Relieving the Burden: Identifying Diminishing Returns to Create More Efficient Tasks

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Abstract

Background: If well-constructed, efficient measures of cognition have the potential to increase validity, while decreasing research burden and costs for participants and assessors with little loss of reliability. There is the possibility that long and difficult measures create even noisier data for people with serious mental illness compared to the general population. In this study, we aim to assess the extent to which working memory and reinforcement learning tasks can be made more efficient.

Methods: Participants included 185 people with serious mental illness and 75 controls. Internal consistency (Cronbach's α, Item-Total Correlations, and/or Spearman-Brown Prophecy Correlations) and test-retest reliability (Intraclass Correlations) were calculated for increasing subsets of trials on each task to assess the point at which reliability reached acceptable and good levels or reached diminishing returns.

Results: Generally, the tasks had met acceptable internal consistency values by the time only half of the values had been included and either good reliability or diminishing returns by the time two-thirds of the task trials were considered. This was largely similar for test-retest on the working memory tasks, but acceptable test-retest reliability was mostly never achieved on the reinforcement learning tasks.

Conclusions: Overall, each of the working memory and reinforcement learning tasks can be made 25-50% more efficient without significant loss of psychometric integrity. However, there may be limitations on the utility of some of the tasks due to acceptable test-retest reliability never being achieved.

Introduction

Though there is an inherent tradeoff between test reliability and efficiency, when done carefully efficient measures can be implemented with little to no loss of reliability, increased validity, and decreased research burden. Despite the many benefits of participating in research (positive social interactions, feelings of contributing, receiving resources and feedback, compensation, etc.), protocols can potentially be tiresome, emotional, frustrating, stressful, or difficult. This may be exacerbated for people with serious mental illness due to symptoms, cognitive and functional impairments, and decreased stress tolerance (Miret et al., 2016). Researchers have put substantial efforts into protecting rights and improving experiences for paticipants, but lengthy days full of clinical assessments, brain scans, cognitive testing, and other time-consuming measures are still common and, at times, necessary. These fatiguing or stressful days can be understood at construct-irrelevant variance, which is a threat to validity, and should be avoided according to the educational and psychological testing standards (American Educational Research Association, 2018). Efficient cognitive assessments are one way in which research burden can be reduced. Therefore, by leading to the collection of larger samples of more valid data, efficient cognitive testing can improve the conclusions and recommendations that can be extrapolated, all while improving the research experience for everyone.

The first consensus conference of the Measurement and Treatment Research to Improve Cognition in Schizophrenia (MATRICS) initiative defined five criteria for the selection of cognitive tests in research: 1) test-retest reliability, 2) high utility as a repeated measure, 3) relationship to functional outcome, 4) potential changeability in response to pharmacological agents, and 5) tolerability and practicality (Green et al., 2004). Tolerability and practicality were included because although longer tests can lead to more reliable measurements, they also have a

higher likelihood of participant attrition and can reduce validity due to fatigue and loss of motivation (Green et al., 2004). Therefore, it is critical to prioritize the most efficient methods of assessing each cognitive domain.

Here, tolerability refers to the experience of a treatment, test, or protocol from a participant's point of view (Green et al., 2004). Historically, tolerability in psychosis research referred to the ability to withstand a medication or other treatment and the side effects of those treatments. More recently researchers have extended the definition of tolerability to reflect to the length, difficulty, repetitiveness, and other features of a measure that might make it less pleasant for the participant (Carter et al., 2012; Green et al., 2004). Tolerability is increasingly gaining traction as an important criterion for studies including people with psychosis (Green et al., 2004; Kern et al., 2013; Ludwig et al., 2017; Reddy et al., 2015). Despite this, examination of the tolerability of measures, as well as the best way to assess tolerability, is limited in the literature.

Tolerability has primarily been measured by collecting ratings on a 7-point Likert scale of "Extremely Unpleasant" to "Extremely Pleasant." Tasks tend to have mean ratings between 4 and 5.5 (and standard deviations between 1 and 1.5) indicating that most participants felt the tasks were neutral to somewhat pleasant (Kern et al., 2013; Ludwig et al., 2017; Nuechterlein et al., 2008; Reddy et al., 2015). Because of this, other MATRICS test selection criteria have instead been used to determine the most appropriate test per domain (Kern et al., 2013; Ludwig et al., 2017; Nuechterlein et al., 2008; Reddy et al., 2015). However, the lack of variance between ratings suggest this Likert scale may not be a sensitive or valid measure of tolerability. Other measures of tolerability have also been examined, including length of assessment, proportion of participants "showing good interest," proportion of individuals "refusing to be re-interviewed with the assessment," attrition/drop-out counts, as well as participant reports (e.g.,

negative experiences, excessive stress, ease of test) (Hesse et al., 2017; Trémeau et al., 2008). However, these efforts have not always led to attempts to make tasks more efficient and tolerable for participants.

With long measures and protocols, participants may also experience cognitive fatigue, which is the decrement in an individual's ability to complete cognitive tasks to the best of their ability after partaking in prolonged, challenging, or sustained cognitive work that use up mental resources (Ackerman & Kanfer, 2009; Holtzer et al., 2010; Mullette-Gillman et al., 2015). Cognitive fatigue is typically measured by increased error rates and slower reaction times over time on tasks. This phenomenon has been repeatedly observed in healthy individuals, such as during standardized testing in schoolchildren (Sievertsen et al., 2016), eight hours of laboratory pipetting (Yung et al., 2017), and performing the Stroop task for extended periods of time (Wang et al., 2014) (see Ackerman & Kanfer, 2009; Jensen et al., 2013 for examples of where cognitive fatigue was not found). Cognitive fatigue has also been observed while performing a second task that was given after an initial fatiguing task, showing that effects can be long-lasting and transferrable (Gergelyfi et al., 2015; Mullette-Gillman et al., 2015; van der Linden et al., 2003). Evidence is limited, but it has also been posited that people with psychosis experience cognitive fatigue more quickly than other populations, as observed on the Stroop (Everett et al., 1989). While there may be circumstances when inducing fatigue is of interest, this should generally be avoided to 1) reduce noise and obtain more valid measurement of the cognitive domain(s) of interest and 2) avoid unnecessary strain and stress on participants.

