

Reliability Assessment of Self-Prioritization Effect as Measured by the Perceptual Matching Task

Zheng Liu^{1,2†}, Mengzhen Hu^{1†}, Yuanrui Zheng¹, Jie Sui³,
Chuan-peng Hu^{1*}

^{1*}School of Psychology, Nanjing Normal University, Nanjing, China.

^{2*} School of Humanities and Social Sciences, The Chinese University of Hongkong (Shenzhen), Shenzhen, China.

^{3*}School of Psychology, University of Aberdeen, Old Aberdeen, Scotland.

*Corresponding author(s). E-mail(s): hu.chuan-peng@nnu.edu.cn;
hcp4715@hotmail.com;

[†]These authors contributed equally to this work.

Abstract

Recent years have witnessed a growing focus on the reliability of cognitive tasks, driven in part by the reliability paradox. This paradox stems from the observation that while cognitive tasks yield consistent experimental effects, they do not exhibit the same reliability when assessing individual differences. In this pre-registered study, we investigated the reliability of the Self Perceptual Matching Task (SPMT), a widely used tool for investigating the cognitive processes underlying the self-prioritization effect (SPE), an effect that people perform better when stimuli are associated to the self than when they are to others. We evaluated the reliability of 24 SPE measures from 17 datasets (N = 805), all utilizing the SPMT. We calculated Monte-Carlo based split-half reliability (r) and intra-class correlation coefficient (ICC2) for each SPE measure. Our findings reveal a robust group-level SPE effect across datasets. However, when it comes to individual differences, SPE measures derived from Reaction Times (RT) and Efficiency exhibit relatively higher, compared to other SPE measures, but still unsatisfied split-half reliability (approximately 0.6). Similarly, for the reliability across multiple time points, as assessed by ICC2, RT and Efficiency demonstrate low levels of test-retest reliability (close to 0.5). These outcomes uncover the presence of a reliability paradox in the context of SPMT-based SPE assessments. While nearly all the measures of SPE display robust experimental effects, their reliability are unsatisfying. We discussed the implications of the current study for future studies.

插入: in SPMT

Would it be better expressed this way: "– an effect where individuals demonstrate enhanced performance when stimuli are self-associated compared to stimuli associated with others"

The current study assessed

是不是吧we换成this/present study比较好?

was evaluated

Keywords: Self-Prioritization Effect (SPE), Self-Perceptual Matching Task (SPMT), Reliability, Multiverse

1 Introduction

The Self-Prioritization Effect (SPE) reflects individuals' biased responses towards self-related information in comparison to **that** related to others. This **phenomenon**, documented in 1950s (Cherry, 1953), holding a central position within cognitive psychology and underscoring a core facet of human cognition and self-awareness (Sui & Humphreys, 2017). This effect has been found in a broad range of cognitive tasks (Cunningham et al., 2008; Rogers et al., 1977; Sui et al., 2012). Despite SPE is often argued to be a self-specific effect, it has been challenging to be disassociated from familiarity effect. That is, the self-related stimuli, such as own objects, own faces (Keenan et al., 2000; Kircher et al., 2000; Turk et al., 2002), own voices (Hughes & Harrison, 2013; Payne et al., 2021), or own names (Constable, Rajsic, et al., 2019) are usually more familiar to participants than those other-related stimuli. To overcome such limitation, Sui et al. (2012) introduced the Self-Perceptual Matching Task (SPMT), where the self-relatedness (and other-relatedness) is acquired in the lab. In this task, participants first associate geometric shapes with person labels (e.g., circle = you, triangle = best friend, square = stranger) and then perform a matching task, judging whether a shape-label pair presented on the screen match the acquired relationship. A typical pattern from this task is that shapes associated to the self **enjoy** an advantage over shapes related to others. This SPE from SPMT was subsequently **observed by many researchers** (Constable, Elekes, et al., 2019; Golubickis et al., 2020; Golubickis et al., 2017; Hu et al., 2020), highlighting the robustness of the effect.

The reliability of SPE as measured by SPMT, however, has not been examined.

Here, reliability of a cognitive tasks refers to its consistency and dependability in producing consistent results for the same person across sessions or times (Parsons et al., 2019; Zorowitz & Niv, 2023). One commonly method to assess reliability is the Split-Half Reliability (r), where a test is divided into two halves, and the correlation between the data from these two halves is calculated. A high correlation suggests that the test is internally consistent and measures the same construct reliably (Pronk et al., 2022). Another widely used method is the test-retest reliability, which refers to the extent to which a measurement or assessment tool produces consistent and stable results over time when administered to the same group of individuals under identical conditions (Kline, 2015). Both methods are from classical test theory in psychometrics (Borsboom, 2005), but they are less known to experimental psychologists. In experimental research, researchers usually focus on the robustness of experimental effects. Robustness, in this context, pertains to the extent to which a cognitive task consistently produces the same effect at the group level across various independent participant samples. For example, the "group effect" in the Stop-Signal Task refer to differences in reaction times between different stop signal delays (Hedge et al., 2018). An effect is considered robust if these differences can be consistently observed in different samples performing the stop-signal task.

information

effect?

has been subsequently replicated by many studies

exhibit

Although the replicated SPE exists, as measured by SPMT, the reliability of this effect has not

绿色部分是不是可以放开头?

In recent years, driven by a growing interest in employing cognitive tasks to assess individual differences, researchers have turned their attention to evaluating the reliability of cognitive tasks (e.g., Karvelis et al. (2023) and Kucina et al. (2023)). However, existing findings have raised concerns about the reliability of many cognitive tasks (Hedge et al., 2018; Rouder & Haaf, 2019), with a considerable body of research highlighting the moderate to low level reliability found in the cognitive task measurements (Clark et al., 2022; Enkavi et al., 2019; Green et al., 2016). For instance, Hedge et al. (2018) report a range of test-retest reliabilities pertaining to frequently employed experimental task metrics (such as Stroop and Stop-Signal Task), with a notable prevalence of discrepancy between the low reliability for individual differences and the robustness of the experimental effects. This discrepancy, named as the “reliability paradox” (Logie et al., 1996), has gain much attention in recent years. As other cognitive tasks, SPMT is also employed by researchers as a measure of individual differences in SPE. For example, a recent study examine the individual difference of SPE and how these individual differences are correlated to brain network (Zhang et al., 2023). Likewise, in clinical investigation, the SPMT has been incorporated to assess deviations in self-processing among specific populations, including individuals affected by autism or depression (Hobbs et al., 2023; Y. S. Liu et al., 2022). This trend calls for assessing the reliability of SPMT as a measurement of SPE.

Further, the variability in quantifying SPE calls for a comprehensive examination of the reliability of different SPE measures. As simple as the SPMT, there are multiple approaches to quantify the SPE, encompassing various indicators and baselines. In a typical SPMT experiment, two direct outcomes are generated: Reaction Times (RT) and choices. The RT and Accuracy (ACC) are two most widely used indicators of SPE. Several other indicators can be derived from reaction times and choice: Efficiency (η) (Humphreys & Sui, 2015; Stoeber & Eysenck, 2008), sensitivity score (d -prime) of Signal Detection Theory (SDT) (Hu et al., 2020; Sui et al., 2012), and drift rate (v) and starting point (z) estimated using the Drift Diffusion Model (DDM) (Golubickis et al., 2017). In addition to the variability of indicators, SPE can be estimated by calculating the difference between self condition and different baselines. Indeed, the selection of baselines varies across studies, including “Close other” (e.g., Friend) (Navon & Makovski, 2021; Svensson et al., 2022), “Stranger” (Constable et al., 2021; Orellana-Corrales et al., 2020), “Celebrity” (e.g., “LuXun”) (Qian et al., 2020) and “Non-person” (e.g., None) (Schäfer & Frings, 2019). As a result, two pivotal questions regarding the reliability of the SPMT remain unresolved: First, given the variability of indicators (RT, ACC, d' , η , v , z) and choice of baseline conditions (“Close other”, “Stranger”, “Celebrity”, and “Non-person”), which way of quantifying SPE is the most reliable one(s)? Second, is the SPE measured by SPMT suitable for assessing individual differences? Addressing these questions is crucial for establishing the formal reliability analysis of SPMT measurements, allowing for accurate assessment of the SPE and its implications in various domains.

To address these two questions, the present study investigated the reliability of SPE measures computed using different indicators under various baseline conditions in the SPMT. This was achieved by re-analyzed 17 independent datasets ($N = 805$) from 9 papers and 2 unpublished projects that employees SPMT. In order

to comprehensively assess the SPE measures derived from SPMT, we examined six indicators (RT, ACC, d -prime, η , v , z) under four baseline conditions (“Close other”, “Stranger”, “Celebrity”, and “Non-person”). We first assessed the experimental effect of these SPE measures using meta-analytical assessments. Given the methods available for evaluating the reliability of cognitive tasks, we examined the individual level consistency via permutation-based Split-half reliability (r), and consistency of task performance over time using Intraclass correlation coefficient (ICC2, Two-way random effect model). The findings of our study provide valuable insights into the reliability of SPMT and its indicators, having the potential to facilitate the future utilization of SPMT in research, clinical settings, and personal performance monitoring.

