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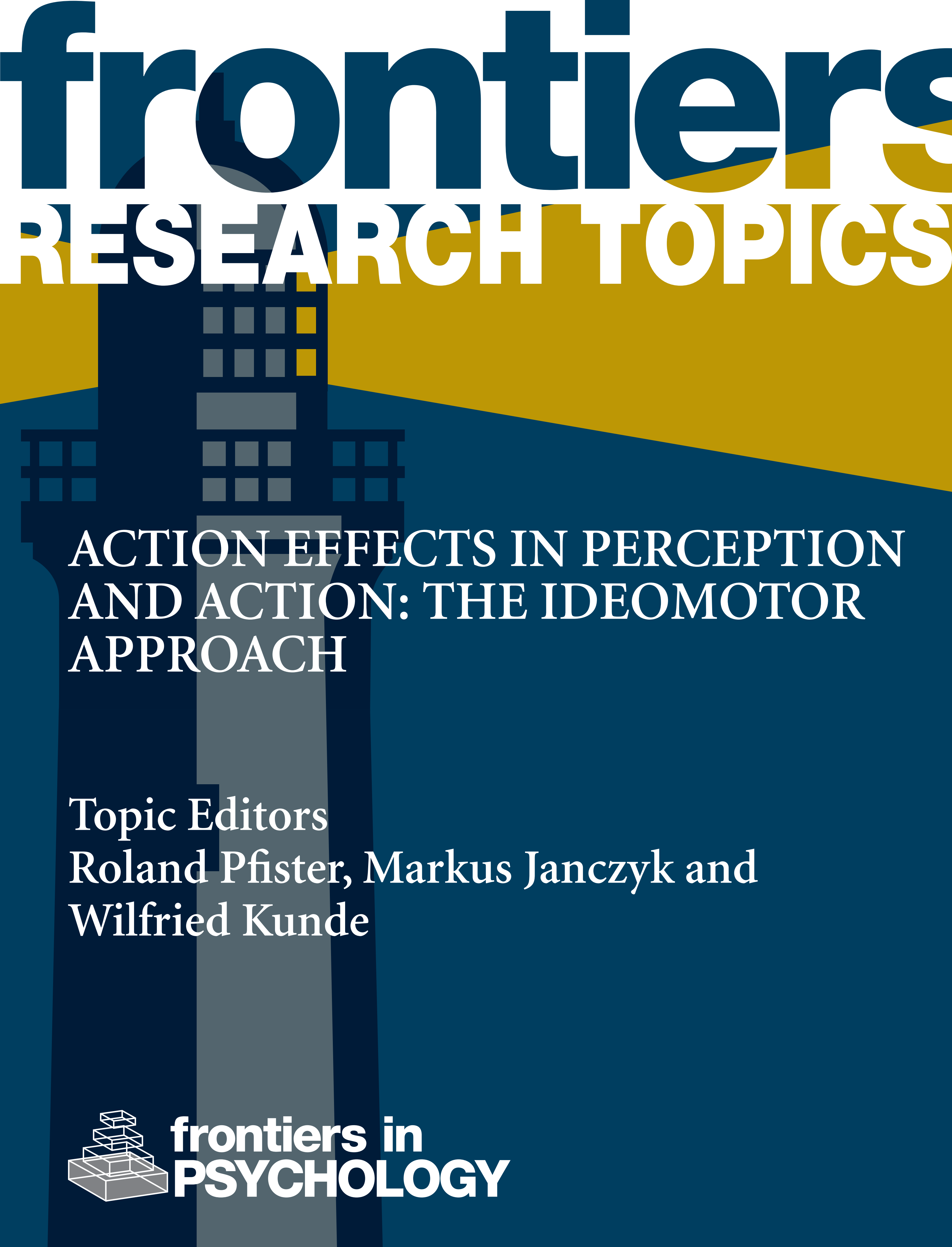

ACTION EFFECTS IN PERCEPTION AND ACTION: THE IDEOMOTOR APPROACH

Topic Editors
Roland Pfister, Markus Janczyk and
Wilfried Kunde

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ACTION EFFECTS IN PERCEPTION AND ACTION: THE IDEOMOTOR APPROACH

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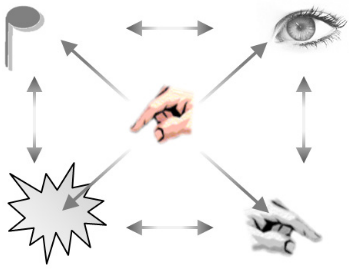

