuous SNARC slopes to differ from zero and to be positive, a reversed SNARC effect is | The SNARC effect is usually detected with the standard analysis in both MC and PJ. We strongly expect to find it in this study as well, especially with our large sample size. Not finding the continuous SNARC effect would speak against its robustness in MC and/or PJ and be very surprising.<br><br>Note that this replication check will be used as a basis for all further analyses (i.e., finding the SNARC effect with the standard analysis in both tasks is a prerequisite for testing the hypotheses in this study). |

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| | | our hypotheses in the Bayesian framework (analogously to statistical power simulations in the frequentist framework). The respective Bayesian test was conducted for the dataset in each of these 5000 simulations. Specifically, the required sample size was determined by making sure that the proportion of Bayesian tests revealing at least moderate evidence for the alternative hypothesis (BF10 > 3) or null hypothesis (BF10 < 1/3) is .90. This procedure resulted in a maximal sample size of *n* = 1700, see RMarkdown script at [https://osf.io/4wpv6/](https://osf.io/4wpv6/). | | | observable (i.e., association of small/large numbers with the right/left, respectively), which is highly unlikely. | |
| Can the presence of a MARC effect be replicated in PJ, and can its absence be replicated in MC? | **Replication Check 2:**<br><br>A MARC effect will arise in PJ, but not in MC, because the activation of parity seems not to be automatic when parity is task-irrelevant | | 1. Regression of dRTs on contrast-coded number parity (i.e., -0.5 for odd and +0.5 for even numbers; see models MC-3 and PJ-3) for each task separately per participant (as in Nuerk et al., 2004)<br><br>2. Two two-sided Bayesian one-sample *t*-tests of MARC slopes against zero for each task separately | Replication checks aim at replicating observations that are typically made in the used paradigm, instead of testing new hypotheses. The sensitivity of the tests, however, depends on the final sample size determined by the SBF+maxN approach used for the hypotheses. | If evidence is found for the MARC slopes to differ from zero and to be negative, the MARC effect is detectable. This would be in line with previous literature for PJ.<br><br>If evidence is found against the MARC slopes to differ from zero, no MARC effect is observable. This would be in line with previous literature for MC.<br><br>If evidence is found for the MARC slopes to differ from zero and to be positive, a reversed MARC effect is observable (i.e., association of odd/even numbers with the right/left, respectively), which is highly unlikely. | The MARC effect is usually detected in PJ, but not in MC. A theory accounting for this is that the spatial mapping of number parity is automatic, but only when number parity is activated by the task instructions. However, number parity is not activated when being task-irrelevant, thus no spatial mapping occurs for it. We expect a replication in this study as well, and not finding the MARC effect would be rather surprising in a large Western sample. However, because a considerable proportion of Western individuals reveals a reversed MARC effect (e.g., descriptively 60% regular and 40% reversed in Cipora, Soltanlou, et al., 2019), not finding evidence for the regular MARC effect |

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| | | | | | | would not preclude further analyses. |
| Can responses be replicated to be faster in MC than in PJ? | **Replication Check 3:**<br><br>RTs are shorter in MC than in PJ. | | One two-sided Bayesian paired *t*-test to compare mean RTs per participant between tasks against zero | Replication checks aim at replicating observations that are typically made in the used paradigm, instead of testing new hypotheses. The sensitivity of the tests, however, depends on the final sample size determined by the SBF+maxN approach used for the hypotheses. | Evidence for faster responses in MC than in PJ would be in line with previous literature. | The processing of number magnitude is highly automatized and a primitive in numerical cognition (Tzelgov et al., 2015). In contrast, the processing of number parity is not as highly automatized; it needs to be executed intentionally and is therefore slower. Finding no difference in RTs between tasks or even the reversed pattern would be highly surprising. |
| Can the Numerical Distance Effect (NDE) in MC be replicated? | **Replication Check 4:**<br><br>An NDE will arise in MC (i.e., faster reactions with increasing numerical distance between the stimulus and the reference number 5). | | 1. Regression of RTs on numerical distance (i.e., difference between the number and the criterion number 5) and continuous magnitude (1, 2, 3, 4, 6, 7, 8, or 9) for each participant separately (as in Hohol et al., 2020)<br><br>2. One two-sided Bayesian one-sample *t*-test of numerical-distance slopes against zero | Replication checks aim at replicating observations that are typically made in the used paradigm, instead of testing new hypotheses. The sensitivity of the tests, however, depends on the final sample size determined by the SBF+maxN approach used for the hypotheses. | If evidence is found for the NDE slopes to differ from zero and to be negative, the NDE is detected, which would be in line with previous literature.<br><br>If evidence is found against the NDE slopes to differ from zero, no NDE effect is observable.<br><br>If evidence is found for the NDE slopes to differ from zero and to be positive, a reversed NDE is observable (i.e., faster reactions with increasing | The NDE is usually detected in MC. We strongly expect to find it in this study as well, especially with our large sample size. Not finding the NDE would speak against its robustness and be very surprising. |