2 Methods

2.1 Ethics Information

As this study is a secondary analysis of pre-existing data sourced from publicly available datasets or archived data previously collected by the author’s group, informed consent and confidentiality are not applicable.

2.2 Experimental Design

Here we provide a detailed overview of the original experimental design of SPMT, as described in the Experiment 1 by Sui et al. (2012). The original SPMT used a 2 by 3 within-subject design. The first independent variable, labeled “Matching,” consisted of two levels: “Matching” and “Non-matching”, indicating whether the shape and label were congruent. The second independent variable, labeled “Identity”, comprised three levels: “Self”, “Friend”, and “Stranger”, representing the corresponding identity associated with the shape.

The original SPMT consisted of two stages (refer to Fig. 1). In the first instruction stage (learning stage), participants were instructed to associate three geometric shapes (circle, triangle and square) with three labels (self, friend, and stranger) for approximately 60 seconds. The shape-label associations were counter-balanced across participants. In the second phase (formal experimental phase), participants completed a perceptual matching task. Each trial started with a fixation cross displayed in the center of the screen for 500 ms, followed by a shape-label pairing and fixation cross for 100 ms. the screen then went blank for 1500 ms, or until a response was made. Participants were required to judge whether the presented shape and label matched the learned associations from the learning phase and respond as quickly and accurately as possible by pressing one of two buttons within the allotted timeframe. Prior to the formal experimental phase, participants completed a training session consisting of 24 practice trials. After the training, participants completed six blocks of 60 trials in the matching task, with two matching types (matching/non-matching) and three shape associations, for a total of 60 trials per association. Short breaks lasting up to 60 seconds were provided after each block.

needed/should to

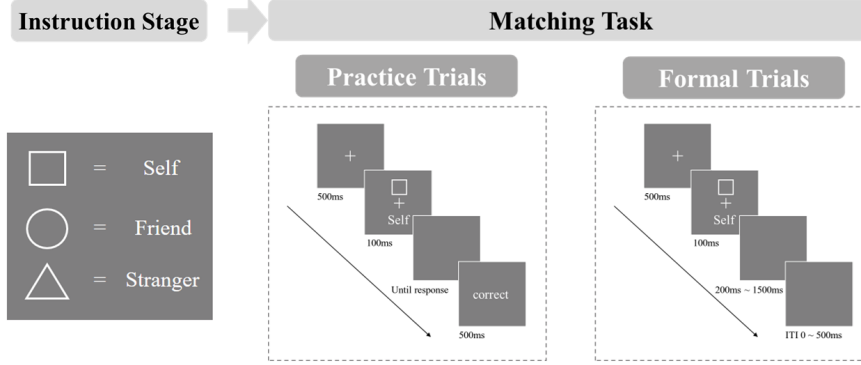


Fig. 1 Procedure of the original SPMT in Experiment 1 (Sui et al., 2012). *Note:* The relation between shape-label pairs was counter-balanced between participants.

2.3 Datasets Acquisition

Initially, two datasets that employed the SPMT were available to us: one from an unpublished project conducted in our laboratory (Hu et al., 2023), for which we provide more details in the supplementary materials, and the other provided by our collaborators (Liu et al., 2023). Concurrently, we are conducting a meta-analysis on SPE using the SPMT (pre-registration available at OSF (<https://osf.io/euqmf>)). During this process, we identified an additional 13 papers with datasets potentially suitable for our present study. The selection of these papers was based on specific criteria:

- 1) The paper must primarily utilize the SPMT as their method.
- 2) The experimental design should not incorporate any stimuli that could potentially trigger a familiarity effect (e.g., using self-face, self-name).
- 3) The trial-level data is either openly available or declared to be obtainable upon request, enabling us to estimate at least one reliability index.

Among the 13 papers included, 7 papers made their trial-level data publicly available (Constable & Knoblich, 2020; Constable et al., 2021; Golubickis & Macrae, 2021; Navon & Makovski, 2021; Qian et al., 2020; Schäfer & Frings, 2019; Svensson et al., 2022). For the remaining 6 papers, we reached out to the authors and requested access to their trial-level data. Out of those 6 requests, 3 papers provided us with trial level data (Kolvoort et al., 2020; Woźniak et al., 2018; Y. Xu et al., 2021). However, in one article, the author did not provide the explanation of the shape and label in the original data (Kolvoort et al., 2020). As a result, we were unable to analyze the raw data in this context. Two papers provided us only with descriptive results (Cheng & Tseng, 2019; Martínez-Pérez et al., 2020), which unfortunately could not be used for calculating reliability. Additionally, one paper referred to data being shared on the Open Science Framework (OSF) platform (<https://osf.io/pcv3u/>) (Bukowski et al.,

2021), but we found that the repository was empty, making it ineligible for the current analysis.

In total, our analysis comprised raw data from 9 papers and 2 unpublished projects from our laboratory and collaborators. It is important to highlight that the research culture discourages direct replications (Makel et al., 2012). As a result, all the datasets included in our analysis underwent some degree of modification to the original design (e.g., change shapes, modify sequence) as well as including additional independent variables (refer to Table 1 for specification). For our analysis, we focused exclusively on using data that adhered to the original design of SPMT, without incorporating any stimuli that could potentially trigger a familiarity effect. Additionally, we specifically utilized data from control conditions in the studies, excluding any conditions that involved the presence of other independent variables (e.g., mood changes). In the end, we were able to incorporate 17 independent datasets from the above-mentioned papers and projects. Nonetheless, not all studies incorporated retest sessions. If a publicly available datasets did not include retest session with SPMT, we excluded it from calculating the Intraclass correlation coefficient and only considered the split-half reliability. The details of the included studies and conditions in the datasets are described in Table 1.

2.4 Analysis

Analysis plans for this study were preregistered on OSF (<https://osf.io/pcv3u/>). All analyses in this paper were performed using the statistical software R (Dalgaard, 2010). The drift rate (v) and starting point (z) of the Drift-Diffusion Model (DDM) was **obtained** using the “RWiener” package (Wabersich & Vandekerckhove, 2014).

The visual representation of the current study’s road-map can be found in Fig. 2 and will be further elucidated in the subsequent sections. After collecting the data from each independent study, we performed data **cleaning** and then calculated the six indicators as well as the twenty-four SPE measures computed using different indicators and baseline conditions. Then, we computed the effect sizes (Hedges’ g) of these SPE measures. In addition to effect sizes, we calculated the split-half reliability of these SPE measures. If there were test-retest data, we also calculated the test-retest reliability using the intraclass correlation coefficient (ICC2, see Section 2.4.4 for details).

2.4.1 Data Pre-processing

For all the seventeen datasets (see Table 1), we applied the following exclusion criteria for excluding data:

1. Participant Exclusion Criteria
 - (i) Participants who had wrong trial numbers because of procedure errors is excluded from the analysis,
 - (ii) participants with an overall accuracy < 0.5 is excluded from the analysis,
 - (iii) participants with any of the conditions with zero accuracy is excluded from the analysis.
2. Trial Level Data Exclusion Criteria