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Over the last decade, experimental findings from a variety of paradigms and fields have given rise to a renaissance of the Ideomotor Approach - an elegant theory of voluntary action that was first described by 19th century philosophers. This classical theory as well as its modern extensions put special emphasis on the role of action effects and anticipative processes for action control. While there is growing consensus on the importance of these phenomena, we are just now beginning to understand the integrative power of the ideomotor approach in a variety of fields such as perception and action, imitation, tool-use, and cognitive neuroscience. This Research Topic is devoted to such emerging perspectives on ideomotor action - ranging from neural correlates up to social behaviour. Empirical as well as integrative theoretical contributions are welcome from all fields related to ideomotor theory.

Table of Contents

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EDITORIAL
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Editorial: Action effects in perception and action

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Good scientific theories should be simple, valid, and stimulating. It seems that ideomotor theory, which has been the core theme behind the research topic on "*Action Effects in Perception and Action*," has done a fairly good job in terms of these three criteria. First, it is rather simple: goal-directed actions are assumed to be selected and addressed by anticipating their sensory consequences; crucially, learned bidirectional associations between sensory representations and motor commands ensure that these anticipations eventually result in overt behavior. Secondly, numerous observations comply with its basic predictions, derived from philosophical analyses of the nineteenth century (cf. Stock and Stock, 2004; Pfister and Janczyk, 2012). Accordingly, the validity of ideomotor theory has been documented by extensive empirical research over the last decades (e.g., Elsner and Hommel, 2001; Hommel et al., 2001; Kunde, 2001; see also Shin et al., 2010).

Thirdly, ideomotor theory (still) seems to stimulate contemporary research. Otherwise the impressive range of topics that have come together in the present research topic can hardly be explained. These topics range from investigations of how attention and perception are modulated by intentions and expectations (Kemper et al., 2012; Wykowska and Schubö, 2012), to applied settings such as aging and tool-use (Sutter et al., 2012), task-switching (Lukas et al., 2013), to social influences on action coding (Colzato et al., 2012; Nishimura and Michimata, 2013) and a developmental perspective on action effects in object manipulation (Knudsen et al., 2012). These new perspectives are backed up by studies on two prevailing questions in ideomotor research: The formation of action-effect associations (Herwig and Waszak, 2012; Janczyk et al., 2012; Ruge et al., 2012)—including a first step toward addressing individual differences in ideomotor learning (Muhle-Karbe and Krebs, 2012)—and the role of such associations for action control (Gaschler and Nattkemper, 2012; Walter and Rieger, 2012; Ziessler et al., 2012).

Furthermore, three notable articles explore theoretical refinements of ideomotor theory by addressing the virtue of visuomotor priming for ideomotor research (Thomaschke, 2012), hierarchical coding of action-effect relations (Ondobaka and Bekkering, 2012) and computational constraints for ideomotor theory (Herbort and Butz, 2012).

In the light of these and other recent empirical and theoretical advances (cf. Shin et al., 2010), it seems as if twenty-first century ideomotor theory accounted for almost all areas of cognitive psychology. On careful consideration, however, it also seems as if a particular area is still underrepresented in the ideomotor community, and this area is the concept of working memory. Whereas there are a several short hints to "memory traces" or "long-term memory" throughout the articles of the research topic, the concept of working memory is mentioned only a single time (Thomaschke, 2012, p. 4). Arguably, however, anticipated action effect must be represented somewhere in the cognitive architecture—and working memory appears a likely place for these representations. In our view, this state of affairs is indicative of the current theoretical state and calls for a better exchange between the respective scientific communities.

