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ORIGINAL ARTICLE

Individual differences in conflict-monitoring: testing means and covariance hypothesis about the Simon and the Eriksen Flanker task

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Abstract Conflict and context slow-down have been proposed as indicators of a conflict-monitoring system that initiates cognitive control to resolve conflicts in information processing. We investigated individual differences in conflict-monitoring and their associations with working memory (WM) and impulsivity. A total of 150 adults completed a Simon and an Eriksen flanker task, together with measures of WM and impulsivity. On both tasks, responses were slower and less accurate on incompatible than on compatible trials (conflict effect), and the conflict effect was larger when the preceding trial was compatible than when it was incompatible (context effect). Stimulus repetition did not explain the context effect. Individual differences could be attributed to three separable factors for each task: general speeded performance, conflict effect, and context effect. Evidence for across-task generality of these factors was sparse. Associations of these factors with impulsivity were weak at best. WM was correlated with general speed, and also with some but not all factors reflecting conflict-related processes.

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Introduction

Conflicts, the existence of two or more competing response alternatives, are incidents our cognitive system experiences frequently in day to day life. Cognitive control, and control of cognitive control, has been argued to rely on the detection of conflicts (Botvinick, Carter, Braver, Barch, & Cohen, 2001). Botvinick et al.'s (2001) theory on conflict monitoring assumes "a system that monitors for the occurrence of conflicts in information processing" (p. 625). The conflict monitoring system further "evaluates current levels of conflict, then passes this information on to centers responsible for control, triggering them to adjust the strength of their influence on processing." (p. 625). Botvinick et al. (2001) suggest that the anterior cingulate cortex (ACC) is the brain region most sensitive to conflict signals. The ACC serves as a detection unit and recruits cognitive control in order to resolve the detected conflict.

Experimental paradigms that incorporate response conflict, e.g. the Stroop, Eriksen, or Simon tasks, have been used to test the conflict monitoring hypothesis. ACC activation linked to incompatible trials indicating conflict sensitivity was repeatedly found for the Eriksen paradigm (Botvinick, Nystrom, Fissel, Carter, & Cohen, 1999; van Veen, Cohen, Botvinick, Stenger, & Carter, 2001). ACC activity along with response conflict was also observed with different variants of the Stroop tasks (Barch, Braver, Akbudak, Conturo, Ollinger, & Snyder, 2001; Kerns, Cohen, MacDonald, Cho, Stenger, & Carter, 2004) and the Simon task (Peterson, Kane, Alexander, Lacadie, Skudlarski, Leung, May, & Gore, 2000; see Botvinick, Cohen, & Carter, 2004 for a review).

The Eriksen as well as the Simon task have been repeatedly used for the study of conflicts. In a prominent version of the Eriksen task a left or right pointing target arrow is flanked with additional arrows pointing in the same direction (compatible condition) or the opposite direction (incompatible condition). The target stimulus requires a key press labeled and located in correspondence to the target arrow. The occurrence of incompatible flanker stimuli leads to a slow-down in reaction time (RT). In a prominent version of the Simon task, one or the other of two shapes, e.g. a circle and a rectangle, appear in a random sequence above or below a centered fixation point. The response keys in such a vertical version of the task would be located vertically to each other, e.g. the up key for the rectangle and the down key for the circle. The compatible condition, where the shape occurs in a location that matches the location of the response key (i.e., a rectangle in the upper half of the screen, or a triangle in the lower half) leads to a facilitation of responses, first observed by Simon (1969).

The above phenomena reflect modulation of RT dependent on stimulus–stimulus compatibility (Erikson task) or stimulus-response compatibility (Simon task). These compatibility effects have been explained through dual route models of information processing (Kornblum , Hasbroucq, & Osman, 1990; De Jong, Liang, & Lauber, 1994). These models distinguish a conditional and an automatic route of information processing operating in parallel. The conditional route is characterized by rule-based information processing, top–down control, and is slower than the automatic route, which refers to bottom–up processing based on pre-existing stimulus-response (S-R) associations caused by dimensional overlap between S-R sets (Kornblum et al., 1990). Response selection is swift and flawless if both routes support the same response. However, if the outcomes of the two processes differ, the incongruity of the outcome has to be resolved by additional cognitive effort. This causes a deceleration and increased error proneness of the response. Incompatible information in the Eriksen (flankers pointing to the opposite direction) and Simon task (stimulus and associated response at opposing locations) leads to a wrong response tendency arising from the automatic route, which results in conflict with the conditional route. The need to resolve the conflict leads to slower responses in incompatible trials.

Conflict effects in ACC activation could be shown to be at least partly sequence modulated (Gratton, Coles, & Donchin, 1992; Botvinick et al., 1999; Kerns et al., 2004). The modulation of conflict effects indicates a dependence of RT and ACC activation in trial n on the level of conflict in the preceding trial n-1. Compatible trials following compatible trials (cC) receive an additional acceleration compared to compatible trials preceded by incompatible trials (iC). In contrast, incompatible trials following compatible trials (cI) are slower compared to those preceded by incompatible trials (iI). This sequential dependency effect can be explained with the conflict monitoring hypothesis and extended adjustment in cognitive control on situational demands: constant monitoring leads to an increase of cognitive control after incompatible trials, advantageously for forthcoming incompatible trials (the automatic route is blocked), but adverse for performance in upcoming compatible trials (there is no benefit from the automatic route).

However, this explanation of sequence modulated conflict effects as dynamic adjustments of cognitive control has been challenged. Mayr, Awh and Laurey (2003) argued that the sequential dependency effect in the Erikson task can be attributed to repetition priming from identical stimulus configurations in consecutive trials, which would lead to the same expected RT pattern. This issue was addressed by several studies that still found sequential modulation effects even when effects of stimulus priming were controlled for (Ullsperger, Bylsma, & Botvinick, 2005; Wühr & Ansorge, 2005; Kunde & Wühr, 2006; Freitas, Bahar, Yang, & Banai, 2007). Further investigations (Notebaert, Gevers, Verbruggen, & Liefooghe, 2006) extended these findings and showed that stimulus priming is a process that can be considered bottom–up whereas conflict adaptation adjustments are rather top–down. Experimental evidence suggests that an inhibitory mechanism affects the automatic route dependent on the correspondence of the preceding trials (Stürmer, Leuthold, Soetens, Schröter, & Sommer, 2002). These results indicate that stimulus priming alone cannot explain the sequential dependency effect.

Although experimental and psychophysiological research on the idea of cognitive control through conflict detection is booming, individual differences within the theoretical framework of conflict-monitoring have received very little attention so far. Based on the framework of conflict-monitoring individual differences could be due to several sources.

