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Are facet-specific task trainings efficient in improving children's executive functions and why (they might not be)? A multi-facet latent change score approach

Sascha Zuber [a,b,c,*], Emilie Joly-Burra [a,c,d], Caitlin E.V. Mahy [e], Vanessa Loaiza [f], Matthias Kliegel [a,c,d]

[a] Center for the Interdisciplinary Study of Gerontology and Vulnerability, University of Geneva, CH-1205 Geneva, Switzerland
[b] Institute on Aging & Lifelong Health, University of Victoria, British Columbia V8N 5M8, Canada
[c] Swiss National Centre of Competences in Research LIVES–Overcoming vulnerability: Life course perspectives, Lausanne and Geneva, Switzerland
[d] Faculty of Psychology and Educational Sciences, University of Geneva, CH-1205 Geneva, Switzerland
[e] Department of Psychology, Brock University, St. Catharines, Ontario L2S 3A1, Canada
[f] Department of Psychology, University of Essex, Colchester CO4 3SQ, UK

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ABSTRACT

It currently remains unclear how facet-specific trainings of three core modules of executive function (EF; updating, switching, and inhibition) directly compare regarding efficacy, whether improvements on trained tasks transfer to nontrained EF tasks, and which factors predict children's improvements. The current study systematically investigated three separate EF trainings in 6- to 11-year-old children (N = 229) using EF-specific trainings that were similar in structure, design, and intensity. Children participated in pre- and posttest assessments of the three EFs and were randomly allocated to one of three EF trainings or to an active or passive control group. Multivariate latent change score models revealed that only the updating group showed training-specific improvements in task performance that were larger compared with active controls as well as passive controls. In contrast, there were no training-specific benefits of training switching or inhibition. Latent changes in the three EF tasks were largely independent, and there was no evidence of transfer effects to nontrained EF

* Corresponding author at: Center for the Interdisciplinary Study of Gerontology and Vulnerability, University of Geneva, CH-1205 Geneva, Switzerland.
E-mail address: sascha.zuber@unige.ch (S. Zuber).

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tasks. Lower baseline performance and older age predicted larger changes in EF performance. These seemingly opposing effects support compensation accounts as well as developmental theories of EF, and they highlight the importance of simultaneously accounting for multiple predictors within one model. In line with recent theoretical proposals of EF development, we provide new systematic evidence that questions whether modular task trainings represent an efficient approach to improve performance in narrow or in broader indicators of EF. Thereby, this evidence ultimately highlights the need for more comprehensive assessments of EF and, subsequently, the development of new training approaches.

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Introduction

Executive function (EF) represents a group of higher-order cognitive processes that enable individuals to be attentive, to solve problems, to pursue goals, and to regulate behaviors, thoughts, and emotions (Diamond, 2013; Zelazo et al., 2008). From a developmental perspective, EF is crucial, for example, because it contributes to children's attainment of autonomy, socioemotional functioning, and academic performance (Best et al., 2011; Dawson & Guare, 2018; Denham et al., 2015; Diamond, 2016; Liew, 2012; Riggs et al., 2006). Motivated by the high everyday relevance of broader and more ecological indicators of EF, extensive research has aimed to improve children's performance on more narrow laboratory measures of EF via cognitive training (Strobach & Karbach, 2021). Such efforts are based on the concept of *transfer,* which postulates that repeatedly performing a cognitive task will improve performance on tasks that deploy similar cognitive processes (often labeled *near transfer*) and that this might even improve performance on structurally more distant tasks deploying similar cognitive processes (often labeled *far transfer;* e.g., Kliegel et al., 2017; Strobach & Karbach, 2021).

In this context, the three most widely studied and trained EF components are *updating* of information in working memory, *switching* attention between different task sets, and *inhibition* of automatic or predominant responses or of irrelevant distractors (Miyake et al., 2000). It is crucial to highlight here that this three-partite view of EF has received increasing criticism (e.g., Doebel, 2020; Perone et al., 2021). Importantly, EF is still developing during childhood and certain components may fully mature only later in life (e.g., switching; Garon et al., 2008; Karr et al., 2018; Müller & Kerns, 2015). Thus, it is unlikely that EF is best understood with a three-dimensional model (compared with a one- or two-dimensional model) across all ages (see Karr et al., 2018; Miyake & Friedman, 2012). Related to this, more and more research questions such modular views of different EF components and whether training performance on these rather narrow indicators of EF can actually transfer to broader outcomes (Diamond & Ling, 2016; Kassai et al., 2019; Perone et al., 2021). Recent theoretical contributions therefore urge that EF should be conceived more comprehensively as using control "in the service of particular goals that activate and are influenced by diverse mental content such as knowledge, beliefs, and values" (Doebel, 2020, p. 952). Building on this view, rather than static modules that are activated one at a time, EF may be better understood as a dynamic system that allows momentary behavior by assembling multiple components (physiological, cognitive, emotional, and motor processes together with the social and physical forces) of prior experiences and abilities to pursue a goal (Perone et al., 2021).

Despite such proposals and calls for more comprehensive views of EF, the three-component model is currently still a persisting conceptualization of EF, with updating, shifting, and inhibition representing the most widely studied and trained EF components during childhood (for an in-depth review, see Müller & Kerns, 2015). Thus, the goal of the current study was to systematically compare cognitive trainings of these three EFs. Although our study focused on these narrow measures of EF, we discuss findings within the broader context of the current EF literature, which will ultimately lead to new insights that align with these more comprehensive views.

Cognitive training and EF

A significant body of literature suggests that computerized cognitive process trainings can enhance EF performance on laboratory tasks (for reviews, see, e.g., Diamond & Ling, 2016, 2020; Kliegel et al., 2017; for meta-analyses, see, e.g., Cao et al., 2020; Sala & Gobet, 2017; Scionti et al., 2020; Takacs & Kassai, 2019). However, most previous studies have focused on training either a single EF per study or all three core EFs simultaneously. So far, only one study has applied specific separate trainings for each EF component within a sample of typically developing children (Johann & Karbach, 2020). Johann and Karbach (2020) compared standard training with game-based training of the three EFs and examined potential transfer effects to mathematical and reading abilities in 153 8- to 11-year-old children. They found that both trainings improved EF performance. They also found transfer effects to reading abilities. EF improvements were greater in children who participated in a game-based switching or inhibition training compared with the passive control group, and these improvements persisted at a 3-month follow-up.

Johann and Karbach's (2020) study thus provided the first integrative insights into the broader long-lasting benefits of training EF to improve non-EF domains. However, although the authors reported no transfer between the three EFs, they did not examine in detail how improvements in each EF directly compared with the others or whether improvements may be interrelated. The authors suggested that transfer between EFs might not have occurred because each training program consisted of a set of multiple tasks and that training on only a single EF task might facilitate transfer to untrained EF tasks. Despite these relevant suggestions, systematic comparisons of the three EF trainings are currently still lacking, leaving a series of conceptually important questions unanswered—such as how different EFs directly compare in terms of how easily task performance can be improved, whether improved performance in one EF relates to improved performance in the other EFs, or which factors predict training benefits in children. Building on and extending recent work such as that of Johann and Karbach (2020), the current study set out to tackle these pressing questions.