Similarly, while the sketched developments and directions are admirable on their own, they also pose a new challenge for scholars of action and perception. This challenge relates to an explicit treatment of the relations—commonalities and differences—of the ideomotor approach to other general frameworks for action and perception. For instance, the neuroscientific approaches of predictive coding (Rao and Ballard, 1998), the Bayesian brain (Knill and Pouget, 2004), and the free-energy principle (Friston, 2010) seem to share many features with the principles of effect-based action control even though the different accounts are rarely discussed in the same place (and are nourished by distinct scientific communities). In the same vein, relations to accounts for the perception of self-generated action effects (Haggard et al., 2002; Baess et al., 2011; Moore and Obhi, 2012) need a more explicit treatment, and so do the relations to mathematical models of human motor control (Wolpert and Ghahramani, 2000).

In the meantime, we would like to thank all authors who joined the enterprise of this research topic, and all reviewers who commented on the presented papers. It was a pleasant enterprise from beginning to end, i.e., from sending out the first invitations up to the final, joint action effect which is the research topic itself.

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Similar mechanisms of movement control in target- and effect-directed actions toward spatial goals?

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Previous research has shown that actions conducted toward temporal targets and temporal effects are controlled in a similar way. To investigate whether these findings also apply to spatially restricted movements we analyzed movement kinematics of continuous reversal movements toward given spatial targets and toward self-produced spatial effects in two experiments. In Experiment 1 target- and effect-directed movements were investigated in three different goal constellations. A spatial target/effect was always presented/produced on one movement side, on the other side either (a) no target/effect, (b) the same target/effect, or (c) a more difficult target/effect was presented/produced. Results showed that both target-directed and effect-directed movements have a typical spatial kinematic pattern and that both can be equally well described by linear functions as suggested by Fitts' Law. However, effect-directed movements have longer movement times. In Experiment 2 participants performed target-directed movements to the one side and effect-directed movements to the other side of a reversal movement. More pronounced spatial kinematics were observed in effect-directed than in target-directed movements. Together, the results suggest that actions conducted toward spatial targets and spatial effects are controlled in a similar manner. Gradual differences in the kinematic patterns may arise because effects are cognitively more demanding. They may therefore be represented less accurately than targets. However, there was no indication of qualitative differences in the cognitive representations of effects and targets. This strengthens our assumption that both targets and effects play a comparable role in action control: they can both be viewed as goals of an action. Thus, ideomotor theories of action control should incorporate action targets as goals similar to action effects.

Keywords: action targets, action effects, motor control, visual-spatial action goals, movement kinematics, ideomotor theory

INTRODUCTION

Every day we perform intentional, goal-directed actions. Action goals differentiate an action from pure movement and fall into two broad categories. The goal of an action can either consist of generating a change in the environment (i.e., to produce an effect, for example turning on a switch in order to illuminate a dark room) or of changing one's own situation in the environment (i.e., to move to a physical target, for example reaching out in order to grasp a cup). In the following we refer to these different types of goal-directed actions as effect-directed and target-directed actions, respectively.

Action goals have been known to play an important role in movement organization for a long time. In the present paper action goals are viewed in the light of the ideomotor theory of action control (James, 1890/1981; Prinz, 1997). The ideomotor theory has found broad empirical evidence (Elsner and Hommel, 2001, 2004; Hommel et al., 2003; for a historical overview see Stock and Stock, 2004) and states that an action is selected, initiated, and executed by anticipating the perceptual consequences of the action in question. Here we assume that both targets and effects are represented as action goals in motor control in the sense of the ideomotor theory. The representation of the intended perceptual consequences, in both target- and effect-directed movements, is responsible for the initiation, selection, and execution of a movement. In effect-directed actions the goal is the production of the effect and the manipulation of the environment itself. Target-directed actions also entail the representation of action goals such as "to be at a certain place at a given time."

