Neither measurement error nor speed-accuracy trade-offs explain the difficulty of establishing
attentional control as a psychometric construct:

Evidence from a latent-variable analysis using diffusion modeling

Alodie Rey-Mermet ^{1, 2*}

Henrik Singmann ^{1,3,4*}

Klaus Oberauer ¹

¹ University of Zurich and University Research Priority Program „Dynamics of Healthy Aging”

² Swiss Distance University Institute

³ University College London

⁴ University of Warwick

Author Note

Alodie Rey-Mermet, Faculty of Psychology, Swiss Distance University Institute, Brig, Switzerland (alodie.rey-mermet@fernuni.ch), and Department of Psychology, Cognitive Psychology Unit, University of Zurich, Switzerland, University Research Priority Program (URPP) "Dynamics of Healthy Aging", University of Zurich, Switzerland; Henrik Singmann, Department of Experimental Psychology, University College London, UK, Department of Psychology (h.singmann@ucl.ac.uk), University of Warwick, UK, and Department of Psychology, Cognitive Psychology Unit, University of Zurich, Switzerland; Klaus Oberauer, Cognitive Psychology Unit, Department of Psychology, University of Zurich, Switzerland (k.oberauer@psychologie.uzh.ch).

* Shared first authorship.

We analyzed archival data that can be accessed at <https://osf.io/rygex/>. Our complete analysis scripts and results will be posted at <https://osf.io/tyv9g/> once the manuscript will be accepted for publication. Some results of the present work were presented at the 59th Annual Meeting of the Psychonomic Society, New Orleans, USA, November 2018.

Correspondence should be addressed to Alodie Rey-Mermet, Swiss Distance University Institute, Faculty of Psychology, Bahnhofstrasse 9a, 3904 Naters, email alodie.rey-mermet@fernuni.ch.

Abstract

Attentional control refers to the ability to maintain and implement a goal and goal-relevant information when facing distraction. So far, previous research has failed to substantiate strong evidence for a psychometric construct of attentional control. This has been argued to result from two methodological shortcomings: (a) the neglect of individual differences in speed-accuracy trade-offs when only speed or accuracy is used as dependent variable, and (b) the difficulty of isolating attentional control from measurement error. To overcome both issues, we combined hierarchical-Bayesian Wiener diffusion modeling with structural equation modeling. We re-analyzed the dataset from Rey-Mermet, Gade, and Oberauer (2018), which includes data from a large set of attentional-control tasks from young and older adults. Even when accounting for speed-accuracy trade-offs and removing measurement error, measures of attentional control failed to correlate with each other and to load on a latent variable. These results emphasize the necessity of rethinking attentional control.

Keywords: executive functions, cognitive control, attentional control, individual differences, hierarchical-Bayesian Wiener diffusion modeling, structural equation modeling

1. Introduction

For more than 20 years, individual-differences researchers have put forward attentional control as a cognitive psychometric construct (e.g., Engle et al., 1999; Miyake et al., 2000). This construct – also referred to as cognitive control, executive control, executive attention, or executive functions – refers to a person’s ability to maintain and implement a goal and goal-relevant information in the face of distraction (von Bastian et al., 2020). This ability is meant to be general, contributing to success in many different tests and situations demanding attentional control. As such, it should be measurable as a latent variable (i.e., factor) that represents the shared variance of multiple empirical measures of attentional control. The key prerequisite for establishing such a psychometric construct is finding substantial positive correlations between multiple measures of the construct. However, previous research has failed to substantiate strong evidence for the existence of such a psychometric construct (see Karr et al., 2018, for a re-analysis of previous datasets; see Rey-Mermet et al., 2019, for an overview of the literature). Proponents of the attentional-control construct have argued that this state of affairs results from two methodological issues (see, e.g., Draheim et al., 2019): (a) the difficulty of isolating attentional control from measurement error, and (b) the neglect of individual differences in speed-accuracy trade-offs. The purpose of the present study was to establish attentional control as a psychometric construct when both issues were overcome. To this end, we combined hierarchical-Bayesian Wiener diffusion modeling with structural equation modeling (SEM).

1.1 Why is it difficult to establish attentional control at the latent-variable level?

At first glance, individual differences research was successful in establishing attentional control as a latent variable (e.g., Engle et al., 1999; Miyake et al., 2000). However, recent research has put this conclusion into question (see Rey-Mermet et al., 2019; Schubert & Rey-

Mermet, 2019; von Bastian et al., 2020, for overviews). First, in some studies, attentional-control tasks did not correlate consistently with each other (e.g., De Simoni & von Bastian, 2018; Guye & von Bastian, 2017; Paap & Greenberg, 2013; von Bastian et al., 2016), and in some cases they failed to load on a factor (Klauer et al., 2010; Krumm et al., 2009) or they had to be merged with other tasks to load on a factor (e.g., Brydges et al., 2012; Hedden & Yoon, 2006; Klauer et al., 2010; van der Sluis et al., 2007). Second, when a factor was established, it was often not coherent (e.g., Chuderski, 2015; Chuderski et al., 2012; Chuderski & Necka, 2012; Hull et al., 2008; Kane et al., 2016; McVay & Kane, 2012; Pettigrew & Martin, 2014; Shipstead et al., 2014; Unsworth et al., 2009; Unsworth & Spillers, 2010). That is, one task's factor loading was substantially higher than the loadings of the remaining tasks. Thus, the factor represented predominantly variance in one task, rather than common variance across multiple measures (see Rey-Mermet et al., 2018, 2019, 2020). Third, a re-analysis of previous datasets suggests a bias toward the publication of well-fitting but nonreplicable structural models of attentional control (Karr et al., 2018).

Proponents of the attentional-control construct have put forward two methodological reasons to explain the difficulty of establishing attentional control at the latent-variable level. A first reason is the contamination of attentional-control measures with measurement error (e.g., Hedge et al., 2018). This problem is severe because these measures must isolate attentional control from other sources of variance in task performance (e.g., intelligence, mental speed). To do so, attentional-control measures typically rely on measuring the performance difference between two conditions, one with high and one with low demand on attentional control. For example, in a color Stroop task, participants have to name the color of color words while ignoring the word meaning. The usual approach for isolating attentional control is by computing

the congruency effect, that is, by subtracting the mean RT on baseline trials (e.g., congruent trials, such as the word “red” printed in red for the color Stroop task) from the mean RT on trials with high demand on attentional control (i.e., incongruent trials, such as the word “green” printed in red). The problem with this approach is that there is much trial-by-trial variability in RTs, adding measurement noise to the mean RT that is unknown and unaccounted for (Rouder et al., 2019; Rouder & Haaf, 2019). Taking the difference of two – typically highly correlated – mean RTs removes much of the systematic individual-differences variance while compounding the measurement noise. Therefore, RT differences between congruent and incongruent conditions often have poor reliability (e.g., Hedge et al., 2018).

This shows that there is the need for a better approach of isolating true attentional-control variance. One approach used to bypass computing a difference score in SEM has been to apply a bifactor modeling approach. In this approach, two factors are modeled: (a) a general or common factor in which all trials with both low and high demand on attentional control are forced to load on, and (b) a specific attentional-control factor in which only trials with high demand on attentional control are forced to load on. The general or common factor is thus meant to extract all individual differences in non-attentional-control processes, whereas the factor extracting individual differences in attentional-control processes is the specific attentional-control factor. The results showed, however, no coherent specific factor of attentional control, thus further emphasizing the difficulty of establishing attentional control at the latent-variable level even when difference scores were not used (e.g., Keye et al., 2009; Rey-Mermet et al., 2018, 2019, 2020).

A second reason for the difficulty of establishing attentional control at the latent-variable level is that most attentional-control measures rely exclusively on response times, so that

individual differences in speed-accuracy trade-offs are disregarded (Draheim et al., 2019). Some participants might favor speed over accuracy, while others favor accuracy over speed. Hence, a measure that ignores individual differences in accuracy could miss a substantial part of the variance in attentional-control ability. One way to consider individual differences in speed-accuracy trade-offs is to integrate both dependent measures (i.e., RTs and accuracy) into a single score. Unfortunately, these scores are generally not able to unambiguously account for differences in speed-accuracy trade-offs (e.g., Liesefeld & Janczyk, 2019; Vandierendonck, 2017). Another way to take into account speed-accuracy trade-offs is to push all attentional-control variance into accuracy, for example by measuring attentional control under a time limit, which is calibrated according to each person's performance. This procedure has so far resulted in mixed evidence. When the time limit reached by the participant was used as the dependent measure, a factor of attentional control was reported (Draheim et al., 2020). In contrast, when in addition to measuring attentional control through accuracy, the calibration procedure was used to remove measurement error by reducing individual differences in non-attentional-control processes, no psychometric construct of attentional control was established (Rey-Mermet et al., 2019). However, the time deadline may affect how participants engage with the tasks, thus potentially undermining the validity of these tasks as measures of attentional control.

1.2 How to bypass the contamination of attentional-control measures with measurement error and the neglect of individual differences in speed-accuracy trade-offs?

One approach to address both issues – that is, the contamination of attentional-control measures with measurement error and the neglect of individual differences in speed-accuracy trade-offs – is to employ a cognitive model, the diffusion model (e.g., Donkin & Brown, 2018; Ratcliff & McKoon, 2008), as measurement model. This means that the parameters obtained

from the diffusion model are used as measures of the latent variables of interest, such as the ability to control attention.

The diffusion model is a well-validated model of simple decisions that uses the full set of behavioral data to estimate three parameters of theoretical interest: (1) The drift rate, reflecting the rate at which information in favor of one or the other decision accumulates over time, (2) the caution parameter controlling the speed-accuracy trade-off, and (3) the non-decision time capturing primarily the speed of sensory and motor processes. This model accounts for the effect of speed-accuracy trade-offs by separating the quality of information processing (i.e., the drift rate) from the person's speed-accuracy setting (i.e., the caution parameter). Moreover, the diffusion model addresses the problem of measurement error in two ways. First, the diffusion model is fitted to the joint distribution of RT and accuracy, which thus considerably reduces the effect of individual RTs, and the associated noise, on the parameter estimates compared to mean RTs. Second, the diffusion-model parameters jointly determine the width of the RT distribution, thereby accounting for the trial-by-trial variability of each participant.

So far, only two studies on attentional control have combined a diffusion modeling approach with SEM or at least a correlative approach. In a first study (Hedge et al., 2020), the diffusion model for conflict tasks (Ulrich et al., 2015) – an extension of the diffusion model – was applied to different datasets including two to three Stroop-like tasks. In this model, in addition to the three typical parameters, a rescaled gamma function was modeled to capture conflict processing (in particular, the assumption that the stimulus features to be ignored have an early impact on the decision processes). This changes the interpretation of the drift-rate parameter which, in this case, reflects the processing efficiency once the conflict has been resolved (rather than the quality of the evidence in the complete decision process, as typically

interpreted in the standard diffusion model). Hedge et al. (2020) reported significant zero-order correlations ($r_s = .32$ to $.56$) for the parameters reflecting non-conflict processing in their diffusion modeling approach (e.g., the drift rate, boundary separation, and non-decision time). However, the correlations between parameters reflecting conflict processing (i.e., those underlying the gamma function) were weak ($r_s = .04$).