Performance improvements in the trained EF

Currently, it is unclear how updating, switching, and inhibition directly compare in terms of specific performance improvements in the trained EF. Performance improvements are most frequently observed in studies that train updating, whereas results are less consistent when training switching or inhibition (e.g., Kassai et al., 2019; Rapport et al., 2013; Takacs & Kassai, 2019). Previous findings need to be interpreted with caution, however, because updating also represents the most studied EF component and thus is most likely to produce a larger number of positive findings (Takacs & Kassai, 2019). Furthermore, studies typically train only one specific EF and contrast benefits with either an active or passive control group. Yet, studies largely vary in terms of target population, design, and training intensity, which further contributes to the inconsistent pattern of EF training benefits (Klingberg, 2010), making it difficult to investigate whether performance is more likely to improve on certain EF tasks.

Transfer to performance improvements in untrained EF tasks

Even more debated is the extent to which modular task trainings translate into performance improvements on untrained EF tasks (Diamond & Ling, 2020; Smid et al., 2020). For each EF, transfer effects have been inconsistent (for reviews and meta-analyses, see Kliegel et al., 2017; Klingberg, 2010; Melby-Lervåg et al., 2016; Morrison & Chein, 2011; Sala & Gobet, 2017, 2020), potentially again because of studies examining one EF training at a time and trainings being heterogeneous across studies. Importantly, there is currently no systematic examination of whether improvement in one EF task directly translates into improvements in other EF tasks.

Theoretical accounts and predictors of training benefits

Although research consistently shows that there is important variance in how much individuals benefit from EF trainings (e.g., Cao et al., 2020; Smid et al., 2020; Traut et al., 2021), there is currently

no consensus on which factors predict training benefits in children or on which theoretical and developmental accounts best describe training mechanisms in children. From a theoretical perspective, *compensation accounts* suggest that training benefits are largest for individuals who have an initial disadvantage in performance (e.g., individuals with low baseline performance, atypically developing children, children at higher developmental risk or from lower socioeconomic conditions; Karbach et al., 2017; Smid et al., 2020; Strobach & Karbach, 2021; Traut et al., 2021). Accordingly, children with lower initial performance have more room to improve and engaging in new cognitive activities might be more beneficial for them. In contrast, *magnification accounts* suggest that children with initial advantages in performance benefit more because they are more able to fully engage in the intervention program and build on already existing skills (e.g., Foster et al., 2017; Lövdén et al., 2012; Swanson, 2014, 2015). Although both accounts have been supported by empirical studies (see Katz et al., 2021; Traut et al., 2021), they assign a central but opposing role to baseline performance.

Similarly, from a developmental perspective, there currently is no agreement on the role of age. On the one hand, younger children may benefit more because of greater neuroplasticity and because their EFs are being differentiated into distinct abilities (Best & Miller, 2010; Huizinga et al., 2006). On the other hand, older children may benefit more because the neural underpinnings of EF—prefrontal networks—continue to undergo important structural and synaptic changes during late childhood and throughout adolescence (Best & Miller, 2010; Diamond, 2013). So far, the literature has mostly compared rather distant age groups, typically with a focus on older versus younger adults (Katz et al., 2021). Therefore, it remains unclear whether early school-age children versus preadolescents may benefit more from EF trainings. Certain meta-analyses suggest larger training benefits for younger children (e.g., Cao et al., 2020; Cepeda et al., 2001; Wass et al., 2012), whereas others do not find age effects (Kassai et al., 2019; Scionti et al., 2020). Furthermore, other demographic variables that could interact with age, namely gender, remain largely unstudied even though boys and girls may respond differently to computerized tasks (e.g., Delalande et al., 2020; Martinovic et al., 2016).

The current study

This study aimed to provide the first systematic and comprehensive examination of how facet-specific single-task trainings directly compare with each other across the entire middle childhood and whether they translate into benefits in untrained EFs. We aimed to extend Johann and Karbach's (2020) study by (a) systematically disentangling whether benefits translate to performance improvements in untrained EF tasks, (b) extending the age range to cover the entire primary school period (i.e., 6–11 years), (c) more directly examining the role of multiple predictors (i.e., baseline performance and age, controlling for gender) within a single latent change score model, and (d) including an active control group for which the activity was closely matched to the training interventions regarding task design, difficulty, adaptability of difficulty, intensity, and duration.

The current study thereby aimed to answer the following research questions. First, how do the different EFs directly compare in terms of how easily task performance can be improved? Second, does improved performance on one EF task relate to improved performance on tasks deploying other EFs? Third, which factors predict training benefits? Fourth, is there support for compensation versus magnification accounts of cognitive training when all predictors are considered simultaneously within one model? Fifth, would the efficacy of the training be different when comparing training groups with active versus passive controls?

Method

Participants

A total of 239 school-aged children initially participated in the study. They were recruited through advertisements at publication locations, schools, and the experimenters' network. In view of the important differences in the training literature between typically and atypically developing children as well as the large age range of our sample, we excluded children whose indices of general cognitive functioning were outliers in order to render the sample more homogeneous in terms of overall devel-

opment of cognitive functioning. Therefore, 9 children were excluded from subsequent analyses because they scored below 2.5 standard deviations of their age group norms on fluid and/or crystallized intelligence measures (assessed via the Matrices and Vocabulary subtests of the Wechsler Intelligence Scale for Children–Fourth Edition (WISC-IV; Wechsler, 2004). Of these 9 children, 1 child came from the updating group, 4 children came from the inhibition group, 3 children came from the active control group, and 1 child came from the passive control group. It is important to highlight that the exclusion occurred after data collection and that the cutoff was adapted during revisions of this manuscript (i.e., we initially planned to exclude scores below 2 standard deviations, which would exclude 1 additional participant). Note that the pattern of results of our findings would remain the same if analyses were performed on data including all participants as well as if the cutoff was 2 standard deviations. The remaining children did not report any history of (neuro)psychopathology (as indicated by children's caregivers in questionnaires) and either were native French speakers or had fluent proficiency in French. All children and their caregivers gave informed consent.

The final sample consisted of 230 children (M_{age} = 8 years 4 months, *SD* = 1 year 5 months), 121 of which were female (53 %; there were no significant differences in age between genders, $p = .502$). Children's ethnicity was not collected because this is not common practice in the country of data collection. Before pretest assessment, children were randomly allocated to one of the five groups (updating training, switching training, inhibition training, active control, or passive control). Table 1 displays the number of children, percentage of girls, and age per experimental group. Analyses of variance (ANOVAs) and subsequent Tukey HSD (honestly significant difference) tests showed that there were no significant differences between any of the experimental groups regarding percentage of girls or age (all *p*s > .05). A chi-square test of homogeneity indicated that the number of children did not significantly vary between groups, $\chi^2(4) = 1.15$, $p = .89$.

