How can we achieve a good measurement of attentional control?

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Abstract

Attentional control refers to the basic ability necessary to maintain a goal and goal-relevant features in the face of distraction. Many researchers conceptualize attentional control as a psychometric construct that can explain individual differences across experimental tasks and situations. However, recent research has challenged this idea by observing difficulties with establishing reliable and valid measures of attentional control. The goal of the present theoretical review is threefold: (1) to review and discuss the current methodological challenges that contribute to these difficulties, (2) to move beyond these methodological challenges to highlight theoretical challenges, and (3) to propose an approach combining formal modeling with experimental and correlational methods to address these methodological and theoretical challenges.

Keywords: Attentional control, measurement, validity, reliability, formal modeling

How can we achieve a good measurement of attentional control?

What is attentional control? Executive function? Cognitive control? Inhibition? Researchers in psychology and neurosciences have explored these questions for more than a century. The concept that these different terms refer to is used as a key construct to explain group or interindividual differences in diverse areas, such as intelligence (Draheim et al., 2021), bilingualism (Paap, 2019), addiction (Verdejo-Garcia et al., 2019), or psychopathology (Lipszyc & Schachar, 2010; Wright et al., 2014). It has also been used to predict behaviors and disorders, such as impulsive behaviors (Sharma et al., 2014) or neurodevelopmental and mood disorders in later childhood (Hawkey et al., 2018). However, as diverse as these research areas are, so are the terminologies and definitions (see, e.g., Jurado & Rosselli, 2007; Oberauer, 2024). For example, in some conceptualizations, attentional control has been defined as the processes guiding attention ("control of attention"; e.g., Baddeley, 1986; Miyake et al., 2000; Norman & Shallice, 1980). In other conceptualizations, it has been framed as how cognitive processes are controlled in contrast to automatic processes (e.g., Draheim et al., 2021; Shiffrin & Schneider, 1977). A few years ago, a large group of researchers proposed a consensus definition that conceptualizes attentional control as the basic ability necessary to maintain a goal and goal-relevant features in the face of distraction (von Bastian et al., 2020). This consensus definition represents a first step towards a shared understanding of the concept of attentional control. Nevertheless, despite having a consensus on this broad verbal definition, no consensus has emerged on how we should measure attentional control. The present study aims to address this gap.

Each of these terms – attentional control, executive function, cognitive control, and inhibition – is related to a different history or research field. In the present article, we followed von Bastian et al. (2020) by opting for the term attentional control. The reason is that this term is least related to one specific theory or research area. Whereas the terminology has been diverse, the construct has typically been measured with a similar set of tasks (see Figure 1 for an overview of the most common tasks). For example, the most commonly used task is the color Stroop task (e.g., MacLeod, 1991; Stroop, 1935). In this task, participants are asked to indicate the print color of a color word, while ignoring the meaning of the word. Attentional control is typically measured as a difference in performance between two conditions. In the incongruent condition, attentional control is assumed to be required to resolve a response conflict between the print color (i.e., the goal-relevant information) and the word meaning (i.e., the distracting information). In the congruent condition, no or less attentional control is required because both goal-relevant and distracting information lead to the same response.

To perform the Stroop task or any attentional-control task, numerous underlying processes or abilities are required from participants. For example, to provide a response, participants are required to encode and process information and to execute a motor response (e.g., saying the color or pressing a key). In addition to these basic processes or abilities, participants may apply strategies or meta-cognitive processes or abilities (e.g., how confident people want to be in their response). Thus, each attentional–control task captures a variety of processes or abilities. This so-called task-impurity problem has long been known and discussed (e.g., Burgess, 1997; Miyake et al., 2000). From an experimental perspective, this problem has been addressed by subtracting the congruent condition with minimal attentional-control demands from the incongruent condition with strong attentional-control demands.

At the group level, results are very robust. For example, in the Stroop task, nearly (if not) all studies showed that participants are on average slower and more error prone in the incongruent condition than in the congruent condition (e.g., Ebersole et al., 2016; MacLeod, 1991). Moreover, these results are observed across a range of similar tasks (e.g., Simon and flanker tasks, see Figure 1). This similarity in the tasks and in the findings has led some authors to conceptualize attentional control as a unitary construct, implying that all attentional-control tasks are measuring the same construct (e.g., Burgoyne et al., 2023; Draheim et al., 2021). There exist, however, other conceptualizations. For example, attentional control has been conceptualized as a set of diverse and related constructs (Miyake et al., 2000). In this conceptualization, the constructs that were put forward were the ability to suppress prepotent responses and ignore irrelevant information (inhibition), the ability to shift the attention to other tasks or perceptual dimensions (shifting), and the ability to replace the currently stored information in working memory by new information (updating). In another conceptualization, attentional control has been assumed as a general construct (common executive functions) with two more specific subconstructs (i.e., switching and updating; Miyake & Friedman, 2012). In this case, there was no specific subconstruct of inhibition because variance in the inhibition tasks was assumed to be explained entirely by the general construct. Finally, attentional control has also been conceptualized as a set of diverse but independent or loosely related constructs (Conway et al., 2021; Egner, 2008).

All these conceptualizations assume that the construct of attentional control – and/or its subcomponents – can be validly assessed using various attentional-control tasks. Most research has tested this assumption by testing the amount of shared variance across measures using correlations and structural equation modeling (SEM). The expectation behind this line of research is that a person who shows good attentional-control ability in one task should tend to show good attentional-control ability in other tasks. Thus, different tasks of attentional control should be positively correlated in such a way that their shared variance can be described by one factor. Practically, this means that all, or most, measures of attentional control should have non-negligible loadings on a factor representing attentional control. This does not require that the correlation matrix between all attentional-control measures is well-fitted by a single-factor model. The attentional-control construct could have a hierarchical structure, and such a hierarchical structure has indeed been proposed (Miyake et al., 2000). If that is the case, then the common variance of attentional-control measures is represented by a higher-order factor, as in the original work of Miyake et al. (2001), or as a general factor in a bi-factor model, as in their subsequent work (Miyake & Friedman, 2012).

At first glance, this line of research was successful at establishing shared variance among many measures of attentional control (e.g., Chuderski, 2014; Engle et al., 1999; Kane et al., 2004; Martin et al., 2019; McVay & Kane, 2012; Miyake et al., 2000; Redick et al., 2016; Unsworth et al., 2010, 2014, 2021; Unsworth & Spillers, 2010). Furthermore, consistent evidence has been reported that the typical measures of switching – switch costs (see Figure 1) – correlate substantially among several variants of the task-switch paradigm, and this shared variance can be represented by a single factor (Frischkorn & von Bastian, 2021; Hartung et al., 2020; von Bastian & Druey, 2017).

However, empirical evidence is overall less clear than this line of research has suggested. First, the models that provided a good fit to the data and established a factor of attentional control were not replicable, possibly due to overfitting in underpowered samples (Karr et al., 2018). Second, although task switching seems well-established as a factor, there are methodological and theoretical challenges with updating and inhibition. Regarding updating, measures usually correlate highly with each other. That finding, however, is ambiguous because most studies assess updating as performance in working-memory tasks that include an updating demand. These performance scores represent a mixture of the ability to maintain information in working memory and the ability to update that information. Unless we define maintenance in working memory as a form of attentional control, only the latter – the ability to update working memory – can be considered a measure of attentional control. Few studies have attempted to separate maintenance and updating abilities. Frischkorn et al. (2022) contrasted updating and no-updating versions of the same working-memory tasks and found that only a small portion of the variance in performance shared among the updating tasks reflects updating ability – barely enough to identify a factor. A more optimistic picture arises from work with an updating paradigm designed to isolate one component of the updating process, the removal of outdated information (Ecker et al., 2014). The speed of removal is correlated across versions of that paradigm, and their shared variance is well described by a factor (Frischkorn & von Bastian, 2021; Rey-Mermet et al., 2020; Singh et al., 2018).

Regarding inhibition, the picture is even worse. Although inhibition has been considered as the basic ability necessary in all tasks (Miyake & Friedman, 2012), inhibition has most stubbornly resisted efforts to demonstrate consistent evidence for shared variance across the measures (see Rey-Mermet et al., 2019; Schubert & Rey-Mermet, 2019; von Bastian et al., 2020, for overviews). First, correlations between different inhibition measures are low and often close to zero (e.g., Erb et al., 2023; Paap & Greenberg, 2013). Second, when a SEM approach was used, some studies reported that the inhibition measures failed to load on a factor of attentional control (e.g., De Simoni & von Bastian, 2018; Krumm et al., 2009). In other studies, the inhibition measures were merged with other measures to establish a factor (e.g., Brydges et al., 2012; Hedden & Yoon, 2006; Klauer et al., 2010; van der Sluis et al., 2007). Third, when a factor was established and the model had a good fit to the data, the factors were often not coherent (e.g., Chuderski, 2015; Chuderski et al., 2012; Gärtner & Strobel, 2021; Hartung et al., 2020; Hull et al., 2008; Kane et al., 2016; Pettigrew & Martin, 2014; Rey-Mermet et al., 2018, 2019, 2020, 2025; Shipstead et al., 2014; Unsworth et al., 2009). That is, error variances were high, and most factor loadings were low, with only one or two loadings being high. Thus, these factors did not represent common variance across the measures, thus undermining their interpretation as an attentional-control construct generalizing across the measures.