Table 1 Datasets Information

Author & Publication Year	Study	Independent Variable				# of Trials per Condition	SPE Indices					Reliability	
		IV 1	IV 2	IV 3	IV 4		ACC	d	Eff	ψ	z	ICC	SHR
Hu et al. (2023)	1	Matching	Identity	Emotion Control , Neutral, Happy, Sad	Session 1-6	33	✓	✓	✓	✓	✓	✓	✓
Constable and Knoblich (2020)	1	Matching	Identity	Switch Identity Partner, Stranger	Phase 1-2	92	✓	✓	✓	✓	✓	✓	✓
Constable et al. (2021)	2	Matching	Identity Self; Stranger Identity Self; Stranger; Celebrity Identity Self; Celebrity Identity Self; Mother, Acquaintance Identity	---	---	56	✓	✓	✓	✓	✓	✓	✓
Qian et al. (2020)	1	Matching	Stranger Identity Self; Stranger; Celebrity Identity Self; Celebrity Identity Self; Mother, Acquaintance Identity	Cue With, Without	---	25	✓	✓	✓	✓	✓	✓	✓
Schäfer and Frings (2019)	2	Matching	Stranger; Celebrity Identity Self; Celebrity Identity Self; Mother, Acquaintance Identity	Cue With, Without	---	32	✓	✓	✓	✓	✓	✓	✓
	1	Matching	Celebrity Identity Self; Mother, Acquaintance Identity	Cue With, Without	---	35	✓	✓	✓	✓	✓	✓	✓
Golubickis and Macrae (2021)	1	Matching	Identity	Presentation Mixed ; Blocked	---	30	✓	✓	✓	✓	✓	✓	✓
Navon and Makovski (2021)	1	Matching	Identity	---	---	13	✓	✓	✓	✓	✓	✓	✓
	3	Matching	Identity Self; Father; Stranger Identity Self; Friend Identity Self; Friend Identity Self; Frequency Self > Friend	---	28	60	✓	✓	✓	✓	✓	✓	✓
Svensson et al. (2022)	4	Matching	Stranger Identity Self; Friend Identity Self; Friend Identity Self; Frequency Self > Friend	---	27	60	✓	✓	✓	✓	✓	✓	✓
	1	Matching	Stranger Identity Self; Friend Identity Self; Friend Identity Self; Frequency Self > Friend	---	20	50	✓	✓	✓	✓	✓	✓	✓
Y. Xu et al. (2021)	2	Matching	Friend Identity Self; Friend Identity Self; Frequency Self > Friend	---	24	100	✓	✓	✓	✓	✓	✓	✓
	3	Matching	Friend Identity Self; Friend Identity Self; Frequency Self > Friend	---	25	100	✓	✓	✓	✓	✓	✓	✓
Wozniak et al. (2018)	1	Matching	Friend Identity Self; Friend Identity Self; Frequency Self > Friend	Tasks Modified; Unmodified	---	105	✓	✓	✓	✓	✓	✓	✓
	2	Matching	Friend Identity Self; Friend Identity Self; Frequency Self > Friend	Facial Gender Male; Female	---	18	✓	✓	✓	✓	✓	✓	✓
Liu et al. (2023)	1	Matching	Identity Self; Stranger	Facial Gender Male; Female	---	18	✓	✓	✓	✓	✓	✓	✓
	2	Matching	Identity Self; Stranger	Facial Gender Male; Female	---	16	✓	✓	✓	✓	✓	✓	✓

Study represents different studies from a single article; IV: independent variable. For IV3 and IV4, we only included the baseline conditions that are similar to the original design in Sui et al. (2012), which were highlighted in **BOLD** font. If other variables that could be counterbalanced are indicated by underscores, we will solely utilize these variables as stratification variables during the split-half process

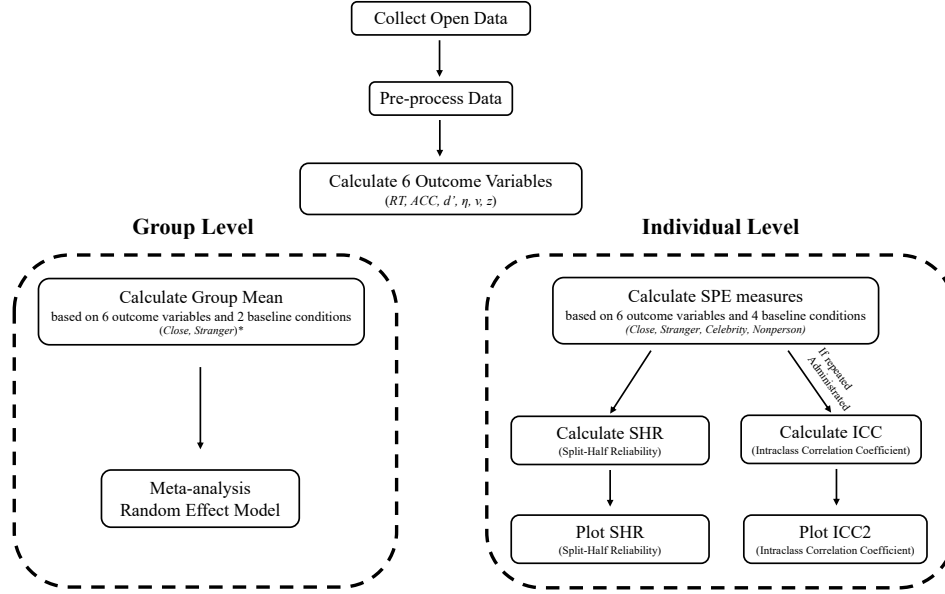


Fig. 2 Roadmap of the current study. *Note:* Only one paper have Celebrity and Nonpersons baseline, thus no included in the meta-analysis

- (i) Trials where the keypress occurs outside the two required keys and non-responsive trials are excluded from the analysis,
- (ii) the practice trials are excluded,
- (iii) the experimental design involved independent variables more than self-referential and matching, e.g., included valence of emotion as a third independent variable.

2.4.2 Calculating the Indicators and SPE Measures

For each study, we calculated six indicators for each experimental condition: Mean Reaction Time (RT), Accuracy (ACC), Sensitivity Score (d'), Efficiency (η), Drift Rate (v), and Starting Point (z). Reaction Time and Accuracy **are** obtained directly from the datasets, while Sensitivity Score and Efficiency are calculated based on Reaction Time and Accuracy. SPE Measures were computed using different indicators under four baseline conditions (see Table 2).

要不要和上一句语态一样用过去式？

2.4.3 Estimating the Robustness of SPE

Calculation of Effect Sizes. In this study, the robustness of experimental effects (group-level effect) of SPE in SPMT was calculated using meta-analytical assessments. In our analysis of effect sizes, we employed a random effects model as the primary approach, given the anticipated heterogeneity among participant sample (Page et al.,

Table 2 Indicators and SPE Measures Calculation

Indicators		Indicators Calculation	SPE Measures Calculation	Source
Reaction Time (RT)		Total Reaction Time / Total Responses	RT _{other-matching} - RT _{self-matching}	Sui et al. (2012)
Accuracy (ACC)		# of Correct Responses / Total Responses	ACC _{self-matching} - ACC _{other-matching}	Sui et al. (2012)
d' -prime (d')		$Z(\text{Hits}) - Z(\text{False Alarms})$	$d'_{\text{self-matching}} - d'_{\text{other-matching}}$	Sui et al. (2012)
Efficiency (η)		MRT/ACC	$\eta_{\text{self-matching}} - \eta_{\text{other-matching}}$	Humphreys and Sui (2015) and Stoerber and Eysenck (2008)
Drift Rate (v)		Parameters decomposed from RT based on standard DDM	$v_{\text{self-matching}} - v_{\text{other-matching}}$	Golubickis et al. (2017)
Starting Point (z)			$z_{\text{self-matching}} - z_{\text{other-matching}}$	Golubickis et al. (2017)

Note: $Z(\cdot)$ denotes the calculation of Z-score. In this context, “hit” refers to the ACC in matching trials, while “false alarm” refers to the error rate (1- ACC) in mismatch trials; the condition “Other” vary across contrast, we calculated the SPE for each “Other” condition. More specifically, we calculated the differences for “Self vs Close”, “Self vs Stranger”, “Self vs Celebrity” and “Self vs Non-person”.

感觉把上一个逗号改成句号，which改成hedges'g不容易混淆

2021). The effect size index used for all outcome measures is Hedges' g , a correction of Cohen's d that accounts for bias in small sample sizes (Hedges & Olkin, 1985), which represents the magnitude of the difference between the self and baseline condition. Descriptive statistics, including sample size, mean, and standard deviation, are employed to calculate Hedges' g from the datasets. When calculating Hedges' g , we have reversed scored the effect size for variables with negative values (Reaction Time, Efficiency). Conversely, for all indicators, a positive effect size indicates a bias towards associating stimuli with the self rather than with baseline associations. For the estimation and interpretation of effect sizes, effect size around 0.2 was interpreted as small effect size, around 0.5 as medium effect size, and around 0.8 as large effect size (Fritz et al., 2012; Hedges & Olkin, 1985).

2.4.4 Estimating the Reliability of SPE

Split-half reliability. We assessed the split-half reliability by first splitting the trial-level data into two halves and calculating the Pearson correlation coefficients. To ensure methodological rigor, we used four data splitting approaches for splitting the trial-level data: first-second, odd-even, permuted, and Monte Carlo (Kahveci et al., 2022; Pronk et al., 2022). The first-second approach splits trials into the first half and the second half. The odd-even approach splits the trials into sequences based on their odd or even numbers. The permutation approach shuffled the trial order and randomly assigned trials to two halves. The Monte Carlo approach is similar to the permutation approach, but iterates the process multiple times, usually thousands of times, to calculate the average and 95% confidence intervals of the split-half reliability. In our analyses, we first stratified the trial-level data for each participant in the study based on experimental conditions. For example, in the case of a 2 by 3 within-subject design, we stratified the data based on the two independent variables: identity (self, stranger, friend) and matching (matching, non-matching). Subsequently, we applied the four splitting approaches (Pronk et al., 2022). When using Monte Carlo approach, we randomly spitted the stratified data into two halves for 5000 times, which resulted in 5000 pairs of two halves of the data. Next, we calculated 5000 Pearson correlation coefficients for these 5000 pairs. After that, we calculated the mean and 95% confidence intervals of the 5000 correlations coefficients. The first-second split, odd-even split, and permuted split were similar to the Monte Carlo approach except each of these approaches only resulted in a single reliability coefficient. Finally, after computing the split-half reliability coefficients for each dataset, substantial variations were observed across the datasets. To derive a more accurate estimation of the average split-half reliability for each SPE measures, we synthesized these reliability coefficients via a meta-analytical approach. We weighed the reliability coefficients based on the trial numbers of each study since the number of trials typically significantly influences the reliability of cognitive experiments (Kucina et al.2023) (see also Supplementary Fig. 5 for our exploratory analysis). The weighted-average reliabilities was calculated use the “aggregate.escale” function in the “metafor” Package (Viechtbauer, 2010). We reported the synthesized split-half reliability and its 95% confidence interval in the results section. Although there is no strict criterion for defining the level of split-half reliability for psychological and educational measures, a widely accepted guideline

for split-half reliability coefficient is that a value of 0.5 is “poor”, a value of 0.70 is “acceptable”, and a value greater than 0.8 means excellent reliability (Cicchetti & Sparrow, 1981).