Procedure

Fig. 1 illustrates the procedure of the study for the five experimental groups separated by study phase. Pre- and posttest assessments consisted of two sessions each (∼45 min per session) during which children performed different EF tasks as well as other cognitive tasks (e.g., measuring fluid and crystalized intelligence) in a pseudorandomized order. Sociodemographic questionnaires were filled out by children's parents between the two pretest assessments. Pre- and posttest assessments were separated by 4 weeks on average. During this period, the three EF training groups and the active control group participated in eight sessions on a computer (∼20–25 min each; two sessions per week), whereas the passive control group did not participate in any activities. All sessions (pretest, posttest, and trainings) were conducted by two experimenters (one leading the experiment and one being present due to ethical requirements for testing children) in a quiet environment where children were not distracted.

Measures

Pretraining assessment of fluid and crystallized intelligence

Fluid intelligence: Matrices of WISC-IV. For each trial in the Matrices subtest of the WISC-IV (Wechsler, 2004), children were shown a 2 × 2 grid of four boxes with the bottom-right box displaying a question mark and the other boxes displaying images. Below this grid, six images were displayed and children were instructed to select the image that would complete the series above (e.g., selecting a green light bulb among bulbs of other colors). The task consisted of 32 grids in total but was ended earlier if children selected 4 incorrect answers in 5 consecutive trials. The outcome measure was the number of correct responses (note that raw scores were age-standardized).

Crystallized intelligence: Vocabulary of WISC-IV. In the Vocabulary subtest of the WISC-IV (Wechsler, 2004), children were asked to explain the meanings of words (e.g., "What is an umbrella?") and received 2 points for correct answers (e.g., "to protect you from rain"), 1 point for partial vague answers (e.g., "you hold it above your head"), and 0 points for incorrect answers. The task consisted

Table 1
Number of children, percentage of girls, and age per experimental group.

		Gender	Age			
Training group	*N*	% Girls	*M*	*SD*	*Min*	*Max*
Updating	48	50	8;8	1;5	5;10	11;5
Switching	47	53	8;0	1;6	5;11	10;8
Inhibition	42	59	8;4	1;4	6;5	11;2
Active control	43	49	8;6	1;6	5;10	10;11
Passive control	50	54	8;1	1;5	6;4	10;7
ANOVA		p = .91	p = .15			

Note. Ages are in years;months. ANOVA, analysis of variance.

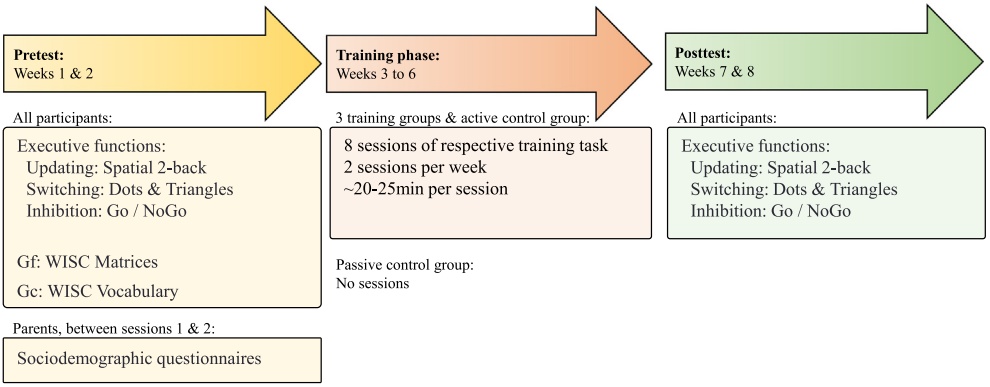

Fig. 1. Study procedure separated per experimental group and study phase. Gf, fluid intelligence; Gc, crystallized intelligence.

of 31 words in total but was ended earlier if children gave 5 consecutive incorrect answers. The outcome measure was the sum of points (note that raw scores were age-standardized).

Pre- and posttraining assessment of EF performance

Updating: Spatial 2-back task. For each trial in this task (adapted from Jaeggi et al., 2011), children were shown a 3 × 2 grid of six boxes and needed to indicate whether a cartoon character was displayed in the same box as 2 trials before (by pushing the green button stuck on the right arrow key) or not (by pushing the red button stuck on the left arrow key) (see Fig. 2). Children first performed a practice block of 17 trials (which was repeated if accuracy was <60 %). This was followed by five test blocks of 17 trials, each containing 5 hit trials (for which the character was in the same location as 2 trials before) and 12 non-hit trials. For the parallel version of the posttest assessment, a different cartoon character was used and hit/non-hit trials appeared in a different order. In both assessments, the order of hit/non-hit trials was the same for all participants. The outcome measure was the proportion of correctly detected hits minus the proportion of false alarms on non-hit trials.

Switching: Dots & triangles task. This task (adapted from Huizinga et al., 2006) consisted of two single-task blocks (Task A and Task B) and a mixed-task block (Task A/B). For each trial, children saw a grid of 4 × 4 boxes and, using the four arrow keys, needed to answer whether there were more dots (i.e., frog faces) on the left or right half of the grid (Task A) or whether there were more triangles (i.e., cherries) in the top or bottom half of the grid (Task B) (see Fig. 2). Children first worked on both single-task blocks (in counterbalanced order), which consisted of 10 practice trials (and an additional 10 practice trials if accuracy was <60 %) and 40 experimental trials. Children then worked on the mixed-task block, which consisted of 21 practice trials (and 21 possible re-practice trials) and 81 experimental trials. The mixed-task block shifted between Task A and Task B every 4 trials. For the parallel version

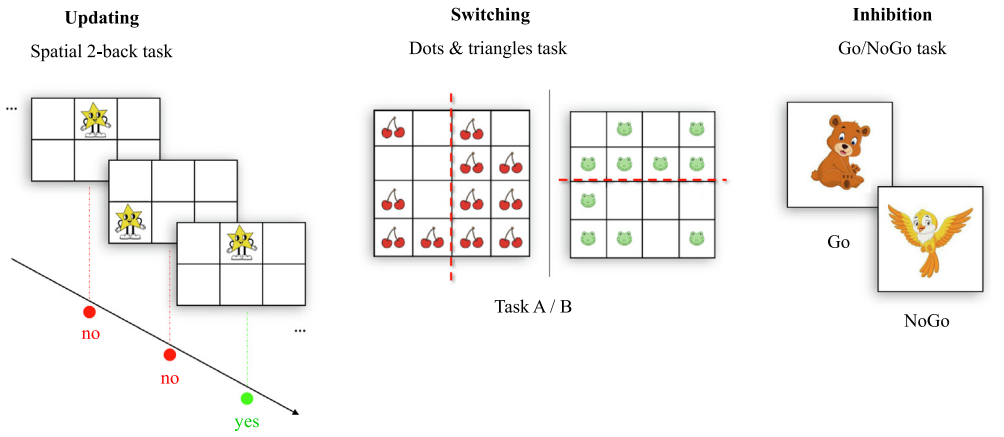

Fig. 2. Example stimuli for updating, switching, and inhibition tasks.

of the posttest assessment, stimuli were inversed (i.e., dots were used for Task B and triangles were used for Task A). The outcome measure was switching costs (i.e., mean reaction time on switch trials minus mean reaction time on nonswitch trials, both on trials with correct responses only). Note that reaction times were initially recorded in milliseconds but were rescaled to seconds to avoid variances being much larger than on the other outcome measures.