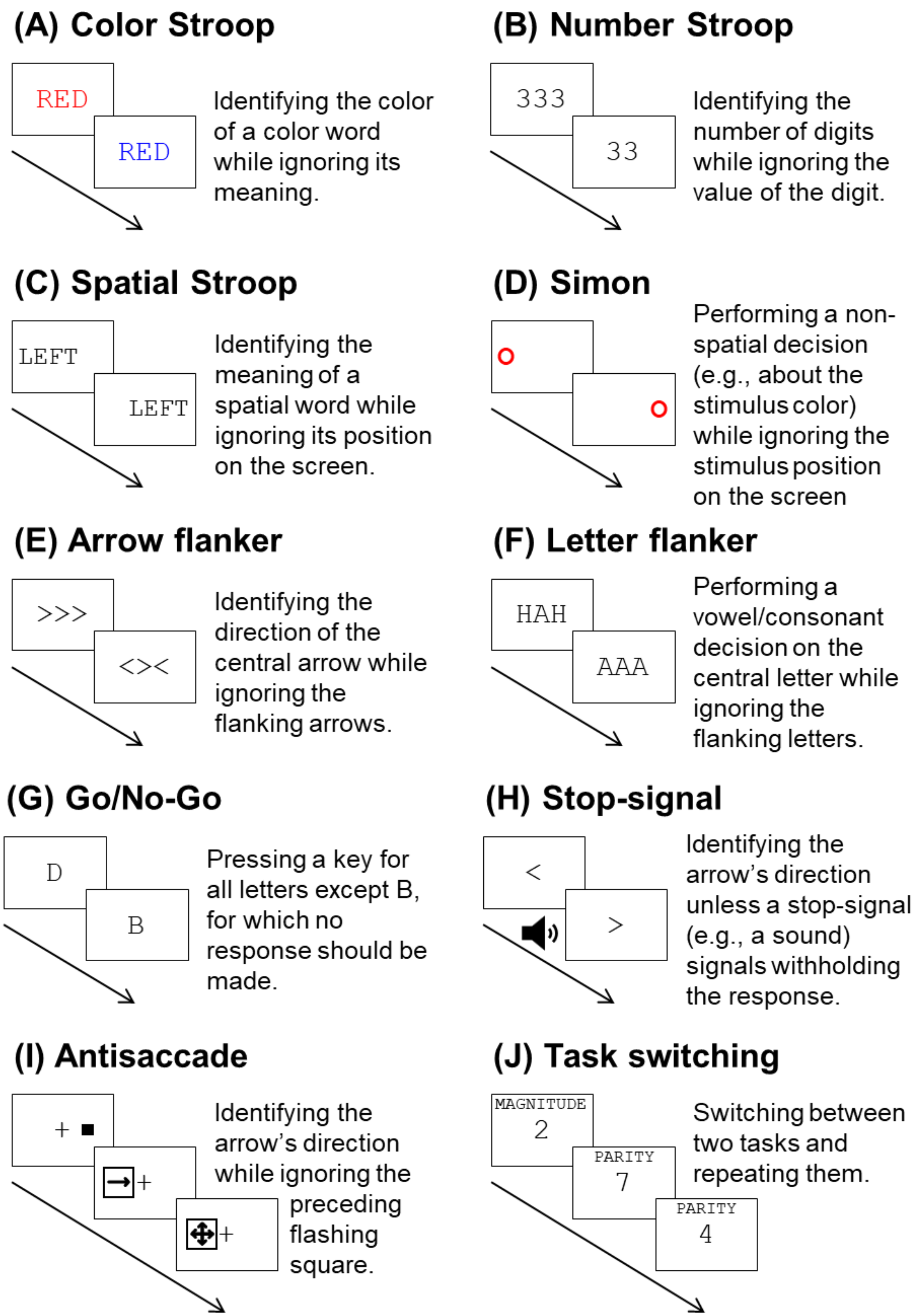
Together, these results emphasize the difficulty of establishing shared variance across the measures of attentional control. They challenge the view of an attentional-control construct that can be measured across different tasks. Concretely, this means that individuals with a low attentional-control ability in one attentional-control task (e.g., the Stroop task) do not show a low attentional-control ability in other attentional-control tasks (e.g., the Simon or flanker tasks). This lack of convergence has sparked an ongoing debate about the sources of this discrepancy and how it might be resolved. This debate has focused on several methodological challenges related to the high measurement error observed in attentional-control measures and the neglect of individual differences in speed-accuracy trade-offs (Draheim et al., 2021, 2024; Haines et al., 2023; Hedge, Powell, & Sumner, 2018b; Hedge et al., 2022; Löffler et al., 2024; Rey-Mermet et al., 2018, 2019, 2025; Robinson & Steyvers, 2023; Rouder et al., 2023; Rouder & Haaf, 2019; Unsworth et al., 2021). However, theoretical challenges, such as those questioning the processes induced in attentional-control tasks, have been rather neglected.

In the present article, we argue that we cannot solve the methodological challenges without refining our theoretical understanding. Thus, we aim to advance this debate by addressing both methodological and theoretical challenges and by proposing an approach on how theoretical and methodological challenges may be tackled together. The present article is organized as follows:

We first review the methodological challenges and the solutions that were proposed to overcome them. Second, we discuss the theoretical challenges. Third, we present an approach combining formal modeling with experimental and correlational methods. This approach will allow us to develop good – that is, reliable and valid – measures of attentional control. With such measures, we can then test whether attentional control is a key psychological construct that contributes to performance across different tasks and situations.

Figure 1

Overview of some widely used attentional-control tasks


(A) Color Stroop
RED
RED
Identifying the color of a color word while ignoring its meaning.
(B) Number Stroop
333
33
Identifying the number of digits while ignoring the value of the digit.
(C) Spatial Stroop
LEFT
LEFT
Identifying the meaning of a spatial word while ignoring its position on the screen.
(D) Simon
Performing a non-spatial decision (e.g., about the stimulus color) while ignoring the stimulus position on the screen
(E) Arrow flanker
>>>
<><
Identifying the direction of the central arrow while ignoring the flanking arrows.
(F) Letter flanker
HAH
AAA
Performing a vowel/consonant decision on the central letter while ignoring the flanking letters.
(G) Go/No-Go
D
B
Pressing a key for all letters except B, for which no response should be made.
(H) Stop-signal
<
>
Identifying the arrow's direction unless a stop-signal (e.g., a sound) signals withholding the response.
(I) Antisaccade
+
+
+
Identifying the arrow's direction while ignoring the preceding flashing square.
(J) Task switching
MAGNITUDE
2
PARITY
7
PARITY
4
Switching between two tasks and repeating them.

Note. (A-F). In each panel, the first trial is congruent, and the second trial is incongruent. Typically, in these tasks, the measure of interest is the response time and/or error rate difference between the congruent and the incongruent trials. (D) In the Simon task, participants performed a non-spatial decision using key presses. In the panel, the response mapping would be left key for red and right key for blue. (G) In the go/no-go task, there are two types of trials: go trials (e.g., all letters except B) and no-go trials (e.g., the letter B). No-go trials appear on a random subset of trials. The main measure of interest is usually the error rate in no-go trials (i.e., commission errors). (H) In the stop-signal task, the go-stimulus (e.g., an arrow) is presented on most trials. On a random subset of trials (usually 25%), an additional stop-signal (e.g., a sound) is presented. The time between the stimulus onset and the stop-signal (i.e., the stop-signal delay) is commonly adapted using a staircase algorithm. This algorithm ensures that participants successfully inhibit their response in around 50% of the trials with a stop-signal. Typically, the measure of interest is the stop-signal reaction time, that is, the time it takes participants to stop. (I) In the antisaccade task, the flashing square typically creates a tendency to perform an eye movement (saccade) towards it. In most implementations, participants perform only the antisaccade condition. In that condition, the target arrow is presented on the opposite side as the flashing square. Therefore, participants are required to ignore the flashing square in order to identify the target arrow. In some implementations, participants perform a prosaccade condition in addition to the antisaccade condition. In the prosaccade condition, the target arrow is presented on the same side as the flashing square. The measure of interest is the accuracy in the antisaccade condition. (J) In the task-switching paradigm, participants are typically asked to complete several tasks (e.g., parity and magnitude decisions for numbers). In certain blocks, the tasks are mixed and in other blocks, there is only one task. On some trials in the mixed blocks, the same task is repeated (i.e., task-repetition trials) and on others, the task changes (i.e., task-switch trials). The measures of interest are mixing and switching costs. Mixing costs are calculated as the performance difference between blocks of mixed tasks and blocks of a single task. Switching costs are calculated as the performance difference between task-switch trials and task-repetition trials.

Methodological challenges

The difficulty of establishing attentional control as a construct that can be assessed with different measures has been argued to result from several methodological challenges. These can be classified into two broad categories: (1) the high measurement error illustrated, for example, by low reliability estimates (Hedge, Powell, & Sumner, 2018b; see also Draheim et al., 2019) and imprecise and biased correlation estimates (e.g., Rouder et al., 2023), and (2) the neglect of individual differences in speed-accuracy trade-offs (Draheim et al., 2021, 2024; Rey-Mermet et al., 2019, 2025). We discuss each of these challenges subsequently.

High measurement error

Current measures of attentional control may reflect too much measurement error (i.e., trial-by-trial variance) to be useful for measuring attentional control. As measurement error is, by definition, unrelated across tasks, this could explain the difficulty of observing common variance across the attentional-control measures.

Researchers have put forward two different solutions to deal with the high measurement error in attentional-control measures. One is to increase the ratio of systematic (true-score) variance to measurement error, that is, to boost reliability. The other is to use statistical models to separate systematic variance from measurement error (e.g., Rouder et al., 2023). We next discuss each of these solutions in detail.

Boosting reliability

Reliability estimates are often found to be low for attentional-control measures (e.g., Hedge, Powell, & Sumner, 2018b; Paap & Sawi, 2016; Rabbitt, 1997; Whitehead et al., 2019; Wöstmann et al., 2013). Reliability has been estimated in two ways (see, e.g., Parsons et al., 2019, for an overview about the different reliability estimates): the test-retest correlation between two measurement points (e.g., two sessions) and the internal consistency (e.g., correlations between odd and even trials, or coefficient alpha). Overall, the results showed a large range of reliability estimates, with the minimum values indicating low reliability (e.g., see Faßbender et al., 2023; von Bastian et al., 2020, for overviews). Researchers have suggested three solutions to boost reliability.

The first is to avoid the use of difference scores and instead use only the condition demanding attentional control (e.g., the incongruent condition in the Stroop task). The rationale for this recommendation is that low reliability is assumed to result from the use of difference scores, which present three key problems. First, difference scores reduce reliability by summing measurement error across the two conditions used to compute them (Schubert et al., 2022). Second, difference scores may remove some systematic variance associated with attentional control (see, e.g., Draheim et al., 2019; Frischkorn, Wilhelm, et al., 2022). This may happen if the baseline condition also requires some attentional control (Schubert et al., 2022). Third, variance in difference scores is affected by individual differences in processing speed and speed-accuracy trade-offs (Hedge et al., 2022; Hedge, Powell, Bompas, et al., 2018; Hedge, Powell, & Sumner, 2018a; Miller & Ulrich, 2013; Paap & Sawi, 2016).

However, considering only the condition demanding attentional control to bypass the use of difference scores is problematic for two reasons. First, the low reliability cannot be explained only by the use of difference scores. For example, task-switch costs are difference scores and yet have a median reliability estimate of .82 (see von Bastian et al., 2020). Second, considering only the condition demanding attentional control does not address the task-impurity problem. This has been recently highlighted by Löffler et al. (2024). In that study, the factor assumed to reflect attentional control was formed with only performance from the conditions demanding (more) attentional control. This factor was completely explained by performance in simple speeded choice tasks that are typically used to assess processing speed.

To address the task-impurity problem without using difference scores, some studies (Rey-Mermet et al., 2019, 2020, 2025) relied on a bifactor modeling approach in SEM. In these studies, performance on baseline conditions (e.g., congruent conditions) and conditions demanding attentional control (e.g., incongruent conditions) were forced to load on a general factor. Additionally, performance in conditions requiring attentional control was forced to load on a specific factor. This second factor was intended to capture attentional control after accounting for the variance shared with baseline conditions. However, these studies found no evidence for a coherent specific factor representing common attentional control variance across tasks; instead, all systematic variance was accounted for by the general factor (Rey-Mermet et al., 2019, 2020, 2025). To conclude, avoiding the computation of difference scores by considering only the condition demanding attentional control may result in higher reliability estimates. However, the variance of these scores is predominantly variance shared with similar tasks demanding little, if any, attentional control, and that variance is unlikely to reflect attentional control.