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| | | | | | numerical distance between the stimulus and the reference number 5), which is highly unlikely. | |
| What is the shape of the SNARC effect in MC? | **Hypothesis 1:**<br><br>The MC-SNARC will be categorical. | | 1. Regression of dRTs on continuous magnitude (model MC-1) and on categorical (contrast-coded with -0.5 for small and +0.5 for large numbers) magnitude (model MC-2)<br><br>2. Logit-transformation of $R^2$ for each model for each participant separately to approximate two normal distributions<br><br>3. Comparison of the logit-transformed $R^2$ between MC-1 and MC-2 in a two-sided paired *t*-test (as in Koch et al., 2023)<br><br>Additional purely Bayesian approach:<br>2. Leave-one-out cross validation to determine which of the two predictors better fits the data (using the R package brms by Buerkner, 2017, and | The main goal of this study is to determine the shape of the SNARC effect in MC and PJ (Hypotheses 1 and 2). For comparing the fit of a continuous and a categorical model against each other in MC and PJ separately, the effect size of interest (ESOI) must be chosen in a standardized unit. We determined Cohen's *d* = 0.2 as ESOI, because it is considered to be a small effect (Cohen, 1988). This ESOI was used to determine the maximal sample size for the SBF+maxN approach. | Evidence for a higher logit-transformed $R^2$ for the categorical (MC-2) than continuous (MC-1) MC-SNARC speaks for a stepwise shape of the MC-SNARC (in line with Hypothesis 1). | Results from many previous studies lead to the hypothesis that the SNARC effect is categorical in MC, but continuous in PJ. In this thorough investigation with a sufficiently large sample, we will investigate this difference systematically by comparing the fit of the two statistical models. |

| | | | | | | |
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| | | | the R package loo by Vethari et al., 2017) | | | |
| What is the shape of the SNARC effect in PJ? | **Hypothesis 2:**<br><br>The PJ-SNARC will be continuous. | | 1. Regression of dRTs on continuous magnitude (model PJ-1) and on categorical (contrast-coded with -0.5 for small and +0.5 for large numbers) magnitude (model PJ-2)<br><br>2. Logit-transformation of $R^2$ for each model for each participant separately to approximate two normal distributions<br><br>3. Comparison of the logit-transformed $R^2$ between PJ-1 and PJ-2 in a two-sided paired *t*-test (as in Koch et al., 2023)<br><br>Additional purely Bayesian approach:<br>2. Leave-one-out cross validation to determine which of the two predictors better fits the data (using the R package brms by Buerkner, 2017, and the R package loo by Vethari et al., 2017) | | Evidence for a higher logit-transformed $R^2$ for the continuous (PJ-1) than categorical (PJ-2) PJ-SNARC speaks for a linear shape of the PJ-SNARC (in line with Hypothesis 2). | |