Test-Retest Reliability (ICC). The Intraclass Correlation Coefficient (ICC) serves as a widely recognized measure for evaluating test-retest reliability (Fisher, 1992). Differing from the Pearson correlation coefficient, which primarily quantifies the linear association between two continuous variables, the ICC extends its prowess to scenarios involving multiple measurements taken on the same subjects, while also considers both the correlation and agreement between multiple measurements, making it a more comprehensive measure of test-retest reliability (Koo & Li, 2016). Since our primary aim is to evaluate the appropriateness of the SPMT in assessing individual differences and repeated administration, to achieve this objective, we assessed the test-retest reliability of the six indicators for our dataset that involved test-retest sessions using the function “ICC” in the “psych” package (Revelle, 2017). We focused on using the Two-way random effect model (ICC2) within the ICC family (Chen et al., 2018; T. Xu et al., 2023). ICC2 gives an estimate of the proportion of total variance in measurements that is attributed to between-subjects variability (individual differences) and within-subjects variability (variability due to repeated measurements) (T. Xu et al., 2023). For the calculation of ICC2 estimates, the formula is:

$$ICC2 = \frac{MSBS - MSE}{MSBS + (k - 1)MSE + \left(\frac{k}{n}\right)(MSBM - MSE)}, \quad (1)$$

where $MSBS$ is the mean square between subjects, MSE is the mean square error, $MSBM$ is the mean square between measurements, k is the number of measurements, n is number of participants.

The traditional benchmarks for interpreting ICC values are as follows: ICC less than 0.50 suggests poor reliability; ICC between 0.50 and 0.75 suggests moderate reliability; ICC between 0.75 and 0.9 suggests good reliability; ICC above 0.9 suggests excellent reliability (Cicchetti & Sparrow, 1981; Kupper & Hafner, 1989).

3 Deviation from Preregistration

We adhered to our pre-registration plan as much as possible, however, there were a few differences between the current report and pre-registration document. First, in our initial preregistration plan, we did not anticipate conducting an analysis on the group-level effect of SPE due to the perceived robustness of the effect across a diverse range of research. However, as our study progressed, we recognized the value in providing a more comprehensive assessment. Thus, we included a estimation of pooled effect sizes across included study to represent the group-level effect. Second, we used a different algorithm for estimating parameters of the drift-diffusion model. In the registration, we planned to estimate the drift rate (v) and starting point (z) of the Drift-Diffusion Model using the “fit_ezddm” function from the “hausekeep” package (Lin et al., 2020). This function served as a wrapper for the EZ-DDM function (Wagenmakers et al., 2007). However, we observed limitations in the algorithm’s ability to accurately estimate parameter z during parameters recovery (details provided in the Supplementary Materials, section 2). After comparing the 5 algorithms, we found

that the “RWiener” package (Wabersich & Vandekerckhove, 2014) achieved a favorable balance between accuracy, confidence interval and computational efficiency, making it the most suitable choice for our analysis. Nevertheless, for transparency, we have included the results from ezDDM in the supplementary materials (see Supplementary, Fig. 2-4). Third, we did not explicitly state in the preregistration report that we would perform a weighted average of the Monte Carlo split-half reliabilities for all obtained studies. However, considering that the number of trials has a significant impact on reliability (Kucina et al., 2023), during the formal analysis, we assigned different weights to each study based on the number of trials and performed a weighted average of the split-half reliabilities. Forth, in our original preregistration, we outlined our intention to include both ICC2 and ICC2k in our data analysis. However, as our understanding of Intraclass Correlation Coefficients (ICC) improved, we realized that ICC2 is the appropriate index for our research purpose. More specifically, ICC2k was mentioned in the preregistration as an index of robustness of group-level effect, but it turned out to be another index of reliability for individual differences. We corrected this misinterpretation of ICC2k in the final report. Fifth, we conducted exploratory analysis using the data we collected to investigate the relationship between the number of trials, Monte Carlo split-half reliability, and effect size (Hedges’ g) (refer to Supplementary Fig. 6-8). Finally, the writing of the current manuscript was improved based on the preregistration. For example, in our preregistration, we included different baseline conditions when calculating SPE in the method section but did not mention this in our introduction and abstract. In this final report, we improved the writing and adjusted the introduction and abstract accordingly.

4 Results

has?

In seventeen independent datasets, 14 of them contain data for the Close other, 13 of them contain data for Stranger, 1 of them have the data for Celebrities, 1 of them has the data for Nonperson. Since there is only one paper for “Celebrity” and one for “Nonperson”, their results were less robust and were presented in the supplementary materials.

4.1 Group Level Effect of SPE

We conducted a meta-analytical assessment to examine the robustness of SPE as measured by SPMT. We used random effect model to synthesize the effect across different studies, with Hedges’ g as the index of effect size. We found that all measures of SPE, except the parameter z estimated from DDM, exhibited moderate to large effect sizes (see Table 3 for numeric results for all six SPE measures and Fig. 3 for forest plots of effect sizes for RT). Our findings indicated a robust and substantial group-level effect of SPE. There were strong evidence of homogeneity among studies. The I^2 value, all being greater than 75%, indicates high heterogeneity among studies, justifying the selection of the random effect model (Borenstein et al., 2021).

Table 3 Meta-analytical Results of SPE Measures in SPMT

Baseline	Indicators	g [95%CI]	No. of Studies	Q	p	I^2
Close	RT	0.47 [0.30, 0.63]	14	68.67	< .001	84.94%
	ACC	0.73 [0.42, 1.03]	14	144.57	< .001	92.87%
	d'	0.44 [0.28, 0.59]	14	81.96	< .001	83.02%
	η	0.88 [0.50, 1.25]	14	128.47	< .001	94.67%
	v	0.54 [0.32, 0.76]	14	142.79	< .001	91.16%
Stranger	z	0.15[-0.03, 0.33]	14	122.30	.11	88.95%
	RT	0.59 [0.40, 0.78]	13	55.30	< .001	83.20%
	ACC	0.78 [0.48, 1.08]	13	77.78	< .001	88.60%
	d'	0.35 [0.21, 0.50]	13	47.81	< .001	75.38%
	η	0.92 [0.56, 1.29]	13	98.79	< .001	93.30%
	v	0.44 [0.28, 0.59]	13	50.98	< .001	79.33%
	z	0.08[-0.09, 0.24]	13	70.48	.37	84.44%