However, so far studies investigating predictions derived from ideomotor theory have mainly been concerned with the role of action effects. If action targets are considered at all, they are usually not treated as major goals of an action but as subgoals. For example, action targets are sometimes defined as the location at which an event has to occur (e.g., participants perform a key press in a certain location) before an effect occurs (e.g., an effect tone; Hoffmann et al., 2009). In this kind of situation targets and effects are related, and effects are higher in the goal hierarchy. In other terms, according to ideomotor theories, which distinguish between proximal (related more closely to the body) and distal (related to the environment) action effects (Prinz, 1987; Hoffmann et al., 2007), effects are more distal than targets in such experiments. Such a scenario applies of course to many everyday situations but not to all. As outlined above, it is not always the goal of an action to produce a change in the environment (to produce an effect), but it is also sometimes the goal to change one's own situation in the environment (e.g., to move to a target). In the present study, we treated targets and effects as two different types of goals, which may be hierarchically equal and independent from each other. Thus, we designed the experiments in a way that the cognitive representations of targets and effects reside on the same level of "distality." Participants moved to visuo-spatial targets and moved to produce visuo-spatial effects. In both instances, participants received the same proximal effects (i.e., proprioception, kinesthesis), but the distal goal representations differed. With effects, the distal goal representation consisted of the occurrence of the effect, whereas with targets the distal goal representation consisted of being in a certain position. Still, as both goal representations are major action goals, they should have a similar influence on movement execution.

Thus, the major goal of the present study was to investigate the commonalities and differences between target-directed and effect-directed actions and their underlying mechanisms of action control. Recently, we have shown that the same mechanisms of action control underlie movements directed toward auditory-temporal targets and auditors-temporal effects (Walter and Rieger, 2012). Walter and Rieger (2012) showed that typical temporal movement kinematics emerged when participants synchronized movements with regularly presented tones (target-directed movements) or produced tones themselves (effect-directed movements). We concluded that both targets and effects can be seen as goals of an action influencing movement execution by the anticipation of upcoming events. This study however only investigated auditory-temporal stimuli as action goals. In the present study, we wanted to investigate whether our previous conclusions extent to visual-spatial action goals. This is not self-evident, because differences in the way spatially and temporally restricted movements are controlled are observed in some studies (e.g., Heuer, 1993; Franz et al., 1996; Maslovat et al., 2011).

The role of visual-spatial targets for movement planning and initiation has been demonstrated. For example, people bring their hand in a position that may be uncomfortable at the beginning of a grasping movement but that will allow them to be in a comfortable posture that facilitates optimal control at the end of the movement (known as the end-state comfort effect, for a review see Rosenbaum et al., 2012). Further, if participants have initial information about a second target in a two-step movement sequence, but no information about the first target before the beginning of the sequence, movements are initialized faster than when they have no information about both targets in the sequence (Herbort and Butz, 2009). This finding is consistent with models of anticipatory movement planning that claim that in a movement sequence each step is planned in reverse order (Fischer et al., 1997) and confirms the assumption that upcoming targets are processed and movement execution toward them can be partially planned, resulting in faster movement initiation.

A wide variety of studies investigated the role of visual-spatial targets for movement execution. Over a century ago Woodworth described that it is impossible to be fast and accurate at the same time when moving toward a visual target (Woodworth, 1899). This limitation of the motor system known as speed-accuracy tradeoff has been mathematically described by Fitts (Fitts, 1954; Fitts and Peterson, 1964) showing that movement time (MT) increases linearly with task difficulty. Fitts specified task difficulty (index of difficulty: ID) as a function of target width and target distance (for a review and different ways to calculate ID see Plamondon and Alimi, 1997). This relation is widely known as Fitts' Law and has inspired scientific research until today, especially in the field of human computer interface studies. Fitts' Law holds for bimanual tasks as well as tasks performed by dyads (Mottet et al., 2001). Further, Fitts' Law can be applied for translational as well as rotational movements (Stoelen and Akin, 2010) and has been studied intensively for distant aiming tasks with computer devices (Kopper et al., 2010). Whereas most studies investigated pointing and aiming with discrete tasks (for a review see Elliott et al., 1991), in some studies continuous tasks were used (e.g., Mottet et al., 2001). The kinematics of movements aimed at spatial targets frequently show asymmetric velocity profiles (Elliott et al., 2001). Specifically, movements toward spatial targets show a kinematic pattern that differs substantially from the kinematics of movements toward non-targets. Movements toward spatial targets reach peak velocity earlier and have relatively long MTs (Rieger, 2007). We will refer to this pattern as spatial movement kinematics in the following. Such spatial movement kinematics lead to prolonged time in the target area at the end of the movement. This additional time can be used to increase spatial accuracy (Novak et al., 2000; Elliott et al., 2001; Rieger, 2007).