Inhibition: Go/NoGo task. In this task (adapted from Schulz et al., 2007), children were shown a series of animal pictures and needed to push the spacebar as fast as possible as soon as a new picture appeared (Go stimuli, 75 % of all trials) except for birds (for which no response needed to be made; NoGo stimuli, 25 % of all trials) (see Fig. 2). Go and NoGo trials were presented in a pseudorandomized order. Children practiced this first for 16 trials (and for another 16 practice trials if performance was <60 %) and then worked on a block of 96 trials. For the parallel version of the posttest assessment, monkeys were used as NoGo stimuli (note that monkeys were used as Go stimuli at pretest, but birds were not used as Go stimuli at posttest). The outcome measure was inhibition accuracy (i.e., the proportion of correctly inhibited NoGo trials).

Training programs

The three EF and the active control programs were similar in terms of training design and intensity. Each program consisted of eight sessions lasting 20 to 25 min each. For the three EFs, training tasks resembled the pre–post assessment of the same EF (see "Pre- and posttraining assessment of EF performance" section above). Specifically, the updating trainings consisted of a spatial 2-back paradigm for which children needed to indicate for each trial whether a cartoon character was displayed in the same location as 2 trials before. Each session consisted of 170 trials (50 hit trials). The switching training tasks consisted of a Task A/Task B switching paradigm for which children needed to indicate either whether stimuli belonged to one category versus another category (Task A) or whether one object versus two objects were displayed (Task B). The paradigm switched between tasks on every third trial, and each training consisted of 410 trials (200 switching trials). The inhibition training consisted of a Go/NoGo response inhibition paradigm for which children needed to push the spacebar as fast as possible after stimuli appeared on the screen (Go trials) except for when the stimulus corresponded to a specific category (NoGo trials). Each session consisted of 320 trials (80 NoGo trials). In the active control training, children needed to categorize images (similar to the categorization tasks of the switching paradigm but without needing to switch between different task sets, thereby not particularly tapping into EF). Each training session consisted of 410 trials.

For all four training programs, task difficulty was individually adapted to children's performance throughout the program. During each training session, there were a total of 10 difficulty levels to which children could advance or revert depending on their performance. Between levels, difficulty

was increased by decreasing how long a stimulus and fixation cross were presented and, for certain levels, by presenting more complex stimuli (i.e., more spatial locations and more challenging maps for the updating training; categories that are more difficult to distinguish for the inhibition training). To be able to compare training progress between groups, one outcome measure was the highest level achieved in each training session. In addition, group-specific outcome measures were the proportion of correctly detected hits minus the proportion of false alarms on non-hit trials for updating, switching costs on trials with correct responses only for switching, inhibition accuracy for inhibition, and mean reaction time on correctly categorized trials for active control training. More detailed descriptions of the different trainings can be found online in Supplementary Material S1. The passive control only participated in pre- and posttest assessments without receiving any activities between the two assessment times (i.e., "business as usual" control group).

Statistical analyses

First, to assess how performance on the training tasks changed throughout the training, we compared the highest task level achieved as well as task-specific outcome measures on the first versus last training session by conducting a paired-sample *t* test for each training group separately (including the active control group). These *t* tests as well as descriptive statistics, correlations, ANOVAs, and Tukey HSD test were conducted with jamovi software.

Second, to assess predictors of pre–post change and potential transfer effects to untrained EFs, we conducted factorial latent change score modeling (LCSM). To examine means and variances in change of each EF task as well as how these changes correlated, in a first LCSM (Model 1; see Panel A of Fig. 3), we computed latent variables of change as the difference between pre- and posttest performance for each of the three EF tasks. To examine whether change in one EF task was related to change in the other EF tasks, latent change variables were allowed to covary. Performances at pretest for the three EF tasks were also allowed to covary.

Third, to investigate predictors of changes in EF performance, in a second LCSM (Model 2; see Panel B of Fig. 3), the following variables were added as predictors of the three latent change scores: (a) baseline performance, (b) children's age, (c) gender, and (d) the specific training group. Four dummy variables were created to investigate the effect of each training group compared with the active control group (i.e., the updating group has the value 1 on the updating training variable and has the value

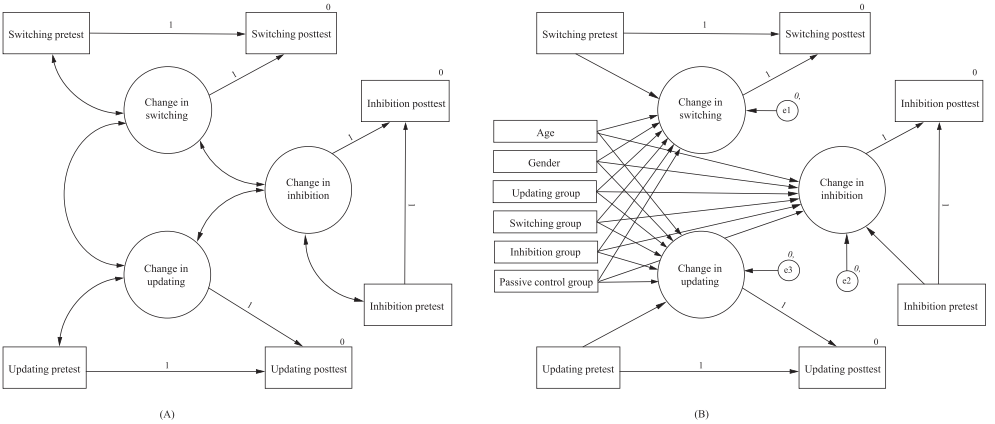

Fig. 3. Factorial latent change score models. (A) Latent score change Model 1. The latent change variables have estimated means and variances. Single-headed arrows represent regression coefficients, whereas two-headed arrows represent covariances. For the purpose of readability, correlations between pretests for the three EF tasks are not depicted. (B) Model 2, where predictors of change are included in the model and allowed to covary. The error terms (e1, e2, and e3) indicate residual variances from the latent change scores. For the purpose of readability, covariances between the different predictors and covariances between residual variances of change are not depicted.

0 on the three remaining dummy variables; the active control group has the value 0 on all four variables). A significant effect of a dummy variable means that this group displayed larger EF changes than the active control group. Residual variances of latent change variables were allowed to covary.