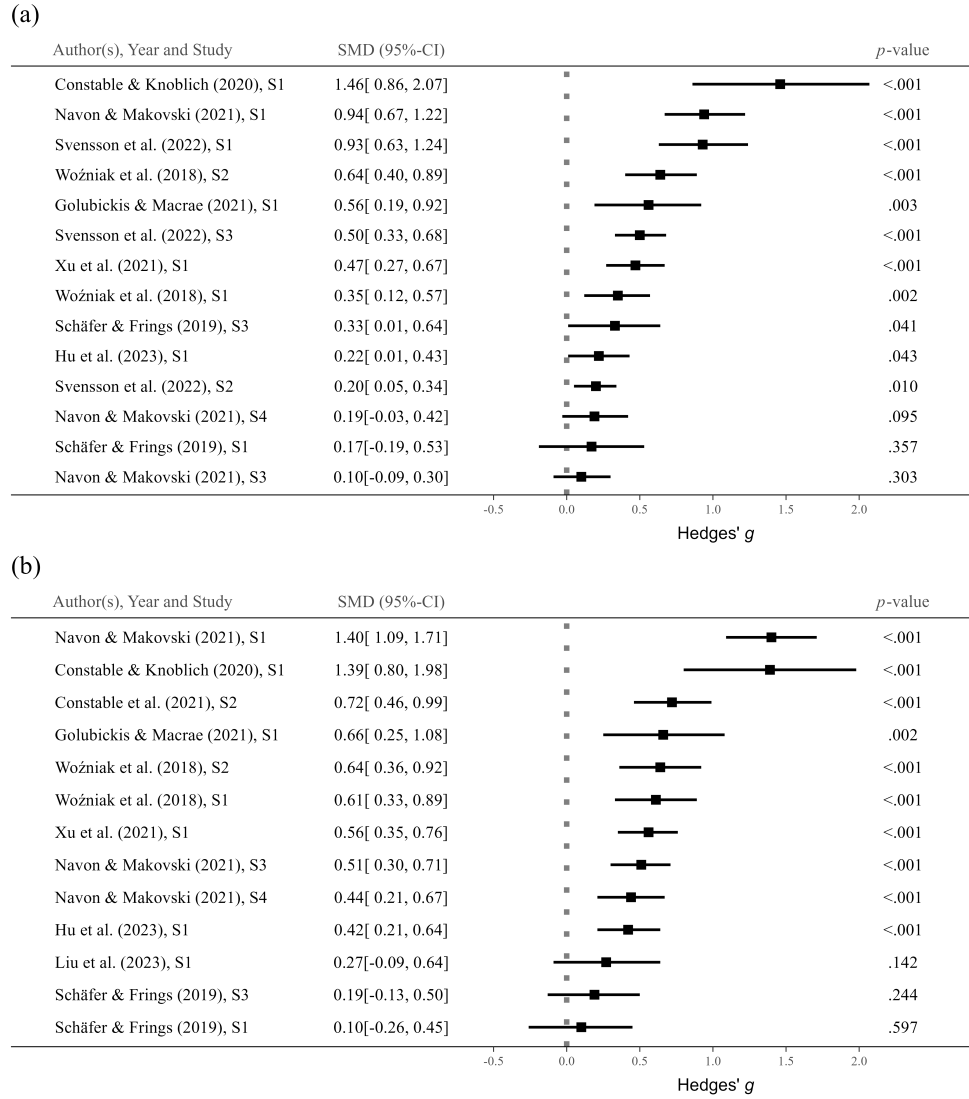


Fig. 3 (a)Forest Plot for RT for “Self – Close” (b) Forest Plot for RT for “Self – Stranger”

4.2 Split-half Reliability

We used four different approaches to split the data when calculating split-half reliability: the first-second, odd-even, permuted, and Monte Carlo methods. Also, we used the weighted average split-half reliability as the overall reliability across studies. Here we only presented the results from Monte Carlo split-half method both for clarity and for the robustness of this approach (Pronk et al., 2022) (see Fig.4(a)). The results of

the results revealed that

three split-half methods can be found in the supplementary materials (see Supplementary Fig. 3).

We found that, among all SPE measures, the four with highest split-half reliabilities are as follows: Reaction Time (RT) with “Stranger” as baseline ($r = .65, SE = .02, p < .001, 95\%CI [.61, .70]$); Efficiency (η) with “Stranger” as baseline ($r = .64, SE = .03, p < .001, 95\%CI [.59, .69]$); RT with “Close other” as baseline ($r = .58, SE = .02, p < .001, 95\%CI [.54, .63]$); η with “Close other” as baseline ($r = .57, SE = .02, p < .001, 95\%CI [.52, .62]$). These SPE measures achieved a split-half reliability around 0.6 or higher, which is considered **an** acceptable. For all other SPE measures, the reliability is around 0.5 or lower, indicating poor reliability. These include Accuracy (ACC), Sensitivity Score (d'), Drift Rate (v), and Starting Point (z) under four baselines. It is worth noting that split-half reliability of z , starting point parameter estimated from DDM, for all baselines are around 0, which suggests a lack of reliability.

4.3 Test-retest Reliability

ICC could only be calculated for dataset from our laboratory (Hu et al., 2023), which has 2 baselines, the “Close other” and “Stranger”, in the experimental design. The ICC2, which measure the reliability for individual differences, aligns with the findings observed in split-half reliability estimation (see Fig.4(b)). Specifically, when using “Close other” as baseline, the ICC2 for SPE measured by RT is 0.53 (95% CI = [.39, .69]), and for Efficiency, it is 0.52 (95% CI = [.38, .68]). Meanwhile, when “Stranger” was used as baseline, the ICC2 for RT is 0.58 (95% CI = [.45, .73]), and for Efficiency, it is 0.35 (95% CI = [.21, .52]). All other measures of SPE exhibited reliability lower than 0.5. To test the robustness of the results, we explored one additional datasets that included re-test session but deviated strongly from the original SPMT, the result showed similar pattern here (see Supplementary Fig. 4).

5 Discussion

In this pre-registered study, we investigated the reliability of different measures for self-prioritization effect (SPE) based on data from the self perceptual matching task (SPMT). Our analyses revealed that, except parameters z from DDM, all the other measures exhibited robust SPE. However, when it came to the reliability, only two measures of SPE, Reaction Time and Efficiency, exhibited acceptable to moderate reliability, among all indicators that has been reported in the literature. Our results suggested that the current implementation of SPMT was not well-suited for assessing individual differences. Taken together, our study revealed a “reliability paradox” of SPE as measured by SPMT. These findings provided important methodological insights to future studies of SPE. First, the reaction times and efficiency appeared to be the best measures among all different ways to measure SPE (the other were accuracy, d' , parameter v and z from DDM). Our results revealed that the reaction times and efficiency performed relatively well on both group-level and individual-level. On group level, effect sizes of SPE as measured by reaction times and efficiency were moderate to large effect; on individual-level, SPE as measured by reaction times and efficiency were higher for both split-half and test-retest reliability than other measures

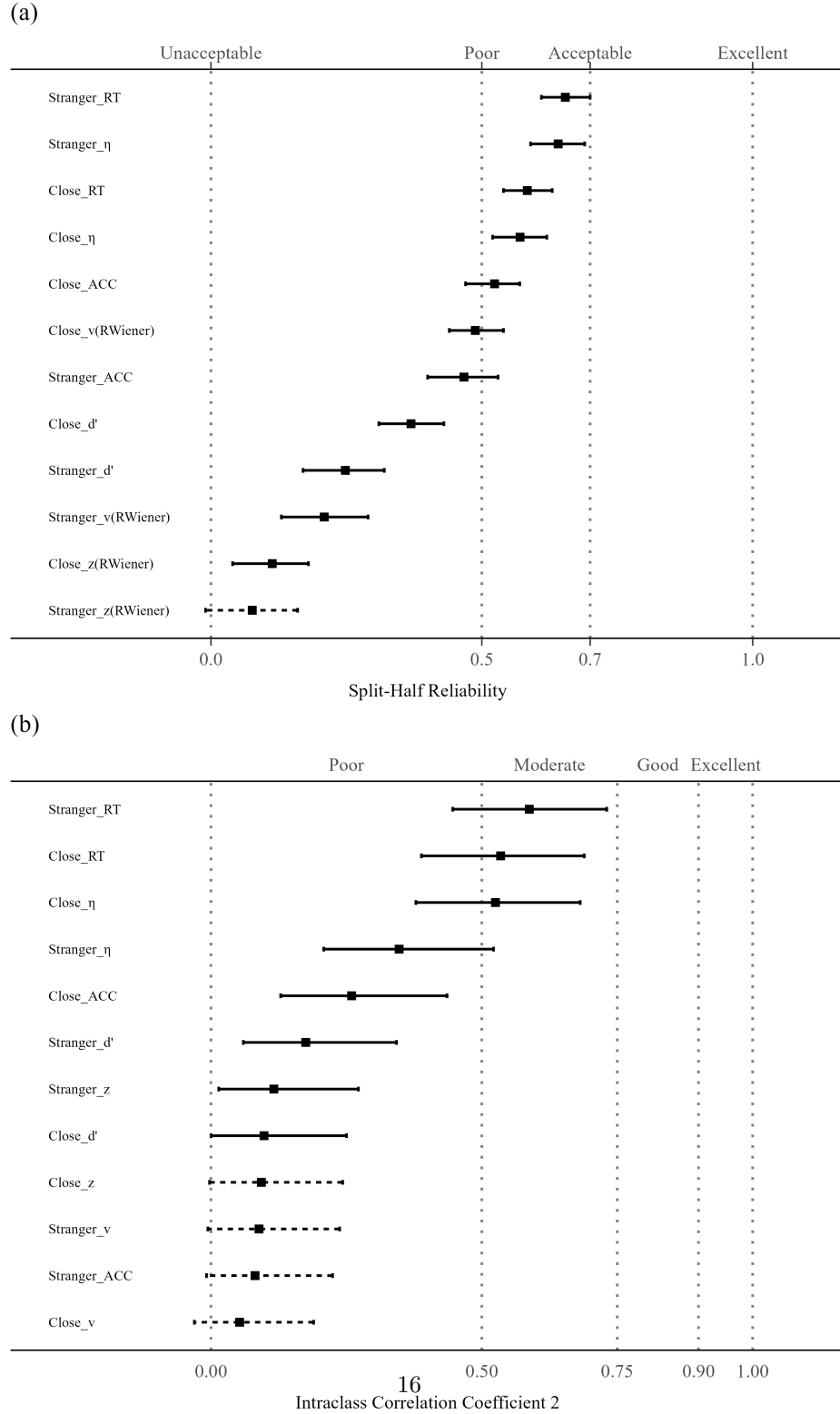


Fig. 4 a) The Weighted Average Split-Half Reliability (Monte-Carlo) and (b) Intraclass Correlation Coefficient for Different SPE Measures. *Note:* The vertical axis represents 12 different SPE measures, combining six indicators (RT, ACC, d' , η , v , z) and four baseline conditions (close other, stranger). The weighted average split-half reliability (figure a) and ICC values and their corresponding 95% confidence intervals are illustrated using points and lines. The dashed line indicates that the confidence interval for that point estimate extends across 0, implying a non-significant value. Due to the fact that there is only one paper for “Celebrity” and one for “Nonperson”, their results is presented in the supplementary materials.