In the second study (Weigard et al., 2020), the EZ diffusion model (Wagenmakers et al., 2007) was used to estimate the drift rates separately for trials with low and high demand on attentional control. In a first approach, they fit a bi-factor model with a general factor on which all drift rates from all tasks loaded, and specific factors – one for each task – on which the drift rates of the same task loaded. The results showed a coherent general factor and coherent specific task factors for most tasks. In a second approach, the drift rates were then subtracted between both low and high attentional-control conditions to obtain measures of attentional control. SEM on these drift-rate differences, however, identified no model including a coherent factor of attentional control.

Together, these studies represent a first step by demonstrating the feasibility of combining diffusion model with a correlative approach or SEM. However, they are not sufficient to determine whether a coherent factor of attentional control can be established when individual differences in speed-accuracy trade-offs and measurement error were accounted for. The study of Hedge et al. (2020) has two limitations. One is that the extension of the diffusion model by a conflict-resolution parameter increases the model complexity, and with it the risk of overfitting noise in the data (e.g., van Ravenzwaaij & Oberauer, 2009). The other is that the analyzed datasets included only two to three Stroop-like tasks, and no SEM was computed. This limits the generality of these findings and does not allow us to conclude whether the impact of speed-

accuracy trade-offs and/or the contamination of attentional-control measures by measurement error explain the difficulty of establishing attentional control at the latent-variable level. The study of Weigard et al. (2020) also has limitations: Their bifactor model did not include a specific attentional-control factor. Therefore, this approach enables no conclusion about whether attentional control can be established at the latent-variable level. Furthermore, in the second approach, computing the drift rates separately for trials with low and high demand on attentional control and then subtracting them compounds the measurement error of the two drift-rate estimates in the same way as subtracting mean RTs does. This counteracts the reduction of measurement error that can be achieved when combining diffusion modeling with SEM.

1.3 The present study

The purpose of the present study was to establish a coherent factor of attentional control when individual differences in speed-accuracy trade-offs and measurement error were accounted for. To this end, we combined a *hierarchical-Bayesian Wiener diffusion model* (e.g., Donkin & Brown, 2018; Ratcliff & McKoon, 2008; Vandekerckhove et al., 2011) with SEM. To perform these analyses, we reused the dataset from Rey-Mermet et al. (2018). In that study, attentional control was measured with a wide variety of tasks assumed to assess inhibition (i.e., the ability to suppress pre-potent responses and/or ignore irrelevant information; see Table 1). This is relevant because inhibition tasks are mainly used to assess attentional control (see Rey-Mermet et al., 2019, for an overview of the tasks used in previous research). Moreover, inhibition tasks have been assumed to measure the core ability necessary for attentional control (Miyake & Friedman, 2012). Thus, if the central construct of attentional control collapses, this questions attentional control as a whole.

In the present study, we opted for the hierarchical-Bayesian Wiener diffusion model

because it combines the strength of the diffusion model and of the Bayesian-hierarchical framework to reduce the measurement error. Specifically, it takes into account the measurement error in three different ways. First, the Wiener diffusion model addresses the problem of measurement error – that is, the noise arising from trial-by-trial variability of RTs – because it explicitly models the shape of the trial-by-trial RT distribution. In particular, it predicts the presence of long RTs, thus accounting for the right-skewed nature of the RT distribution by design. Second, the hierarchical-Bayesian framework (Vandekerckhove et al., 2011) substantially reduces the measurement error in the individual-level effects because the parameter estimates of each individual participant are informed by the data of all other participants (e.g., Rouder & Haaf, 2019). Third, because we modeled the variance-covariance matrix of the individual-level effects – the basis for the SEM analysis – as a parameter of the hierarchical model, this joint estimation decreased the measurement error in the variance-covariance matrix, compared to a two-step procedure in which first the diffusion model parameters are estimated, and the correlations are then subsequently calculated from these estimates (cf. Hedge et al., 2020; Weigard et al., 2020). Furthermore, because we implemented our model in Stan (Carpenter et al., 2017), we could use a non-informative LKJ prior for the correlation matrix as well as independent weakly informative priors for the variances. This approach has two advantages over common alternatives in Bayesian approaches, such as a (scaled) inverse-Wishart prior for the variance-covariance matrix (e.g., JAGS, Plummer, 2003; Rouder et al., 2019; see Gelman & Hill, 2007): The LKJ prior is conceptually and computationally simpler, and it is less likely to have an effect on the estimation of the variance-covariance matrix.

Moreover, we selected the hierarchical-Bayesian Wiener diffusion model because it can be applied to the broad variety of tasks measured by Rey-Mermet et al. (2018). This is, for

example, not the case for the diffusion model for conflict tasks, which was specifically developed to account for the flanker and Simon tasks (Ulrich et al., 2015) and then recently extended to Stroop tasks (Ambrosi et al., 2019; Hedge et al., 2019, 2020). Another critical advantage of using the Wiener diffusion model, and not a more complicated diffusion model, such as the diffusion model for conflict tasks, is that choosing a simpler model variant strikes a good compromise in the statistical *bias-variance trade-off* (e.g., Yarkoni & Westfall, 2017). Any statistical model needs to strike a balance between the ability to adequately describe the signal in the data and at the same time avoid overfitting the noise. Our model accounts for the main pattern in the data that are relevant for our research question (i.e., the impact of the speed-accuracy trade-off and measurement error). A more complex diffusion model may be able to account for some more subtle data patterns of some of the tasks (i.e., achieve lower bias), but is more likely to overfit (i.e., higher variance; White et al., 2018). This is in line with theoretical and empirical results showing that simpler diffusion models outperform complex diffusion models in terms of predictive ability and parameter stability (Boehm et al., 2018; Dutilh et al., 2019; van Ravenzwaaij & Oberauer, 2009).

How did we combine the hierarchical-Bayesian diffusion model with SEM? The hierarchical-Bayesian diffusion model provided us with a posterior distribution of the variance-covariance matrix of the diffusion-model parameters across tasks. In a second analysis step, we applied SEM to the posterior distribution of the variance-covariance matrix. We did this once with the posterior means (as point estimates of the variances and covariances), and again multiple times with samples from the full posterior distribution to gauge what the uncertainty in the posteriors implies for the uncertainty of SEM fits and parameter estimates. Moreover, we used two approaches to isolate the attentional-control variance. Following the predominant

practice in previous research, we used a *difference coding* approach to estimate parameters differences between the two conditions (i.e., trials with low and high demand on attentional control). To avoid the measurement error from the subtraction of drift rates, we directly modeled a difference parameter representing the mean difference between trials with low and high demand on attentional control. In addition, to overcome the problems associated with difference scores, we employed a *condition coding* approach, which starts from parameters in each condition and uses a bifactor SEM to extract attentional-control variance.

We hypothesized that if the difficulty of establishing attentional control as a psychometric construct results from the neglect of individual differences in speed-accuracy trade-offs and/or the large measurement error in attentional-control measures, we would find a coherent factor of attentional control. The reason is that we employed a state-of-the-art computational approach for addressing these issues: The Wiener diffusion model accounts for speed-accuracy trade-offs in a theoretically principled way, and our hierarchical-Bayesian implementation estimated the variance-covariance matrix – the basis for the SEM – as a set of latent model parameters separated from trial-by-trial noise, thereby minimizing the influence of measurement error. In contrast, if the difficulty of establishing attentional control as a psychometric construct reflects the true nature of attentional control – namely, that a *general* ability to control attention does not exist – then we would expect to find no coherent factor of attentional control.

We expected to establish a coherent factor of attentional control on the drift-rate parameter. The reason is that the drift-rate parameter is usually assumed to reflect the effect of attentional control in attentional-control tasks (e.g., White et al., 2018). More specifically, the drift rate represents the rate with which information for the correct response is accumulated in a given trial (e.g., Ratcliff & McKoon, 2008). In attentional-control tasks, the ability to control

attention is assumed to play a primary role in how well individuals can extract information from the relevant aspect of the stimulus to select the correct response (e.g., Hübner et al., 2010; Ridderinkhof, 2002; Verguts & Notebaert, 2009; Wiecki et al., 2013). Thus, a higher ability to control attention should result in a more efficient information processing, which, in turn, should impact the rate with which information towards the correct response is accumulated – that is, the drift rate. This suggests that relative to a condition with low demand on attentional control, the drift rate should be reduced in the condition with high demand on attentional control because the irrelevant and misleading information of this condition pushes the accumulator towards the wrong response. Moreover, an individual with better attentional-control ability should show a smaller reduction of drift rates between both conditions because of their better ability to prevent irrelevant and misleading information from affecting the drift rate.

In contrast, if a coherent factor is established on the caution or non-decision time parameters, this would not support attentional control as a psychometric construct. The reason is that these parameters are assumed to capture speed-accuracy trade-offs and the speed of sensory and motor processes, respectively.

2. Method

2.1 Participants

Our analysis is based on data from 120 young adults and 143 older adults collected by Rey-Mermet et al. (2018), that is, their full sample after removing participants that did not meet their psychiatric or demographic inclusion criteria or for which some error occurred during testing. On a by-task basis, we removed the data from participants for which more than 2.5% of the trials were outside the response window of 2 seconds. This procedure removed a total of 4.6% of trials. The reason for this is that responses outside this window were not recorded (i.e.,

the RT distribution was censored) and applying the diffusion model to an RT distribution where a non-negligible part of the distribution is censored can lead to biased estimates. The final sample size for each task is presented in Table 1.

2.2 Tasks

We reanalyzed eight tasks (i.e., the color Stroop, number Stroop, arrow flanker, letter flanker, Simon, global-local, positive compatibility, and negative compatibility tasks) from Rey-Mermet et al. (2018). These were the tasks for which the attentional-control measure was the difference between congruent and incongruent trials, so the same diffusion model could be applied to all tasks. The tasks are shortly described in Table 1. A complete description of the tasks (including material and procedure) can be found in Rey-Mermet et al. (2018).

2.3 Data Preparation

In addition to the 4.6% of trials discussed above, we excluded 0.15% of trials where either the computer malfunctioned, participants used an illegitimate response key, or responded too fast (i.e., faster than 0.2 seconds). On a by-trial basis, we also removed the remaining 0.1% of trials with responses outside the 2 seconds response window.

2.4 Data Analysis

2.4.1 Diffusion Modeling

We used a hierarchical-Bayesian variant of the Wiener diffusion model (Vandekerckhove et al., 2011) with an accuracy coding. This model accounts for the entire data (i.e., RT distributions of correct and error trials) with three latent parameters: (a) the drift rate, a measure of the efficiency of information processing in the decision process, (b) the boundary separation, a measure of response caution that controls the speed-accuracy trade-off, and (c) the non-decision time. To allow the model to adequately account for age effects, we applied it separately to all

individual choices and associated RTs across tasks in each age group. Furthermore, we applied two different parametrizations – a difference-coding parametrization and a condition-coding parametrization – to the data of each age group.

In the *difference-coding* parametrization, we only considered the congruent and incongruent trials (i.e., we removed the 33% neutral trials). For the drift rate, we estimated two fixed-effect parameters per task: (a) the mean in the congruent trials and (b) a difference parameter representing the mean difference between congruent and incongruent trials. This difference parameter was our main measure of attentional control. In the *condition-coding* parametrization, we considered all trial types (i.e., incongruent, congruent, and neutral). For the drift rate, we estimated three fixed-effect parameters, that is, one mean drift rate for each trial type. For both parametrizations, we estimated one fixed-effect parameter per task for the boundary separation and for the non-decision time, respectively.