First, subjects could differ in their sensitivity to detect conflict or in their evaluation of the strength of conflicts. Such differences could distinguish highly attentive subjects with a very “cautious and active” monitoring system from inattentive subjects with a “relaxed or passive” monitoring system. Stürmer et al. (2002) could show that the amount of conflict and of conflict adaptation were influenced by the proportion of compatible stimuli in the experiment; when most trials are compatible, the conflict and conflict adaptation effects on incompatible stimuli are larger. This indicates that conflict monitoring is best understood as a process varying in intensity rather than a status with only on/off-alternatives. Such results as reported by Stürmer et al. (2002) give credibility to the idea of individual differences in conflict-adaptation. Other possibilities for differences in experienced strength of conflict could be due to personality factors or learning history. A strong tendency to avoid mistakes could be indicative of conscientiousness.

It is also possible to understand impulsivity as the other end of the conscientiousness dimension. Impulsivity can be defined as a preference for reflexive behaviour over well-reasoned responses and a tendency to react fast to unknown stimuli (Cloninger, Svrakic, & Przybeck, 1993). Impulsivity has been linked to information processing deficits expressing a lack of inhibitory control (Enticott, Ogloff, & Bradshaw, 2006). This inhibitory lack leads to faster responses and the inability to overrule prepotent response tendencies. Enticott et al. (2006) confirmed positive associations between Stroop costs and impulsivity factors. In terms of psychophysiological predictions a poorly active ACC would be associated with poor conflict detection and thus to an inability to inhibit prepotent responses leading to the impulsive response style just described (Bush et al., 1999; Stahl & Gibbons, 2007).

A second process susceptible to individual differences is the recruitment of cognitive control, which could vary across subjects in its intensity or efficacy. The concept of cognitive control as referred to in conflict-monitoring theory can be circumscribed as the enforcement of controlled information processing and the inhibition of inappropriate and task irrelevant information. Some researchers (Engle & Kane, 2004; Engle, 2002; Engle, Kane, & Tuholski, 1999) have argued that cognitive control is closely related to the construct of working memory (WM). Engle et al. argue that WM reflects the ability to focus attention and to inhibit task-irrelevant information that interferes with current goals. Importantly, individual differences in WM have been linked to individual differences in fluid intelligence and reasoning (Conway, Kane & Engle, 2003; Kyllonen, 1996; Oberauer, Süß, Wilhelm, & Sander, 2007) and many research groups place WM as a core variable in the center of human cognitive abilities (Oberauer, Schulze, Wilhelm, & Süß, 2005; Engle, Tuholski, Laughlin, & Conway, 1999; Kyllonen & Christal, 1990).

Several studies support the idea that WM is more strongly associated with performance in high-conflict trials than it is with performance in low-conflict trials. Kane and Engle (2003) showed that subjects low in WM (low spans) exhibited larger RT facilitation and committed more errors than high spans in a Stroop task. Poorer performance of low spans was also observed in an antisaccade task, high spans being faster and more accurate (Kane, Bleckley, Conway, & Engle, 2001). Osaka et al. (2003) found an increase of ACC activation in high spans with rising WM demands compared to low spans, which the authors interpreted as indicative of a more effective ability to manipulate attentional control in high spans.

The currently available results on individual differences in conflict-monitoring are insufficient and call for a much stronger appreciation of individual differences in future research. We are unaware of studies using multivariate techniques to investigate and establish individual differences in conflict tasks. In order to establish reliable and valid models of individual differences in conflict monitoring such methods are mandatory. This study aims to (a) bridge methods from classical experimental research and individual difference research relying on well studied conflict paradigms, the Simon and the Eriksen task, (b) measure the constructs in question on a multivariate level and to (c) connect individual differences in these constructs to individual differences in relevant criteria.

Based on the conflict-monitoring model of Botvinick and colleagues, we formulate the following assumptions and hypotheses about individual differences in indicators of conflict and control:

(1) Trials with low conflict (i.e., compatible trials) are completed faster and with fewer errors than trials with high conflict (i.e., incompatible trials). The size of this conflict effect varies across individuals. We assume that its size depends on two hypothetical person variables, the amount of interference a person experiences in incompatible trials from the irrelevant information (e.g., flankers in the Eriksen task), and the amount of cognitive control recruited in response to detection of that interference.

(2) Because control settings established in response to conflict on one trial perseverate into the following trial, we expect sequential-dependency effects, also known as context slow-down. Specifically, we expect the RT pattern cC < iC < iI < cI, and the reverse for accuracies. This pattern should be found even when repetition priming (Mayr et al., 2003) is controlled. Again, we expect individual differences in the size of that effect. We assume that the size of the effect again depends on two hypothetical person variables, the amount of cognitive control recruited on incompatible trials, and the tendency to maintain these control settings across trials, rather than resetting them to a neutral value after each trial.

(3) From the above (1) and (2) it follows that conflict effects and context effects should be moderately correlated, because they share variance of one person variable, the amount of cognitive control recruited in response to conflict. Individuals with a disposition to react strongly to perceived conflict by mustering a high degree of cognitive control will show relatively small conflict effects but relatively large context effects. Therefore, we expect a moderately negative correlation between conflict effects and context effects.

(4) If working memory capacity reflects to a large degree the ability to control cognition and action in situations of high interference or conflict, as suggested by Engle, Kane, and their colleagues, then individual differences in conflict effects should correlate with working memory capacity. Specifically, high-capacity individuals

should recruit larger amounts of cognitive control and therefore show smaller conflict effects, possibly accompanied by larger context effects.

(5) One implicit assumption of the theories of Botvinick et al., as well as of Engle et al., is that the ability to monitor and to control conflict generalizes, at least to some degree, across tasks and situations. Therefore, it should be possible to identify a sizeable amount of variance in conflict and context effects that is shared across several conflict tasks (such as the Eriksen flanker task, the Stroop task, and the Simon task).

Method

Participants

A total of 150 participants aged between 18 and 36 were recruited via university e-mail list server and community newspaper advertisements. All participants received a payment of 15 € and were offered feedback about their performance.

Two persons were dropped from all further analysis because they displayed unreasonably long RT in all tasks compared to other participants, questioning their compliance with or understanding of instructions. All analyses are based on the data from the remaining 148 persons (53 males). Participants had a mean age of 24.6 years (SD 3.8 years).

Measures

Five left or right pointing arrows were used as stimuli for the Eriksen task. Participants were required to indicate the direction of the middle (3rd) arrow pointing left or right. The flanker arrows all pointed in the same direction, either the same direction as the target arrow (compatible) or in the opposite direction (incompatible). The arrows appeared in the centre of the screen and measured each about 50×10 mm (width $\times$ length). Participants responded by pressing one of two horizontally arranged keys, the left key for a left-pointing target arrow, or the right key for a right-pointing target arrow.