of SPE. Moreover, for different baseline conditions used for calculating SPE in the literature, “Stranger” and “Close other” (e.g., friends, or mother) are the most commonly utilized. Notably, “Stranger” produced slightly higher effect size for almost all the six indicators and demonstrated greater reliability when it came to reaction times. Therefore, for researchers interested in balancing between the group-level SPE and reliability, using reaction times as the indicator and “Stranger” as baseline might be a good choice.

Second, taken the group-level robustness and individual-level results together, our findings aligned with the “reliability paradox” of cognitive tasks (Hedge et al., 2018; Logie et al., 1996). We observed that the majority of the SPE measures demonstrated moderate to large effect sizes when analyzed at the group level. However, when considering individual differences, only the SPE measures derived from reaction times (RT) and Efficiency (η) displayed comparatively higher values than other SPE measures but still did not meet the criteria for a satisfactory split-half reliability. Likewise, when examining reliability across multiple time points using ICC2, RT and Efficiency still ranked the highest but only showed moderate levels of test-retest reliability. The precise causes behind the reliability paradox observed in SPE measurements using the SPMT warrant thorough investigation. However, one of the most plausible explanations is that the SPMT, like other cognitive tasks, tends to exhibit minimal variability among participants while maximizing the detection of SPE at the group level (Liljequist et al., 2019). Consequently, this reliability paradox sheds light on the specific types of inquiries that the SPMT can proficiently address and those it cannot.

More specifically, the relatively low reliability of all the SPE measures calls for attention when researchers are interested in measuring individual differences, such as in clinical settings (e.g., Karvelis et al. (2023)), or searching an association with data from questionnaires (Hedge et al., 2018)). As the SPMT was designed to achieve robust group-level SPE rather than to measure individual differences, researchers need to re-design the task if they are interested in assessing individual difference. Recently, researchers researcher have proposed several ways to enhance the reliability of cognitive task, such as gamification (Friehs et al., 2020), using latent model (Enkavi et al., 2019) or generative models (Haines et al., 2020) to analyze the data. Some of these suggestions has already been validated by empirical data. For example, Kucina et al. (2023) re-designed cognitive conflict task by incorporating more trials and gamification indeed improved the reliability as compared to traditional Stroop task alone. Our exploratory analyses of the relationship between trial numbers and reliability also suggest that increasing trials numbers may improve reliability (please refer to the supplementary section 6).

Finally, another noteworthy result is the notably low split-half and test-retest reliability observed in the parameters (v and z) derived from the Drift Diffusion Model. In our analyses, we applied common and easy-to-use methods to datasets that included at least 60 trials, and estimated parameters for each condition of each participant and then calculated the reliability. The reliability of both the drift rate (v) and the starting point (z) fell well below acceptable levels. These findings raised concerns about applying the standard drift diffusion to data from SPMT directly. Previous studies found that standard drift diffusion model did not fit the data from matching task (Groulx

et al., 2020). Similarly, Schaaf et al. (2023) recently found poor reliability for the parameter estimates of the standard reinforcement learning model in cognitive tasks. These findings called for a more principled approach when modeling behavioral data to more accurately capture the fundamental cognitive processes at play (e.g., Wilson and Collins (2019)), instead of applying the standard DDM blindly.

5.1 Implications of the Current Study

indicators

Our findings can offer an initial guide for researchers considering the use of SPMT. Firstly, we recommend that researchers employ reaction times and efficiency as the **indicator** of SPE since they strikes a balance between achieving a substantial effect size at the group level and ensuring reliability at the individual level. Second, if researchers are interested in relatively bigger group-level effect size, considering the use of the “Self vs Stranger” contrast may prove beneficial. Third, if feasible, increasing the number of trials is advisable as it may enhance the overall reliability of the measurements. Lastly, we caution against the indiscriminate application of the standard drift-diffusion model and instead advocate for a principled modeling approach.

5.2 Limitations

Several limitations warrant acknowledgment. Firstly, although we made efforts to enhance sample diversity by including open data when available, it is important to note that the majority of our samples still consisted of individuals from what is commonly referred to as “(W)EIRD” populations (Rad et al., 2018; Yue et al., 2023), most of the participants are recruited from universities and are healthy adults. As a result, our findings may not be fully representative of the broader population, and it is necessary to include a more diverse sample to ensure greater generalizability of the paradigm. Secondly, our results reported here assessed the robustness and reliability of SPE with the original experimental design of Sui et al. (2012), which means the robustness and reliability of different variants of SPMT still need further investigation. For a more systematical meta-analysis of SPE measured by SPMT, please see our on-going project (<https://osf.io/euqmf>). Thirdly, when assessing the intraclass correlation coefficients (ICC2), only one dataset had longitudinal data available, which could potentially limit the representativeness of the results. This issue is mitigated by the fact that additional analysis of two datasets (see supplementary section 4) that with different design showed similar results as we reported in the main text.

6 Conclusion

This study provides an empirical assessment of the reliability of the self-perceptual matching task (SPMT). We found a robust self-prioritization effect for Reaction Times and Efficiency. Mean while, the reliability of the most robust SPE measure falls short of being satisfactory. The results of the current study may serve as a bench marker for the improvement of future studies.

Acknowledgments

The authors declare that this research received no external funding.

Author Contributions

HCP contributed to the conception and supervision of the study. HCP contributed to data collection. ZL, ZYR and HMZ write the simulation code for pre-registration. HMZ collected the datasets and performed the data pre-processing, analysis and visualize the results. In addition, ZL, HMZ and HCP contribute to discussing the results and the drafting of the final manuscript. HCP, JS, ZL and HMZ critically revise the manuscript.

Data and Material Availability

The pre-registration plan is available at OSF(<https://osf.io/zv628>). The de-identified raw data from our lab is available at Science Data Bank (<https://doi.org/10.57760/sciencedb.08117>). The simulated data is accessible on GitHub (<https://github.com/Chuan-Peng-Lab/ReliabilitySPE>).

Code Availability

Code used to simulate and analyze the data is made accessible on GitHub (<https://github.com/Chuan-Peng-Lab/ReliabilitySPE>).

Competing Interests

The authors declare no competing interests.

References

- Borenstein, M., Hedges, L. V., Higgins, J. P., & Rothstein, H. R. (2021). *Introduction to meta-analysis*. John Wiley & Sons.
- Borsboom, D. (2005). *Measuring the mind: Conceptual issues in contemporary psychometrics*. Cambridge University Press.
- Bukowski, H., Todorova, B., Boch, M., Silani, G., & Lamm, C. (2021). Socio-cognitive training impacts emotional and perceptual self-salience but not self-other distinction. *Acta Psychologica*, 216, 103297. <https://doi.org/10.1016/j.actpsy.2021.103297>
- Chen, G., Taylor, P. A., Haller, S. P., Kircanski, K., Stoddard, J., Pine, D. S., Leibenluft, E., Brotman, M. A., & Cox, R. W. (2018). Intraclass correlation: Improved modeling approaches and applications for neuroimaging. *Human brain mapping*, 39(3), 1187–1206. <https://doi.org/10.1002/hbm.23909>
- Cheng, M., & Tseng, C.-h. (2019). Saliency at first sight: Instant identity referential advantage toward a newly met partner. *Cognitive Research: Principles and Implications*, 4(1), 1–18. <https://doi.org/10.1186/s41235-019-0186-z>