| | | | | | | |
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| Can the numerical size effect (NSE) be found in both tasks, and does it differ between tasks regarding its strength? | **Hypothesis 3a:**<br><br>An NSE will arise in both tasks.<br><br>**Hypothesis 3b:**<br><br>The NSE will be stronger in MC than in PJ. | | 1. Regression of RTs on numerical distance (i.e., difference between the number and the criterion number 5) and continuous magnitude (1, 2, 3, 4, 6, 7, 8, or 9) for each participant separately (as in Hohol et al., 2020)<br><br>2. One two-sided Bayesian one-sample *t*-test of continuous-magnitude slopes against zero<br><br>3. One two-sided Bayesian paired *t*-test between continuous-magnitude slopes | As explained above, we chose Cohen's *d* = 0.2 as ESOI for Hypotheses 1a and 1b. We decided to use the same ESOI for Hypotheses 3a and 3b for consistency reasons. | If evidence is found for the NSE slopes to differ from zero and to be positive, the NSE is detected, which would be in line with previous literature.<br><br>If evidence is found against the NSE slopes to differ from zero, no NSE effect is observable.<br><br>If evidence is found for the NSE slopes to differ from zero and to be negative, a reversed NSE is observable (i.e., faster reactions with increasing numerical magnitude), which is highly unlikely. | The NSE is usually detected in MC and PJ. We expect to find it in this study as well, especially with our large sample size. Not finding the NSE would speak against its robustness and be surprising, although some individuals seem to reveal a reversed NSE (showing that it is less consistent as the NDE; Hohol et al., 2020). |

*Notes*. For an overview of all regression models, see Table 2 in the manuscript. $BF_{10}$ refers to the Bayes Factor, i.e., probability of the alternative hypothesis over the null hypothesis.

# Shape of SNARC: How task-dependent are Spatial-Numerical Associations?

## A highly powered online experiment

Lilly Roth

Version 1: May 27th, 2024

This script provides sample size estimations for our Registered Report on the task dependency of spatial-numerical associations and more precisely of the SNARC effect (Dehaene et al., 1993, https://doi.org/10.1037/0096-3445.122.3.371). We expect the SNARC effect in a bimanual response setup with numbers from 1 to 9 (excluding 5) to differ between magnitude classification (MC; judging smaller vs. larger than 5) and parity judgment (PJ; judging odd vs. even).

We decided to calculate Bayes Factors (BFs) in our data analysis to be able to quantify evidence both in favor and against differences in the SNARC effect between MC and PJ and their the relationship. We will interpret BFs as proposed by Dienes (2021, https://doi.org/10.1037/cns0000258): A resulting BF10, which is the BF for the alternative hypothesis (H1) over the null hypothesis (H0), will be treated as moderate or strong evidence **for H1** if it is greater than 3 or 10, respectively, and as moderate or strong evidence **for H0** if it is smaller than 1/3 or 1/10, respectively.

We will make use of the "Sequential Bayes Factor with maximal n" (SBF+maxN) approach described by Schönbrodt and Wagenmakers (2018, https://doi.org/10.3758/s13423-017-1230-y) with recruitment steps of 50, and determine the maximal sample size in this script. For this, we ran simulations of the probability to obtain evidence for a true underlying effect of the size which we consider to be minimally relevant and of the probability to obtain evidence against a truly absent effect, striving for these probabilities to be as high as 0.90.

We calculated Bayes Factors with the R package *BayesFactor* by Morey et al. (2015, https://CRAN.R-project.org/package=BayesFactor). All Bayesian tests will be run two-sided. This script was created with the R packages *rmarkdown* by Allaire et al. (2023, https://cran.r-project.org/web/packages/rmarkdown/index.html) and *knitR* by Xie et al. (2023, https://cran.r-project.org/web/packages/knitr/index.html). The script can be downloaded from https://osf.io/4wpv6/.

```
rm(list = ls())
library("BayesFactor")
library("rmarkdown")
library("knitr")
library("tinytex")
set.seed(123)
```

# Parameters for simulations

## Minimal effect size of interest (ESOI) for magnitude classification (MC) and parity judgment (PJ)

The ESOI we chose for this study must be expressed in a standardized unit, as the main aim is to compare the model fits (logit-transformed R^2) for Hypothesis 1. Specifically, we chose to use a small effect size expressed as Cohen's $d = 0.2$:

```
esoi <- 0.2
```

Note that the same ESOI will be used for Hypothesis 2 (task-order effects) and Hypothesis 3 (Numerical Size Effect). Smaller effect sizes would not be practically meaningful, because $d = 0.2$ corresponds to around only 1% of explained variance (calculated according to Ruscio, 2008, using the conversion formula assuming equal-sized groups, see their Table 2).