Stimuli for the Simon task were diamonds or squares. Both shapes were of the same size (30×30 mm) and appeared above or below a centred fixation cross that remained on the screen until the response. Participants responded by pressing one of two keys vertically arranged on a custom built keyboard; the upper key was labelled with an upward pointing arrow, and the lower key with a downward pointing arrow. The diamond shape was assigned to the upper key, and the square to the lower key. Thus, a diamond in the upper half of the screen, or a square in the lower half, were compatible trials, whereas a diamond in the lower half and a square in the upper half were incompatible stimuli.

The trial and block settings were identical for the Simon and the Eriksen task. Each trial started with a centred fixation cross, followed by the target stimulus after 500 ms. The target stimulus remained on the screen until the participant responded with a valid key press. The interval between a response and the next fixation cross interval was set to 1,000 ms.

Altogether 1,254 stimuli were presented for each task, organized in 38 blocks of 33 stimuli preceded by four practice blocks of 12 stimuli not included in analyses. The first stimulus was counted as warm-up and was also not included in the analyses. Feedback was given after every block indicating average performance in milliseconds and percentage correct. In order to reduce ceiling effects and individual differences in speed-accuracy-trade-off settings, feedback was adjusted to subjects' performance. Participants were told to be faster, if their percentage correct exceeded 87% (four errors per block), stay the same if their performance was 87% correct and become more accurate if their performance dropped below 87% correct.

The experimental design was a two (current trial: compatible vs. incompatible; indicated by a capital C or I) by two (previous trial: compatible vs. incompatible; indicated with a lower-case c or i) by two (stimulus sequence: identical vs. non-identical stimuli) within subject-factor design for both tasks. Stimuli for each condition were balanced, resulting in 152 stimuli per condition.

Measures of WM included the rotation span, memory updating, and counting span task (Wilhelm & Oberauer, 2006). In the rotation span task participants had to memorize a sequence of arrows that vary in length and pointing direction while deciding whether or not rotated letters were mirror-reversed (Shah & Miyake, 1996). In the memory-updating task, participants had to memorize numbers and their positions in a grid, and update the numbers by applying simple arithmetical operations to them (Oberauer et al., 2000). In the counting span task participants counted sets of blue circles and memorized each count, while being distracted by blue squares and green circles. They decided whether each count was odd or even, and recalled all counts at the end of a series of varying length. All tasks were fully computerized.

Participants also worked on a demographic questionnaire and an abbreviated version of the UPPS (urgency, premeditation, perseverance, and sensation seeking) scales by Whiteside and Lynam (2001) to measure four dimensions of impulsivity (see Keye, Wilhelm, & Oberauer, in press, for details). UPPS incorporates the scales urgency (e.g. “I have trouble controlling my impulses.”), premeditation (e.g. “I like to stop and think things over before I do them.”), perseverance (e.g. “I finish what I start.”), and sensation seeking (e.g. “I generally seek new and exciting experiences and sensations.”), each measured with five items. Answers were given on a five-point rating scale ranging from “strong disagreement”, “disagreement”, “neutral”, “agreement”, to “strong agreement”.

The measures were administered in the following sequence: demographic and UPPS questionnaire, rotation span task, Eriksen task, memory updating task, break, Simon task, and counting span task. The experiment lasted about 150 min including a 10 min break after approximately 80 min. All measures were programmed with Inquisit 2.0© and run on identical laptop computers equipped with 12.1’’ Color TFT displays. The resolution was set to 1,024 × 768 pixels. A custom-made keyboard was used as input device.

Data analyses

RTs of wrong responses were not included in the analyses of the Eriksen and Simon tasks. A trimming procedure was applied eliminating all unreasonably long or short RTs. Boundaries for trimming were set to 150 ms at the lower end, and to the individual mean RT plus 3.5 individual standard deviations (SD) at the upper end. This procedure was run repeatedly until no RTs were excluded by the procedure. Trimming was always done separately for each person and task. All RT data analyses are based on this trimmed data set.

All WM tasks were scored with a partial credit scoring procedure (Conway, Kane, Bunting, Hambrick, Wilhelm, & Engle, 2005). In the counting span and rotation span tasks participants had to be correct in at least 80% of the secondary tasks (odd–even decision and mirror-reversal decision, respectively) to ensure that participants had worked on the secondary task sufficiently to affect performance on the primary task. This was not achieved by one participant for the rotation span task leading to the deleting of this rotation span score. Missing values were substituted by Missing Value Analysis (SPSS 12.0) using the estimation maximization procedure. Little’s Missing Completely at Random test (MCAR, Little, 1988) was run to ensure that the missings occurred completely at random ($\chi^2 = 0.3$, df = 2, $p = 0.85$). Therefore, the missings were replaced through an EM estimation procedure.

We analyzed RT and accuracy data separately through structural equation models (SEM). We used a model structure that is particularly suited for analyzing individual differences in experimental effects (Oberauer, Wilhelm, & Schmiedek, 2005). A general factor represents the variance in all processes that are common to the experimental and the control conditions; all variables have loadings on this factor, and these loadings are constrained to be equal. For each experimental condition, a specific factor is added on which only the variables from that experimental condition have free loadings. In our case, we added a specific conflict factor on which all variables from incongruent conditions loaded, while loadings of variables from congruent conditions were fixed to zero. A second specific factor was introduced to represent context effects; all variables from congruent trials following incongruent trials, and incongruent trials following congruent trials, have free loadings on this factor, whereas all other variables have zero loadings on it. This model structure is illustrated in Fig. 1.

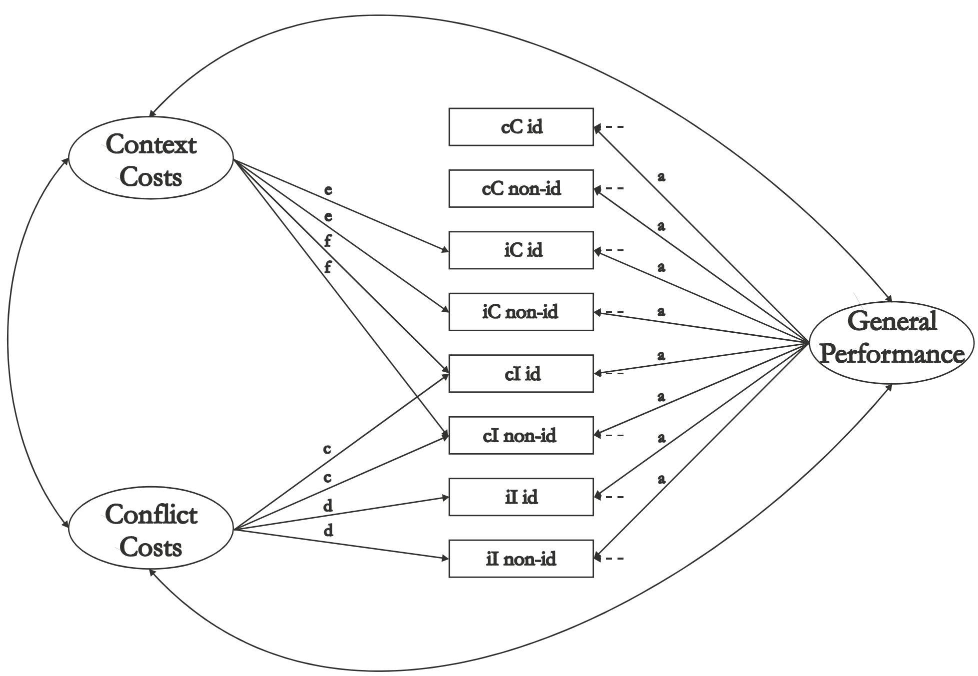

Fig. 1 Illustration of Model 2 note. *Lower-case characters* indicate previous trial compatibility, *upper-case characters* indicate trial compatibility, and non-id/id represents stimulus repetition. *Dotted arrows* indicate residual terms