In addition to the effects of cognitive fatigue on cognitive task performance, it is clear that participants subjectively report feeling fatigued after performing lengthy, monotonous, or difficult tasks (Ackerman & Kanfer, 2009; Borragán et al., 2017; Wang et al., 2014). This is seen

when assessed alone or when ratings are compared between cognitive tasks and control tasks, such as watching a movie (Mullette-Gillman et al., 2015; Solianik et al., 2018; van der Linden et al., 2003; Wang et al., 2014). Therefore, these types of tasks may not fit the tolerability test selection criteria well. It is possible that participants may have difficulty differentiating fatigue from sleepiness, but it has been suggested that fatigue increases faster with high cognitive load, but sleepiness increases faster with low-cognitive load (Borragán et al., 2017), and therefore can lead to noisy data either way.

Creating more efficient measures of cognition also benefits researchers. Practicality, the other aspect of the fifth test selection criteria above, refers to the experience of the research study from the researcher's point-of-view. This may include administration time, financial costs, set-up time, staff training, and complicated administration or scoring (Green et al., 2004). Utilizing more efficient measures would decrease administration times and financial cost per participant, allowing for recruitment of larger sample sizes with the same budget. Larger samples further lead to more reliable and powerful data. Overall, utilizing optimally efficient measures: 1) increases participant tolerability (greater enjoyment and lower attrition and fatigue), 2) increases research practicality (lower time and financial costs per participant), and 3) increases validity (decreasing noise) and power (larger sample sizes).

There have been many efforts to create efficient neuropsychological batteries for assessing cognition in psychosis (Fervaha et al., 2014, 2015; Gold et al., 1999; Hill et al., 2008; Hurford et al., 2011; Kaneda & Keefe, 2015; R. S. Keefe et al., 2003; R. S. E. Keefe et al., 2004; Lam et al., 2017; Nuechterlein et al., 2008; Velligan et al., 2004). These brief measures are reliable and explain a large portion of the variance obtained through comprehensive neurocognitive batteries. While there may be some loss of information from brevity, the gains in

tolerability and practicality make these batteries increasingly popular. There have also been efforts to increase the efficiency of experimental measures of specific cognitive mechanisms. The Cognitive Neuroscience Test Reliability and Clinical Applications for Schizophrenia (CNTRaCS) consortium pioneered the task optimization and psychometric analysis needed to make tasks ready for use in clinical studies (Gold et al., 2012). CNTRaCS has previously optimized measures of goal maintenance (Henderson et al., 2012), visual integration (Silverstein et al., 2012), gain control (Barch et al., 2012), and relational encoding and retrieving (Ragland et al., 2012). More recently, CNTRaCS began optimizing reinforcement learning and working memory tasks for assessment of people with psychosis (Barch et al., 2017; Gold et al., 2019). Basic psychometric properties of these tasks are described elsewhere (Pratt et al., 2021, 2023). The aim of this study was to evaluate the extent to which these freely available working memory and reinforcement learning tasks could be made more efficient without sacrificing psychometric integrity.

Methods

Participants:

313 participants were recruited: 75 healthy controls (HC), 62 people with bipolar disorder with psychotic features (BP), 85 people with schizoaffective disorder (SczA; 25 unmedicated), and 91 people with schizophrenia (Scz; 23 unmedicated). Participants gave written, informed consent. Only medicated participants were included in this study, as unmedicated participants were not retested. Other reasons for exclusion (n=5) are described elsewhere, along with the demographics and inclusion/exclusion criteria [redacted for masked review]. The final sample for this study was 75 HC, 60 BP, 57 SczA, and 68 Scz. For the assessment of reliability in this

study, all individuals with serious mental illness were combined into one group (SMI = 185). This study was IRB approved.

Tasks and Procedures:

Participants completed three visits. The first visit consisted of an IQ screen, diagnostic interview, and clinical and functional scales. Clinical symptoms were rated using the Brief Psychiatric Rating Scale (BPRS; Ventura et al., 2000), the Young Mania Rating Scale (YMRS; Young et al., 1978), the Bipolar Depression Rating Scale (BDRS; Berk et al., 2007), and the Clinical Assessment Interview for Negative Symptoms (CAINS; Kring et al., 2013). All raters achieved agreement (ICC $\geq$ 0.80) with "gold standard" ratings to create good interrater reliability with a drift prevention interview rated across sites every 2-4 weeks throughout the study.

Following the baseline clinical visit, two sets of cognitive testing sessions—referred to here as Time 1 and Time 2—were completed approximately one month apart. The cognitive tasks in this paper are explained below. Other procedures can be found elsewhere (Pratt et al., 2021).