A second solution to boost the reliability of attention-control measures was to increase the number of trials (Hedge, Powell, & Sumner, 2018b; H. J. Lee et al., 2023; Rouder et al., 2023). Rouder et al. (2023) suggested that reliable estimates of attentional control would be obtained if each participant performs 800 trials per condition (baseline condition and condition demanding attentional control). Studies that evaluated the role of increasing the number of trials found higher reliability estimates with larger trial numbers (Enkavi et al., 2019; Haaf et al., 2025; Hedge, Powell, & Sumner, 2018b; H. J. Lee et al., 2023; Rouder & Haaf, 2019). Nevertheless, these higher reliability estimates did not have the expected effect on the correlations between attentional-control measures from different tasks. That is, most correlations were still lower than .30 (see Hedge, Powell, & Sumner, 2018b). Moreover, a side effect of increasing the number of trials is increasing practice, potentially resulting in the automatization of performing the tasks. This automatization may lead to reduced attentional-control requirements (e.g., Norman & Shallice, 1986; Rabbitt, 1997). One way out of that conundrum would be to design the tasks in a way that allows running many trials without automatization, for instance by changing the task rules after every block.

A third solution that was proposed to boost reliability was to increase the magnitude of the experimental contrast reflecting attentional control, and with it, the amount of systematic variance in the difference score. So far, this has been implemented in two studies, both reporting increased reliability estimates (Kucina et al., 2023; Moretti et al., 2025). Only Moretti et al. (2025) investigated the correlations between two attentional-control measures. The results showed a wide range of values (r = .11 to .66), depending on the experimental manipulation used to increase the difference score and the dependent variable. Therefore, increasing the magnitude of the experimental contrast reflecting attentional control may represent a first promising step to increase the reliability of difference scores without compromising their validity.

The use of statistical models

Another way to separate systematic variance from measurement error is through statistical models. For example, Faßbender et al. (2023) used a trait and state decomposition approach to formally partition the systematic variance in attentional-control measures into two sets of components: trait and state. The trait components reflect stable individual differences shaped by genetic and environmental factors. The state components reflect temporary influences, such as motivation, emotion, or arousal. Other researchers have used a hierarchical Bayesian modeling approach to separate measurement error from systematic variance within each task (Donzallaz et al., 2024; Haines et al., 2023; Rey-Mermet et al., 2025; Rouder et al., 2023; Snijder et al., 2024). Rouder et al. (2023) found that the degree of measurement error was very high in attentional-control tasks. Moreover, even when this high degree of measurement error was taken into account, the correlations between the attentional-control measures were low, and the correlation estimates were found to be imprecise. Rouder et al. (2023) concluded that it is impossible to determine whether the low correlations between attentional-control measures from different tasks result from a high measurement error or from a true lack of shared variance. Rey-Mermet et al. (2025) went one step further by combining a hierarchical Bayesian approach with the diffusion model to model trial-by-trial variability in response times and accuracy. In this case, the results showed quite precise correlation estimates between attentional-control measures from different tasks (i.e., differences between drift rates in congruent and incongruent conditions). However, SEM identified no model in which the fit to the data was good and a coherent factor of attentional control was consistently estimated. This led Rey-Mermet et al. (2025) to conclude that it is a true lack of shared variance that explains the low correlations and the difficulty of establishing a factor of attentional control.

Together, all these studies highlight that using advanced modeling techniques allows researchers to appropriately account for measurement error or even decrease it. However, the amount of shared variance across measures was still low, as shown by low correlations and no coherent factors in well-fitting models (Donzallaz et al., 2024; Faßbender et al., 2023; Rey-Mermet et al., 2025; Rouder et al., 2023; Snijder et al., 2024).

The neglect of individual differences in speed-accuracy trade-offs

A second challenge that was put forward to explain the difficulty of establishing common attentional-control variance was the neglect of individual differences in speed-accuracy trade-offs. In typical attentional-control tasks, the measures are either based on response times or on accuracy. Although the common instructions are to respond as fast and correctly as possible, participants may adopt different strategies. Some participants may favor speed over accuracy; others may favor accuracy over speed. Furthermore, these differences in speed-accuracy trade-offs may differ across tasks. Together, this may result in the neglect of meaningful variance when the attentional-control measures are estimated using only response times, or only accuracy. Critically, this neglect of meaningful variance may explain the difficulty of observing common variance among the attentional-control measures.

At first glance, one solution to consider individual differences in speed-accuracy trade-offs may be to combine both dependent measures (i.e., response times and accuracy) into a single score. However, these scores often do not sufficiently account for individual differences in speed-accuracy trade-offs (e.g., Liesefeld & Janczyk, 2019; Vandierendonck, 2017, 2018). Researchers have suggested two alternative solutions: The use of a response deadline and the use of evidence accumulation models. We next discuss each of these solutions in detail.

The use of a response deadline approach

In the deadline approach, participants are typically instructed and trained to give their responses within a certain time (i.e., before a deadline). The advantage of a response deadline approach is that it pushes all performance variance into accuracy. If individuals with low attentional-control abilities favor accuracy over speed, they would miss the deadline more frequently in the condition demanding attentional control (e.g., incongruent condition) than in the baseline condition (e.g., congruent condition). In contrast, if individuals with low attentional-control abilities favor speed over accuracy, they would select the incorrect response more frequently in the condition demanding attentional control than in the baseline condition. Accordingly, irrespective of whether individuals favor accuracy over speed or speed over accuracy, low attentional-control abilities would be translated into lower accuracy with a response deadline approach.

So far, three studies have used a deadline approach to take into account speed-accuracy trade-offs (Draheim et al., 2021, 2024; Rey-Mermet et al., 2019). In the two studies by Draheim and colleagues (2021, 2024), the deadline for several conflict tasks (e.g., Stroop, flanker) was adapted for each individual, increasing it after errors and decreasing it after correct responses. The dependent variable was, however, not accuracy, but the person’s deadline averaged across a set of trials at the end of the adaptation procedure. In both studies, Draheim and colleagues reported substantial correlations and well-fitting models with factors representing common variance across the measures. Their approach solves the problem of speed-accuracy trade-offs. However, it does nothing to isolate attentional control from other processes or abilities (cf. the task-impurity problem). This is because their measure simply reflects how well people do the task, including the large proportion of variance that the incongruent condition shares with the congruent condition.

In a third study, Rey-Mermet et al. (2019) implemented the response deadline in two steps. First, for each task, participants performed calibration blocks with a neutral condition (i.e., a condition in which only one feature is relevant, such as a row of four blue X's for the color Stroop task). In these blocks, the duration of the response deadline was adapted individually. In the second step, participants performed experimental blocks including incongruent and congruent trials with the response deadline fixed at the individually calibrated duration. Rey-Mermet et al. (2019) used as dependent measures the difference in error rates between incongruent and congruent trials. In addition to removing individual differences in speed-accuracy trade-offs, this two-step approach addresses the task-impurity problem in two ways. First, the calibration procedure diminishes the individual differences in those abilities that are shared between all experimental conditions (neutral, congruent, and incongruent). Second, taking the difference between incongruent and congruent trials further removes contributions of shared variance. The results showed no systematic correlations between the attention-control measures, and no well-fitting models with coherent factors. Therefore, these findings suggest no common attentional-control variance when individual differences in speed-accuracy trade-offs and the task-impurity problem are addressed.

The use of evidence accumulation models as measurement models

The response deadline approach may have a drawback. It may change the nature of tasks, thus casting doubt on whether the measures still assess attentional control. As an alternative, researchers have suggested the use of evidence accumulation models to disentangle individual differences in speed-accuracy trade-offs from the measures of attentional control. One popular evidence accumulation model is the diffusion model that models response times and accuracy in simple two-choice decisions (Ratcliff, 1978). In the simplest version of the model, three parameters of theoretical interest are estimated: (1) The drift rate is assumed to represent the rate at which information in favor of one or the other response accumulates over time. (2) The caution parameter is assumed to reflect the person's speed-accuracy setting. (3) The non-decision time is assumed to capture the speed of sensory and motor responses. This model formalizes speed-accuracy trade-offs as one parameter (i.e., the boundary separation parameter, reflecting response caution) and the effect of attentional control as another (i.e., the drift rate). The drift rate is often assumed to be the parameter that is affected by attentional control (Donzallaz et al., 2024; Löffler et al., 2024; Rey-Mermet et al., 2025; Weigard et al., 2021; Yangüez et al., 2024). The reason is that the ability to control attention is assumed to play a major role in how well individuals can extract the relevant information to select the correct response (e.g., Hübner et al., 2010; Ridderinkhof, 2002; Wiecki & Frank, 2013). In the incongruent condition, the distracting information is evidence in favor of the wrong response, thereby slowing evidence accumulation towards the correct response. People with higher attentional-control abilities can minimize the influence of distracting information, leading to higher drift rates in the incongruent condition.

So far, five studies have used the diffusion model to disentangle individual differences in speed-accuracy trade-offs from measures of attentional control (Hedge et al., 2022; Löffler et al., 2024; Rey-Mermet et al., 2025; Weigard et al., 2021; Yangüez et al., 2024). In two studies (Löffler et al., 2024; Weigard et al., 2021), the results showed no well-fitting model including a coherent factor of attentional control different from processing speed. In a third study (Yangüez et al., 2024), the results showed well-fitting models when attentional control was assessed using only the conditions demanding attentional control (i.e., when the task-impurity problem was not addressed). In a fourth study (Hedge et al., 2022), the diffusion model for conflict tasks was used. This diffusion model was specifically developed for attentional-control tasks, such as the flanker or Simon tasks (Ulrich et al., 2015). It includes an additional set of parameters used to model the effect of distracting information on task performance. These parameters are assumed to capture the early impact of the distracting features on the decision process (see below for further details). Despite the advantage of modeling attentional control more specifically, the results showed low correlations between these parameters across different attentional-control tasks. In a fifth study (Rey-Mermet et al., 2025), a hierarchical-Bayesian diffusion model was used. This analytical approach combines the ability of the diffusion model to integrate speed and accuracy with a hierarchical-Bayesian statistical framework to account for measurement error (e.g., Ratcliff & McKoon, 2008; Vandekerckhove et al., 2011). Despite these advantages, SEM identified no model in which the fit to the data was good and the drift rates consistently form a coherent factor of attentional control.