Structural equation models were run with AMOS 7.0© using ML-estimation. Adequacy of models was judged by inferential and descriptive fit indices focusing on the Comparative-Fit-Index (CFI) and Standardized-Root-Mean-Square-Residual (SRMR) (Hu & Bentler, 1999).[1] We used the χ^2 difference to test for significant differences between competing models.[2] Reliability estimates are provided for latent factors given by ω which indicates the variance a latent factor represents considering the total variance of a scale, i.e. composite reliability (McDonald, 1999).

Results

Descriptive statistics for the Eriksen and the Simon task are presented in Table 1. An ANOVA was run to estimate the magnitude of effects of independent variables. Effect sizes of experimental factors are expressed by partial η^2 (Pierce, Block, & Aguinis, 2004), the proportion of experimental effect plus error variance that is attributable to the experimental factor. There were large main effects of trial compatibility for RT and accuracies in both the Eriksen and the Simon task. Effects of previous trial compatibility were rather small for RTs in the Eriksen compared to the Simon task, though for both tasks effects were much smaller than effects of trial compatibility. Regarding accuracies, effect size of previous trial compatibility was larger than in RT data for both tasks. There were also interaction effects found for previous trial and current trial compatibility in all tasks for RTs and accuracies. Effects for stimulus repetition were generally small and completely absent in the accuracy data of the Simon task. Therefore, the sequential dependency (or context) effects cannot be attributed to repetition priming, as conjectured by Mayr et al. (2003). The results of the ANOVA are summarized in Table 2.

The feedback provided after each block successfully helped to keep the level of mean accuracies at a level of around 90% correct responses. Descriptive and inferential results also confirm that feedback did not impair experimental effects of the independent variables on accuracies. We therefore will report further analyses of individual differences separately for RT and accuracy data.

Table 1 Descriptive statistics for the Eriksen and the Simon task

	RT		Accuracy	
	Mean	SD	Mean	SD
Eriksen task				
Identical				
cC	401	31	0.98	0.03
iC	414	34	0.97	0.04
cI	495	62	0.83	0.09
iI	486	50	0.89	0.07
Non-identical				
cC	396	32	0.98	0.03
iC	412	35	0.98	0.03
cI	494	57	0.83	0.09
iI	486	50	0.89	0.06
Simon task				
Identical				
cC	402	42	0.96	0.04
iC	432	48	0.93	0.04
cI	464	52	0.81	0.08
iI	444	48	0.91	0.06
Non-identical				
cC	400	43	0.96	0.04
iC	429	45	0.93	0.05
cI	460	49	0.82	0.08
iI	441	47	0.92	0.05

Reaction-time data

A prerequisite for CFA (confirmatory factor analyses) modelling with ML-estimation are normally distributed data. RT data are usually heavily skewed in their frequency distribution (Ratcliff & Murdock, 1976; Heathcote, Popiel, & Mewhort, 1991). Therefore, all RT data were log-transformed to approximate normal distributions.

In a first step, measurement models for these data were established for the Eriksen and the Simon task separately. A general factor was assumed covering general performance variance in each task. These simple general-factor models did not fit well (Eriksen task: $\chi^2 = 778$, df = 20, $p < 0.01$, CFI = 0.74, SRMR = 0.10, Simon task: $\chi^2 = 320$, df = 20, $p < 0.01$, CFI = 0.89, SRMR = 0.02). In a second step, experimental factors representing the manipulations of conflict (i.e., whether the current trial is compatible or incompatible) and for context (i.e., whether the preceding trial's compatibility matched that of the current trial) were introduced. The conflict factor represents individual differences in the size of the conflict effect, and the context factor represents individual differences in the size of the context

[1] The RMSEA will not be considered for evaluating fit here, because of its known sensitivity to sample size and number of variables leading to a tendency to overreject models (Hu & Bentler, 1999; Kenny & McCoach, 2003).

[2] When two models are nested, that is, one is a special case of the other obtained by fixing some of the free parameters, the difference in χ^2 can be used to assess whether the less constrained model (i.e., the model with more free parameters) has a significantly better fit. This difference, expressed as $\Delta\chi^2$, is evaluated for significance, using the difference of df of the two models as df (this difference is written Δdf). A significant $\Delta\chi^2$ is evidence that the freely estimated parameters in the less constrained model differ significantly from their fixed values in the more constrained model, and the less constrained model is preferred. Otherwise, the more constrained model is preferred because it is more parsimonious.

Table 2 Results of ANOVA for RT and accuracy data from the Eriksen and the Simon task

	Eriksen task									
	RT					Accuracies				
	F	df_{hypo}	df_{err}	p	Partial η^2	F	df_{hypo}	df_{err}	p	Partial η^2
(1)Trail comp.	981	1	147	0.00	0.87	482	1	147	0.00	0.77
(2)Previous trial comp.	27	1	147	0.00	0.15	233	1	147	0.00	0.61
(3)Stimulus repetition	23	1	147	0.00	0.14	21	1	147	0.00	0.13
(1) × (2)	214	1	147	0.00	0.59	285	1	147	0.00	0.66
(1) × (3)	16	1	147	0.00	0.10	0	1	147	0.68	0.00
(2) × (3)	7	1	147	0.01	0.05	0	1	147	0.81	0.00
(1) × (2) × (3)	6	1	147	0.02	0.04	0	1	147	0.88	0.00
	Simon task									
	RT					Accuracies				
	F	df_{hypo}	df_{err}	p	Partial η^2	F	df_{hypo}	df_{err}	p	Partial η^2
(1)Trail comp.	922	1	147	0.00	0.86	526	1	147	0.00	0.78
(2)Previous trail comp.	82	1	147	0.00	0.36	294	1	147	0.00	0.67
(3)Stimulus repetition	27	1	147	0.00	0.15	1	1	147	0.39	0.00
(1) × (2)	745	1	147	0.00	0.84	493	1	147	0.00	0.77
(1) × (3)	0	1	147	0.59	0.00	5	1	147	0.02	0.03
(2) × (3)	0	1	147	0.55	0.00	0	1	147	0.68	0.00
(1) × (2) × (3)	1	1	147	0.43	0.00	1	1	147	0.47	0.00

effect (i.e., the degree to which cI trials are slower than iI trials, and the degree to which iC trials are slower than cC trials). The fit of Model 1 was acceptable for both tasks (Eriksen task: $\chi^2 = 28$, df = 11, $p < 0.01$, CFI = 0.99, SRMR = 0.00; Simon task: $\chi^2 = 36$, df = 11, $p < 0.01$, CFI = 0.99, SRMR = 0.01).