Change Detection (CD): This task was designed to assess both working memory storage capacity and attentional engagement so that differences in storage would not be confounded by differences in attentional lapses (Luck & Vogel, 2013). For each trial, participants saw a 5-item encoding array followed by a testing array in which 0, 1, 2, or 5 of the items changed, and participants were instructed to indicate whether there was a change or not (disreguarding the actual number of changes; Gold et al., 2019). The encoding array was shown for 500 ms and the items were evenly spaced colored shapes around an imaginary circle. This was followed by a 1000 ms delay and then the testing array, which had the same number of items/spacing as the

encoding array. 60 trials were presented for each number of changes, resulting in 240 trials. This task differs slightly from the standard change detection paradigm, in which the number of changes is either 0 or 1 (Luck & Vogel, 2013). This new version makes it possible to estimate accuracy and three computational modeling parameters: working memory capacity (*K*), the probability that the participant was paying attention on a given trial (*A*), and the probability that the participant indicated a change if there was not one, which approximates guessing (*G*). These parameters, along with overall accuracy (proportion correct), were the dependent measures (Feuerstahler et al., 2019).

Change Localization (CL): This task is simpler and faster than change detection, but it leads to a single measure of storage capacity that could potentially be influenced by lapses of attention (Zhao et al., 2022). For each trial, participants saw a 5-item encoding array followed by a testing array in which they had to locate the one item that had changed (Gold et al., 2019). The encoding array was shown for 500 ms and the items were evenly spaced colored shapes around an imaginary circle. This was followed by a 1000 ms delay and then the testing array, which had the same number of items/spacing as the encoding array. Participants were instructed to click on the item that had changed, emphasizing accuracy and not speed. There were 60 trials and working memory capacity (proportion correct x 5, the memory array size) was the dependent measure.

Implicit Probabilistic Incentive Learning Tasks (IPILT): The IPILT was designed as a signal-detection to provide an objective, laboratory-based measure of hedonic capacity (Pizzagalli et al., 2005, 2008). It was modified to include both a positive (IPILT-P) and negative (IPILT-N) reinforcement version (Barch et al., 2017). Participants made perceptual discriminations between two variants of a briefly shown (100 ms) line-drawn stimulus. On the

IPILT-P, ~40% of correct responses received the feedback "Correct! You Win!," and participants gained $0.05. On the IPILT-N, participants started with $3.60 and lost $0.05 on ~40% of incorrect trials and received the feedback "Sorry. You Lose." One of the two stimulus variants from the set was associated with three times as much feedback (RICH) as the other stimulus variant (LEAN) in each block. The IPILT-P and IPILT-N each had three blocks of 60 trials. Both versions were given at sessions 1 and 2. The dependent measures included response bias (log *b*) and discriminability (log *d* or *d'*; equations in supplement).

Explicit Probabilistic Incentive Learning Tasks (EPILT): The EPILT was designed to measure how dopamine affects instrumental learning (Pessiglione et al., 2006). It was adapted to assess explicit learning from gain and avoiding loss incentives (Barch et al., 2017). There were two phases: training and transfer. During training, participants were explicitly asked to simultaneously learn value discriminations for 4 pairs of images over 160 trials. Two of the pairs were Gain conditions, where the optimal choice was associated with a gain of $0.05 and the word "WIN!" The non-optimal choice resulted in no gain of money and received the feedback "Not a winner. Try again!" For one of the Gain pairs, the optimal response was reinforced with a 90/10 ratio of win/not win and the other had a reinforcement ratio of 80/20 win/not win. The other 2 pairs of stimuli were Avoid Loss conditions, with a not lose/lose response pattern. These used the same reward probabilities, where the optimal choice resulted in no loss of money and the feedback "Keep your money!" and the non-optimal choice resulted in a loss of $0.05 and the feedback "LOSE!" The dependent measure for this phase was percent accuracy.

For the transfer phase, the original 4 training pairs were each presented 4 times, but there were also 58 novel pairings, totaling 72 trials. Novel pairings included only trained images, which presented with all other combinations of trained images. Participants were given the

instruction to choose the image in each pair that they thought was "best," and were not given feedback. The accuracy selecting the more beneficial image in a pairing was the primary dependent measure. The pairings of interest were: 1) Frequent Winner versus Frequent Loser (FWvsFL), 2) Frequent Winner versus Infrequent Winner (FWvsIW), 3) Frequent Winner versus Frequent Loss Avoider (FWvsFLA), and 4) Frequent Loss Avoider versus Infrequent Winner (FLAvsIW). See Barch et al., 2017 for further interpretation of these pairings.

Data Analysis:

To examine the minimum number of trials needed per task while maintaining reliability, a series of reliability analyses were conducted starting with a small subset of trials (*n*, which depended on task) and adding another same-sized subset of the linearly subsequent trials until all of them have been added, split by group. To assess internal consistency, Cronbach's α and the mean item-total correlation (ITC) were calculated. To examine test-retest reliability, Intraclass Correlations (ICCs) with a two-way random effects model assessing degree of agreement between testing sessions was utilized (McGraw & Wong formula ICC(A,1); McGraw & Wong, 1996; Shrout & Fleiss, 1979). In order to interpret and make recommendations, we utilize established benchmarks from the literature. For Cronbach's α, it has been suggested that a value of 0.7 is acceptable, particularly for basic research (Nunnally & Bernstein, 1994), and that an α greater than 0.9 begins to reflect a high amount of redundancy in the test (McClelland, 1980; Streiner, 2003), pointing to an ideal α between 0.8 and 0.9 for clinical trials. For ITC's, we can interpret that values greater than 0.3 are rather good (Cristobal et al., 2007; Nunnally & Bernstein, 1994) for our purposes. Further, for test-retest reliability, ICC's should be at minimum 0.6, and ideally above 0.75 (Cicchetti, 1994).