Methodological challenges: Conclusion

Researchers have put forward several methodological challenges to explain the difficulty of observing shared variance across the measures of attentional control. A broad range of solutions has been proposed to solve these methodological problems. The results are mixed, depending on whether or not the task-impurity problem was addressed. When it was not, the results support common variance across the attentional-control measures (e.g., Draheim et al., 2021, 2024; Weigard et al., 2021; Yangüez et al., 2024). However, because of the task-impurity problem, it is unclear to what extent this common variance is attentional-control variance. In contrast, when the task-impurity problem was addressed, the results support no common variance across the attentional-control measures (e.g., Hedge et al., 2022; Löffler et al., 2024; Rey-

Mermet et al., 2019, 2025; Rouder et al., 2023; Snijder et al., 2024). Therefore, we argue that the difficulty to establish shared variance across the attentional-control measures remains.

Given these results, some researchers have now started the search for the unicorn task that is exempt from all these methodological challenges. Some candidates are the antisaccade task, the go/no-go task, and the stop-signal task. The dependent measures of these tasks are not typically difference scores, and nearly all previous studies report a reliability estimate higher than .70 (e.g., Chuderski et al., 2012; Hedge, Powell, & Sumner, 2018b; von Bastian et al., 2020). However, previous research has highlighted difficulties with the implementations of these tasks to ensure sufficient variability and to account for non-attentional-control processes (Logan, 1994; Matzke et al., 2018; Verbruggen et al., 2019; Young et al., 2018). Even if implemented carefully, recent research has shown that these tasks may suffer from the task-impurity problem (see Frischkorn & Oberauer, 2025; Rouder et al., 2022, for recent research about the antisaccade task).

Beyond methodological challenges: Theoretical challenges

We argue that even if we can solve the methodological challenges, we still need to refine our theoretical understanding of attentional control. That is, the question is now: Are we really assessing *the construct of attentional control* with the measures of attentional control? This question is thus a question of validity, that is, to what extent the attentional-control measures assess what they are intended to assess (see, e.g., Borsboom et al., 2004). Validity cannot be directly tested. Instead, the general approach is to determine the validity of a measure through different criteria or properties, such as content validity, convergent validity, discriminant validity, criterion validity, and external validity (e.g., Brysbaert, 2024). The most common are summarized in Table 1.[1]

Table 1

Description of the Different Criteria or Properties Typically Used to Gauge the Validity of a Measure

Criterion/Property	Description
Content validity	The extent to which a measure accurately covers the full scope of application of the construct that it is intended to assess. A measure with good content validity represents all aspects or dimensions of the construct being assessed. Evaluating a construct's content validity requires a consensus about what a particular construct encompasses (e.g., its dimensionality).
Convergent validity	The extent to which different measures of the same construct are correlated with each other. A measure with a good convergent validity is correlated positively with other measures that are assumed to measure the same construct.
Discriminant validity	Discriminant validity is the complement of convergent validity. It refers to the extent to which a measure is distinct from other measures that are theoretically not related to the construct that it is intended to assess. A measure with discriminant validity should capture a unique aspect of the construct being assessed. A measure with good discriminant validity is not strongly correlated with measures that are assumed to measure different, unrelated constructs.
Criterion validity	The extent to which a measure accurately predicts some external criterion that is theoretically related to the construct that it is intended to assess. A measure with a good criterion validity is related to a specific criterion or outcome. Criterion validity can be divided into concurrent validity (i.e., when the criterion is assessed

[1] Another term commonly mentioned in the same context as these validity criteria or properties is construct validity. According to the APA (1954), construct validity refers to the investigation of the theoretical underpinnings of a measure. To gauge construct validity, researchers need to make theoretical predictions for a measure and then empirically test these predictions. Over the decades, the definition of construct validity was considerably muddled, and is now almost indistinguishable from the overarching term of validity itself. We therefore do not refer to construct validity as a criterion or property and use construct validity and validity interchangeably.

Criterion/Property	Description
	at the same time as the measure) and predictive validity (i.e., when the criterion is assessed at a future point in time).
External validity	The extent to which the results using a measure or a group of measures can be generalized beyond the specific conditions or context in which the measures were used. Results obtained with a measure with good external validity can be applied to other populations, settings, or circumstances.

So far, the validity of the attentional-control measures has been mainly determined using convergent validity, discriminant validity, and criterion validity. The reason is that these properties are quantifiable by estimating correlations or SEM. For example, the low correlations between the attentional-control measures as well as the difficulty of establishing a factor of attentional control indicate missing convergent validity (e.g., Chuderski et al., 2012; Kane et al., 2016; Klauer et al., 2010; Krumm et al., 2009; Paap & Greenberg, 2013; Rey-Mermet et al., 2018, 2019, 2020, 2025; Shipstead et al., 2014; Unsworth et al., 2009; von Bastian et al., 2016). Discriminant validity was evaluated by assessing to what extent the measures of attentional control are distinct from those of processing speed or working memory (e.g., Draheim et al., 2021, 2024; Löffler et al., 2024; Rouder et al., 2022). Criterion validity was determined by examining the relationship of attentional-control measures with intelligence or impulsive behaviors (e.g., Bartholdy et al., 2016; Draheim et al., 2021, 2024; Sharma et al., 2014; Smith et al., 2014).

Testing the convergent, discriminant and criterion validity is, however, more complicated than it seems at first glance (see, e.g., Borsboom et al., 2004). When cognitive measures assess the same construct, performance on these measures should be highly correlated. However, because of measurement error, these high correlations are often attenuated. Likewise, when cognitive measures assess different constructs, performance on these measures should not be correlated at all. However, cognitive measures generally tend to correlate positively among each other – regardless of the constructs assessed. This positive manifold implies that correlations between measures of the same and different constructs can only be evaluated relative to each other. For example, it makes sense to claim that the low correlations between measures of different constructs speak in favor of discriminant validity only if the correlations among the measures of the same construct are higher and thus show good convergent validity. Conversely, it makes sense to claim that medium-sized correlations between measures of the same construct speak in favor of convergent validity only if the correlations among the measures for different constructs are lower.

These challenges of establishing convergent and discriminant validity have two negative consequences. First, since correlations must be interpreted in relation to each other, the criteria for gauging convergent or discriminant validity can shift across validation studies or research groups, undermining consistency. Second, while patterns of correlations can reveal which measures cluster together or stand apart, they cannot specify what these measures truly capture. To properly imply validity from empirical correlational structures, one has to theoretically establish the relationships between the measures and the constructs to be assessed.

The challenges of testing convergent and discriminant validity are particularly pronounced in the field of attentional control. Because of the low reliability of the attentional-control measures, the correlations are often drastically attenuated (Rouder et al., 2023). Thus, it is difficult to distinguish between correlations speaking in favor of or against convergent validity or even discriminant validity (see, e.g., Miyake et al., 2000). Furthermore, even when the correlations are larger, the pattern of higher correlations for measures assessing the same construct and lower correlations for measures assessing different constructs is often not observed. For example, recent research has reported correlations of similar magnitude (.60) within and between processing-speed measures and attentional-control measures (see, e.g., Burgoyne et al., 2023; Draheim et al., 2024).

If testing convergent, discriminant, and criterion validity is neither successful nor sufficient to ensure validity, the question is: How should we test the validity of attentional-control measures? We argue that validation of psychological measures – the process of establishing validity – requires mainly theoretical work (Borsboom et al., 2004; Strauss & Smith, 2009). One principled approach to construct validation that has been discussed repeatedly is formal modeling (e.g., Borsboom, 2006; Grahek et al., 2021). Following this approach, we propose to use formal modeling as a means to study the construct of attentional control and its relationship to attentional-control measures.

Next, we first outline how formal models can address the challenges of validity. Then, we describe how formal models can be used to address the challenges of convergent, discriminant, and criterion validity.

Formal modeling to address the challenges of validity

Formal models of cognition generally translate a verbal theory into a set of mathematical equations. These equations reflect the theoretical assumptions about cognitive mechanisms and processes that generate behavior. The equations usually have free parameters that describe some characteristic of a cognitive mechanism or process (e.g., the rate at which something happens, the capacity of some mechanism); these parameters need to be estimated from the data.

The use of formal models has three advantages. First, it allows us to understand the functional relationship between cognitive processes and observed data. Second, it gives us the possibility to model how changes in a specific cognitive process of interest are reflected in observed data. Third, it makes it possible to measure model parameters representing cognitive processes.

For the validation of psychological measures, formal modeling provides a key theoretical link between a psychological construct and its empirical measure. Borsboom and colleagues (2004) argue that for a measure to be valid, the link between a construct and the measure needs to be causal. For example, only if differences in attentional control cause differences in an attentional-control measure, then the attentional-control measure is valid. To support such a claim, we need a theory in which the causal link from construct to measure is assumed, and empirical support for that theory. In a nutshell, formal models mathematically link constructs to behavioral measures, therefore providing quantifiable predications that can be tested empirically.

Overview of formal models of attentional control

Formal models of attentional control are grounded in underlying theories of cognition. They allow researchers to distinguish parameters that reflect attentional control from parameters that reflect unrelated strategies, processes, or abilities, such as processing speed, the ability to hold task instructions available, or response conservativeness. Table 2 provides an overview of broad categories of formal models specifically developed for attentional control. As shown in this table, each of these models was originally designed to fit a small set of attentional-control tasks (i.e. one to maximal three tasks).

Table 2

Overview of formal models of attentional control.

Type	Reference and Name	Task(s)
Parallel distributed processing (PDP)	Cohen, Dunbar, and McClelland (1990); Cohen, Servan-Schreiber, and McClelland (1992)	Stroop
	Zorzi and Umilta (1995)	Simon
	Zhang et al. (1999): PDP model of compatibility	Stroop
	Botvinick et al. (2001): Conflict monitoring model	Flanker, Stroop
	Gilbert & Shallice (2002)	Task switching
	Rougier et al. (2005)	Wisconsin card sorting[1]
	Brown et al. (2007)	Task switching
	Oberauer et al. (2013)	Task switching
	Wiecki & Frank (2013)	Antisaccade, Stop-Signal
	Schmidt et al. (2016): Parallel Episodic Processing (PEP) model 2.0	Stroop, Flanker
Bayesian inference	Yu et al. (2009)	Flanker
	Shenoy & Yu (2011): Rational decision-making model	Stop-signal
	Braver (2012): Dual mechanisms of control model	Stroop
Evidence accumulation	Hübner et al. (2010): Dual-stage two-phase model	Flanker
	White et al. (2011): Shrinking spotlight model	Flanker
	Logan et al. (2014): Stop-Signal Racing Wald Model	Stop-signal
	Ulrich et al. (2015): Diffusion model for conflict tasks (DMC)	Stroop[2], Simon, Flanker
	Steyvers et al. (2019)	Task-switching
	Matzke et al. (2020): Stop-Signal Lognormal Race Model	Stop-signal
	López & Pomi (2024): Dual-Route Evidence Accumulation Model	Simon, Flanker
	Lee & Sewell (2024): Revised DMC	Simon, Flanker
Race	Logan (1981); Logan & Cowan (1984): Independent horse race model	Stop-signal
	Matzke et al. (2013): Bayesian Estimation of Ex-Gaussian Stop-Signal reaction time distributions	Stop-signal