Because the ANOVA results indicate that there were essentially no mean effects for stimulus repetition, we constrained the unstandardized loadings of corresponding indicator pairs to be equal. Hence, indicators only differing in stimulus repetition were treated as akin to each other. Furthermore, we constrained indicators loadings in accordance to the expected variance based on the experimental manipulation. These constraints resulted in Model 2, shown in Fig. 1. Although the strict constraints of Model 2 lead to a decrease in fit, it still fits acceptable for both models (for the Eriksen task, $\chi^2 = 42$, df = 20, $p < 0.01$, CFI = 0.99, SRMR = 0.00; $\Delta\chi^2 = 14$, Δdf = 9; for the Simon task, $\chi^2 = 37$, df = 20, $p < 0.01$, CFI = 0.99, SRMR = 0.00; the latter was not significantly worse than the unconstrained Model 1; $\Delta\chi^2 = 1$, Δdf = 9).

Based on Model 2 for each task, we investigated whether the experimental factors could be removed. In the model for the Eriksen task, the exclusion of the conflict factor led to a dramatic loss of fit ($\Delta\chi^2 = 711$, Δdf = 4). Dropping the context factor also impaired model fit—though not nearly as dramatically ($\Delta\chi^2 = 130$, Δdf = 4). Dropping both factors lead to a completely unacceptable model ($\Delta\chi^2 = 1035$, Δdf = 7). Not all correlations between latent factors were significant. The general Eriksen RT-factor correlated at 0.12 (n.s., $\Delta\chi^2 = 2$, Δdf = 1 for restriction to zero) and 0.44 ($\Delta\chi^2 = 13$, Δdf = 1 for restriction to zero) with the conflict and the context factor, respectively. The Eriksen context and conflict factor showed a very high correlation at 0.76, restricting it to zero did lead to a significant loss of fit ($\Delta\chi^2 = 34$, Δdf = 1).

A similar pattern of results was found for the Simon task. Excluding the conflict factor ($\Delta\chi^2 = 245$, Δdf = 4) or the context factor ($\Delta\chi^2 = 116$, Δdf = 4) or both ($\Delta\chi^2 = 295$, Δdf = 7) again reduced the model fit considerably. Correlations between factors reached significance only for the context and the conflict factor at 0.48.

Taken together, Model 2 was accepted as a useful measurement model for both tasks. Based on the constrained measurement models for both tasks, their combination was tested. This integrated measurement model (Model 3) was estimated with saturated factor correlations (i.e., each factor was allowed to correlate freely with each other factor). Model 3 had a good fit ($\chi^2 = 141$, df = 95, $p < 0.01$, CFI = 0.99, SRMR = 0.01). Correlations of factors are summarized in Table 3. The context and the conflict factor correlate with each other for both tasks, though for the Simon task to a lower degree. Among the Eriksen-task factors, the significant correlation of the general RT factor with the context factor indicates that higher RTs in general go along with longer RTs in succeeding trials that are altered in trial compatibility compared to the preceding trial. The Eriksen

Table 3 Intercorrelations of RT Factors from Model 3

	Eriksen task factors			Simon task factors		
	General RT	Conflict	Context	General RT	Conflict	Context
Eriksen task factors						
General RT	1					
Conflict	0.12	1				
Context	0.46**	0.73**	1			
Simon task factors						
General RT	0.67**	−0.01	0.14	1		
Conflict	−0.07	0.14	0.07	−0.11	1	
Context	0.10	0.30**	0.39**	−0.06	0.48**	1

Table 4 Correlations of RT factors from the Eriksen and Simon task with WM and UPPS factors

	Eriksen task factors			Simon task factors		
	General RT	Conflict	Context	General RT	Conflict	Context
WM	−0.48**	−0.07	−0.18	−0.38**	−0.16	−0.22*
$\chi^2 = 208$, df = 144, $p < 0.01$, CFI = 0.99, SRMR = 0.03						
Urgency	0.03	0.05	0.11	−0.02	−0.03	0.03
Premeditation	0.05	0.06	−0.11	0.10	−0.02	−0.13
Perseverance	0.01	0.03	−0.15	0.10	−0.04	−0.06
Sensation seeking	−0.19*	−0.01	−0.02	−0.08	0.16*	−0.13
$\chi^2 = 185$, df = 140, $p < 0.01$, CFI = 0.99, SRMR = 0.01						

Significance of correlations is indicated by * for $p < 0.05$ and ** for $p < 0.01$

conflict and context factor are highly associated, pointing out that longer RTs in incompatible trials compared to compatible ones (conflict factor) go along with longer RTs in succeeding trials that are altered in trial compatibility (context factor). This correlational pattern is similar for the Simon task. Again, the context and conflict factor show a positive correlation, though to a lower degree than the Eriksen conflict and context factors. The general RT factor of the Simon task did not correlate with any experimental factor, neither the context nor the conflict factor.

With respect to the correlations between factors for different tasks, both factors for general RT performance correlate highly with each other, probably reflecting common variance due to the general speed of information processing. There was no association between the two conflict factors. The two context factors had a moderate positive correlation with each other. These findings question the generality of individual differences in conflict control.

Based on Model 3 associations with WM were tested in Model 4. A general factor model of WM with the three indicators counting span task, rotation span task, and memory updating task was added to Model 3.[3] Loadings on the WM factor were significant with 0.58 for counting span, 0.66 for rotation span, and 0.69 for memory updating. Negative associations with WM were found only for the general performance factors and the context factor from the Simon task (Table 4). These correlations seem to support the interpretation that subjects with higher scores in WM are generally faster and, and experience less context slowdown in the Simon task.

In order to assess incremental predictive power of the conflict and context factors over and above the two general task factors, a regression model was estimated to further understand associations between latent RT factors and WM (MacCallum, Wegener, Uchino, & Fabrigar, 1993). Instead of estimating correlations as in Model 4, in Model 4b the relations between Simon and Eriksen factors with the WM factor were estimated as regression relations ($\chi^2 = 198$, df = 137, $p < 0.01$, CFI = 0.99, SRMR = 0.03). The only significant path to explain variance in the WM factor was the Eriksen general RT factor ($r = -0.52$**). Obviously, the collinearity between the Eriksen and Simon factor keeps the general RT factor from the Simon task from showing a strong contribution to WM over and above the Eriksen factor. None of the conflict or context slow-down factors contribute significant unique variance to the prediction of WM (Fig. 2).