- Cherry, E. C. (1953). Some experiments on the recognition of speech, with one and with two ears. *The Journal of the acoustical society of America*, 25(5), 975–979. <https://doi.org/10.1121/1.1907229>
- Cicchetti, D. V., & Sparrow, S. A. (1981). Developing criteria for establishing interrater reliability of specific items: Applications to assessment of adaptive behavior. *Am J Ment Defic*, 86(2), 127–137. <https://psycnet.apa.org/record/1982-00095-001>
- Clark, K., Birch-Hurst, K., Pennington, C. R., Petrie, A. C., Lee, J. T., & Hedge, C. (2022). Test-retest reliability for common tasks in vision science. *Journal of vision*, 22(8), 18–18. <https://doi.org/10.1167/jov.22.8.18>
- Constable, M. D., Elekes, F., Sebanz, N., & Knoblich, G. (2019). Relevant for us? we-prioritization in cognitive processing. *Journal of Experimental Psychology: Human Perception and Performance*, 45(12). <https://doi.org/10.1037/xhp0000691>
- Constable, M. D., & Knoblich, G. (2020). Sticking together? re-binding previous other-associated stimuli interferes with self-verification but not partner-verification. *Acta Psychologica*, 210, 103167. <https://doi.org/10.1016/j.actpsy.2020.103167>
- Constable, M. D., Rajsic, J., Welsh, T. N., & Pratt, J. (2019). It is not in the details: Self-related shapes are rapidly classified but their features are not better remembered. *Memory & Cognition*, 47, 1145–1157. <https://doi.org/10.3758/s13421-019-00924-6>
- Constable, M. D., Becker, M. L., Oh, Y.-I., & Knoblich, G. (2021). Affective compatibility with the self modulates the self-prioritisation effect. *Cognition and Emotion*, 35(2), 291–304. <https://doi.org/10.1080/02699931.2020.1839383>
- Cunningham, S. J., Turk, D. J., Macdonald, L. M., & Macrae, C. N. (2008). Yours or mine? ownership and memory. *Consciousness and cognition*, 17(1), 312–318. <https://doi.org/10.1016/j.concog.2007.04.003>
- Dalgaard, P. (2010). R development core team (2010): R: A language and environment for statistical computing.
- Enkavi, A. Z., Eisenberg, I. W., Bissett, P. G., Mazza, G. L., MacKinnon, D. P., Marsch, L. A., & Poldrack, R. A. (2019). Large-scale analysis of test–retest reliabilities of self-regulation measures. *Proceedings of the National Academy of Sciences*, 116(12), 5472–5477. <https://doi.org/10.1073/pnas.1818430116>
- Fisher, R. A. (1992). Statistical methods for research workers. *Springer New York*. https://doi.org/10.1007/978-1-4612-4380-9_6
- Friebs, M. A., Dechant, M., Vedress, S., Frings, C., & Mandryk, R. L. (2020). Effective gamification of the stop-signal task: Two controlled laboratory experiments. *JMIR Serious Games*, 8(3), e17810. <https://doi.org/10.2196/17810>
- Fritz, C. O., Morris, P. E., & Richler, J. J. (2012). Effect size estimates: Current use, calculations, and interpretation. *Journal of experimental psychology: General*, 141(1), 2. <https://doi.org/10.1037/a0024338>
- Golubickis, M., Falbén, J. K., Ho, N. S., Sui, J., Cunningham, W. A., & Macrae, C. N. (2020). Parts of me: Identity-relevance moderates self-prioritization. *Consciousness and cognition*, 77, 102848. <https://doi.org/10.1016/j.concog.2019.102848>

- Golubickis, M., Falbén, J. K., Sahraie, A., Visokomogilski, A., Cunningham, W. A., Sui, J., & Macrae, C. N. (2017). Self-prioritization and perceptual matching: The effects of temporal construal. *Memory & Cognition*, 45, 1223–1239. <https://doi.org/10.3758/s13421-017-0722-3>
- Golubickis, M., & Macrae, C. N. (2021). Judging me and you: Task design modulates self-prioritization. *Acta Psychologica*, 218, 103350. <https://doi.org/10.1016/j.actpsy.2021.103350>
- Green, S. B., Yang, Y., Alt, M., Brinkley, S., Gray, S., Hogan, T., & Cowan, N. (2016). Use of internal consistency coefficients for estimating reliability of experimental task scores. *Psychonomic Bulletin & Review*, 23, 750–763. <https://doi.org/10.3758/s13423-015-0968-3>
- Groulx, J. T., Harding, B., & Cousineau, D. (2020). The ez diffusion model: An overview with derivation, software, and an application to the same-different task. *The Quantitative Methods for Psychology*, 16(2), 154–174.
- Haines, N., Kvam, P. D., Irving, L. H., Smith, C., Beauchaine, T. P., Pitt, M. A., Ahn, W.-Y., & Turner, B. M. (2020). Theoretically informed generative models can advance the psychological and brain sciences: Lessons from the reliability paradox. <https://doi.org/10.31234/osf.io/xr7y3>
- Hedge, C., Powell, G., & Sumner, P. (2018). The reliability paradox: Why robust cognitive tasks do not produce reliable individual differences. *Behavior research methods*, 50, 1166–1186. <https://doi.org/10.3758/s13428-017-0935-1>
- Hedges, L., & Olkin, I. (1985). *Statistical methods for meta-analysis*, academic press.
- Hobbs, C., Sui, J., Kessler, D., Munafò, M. R., & Button, K. S. (2023). Self-processing in relation to emotion and reward processing in depression. *Psychological medicine*, 53(5), 1924–1936. <https://doi.org/10.1017/S0033291721003597>
- Hu, C.-P., Peng, K., & Sui, J. (2023). Data for training effect of self prioritization[ds/ol]. v1. *Science Data Bank*. <https://doi.org/10.57760/sciencedb.08117>.
- Hu, C.-P., Lan, Y., Macrae, C. N., & Sui, J. (2020). Good me bad me: Prioritization of the good-self during perceptual decision-making. *Collabra. Psychology*, 6(1), 20. <https://doi.org/10.1525/collabra.301>
- Hughes, S. M., & Harrison, M. A. (2013). I like my voice better: Self-enhancement bias in perceptions of voice attractiveness. *Perception*, 42(9), 941–949. <https://doi.org/10.1068/p7526>
- Humphreys, G. W., & Sui, J. (2015). The salient self: Social saliency effects based on self-bias. *Journal of cognitive psychology*, 27(2), 129–140. <https://doi.org/10.1080/20445911.2014.996156>
- Kahveci, S., Bathke, A., & Blechert, J. (2022). Reliability of reaction time tasks: How should it be computed? <https://doi.org/10.31234/osf.io/ta59r>
- Karvelis, P., Paulus, M. P., & Diaconescu, A. O. (2023). Individual differences in computational psychiatry: A review of current challenges. *Neuroscience & Biobehavioral Reviews*, 105137. <https://doi.org/10.1016/j.neubiorev.2023.105137>

- Keenan, J. P., Wheeler, M. A., Gallup, G. G., & Pascual-Leone, A. (2000). Self-recognition and the right prefrontal cortex. *Trends in cognitive sciences*, 4(9), 338–344. [https://doi.org/10.1016/S1364-6613\(00\)01521-7](https://doi.org/10.1016/S1364-6613(00)01521-7)
- Kircher, T. T., Senior, C., Phillips, M. L., Benson, P. J., Bullmore, E. T., Brammer, M., Simmons, A., Williams, S. C., Bartels, M., & David, A. S. (2000). Towards a functional neuroanatomy of self processing: Effects of faces and words. *Cognitive Brain Research*, 10(1-2), 133–144. [https://doi.org/10.1016/S0926-6410\(00\)00036-7](https://doi.org/10.1016/S0926-6410(00)00036-7)
- Kline, P. (2015). *A handbook of test construction (psychology revivals): Introduction to psychometric design*. Routledge.
- Kolvoort, I. R., Wainio-Theberge, S., Wolff, A., & Northoff, G. (2020). Temporal integration as “common currency” of brain and self-scale-free activity in resting-state eeg correlates with temporal delay effects on self-relatedness. *Human brain mapping*, 41(15), 4355–4374. <https://doi.org/10.1002/hbm.25129>
- Koo, T. K., & Li, M. Y. (2016). A guideline of selecting and reporting intraclass correlation coefficients for reliability research. *Journal of chiropractic medicine*, 15(2), 155–163. <https://doi.org/10.1016/j.jcm.2016.02.012>
- Kucina, T., Wells, L., Lewis, I., de Salas, K., Kohl, A., Palmer, M. A., Sauer, J. D., Matzke, D., Aidman, E., & Heathcote, A. (2023). Calibration of cognitive tests to address the reliability paradox for decision-conflict tasks. *Nature communications*, 14(1), 2234. <https://doi.org/10.1038/s41467-023-37777-2>
- Kupper, L. L., & Hafner, K. b. (1989). On assessing interrater agreement for multiple attribute responses. *Biometrics*, 45(3), 957–967. <https://doi.org/10.2307/2531695>
- Liljequist, D., Elfving, B., & Skavberg Roaldsen, K. (2019). Intraclass correlation—a discussion and demonstration of basic features. *PloS one*, 14(7), e0219854. <https://doi.org/10.1371/journal.pone.0219854>
- Lin, H., Saunders, B., Friese, M., Evans, N. J., & Inzlicht, M. (2020). Strong effort manipulations reduce response caution: A preregistered reinvention of the ego-depletion paradigm. *Psychological science*, 31(5), 531–547. <https://doi.org/10.1177/0956797620904990>
- Liu, Sui, J., & Hildebrandt, A. (2023). To see or not to see: The parallel processing of self-relevance and facial expressions. *Manuscript submitted for publication*.
- Liu, Y. S., Song, Y., Lee, N. A., Bennett, D. M., Button, K. S., Greenshaw, A., Cao, B., & Sui, J. (2022). Depression screening using a non-verbal self-association task: A machine-learning based pilot study. *Journal of Affective Disorders*, 310, 87–95. <https://doi.org/10.1016/j.jad.2022.04.122>
- Logie, R. H., Sala, S. D., Laiacona, M., Chalmers, P., & Wynn, V. (1996). Group aggregates and individual reliability: The case of verbal short-term memory. *Memory & Cognition*, 24, 305–321. <https://doi.org/10.3758/BF03213295>
- Makel, M. C., Plucker, J. A., & Hegarty, B. (2012). Replications in psychology research: How often do they really occur? *Perspectives on Psychological Science*, 7(6), 537–542. <https://doi.org/10.1177/1745691612460688>