Type	Reference and Name	Task(s)
	Noorani and Carpenter (2013): LATER model	Antisaccade, Flanker
	Gronau et al. (2024)	Stop-signal
Race + Evidence accumulation	Tanis et al. (2024): Hybrid model	Stop-signal
Theory of Visual Attention (TVA)	Logan & Gordon (2001): Model of executive control of TVA; Logan and Bundesen (2003, 2004)	Task-switching
ACT-R	Sohn & Anderson (2001)	Task-switching
	Altmann & Gray (2008): Cognitive control model	Task-switching
Reinforcement learning	Verguts & Notebaert (2008)	Task-switching, Stroop
Neural field	Bompas et al. (2020): DINASAUR	Stop-signal

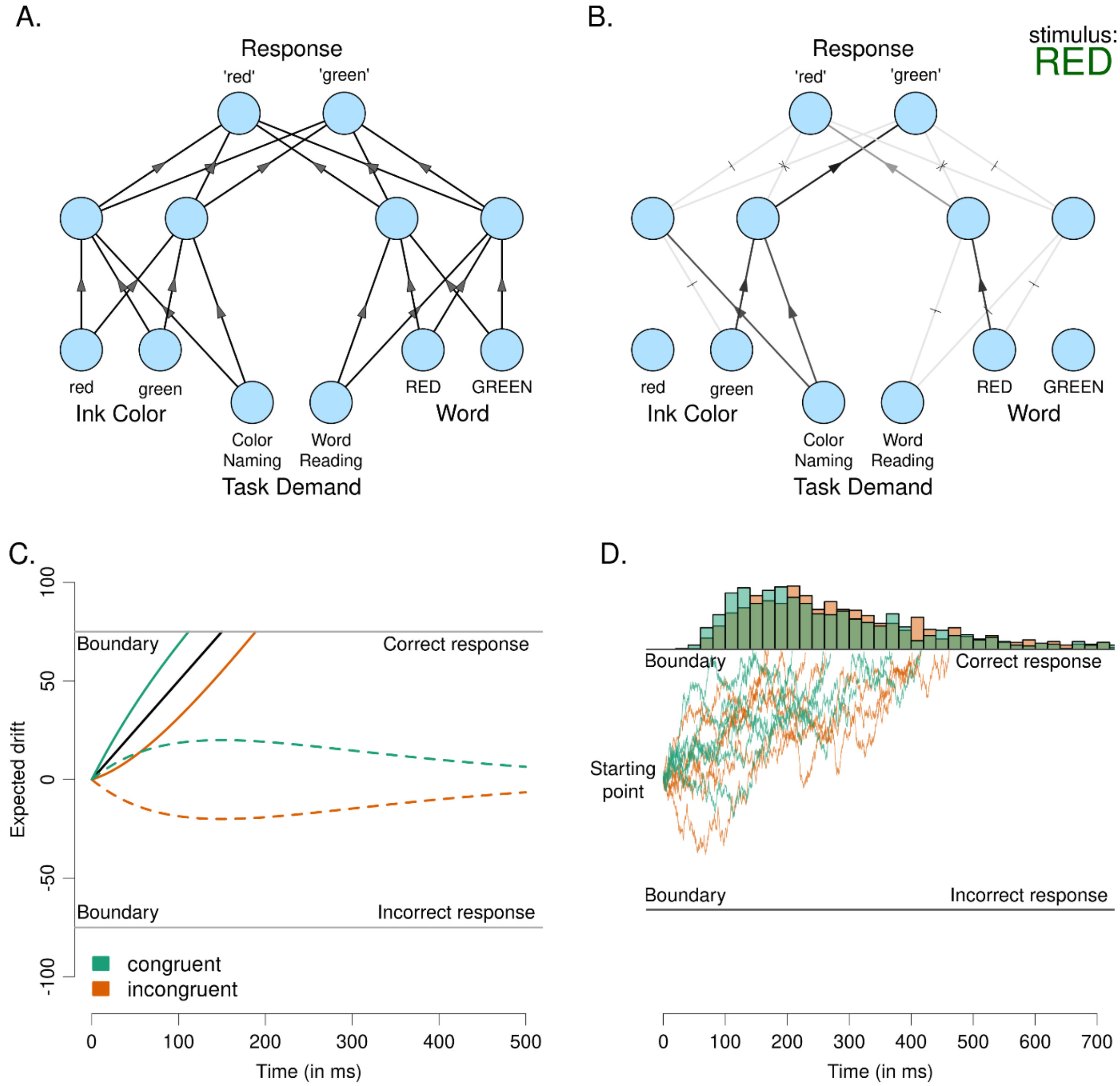
Note. For each type, the models are presented in chronological order. Empirical effects that are modeled are the congruency effect in Stroop, Simon and flanker tasks, the stop-signal performance in the stop-signal task, switch costs in the task-switching paradigm, and accuracy in the antisaccade task. In addition to the models presented in the table, there is the conflict LBA from Heathcote et al. (n.d.). However, this model has not been sufficiently described and validated in the peer-reviewed literature.

[1]The Wisconsin Card Sorting Test is a neuropsychological test designed to measure deficits in attentional control. During the test, participants sort cards based on varying criteria, such as color, shape, or quantity, and must adjust their strategies as the sorting rules change unpredictably, which requires them to inhibit previous responses and develop new ones.

[2] In Ulrich et al. (2015), the model was applied to the Simon and flanker tasks only. Recently, this model has been further used on the Stroop task (Ambrosi et al., 2019; Hedge et al., 2019, 2022).

Figure 2

Two examples of formal models of attentional control.



Note. (A, B) The parallel distributed processing model by Cohen et al. (1990, adapted after Figure 1, p. 336). (C, D) the diffusion model for conflict tasks by Ulrich et al. (2015, adapted after Figure 4, p. 157). (A) shows a schematic depiction of the parallel distributed processing model by Cohen et al. (1990). (B) shows activation (arrows) and inhibition (crossed out lines) when the instruction is to name the color of a word and the stimulus is the word “RED” written in green. Then, two activation arrows cross over to different response options. (C) shows a schematic depiction of the diffusion model for conflict tasks by Ulrich et al. (2015) with the expected drifts for the congruent and incongruent condition. The dashed lines represent the automatic activation of the responses, the black line represents the activation of responses through controlled processing of the stimulus. The colored solid lines represent the combined drift. (D) shows six example drift processes per congruency condition. They all start at the starting point and move depending on their combined drift from panel (C). A response is given when one of the boundaries is hit. The histograms show the finishing times of the correct responses. Response times are the sum of the non-decision times (not shown) and the finishing times.

Most models presented in Table 2 can be roughly grouped into two classes. The first class includes parallel distributed processing models, neural networks and Bayesian inference models. These models can be grouped together because their origin is in the connectionist framework (McClelland, 1993; Rumelhart & McClelland, 1987). In this framework, processing units – simple input/output functions that resemble nerve cells – are connected through paths. These connection paths can have an excitatory or inhibitory value. For most of the models, this network of units processes input to produce an output (or a response), it learns , and, crucially, it includes control processes modulating response selection. Thus, according to this model class, attentional control is deeply interconnected with learning. An early example of this modeling approach is the parallel distributed processing model by Cohen et al. (1990). The model is specified for the Stroop task and is based on the assumption that automatic processes are learned from repeated experience and modulated by attentional control. A depiction of this model is shown in Figure 2A and B. Panel A shows a simple processing network for a Stroop task. The first layer of processing units in the network relate to the input, including both stimulus features and task instructions. After an intermediate processing layer, there is the third layer which relates to output, that is, which response to give. Distraction in the model occurs when two inputs are processed in parallel and feed into different output units. Figure 2B shows the processing of an incongruent stimulus, the word red written in green. In this case, the paths from the word input and color input are both activated and cross over to different responses, leading to interference. Attentional control is executed by task units that activate the processing paths that correspond to the task demands ("respond to color") and simultaneously deactivate – or inhibit – processing paths that do not correspond to the task demands. Based on this control, the interfering path from the word is less activated than the path from the ink color. In the models' final stage of processing, a response is selected based on a random walk for each response option (not shown in the figure).

The second class of models includes evidence accumulations models and race models. Most models from this class focus on processes for response selection. They specify some form of evidence accumulation or race process, either separately for each response option or a single accumulation process for competing response options. Attentional control is specified as a process that directly modulates the evidence accumulation. An example of the second class of models is the diffusion model for conflict tasks (DMC; Ulrich et al., 2015). The model is illustrated in Figure 2C and D. According to the diffusion model for conflict tasks, response selection in attentional control tasks is shaped by two processes. One of the processes is controlled and deliberate, and the other process is fast and automatic. The controlled process is represented by a standard diffusion model in which evidence from the target feature (e.g. print color in the Stroop task) is accumulated at a constant rate. This is depicted by the continuous black line in Figure 2C. Crucially, this process is the same for congruent and incongruent trials and undisturbed by task-irrelevant information. The automatic process, by contrast, is activated by task-irrelevant information (e.g., word meaning in the Stroop task). It is depicted by the dashed lines in Figure 2C. The lines deviate for the congruent and incongruent trials, and their biggest difference is early in the evidence accumulation process. Over time, the influence of the automatic process gradually decreases, and the controlled process takes over. The model assumes that attentional control mitigates the influence of the automatic processing component. Individuals with strong attentional control therefore should have a reduced amplitude of the dashed curve and/or an earlier peak, indicating that the controlled processing takes over sooner (Hedge et al., 2022). The continuous colored lines in Figure 2C show the drift rates for the congruent and incongruent trials as a combined function of controlled and automatic processes. The effect of the difference between the two drift rates is shown in Figure 2D through a sample of evidence accumulation paths. In these examples, all paths end up with the correct response. However, there are differences in the processes' finishing times – incongruent trials have longer finishing times than congruent trials.

Empirical validation of formal models

Formalizing models, such as those listed in Table 2, serves as a theoretical validation that the construct of interest – in our case, attentional control – plays a major role in the observed behavior from these tasks. In addition to this theoretical validation, there are numerous forms of technical and empirical model validation. For example, there are parameter recovery studies in which data are simulated based on set parameter values, and then the model is fitted using the simulated data to assess how well we can recover the original parameters. Parameter recovery studies ensure that the technical implementation of the model works as expected. However, parameter recovery studies rarely rely on real behavioral data.

The form of validation that relies on real behavioral data is empirical validation. Empirical validation is the process of comparing predictions of formal models for the data with actual empirical observations. In contrast to verbal models, the empirical validation of formal models allows researchers to make fine-grained predictions of data patterns that arise from the formalized cognitive processes. All the models from Table 2 have been empirically validated with experimental data to some extent. However, this initial validation is often small in scope. Here, we discuss three ways to empirically validate formal models.