In addition to the fixed-effect (or group-level) parameters, we estimated by-participant random-effects, the correlations among all random-effects for each fixed-effect parameter, and individual-level parameters. This resulted in quite large models. The smallest model (difference coding for young adults) had 5022 parameters (among this, 630 correlations) for 157,737 observations. For each participant and task, the average number of trials was 150 for the difference-coding parametrization (minimum = 48, maximum = 192) and 224 (minimum = 72, maximum = 288) for the condition-coding parametrization.

We estimated the model using brms (Bürkner, 2017). This allowed us to estimate the variance-covariance matrix in two parts, a correlation matrix and a variance vector. For the correlation matrix, we used a non-informative LKJ prior with concentration parameter = 1, and for the variances a weakly informative t -distribution prior ($df = 3$, scale = 10). All other priors

were also weakly informative (i.e., Cauchy with scale of 5 for mean drift rate parameters for both coding parametrizations, and with scale of 2.5 for drift rate difference parameters for difference coding). We estimated all diffusion-model parameters and correlations on the unconstrained real line (i.e., no link functions). In other words, we assumed a multivariate normal distribution for all individual-level parameters.

We estimated each model using 4 independent chains with 500 warmup samples per chain and retained a further 500 post-warmup samples. Chain convergence was good (i.e., $\hat{R} < 1.05$) for all fixed-effect and random-effect parameters, with the exception of three correlation parameters that were not important for our results (i.e., three correlations between a drift-rate difference parameter in one task and a non-decision time parameter in a different task had $\hat{R} < 1.07$; one for the old age group and two for the young age group). Estimating each model took more than one month on a high-performance desktop computer.

2.4.2 Structural Equation Modeling

For each age group, we first estimated a series of SEMs using the posterior means of each value in the variance-covariance matrix of the diffusion-model parameters (each analysis only used a subset of the full covariance matrix; e.g., only the covariances among the difference parameters of the drift rate). Then, we fitted the same series of SEMs separately to 500 variance-covariance matrices sampled from the posterior distribution. The latter analysis takes into account the uncertainty in the estimate of the covariance matrix, which depends in part on the measurement error in the data.

Latent variable models were estimated in R (R Core Team, 2020) using the lavaan package (Rosseel, 2012). Model fit was evaluated via multiple fit indices (Hu & Bentler, 1998, 1999): The χ^2 goodness-of-fit statistic, Bentler's comparative fit index (CFI), the root mean

square error of approximation (RMSEA), the standardized root-mean-square residual (SRMR), the Akaike information criterion (AIC), and the Bayesian information criterion (BIC). For the χ^2 statistic, a small, non-significant value indicates good fit. For the CFI, values larger than .95 indicate good fit, and values between .90 and .95 indicate acceptable fit. RMSEA values smaller than .06 and SRMR values smaller than .08 indicate good fit. However, as the RMSEA tends to over-reject true population models at small sample size (i.e., smaller than 250; Hu & Bentler, 1998), we report it for the sake of completeness only. For the AIC and the BIC, smaller indices indicate better fit.

In addition, the following criteria had to be met for a model to be considered an adequate representation of a latent variable: (1) the Kaiser-Meyer-Olkin (KMO) index – a measure of whether the correlation matrix is factorable – should be larger than .60 (Tabachnick & Fidell, 2019); (2) most of the error variances should be lower than .90; (3) most of the factor loadings should be significant and larger than .30; (4) no factor should be dominated by a large loading from one task; (5) the amount of shared variance across tasks – that is, “factor reliability” as assessed by the coefficient ω for the single-factor models (Raykov, 2001), and the hierarchical omega ω for the bifactor models (McDonald, 1999) – had to be reasonably high (i.e., about .70 in single-factor models and .20 in bifactor models; Gignac & Kretzschmar, 2017).

3. Results

For each age group, we first assessed the model fit of the diffusion model comparing the posterior predictive distribution – that is, a distribution of simulated data generated from samples from the posterior distribution of the model parameters – with the actual data. Second, we examined the correlational pattern of all parameters, and investigated the relations between them at the latent-variable level through SEM.

3.1 Young Adults

3.1.1 Diffusion Modeling

Figure 1 show the fits of the diffusion model and includes numerical summaries of fit. We used a measure of absolute agreement between observed and predicted data, the concordance correlation coefficient (*CCC*; Barchard, 2012). For both parametrizations and all tasks, for both posterior predictive mean and across the posterior predictive distribution, mean RTs are recovered almost perfectly ($CCC \geq .90$) and accuracy was recovered at least acceptably ($CCC \geq .64$).

In line with previous findings, recovery of the congruency effects was less impressive. Recovery was worse for the difference coding, which included parameters capturing the congruency effects, than for the condition coding, which did not include such parameters. Furthermore, recovery differed markedly between the posterior predictive mean and across the full posterior predictive distribution. When only considering the former, recovery was good (with one exception) for the condition coding. However, recovery across the individual samples of the posterior predictive distribution was in many instances poor, suggesting that the posterior predictive mean paints a rather optimistic picture. The largest misfit was observed for the accuracy congruency effects, where the predicted effects showed a markedly reduced range compared to the observed ones. This suggests that even with a large number of observations, the information provided by the data was sometimes not enough to overwhelm the only weakly informative priors. This particularly applies to the difference coding.

3.1.2 Correlations and Structural Equation Modeling

Correlations are presented in the upper part of Table 2 for the drift rate with the difference coding, in the upper part of Table 3 for the drift rate with the condition coding, in the

upper part of Table 4 for the boundary separation, and in the upper part of Table 5 for the non-decision time. SEM results are reported next for each of these parameters separately.

3.1.2.1 Drift Rate with Difference Coding

In this analysis, we used the posterior mean correlations of drift-rate differences between congruent and incongruent conditions in each task as the measures of attentional control. Nearly all correlations were low ($\leq .20$), and none were credibly different from zero (see Table 2, upper part). Overall, the correlation matrix had a KMO index (.57) slightly under the lower limit of .60, indicating only marginal factorability of the correlation matrix.

In the next step, we aimed to find a coherent factor of attentional control. To this end, we fitted Model 1 in which the drift-rate differences from all tasks loaded on a single factor. This model is depicted in Figure 2a. This model provided a good fit to the data, $\chi^2(20, N = 120) = 11.72, p = .925, CFI = 1, RMSEA [90\% CI] = 0 [0, .03], SRMR = .05, AIC = 387.80, BIC = 432.40$. However, only three measures loaded significantly on the factor, with the loading of the letter flanker task dominating the remaining loadings. Moreover, error variances were high for most measures (see Figure 2a), and factor reliability was low ($\omega = .35$). Together, this indicates that the factor mainly represents the variance of one measure. Thus, although the model provided a good fit to the data, it had low explanatory power.

3.1.2.2 Drift Rate with Condition Coding

This analysis started from the posterior mean correlations of mean drift rates of congruent and incongruent conditions, and used a bifactor model (Model 2) to extract the variance of attentional control from them. As expected, correlations of drift rates between the incongruent and congruent trials of the same task were high ($\geq .52$) and credible. Furthermore, although most correlations between the different tasks were lower (between .04 and .41), many were still

credible (see Table 3, upper part). Overall, the correlation matrix had a good KMO index (.73).

In the bifactor model, the drift rates of both congruent and incongruent conditions of all tasks were forced to load on a general factor, which reflects variance in all abilities involved in task performance regardless of the condition's attentional-control demand. The drift rates from the condition assumed to demand more attentional control (i.e., the incongruent condition in all tasks, except for the negative-compatibility task for which the congruent condition demands more control) were forced to additionally load on an attentional-control factor. In this way, the attentional-control factor reflects the common variance across tasks that is not accounted for by the general factor. In addition, error variances from the measures of the same task were allowed to correlate. Moreover, to avoid negative variance, factor loadings were constrained to be positive. This model is depicted in Figure 2b.

This model provided a good fit to the data, $\chi^2(88, N = 120) = 86.86, p = .514, CFI = 1$, RMSEA [90% CI] = 0 [0, .05], SRMR = .06, AIC = 3237.64, BIC = 3371.44. However, although all measures loaded significantly on the general factor, nearly all loadings for the attentional-control factor were low and not significant. Moreover, the factor reliability for this specific factor was very low ($\omega = .08$). Thus, the attentional-control factor did not represent much common variance across the measures.

3.1.2.3 Boundary separation

Most correlations were between .20 and .43, and credibly differed from 0 (see Table 4, upper part). Overall, the correlation matrix had a good KMO index (.76).

In the next step, we fitted a Model 1 in which all tasks loaded on a single factor. This model is depicted in Figure 2c. This model provided an acceptable fit to the data, $\chi^2(20, N = 120) = 30.44, p = .063, CFI = .91$, RMSEA [90% CI] = .07 [0, .11], SRMR = .07, AIC = 427.16, BIC

= 471.76. All loadings were relatively high and significant, and error variances were low (see Figure 2c). Factor reliability was also high ($\omega = .70$). Thus, these results indicate a coherent factor, reflecting individual differences in speed-accuracy trade-off, or caution, that generalize across tasks.

3.1.2.4 Non-decision time

Most correlations were low ($\leq .20$) and did not credibly differ from 0 (see Table 5, upper part). However, the correlation matrix had a good KMO index (.64).

As before, we fitted a Model 1 in which all tasks loaded on a single factor. However, this model provided a bad fit, $\chi^2(20, N = 120) = 31.95, p = .044$, CFI = .82, RMSEA [90% CI] = .07 [.01, .11], SRMR = .07, AIC = -3552.53, BIC = -3507.93.

3.2 Older Adults

3.2.1 Diffusion Modeling

Figure 3 shows the fit of the diffusion model. As for young adults, overall recovery of mean RTs ($CCC \geq .92$) and accuracy was acceptable ($CCC \geq .63$). The only exception was the arrow-flanker task, which showed a ceiling effect in accuracy. For the congruency effect, we saw a very similar pattern as for the young adults. Recovery was better for the condition coding than the difference coding, and the posterior predictive mean recovered markedly better than individual samples from the posterior predictive distribution. The one difference was that recovery for the RT congruency effect was a bit better than for the young participants, but recovery of the accuracy effect was worse (i.e., the restricted range in the predicted accuracy effect was even more noticeable). However, overall individual differences were still adequately captured for most tasks.

3.2.2 Correlations and Structural Equation Modeling

Correlations are presented in the lower part of Table 2 for the drift rate with the difference coding, in the lower part of Table 3 for the drift rate with the condition coding, in the lower part of Table 4 for the boundary separation, and in the lower part of Table 5 for the non-decision time. SEM results are reported next for each of these parameters separately.

3.2.2.1 Drift Rate with Difference Coding

All correlations between drift-rate differences were low ($\leq .23$) and not credible (see Table 2, lower part), with one exception (i.e., the correlation between arrow-flanker and positive-compatibility measures). Overall, the correlation matrix had a KMO index (.56) slightly under the limit of .60.

Similar to the young adults, we fitted Model 1 in which all drift-rate differences were forced to load on a single factor. This model provided a bad fit to the data, $\chi^2(20, N = 140) = 24.34, p = .228, CFI = .85, RMSEA [90\% CI] = .04 [0, .09], SRMR = .06, AIC = 607.96, BIC = 655.03$.

3.2.2.2 Drift Rate with Condition Coding

Correlations between the drift rates of incongruent and congruent conditions of the same task were high ($\geq .71$) and credible. Furthermore, although most correlations between the different tasks were lower (between .04 and .48), many were still credible (see Table 3, lower part). Overall, the correlation matrix had a good KMO index (.61).