Studies investigating the role of visual-spatial effects have mainly been conducted in the context of the ideomotor theory of action control (e.g., Hommel, 1993; Hommel et al., 2001; Kunde et al., 2007). It has been shown that participants respond faster if an action produces an effect that is spatially compatible with their response (action-effect-compatibility, e.g., Kunde, 2001). Kunde (2001) showed that in compatible conditions (e.g., a left hand key press produces a light flash on the left side of the monitor) responses are initiated faster than in incompatible conditions (e.g., the left hand key press produces a light flash on the right side of the monitor). The role of action effects has also been investigated when participants use tools for generating visual-spatial action effects. For example, when participants produce a rightward or leftward movement of a cursor on a display (that is a visual-spatial effect) by moving a steering wheel clockwise or counter-clockwise, movements are initiated faster when stimulus location (left-right tones) correspond to the direction of the produced effect (stimulus-effect-compatibility, Proctor et al., 2004). Similarly, mental rotations facilitate manual rotations when the direction of the visual effect is compatible with the mental rotation (Janczyk et al., 2012). Whereas many studies investigated the role of visual-spatial effects for movement selection and initiation the question of their role for movement execution is rarely addressed. In other domains, it has however been shown that effect anticipation also affects action execution (Kunde, 2003; Kunde et al., 2004).

To sum up, the existing literature on the role of visual-spatial targets and the role of visual-spatial action effects for movement control suggests that visual-spatial targets as well as visual-spatial effects may both serve as action goals in the sense of the ideomotor theory. To the best of our knowledge the role of visual-spatial targets and effects for action control has however not been systematically investigated in one study under comparable conditions when they reside on the same level of "distality." This is what we did in the present study.

Even though targets and effects may both serve as action goals, physical targets and effects also have some features that make them clearly distinguishable from each other. Targets are externally generated and usually present in the environment before, during, and after the movement. Thus they can provide precise information for movement aiming and movement correction. In contrast, effects are only present in the environment after the movement has been executed (and often only for a limited amount of time) and their anticipatory representation relies solely on internal generation. As a consequence, memory and learning processes play a more prominent role in effect-directed than target-directed movements. Attention demands may also be higher in effect-directed movements than in target-directed movements, because in addition to other types of feedback the visual action effect has to be monitored in effect-directed actions. As a consequence, performing effect-directed in comparison to target-directed actions should be cognitively more demanding.

Thus, evidence suggests that movements toward spatial targets could be controlled in a similar way as movements toward spatial effects, as they are both goals of an action. Their different features could however also lead to differences in movement control. In the present study we wanted to investigate whether movements toward spatial targets and spatial effects are controlled in a similar way by comparing movements toward visual-spatial targets and movements toward self-produced visual-spatial effects. To this aim, we compared the kinematics of movements generating visual-spatial effects and the kinematics of movements toward visual-spatial targets. Participants performed continuous reversal movements on the medial-lateral axis. In target-directed movements they reversed their movement on constantly presented spatial targets, whereas in effect-directed movements they produced spatial stimuli themselves. We analyzed *how* target-directed and effect-directed movements are executed.

EXPERIMENT 1

Participants performed continuous reversal movements on the medial-lateral axis. They were asked to move continuously back and forth and reverse their movements within black boxes that were constantly present during an experimental trial (target conditions) or were asked to move constantly back and forth and to produce black boxes in the same position as in target conditions when they reverse their movements. We analyzed *how* target-directed and effect-directed movements are executed.

Targets and effects were presented in three different goal constellations (see **Figure 1**, left panel). On one side of the movement always the same standard box was presented/to-be-produced. On the other side either (a) no box (one goal constellation), (b) the same standard box (same goals constellation), or (c) a different