For Change Detection, reliability was examined for the accuracy split by condition (No-change, One-change, Two-change, or Five-change). As the trials were randomized with a fixed interval of 2 trials per condition within every 10 trials, intervals of 2 trials at a time until all 60 trials were included, totaling 30 repetitions per statistical test. Change Localization followed the same procedure for its one condition. For the IPILT-P and IPILT-N accuracy, intervals of 3 trials at a time until all 180 were included, totaling 60 analytical repetitions per test; for response bias and discriminability, intervals of 15 trials were used in order to have enough data points to calculate them, totaling 12 repetitions. The EPILT-Train was split into intervals of 4 trials that captured each of the four reinforcement contingencies for 40 repetitions until all 160 trials were included, totaling 4 repetitions. Lastly, for the EPILT-Transfer, cycles were split into groups of 9 trials that included 1-3 trials per condition (based on the frequency in which the condition appears) until all 72 trials were included, equaling 8 repetitions.

Materials and analysis code for this study are available by emailing the corresponding author.

Results

Summarized results and recommendations for all tasks can be found in Table 1.

Change Detection: For the observable variables (Figure 1), this task reaches acceptable reliability after about half the trials, and good reliability about two-thirds of the way into the task. Aside from an overall reduction in the number of trials, this task may be made more efficient by cutting extra trials from the No-change condition, as this condition achieves internal consistency with fewer trials compared to other conditions. It is notable that the No-change condition for HCs and the Five-change condition for both groups never reach acceptable test-retest reliability, even when all trials are considered. This is likely due to floor (No-change due to guessing) and

ceiling effects (Five-change because it's an attention capture) and does not raise concerns for the utility of this task. For the computational parameters (Figure 2), acceptable and good internal consistency are also reached between half and two-thirds of the way into the task. However, the test-retest concerns extend beyond the guessing and attention parameters and minimum acceptable values are never achieved for the SMI group for the working memory parameter, which might limit the utility of this parameter.

Change Localization: Similarly, change localization reaches acceptable internal consistency after less than half the trials and optimal reliability two-thirds of the way in (Figure 3). Minimum acceptable test-retest reliability is achieved at approximately two-thirds of the way into the task, which should be considered when adjusting the task length.

IPILT: Internal consistency for both the IPILT-P and IPILT-N reaches an acceptable reliability with around a quarter of the trials included (Figures 4 and 5). Good reliability was more variable, but on average it was achieved with half of the trials of the original task. However, test-retest reliability never achieved acceptable values for either task, which may affect the utility of these tasks depending on the intended use.

EPILT: The training condition could be cut by a half to a third without sacrificing internal consistency (Figure 6). However, minimum acceptable test-retest reliability is achieved around the two-thirds mark. The transfer condition continues to benefit from the addition of more trials and thought should be given to which trials would be removed if any, which likely should be the new pairings. The transfer condition does not have acceptable test-retest reliability even with all trials, which should also be considered when deciding whether this task is appropriate for the intended use.

Discussion

In this study we set out to examine the extent to which two working memory and two reinforcement learning tasks could be made more efficient to reduce testing burden on participants. In general, we observed that these tasks can reach the same or similar level of reliability with fewer trials per task. When only considering internal reliability, we find that each of these tasks can be made anywhere from 25%-50% shorter and still have appropriate reliability values. However, some tasks never achieved minimum acceptable test-retest reliability, which may reduce the utility of these tasks, depending on the need of the study.

It is well established that for classical test theory measurements, reliability increases with the number of trials in a test (Cortina, 1993; Streiner, 2003). While the tasks examined in this study were largely no exception, continuously diminishing reliability gains per trial are consistently seen above and beyond the value that is considered appropriate, or even good, for use in studies and clinical trials of serious mental illness. Additionally, increased reliability is often achieved at the expense of validity, and it is possible that reducing reliability can lead to increases in validity (Silverstein, 2008). Therefore, it is possible that the improvement in reliability beyond the point where there are diminishing returns could be leading to more noise in the data due to factors such as cognitive fatigue.

Using the benchmarks described in the methods, in combination with examining the maximum reliability achieved per task, we can make early recommendations about how long the tasks examined here truly need to be for use in future studies. Change detection could be shortened by 33%, from 60 trials per condition to 40 trials per condition, without significant loss of reliability. However, the number of trials could also be examined separately for each condition. The No-change condition appears to need fewer trials (30-35) to reach diminishing

returns on its reliability; the benchmark for good reliability for $\widehat{G}$ is similar to the No-change condition. Whereas the integrity of the task may benefit if the One-change and Two-change conditions (and dependently $\widehat{K}$) keep near 50 trials. Lastly, the Five-change condition and $\widehat{A}$ both optimally need about two-thirds of the trials. It is also important to note that the reliability found for our efficient subset is similar to those in the literature that includes many more trials. For example, in one version of the Change Detection task with 540 trials, researchers found a Cronbach's α of > 0.9 and a between-session reliability measured by Pearson's correlations of 0.71 (Xu et al., 2018). Additionally, Change Localization could likely be shortened from 60 trials to 36 for more experimental studies and around 50 trials for clinical trials. Again, considering only this subset of trials, reliability reaches a similar value to what has previously been observed in the literature. Internal consistency, using split-half reliability, for Change Localization has previously been observed to be 0.93 (Zhao et al., 2022), and the test-retest reliability, using ICC has been reported as 0.76 (Johnson et al., 2013).