In the next step, we fitted the same bifactor model (Model 2) as for the young adults. This model is depicted in Figure 4a. This model provided a good fit to the data, $\chi^2(88, N = 140) = 116.21, p = .024, CFI = .98, RMSEA [90\% CI] = .05 [.02, .07], SRMR = .06, AIC = 4194.70, BIC = 4335.90$. However, although all measures loaded significantly on the general factor, most loadings for the attentional-control factor were low and not significant (see Figure 4a).

Moreover, the reliability for this specific factor was very low ($\omega = .05$). Thus, this model estimation did not result in a coherent factor of attentional control.

3.2.2.3 Boundary separation

Most correlations were low ($\leq .20$) and not credible (see Table 4, lower part). However, the correlation matrix had a good KMO index (.68).

Similar to young adults, we fitted a single-factor model. However, this model provided a bad fit, $\chi^2(20, N = 140) = 33.44, p = .030$, CFI = .84, RMSEA [90% CI] = .07 [.02, .11], SRMR = .08, AIC = 1808.13, BIC = 1855.19.

3.2.2.4 Non-decision time

Most correlations were low and not credible (see Table 5, lower part). Overall, the correlation matrix had a good KMO index (.70).

As before, we fitted a single-factor model. This model is depicted in Figure 4b. This model provided an acceptable fit to the data, $\chi^2(20, N = 140) = 28.69, p = .094$, CFI = .91, RMSEA [90% CI] = .06 [0, .10], SRMR = .06, AIC = -3035.76, BIC = -2988.7. All tasks loaded significantly on the hypothesized factor, and error variances were relatively low (see Figure 4b). However, factor reliability was only of modest size ($\omega = .56$). Thus, the results suggest a moderately coherent factor.

3.3 SEM analyses on 500 covariance matrices sampled from the posterior distribution

In a next step, we fitted the models for all parameters (i.e., the drift rates in both difference and condition coding parametrization, as well as the boundary separation, and the non-decision time) in both age groups (young and older adults) to 500 covariance matrices sampled from the posterior distribution. This generated a distribution of 500 SEM fits that reflects the uncertainty of the correlation estimates, and a corresponding distribution of 500

values for each statistic reflecting model fit and model adequacy, approximating their posterior distribution.

An overview of the goodness-of-fit statistics of these model assessments is shown in Figure 5a. As shown in this figure, only a few models provided an acceptable fit to the data, whereas the majority of the models provided a bad fit. Moreover, for all models with an acceptable fit, the attentional-control factor was not coherent because the factor reliability was low to very low (see Figure 5b). The factor reliability was, however, higher and around .70 for the general factor in bifactor models in young and older adults, for the boundary-separation factor in young adults, and for the non-decision time in older adults.

3.4 Multiverse Approach

To test the robustness of our results, we reran the analyses by applying the following modifications. First, because the hierarchical-Bayesian Wiener diffusion modeling with the difference coding did not recover the data for all measures well (see Figures 1 and 3), we estimated the models to several subsets of data in which the tasks with a poor diffusion-model fit were excluded. For the young adults, we fitted single-factor models excluding the following sets of tasks: (a) color Stroop, (b) global-local, (c) color Stroop and global-local, (d) color Stroop and letter flanker, (e) color Stroop, global-local, and letter flanker, (f) color Stroop, letter flanker, and negative compatibility, (g) color Stroop, global-local, letter flanker, and negative compatibility, (h) color Stroop, global-local, letter flanker, and number Stroop, (i) color Stroop, global-local, letter flanker, negative compatibility, and number Stroop, and (j) color Stroop, global-local, letter flanker, negative compatibility, and positive compatibility. For the older adults, we fitted single-factor models excluding the task(s): (a) negative compatibility, (b) negative compatibility and letter flanker, (c) negative compatibility and positive compatibility, (c) negative compatibility,

letter flanker, and positive compatibility, (d) negative compatibility, letter flanker, and number Stroop, (e) negative compatibility, letter flanker, positive compatibility, and number Stroop, (f) negative compatibility, letter flanker, positive compatibility, number Stroop, and Simon, (h) negative compatibility, letter flanker, positive compatibility, number Stroop, and arrow flanker.

Second, for the condition coding, we also fitted bifactor models in which neutral conditions, rather than congruent conditions, were included as baseline conditions that loaded only on the general factor. Third, all bifactor models were estimated with different constraints: (a) factor loadings were constrained to be positive, (b) error variances were constrained to be positive, and (c) neither factor loadings nor error variances were constrained to be positive.

The goodness-of-fit statistics of these model assessments and the parameter estimates can be found at <https://osf.io/tyv9g/> (once the manuscript will be accepted for publication). None of these estimations resulted in a well-fitting model with a coherent factor of attentional control (i.e., with a high factor reliability, low error variances, and significant but non-dominant loadings for the attentional-control factor).

4. Discussion

The purpose of the present study was to establish a coherent factor of attentional control when individual differences in speed-accuracy trade-offs and measurement error were accounted for. To this end, we re-analyzed the data from Rey-Mermet et al. (2018) by computing SEM using correlations based on parameters (drift rates, or congruency effects on drift rates, boundary separation and non-decision time) of a hierarchical-Bayesian Wiener diffusion model. This combines a well-tested cognitive model of response selection – including speed-accuracy trade-offs – as measurement model with the current gold standard for statistical modelling of trial-by-trial noise (Rouder et al., 2019).

The present results showed that for both young and older adults, the drift-rate parameters and the congruency effects on drift rates hardly correlated across tasks. Moreover, irrespective of how we isolate attentional-control variance (i.e., using drift-rate differences in a single-factor model, or through a bifactor model of drift rates), SEM identified no model including a factor that generalizes across tasks. Therefore, even if we take individual differences in speed-accuracy trade-offs into account and isolate attentional control from measurement error, no coherent factor emerges. Moreover, bypassing difference scores by applying a bifactor-modeling approach did not solve the issue (see also Rey-Mermet et al., 2018, 2019, 2020; cf. Draheim et al., 2019, 2020). These findings indicate that the difficulty of establishing attentional control as a psychometric construct is not primarily a measurement problem. This result challenges attentional control as a psychometric construct. Therefore, we need to rethink either how we assess attentional control or how we think about attentional control as a psychometric construct.

4.1 Attentional control does not exist as a psychometric construct: Is this conclusion warranted?

One may argue that our conclusion is not warranted because (a) we did not have enough power in the present study, (b) the dataset was not representative, and (c) we controlled too much for non-attentional-control processes. We next discuss each of these concerns in detail.

4.1.1 Enough Power?

A first concern may be whether in the present study, we have enough power – or precision of measurement in a Bayesian framework – to establish attentional control at the latent-variable level or to accept the null hypothesis (i.e., to determine that attentional control at the latent-variable level has no empirical basis). First, the sample sizes for each of our age groups was approximately equal to the sample sizes of the original studies establishing the idea of

attentional control as a psychometric construct (Engle et al., 1999; Miyake et al., 2000). If these sample sizes are enough to establish this idea, the almost complete absence of evidence for the idea with a considerably improved analysis strategy and a large number of different attempted SEMs cannot be easily dismissed. Second, inspection of Tables 2 to 5 shows that the correlation coefficients, which form the basis for the SEMs, are estimated quite precisely in most cases. More specifically, in most cases in which the correlations do not differ credibly from 0, the Bayesian 95% Credible Intervals, reflecting the width of the posteriors, are quite narrow around 0 and generally exclude medium or large correlations as improbable. The data provides sufficient evidence that nearly all of the correlations of the drift-rate difference parameters across tasks are small at best (Table 2). Moreover, most of their posteriors had substantial mass on both sides of zero, implying that the correlations are equally likely to be positive (the predicted direction) or negative (which is incompatible with the idea of a general attentional control factor). This suggests that our correlation coefficients are precise enough to warrant our conclusion.

The generally positive assessment of the precision with which we estimated the correlations contrast with the results reported by Rouder et al. (2019) who used the same data set. Their results showed low correlations suffering from a large degree of imprecision (i.e., wide posterior distributions), leading them to the conclusion that most studies using attentional-control tasks “are doomed to fail” (Rouder et al., 2019, p. 8). However, even though they also used a hierarchical-Bayesian model, their model assumed that response times follow a normal distribution, an assumption that the data clearly violates. Thus, it appears that our approach of using a dedicated response time distribution, the Wiener diffusion model, improves the precision

of the correlation estimates to a noticeable degree.¹ With such an approach, it is possible to draw conclusions from studies using attentional-control tasks. However, the main conclusion is that no psychometric construct of attentional control exists.

4.1.2 Representativeness of the dataset

A second concern may be whether the dataset we used is sufficiently representative to draw our conclusions. For example, in the present dataset, we observed a median correlation of $r = .08$ for the RT difference scores (see Rey-Mermet et al., 2018). In contrast, von Bastian et al. (2020) reported in a systematic review of the literature that the overall median correlation between behavioral attentional-control measures is about $r = .16$. However, when only inhibition tasks are considered, the median correlation lowers to $r_s = .11$. The median correlation further lowers to $r = .09$ when the same tasks as in the present dataset were included in the computation. Thus, the median correlation we observed in the present dataset are very close to those observed in the literature using the same tasks. Moreover, in the present dataset, we had a higher median reliability ($\rho = .72$) than the median reliability reported by von Bastian et al. (2020) for the same inhibition measures as those used in the present study ($\rho = .66$). Together, this suggests that the dataset we selected had estimates in the range of most previous research, thus speaking in favor of its representativeness.

It should be noted that in contrast to most previous research, the dataset we used in the present study has the substantial advantage that the tasks were designed to experimentally control for non-attentional-control processes (see Rey-Mermet et al., 2018). For example, the

¹ Rouder et al. (2019) also report (their Footnote 2) that in their simulations, the inverse-Wishart prior for the variance-covariance matrix showed “better performance” than an approach using an LKJ prior. However, as their normal model differed considerably from the Wiener diffusion model, it seems questionable whether this would also apply to the present model. This might be especially the case as the LKJ prior is generally preferred in the literature (e.g., McElreath, 2020).

impact of episodic memory and associative learning was controlled for by excluding immediate stimulus repetitions and by avoiding stimulus overlap across tasks. Additionally, demands on procedural working memory were minimized by presenting the stimulus-response mapping at the bottom of the screen. Therefore, it is possible that these different ways of controlling for non-attentional-control processes have reduced the contribution of these processes to shared variance between attentional-control measures. This could explain the slightly lower median correlation, and at the same time the higher median reliability observed in the present dataset compared to the values reported by von Bastian et al. (2020).

4.1.3 Control for non-attentional-control processes

A third concern may be whether our efforts to control for non-attentional control processes in the measures of attentional control might have removed specific attentional-control variance. In particular, using a design in which incongruent and congruent trials were intermixed within the same block and occurred with the same frequency could be considered as suboptimal. The reason is that under these conditions (i.e., a mixed-block design with equal frequency of both trial types), attentional control has been suggested to affect performance on congruent trials (e.g., Heitz & Engle, 2007; Kane & Engle, 2003; Unsworth et al., 2004). This was interpreted as the result of attentional control being overused. That is, in a design in which half of the trials are incongruent, it might be parsimonious to use attentional control on all trials, including those trials that do not require attentional control (i.e., congruent trials). However, for most tasks we used (see the Simon task for an exception), neutral trials were presented besides incongruent and congruent trials. Thus, incongruent trials – that is, trials in which attentional control is assumed to be required – were rare (i.e., 33%) in comparison to trials in which attentional control is not

assumed to be required (i.e., congruent and neutral trials). Therefore, in our design, it would not have been more parsimonious to use attentional control on all trials.