Associations with the UPPS factors were tested based on latent factor scores. Latent factor scores were estimated in a four-factorial measurement model (Keye, Wilhelm, & Oberauer, in press). We did not estimate a full structural model in order to keep model complexity within reasonable

[3] Fit for a separate measurement model of WM cannot be reported because with three indicators the measurement model is just identified with 0 degrees of freedom.

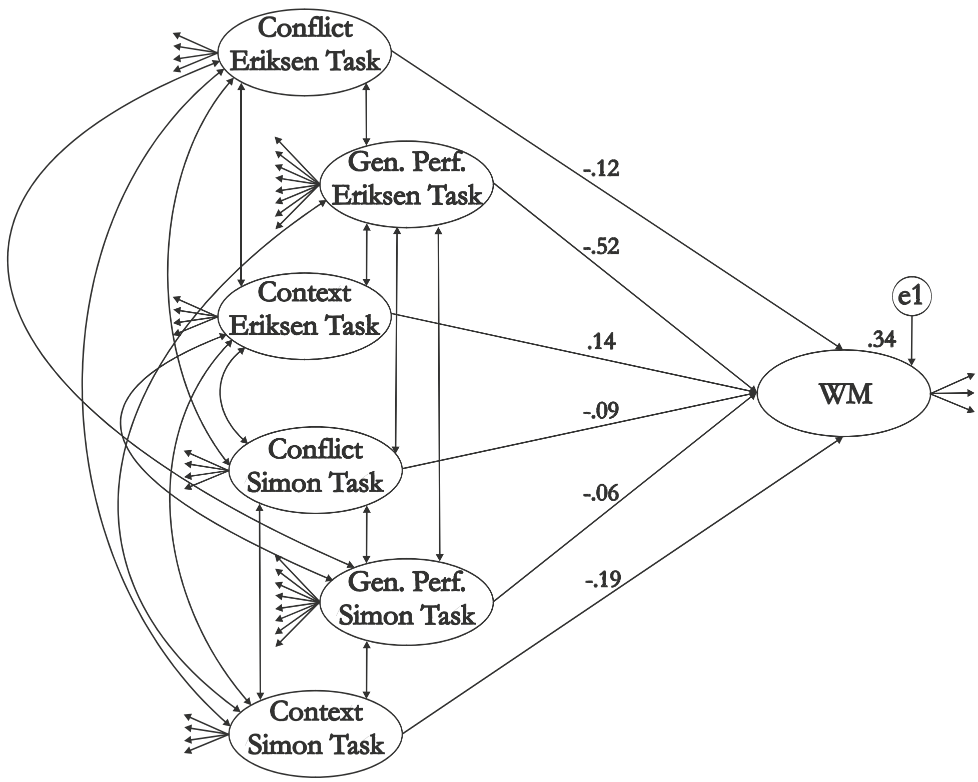

Fig. 2 Illustration of Model 4b. Loadings of indicators (see Appendix) and correlations between factors are not reported to keep the model readable

bounds in light of the modest sample size. The intercorrelated factor scores were introduced as manifest variables into Model 3, thereby leading to Model 5. Table 4 summarizes the correlations computed in a model estimating all correlations between latent factors and UPPS factor scores. Associations between the factor scores and latent factors for general RT, context slow-down, and conflict slow-down were generally low or zero, and, with two exceptions, insignificant. The exceptions appear unsystematic and therefore are unlikely to reflect a meaningful relationship between conflict control and self-report measures of impulsivity.

Accuracy data

The ANOVA results (Table 2) clearly demonstrated that the experimental manipulation of trial compatibility and previous trial compatibility also showed strong effects in both tasks on the level of accuracy. The effects for identical stimulus repetition on accuracies were very low for the Eriksen task and completely absent for the Simon task.

Accuracy data were analyzed with the same series of models as the RT data. A probit-transformation was applied to the accuracy data before analyses because of the severe deviation of accuracies from normal distributions (Cohen & Cohen, 1983). Again, in a first step measurement models were estimated for both tasks independently.

The estimation of Model 1 for the Eriksen task did not converge. The exclusion of the context factor from Model 1 led to a good fitting model ($\chi^2 = 18$, $df = 15$, $p = 0.28$, $CFI = 0.99$, $SRMR = 0.02$). In this model a correlation ($r = 0.41$) between the error terms of both cI indicators was strongly indicated through residual correlations. The iC indicators had only trivial loadings on the context factor, probably as a consequence of ceiling effect that reduced the variance of these variables. The introduction of constraints as in Model 2 resulted in a significant loss of fit ($\Delta\chi^2 = 56$, $\Delta df = 9$). Therefore, the modified Model 1 was accepted as measurement model for the Eriksen accuracies.

In contrast to the Eriksen data, the accuracy data of the Simon task were fitted well by Model 1 ($\chi^2 = 11$, $df = 12$, $p = 0.52$, $CFI = 1.00$, $SRMR = 0.02$). The context and the conflict factors did not correlate ($r = .00$). Constraints on the unstandardized loadings led to serious estimation problems and resulted in no reliable model. Therefore, Model 1 was accepted as measurement model for the Simon accuracies.

The two measurement models were integrated into a general measurement model with free correlations between all factors (Model 3), which fitted the data reasonably well ($\chi^2 = 107$, $df = 84$, $p = 0.05$, $CFI = 0.99$, $SRMR = 0.03$). Correlations of factors are reported in Table 5. Results indicate that there was a considerable degree of generality across tasks, not only for the general accuracy factors but also for the factors representing conflict effects. The generality of the context factor could not be assessed because there was no such factor for the Eriksen-task model.