In the following, we estimate what SNARC slopes the ESOI $d = 0.2$ corresponds to (continuous and categorical slopes in MC and continuous slopes in PJ). That is, we convert the effect size from a standardized unit to the practical unit. This can be found out by multiplying Cohen's $d$ with a plausible standard deviation. We looked up **previously observed standard deviations** (and chose rather conservative values):

#### Continuous number-magnitude slope in MC

The following standard deviations are given in milliseconds:

26 in Bachot et al. (2005),
11 in Cheung et al. (2015),
13 on average in Deng et al. (2017),
11 in Fattorini et al. (2015),
13 in Georges et al. (2017),
6 in Ito & Hatta (2004),
25 on average in Mourad & Leth-Steensen (2017),
22 in healthy controls in Pinto et al. (2021),
24 in healthy controls in van Dijck et al. (2012)

The continuous MC-SNARC (i.e., increase in right- over left-hand advantage in milliseconds per increase in number magnitude of 1 unit) that corresponds to Cohen's $d = 0.2$ is approximately:

```
SD.MC.continuous <- 20
-esoi * SD.MC.continuous
```

```
## [1] -4
```

#### Categorical number-magnitude slope in MC

The following standard deviations are given in milliseconds:

41 in Didino et al. (2019) (SE = 7.39 for 32 participants),
51 in Hohol et al. (2020)

The categorical MC-SNARC (i.e., increase in right- over left-hand advantage in milliseconds for the switch from small to large numbers in number magnitude of 1 unit) that corresponds to Cohen's $d = 0.2$ is approximately:

```r
SD.MC.categorical <- 50
-esoi * SD.MC.categorical
```

```
## [1] -10
```

### Continuous number-magnitude slope in PJ

The following standard deviations are given in milliseconds:

12 in Shaki, fischer, & Petrusic (2009),
9 in Fattorini, Pinto, Rotondaro, and Doricchi (2015),
10 in Cipora, Soltanlou, Reips, and Nuerk (2019)

In an extensive reanalysis of existing PJ datasets, Cipora, van Dijck, et al. (2019; https://doi.org/10.31234/osf.io/bwyr3) report SD for unstandardized continuous SNARC slopes from 18 previous studies between 5.81 and 12.75.

The continuous PJ-SNARC (i.e., increase in right- over left-hand advantage in milliseconds per increase in number magnitude of 1 unit) that corresponds to Cohen's $d = 0.2$ is approximately:

```r
SD.PJ.continuous <- 10
-esoi * SD.PJ.continuous
```

```
## [1] -2
```

To sum up, as ESOI, a small effect size expressed in a standardized unit was chosen, namely Cohen's $d = 0.2$. This corresponds to a continuous MC-SNARC of -4, to a categorical MC-SNARC of -10, and a continuous PJ-SNARC of -2.

## Simulation loops

We also need to set a parameter for the number of samples to be drawn in the Bayes Factor simulations for each test:

```r
rep <- 5000
```

# One-sample *t*-test / paired *t*-test:

We will need one-sample $t$-tests for Hypotheses 1a, 1b, and 3b. We will need a paired $t$-test for Hypothesis 3a.

We try out different sample sizes (n.onesample.H1), simulate data for these sample sizes with the ESOI (note that Cohen's $d$ follows the standard normal distribution and hence $sd = 1$), calculate the Bayes Factor (BF.onesample.H1) for a test in each of 5000 iterations, and estimate the probability for finding at least moderate evidence for a true underlying effect by the proportion of iterations revealing at least moderate evidence (p.onesample.H1):

```r
n.onesample.H1 <- 440
BF.onesample.H1 <- replicate(rep, {
  d <- rnorm(n = n.onesample.H1, mean = esoi, sd = 1)
  extractBF(ttestBF(d, mu = 0, alternative = "two.sided"))$bf
})
(p.onesample.H1 <- format(round(mean(BF.onesample.H1 > 3), 3), nsmall = 3))
```

```
## [1] "0.910"
```

In order to achieve a probability of 0.90 to find at least moderate evidence (BF10 > 3) for the minimally relevant effect of $d = 0.2$, $n = 440$ datasets need to be collected.