Fourth, to examine whether the efficacy of the training would be judged differently when comparing training groups with active versus passive controls, we computed a third model (Model 3) in which we used the passive control group as a reference group. Note that although changing the reference group can result in different parameter estimates, this model is mathematically equivalent to Model 2 in terms of model fit, which therefore is reported only for Model 2. LCSMs were estimated in IBM SPSS AMOS (Version 26) using maximum likelihood estimation. As indicated by the Little MCAR (missing completely at random) test (computed in IBM SPSS Version 26), missing data (which were less than 1 % of all data points) were missing completely at random, $\chi^2(41) = 48.54$, $p = .20$, and therefore subsequently were imputed using full information maximum likelihood (Little, 1988). We assessed the goodness of fit for the two LCSMs using the χ^2/df ratio, the root mean square error of approximation (RMSEA), and the comparative fit index (CFI). Model fits were considered good when the χ^2/df ratio was less than 3, when the RMSEA was between 0 and .06, and when the CFI was greater than .95 (Hu & Bentler, 1999).

Results

Descriptive statistics

Table 2 presents means, standard deviations, and correlations between age, pretest performance, and posttest performance on the three EFs tasks across all groups. Fig. 4 displays pre- and posttest performances on the three EF tasks separated by group.

Changes in performance across training sessions

Fig. 5 depicts trajectories of the highest level achieved on each training session (i.e., group mean) for the four training groups separately. It also displays performance trajectories on each training session on the four training tasks. Regarding the highest levels achieved, paired-sample *t* tests between the first and eighth training sessions showed that there were significant differences with large improvements for the updating group, $t(45) = 12.95$, $p < .001$, $d = 1.91$, and the switching group, $t(40) = 6.83$, $p < .001$, $d = 1.07$. In contrast, performances of the inhibition and active control groups did not significantly improve on the highest level achieved, $t(37) = 0.45$, $p = .65$, $d = 0.07$, and $t(42) = -0.84$, $p = .41$, $d = -0.13$, respectively. Regarding performance on the trained tasks, paired-sample *t* tests between the first and eighth training sessions showed that there were significant improvements for the updating group, $t(45) = 5.13$, $p < .001$, $d = 0.76$, and the switching group, $t(40) = -5.10$, $p < .001$, $d = -0.80$. In contrast, performance of the inhibition group did not significantly improve, $t(37) = 0.88$, $p = .39$, $d = 0.14$, whereas the active control group became significantly slower across sessions, $t(42) = 3.91$, $p < .001$, $d = 0.60$.

Baseline differences

To ensure that training improvements were not confounded with potential differences in performance at baseline, we compared pretest performance of the five groups for each EF. Tukey HSD analyses indicated that there were no significant differences between any of the five groups in baseline performance on updating, switching, or inhibition except for one; the inhibition group performed significantly worse than the passive control group on the switching task at baseline, $t(220) = -2.80$, $p = .043$ (all other *p*s > .05).

Variability in change and transfer effects to other EF tasks

Model 1 showed excellent fit, $\chi^2(6) = 7.06$, $\chi^2/df = 1.18$, $p = .32$, CFI = .99, RMSEA = .03. Parameter estimates are reported in Table 3. Mean changes between pre- and posttest were significant in updat-

Table 2
Descriptive statistics and correlations between age and pre- and posttest performances on the three executive function tasks (across all groups).

	M (*SD*)	1	2	3	4	5	6
1. Age (years)	8.29 (1.43)	–					
2. Updating pretest	0.35 (0.25)	.33***	–				
3. Updating posttest	0.46 (0.29)	.28***	.46***	–			
4. Switching pretest	0.38 (0.32)	−.21**	−.16*	−.11	–		
5. Switching posttest	0.27 (0.27)	−.26***	−.20**	−.19**	.37***	–	
6. Inhibition pretest	0.81 (0.14)	.20**	.19**	.26***	−.06	−.05	–
7. Inhibition posttest	0.82 (0.15)	−.01	.07	.15*	−.07	−.08	.33***

Note. Updating score is the proportion of correctly detected hits minus the proportion of false alarms on non-hit trials. Switching score is the switching cost in seconds (i.e., mean reaction time on shift trials minus mean reaction time on non-shift trials on trials with correct responses only). Inhibition score is the inhibition accuracy (proportion of correctly inhibited NoGo trials).
*p < .05.
**p < .01.
***p < .001.

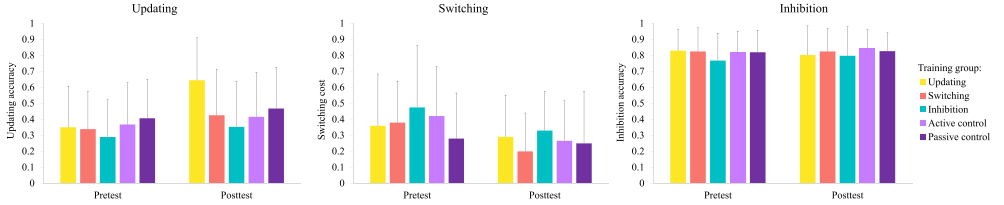

Fig. 4. Pre- and posttest performances on the three EF tasks separated by training group. Updating accuracy is the proportion of correctly detected hits minus the proportion of false alarms on non-hit trials. Switching cost is the switching cost in seconds (i.e., mean reaction time on switch trials minus mean reaction time on non-switch trials on trials with correct responses only). Inhibition accuracy is the proportion of correctly inhibited NoGo trials. Error bars represent standard deviations.

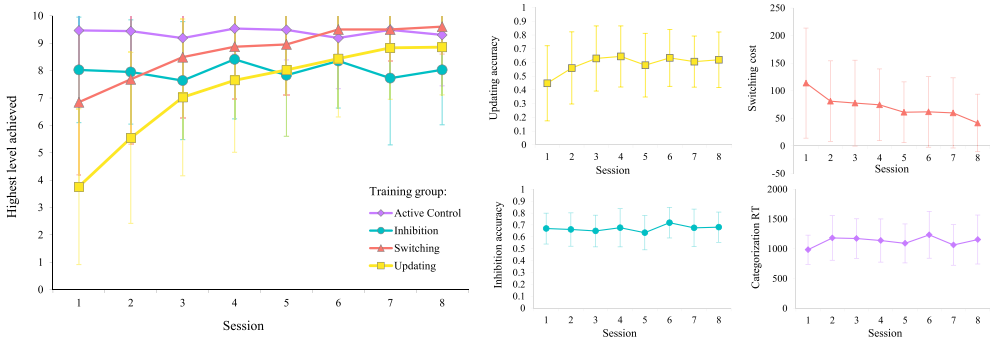

Fig. 5. Progression of highest level achieved (large figure at left) and performance (small figures at right) across the eight training sessions separated per training group. Updating accuracy is the proportion of correctly detected hits minus the proportion of false alarms on non-hit trials. Switching cost is the switching cost in milliseconds (i.e., mean reaction time on switch trials minus mean reaction time on non-switch trials on trials with correct responses only). Inhibition accuracy is the proportion of correctly inhibited NoGo trials. Categorization RT is the mean reaction time in milliseconds on correctly categorized trials of the active control training task. Error bars represent standard deviations.

ing and switching but not in inhibition. Skewness values of the factor scores for the mean changes in updating, switching, and inhibition were 0.04, 0.36, and −0.11, respectively, whereas kurtosis values were 0.19, 1.64, and 0.88, respectively. Table 4 shows changes in performance per EF and per group. In addition, variances in change (i.e., variances of the latent change scores) were significant in the three

EFs, indicating that for each EF there was interindividual variability in change. However, changes in the three EFs did not correlate (all *ps* > .05), indicating that changes in EFs were independent from each other.