The IPILT-P reaches good internal consistency by 90 trials, which is substantially shorter than the originally 180 trials. Though internal consistency does increase somewhat with more information, it does not particularly change the interpretation of the reliability, and the full number of trials may be redundant and may harm validity. This is echoed in the IPILT-N, where the good internal consistency benchmark is achieved by 75 out of 180 trials. For the EPILT-Training phase, reducing the length of the task from 160 trials to 132 trials would suffice. The EPILT-Transfer could be halved for use in basic experimental studies but would benefit from keeping all 72 trials. However, neither of these reinforcement learning tasks reach minimum acceptable test-retest reliability. It is notable, however, that test-retest reliability for earlier iterations of these tasks are reported to have similar test-retest reliability. For the IPILT, the first

and second testing had a Pearson correlation of 0.57 (Pizzagalli et al., 2005), which is similar to our findings in this study. For the EPILT, one study found the ICC between testing sessions to be 0.43 (Weidinger et al., 2019), which also has a comparable interpretation to our study. This is indication that the low test-retest reliability of these measures are not unique to our study. With this low test-retest reliability, it is possible that irrelevant artefacts are being measured in this task. However, as these are reward-based tasks, it is also possible that they are particularly state dependent (mood, affect, hunger, etc.), which is leading to the low test-retest reliability. Therefore, it may be cause for concern that there is a discrepancy of internal consistency and test-retest reliability that is dependent on the researcher's intended use for the tasks, which should be carefully considered.

While this is just one sample of data, it appears that these tasks can be made significantly shorter, reducing the burden on both participants and researchers, and allowing for the collection of larger samples of more valid data. As mentioned previously, increasing the number of trials typically increases reliability from a classical test theory perspective, in particular Cronbach's α and ICCs. Despite this, we are able to strike the balance between psychometric strength and efficiency, without detracting from test integrity much. Of course, there will be variation in the exact number of trials needed to reach the benchmark values in individual studies and the reliability boundaries are ultimately arbitrary (though thoroughly established and accepted). All else being equal, higher levels of reliability would be expected in populations with more true score variance and lower levels of reliability would be expected in populations with less true score variance (Chapman & Chapman, 1978). Therefore, researchers may want to consider true score variance, target sample size, and other study design details, as well as lean on the more conservative estimates from this study until the lower boundaries can be replicated. This study

did find comparable reliability for these tasks to what has been previously observed in the literature, and therefore our efficiency findings are likely to largely generalize to other studies.

One limitation of this study is that we did not directly compare the reliability of different length versions of these tasks; while subsetting the trials in the tasks in the order that they appear may be the best way to estimate how a shorter task may look, participant expectations about the length of the task might change behavior. Therefore, it is possible that different prior expectations of the task length means that the reliabilities calculated here may not be a completely accurate representation of the shorter version of the task. Despite this limitation, information from this paper can be used to adapt these working memory and reinforcement learning tasks to be more efficient. Future research will involve piloting updated versions of these tasks and see if the reliabilities reported here can be replicated independent of the context of the longer task. Further, future studies should use item response theory methods to identify other ways to make these tasks more efficient.

The findings outlined here provide evidence that researchers can utilize shorter and more efficient versions of cognitive tasks to increase the tolerability and practicality of working memory and reinforcement learning tasks without sacrificing the psychometric integrity of them. Implementing the information found here can not only make individuals' experience of participating in research more positive, but will also reduce the strain on researchers by leading to shorter visits freeing up more time and money to gather data from larger sample sizes, and potentially produce more valid measurements of participants abilities that are affected by fatigue and other extraneous effects. Therefore, when done responsibly and carefully, designing studies with this in mind have the potential to create more powerful data and replicable results, while reducing research burden.

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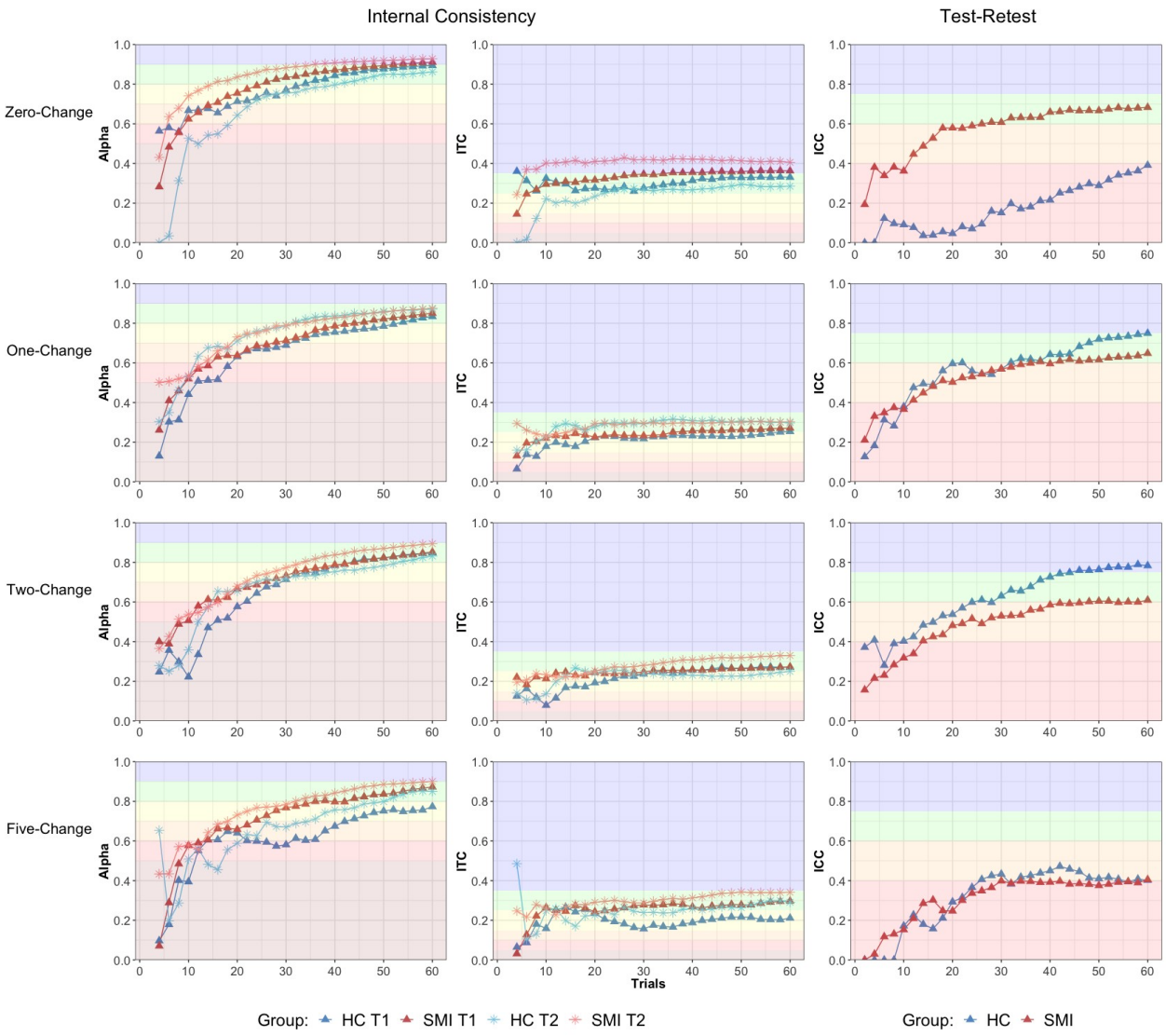
Tables and Figures