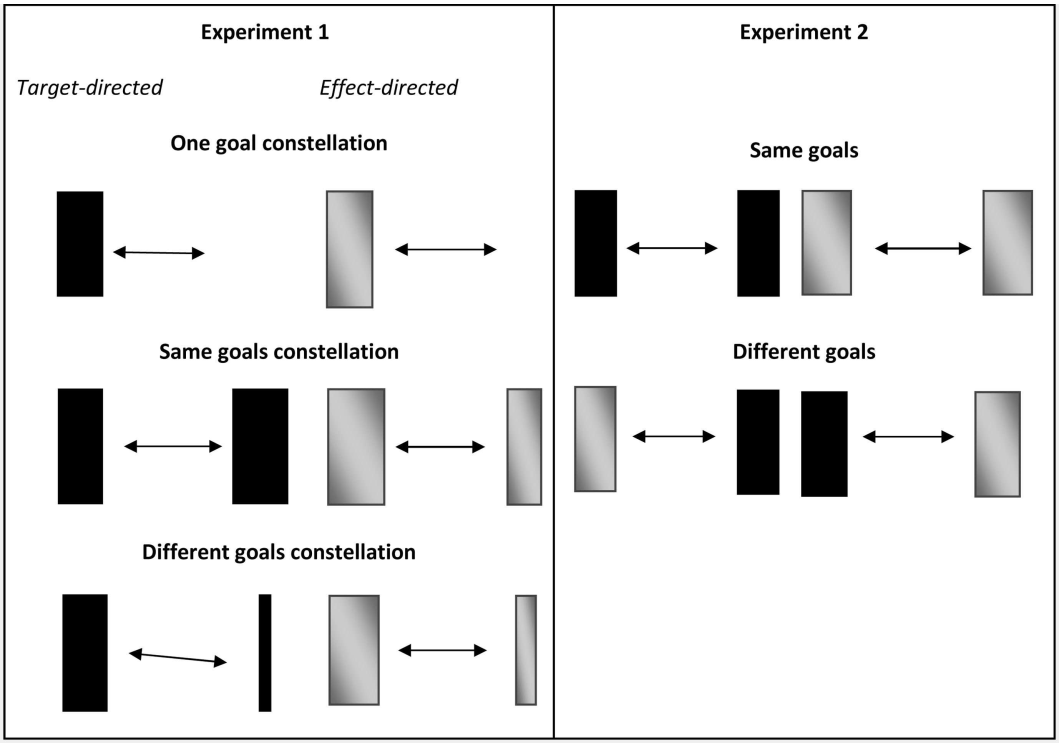

FIGURE 1 | Graphical overview of the goal constellations in Experiment 1 and Experiment 2. Black boxes represent targets, gray boxes represent effects. Note that the color of targets as well as effects was black in the experiment. Wide boxes represent standard boxes (width: 2 cm, ID: 2.7), narrow boxes represent the more difficult boxes (width: 0.56 cm, ID: 4.3). In target-directed movements participants were asked to reverse their movements within constantly presented black boxes, while in effect-directed movements such boxes were self-produced as they only appeared whenever participants reached the *x*-position of the inner edge of the to-be-produced boxes.

box with a higher Index of difficulty (different goals constellation) was presented/to-be-produced.

We expected that in the one goal constellation both target- and effect-directed movements toward the standard box show spatial kinematic patterns (early peak velocity, relatively long movement times) compared to movements toward the no box side. No such differences should be observable in the same goals constellations. In different goals constellation target-directed movements toward the more difficult box (Fitts, 1954) should show more pronounced spatial movement kinematics compared to movements toward the standard box. As we assume that both targets and effects can be viewed as goals of an action we expected to observe similar movement kinematics in target and effect conditions. We expected that effect-directed movements have higher spatial variability since the exact position of the effect is only seen at the endpoint of the movement and thus has to be remembered, which is cognitively more demanding. Nevertheless, we expected that Fitts' Law (Fitts, 1954) can equally well describe target and effect conditions. The comparison of target- and effect-directed movements across goal constellations is of particular interest in order to investigate how the goal representations in target- and effect-directed movements are formed. Not only the presence/absence of a visual target is important for movement execution, but also its characteristics (i.e., target width). It is not clear, whether this will also be observed for self-produced visual effects. If only the presence/absence of a visual effect is represented but not its characteristics (width), movement kinematics in the same and different goals constellation should not differ in the effect condition (but they should differ from the kinematics in the one goal constellation). However, if the characteristics of the visual effect (width) are represented in effect conditions, movement kinematics in the same goals and different goals constellation should differ from each other, similar to what we expect in target conditions.