Predictors of change and transfer to untrained EF tasks

Because there was significant variance in change on all three EFs, Model 2 examined predictors of change for each EF. Model 2 showed excellent fit, $\chi^2(6) = 10.58$, $\chi^2/df = 1.76$, $p = .10$, CFI = .99, RMSEA = .06. When all predictors were considered simultaneously, they predicted substantial portions of variance in changes in updating, switching, and inhibition (30 %, 47 %, and 33 %, respectively). Raw and standardized estimates for each EF are reported in Table 5. In sum, results show that change in updating was predicted negatively by updating performance at pretest but positively by age. In addition, only the updating group showed larger changes than the active control group. Change in switching[1] was predicted negatively by switching costs at pretest and by age. None of the groups showed larger changes in switching than the active control group. Change in inhibition was predicted negatively by baseline inhibition performance, indicating that children with lower initial performance showed greater improvements. There were no effects of age or group on change in inhibition. Gender did not predict change in any of the three EFs. As for correlations between the latent change variables in Model 1, residual errors for the latent change score variables in the three EFs did not correlate in Model 2, again indicating that changes in EFs were independent from each other. Furthermore, results show that none of the training group variables significantly predicted changes in untrained EFs, again indicating no evidence for transfer effects to untrained EFs.

Active versus passive controls as reference group

Finally, using the passive controls as a reference group in Model 3 revealed a similar pattern of results as in Model 2. Therefore, regression weights, standard error of estimation, and *p* values for predictors of latent changes in EFs in Model 3 are displayed in Supplementary Material S2.

Discussion

The current study set out to perform the first systematic comparison of facet-specific single-task EF trainings across the entire middle childhood. It aimed to (a) directly compare whether updating, switching, and inhibition performance improve when these three EF tasks are trained under similar conditions, (b) examine whether training certain EF tasks improves performance on the other tasks, (c) investigate which factors predict training benefits when accounting for all predictors simultaneously within a single model, including whether predictors support compensation versus magnification accounts of cognitive training, and (d) study whether the efficacy of the training would be judged differently when comparing training groups with active versus passive controls.

Does performance improve when training on EF tasks?

Overall, our findings show that updating training is the most likely to produce performance improvements when the three EFs are trained under similar conditions using a single-task training paradigm. In contrast, we find no evidence that the other trainings benefit children's EF performance beyond retest effects, general learning, and short-term developmental changes. In more detail, the updating group improved performance across training sessions, and only children in this group displayed larger pretest-to-posttest improvements in updating performance than the active and passive control groups. On average, children of the updating group increased their accuracy by 85 %, whereas

[1] For the interpretation of switching results, it is important to keep in mind that (a) pre- and posttest scores represent switching costs (hence, larger scores indicate worse performance) and (b) mean change in switching was negative, representing a reduction of switching costs (hence, larger negative regression weights of predictors indicate a larger reduction of switching costs).

Table 3
Estimated means, variances, and covariances for the latent score change variables in Model 1.

Parameter	Estimate	*SE*	*p*
Mean change in updating	**.11**	.02	<.001
Mean change in switching	**−.11**	.02	<.001
Mean change in inhibition	.01	.01	.60
Variance of change in updating	**.08**	.01	<.001
Variance of change in switching	**.11**	.01	<.001
Variance of change in inhibition	**.03**	.01	<.001
Change in updating ↔ change in switching	−.01	.01	.50
Change in updating ↔ change in inhibition	.01	.01	.82
Change in switching ↔ change in inhibition	−.01	.01	.56

Note. Double-headed arrows denote covariances. *SE*, standard error of estimation. Significant estimates are in bold.

Table 4
Changes in performance per executive function and group.

	Updating			Switching			Inhibition		
Improvement	Increase in performance accuracy	% of pretest	*SD* of pretest	Reduction of cost (in ms)	% of pretest	*SD* of pretest	Increase in performance accuracy	% of pretest	*SD* of pretest
Across all groups	.11	32.85	0.47	115	30.30	0.36	.01	0.81	0.05
Updating group	.29	84.76	1.14	69	19.05	0.21	−.03	−2.31	−0.20
Switching group	.09	25.86	0.37	181	47.19	0.70	−.01	−0.12	−0.01
Inhibition group	.06	21.17	0.27	144	30.80	0.37	.03	3.96	0.18
Active controls	.05	14.27	0.20	165	39.15	0.53	.03	3.31	0.20
Passive controls	.06	15.28	0.25	30	10.48	0.10	.01	0.83	0.05

Table 5
Regression weights, standard errors of estimation, and *p* values for predictors of latent changes in executive functions in Model 2.

	Predicting change in updating				Predicting change in switching				Predicting change in inhibition			
Predictor	*b*	*β*	*SE*	*p*	*b*	*β*	*SE*	*p*	*b*	*β*	*SE*	*p*
Updating pretest	**−.52**	**−.46**	.07	<.001	–	–	–	–	–	–	–	–
Switching pretest	–	–	–	–	**−.73**	**−.70**	.05	<.001	–	–	–	–
Inhibition pretest	–	–	–	–	–	–	–	–	**−.66**	**−.55**	.07	<.001
Age (years)	**.03**	**.13**	.01	.03	**−.04**	**−.17**	.01	<.001	−.01	−.06	.01	.33
Gender	.01	.02	.03	.73	−.02	−.03	.03	.54	.01	.03	.02	.65
Updating group	**.24**	**.34**	.05	<.001	.06	.08	.05	.23	−.04	−.10	.03	.14
Switching group	.04	.05	.05	.45	−.07	−.08	.05	.20	−.03	−.07	.03	.36
Inhibition group	−.02	−.03	.05	.66	.05	.06	.05	.34	−.03	−.07	.03	.30
Passive control group	.04	.06	.05	.40	.01	.02	.05	.78	−.03	−.07	.03	.31
R^2	.30				.47				.33			

Note. b, raw regression weight; *β*, standardized regression weight; *SE*, standard error of estimation. Updating pretest is the proportion of correctly detected hits minus the proportion of false alarms on non-hit trials. Switching pretest is the switching costs in seconds (i.e., mean reaction time on shift trials minus mean reaction time on non-shift trials on trials with correct responses only). Inhibition score is the inhibition accuracy (proportion of correctly inhibited NoGo trials). Gender is coded 0 for girls and 1 for boys. To code for group, four dummy variables were computed, with the active control group as a reference. Significant estimates are in bold.

the switching, inhibition, active control, and passive control groups improved by only 19 % on average. Although the switching group improved switching performance throughout the training, children did not show significantly larger improvements than the active or passive control groups from pretest to posttest. Furthermore, there were no improvements across inhibition training sessions, and there were no group differences in changes in inhibition performance from pretest to posttest.