Similarly, one might argue that congruent trials are questionable as a baseline because they may involve a task conflict (e.g., Steinhauser & Hübner, 2009). For example, in the color Stroop task, when the word “red” is presented in the print color red, it may be unclear whether the decision to be performed is the one about the print color or the one about the word meaning (although both decisions would result in the same response). This uncertainty or task conflict may require attentional control, thus making congruent trials an inappropriate baseline. However, in the present study, we observed similar results when neutral trials – that is, trials in which there is no task conflict because only response-relevant features are presented – were used as baseline. Together, these results as well as the design used in the present dataset challenge the assumption that our congruent trials are not an appropriate baseline to remove non-attentional-control processes.

4.2 Boundary separation and non-decision time

Besides the drift rates, the results showed a coherent factor for the boundary separation in young adults, and – to some extent – for the non-decision time in older adults. As boundary separation is typically interpreted as a measure of response caution (Wagenmakers, 2009), such as when preferring accuracy over speed or speed over accuracy, establishing a coherent factor with that parameter suggests that young adults applied the same response style across several tasks (see also Hedge et al., 2020). This seems in line with the assumption of a consistent metacontrol state (Hommel & Wiers, 2017), which enables participants to implement a response style and to vary it if required. Apparently, older adults are less consistent across tasks in setting their degree of caution.

Finding a coherent factor in the non-decision time for older adults was more surprising because the non-decision time is defined as a parameter reflecting task-specific processes (Wagenmakers, 2009). One way to interpret this result is to consider that this factor was observed only in older adults. It is known that the non-decision time differentiates between old and very old adults (Ratcliff et al., 2010). Thus, it is possible that this non-decision time factor reflects age-related differences in sensory and motor processes in our sample of older adults. Another possibility is that in our hierarchical-Bayesian approach that does not consider contaminant responses (Ratcliff & Tuerlinckx, 2002), the individual differences in non-decision time reflect individual differences in the fastest RTs, and these have a similar ordering across tasks for older adults.

One may wonder why in addition to modeling separate drift rates for congruent and incongruent trials, we did not take a similar approach for the boundary separation and non-decision time. The main argument against this is again the statistical bias-variance trade-off (Yarkoni & Westfall, 2017). We want our diffusion model to be as complex as necessary to account for the main pattern in the data, but not any more complex. In line with this idea, psychometric applications of the diffusion model (e.g., van der Maas et al., 2011; Vandekerckhove, 2014) usually apply the simplest possible model variants, similar to our approach here. Furthermore, the previous studies investigating individual differences in attentional control with diffusion models also appeared to have only allowed the drift rate to differ across congruency conditions (Hedge et al., 2020; Weigard et al., 2020).

Nevertheless, we attempted to estimate models in which either all three parameters, or only the drift rate and the boundary separation, were allowed to differ across congruent and incongruent trials. However, these models failed to converge and only produced “divergent

transitions” in the MCMC sampling process, indicating a pathological likelihood surface or, more colloquially, a mis-specified model (e.g., Betancourt, 2017). Thus, at least given the available data, and with the goal in mind to estimate the variance-covariance matrix with a non-informative prior for the correlation matrix (which we believe is one of the unique strengths of our approach), the model in which only the drift rate is allowed to vary between congruent and incongruent trials is not only the psychometrically most appropriate model, it is also the only computationally feasible diffusion model.

5. Conclusion

There is always the possibility that a more sophisticated modelling approach coupled with even more data is eventually able to reliably uncover a latent variable reflecting general attention-control ability. However, given the considerable effort that has been invested in this question and the overall meager success rate (e.g., von Bastian et al., 2020), this possibility seems more theoretical than realistic. Taken together, the available evidence challenges the existence of attentional control as a psychometric construct. This calls into question the widely held assumption that people differ in a general ability to control attention. It also questions research in which a general attentional-control ability is used as a construct explaining group differences, as for example, in developmental psychology, aging research, bilingualism, and psychopathology.

Acknowledgements

This work was financially supported by the University Research Priority Program „Dynamics of Healthy Aging” of the University of Zurich, by a grant from the Velux Foundation to A. Rey-Mermet and K. Oberauer, and a grant from the Swiss National Science Foundation to H. Singmann [grant number: 100014_179121].

Author Contributions

All authors contributed to the study design. A. Rey-Mermet and H. Singmann performed the data analysis. All authors contributed to the interpretation of the results. A. Rey-Mermet and H. Singmann drafted the manuscript. K. Oberauer provided critical revisions. All authors approved the final version of the manuscript for submission.

References

- Ambrosi, S., Servant, M., Blaye, A., & Burle, B. (2019). Conflict processing in kindergarten children: New evidence from distribution analyses reveals the dynamics of incorrect response activation and suppression. *Journal of Experimental Child Psychology*, 177, 36–52. <https://doi.org/10.1016/j.jecp.2018.06.006>
- Barchard, K. A. (2012). Examining the reliability of interval level data using root mean square differences and concordance correlation coefficients. *Psychological Methods*, 17(2), 294–308. <https://doi.org/10.1037/a0023351>
- Betancourt, M. (2017). A Conceptual Introduction to Hamiltonian Monte Carlo. *ArXiv:1701.02434 [Stat]*. <http://arxiv.org/abs/1701.02434>
- Boehm, U., Annis, J., Frank, M., Hawkins, G., Heathcote, A., Kellen, D., Kryptos, A.-M., Lerche, V., Logan, G. D., Palmeri, T., Ravenzwaaij, D. van, Servant, M., Singmann, H., Starns, J., Voss, A., Wiecki, T., Matzke, D., & Wagenmakers, E.-J. (2018). Estimating Across-Trial Variability Parameters of the Diffusion Decision Model: Expert Advice and Recommendations. *Journal of Mathematical Psychology*, 87, 46–75. <https://doi.org/10.31234/osf.io/km28u>
- Brydges, C. R., Reid, C. L., Fox, A. M., & Anderson, M. (2012). A unitary executive function predicts intelligence in children. *Intelligence*, 40(5), 458–469. <https://doi.org/10.1016/j.intell.2012.05.006>
- Bürkner, P.-C. (2017). brms: An R Package for Bayesian Multilevel Models Using Stan. *Journal of Statistical Software*, 80(1), 1–28. <https://doi.org/10.18637/jss.v080.i01>

Carpenter, B., Gelman, A., Hoffman, M., Lee, D., Goodrich, B., Betancourt, M., Brubaker, M.

A., Guo, J., Li, P., & Riddell, A. (2017). Stan: A probabilistic programming language.

Journal of Statistical Software, 76(1), 1–32. <https://doi.org/10.18637/jss.v076.i01>

Chuderski, A. (2015). High intelligence prevents the negative impact of anxiety on working memory. *Cognition and Emotion*, 29(7), 1197–1209.

<https://doi.org/10.1080/02699931.2014.969683>

Chuderski, A., & Necka, E. (2012). The contribution of working memory to fluid reasoning:

Capacity, control, or both? *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 38(6), 1689–1710. <https://doi.org/10.1037/a0028465>

Chuderski, A., Taraday, M., Nęcka, E., & Smoleń, T. (2012). Storage capacity explains fluid intelligence but executive control does not. *Intelligence*, 40(3), 278–295.

<https://doi.org/10.1016/j.intell.2012.02.010>

De Simoni, C., & von Bastian, C. C. (2018). Working memory updating and binding training:

Bayesian evidence supporting the absence of transfer. *Journal of Experimental Psychology: General*, 147(6), 829–858. <https://doi.org/10.1037/xge0000453>

Donkin, C., & Brown, S. D. (2018). Response Times and Decision-Making. In J. T. Wixted & E.-J. Wagenmakers (Eds.), *Stevens' Handbook of Experimental Psychology and*

Cognitive Neuroscience (pp. 1–33). Wiley.

<https://doi.org/10.1002/9781119170174.epcn509>

Draheim, C., Mashburn, C. A., Martin, J. D., & Engle, R. W. (2019). Reaction time in

differential and developmental research: A review and commentary on the problems and alternatives. *Psychological Bulletin*, 145(5), 508–535.

<https://doi.org/10.1037/bul0000192>

- Draheim, C., Tsukahara, J. S., Martin, J., Mashburn, C., & Engle, R. W. (2020). A toolbox approach to improving the measurement of attention control. *Journal of Experimental Psychology: General*, Advance online publication. <https://doi.org/10.31234/osf.io/q985d>
- Dutilh, G., Annis, J., Brown, S. D., Cassey, P., Evans, N. J., Grasman, R. P. P. P., Hawkins, G. E., Heathcote, A., Holmes, W. R., Krypotos, A.-M., Kupitz, C. N., Leite, F. P., Lerche, V., Lin, Y.-S., Logan, G. D., Palmeri, T. J., Starns, J. J., Trueblood, J. S., van Maanen, L., ... Donkin, C. (2019). The Quality of Response Time Data Inference: A Blinded, Collaborative Assessment of the Validity of Cognitive Models. *Psychonomic Bulletin & Review*, 26(4), 1051–1069. <https://doi.org/10.3758/s13423-017-1417-2>
- Engle, R. W., Tuholski, S. W., Laughlin, J. E., & Conway, A. R. A. (1999). Working memory, short-term memory, and general fluid intelligence: A latent-variable approach. *Journal of Experimental Psychology: General*, 128(3), 309–331. <https://doi.org/10.1037/0096-3445.128.3.309>
- Gelman, A., & Hill, J. (2007). *Data analysis using regression and multilevel/hierarchical models*. Cambridge University Press.
- Gignac, G. E., & Kretzschmar, A. (2017). Evaluating dimensional distinctness with correlated-factor models: Limitations and suggestions. *Intelligence*, 62, 138–147. <https://doi.org/10.1016/j.intell.2017.04.001>
- Guye, S., & von Bastian, C. C. (2017). Working memory training in older adults: Bayesian evidence supporting the absence of transfer. *Psychology and Aging*, 32(8), 732–746. <https://doi.org/10.1037/pag0000206>

- Hedden, T., & Yoon, C. (2006). Individual differences in executive processing predict susceptibility to interference in verbal working memory. *Neuropsychology*, 20(5), 511–528. <https://doi.org/10.1037/0894-4105.20.5.511>
- Hedge, C., Powell, G., Bompas, A., & Sumner, P. (2020). *Strategy and processing speed eclipse individual differences in control ability in conflict tasks*. PsyArXiv. <https://doi.org/10.31234/osf.io/vgpxq>
- Hedge, C., Powell, G., & Sumner, P. (2018). The reliability paradox: Why robust cognitive tasks do not produce reliable individual differences. *Behavior Research Methods*, 50(3), 1166–1186. <https://doi.org/10.3758/s13428-017-0935-1>
- Hedge, C., Vivian-Griffiths, S., Powell, G., Bompas, A., & Sumner, P. (2019). Slow and steady? Strategic adjustments in response caution are moderately reliable and correlate across tasks. *Consciousness and Cognition*, 75, 102797. <https://doi.org/10.1016/j.concog.2019.102797>
- Heitz, R. P., & Engle, R. W. (2007). Focusing the spotlight: Individual differences in visual attention control. *Journal of Experimental Psychology: General*, 136(2), 217–240. <https://doi.org/10.1037/0096-3445.136.2.217>
- Hommel, B., & Wiers, R. W. (2017). Towards a Unitary Approach to Human Action Control. *Trends in Cognitive Sciences*, 21(12), 940–949. <https://doi.org/10.1016/j.tics.2017.09.009>
- Hu, L., & Bentler, P. M. (1998). Fit indices in covariance structure modeling: Sensitivity to underparameterized model misspecification. *Psychological Methods*, 3(4), 424.
- Hu, L., & Bentler, P. M. (1999). Cutoff criteria for fit indexes in covariance structure analysis: Conventional criteria versus new alternatives. *Structural Equation Modeling: A Multidisciplinary Journal*, 6(1), 1–55. <https://doi.org/10.1080/10705519909540118>