The most basic empirical validation consists of implementing formal models in software to simulate predicted data patterns (Wilson & Collins, 2019). These predicted patterns are then compared to observed data from one or several experiments. For example, data from congruent and incongruent conditions are simulated using different values for parameters related to attentional control. These simulated data for the two conditions are compared to corresponding empirical data. The main advantage of this approach is that it is very flexible and can be used for all formal models. However, there are three main disadvantages of the approach. First, the criteria to judge whether the simulated data sufficiently correspond to empirical data tend to be very soft, mainly using descriptive statistics. Second, researchers usually focus on a handful of simulations, and do not weigh the effects of sample noise on the simulations as they would for empirical data (Carley, 2009). Third, this form of empirical validation often focusses on key psychological parameters of a model without exploring more ancillary parameters (M. D. Lee et al., 2019). As a result, comparing simulated data to empirical data shows that a model *can* produce certain data patterns, but this approach does not further explore realistic parameter values.

A second way to validate formal models empirically consists of simulating data not only from preset parameter values, as in the basic way, but also from parameter values estimated using observed data (Wilson & Collins, 2019). Models that are validated empirically in this way – thereafter called estimation models – are more closely related to statistical models. However, there is a key distinction between estimation and statistical models. Statistical models typically aim to describe or predict empirical patterns without necessarily specifying underlying processes. In contrast, estimation models seek to capture hypothesized cognitive mechanisms (M. D. Lee et al., 2019). Typically, estimation models are considered as providing a stronger bridge between theory and data than other forms of formal models. Additionally, estimation models allow for more stringent model comparison (Myung & Pitt, 1997). From Table 2, all evidence accumulation and race models are implemented as estimation models. From the first class of models, only one was implemented as an estimation model (Yu et al., 2009).

A third way of empirical validation consists of conducting selective influence studies. In these studies, the effect of experimental manipulations on model parameters is assessed. For example, researchers may try to increase the impact of distracting information in a flanker task by showing target stimulus and distractors more closely together. Ideally, this manipulation would lead to changes only in the attentional-control parameters of the formal model, but not in any of the other parameters. So far, there have been only few selective influence studies for attentional-control models, showing that model parameters largely respond to experimental manipulations of distraction in a way consistent with their cognitive interpretation (Hübner et al., 2010; Hübner & Többel, 2019; Servant & Evans, 2020; White et al., 2011).

Limitations of formal modeling for validation

Based on the empirical validation efforts discussed in the previous section, we can conclude that there is some evidence that formal modeling approaches of attentional control are in line with empirical data from attentional-control tasks. However, formal modeling and empirical validation are at this point far from sufficient to address the validation of attentional control. First, the process(es) of attentional control are formalized somewhat differently for almost every task. There are a few attempts to use the same formal models for similar tasks (e.g., Stroop, flanker, and Simon tasks; see, e.g., Botvinick et al., 2001; P.-S. Lee & Sewell, 2024; Ulrich et al., 2015). Furthermore, in a few studies, the model parameters reflecting attentional control have been related to each other (e.g., Hedge et al., 2022; Robinson & Steyvers, 2023). Despite these few attempts, there exists no model in which attentional control is isolated in the same parameter(s) for several dissimilar attentional-control tasks. Second, many different formal models have been applied to the same attentional-control tasks (see Table 2). However, there is limited research on formal statistical comparison between these models to determine which model has the best fit to data from a specific task. Third, individual differences are generally not considered when formal models of cognition are developed (Hunt & Lansman, 1975; M. D. Lee, 2011). On the face of it, it makes sense to assume that parameters representing cognitive processes vary across people. Some models have accounted for individual variability using hierarchical modelling approaches. However, just as some of the attentional-control tasks are not optimized to differentiate between individuals, neither are most models. This implies that models are designed to capture neither variability in attentional-control ability nor correlations across different tasks.

In sum, several decades of formal modeling of attentional control may help to investigate validity. As shown in Table 2, there exists several models which offer many opportunities to better understand the theoretical underpinnings of attentional-control tasks. However, there are still considerable gaps both in the broad range of formal models and in relating them to individual differences in attentional control.

Formal modeling to address the challenges of convergent, discriminant, and criterion validity

Besides theoretically relating the attentional-control measures to the construct of attentional control, formal modeling can be used to test convergent, discriminant and criterion validity of the attentional-control tasks. A few attempts have been made in this direction. For example, for criterion validity, one path would be to use neurocognitive models that link model parameters to established neurocognitive correlates of the specific processes. Schubert and colleagues (2022) proposed such an approach for validating the dual-stage two-phase model (Hübner et al., 2010) for the flanker task by assessing the relationship between model parameters and electrophysiological measures of attentional control.

For convergent validity, two different approaches have been used: correlating the parameters assumed to tap attentional control and substituting between these parameters. Each of these approaches is next described.

Correlating model parameters between tasks

The first approach used to test the convergent validity of attentional-control measures with formal models was to estimate the correlations between the parameters assumed to be associated with attentional control, and to compute structural equation modeling. In some attentional-control studies, this approach was implemented using a three-step procedure (Löffler et al., 2024; Weigard et al., 2021; Yangüez et al., 2024). In the first step, a standard diffusion model was estimated, and the model parameters associated with attentional control were extracted for each person and each task. In the second step, these parameters were correlated across tasks. In the final step, a structural equation model was estimated using these correlations. As highlighted earlier, the results depended on whether or not variance unrelated to attentional control was controlled for to address the task-impurity problem. Only when this problem was not addressed, substantial correlations between parameters, and a factor representing their shared variance, were obtained.

There are two shortcomings in how the approach of correlating the parameters was implemented in these studies (Löffler et al., 2024; Weigard et al., 2021; Yangüez et al., 2024). First, the standard diffusion model is not ideal for data from attentional-control tasks, such as the Stroop task (Lüken et al., 2025; Ulrich et al., 2015). One reason is that the number of errors typically observed in these tasks is low, and this low number of errors makes it difficult to robustly estimate all diffusion model parameters (Lüken et al., 2025). Another reason is that there is model misfit for fast responses. For these responses, the standard diffusion model predicts fewer errors and smaller attentional-control effects in response times than typically observed in some attentional-control tasks (Ulrich et al., 2015). The second shortcoming is that using a three-step procedure leads to overconfidence in underestimated correlations. The correlations may be underestimated because in the three-step procedure, the measurement error of the diffusion model parameters is ignored. As a consequence, the correlations computed for these parameters across tasks are attenuated and thus underestimated. Moreover, the three-step procedure may lead to overconfidence because the uncertainty that occurs in the estimation of the model parameters in the first step is not transferred to the subsequent steps.

These shortcomings were addressed in two studies (Hedge et al., 2022; Rey-Mermet et al., 2025). The first shortcoming was addressed by Hedge et al. (2022) who applied the diffusion model for conflict tasks (Ulrich et al., 2015). Thus, the effect of attentional control was measured by two parameters that model the amplitude and the time to peak of the automatic process – the smaller the amplitude and the sooner the time to peak the more attentional control. In seven datasets, the results show small or no correlations for the attentional-control parameters.

The second shortcoming was tackled by Rey-Mermet et al. (2025): Instead of fitting the model separately to each participant's data, they used a hierarchical diffusion model to fit the model to all participants and tasks simultaneously. Thus, this approach combines the first two steps of the previous procedure. Moreover, it accounts for the measurement error, thus resulting in more precise correlation estimates. Despite these advantages, the results identified low correlations and no model including a coherent factor of attentional control.

Substituting model parameters

The second approach of testing convergent validity using formal models consists of substituting model parameters across tasks. So far, this approach has only been conducted by Robinson and Steyvers (2023) who employed simple, generic models of evidence accumulation.[2] In a large dataset including performance on a flanker task and on a task-switching paradigm, Robinson and Steyvers (2023) first fitted the model separately to each person's data from each task. This resulted in two sets of parameter estimates (one per task) for each person. However, instead of computing correlations between model parameters across tasks, they took the

[2] Robinson and Steyvers (2023) used a model in which the target information drives evidence accumulation towards the correct response, and the distracting information (e.g., the flankers) drive evidence accumulation to a response that is correct in congruent but incorrect in incongruent trials (Steyvers et al., 2019). Two parameters of the model jointly control the difference in evidence accumulation rate between the target-related evidence and the distractor-related evidence. These parameters reflect the ability to control attention, that is, to selectively up-regulate target-related evidence, and down-regulate distractor-related evidence.

parameters from the first task (e.g., flanker) and inserted them into the model of the second task (e.g., task switching). Thus, based on model parameters of the first task, Robinson and Steyvers (2023) could predict performance in the second task (i.e., task switching). This parameter substitution was first applied within each participant. In a second step, they substituted the parameters across participants. That is, in the first participant, they took the parameters from the first task (e.g., flanker for participant A). Then, they inserted these parameter estimates into the model of the second task for the second participant (e.g., task switching for participant B). Substituting the parameters across participants made the cross-task predictions worse compared to substituting the parameters within participants. This was interpreted as evidence that individual differences in the attentional-control parameters of the model generalize to some extent between flanker and task switching.

This approach seems very promising. So far, however, it has been implemented in one study including two attentional-control tasks (flanker and task switching), a large sample size (1800 trials per task for 495 participants) and participants from a population with a large age range (ages 21 to 80). Future studies may investigate the robustness and generalizability of the approach.

Theoretical challenges: Conclusion

In sum, the present overview shows that formal modeling work in this area is scattered and lacks both theoretical and empirical consolidation. Formal models hardly ever relate to individual differences of attentional control, which would be necessary for a meaningful contribution to the measurement of attentional control. Despite these gaps, formal modeling seems to be a promising path for addressing theoretical challenges related to measurement of attentional control.

Next steps

From the methodological and theoretical challenges, we can draw two conclusions. First, previous research clearly suggests that the effects of attentional control on performance are robust but small at the group level. Such a pattern could only occur if true variability – that is, variability in the attentional-control processes – is limited (Rouder et al., 2023). The second conclusion is that only considering correlations among the attentional-control measures – such as when computing correlations or structural equation modeling – is not sufficient to establish attentional control as a key psychological construct with valid measures. We should go beyond these correlations because these correlations cannot tell us what the measures reflect. This may be particularly the case when the measures of attentional control do not adequality isolate attentional control from non-attentional-control processes and measurement error.

Taken together, these conclusions underscore the need for an approach that both increases true individual variability and ensures that correlations among measures reflect a common underlying construct of attentional control. Achieving this goal will require the development of new tasks or the refinement of existing ones. But how can this be accomplished? As a starting point for the field, we propose an approach that integrates formal modeling with experimental and correlational methods. This framework comprises nine steps, which are summarized in Table 3.