At first glance, our findings might seem to diverge from those of Johann and Karbach (2020), who also reported benefits of training switching and inhibition. However, different conclusions seem to have mainly resulted from how the specific outcome measures were interpreted. Johann and Karbach reported benefits of training inhibition because children responded faster and more often on Go trials. However, as in our study, they also did not find improvements in how often children successfully inhibited responses on NoGo trials (children actually responded *more often* on NoGo trials at posttest), which we consider to be the core indicator of inhibitory control. Overall, both studies suggest no benefits of training inhibition. Similarly, Johann and Karbach reported benefits of training switching because they observed that switching costs decreased in the switching training groups. However, as in our study, there were no significant differences compared with the passive control group. This aligns with our findings and raises the question of whether multiple groups slightly improve on switching tasks (i.e., they become faster at posttest) but that there may be no EF-specific training benefits beyond mere learning, retest effects, and generally faster processing.

Taken together, findings of our study and Johann and Karbach's (2020) study dovetail with previous findings, where significant performance benefits of training updating have more consistently been shown than of training switching or inhibition (e.g., Kassai et al., 2019; Rapport et al., 2013; Takacs & Kassai, 2019). Importantly, the current results confirm this pattern in one systematic overall randomized controlled trial that applied comparable single-task training regimes with these three EF components.

Does training on one EF task improve performance on the others?

Regarding transfer effects to untrained EF tasks, our findings show that training on either updating, switching, or inhibition tasks does not improve performance on the others. Specifically, LCSM shows that the different training groups do not predict the magnitude of change in other EFs and that latent changes in the three EFs are largely independent (i.e., do not correlate). These findings are in line with Johann and Karbach (2020) and several other studies (for reviews, see, e.g., Diamond & Ling, 2016, 2020). Johann and Karbach (2020) argued that the lack of transfer effects may be due to the fact that they applied a multitask training (i.e., trainings consisted of multiple tasks of the same EF component). They suggested that variability in tasks may hinder transfer effects in children and that transfer would be more likely to occur if only one type of task was used during the training. Our study provides additional insights in this regard given that we applied a single-task training for each EF component but—in contrast to these suggestions—also did not observe any transfer effects. Indeed, the few studies that found transfer effects have applied both single- and multitask trainings (e.g., Klingberg et al., 2002, 2005; Kray et al., 2012). We argue that the uniformity versus variability in tasks does not seem to be the main driving mechanism for between-EF transfer effects.

Interestingly, studies that have reported transfer to untrained EFs were largely conducted on atypically developing populations (e.g., children with attention-deficit/hyperactivity disorder; Klingberg et al., 2002, 2005; Kray et al., 2012). Thus, transfer effects between EFs may depend on specific cognitive characteristics of the target population rather than on the training task design. Taking together the systematic evidence of our study and of Johann and Karbach (2020), as well as meta-analytical evidence of Kassai et al. (2019), there is currently no evidence of transfer effects between the three EFs in typically developing children.

Predictors of training benefits and theoretical implications

When baseline performance, age, gender, and training group were considered simultaneously, they predicted substantial 30 %, 47 %, and 33 % of variance in updating, switching, and inhibition changes, respectively. Disentangling the specific role of each predictor while accounting for the other predictors

shows that children with lower baseline performance displayed larger performance improvements on all three EF tasks, whereas older children showed larger improvements on updating and switching tasks, but there was no effect of age on inhibition performance. There was no effect of gender on change for any of the three EF tasks.

Because older children typically display better baseline performance, opposing effects of age and baseline performance may have canceled out or blurred findings in previous studies that applied more classical analyses examining one predictor at a time. Our findings thereby illustrate an important conceptual implication of directly contrasting multiple predictors within one latent change model (Karbach et al., 2017) and suggest two seemingly opposite, yet complementary, mechanisms that drive training effects in children. On the one hand, our findings align with previous studies reporting larger benefits for those who have the most room for improvement (see Karbach et al., 2017; Smid et al., 2020; Strobach & Karbach, 2021; Traut et al., 2021). From a theoretical perspective, we thereby provide more systematic evidence for compensation (rather than magnification accounts) of process-based cognitive trainings.

On the other hand, our findings are in contrast to previous studies reporting larger benefits for younger children or no effects of age (e.g., Cao et al., 2020; Cepeda et al., 2001; Kassai et al., 2019; Scionti et al., 2020; Wass et al., 2012) and suggest that, after accounting for baseline performance, training benefits are larger in older children. This aligns with previous research showing that the neural underpinnings of EFs—most important, the prefrontal cortex—continue to undergo structural and synaptic changes during late childhood and subsequent developmental stages (Best & Miller, 2010; Davidson et al., 2006; Diamond, 2013). Similarly, EF continues to develop and become more distinct across childhood (Karr et al., 2018) and certain components (e.g., switching) may only fully mature later in life (Garon et al., 2008; Müller & Kerns, 2015). Relatedly, previous research also shows that other key cognitive abilities—such as metacognitive skills—develop incrementally with schooling and are more developed by the end of middle childhood (Schneider & Lockl, 2008; Schneider & Löffler, 2016). Increases in metacognition facilitate learning across different school subjects (e.g., Dimmitt & McCormick, 2012; McCormick, 2003; Schneider, 2008; Smortchkova & Shea, 2020), and it is possible that they also bolster training benefits in older children. From a developmental perspective, our findings thereby suggest that training benefits are maximized when children's cognitive abilities are malleable, the underlying cognitive and neural systems are sufficiently developed, and the training occurs during an appropriate developmental stage that favors improvements.

Are training effects interpreted differently when compared with active versus passive controls?

To control for potential benefits of engaging in cognitively stimulating activities, participants' expectations, and other placebo effects, including an active control group has become the gold standard in cognitive training research. Yet, including active controls is also more resource- and time-consuming compared with passive controls, and it currently remains debated whether the type of control group actually affects results or the interpretation of training efficacy (e.g., Au et al., 2020). So far, this issue has mostly been examined with meta-analytical approaches between studies, with those focusing on updating reporting that benefits seem larger when comparing training effects with passive controls (Melby-Lervåg & Hulme, 2013; Sala & Gobet, 2017; Schwaighofer et al., 2015), whereas meta-analyses targeting multiple EFs did not find differences between the two control types (Au et al., 2020; Scionti et al., 2020).

With the current study, we provide the first directly comparable within-study evidence that aligns with latter meta-analyses, demonstrating similar patterns of results between active and passive control groups (Au et al., 2020; Scionti et al., 2020). Although active controls present many methodological advantages, such findings can be highly relevant for the efficient allocation of resources and time in future training studies. They suggest that interpretation of training benefits might not differ when training groups are compared with a passive control group versus an active control group that participated in a cognitively rather low-demanding activity. Depending on the specific study focus, therefore, for many studies it may be more ethical either to compare cognitive trainings with passive controls that are on waiting lists and can participate in the training at a later time or to compare trainings with active control interventions that are more engaging and may better promote children's development while allowing to disentangle specific effects of the different interventions.