- Hübner, R., Steinhauser, M., & Lehle, C. (2010). A dual-stage two-phase model of selective attention. *Psychological Review*, 117(3), 759–784. <https://doi.org/10.1037/a0019471>
- Hull, R., Martin, R. C., Beier, M. E., Lane, D., & Hamilton, A. C. (2008). Executive function in older adults: A structural equation modeling approach. *Neuropsychology*, 22(4), 508–522. <https://doi.org/10.1037/0894-4105.22.4.508>
- Kane, M. J., & Engle, R. W. (2003). Working-memory capacity and the control of attention: The contributions of goal neglect, response competition, and task set to Stroop interference. *Journal of Experimental Psychology: General*, 132(1), 47–70. <https://doi.org/10.1037/0096-3445.132.1.47>
- Kane, M. J., Meier, M. E., Smeekens, B. A., Gross, G. M., Chun, C. A., Silvia, P. J., & Kwapil, T. R. (2016). Individual differences in the executive control of attention, memory, and thought, and their associations with schizotypy. *Journal of Experimental Psychology: General*, 145(8), 1017–1048. <https://doi.org/10.1037/xge0000184>
- Karr, J. E., Areshenkoff, C. N., Rast, P., Hofer, S. M., Iverson, G. L., & Garcia-Barrera, M. A. (2018). The unity and diversity of executive functions: A systematic review and re-analysis of latent variable studies. *Psychological Bulletin*. <https://doi.org/10.1037/bul0000160>
- Keye, D., Wilhelm, O., Oberauer, K., & Ravenzwaaij, D. van. (2009). Individual differences in conflict-monitoring: Testing means and covariance hypothesis about the Simon and the Eriksen Flanker task. *Psychological Research*, 73(6), 762–776. <https://doi.org/10.1007/s00426-008-0188-9>
- Klauer, K. C., Schmitz, F., Teige-Mocigemba, S., & Voss, A. (2010). Understanding the role of executive control in the Implicit Association Test: Why flexible people have small IAT

- effects. *The Quarterly Journal of Experimental Psychology*, 63(3), 595–619.
<https://doi.org/10.1080/17470210903076826>
- Krumm, S., Schmidt-Atzert, L., Buehner, M., Ziegler, M., Michalczyk, K., & Arrow, K. (2009). Storage and non-storage components of working memory predicting reasoning: A simultaneous examination of a wide range of ability factors. *Intelligence*, 37(4), 347–364.
<https://doi.org/10.1016/j.intell.2009.02.003>
- Liesefeld, H. R., & Janczyk, M. (2019). Combining speed and accuracy to control for speed-accuracy trade-offs(?). *Behavior Research Methods*, 51(1), 40–60.
<https://doi.org/10.3758/s13428-018-1076-x>
- McDonald, R. P. (1999). *Test theory: A unified treatment*. Lawrence Erlbaum Associates Publishers.
- McElreath, R. (2020). *Statistical rethinking: A Bayesian course with examples in R and Stan* (2nd ed.). Taylor and Francis, CRC Press.
- McVay, J. C., & Kane, M. J. (2012). Why does working memory capacity predict variation in reading comprehension? On the influence of mind wandering and executive attention. *Journal of Experimental Psychology: General*, 141(2), 302–320.
<https://doi.org/10.1037/a0025250>
- Miyake, A., & Friedman, N. P. (2012). The Nature and Organization of Individual Differences in Executive Functions: Four General Conclusions. *Current Directions in Psychological Science*, 21(1), 8–14. <https://doi.org/10.1177/0963721411429458>
- Miyake, A., Friedman, N. P., Emerson, M. J., Witzki, A. H., Howerter, A., & Wager, T. D. (2000). The unity and diversity of executive functions and their contributions to complex

- “Frontal Lobe” tasks: A latent variable analysis. *Cognitive Psychology*, 41(1), 49–100.
<https://doi.org/10.1006/cogp.1999.0734>
- Paap, K. R., & Greenberg, Z. I. (2013). There is no coherent evidence for a bilingual advantage in executive processing. *Cognitive Psychology*, 66(2), 232–258.
<https://doi.org/10.1016/j.cogpsych.2012.12.002>
- Pettigrew, C., & Martin, R. C. (2014). Cognitive declines in healthy aging: Evidence from multiple aspects of interference resolution. *Psychology and Aging*, 29(2), 187–204.
<https://doi.org/10.1037/a0036085>
- Plummer, M. (2003). JAGS: A Program for Analysis of Bayesian Graphical Models Using Gibbs Sampling. In K. Hornik, F. Leisch, & A. Zeileis, *Proceedings of the 3rd International Workshop on Distributed Statistical Computing (DSC 2003)*, March 20–22,.
- R Core Team. (2020). *R: A language and environment for statistical computing* (3.6.3) [Computer software]. <https://www.R-project.org/>
- Ratcliff, R., & McKoon, G. (2008). The Diffusion Decision Model: Theory and Data for Two-Choice Decision Tasks. *Neural Computation*, 20(4), 873–922.
<https://doi.org/10.1162/neco.2008.12-06-420>
- Ratcliff, R., Thapar, A., & McKoon, G. (2010). Individual Differences, Aging, and IQ in Two-Choice Tasks. *Cognitive Psychology*, 60(3), 127–157.
<https://doi.org/10.1016/j.cogpsych.2009.09.001>
- Ratcliff, R., & Tuerlinckx, F. (2002). Estimating parameters of the diffusion model: Approaches to dealing with contaminant reaction times and parameter variability. *Psychonomic Bulletin & Review*, 9(3), 438–481.

- Raykov, T. (2001). Estimation of congeneric scale reliability using covariance structure analysis with nonlinear constraints. *British Journal of Mathematical and Statistical Psychology*, 54(2), 315–323. <https://doi.org/10.1348/000711001159582>
- Rey-Mermet, A., Gade, M., & Oberauer, K. (2018). Should we stop thinking about inhibition? Searching for individual and age differences in inhibition ability. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 44(4), 501–526. <https://doi.org/10.1037/xlm0000450>
- Rey-Mermet, A., Gade, M., Souza, A. S., von Bastian, C. C., & Oberauer, K. (2019). Is executive control related to working memory capacity and fluid intelligence? *Journal of Experimental Psychology. General*, 148(8), 1335–1372. <https://doi.org/10.1037/xge0000593>
- Rey-Mermet, A., Singh, K. A., Gignac, G. E., Brydges, C. R., & Ecker, U. K. H. (2020). Interference control in working memory: Evidence for discriminant validity between removal and inhibition tasks. *PLOS ONE*, 15(12), e0243053. <https://doi.org/10.1371/journal.pone.0243053>
- Ridderinkhof, R. (2002). Micro- and macro-adjustments of task set: Activation and suppression in conflict tasks. *Psychological Research*, 66(4), 312–323. <https://doi.org/10.1007/s00426-002-0104-7>
- Rosseel, Y. (2012). lavaan: An R package for structural equation modeling. *Journal of Statistical Software*, 48(2), 1–36.
- Rouder, J., & Haaf, J. M. (2019). *A Psychometrics of Individual Differences in Experimental Tasks*. <https://doi.org/10.31234/osf.io/f3h2k>

- Rouder, J., Kumar, A., & Haaf, J. M. (2019). *Why Most Studies of Individual Differences With Inhibition Tasks Are Bound To Fail*. PsyArXiv. <https://doi.org/10.31234/osf.io/3cjr5>
- Schubert, A.-L., & Rey-Mermet, A. (2019). Does Process Overlap Theory Replace the Issues of General Intelligence with the Issues of Attentional Control? *Journal of Applied Research in Memory and Cognition*, 8(3), 277–283. <https://doi.org/10.1016/j.jarmac.2019.06.004>
- Shipstead, Z., Lindsey, D. R. B., Marshall, R. L., & Engle, R. W. (2014). The mechanisms of working memory capacity: Primary memory, secondary memory, and attention control. *Journal of Memory and Language*, 72, 116–141. <https://doi.org/10.1016/j.jml.2014.01.004>
- Steinhauser, M., & Hübner, R. (2009). Distinguishing Response Conflict and Task Conflict in the Stroop Task: Evidence from Ex-Gaussian Distribution Analysis. *Journal of Experimental Psychology: Human Perception and Performance*, 35(5), 1398–1412. <https://doi.org/10.1037/a0016467>
- Tabachnick, B. G., & Fidell, L. S. (2019). *Using Multivariate Statistics* (7th ed.). Pearson.
- Ulrich, R., Schröter, H., Leuthold, H., & Birngruber, T. (2015). Automatic and controlled stimulus processing in conflict tasks: Superimposed diffusion processes and delta functions. *Cognitive Psychology*, 78, 148–174. <https://doi.org/10.1016/j.cogpsych.2015.02.005>
- Unsworth, N., Miller, J. D., Lakey, C. E., Young, D. L., Meeks, J. T., Campbell, W. K., & Goodie, A. S. (2009). Exploring the Relations Among Executive Functions, Fluid Intelligence, and Personality. *Journal of Individual Differences*, 30(4), 194–200. <https://doi.org/10.1027/1614-0001.30.4.194>

- Unsworth, N., Schrock, J. C., & Engle, R. W. (2004). Working Memory Capacity and the Antisaccade Task: Individual Differences in Voluntary Saccade Control. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, 30(6), 1302–1321. <https://doi.org/10.1037/0278-7393.30.6.1302>
- Unsworth, N., & Spillers, G. J. (2010). Working memory capacity: Attention control, secondary memory, or both? A direct test of the dual-component model. *Journal of Memory and Language*, 62(4), 392–406. <https://doi.org/10.1016/j.jml.2010.02.001>
- van der Maas, H. L. J., Molenaar, D., Maris, G., Kievit, R. A., & Borsboom, D. (2011). Cognitive psychology meets psychometric theory: On the relation between process models for decision making and latent variable models for individual differences. *Psychological Review*, 118(2), 339–356. <https://doi.org/10.1037/a0022749>
- van der Sluis, S., de Jong, P. F., & van der Leij, A. (2007). Executive functioning in children, and its relations with reasoning, reading, and arithmetic. *Intelligence*, 35(5), 427–449. <https://doi.org/10.1016/j.intell.2006.09.001>
- van Ravenzwaaij, D., & Oberauer, K. (2009). How to use the diffusion model: Parameter recovery of three methods: EZ, fast-dm, and DMAT. *Journal of Mathematical Psychology*, 53(6), 463–473. <https://doi.org/10.1016/j.jmp.2009.09.004>
- Vandekerckhove, J. (2014). A cognitive latent variable model for the simultaneous analysis of behavioral and personality data. *Journal of Mathematical Psychology*, 60, 58–71. <https://doi.org/10.1016/j.jmp.2014.06.004>
- Vandekerckhove, J., Tuerlinckx, F., & Lee, M. D. (2011). Hierarchical diffusion models for two-choice response times. *Psychological Methods*, 16(1), 44–62. <https://doi.org/10.1037/a0021765>