Table 3

Description of the Steps Proposed to Develop New Attentional-Control Tasks and/or Improve Current Attentional-Control Tasks

Number	Step
1	Using the verbal definition of attentional control as a starting point for theoretical explanation
2	Implementing the verbal definition of attentional control as one or more formal models a. Determining the necessary psychological processes underlying performance in the attentional-control tasks b. Using simulations to ensure that the parameters underlying the attentional-control processes behave as conceptualized c. Using simulations to relate the unobservable psychological processes to observable behavior
3	Creating experimental manipulations where these attentional-control parameters affect actual task performance, and designing different versions of these experimental manipulations
4	Piloting different versions of the experimental manipulations and modelling the data using the formal model in an iterative process. The goal is to obtain one or several versions of a task with sufficient true variability to result in a measure with a good reliability and a robust effect at the group level
5	Performing a parameter recovery simulation based on the different versions of the task (i.e., simulating data from the model and checking that the parameter values that were used can be recovered)
6	Experimental validation using behavioral data
7	Developing further attentional-control tasks using Step 3 to 6
8	Testing the convergent and discriminant validity of attentional-control tasks developed in Step 7
9	Testing the dimensionality of attentional control (e.g., conceptualized as a unitary construct, as a general construct with subconstructs, as different but related constructs, as different and unrelated constructs)

Note. True variability refers to the variability in the attentional-control process. True variability should result in variability in the parameter(s) and in performance.

Step 1: Verbal definition

In the first step, we follow current frameworks for building theories by starting with theoretical explanations that account for the empirical phenomenon of interest (e.g., Borsboom et al., 2021; Oberauer & Lewandowsky, 2019). In this context, we think that it is safest to start with the consensus definition of the construct of attentional control provided by von Bastian and colleagues (2020). According to this definition, "the basic ability in attentional control is maintaining an operative goal, and goal-relevant information, in the face of distraction" (p. 8). Within this definition, von Bastian et al. (2020) characterized two concepts in more detail. First, they defined an operative goal as "one that substantially influences current information processing and action". Second, they explained distraction as a continuous variable, reflecting the degree of prioritization of goal-irrelevant information.

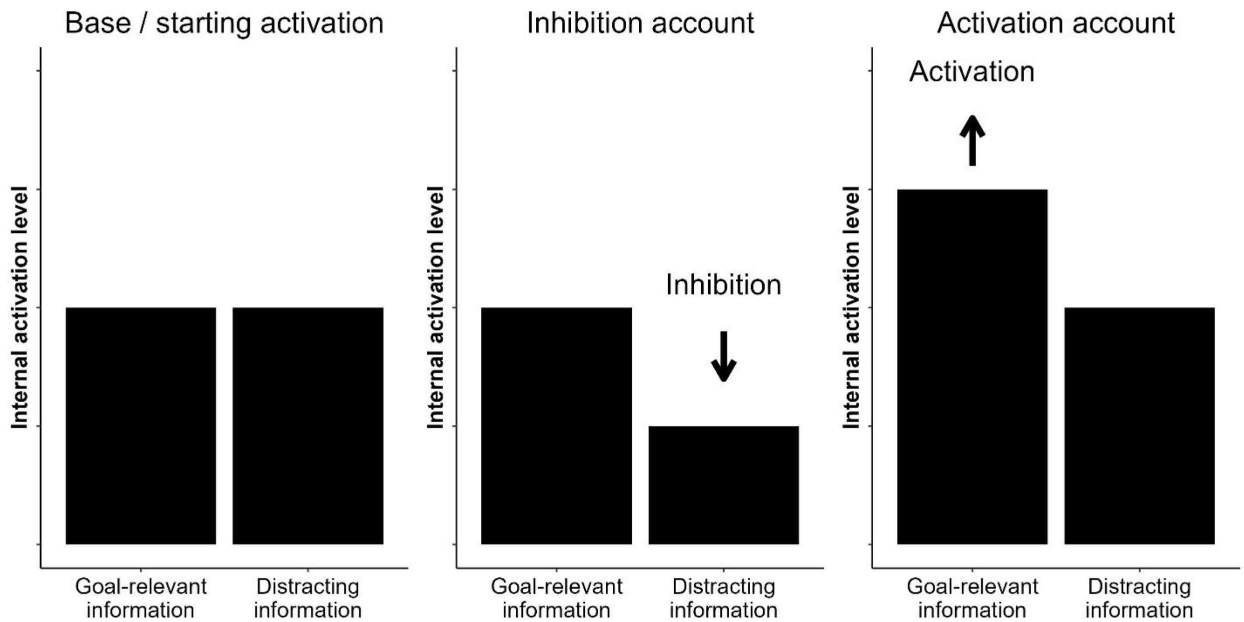
Step 2: Model implementation

In the second step, we propose to implement the verbal definition of attentional control as one or more formal models. The advantage of this approach is that we are able to formally test the theoretical account of attentional control and to make predictions for data (e.g., Borsboom et al., 2021). This second step includes three subordinate steps. First, implementing the verbal definition of attentional control as a formal model requires us to determine the necessary processes underlying performance in the attentional-control tasks (see Step 2a in Table 3). According to the verbal definition of attentional control presented above, such processes are those that maintain an operative goal and influence information processing and action to meet this goal. These processes likely include establishing and maintaining representations of task-relevant information in working memory (i.e., the ability to retain access to a limited amount of information in the service of complex tasks; e.g., Baddeley & Hitch, 1974; Cowan, 1998; Miyake & Shah, 1999; Oberauer, 2009; Oberauer & Kliegl, 2006). Critically, these processes are assumed to be affected by distraction. Principally, there are two ways this distraction can be handled (see Figure 3). First, distracting information can be suppressed or inhibited so that goal-relevant information is prioritized. Second, goal-relevant information can be enhanced or activated more strongly, thereby reducing the relative impact of distracting information. Prior research suggests that inhibition itself can be implemented as different mechanisms. For example, inhibitory processes may result in filtering of information so that the focus of attention is on relevant information (see, e.g., Cohen et al., 1990; Hübner et al., 2010; Munakata et al., 2011; Verguts & Notebaert, 2009; White et al., 2011, 2018). Furthermore, inhibitory processes may result in stopping any responding ("global stopping"; e.g., Logan et al., 2014; Wiecki & Frank, 2013; but see Chatham et al., 2012, for evidence against global stopping processes).

A formal model should help us to determine whether all these processes – that is, filtering, global stopping, and activation – contribute to performing successfully in attentional-control tasks. To this end, such a model should incorporate parameters reflecting all of these processes. When implementing this model, we should, however, carefully check whether these processes are distinct enough to be identifiable. If not, the model should be revised to avoid severe parameter tradeoffs. So far, there exist some formal models that incorporate parameters reflecting some of these processes (see Table 2). However, there is no model incorporating parameters for all of these processes. One may argue that we can simply use the existing models to assess attentional control. However, these models have been developed to match data from current attentional-control tasks that are known to be suboptimal regarding, for example, their reliability and validity. Therefore, if we aim to develop new tasks or improve the current tasks substantially, we need to develop new formal models. Nevertheless, the existing models may be used as a starting point. In this case, we should carefully examine which components of these models relate to the verbal definition of attentional control and hence, we should keep.

Figure 3

Schematic Depiction of How Goal-Relevant and Distracting Information Are Handled According to the Inhibition Account and Activation Account



Note. Left panel: Base activation level. Central panel: Inhibition account. Right panel: Activation account. According to the inhibition account (middle panel), distracting information are inhibited relative to the starting activation level (left panel). According to the activation account (right panel), goal-relevant information is activated relative to the starting activation level (left panel).

The second subordinate step in implementing the verbal definition of attentional control as one or more formal models consists of ensuring that the parameters underlying the critical processes behave as conceptualized (see Step 2b in Table 3). For example, as illustrated in Figure 3, a parameter representing activation should increase the internal activation level of goal-relevant information. In contrast, a parameter representing inhibition should decrease the internal activation level of distracting information. Although these internal activation levels are not directly observable, they can be inferred through model simulations. In this way, each processing stage implemented in the formal model can be systematically evaluated via simulation to ensure it behaves in accordance with theoretical expectations.

The third subordinate step in developing the formal model consists of the specification of a link function that translates parameters representing unobservable psychological processes into observable behavior (see Step 2c in Table 3; Kellen et al., 2021).[3] This step allows us to assess how variations in individual parameters influence observable behavior. That is, each parameter should be systematically manipulated in simulation studies to assess its functional role. For example, increasing the value of the parameter representing activation should increase the simulated performance. Conversely, decreasing the value of the parameter representing activation should decrease the simulated performance. In this subordinate step, we can assess whether variation of activation and inhibition processes should lead to distinct predictions in simulated performance.

Steps 3 and 4: Task designs and piloting

Once we have tested the different variations in the parameters on simulated performance and have identified the attentional-control parameters, we should create experimental manipulations where these attentional-control parameters affect actual task performance. For example, if the simulations of Step 2 identify filtering as the relevant process in attentional control, we should create experimental manipulations where filtering affects performance in the tasks. In contrast, if global stopping is identified as the relevant process, we should create experimental manipulations where global stopping affects performance in the task. Ideally, in this step, we should be able to develop experimental manipulations in a task and to design

[3] To clarify the concept of a link function, let us illustrate this point with the diffusion model as an example. In this model, the Wiener distribution uses the drift rate, response caution and non-decision time parameters to predict response times and accuracy. In this example, the probability density function of the Wiener distribution serves as the link function.

different versions of this task. This could be done by developing an entirely new task or by improving a current task.

In Step 4, we should be able, in a virtuous cycle, to pilot the different versions of the task and to model the data using the formal models until we reach a version of the task that yields a large behavioral effect. According to the model, this effect should depend strongly on the degree of attentional control, so that differences in the size of the effect reflect individual differences in the attentional-control parameter(s). In an optimal scenario of this step, there should be a large behavioral effect of attentional control at the group level. At the same time, there should be enough variability in this behavioral effect at the individual level to result in good reliability. Step 4 may also help to determine whether any behavioral measures capture attentional control sufficiently well. Typically, response times and/or accuracy are used to measure performance. From previous research (e.g., Draheim et al., 2019; Rey-Mermet et al., 2025), we know that some criteria are crucial in the selection of a good behavioral measure. As discussed in previous sections, the behavioral measure should have low measurement error and high true variability, and it should take into account individual differences in the speed-accuracy trade-off. A behavioral measure may ease the use of the task in more applied research. It is to note that based on the current research outlined in the methodological challenges, Step 4 represents the most challenging component of our approach.

Steps 5 and 6: Parameter recovery and experimental validation

The next step – that is, Step 5 of the proposed approach – consists of assessing parameter recovery by simulating performance for the tasks developed in Steps 3 and 4 from the model. When we fit the model to these simulated data, we can check whether we can recover the parameter values that were used. If the recovery study is successful, we can implement Step 6.