- Martínez-Pérez, V., Campoy, G., Palmero, L. B., & Fuentes, L. J. (2020). Examining the dorsolateral and ventromedial prefrontal cortex involvement in the self-attention network: A randomized, sham-controlled, parallel group, double-blind, and multichannel hd-tdcs study. *Frontiers in Neuroscience*, 14, 683. <https://doi.org/10.3389/fnins.2020.00683>
- Navon, M., & Makovski, T. (2021). Are self-related items unique? the self-prioritization effect revisited. <https://doi.org/10.31234/osf.io/9dzm4>
- Orellana-Corrales, G., Matschke, C., & Wesslein, A.-K. (2020). Does self-associating a geometric shape immediately cause attentional prioritization? comparing familiar versus recently self-associated stimuli in the dot-probe task. *Experimental Psychology*, 67(6), 335. <https://doi.org/10.1027/1618-3169/a000502>
- Page, M. J., McKenzie, J. E., Bossuyt, P. M., Boutron, I., Hoffmann, T. C., Mulrow, C. D., Shamseer, L., Tetzlaff, J. M., Akl, E. A., Brennan, S. E., et al. (2021). The prisma 2020 statement: An updated guideline for reporting systematic reviews. *International journal of surgery*, 88, 105906. <https://doi.org/10.1136/bmj.n71>
- Parsons, S., Kruijt, A.-W., & Fox, E. (2019). Psychological science needs a standard practice of reporting the reliability of cognitive-behavioral measurements. *Advances in methods and practices in psychological science*, 2(4), 378–395. <https://doi.org/10.1177/2515245919879695>
- Payne, B., Lavan, N., Knight, S., & McGettigan, C. (2021). Perceptual prioritization of self-associated voices. *British Journal of Psychology*, 112(3), 585–610. <https://doi.org/10.1111/bjop.12479>
- Pronk, T., Molenaar, D., Wiers, R. W., & Murre, J. (2022). Methods to split cognitive task data for estimating split-half reliability: A comprehensive review and systematic assessment. *Psychonomic Bulletin & Review*, 29(1), 44–54. <https://doi.org/10.3758/s13423-021-01948-3>
- Qian, H., Wang, Z., Li, C., & Gao, X. (2020). Prioritised self-referential processing is modulated by emotional arousal. *Quarterly Journal of Experimental Psychology*, 73(5), 688–697. <https://doi.org/10.1177/1747021819892158>
- Rad, M. S., Martingano, A. J., & Ginges, J. (2018). Toward a psychology of homo sapiens: Making psychological science more representative of the human population. *Proceedings of the National Academy of Sciences*, 115(45), 11401–11405. <https://doi.org/10.1073/pnas.1721165115>
- Revelle, W. R. (2017). Psych: Procedures for personality and psychological research. <https://CRAN.R-project.org/package=psych>
- Rogers, T. B., Kuiper, N. A., & Kirker, W. S. (1977). Self-reference and the encoding of personal information. *J Pers Soc Psychol*, 35(9), 677–88. <https://doi.org/10.1037//0022-3514.35.9.677>
- Rouder, J. N., & Haaf, J. M. (2019). A psychometrics of individual differences in experimental tasks. *Psychonomic bulletin & review*, 26(2), 452–467. <https://doi.org/10.3758/s13423-018-1558-y>

- Schaaf, J. V., Weidinger, L., Molleman, L., & van den Bos, W. (2023). Test-retest reliability of reinforcement learning parameters. *Behavior research methods, Advance online publication*. <https://doi.org/10.3758/s13428-023-02203-4>
- Schäfer, S., & Frings, C. (2019). Understanding self-prioritisation: The prioritisation of self-relevant stimuli and its relation to the individual self-esteem. *Journal of Cognitive Psychology*, 31(8), 813–824. <https://doi.org/10.1080/20445911.2019.1686393>
- Stoeber, J., & Eysenck, M. W. (2008). Perfectionism and efficiency: Accuracy, response bias, and invested time in proof-reading performance. *Journal of research in personality*, 42(6), 1673–1678. <https://doi.org/10.1016/j.jrp.2008.08.001>
- Sui, J., He, X., & Humphreys, G. W. (2012). Perceptual effects of social salience: Evidence from self-prioritization effects on perceptual matching. *Journal of experimental psychology. Human perception and performance*, 38(5), 1105–1117. <https://doi.org/10.1037/a0029792>
- Sui, J., & Humphreys, G. W. (2017). The self survives extinction: Self-association biases attention in patients with visual extinction. *Cortex*, 95, 248–256. <https://doi.org/10.1016/j.cortex.2017.08.006>
- Svensson, S. L., Golubickis, M., Maclean, H., Falbén, J. K., Persson, L. M., Tsamadi, D., Caughey, S., Sahraie, A., & Macrae, C. N. (2022). More or less of me and you: Self-relevance augments the effects of item probability on stimulus prioritization. *Psychological Research*, 86(4), 1145–1164. <https://doi.org/10.1007/s00426-021-01562-x>
- Turk, D. J., Heatherton, T. F., Kelley, W. M., Funnell, M. G., Gazzaniga, M. S., & Macrae, C. N. (2002). Mike or me? self-recognition in a split-brain patient. *Nature neuroscience*, 5(9), 841–842. <https://doi.org/10.1038/nn907>
- Viechtbauer, W. (2010). Conducting meta-analyses in r with the metafor package. *Journal of statistical software*, 36, 1–48. <https://doi.org/10.18637/jss.v036.i03>
- Wabersich, D., & Vandekerckhove, J. (2014). The rwienr package: An r package providing distribution functions for the wiener diffusion model. *R Journal*, 6(1).
- Wagenmakers, E.-J., Van Der Maas, H. L., & Grasman, R. P. (2007). An ez-diffusion model for response time and accuracy. *Psychonomic bulletin & review*, 14(1), 3–22. <https://doi.org/10.3758/BF03194023>
- Wilson, R. C., & Collins, A. G. (2019). Ten simple rules for the computational modeling of behavioral data. *Elife*, 8, e49547. <https://doi.org/10.7554/eLife.49547>
- Woźniak, M., Kourtis, D., & Knoblich, G. (2018). Prioritization of arbitrary faces associated to self: An eeg study. *PloS one*, 13(1), e0190679. <https://doi.org/10.1371/journal.pone.0190679>
- Xu, T., Kiar, G., Cho, J. W., Bridgeford, E. W., Nikolaidis, A., Vogelstein, J. T., & Milham, M. P. (2023). Rex: An integrative tool for quantifying and optimizing measurement reliability for the study of individual differences. *Nature Methods*, 1–4. <https://doi.org/10.1038/s41592-023-01901-3>
- Xu, Y., Yuan, Y., Xie, X., Tan, H., & Guan, L. (2021). Romantic feedbacks influence self-relevant processing: The moderating effects of sex difference and facial

- attractiveness. *Current Psychology*, 1–13. <https://doi.org/10.1007/s12144-021-02114-7>
- Yue, L., Zuo, X.-N., & Chuan-Peng, H. (2023). The weird problem in a “non-weird” context: A meta-research on the representativeness of human subjects in chinese psychological research. <https://doi.org/osf.io/y9hwq>
- Zhang, Y., Wang, F., & Sui, J. (2023). Decoding individual differences in self-prioritization from the resting-state functional connectome. *NeuroImage*, 120205. <https://doi.org/10.1016/j.neuroimage.2023.120205>
- Zorowitz, S., & Niv, Y. (2023). Improving the reliability of cognitive task measures: A narrative review. *Biological Psychiatry: Cognitive Neuroscience and Neuroimaging*. <https://doi.org/10.1016/j.bpsc.2023.02.004>