Correlations with WM were small and centered on zero. The only significant correlation was found between WM and the conflict factor from the Eriksen task; it indicated that sub-

Table 5 Correlations of accuracy factors from the Eriksen and Simon task and with WM and UPPS Factors

		Eriksen task factors		Simon task factors		
		General accuracy	Conflict	General accuracy	Conflict	Context
Eriksen task factors	General accuracy	1	–	0.54**	0.22*	0.09
	Conflict	–	1	0.30**	0.39**	0.23*
	WM	0.11	−0.34**	−0.03	0.00	−0.15
	$\chi^2 = 151$, df = 127, $p = 0.07$, CFI = 0.99, SRMR = 0.04					
	Urgency	−0.15	−0.10	−0.11	−0.07	0.04
	Premeditation	0.24**	−0.09	0.12	−0.02	0.04
	Perseverance	0.22**	0.05	0.12	0.14	−0.03
	Sensation seeking	−0.17*	−0.13	−0.11	−0.12	0.01
	$\chi^2 = 161$, df = 128, $p = 0.03$, CFI = 0.99, SRMR = 0.03					

Significance of correlations is indicated by * for $p < 0.05$ and ** for $p < .01$. Intercorrelations of factors from the Eriksen and the Simon task cannot be tested because of missing constraints

jects high in WM tend to perform less accurately in conflicting trials. This result goes against what would be predicted from the assumption that WM contributes to cognitive control. We hesitate interpreting this correlation substantively because it is not predicted by any theoretical account we are aware of, and it could have arisen from chance.[4]

In a last step, the correlated factor scores of the UPPS scales were introduced into Model 3 (Table 5). There was a positive association between the general accuracy factor from the Eriksen task and premeditation as well as perseverance. In contrast, sensation seeking correlated generally negatively and low with all accuracy factors except for the context factor from the Simon task.

Discussion

Experimental paradigms like the Simon and the Eriksen task have been used successfully many times to replicate stimulus-response conflicts as well as sequential dependencies. The conflict-monitoring hypothesis is one prominent and popular account for the explanation of these effects. The present study uses individual differences to test several hypotheses derived from the conflict-monitoring hypothesis and the hypothesis of Engle and Kane (Engle & Kane, 2004; Engle, 2002; Engle, Kane, & Tuholski, 1999) about the relationship of working memory capacity to the ability to control attention and to resolve cognitive conflict. First, an implicit assumption in all theories involving conflict monitoring, control of attention, and working memory as general constructs is that these constructs describe mechanisms involved, at least to some degree, in most (if not all) laboratory tasks and real-life situations that involve cognitive conflict and control. Therefore, different measures of conflict and of control should have a substantial amount of shared variance, reflected in at least moderate positive correlations across task paradigms. Second, individuals with high ability to exert control should have smaller conflict effects, accompanied by larger context effects. Third, individuals with high working memory capacity should have a higher ability to recruit control in response to conflict, and thus have smaller conflict effects and possibly larger context effects.

On the level of mean RTs and accuracies we replicated the well-known findings: there was a substantial effect of stimulus compatibility for both tasks, and there were context effects, such that incompatible trials were particularly slow and error prone when following compatible trials, and compatible trials were particularly fast and errorless when following compatible trials. The expected pattern of results for the sequential dependency effect following cC < iC < iI < cI for RT and the opposite for accuracies could be replicated for both tasks. Overall, the effects of sequential dependency were much weaker than the effects of stimulus compatibility. The context effects could not be explained by repetition priming because stimulus repetition was varied orthogonally to context effects. Stimulus repetition per se had only a negligible effect, despite the considerable statistical power of the present study.

We observed substantial individual differences in conflict and context effects that could be factorially separated from individual differences in general speed: we successfully established latent conflict factors that supposedly reflect individual differences in conflict slow-down in RT, as well as conflict-related error increase in accuracy data. Given the magnitude of the effect and the subtlety of the experimental manipulation this is no small achievement. However, the correlation between the two RT conflict

[4] In investigations testing many correlations, some can become significant merely by chance. It is not straightforward to control this alpha inflation statistically (e.g., by Bonferroni correction) because each correlation speaks to a different hypothesis. Thus, each theoretically predicted correlation is tested only once, and that test should not be penalized by Bonferroni correction—the fact that other hypotheses are tested in the same study does not inflate the alpha error probability for a given prediction. At the same time, there are plenty of opportunities for non-predicted effects to emerge by chance. Therefore, non-predicted correlations, even when statistically significant, must be interpreted with caution.

factors was essentially zero. This indicates that the conflict effects in the two tasks are not caused by one source of variance. Therefore, we could not establish the existence of a general construct "conflict slow-down" cutting across tasks. The latent factors for conflict found in the accuracy data, in contrast, showed a positive association. However, this correlation is still far away from forming a coherent factor of conflict slow-down suggesting that the contribution of general mechanisms of conflict and control to the effects observed in the Eriksen and the Simon task are rather small. This result leaves room for two conclusions. Either one or two of well established and frequently used experimental paradigms of cognitive conflict do not elicit individual differences in the efficiency of a general conflict monitoring and control mechanism, or such a general mechanism, as assumed in the conflict-monitoring hypothesis, does not exist.

The conclusions with respect to individual differences in context slow-down are twofold. Establishing latent factors of individual differences in sequential adaptation was possible for RT data but failed for accuracy data from the Eriksen task. In the latter case, the iC indicators provided no reliable variance. Where latent factors could be established for both individual tasks (i.e., for the RT data), they had a moderate amount of common variance across tasks. The correlation between the RT-context factors for the Eriksen and Simon task could reflect the common variance of a general mechanism of cognitive control. It should be noted, however, that the correlation of 0.39 reflects only 15% common variance. The conflict and context RT factors within the measurement model for the Erikson and Simon task were more highly correlated with each other (0.73 for Eriksen and 0.48 for Simon) than each of them was correlated with the corresponding factors from the other task (0.14 for the two conflict factors, and 0.39 for the two context factors), pointing to a high degree of task-specificity of conflict- and context-related processes.

This specificity of context slow-down could stem from the contrast of horizontal (Eriksen task) versus vertical (Simon task) experimental paradigm (Vallesi, Mapelli, Schiff, Amodio, & Umiltà, 2005; Wascher, Schatz, Kuder, & Verleger, 2001). Although we doubt that manipulations of horizontal versus vertical instances of a task really affect individual differences in conflict or context slow-down so profoundly, it is possible that more highly correlated factors of context slow-down could be derived from experimental paradigms that are matched in terms of spatial orientation of the irrelevant stimulus dimension.

A finding that sits uncomfortably with the conflict-monitoring model is the positive correlation between conflict factors and context factors, both within the Eriksen task (for RTs) and across tasks (for RTs and accuracies). As explained in the introduction, the conflict-monitoring model should, if anything, predict a negative correlation: individuals who apply a high degree of control on a trial should have smaller conflict effects on that trial, leading to low values on the conflict factor. Assuming that their strong control settings persist until the next trial, the same individuals should have large context effects (i.e., dampened conflict effects following incompatible trials, compared to larger conflict effects following compatible trials). In no case did we observe a significant negative correlation between conflict factors and context factors. One potential explanation for the finding of more positive than negative correlations between conflict and context effects could be as follows: a high degree of control might imply not only low conflict on the current trial but also a strong tendency to monitor conflict continuously and to adjust control settings quickly on a trial-to-trial basis, thereby leaving little room for spill-over effects of control settings from previous trials. As a consequence, people with strong control would experience not only smaller conflict but also smaller context effects.