- Vandierendonck, A. (2017). A comparison of methods to combine speed and accuracy measures of performance: A rejoinder on the binning procedure. *Behavior Research Methods*, 49(2), 653–673. <https://doi.org/10.3758/s13428-016-0721-5>
- Verguts, T., & Notebaert, W. (2009). Adaptation by binding: A learning account of cognitive control. *Trends in Cognitive Sciences*, 13(6), 252–257. <https://doi.org/10.1016/j.tics.2009.02.007>
- von Bastian, C. C., Blais, C., Brewer, G. A., Gyurkovics, M., Hedge, C., Kałamała, P., Meier, M. E., Oberauer, K., Rey-Mermet, A., Rouder, J. N., Souza, A. S., Bartsch, L. M., Conway, A. R. A., Draheim, C., Engle, R. W., Friedman, N. P., Frischkorn, G. T., Gustavson, D. E., Koch, I., ... Wiemers, E. A. (2020). *Advancing the understanding of individual differences in attentional control: Theoretical, methodological, and analytical considerations*. <https://doi.org/10.31234/osf.io/x3b9k>
- von Bastian, C. C., Souza, A. S., & Gade, M. (2016). No evidence for bilingual cognitive advantages: A test of four hypotheses. *Journal of Experimental Psychology: General*, 145(2), 246–258. <https://doi.org/10.1037/xge0000120>
- Wagenmakers, E.-J. (2009). Methodological and empirical developments for the Ratcliff diffusion model of response times and accuracy. *European Journal of Cognitive Psychology*, 21(5), 641–671.
- Wagenmakers, E.-J., Van Der Maas, H. L. J., & Grasman, R. P. P. P. (2007). An EZ-diffusion model for response time and accuracy. *Psychonomic Bulletin & Review*, 14(1), 3–22. <https://doi.org/10.3758/BF03194023>

- Weigard, A. S., Clark, D. A., & Sripada, C. (2020). *Cognitive efficiency beats subtraction-based metrics as a reliable individual difference dimension relevant to self-control*. PsyArXiv. <https://doi.org/10.31234/osf.io/qp2ua>
- White, C. N., Servant, M., & Logan, G. D. (2018). Testing the validity of conflict drift-diffusion models for use in estimating cognitive processes: A parameter-recovery study. *Psychonomic Bulletin & Review*, 25(1), 286–301. <https://doi.org/10.3758/s13423-017-1271-2>
- Wiecki, T. V., Sofer, I., & Frank, M. J. (2013). HDDM: Hierarchical Bayesian estimation of the Drift-Diffusion Model in Python. *Frontiers in Neuroinformatics*, 7(14), 1–10. <https://doi.org/10.3389/fninf.2013.00014>
- Yarkoni, T., & Westfall, J. (2017). Choosing Prediction Over Explanation in Psychology: Lessons From Machine Learning. *Perspectives on Psychological Science*, 12(6), 1100–1122. <https://doi.org/10.1177/1745691617693393>

Table 1

Tasks Analyzed in the Present Study.

Task ^a	Decision	Sample size	
		Young adults	Older adults
Color Stroop	Color words (i.e., the words “red”, “blue”, “green”, or “yellow”) or row of four X’s were printed either in red, blue, green, or yellow. Color words were either incongruent to the meaning of the words (e.g., the word “green” printed in green), congruent (e.g., the word “red” printed in red), or neutral (e.g., XXXX printed in green). Participants were instructed to indicate the color of color words while ignoring the meaning of the words.	116	125
Number Stroop	One to four digits or symbols were displayed centrally. The number of digits could be incongruent to the numerical value (e.g., 111), congruent (e.g., 333), or neutral (e.g., \$\$\$). Participants were instructed to count the number of characters while ignoring the numerical value of digit characters.	120	128
Arrow flanker	Five arrows were presented centrally. The direction of the central arrow was either incongruent to the four flanking arrows (e.g., ←←→←←), congruent (e.g., →→→→→), or neutral (e.g., —→—). Participants were asked to respond to the direction of the central arrow (left or right) while ignoring the four flanking characters.	117	138
Letter flanker	Five letters were presented centrally. Participants were asked to decide whether the central letter was a vowel (E or U) or consonant (S or H) while ignoring the four flanking characters. The response to the central letter was either incongruent to the response of the flanking letters (e.g., SSESS), congruent (UUEUU or EEEEE), or neutral (e.g., ##E##).	118	140
Simon	A square or a circle was presented either on the left or right side of the screen or centrally. Participants were asked to indicate the shape (square vs. circle) by using manual responses (a left or a right response key) while ignoring the location of the shape on the screen. The location of the shape on the screen was either incongruent to the location of the response key (e.g., a square presented on the right side but requiring to press the left key), congruent (e.g., a square	119	140

Task ^a	Decision	Sample size	
		Young adults	Older adults
Global-local ^b	presented on the left side and requiring to press the left key), or neutral (e.g., a square presented centrally but requiring to press the left key).		
	Element letters forming a large letter (e.g., a large “Y” built from small “V”s) were presented. In the global version of the task, participants were asked to identify the global letter (i.e., the large “Y”) while suppressing the response induced by the local elements. In the local version of the task, participants were asked to identify the local elements (i.e., the small “V”s) while suppressing the response induced by the global letter. The local elements were either incongruent to the global letter (e.g., a large “Y” built from small “V”s), congruent (e.g., a large “V” built from small “V”s), or neutral (e.g., a large “Z” built from small “V”s).	118	131
Positive compatibility	A prime (e.g., “<<”, “>>”, or “==”) was first presented. To prevent conscious identification, the prime was followed by a mask (which consisted of the overlap of all prime exemplars). A target arrow (i.e., “<<” or “>>”) was then displayed either above or below the mask. Participants were asked to indicate the direction of the target arrow (i.e., left or right) while ignoring the information induced by the prime. The prime and the arrow were either incongruent (e.g., the prime “<<” followed by the target “>>”), congruent (e.g., the prime “<<” followed by the target “<<”), or neutral (e.g., the prime “==” followed by the target “>>”).	114	130
Negative compatibility	This task was similar to the positive compatibility task, except that a delay of 150 ms was introduced between the prime and target. During the delay, the prime is assumed to induce a response, which is then automatically suppressed. However, this response needs to be reactivated when the target and the prime require the same response (i.e., in congruent trials). The negative compatibility effect reflects the time cost of re-activating the primed response after it has been suppressed. Congruent trials are thus the trials assumed to induce attentional control, while incongruent trials are baseline trials.	118	132

Note. In the present tasks (except the negative-compatibility task), there were two types of baseline trials: (1) congruent trials in which there was no conflict between stimulus or response features, and (2) neutral trials in which there was only one response-relevant

feature. For the negative-compatibility task, the two types of baseline trials are incongruent and neutral trials.

^aRey-Mermet et al. (2018) included three additional tasks (i.e., an antisaccade task, a stop-signal task and a task assessing n-2 repetition cost). These tasks were not analyzed in the present study because no diffusion modeling can be applied to them.

^bSimilar to Rey-Mermet et al. (2018), we only analyzed the local version of this task because substantial difference scores are predominantly observed in this version of the task.

Table 2

Posterior Mean Correlation Coefficients and 95% Credibility Intervals for the Drift-Rate Difference Estimate of the Diffusion Model with Difference Coding (Incongruent vs. Congruent Trials).

Age Group	Measure	Color Stroop	Number Stroop	Arrow flanker	Letter flanker	Simon	Positive compatibility	Negative compatibility
Young adults	Color Stroop	-						
	Number Stroop	-.02 [-.30, .26]	-					
	Arrow flanker	-.14 [-.42, .17]	.03 [-.23, .30]	-				
	Letter flanker	-.09 [-.37, .21]	.10 [-.18, .37]	.26 [-.01, .51]	-			
	Simon	-.13 [-.40, .17]	.04 [-.18, .27]	.08 [-.15, .31]	.21 [-.04, .45]	-		
	Local	.11 [-.21, .39]	.03 [-.25, .32]	.06 [-.22, .34]	.09 [-.21, .36]	.19 [-.08, .44]	-	
	Pos. comp.	-.02 [-.31, .27]	.03 [-.22, .28]	.00 [-.25, .25]	.16 [-.11, .41]	.11 [-.11, .33]	.07 [-.21, .35]	-
	Neg. comp.	-.04 [-.32, .23]	.16 [-.10, .41]	.06 [-.20, .33]	.12 [-.16, .37]	.02 [-.21, .25]	.04 [-.23, .30]	.09 [-.15, .35]
Older adults	Color Stroop	-						
	Number Stroop	.11 [-.12, .33]	-					
	Arrow flanker	-.06 [-.25, .14]	-.07 [-.29, .17]	-				
	Letter flanker	-.06 [-.28, .15]	.09 [-.18, .33]	.22 [-.01, .44]	-			
	Simon	-.09 [-.28, .09]	.07 [-.18, .32]	.13 [-.08, .33]	.21 [-.03, .42]	-		
	Local	.13 [-.11, .35]	.05 [-.22, .32]	.07 [-.18, .31]	.08 [-.18, .33]	.05 [-.20, .29]	-	
	Pos. comp.	.14 [-.10, .36]	-.01 [-.26, .24]	.27 [.03, .50]	.23 [-.01, .46]	.04 [-.20, .29]	-.06 [-.31, .19]	-
	Neg. comp.	-.14 [-.38, .12]	-.03 [-.31, .25]	-.12 [-.38, .15]	-.08 [-.35, .20]	-.09 [-.33, .18]	.02 [-.26, .30]	-.25 [-.48, .03]

Note. Values in [] are the lower and upper limit of the 95% Bayesian credibility interval (CI). For values printed in bold, the 95% CI

excludes 0.

Table 3

Posterior Mean Correlation Coefficients and 95% Credibility Intervals for the Drift-Rates Estimates With Condition Coding.

[illegible]