Again, we try out different sample sizes (n.onesample.H0), simulate data for these sample sizes without any true underlying effect (mean = 0), calculate the Bayes Factor (BF.onesample.H1) for a test in each of 5000 iterations, and estimate the probability for finding at least moderate evidence against a truly absent effect by the proportion of iterations revealing at least moderate evidence (p.onesample.H0):

```
n.onesample.H0 <- 160
BF.onesample.H0 <- replicate(rep, {
  d <- rnorm(n = n.onesample.H0, mean = 0, sd = 1)
  extractBF(ttestBF(d, mu = 0, alternative = "two.sided"))$bf
})
(p.onesample.H0 <- format(round(mean(BF.onesample.H0 < 1/3), 3), nsmall = 3))
```

```
## [1] "0.900"
```

In order to achieve a probability of 0.90 to find at least moderate evidence (BF10 < 1/3) against a non-existent effect of $d = 0$, 160 datasets need to be collected.

## Independent-samples *t*-test:

We will need independent-samples *t*-test for Hypotheses 2a and 2b.

We try out different sample sizes (n1.twosamples.H1 and n2.twosamples.H1), simulate data for these sample sizes differing by the ESOI (note that Cohen's $d$ follows the standard normal distribution and hence $sd = 1$), calculate the Bayes Factor (BF.twosamples.H1) for a comparison between the two samples in each of 5000 iterations, and estimate the probability for finding at least moderate evidence for a true underlying difference by the proportion of iterations revealing at least moderate evidence (p.twosamples.H1):

```
n1.twosamples.H1 <- 850 # size of one subsample
n2.twosamples.H1 <- n1.twosamples.H1 # size of the other subsample

BF.twosamples.H1 <- replicate(rep, {
  subsample.1 <- rnorm(n1.twosamples.H1, mean = 0 + esoi, sd = 1)
  subsample.2 <- rnorm(n2.twosamples.H1, mean = 0, sd = 1)
  extractBF(ttestBF(x = subsample.1, y = subsample.2, mu = 0, alternative = "two.sided"))$bf
})
(p.twosamples.H1 <- format(round(mean(BF.twosamples.H1 > 3), 3), nsmall = 3))
```

```
## [1] "0.904"
```

In order to achieve a probability of 0.90 to find at least moderate evidence (BF10 > 3) for the minimally relevant difference between counterbalanced orders of $d = 0.2$, 850 datasets need to be collected for each order.

Again, we try out different sample sizes (n1.twosamples.H0 and n2.twosamples.H0), simulate data for these sample sizes without any true underlying difference, calculate the Bayes Factor (BF.twosamples.H0) for a comparison between the two samples in each of 5000 iterations, and estimate the probability for finding at least moderate evidence against a truly absent difference by the proportion of iterations revealing at least moderate evidence (p.twosamples.H0):

```
n1.twosamples.H0 <- 340 # size of one subsample
n2.twosamples.H0 <- n1.twosamples.H0 # size of the other subsample

BF.twosamples.H0 <- replicate(rep, {
  subsample.1 <- rnorm(n1.twosamples.H0, mean = 0, sd = 1)
  subsample.2 <- rnorm(n2.twosamples.H0, mean = 0, sd = 1)
  extractBF(ttestBF(x = subsample.1, y = subsample.2, mu = 0, alternative = "two.sided"))$bf
})
(p.twosamples.H0 <- format(round(mean(BF.twosamples.H0 < 1/3), 3), nsmall = 3))
```

```
## [1] "0.913"
```

In order to achieve a probability of 0.90 to find at least moderate evidence (BF10 $< 1/3$) against a non-existent difference between counterbalanced orders of $d = 0$, 340 datasets need to be collected for each order.

# Summary and conclusion

By simulating the probability of obtaining evidence in favor of a true underlying effect of $d = 0.2$, and against a truly absent effect of $d = 0$, we found that to achieve 0.90, we need the following sample sizes for the tests:

**One-sample *t*-test / paired *t*-test:**
evidence for H1: 440
evidence for H0: 160

**Independent-samples *t*-test:**
evidence for H1: 850
evidence for H0: 340
Note that this sample size is required per subsample, and thus needs to be doubled for the total sample size.

**Largest required sample size**
The largest total sample size required to test our hypotheses is 2 * 850 = 1700 for the independent-samples *t*-test. Thus, we will use this sample size as the maximal sample size for the SBF+maxN sampling approach with recruitment steps of 50.