Results of associations of conflict and context factors with factors of impulsivity showed a fairly clear pattern. The correlations between factors derived from a self-reported impulsivity questionnaire and latent factors of conflict and context effects were mostly close to zero. Where there were small effects, they indicate that subjects high in sensation seeking showed a tendency to generally faster performance, but only for the Eriksen task. However, we want to stress that these correlations were weak and potentially spurious.

WM correlated consistently in the expected direction with factors of general RT performance, but not with any conflict factors. WM also correlated with one context factor (RTs from the Simon task). These findings do not converge with those of Heitz and Engle (2007), who found that individuals with high WM span were faster than those with low WM span to minimize the distracting effect of incompatible flankers (i.e., the conflict effect). The significant correlations lend no support to the view that WM is related to the ability to control attention and reduce conflict.

The expectation that WM correlates negatively with the latent factors reflecting conflict and context adaptation processes derives from the assumption that these control processes are themselves controlled processes, and thus involve operations of working memory (Engle & Kane, 2004; Kane & Engle, 2002; Kane, Conway, Hambrick, & Engle, 2007). This is not necessarily the case. The conflict monitoring and control theory of Botvinick et al. (2001) can be interpreted as describing an automatic loop from a conflict signal that triggers a conflict detector in the anterior cingulated cortex, and that feeds back a command to focus more exclusively to the relevant stimulus location (Eriksen) or feature dimension (Simon). This loop could operate outside working memory—indeed, the computational models of Botvinick et al. (2001) and Yeung, Botvinick, and Cohen (2004) have no place for working memory processes. If we accept this interpretation, the low correlations of our conflict and context factors with WM would not be surprising. The low correlations with the UPPS scales could also be understood, because the UPPS scales are based on self-report and therefore are likely to reflect intentional, voluntary control processes, rather than automatic control loops that might not even be registered by the person. This interpretation would still leave as an open question why there are systematic individual differences in the operation of the control loop that are nevertheless specific for each task. The high degree of task specificity of individual differences is difficult to understand on the assumption of a task-general control system.

Conclusion

The present study provides, for the first time, evidence for individual differences in conflict and cognitive control that are separable from individual differences in basic task performance. The factors for conflict and for context effects represent variance of experimental effects that have been taken as evidence for conflict-related cognitive processes. Specifically, the conflict factor represents individual differences in the balance between conflict and top–down control counteracting that conflict, whereas the context factor represents individual differences in the strength of control and in the degree to which control settings on one trial are transferred to the next trial.

Researchers addressing cognitive conflict and control through experimental manipulations assume, mostly implicitly, that the indicators of conflict and control measured in one experimental paradigm are representative of conflict and control processes in general. Theories of conflict and control are not meant to apply to a single experimental paradigm but rather to many, if not all, different situations in which cognitive conflict arises. This assumption of generalization from one experimental paradigm to the whole intended scope of a theoretical concept is rarely tested empirically. Research on individual differences offers one way of testing it by investigating the covariation between indicators of conflict and control across individuals. The present study made a first step in that direction. The results showed that the largest part of the variance in conflict and context factors is task specific. This outcome must be sobering for everyone who wants to base strong theoretical claims on experimental findings of a single paradigm.

The strong task-specificity of our measures of conflict and control obtained through our measurement models are also troublesome for theories assuming that cognitive control reflects a general ability that can be linked to working memory capacity. Working memory capacity has been established as a strong general factor of individual differences in cognition (Kane, Hambrick, Tuholski, Wilhelm, Payne, & Engle, 2004; Colom, Flores-Mendoza, & Rebollo, 2003; Oberauer et al., 2000; Conway & Engle, 1996). If it is to be reduced to individual differences in cognitive control, then measures of cognitive control should also reflect a general factor. Our results do not agree well with that assumption. In addition, the evidence we obtained for a relationship between working memory capacity and factors of cognitive control is mixed at best. Future research will have to show whether, when more and more diverse indicators of cognitive control are used, a stronger general factor of control can be extracted from them, and whether that factor is more convincingly related to working memory capacity.

Appendix

Table 6

Table 6 Loadings from Model 4b

Indicator		Latent factor	Standardized regression weight	Standard error of regression weight
Counting span	←	WM	0.58	0.02
Rotation span	←	WM	0.66	0.02
Memory updating	←	WM	0.69	0.02
Eriksen task cC id	←	Gen. Perf. Eriksen task	0.99	0.04
Eriksen task cC non-id	←	Gen. Perf. Eriksen task	0.96	0.04
Eriksen task iC id	←	Gen. Perf. Eriksen task	0.91	0.04
Eriksen task iC non-id	←	Gen. Perf. Eriksen task	0.92	0.04
Eriksen task cI id	←	Gen. Perf. Eriksen task	0.66	0.04
Eriksen task cI non-id	←	Gen. Perf. Eriksen task	0.66	0.04

Table 6 continued

Indicator		Latent factor	Standardized regression weight	Standard error of regression weight
Eriksen task iI id	←	Gen. Perf. Eriksen task	0.75	0.04
Eriksen task iI non-id	←	Gen. Perf. Eriksen task	0.75	0.04
Eriksen task cI id	←	Conflict Eriksen task	0.50	0.06
Eriksen task cI non-id	←	Conflict Eriksen task	0.50	0.06
Eriksen task iI id	←	Conflict Eriksen task	0.56	0.03
Eriksen task iI non-id	←	Conflict Eriksen task	0.59	0.03
Eriksen task iC id	←	Context Eriksen task	0.14	0.02
Eriksen task iC non-id	←	Context Eriksen task	0.14	0.02
Eriksen task cI id	←	Context Eriksen task	0.14	0.05
Eriksen task cI non-id	←	Context Eriksen task	0.14	0.05
Simon task cC id	←	Gen. Perf. Simon task	0.99	0.06
Simon task cC non-id	←	Gen. Perf. Simon task	0.99	0.06
Simon task iC id	←	Gen. Perf. Simon task	0.97	0.06
Simon task iC non-id	←	Gen. Perf. Simon task	0.97	0.06
Simon task cI id	←	Gen. Perf. Simon task	0.95	0.06
Simon task cI non-id	←	Gen. Perf. Simon task	0.95	0.06
Simon task iI id	←	Gen. Perf. Simon task	0.96	0.06
Simon task iI non-id	←	Gen. Perf. Simon task	0.96	0.06
Simon task cI id	←	Conflict Simon task	0.22	0.03
Simon task cI non-id	←	Conflict Simon task	0.22	0.03
Simon task iI id	←	Conflict Simon task	0.33	0.03
Simon task iI non-id	←	Conflict Simon task	0.33	0.03
Simon task iC id	←	Context Simon task	0.20	0.02
Simon task iC non-id	←	Context Simon task	0.20	0.02
Simon task cI id	←	Context Simon task	0.21	0.03
Simon task cI non-id	←	Context Simon task	0.21	0.03

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