Age Group	Measures	Trial type	Color Stroop		Number Stroop		Arrow flanker		Letter flanker		Simon		Local		Pos. comp.	Neg. comp.
			Inc.	Con.	Inc.	Con.	Con.	Inc.	Inc.	Con.	Inc.	Con.	Inc.	Con.	Con.	Inc.
	Arrow flanker	Con.	.09	.25	.11	.17										
		Inc.	-.05, .21	.13, .38	-.02, .25	.03, .30	-									
	Letter flanker	Con.	.13	.27	.18	.20	.80									
		Inc.	-.01, .26	.14, .38	.05, .30	.07, .33	.73, .86	-								
	Simon	Con.	.17	.15	.26	.24	.26	.26								
		Inc.	.04, .30	.02, .29	.13, .39	.11, .37	.13, .38	.13, .40	-							
	Local	Con.	.12	.08	.27	.26	.21	.28	.81							
		Inc.	-.00, .25	-.06, .21	.14, .40	.12, .38	.08, .33	.15, .41	.73, .88	-						
	Pos. comp.	Con.	.01	.10	.20	.28	.26	.29	.41	.38						
		Inc.	-.12, .13	-.03, .22	.07, .32	.16, .41	.12, .38	.16, .41	.30, .53	.25, .50	-					
	Neg. comp.	Con.	-.01	.01	.11	.19	.15	.22	.30	.36	.82					
		Inc.	-.15, .12	-.12, .14	-.03, .24	.06, .33	.02, .28	.09, .35	.17, .42	.22, .48	.74, .88	-				
	Pos. comp.	Con.	.05	.16	.05	.02	.11	.18	.12	.10	.14	.10				
		Inc.	-.10, .19	.02, .30	-.09, .19	-.11, .16	-.02, .24	.05, .31	-.02, .25	-.05, .23	.02, .26	-.03, .23	-			
	Neg. comp.	Con.	.04	.21	.07	.04	.13	.21	.11	.12	.20	.16	.77			
		Inc.	-.11, .18	.07, .35	-.07, .20	-.09, .17	-.01, .27	.08, .33	-.03, .25	-.02, .25	.07, .33	.03, .29	.67, .85	-		
	Pos. comp.	Con.	.13	.04	.20	.23	.19	.17	.17	.16	.14	.07	.05	.06		
		Inc.	-.01, .26	-.10, .18	.07, .34	.09, .37	.06, .32	.04, .30	.03, .31	.01, .30	.01, .28	-.07, .20	-.09, .19	-.09, .21	-	
	Neg. comp.	Con.	.06	.05	.20	.24	.16	.25	.16	.19	.23	.15	.18	.11	.73	
		Inc.	-.08, .20	-.09, .18	.06, .34	.10, .37	.03, .30	.11, .37	.02, .30	.05, .33	.09, .35	.01, .28	.04, .31	-.03, .25	.62, .82	-
	Pos. comp.	Con.	.17	.10	.26	.23	.24	.27	.23	.18	.20	.09	.05	.16	.44	.43
		Inc.	.04, .29	-.02, .23	.14, .38	.11, .36	.12, .36	.14, .38	.10, .36	.06, .30	.08, .32	-.04, .22	-.08, .18	.03, .29	.31, .56	.29, .55
	Neg. comp.	Con.	.16	.16	.20	.23	.18	.25	.18	.16	.18	.11	.07	.16	.39	.48
		Inc.	.03, .29	.04, .29	.07, .32	.10, .35	.06, .29	.13, .37	.04, .30	.04, .29	.06, .30	-.01, .23	-.06, .20	.02, .29	.25, .52	.36, .60

Note. Inc. = Incongruent; Con. = Congruent; Neg. comp. = Negative compatibility; Pos. comp. = Positive compatibility. In each cell,

the value in the first line is the mean, the values in the second line are the lower and upper bounds of the Bayesian 95% CI. For values printed in bold, the 95% CI excludes 0.

Table 4

Posterior Mean Correlation Coefficients and 95% Credibility Intervals for the Boundary Separation of the Diffusion Model with Condition Coding.

Age Group	Measure	Color Stroop	Number Stroop	Arrow flanker	Letter flanker	Simon	Positive compatibility	Negative compatibility
Young adults	Color Stroop	-						
	Number Stroop	.25 [.10, .40]	-					
	Arrow flanker	.31 [.17, .45]	.22 [.08, .38]	-				
	Letter flanker	.21 [.06, .36]	.32 [.16, .47]	.31 [.16, .45]	-			
	Simon	.30 [.14, .43]	.13 [-.03, .29]	.23 [.08, .37]	.40 [.26, .54]	-		
	Local	.43 [.27, .56]	.20 [.05, .36]	.36 [.21, .49]	.32 [.15, .46]	.29 [.13, .44]	-	
	Pos. comp.	.06 [-.09, .22]	.29 [.13, .44]	.12 [-.04, .28]	.35 [.19, .50]	.22 [.06, .37]	.16 [.00, .32]	-
	Neg. comp.	.09 [-.06, .24]	.19 [.03, .35]	.13 [-.02, .28]	.15 [.00, .30]	.11 [-.04, .27]	.15 [-.01, .30]	.27 [.11, .43]
Older adults	Color Stroop	-						
	Number Stroop	.36 [.21, .49]	-					
	Arrow flanker	.09 [-.06, .23]	-.06 [-.21, .09]	-				
	Letter flanker	.21 [.06, .34]	.08 [-.07, .22]	.26 [.11, .40]	-			
	Simon	.01 [-.13, .15]	-.03 [-.18, .12]	.25 [.10, .39]	.37 [.23, .49]	-		
	Local	.12 [-.03, .26]	.09 [-.07, .24]	.18 [.01, .34]	.36 [.20, .50]	.29 [.13, .44]	-	
	Pos. comp.	.06 [-.09, .21]	.02 [-.14, .18]	.14 [-.02, .28]	.21 [.06, .35]	.22 [.08, .36]	.19 [.02, .36]	-
	Neg. comp.	.20 [.05, .34]	.12 [-.04, .27]	.11 [-.03, .26]	.16 [.02, .31]	.02 [-.13, .17]	.06 [-.09, .22]	.20 [.05, .35]

Note. Values in [] are the lower and upper limit of the 95% Bayesian credibility interval (CI). Boundary separation was estimated

averaged across all trial types (i.e., neutral, incongruent, and congruent). For values printed in bold, the 95% CI excludes 0.

Table 5

Posterior Mean Correlation Coefficients and 95% Credibility Intervals for the Non-Decision Time of the Diffusion Model with Condition Coding.

Age Group	Measure	Color Stroop	Number Stroop	Arrow flanker	Letter flanker	Simon	Positive compatibility	Negative compatibility
Young adults	Color Stroop	-						
	Number Stroop	.19 [.02, .33]	-					
	Arrow flanker	.19 [.03, .34]	.08 [-.08, .22]	-				
	Letter flanker	.06 [-.09, .22]	.40 [.26, .53]	.20 [.03, .34]	-			
	Simon	.22 [.06, .37]	.16 [-.00, .32]	.17 [.01, .32]	.34 [.19, .47]	-		
	Local	.17 [.00, .32]	.10 [-.05, .26]	.14 [-.02, .28]	.21 [.05, .35]	.23 [.08, .37]	-	
	Pos. comp.	.02 [-.14, .18]	.10 [-.05, .25]	.19 [.05, .34]	.16 [-.01, .30]	.15 [.00, .30]	-.01 [-.17, .14]	-
	Neg. comp.	.06 [-.09, .21]	.09 [-.06, .25]	.22 [.07, .36]	.15 [-.00, .30]	.10 [-.07, .26]	-.00 [-.17, .15]	.35 [.21, .48]
Older adults	Color Stroop	-						
	Number Stroop	.26 [.11, .40]	-					
	Arrow flanker	.13 [-.03, .28]	.11 [-.04, .26]	-				
	Letter flanker	.21 [.05, .36]	.25 [.09, .39]	.33 [.19, .46]	-			
	Simon	.14 [-.01, .29]	.22 [.07, .37]	.34 [.20, .47]	.41 [.26, .53]	-		
	Local	.10 [-.05, .26]	.07 [-.09, .22]	.26 [.10, .40]	.31 [.16, .45]	.18 [.03, .32]	-	
	Pos. comp.	.02 [-.13, .17]	.06 [-.10, .21]	.17 [.02, .32]	.14 [-.02, .28]	.19 [.04, .33]	.10 [-.05, .25]	-
	Neg. comp.	.16 [-.00, .31]	-.01 [-.18, .15]	.11 [-.04, .25]	.13 [-.02, .27]	.14 [-.01, .28]	.05 [-.10, .20]	.35 [.21, .48]

Note. Values in [] are the lower and upper limit of the 95% Bayesian credibility interval (CI). Non-decision time was estimated

averaged across all trial types (i.e., neutral, incongruent, and congruent). For values printed in bold, the 95% CI excludes 0.

Figure 1. Model fit of diffusion models for young adults comparing observed (x -axis) with predicted statistics (y -axis). First four rows show the fit of the difference-coding parametrization

and the last four rows show the fit of the condition-coding parametrization. Each column shows the statistics for one of the eight tasks used in the SEM. The first, second, fifth, and sixth row show overall mean RT (for correct responses) and accuracy across conditions. The third and seventh row, “Effect (RT)”, and the fourth and eighth row, “Effect (Acc)”, show the congruency effects for RT and accuracy, respectively. Data points are participants and are plotted semi-transparently so that overlapping points appear darker. The predicted statistics show the mean of 500 samples from the posterior predictive distribution (i.e., synthetic data of the same size as the observed data generated from the fitted model). The value in the top left corner of each panel is the concordance correlation coefficient (CCC), which is 1 in case of perfect absolute agreement, for the data points shown. The three values in the lower left corner represent the 2.5% quantile, the median, and the 97.5% quantile of the CCCs when calculated individually for each sample of the posterior predictive distribution.

Figure 2. Young adults. (A) Drift rate with difference coding: One-factor model in which all drift-rate differences between incongruent and congruent trials loaded on a single latent variable (Model 1). (B) Drift rate with condition coding: Bifactor model in which drift rates from the incongruent and congruent trials of all tasks was forced to load on a general factor, and drift rates from the conditions reflecting high attentional control (i.e., the congruent trials of the negative-compatibility task and the incongruent trials of the remaining tasks) were forced to load on an attentional-control factor (Model 2). In this model, factor loadings were constrained to be

positive, and error variances from the measures of the same task were allowed to correlate. (C)

Boundary separation: One-factor model in which the boundary-separation parameters loaded on a single latent variable (Model 1). For Model 1, the numbers next to the straight, single-headed arrows are the standardized factor loadings (interpretable as standardized regression coefficients). For Model 2 (bifactor model), for the sake of clarity, these factor loadings are aligned to each measure (i.e., next to the measures for the general factor and next to the factors for the attentional-control factor). For both model types, the outward numbers adjacent to each measure are the error variances, attributable to idiosyncratic task requirements and measurement error. The numbers adjacent to the curved, double-headed arrows next to each measure in Model 2 (bifactor model) are the correlations between the error variances. For all values, boldface type indicates $p < .05$. Neg. comp. = Negative compatibility; Pos. comp. = positive compatibility; Num. Stroop = Number Stroop; inc. = incongruent; con. = congruent.

Figure 3. Model fit of diffusion models for old adults comparing observed (x -axis) with predicted statistics (y -axis). See Figure 1 note for details.

Figure 4. Older adults. (A) Drift rate with condition coding: Bifactor model in which drift rates from the incongruent and congruent trials of all tasks was forced to load on a general factor, and drift rates from the conditions reflecting high attentional control (i.e., the congruent trials of the negative-compatibility task and the incongruent trials of the remaining tasks) were forced to load on an attentional-control factor (Model 2). In this model, factor loadings were constrained to be positive, and error variances from the measures of the same task were allowed to correlate. (C) Non-decision time: One-factor model in which the boundary-separation parameters loaded on a single latent variable (Model 1). See Figure 2 note for details.

A **B**

Figure 5. Model fitting outcomes with each of the 500 covariance matrices sampled from the full posterior distribution. (A) Overview of the goodness-of-fit statistics for Model 1 and Model 2 in young and older age groups for the drift rate with difference coding, for the drift rate with condition coding, for the boundary separation and for the non-decision time. A model fit was considered as good if the Bentler's comparative fit index (CFI) was larger than .95 and the standardized root-mean-square residual (SRMR) was smaller than .08. A model fit was considered as acceptable if the CFI ranged from .90 to .95 and the SRMR was smaller than .08. Otherwise, the model fit was considered as bad. For the sake of completeness, we separately reported the cases in which the model fit was good or acceptable but the model computation resulted in some warnings (e.g., some estimated variances were negative) and the cases in which the model computation did not converge. The results for the model computations with the covariance matrix from the mean posterior distribution are presented in red. (B) Omega coefficients of factor reliability for single-factor models (Model 1), and hierarchical omegas for bifactor models (Model 2) for which the fit statistics was acceptable to good. Omegas for the model fits with the covariance matrix from the mean posterior distribution are presented in red. Please note that there is no distribution for the single-factor model in young adults because none of the models provided an acceptable or a good fit to the data (see Figure 5a).