METHOD

Participants

Twenty healthy participants (10 female) took part in this experiment. All of them were right-handed according to Edinburgh Inventory (Oldfield, 1971) with a mean laterality quotient of 91 ($SD = 15$). Their mean age was 25.6 years ($SD = 2.4$ years). All of them reported normal or corrected-to-normal vision. They gave informed consent prior to the experiment and received 7 Euro for participation.

Materials and apparatus

Movements were recorded with a 30.5 cm × 45.5 cm Wacom Ultrapad A3 writing pad at a resolution of 500 pixels per cm and at a rate of 172 Hz that was placed on a desk. Participants performed movements with their right (dominant) hand, which was shielded from view by a cover. Participants were able to see their movement trace consisting of a blue circle (4 mm in diameter) on a screen (17″, resolution: 1024 × 768 pixels, vertical refresh rate: 100 Hz). Movement distance on the writing pad equaled movement distance on screen. The screen was placed behind the pad at a distance of 60 cm from the participants and 9 cm higher than the pad. Spatial stimuli consisted of black boxes (distance between the centers 10.6 cm, standard width: 2 cm, $ID = 2.7$, more difficult width: 0.56, $ID = 4.3$) presented 5.3 cm to left and/or the right of the middle of the screen. If only one box was present a black line of 10.6 cm length aligned horizontally in the middle of the screen indicated the approximate length of a movement in a demonstration phase. A red box (0.5 cm × 0.5 cm) presented in the middle of the screen served as a starting box. The software Presentation 14.1 was used for stimulus presentation and data recording.

Procedure

The experiment took place in a dimly lit room. Participants were asked to perform continuous reversal movements on the medial-lateral axis without pausing at the reversal points. Movements were performed in two different goal conditions: target condition and effect condition. When performing target-directed movements, participants were asked to reverse their movements within constantly presented black boxes. When performing effect-directed movements, participants were asked to produce such boxes themselves. Before trials in the effect conditions started these black boxes were presented in an 8 s demonstration phase and participants were instructed to vividly keep the position and the width of the boxes in mind without moving. During experimental trials the box/boxes only appeared when participants reached the x-position of the inner edges of the (at this point in time not visible) boxes. In the instructions for the effect condition, participants were asked to produce such boxes of the same width and at the same position at their movement reversals. In both goal conditions, participants were asked to perform the task as fast and as accurately as possible.

At the beginning of the experiment participants received general instructions explaining all goal constellations and types of movements. Detailed instructions and visual stimuli were also presented on the screen before each trial. Participants started a trial themselves by entering the starting box, which appeared together with the instructions, with their pen whenever they were ready to begin. Trial duration was always 40 s.

Participants performed four training trials: two target condition trials and two effect conditions trials, each in the one goal constellation and the same goals constellation. The combination of three different goal constellations with two goal conditions, together with the balancing of the locations (left, right) of the standard box resulted in 12 experimental trials (in the same goals constellation the same number of trials as in the other constellations was conducted). Trials were presented in random order (restriction: not more than three trials of the same goal condition in a row). Participants completed three series of these 12 trials, after each of those series they had the opportunity to take a short break. The whole experiment took approximately 45 min.

Data analysis

Raw data were smoothed with a non-linear smoothing algorithm (Mottet et al., 1994) by using weighted and moving medians in a seven data point window. After that, pen velocity was determined at each measured point in time (i.e., every 5.8 ms) and then also smoothed with the same algorithm. The first 10 s of each trial were excluded from further analyses. For every goal condition in every goal constellation six trials were available for analysis. Since displacements on the y-axis were small ($M = 0.29$ cm, $SD = 0.28$ cm), only the maximum displacements on the x-axis were analyzed.

The reversal points (onsets and endpoints of a movement in one direction) were defined as the most leftward or rightward points of a movement followed by two data points indicating that the movement direction had changed. Movements were excluded from analysis if (a) participants did not move continuously (not more than 1 mm within the first 50 ms of a movement), (b) movement length was smaller than 5.3 cm (i.e., half of the instructed length of a movement), and (c) participants did not cross the middle line of the screen. Using these criteria less than 1% of movements were excluded from analyses in both target and effect conditions. A preliminary data analysis indicated that there were no differences in the data patterns between movements to the left and the right side. Therefore data were collapsed over this factor. The following statistical procedures were applied to both experiments: (a) if appropriate we report Greenhouse–Geisser corrected F values, (b) only higher order effects are reported if the lower order effects cannot be interpreted on their own, (c) significant effects were further analyzed using paired-sample t-tests, and (d) if appropriate Bonferroni corrected p values are reported.