Table 1: Summarized Task Reliability

Task	Condition	Total # of Trials	Trials per set	Trials to Acceptable Reliability HC	Trials to Acceptable Reliability SMI	Trials to Max or Ideal Reliability HC	Trials to Max or Ideal Reliability SMI
Change Detection	No-change	60	2	20#^	16+	34#^	26
	One-change	60	2	32	28+	54	44
	Two-change	60	2	30	26+	44#	44+
	Five-change	60	2	42^	24^	50*^	44^
	K	240	8	136	144^	186	216#^
	A	240	8	64#^	48#^	80#^	96#^
	G	240	8	40#^	64+	128#^	96+
Change Localization		60	2	24#	22#+	44	48#+
IPILT-P	Accuracy	180	3	45^	51^	75^	87^
	log b	180	15	75^	15^	105#^	105#^
	d'	180	15	45+	60^	90+	120^
IPILT-N	Accuracy	180	3	36^	51^	57^	75^
	log b	180	15	45^	15^	60*^	85^
	d'	180	15	45^	60^	60^	85^
EPILT	Train	160	4	56#+	72+	96#+	100+
	Transfer	72	9	45^	45^	63^	72^

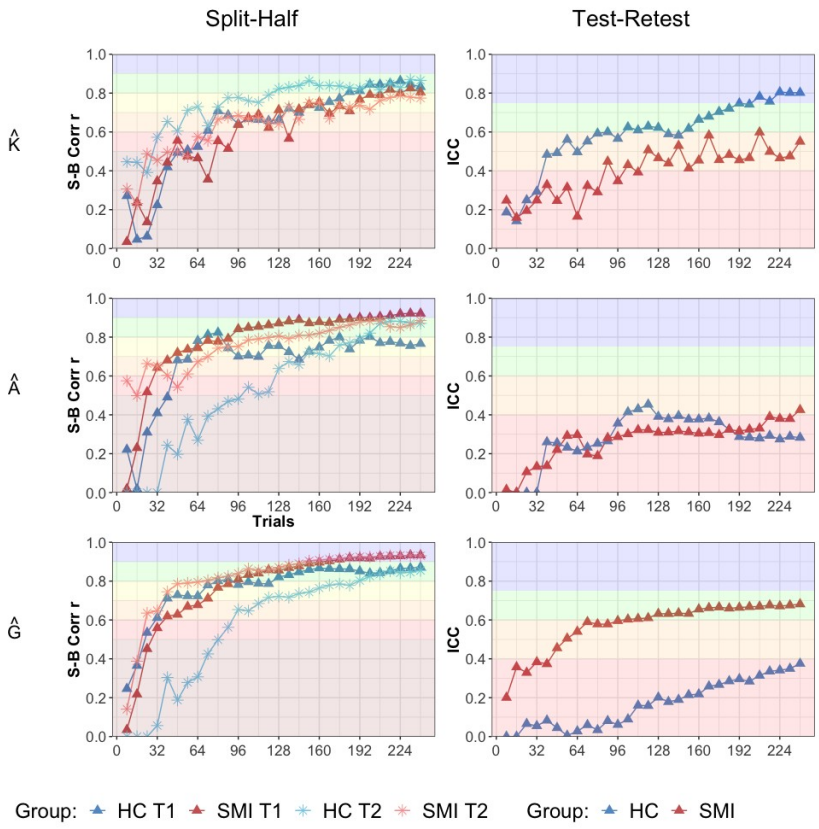
Note: Acceptable reliability refers to a Cronbach's α or Spearman-Brown Prophecy correlation of 0.7 and optimal reliability is 0.8. Max reliability is the reliability plateau if it does not reach 0.8. *Never reaches internal consistency of 0.8; #Time 2 values are met >=2 sets later than Time 1 values; +Acceptable test-retest reliability not met yet; ^Acceptable test-retest reliability never achieved