Implications of the current findings

Looking at the important number of cognitive training studies that have been published over the past decades suggests that, in general, researchers have been rather optimistic that these interventions benefit task performance and children's development in a broader context. Yet, more recent literature has questioned the efficacy of how EFs currently are assessed and trained (e.g., Doebel, 2020; Perone et al., 2021). Overall, the current data align with such skepticism, showing that when performance on three EF components was trained under very similar and comparable conditions, only one training (i.e., updating) led to specific improvements. Importantly, even this training did not improve performance on supposedly related EF tasks (i.e., switching and inhibition), which aligns with current research showing little evidence for far transfer of EF task training (e.g., Diamond & Ling, 2016; Kassai et al., 2019; Perone et al., 2021). Thus, it is questionable how and why such training approaches should lead to broader improvements and generalize to everyday relevant outcomes such as school achievement, behavioral regulation, and attentional control. Although our study does not allow concluding whether task-specific improvements would persist over time or affect any everyday outcomes, the current findings strongly dampen the enthusiasm toward repeated single-task cognitive training interventions and urge for new, more efficient, and more naturalistic approaches.

This is important because for children training interventions are typically carried out in school or child-care facility. Therefore, they take away from crucial time spent on the educational curriculum and other valuable activities that may benefit children's development such as engaging in physical exercise, artistic activities, and mindfulness (for meta-analyses, see Takacs & Kassai, 2019) or in programs providing new strategies to bolster self-regulation, social, and other skills (e.g., McClelland & Tominey, 2015; Petersen, 1995). This seems even more relevant for atypically developing or at-risk populations, which may need support the most and, unfortunately, can least afford to spend time and resources on interventions that currently lack systematic evidence to promote children's development. Taken together, we argue that it is crucial for future research to thoroughly examine whether and how different intervention approaches can lead to broader long-lasting improvements in children's everyday outcomes.

In this context, Doebel (2020) and Perone et al. (2021) presented inspiring new conceptual models of EF and how its development might be fostered. They questioned the validity of the current modular conceptualization of EF and whether it is useful to train performance on these modules. Instead, they suggested that future interventions should target children's specific goals by considering children's prior knowledge, beliefs, values, and more as a dynamic ensemble that allows for momentary behavior to unfold. For example, if a child should learn not to hit another child who took his or her toy, modular task training (e.g., of inhibition) may be rather inefficient. Instead, it may be more useful to build on the child's previous experiences such as expecting that hitting will lead to punishment, having experienced how it feels to be hit by someone, preferring to maintain the friendship with the other child, and more (see Doebel, 2020). Similarly, providing children with contextual multilevel information may help them to link specific goals to environmental cues (e.g., asking how the child feels at the moment, explaining why the other child may have taken the toy, providing a context that allows for the conflict to be solved; see Perone et al., 2021). Together, by strengthening the association between goals, cues, and contextual information rather than training modular task performance, more comprehensive goal-oriented interventions may be better suited for helping children to learn and reproduce the target behavior and thereby ultimately bolster development in real-life contexts.

Limitations and outlook

Although the current study provides important systematic insights into the (in)efficacy of EF trainings and thereby may guide future research toward more integrative and more ecological assessment and training of EF, it also is important to highlight its limitations. One methodological shortcoming is that the current study did not include any follow-up or ecological measures of EF. Besides examining whether training benefits lasted across time, a follow-up would further allow evaluating training-specific versus general effects of the interventions. Yet, because we found training-specific performance improvements at posttest for only one EF task (i.e., updating), we are skeptical that this or

similar training approaches would benefit children across longer periods of time or that they would improve performance on more ecological EF measures or in real-life contexts (see Diamond & Ling, 2016; Kassai et al., 2019; Perone et al., 2021).

Another limitation is that baseline performance levels varied between EFs. For example, updating and switching performances were rather low. Although this may partially be due to our choice of outcome measures, children's performance may also have been affected by the rather low number of trials to assess each EF. In contrast, baseline performance was rather high for inhibition. Although it was sufficiently low at the first training session to leave room for potential improvements, high baseline performance may be particularly challenging for training studies, as supported by our finding that training benefits were largest when baseline performance was lowest.

An important conceptual limitation of the current study as well as other training studies is that they typically do not allow us to conclude *why* one EF may show larger performance benefits. It could be that certain EFs simply are more trainable than others and allow increasing *existing* resources. However, it could also be that because of how EFs are typically assessed, it may be easier to add *new* resources when performing certain tasks (e.g., discovering strategies such as rehearsing spatial locations of previous stimuli before onset of the next stimulus in updating tasks), whereas this may be more difficult for other tasks (e.g., inhibiting the impulse to respond after stimulus onset in inhibition tasks).

A final limitation is that correlations between pre- and posttest measures of the same EF were rather low. One possible explanation for this may be that whereas at pretest all children were relatively comparable, at posttest children had participated in different interventions and thus may have approached certain tasks differently. This also suggests that, besides training-specific effects, other mechanisms—such as task novelty, familiarity, motivation, and fatigue—may have affected posttest performance, which further illustrates issues with current approaches to assess and train EFs.

Conclusion

Our study represents the first systematic and comprehensive examination that directly compared the trainability of the three core EF components across the entire primary school age range using latent change score modeling. It demonstrates that when performance on updating, switching, and inhibition tasks is trained under similar conditions within a single group of children, only updating performance showed training-specific improvements. In terms of potential transfer effects, it underlines that there is currently no systematic evidence for transfer of improvements to nontrained EF tasks. Such findings question how likely it is that classical task paradigm trainings can lead to improvements in even broader outcomes of EF in everyday contexts. Furthermore, taken together with other studies, the findings illustrate that the current conceptualization of EF and how it develops is still incomplete (for similar views, see Doebel, 2020; Perone et al., 2021). A better understanding of how EF should be assessed and conceptualized throughout childhood is necessary before future research will be able to explore new interventions supporting children's real-life behaviors that rely on EF.

In terms of predictors of improvements, our results show that performance improvements are largest for children that are older but still have relatively low EF performance. This provides evidence for opposite yet complementary mechanisms of baseline performance and age, thereby supporting both compensation accounts as well as developmental theories of EF and underlining the importance of accounting for multiple predictors simultaneously. Finally, our findings show that the efficacy of a cognitive training is similar when comparing training groups with active versus passive controls. In certain situations, therefore, it may be more ethical to use passive controls or other interventions that are more likely to benefit children's development than the typical active control paradigms.

Data availability

The datasets generated and analyzed for the current study are not publicly available because participant consent forms did not include authorization for public data sharing. However, data will be made available by the corresponding author (S.Z.) upon reasonable request.

Data availability

The authors do not have permission to share data.

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Appendix A. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jecp.2022.105602.

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