That is, we can use the model to estimate model parameters from actual behavioral data. This has two advantages. First, it allows us to validate the model by assessing whether the experimental manipulations selectively influence the model parameters representing the corresponding cognitive processes (Lerche & Voss, 2019). For example, experimental manipulations of attentional control should not affect parameters reflecting response conservativeness. Second, it allows us to draw substantive conclusions about the relevant cognitive processes from the data. We may, for example, assess which experimental manipulations have the largest effect on model parameters, or – and this is particularly relevant to the study of individual differences – which lead to the largest variability of the effect in performance across individuals.

Step 7: Developing further tasks

The development of one attentional-control task with a reliable and valid behavioral measure leads to Step 7 of the proposed approach. This step consists of developing further tasks that are then used in Step 8 to determine the convergent and discriminant validity of attentional-control measures. In Step 7, close variants of the initial task should first be developed in order to test whether attentional control can be generalized across small variations in the design of the initial task. Second, further attentional-control tasks, which are more different from the initial task, should be developed in order to investigate the limits of the generalization. But what does it mean to develop close variants of the task or different tasks? To illustrate this, we can take the example of working-memory tasks. For instance, two typical working-memory tasks are the complex-span task and the updating task (see Lewandowsky et al., 2010, for specific task designs). In the complex-span task, participants are asked to memorize items. Presentation of the memoranda is interleaved by a second decision. In the updating task, participants are first asked to memorize items and to update these items in the following updating steps. Both tasks can be implemented with different types of stimulus materials (e.g., words, numbers, matrices). Thus, for example, the complex-span task with words as material is a close variant of the complex-span task with numbers as material. At the same time, the complex-span task can be considered different from the updating task because they involve different instructions and goals. However, both tasks load strongly on a factor for working-memory capacity, showing that this construct generalizes across both tasks (e.g., Oberauer et al., 2000, 2003).

Step 8: Testing convergent and discriminant validity

Once we have developed several attentional-control tasks with reliable and valid measures, we can perform Step 8 to test convergent and discriminant validity. Here, we may use the approach of correlating model parameters or substituting model parameters. In addition, we may want to assess whether correlations can be established for behavioral performance itself, and not only for the model parameters.

Testing convergent and discriminant validity requires us to determine relationships between attentional control and other constructs. So far, previous research has used different target constructs to test these validity criteria or properties. For example, one of these target constructs is processing speed. This construct has been assumed to be different from attentional control (cf. Mashburn et al., 2023). Thus, we would expect to observe, at the performance level, higher correlations among attentional-control measures than between the attentional-control measures and processing-speed measures (e.g., Burgoyne et al., 2023; Draheim et al., 2021, 2024).

Another common target used to establish convergent and discriminant validity is working memory. If working memory is assumed to be different from attentional control, we would expect a similar pattern as for processing speed. That is, higher correlations would be expected among attentional-control measures than between attentional-control measures and working-memory measures. However, most researchers have argued for a close relationship between both constructs (e.g., Baddeley & Hitch, 1974; Miyake & Shah, 1999). Some have even argued that individual differences in attentional control explain individual differences in working memory, (e.g., Engle, 2002, 2018). In this case, it is unclear to what extent the correlations among attentional-control measures should be different from those between attentional-control and working-memory measures. Thus, it would be difficult to establish discriminant validity with the construct of working memory. However, once these validity criteria would be established with other constructs, we can use these measures to test the different theoretical assumptions linking individual differences in attentional control and working memory.

Step 9: Testing the dimensionality of attentional control

Testing the convergent and discriminant validity of the attentional-control measures leads us to the final step of our approach. This Step 9 consists of determining the dimensionality of attentional control. As stated in the introduction, previous research has suggested different conceptualizations of the dimensionality of attentional control (see, e.g., von Bastian et al., 2020). In these conceptualizations, attentional control has been proposed as: (1) a unitary construct (see, e.g., Engle, 2002, 2018), (2) different constructs, which are strongly associated with each other (e.g., Miyake et al., 2000), (3) a general construct with specific subconstructs (e.g., Miyake & Friedman, 2012), (4) different constructs, which are not associated with each other, or are associated very loosely (e.g., Conway et al., 2021), and (5) task-specific processes (Rey-Mermet et al., 2018, 2025). Although these conceptualizations have been developed using the typical attentional-control measures with issues in reliability and validity, they can serve as a starting point to test the dimensionality of attentional control with the newly developed tasks. In the proposed approach, these different conceptualizations of attentional control may be related to different architectures of the formal models. This means that if we develop a model of attentional control with N distinct attentional-control parameters, then these parameters may vary independently across participants. In this case, this would imply that attentional control has N distinct dimensions. If several models are required to account for performance in different attentional-control tasks, then all distinct attentional-control parameters from all models represent distinct dimensions of attentional control. To ensure that the dimensions of attentional control are distinct, the relationship between attentional-control parameters should be confirmed using correlations and structural equation modeling for both model parameters and behavioral measures.

Based on the five different conceptualizations presented above, we describe ideal scenarios. These distinguish between five different architectures of formal models and five different correlational patterns. These scenarios are summarized in Table 4.

Table 4

Overview of the Different Conceptualizations of Attentional Control and the Expected Patterns in Formal Models, in Correlational, and in Structural Equation Modeling.

Conceptualization	Attentional-control model parameters across tasks	Correlations	Latent construct of attentional control
Unitary construct	Same	High	One latent construct
Different constructs, which are strongly associated	Several	Moderate	Different but related latent constructs

Conceptualization	Attentional-control model parameters across tasks	Correlations	Latent construct of attentional control
General construct with specific subconstructs	Several	Moderate	A bifactor model with a general factor and one or several subconstructs
Different constructs, which are not associated / associated very loosely	Different	Low to moderate	Different unrelated latent constructs
Task-specific processes	Different	Low	No latent construct

First, if attentional control is conceptualized as a unitary construct (see, e.g., Engle, 2002, 2018), we would expect that attentional control is represented by *the same model parameter* across tasks (e.g., the parameter representing inhibition). As a consequence, the correlations among the model parameters and among the behavioral measures should be high, and structural equation modeling should identify one latent construct of attentional control.

Second, if attentional control is conceptualized as different constructs, which are strongly associated with each other (e.g., Miyake et al., 2000), we would expect that attentional control is represented by *several parameters* across tasks (e.g., the parameters representing inhibition and activation). As a consequence, the correlations among the model parameters and among the behavioral measures should be moderate, and structural equation modeling should identify different but related latent constructs of attentional control.

Third, if attentional control is conceptualized as a general construct with specific subconstructs (e.g., Miyake & Friedman, 2012), we would expect a similar pattern of results as in the previous case with one exception. Structural equation modeling should identify a bifactor or hierarchical model with a general factor and one or several subconstructs.

Fourth, if attentional control is conceptualized as different constructs, which are not associated with each other, or are associated very loosely (e.g., Conway et al., 2021), we would expect that attentional control is represented by *different parameters* across tasks (e.g., the parameter representing inhibition for one task and the parameter representing activation for another task). As a consequence, the correlations among the model parameters and among the behavioral measures should be low to moderate, and structural equation modeling should identify different unrelated constructs of attentional control.

Finally, if attentional control is conceptualized as task-specific processes (Rey-Mermet et al., 2018, 2025), we would expect that attentional control is represented by *different parameters* across tasks (e.g., the parameter representing inhibition for one task and the parameter representing activation for another task). However, in this case, the correlations among the model parameters and among the behavioral measures should be low, and structural equation modeling should identify no latent construct of attentional control.

In sum, the dimensionality of attentional control would be tested using not only the correlational approach but also the architecture of formal models. Thus, Step 9 may represent a clarification of the long-standing debate about the dimensionality of attentional control.

Conclusion

Overall, the debate in recent years around the construct of attentional control has been dominated by methodological challenges such as high measurement error and the neglect of individual differences in speed-accuracy trade-offs. However, to advance the field as a whole, we suggest shifting our attention to some degree to theoretical challenges. Here, we propose an approach using formal modeling to ensure that newly developed tasks of attentional control incorporate not only methodological improvements but are also valid. Only if validity is at the beginning of the research process can we ensure that we actually measure attentional control and no other psychological constructs. Critically, it is not enough to empirically gauge convergent and discriminant validity. These validity criteria or properties cannot tell us what the measures reflect without theoretically relating them to the to-be-assessed construct. This is particularly problematic for attentional control because it is difficult to isolate attentional control from non-attentional-control processes and measurement error. Therefore, establishing the validity of an attentional-control measure is not exclusively an empirical process, but there needs to be a theoretical understanding of what the measure is actually measuring (Borsboom et al., 2004).

The proposed approach has two key goals. First, it is to develop a series of theoretically sound attentional-control measures based on formal models and experimental tasks. Second, it is to determine the dimensionality of attentional control. If we achieve these goals, the next question we should ask concerns the utility of attentional control in everyday life. Which aspects of everyday life can be predicted by our attentional-control ability? There is no obvious answer to this question. Some research addressed the question whether attentional control is related to measures of impulsive behavior in everyday life, such as psychopathology, including ADHD (Barkley, 1997), conduct disorder (Friedman et al., 2020), addiction problems (Eisenberg et al., 2019; Verdejo-Garcia et al., 2021) and obesity (Eisenberg et al., 2019). However, this research is based on measures where reliability and validity might be low. Therefore, these relationships need to be clarified, not only on the empirical level but also on the theoretical level. For example, it is far from clear that attentional control should be related to behaviors such as substance abuse and aggressive behavior. Friedman and Gustavson (2022) discuss a number of reasons why we should not expect a strong relationship.

In the search for good measures of attentional control predicting aspects of everyday life, we need to ensure that we are not reinventing a measure of intelligence. The concept of intelligence refers to the ability to understand complex ideas, to adapt effectively to the environment, to learn from experience, and to engage in various forms of reasoning (e.g., Neisser et al., 1996). Some researchers have postulated the hypothesis that attentional control is closely related to intelligence (e.g., Engle, 2002, 2018; Engle et al., 1999). Based on this hypothesis, the size of the correlation with intelligence has been used as a criterion for selecting measures of attentional control (Draheim et al., 2021). However, the hypothesis linking attentional control and intelligence remains controversial (for dissenting views, see Buehner et al., 2005; Chuderski et al., 2012; Rey-Mermet et al., 2019). Therefore, using measures of intelligence as a criterion for selecting attentional-control measures is problematic. Moreover, if we then use the measures of attentional control to test the hypothesis relating attentional control to intelligence, we complete the loop of circular reasoning.

To sum up, the field of attentional control is at a turning point. By clarifying the theoretical challenges of attentional control, we should be able to fix the methodological challenges and therefore achieve a good measurement of attentional control. To achieve this, we need to extend Cronbach's ideas of merging correlational and experimental methodology (Cronbach, 1957) to incorporate theory building in the form of formal modeling.

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