Figure 1: Reliability of Change Detection Accuracy Per Number of Trials



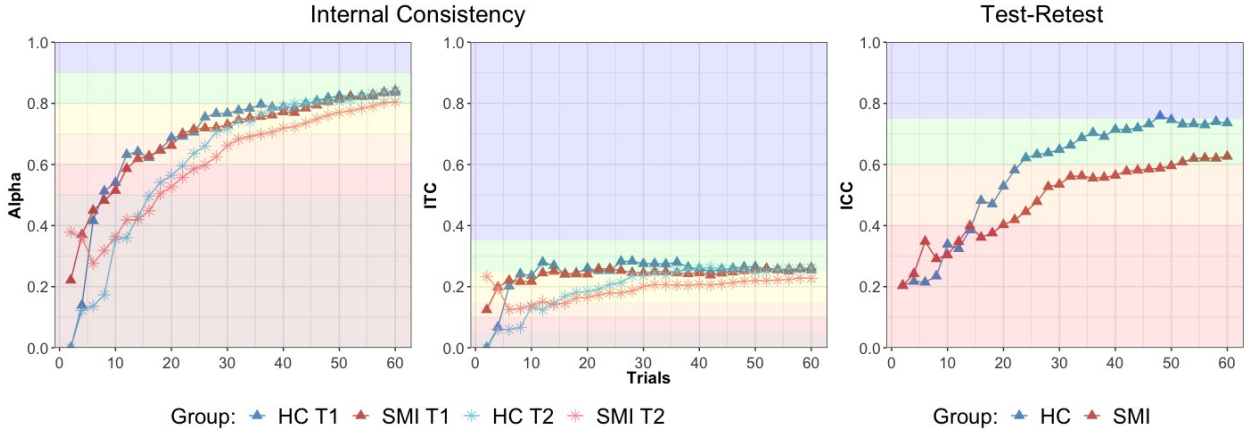
Columns represent Cronbach's α, mean item-total correlations (ITC), and intraclass correlations (ICC(A,1)), respectively. Rows correspond to Change Detection conditions. Within each graph, individual points illustrate the reliability at the respective number of trials. Colors provide interpretation for the fit of the statistic.

Figure 2: Reliability of Change Detection Computational Parameters Per Number of Trials



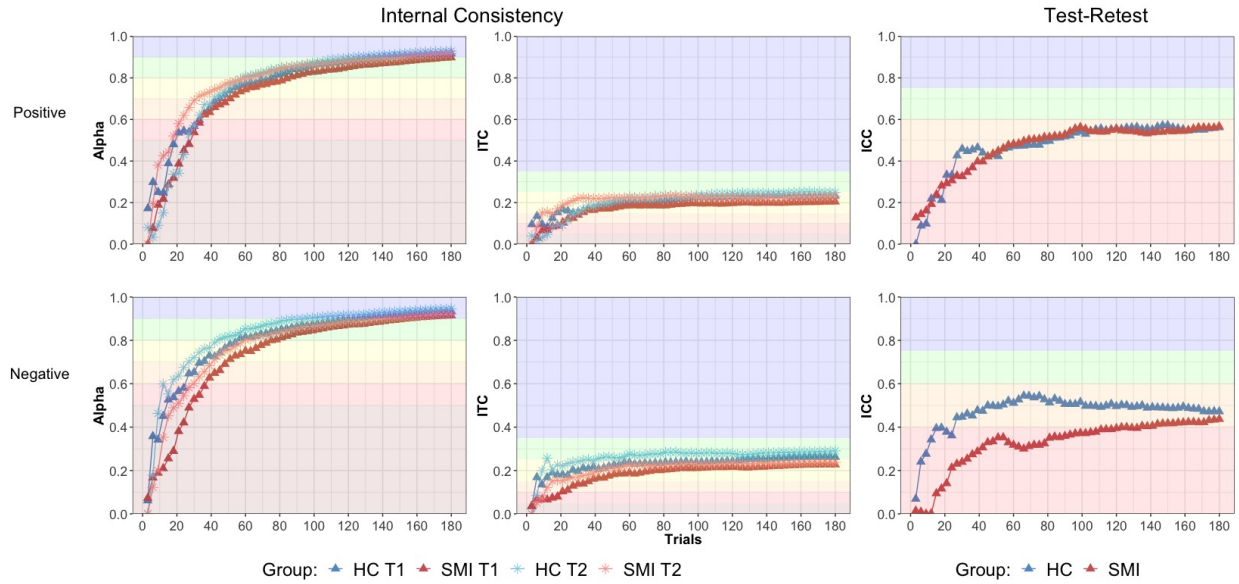
Columns represent Spearman-Brown Prophecy Corrected correlations and intraclass correlations (ICC(A,1)), respectively. Rows correspond to Change Detection computational parameters. Within each graph, individual points illustrate the reliability at the respective number of trials. Colors provide interpretation for the fit of the statistic.

Figure 3: Reliability of Change Localization Accuracy Per Number of Trials



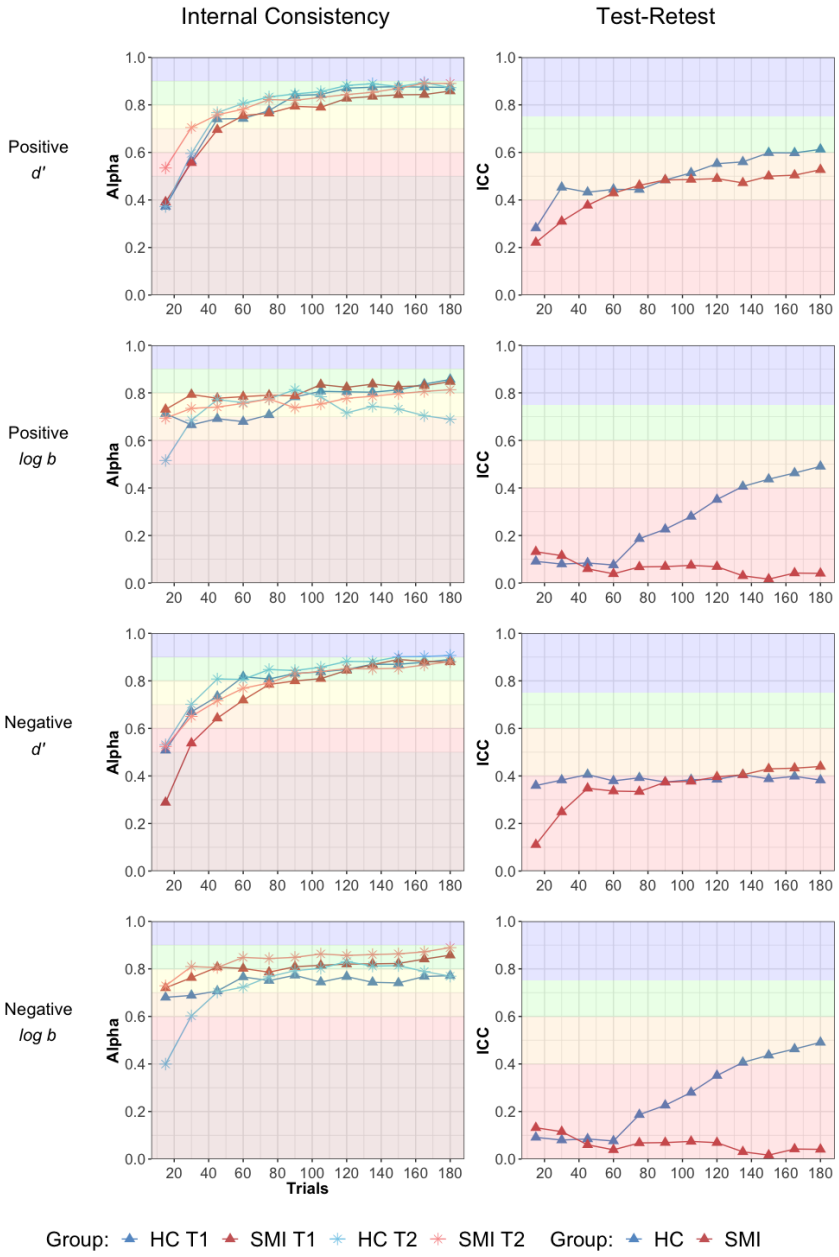
Columns represent Cronbach's α, mean item-total correlations (ITC), and intraclass correlations (ICC(A,1)), respectively. Within each graph, individual points illustrate the reliability at the respective number of trials. Colors provide interpretation for the fit of the statistic.

Figure 4: Reliability of the IPILT Accuracy Per Number of Trials



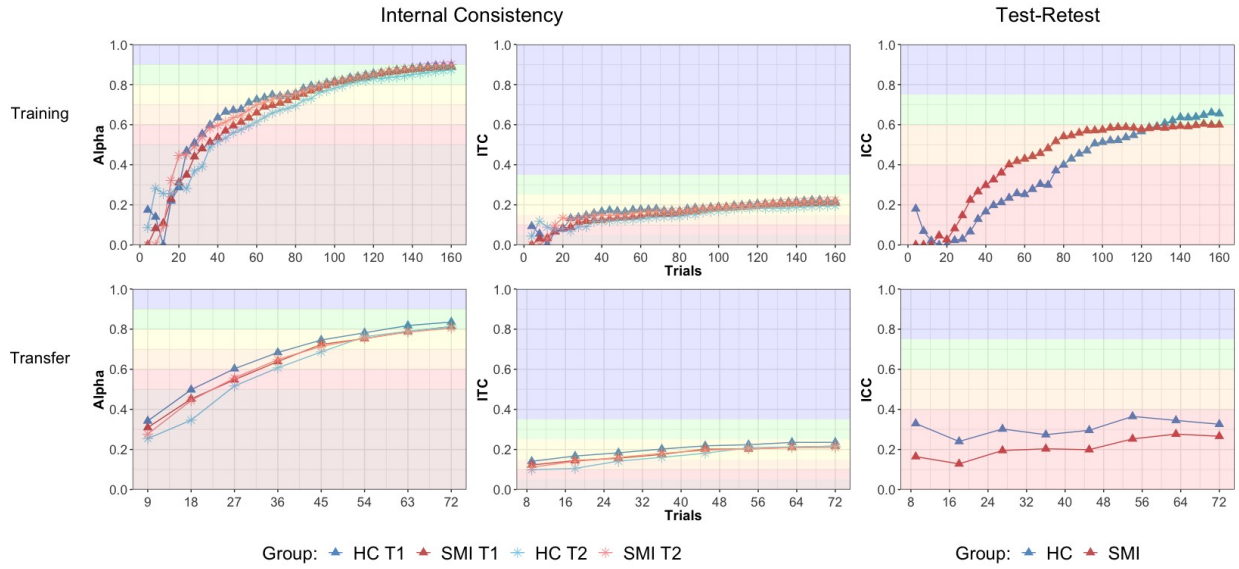
Columns represent Cronbach's α, mean item-total correlations (ITC), and intraclass correlations (ICC(A,1)), respectively. Within each graph, individual points illustrate the reliability at the respective number of trials. Colors provide interpretation for the fit of the statistic.

Figure 5: Reliability of the IPILT Discriminability and Response Bias Per Number of Trials



Columns represent Cronbach's α and intraclass correlations (ICC(A,1)), respectively. Rows correspond to IPILT-P and IPILT-N discrimination (*d'*) and response bias (*log b)*. Within each graph, individual points illustrate the reliability at the respective number of trials. Colors provide interpretation for the fit of the statistic.

Figure 6: Reliability of the EPILT Per Number of Trials



Columns represent Cronbach's α, mean item-total correlations (ITC), and intraclass correlations (ICC(A,1)), respectively. Rows correspond to the Training and Transfer phases of the EPILT, respectively. Within each graph, individual points illustrate the reliability at the respective number of trials. Colors provide interpretation for the fit of the statistic.