The following set of dependent variables was analyzed in both experiments. To characterize the shape of trajectory, the time to reach peak velocity relative to the complete duration of the movement (proportional time to peak velocity in %, PTPV), and the time spent on one movement relative to the time spent on the complete reversal movement (proportional movement time in %, PMT) were analyzed. To characterize temporal performance the duration of a whole reversal movement (in ms, RMT) was analyzed. To characterize spatial performance the variability around the average endpoint of a movement (in cm, EP_V) and movement distance on the x-axis (in cm, Dist_X) were calculated. PTPV, PMT, and EP_V were analyzed using $3 \times 2 \times 2$ repeated measurements analyses of variances (ANOVAs) with the factors GoalConstellation (one goal, same goals, different goals), GoalCondition (targets, effects), and BoxType (standard, manipulated). Note that "manipulated" in the factor Box Type can stand for no box (one goal constellation), the same standard box (same goals constellation), or the more difficult box (different goals constellation). RMT and Dist_X were subjected to 3×2 factors ANOVAs with the factors GoalConstellation (one goal, same goals, different goals) and GoalCondition (targets, effects), because those variables cannot be calculated separately for both sides of the reversal movement.

Furthermore, we calculated effective Index of Difficulty (eID) using effective target width (Welford, 1968; Zhai et al., 2004). In order to analyze whether the same amount of variance is explained by Fitts' Law in target and effect conditions, we used eID and MT of every condition and computed correlations between eID and MT for every participant. The individual correlations were z-transformed (Fisher's z-transformation). t-Tests were run on those transformed values. The average correlations reported here in the text are reconverted from the average Fisher's z-values. We also calculated individual linear regression functions for each participant and each goal condition (target, effect) and used the estimated β values and intercepts for *post hoc* t-test analyses.

As our hypotheses partly consist of null-hypotheses (i.e., we expect no significant differences between target- and effect-directed movements) we calculated confidence intervals in order to assess whether differences between the two conditions are likely to be meaningful (Loftus, 1996). Confidence intervals for within-participant designs were calculated from normalized data according to Cousineau (2005), with the correction procedure suggested by Morey (2008). To gain further evidence for a functional similarity of target- and effect-directed movements we also calculated Pearson correlations between target and effect conditions for PTPV and PMT for each participant. Individual correlations were Fisher z-transformed and the average correlation coefficients reported here are reconverted from the average Fisher's z-values.

RESULTS

Shape of trajectory

Proportional time to peak velocity. There was a significant interaction between GoalConstellation and BoxType, $F(2, 38) = 17.16$, $p < 0.001$, $\eta_p^2 = 0.48$ (see **Figure 2**). In the one goal constellation PTPV was lower when moving toward the standard box ($M = 41.7\%$) than when moving away from it to the no box side ($M = 45.3\%$). In the different goals constellation the opposite pattern was observed: when moving toward the more difficult box, PTPV was lower ($M = 35\%$) than when moving toward the standard box ($M = 42.7\%$). No such difference between the sides was observed in the same goals constellation. There were no significant main effect of and no significant interactions with the factor GoalCondition, indicating that effect- and target-directed movements were performed in a similar way. The average correlation between target conditions and effect conditions was high ($r = 0.78$) also pointing to a functional similarity between them.

Proportional movement time. A significant interaction between GoalConstellation and BoxType, $F(2, 38) = 10.94, p < 0.001, \eta_p^2 = 0.37$ was observed (see **Figure 3**). In the one goal constellation PMT was higher for movements toward the standard box ($M = 51.4$) in comparison to movements to the no box side ($M = 48.6\%$). The reverse pattern was observed in the different goals constellation. Here PMT toward the more difficult box was higher ($M = 52.9\%$) than toward the standard box ($M = 47.1\%$). No such difference between the sides was present in the same goals constellation. Again, there were no significant main effect of and no significant interactions with the factor GoalCondition. Further, again the average correlation between target and effect conditions was high ($r = 0.89$).

Temporal performance

Reversal movement time. There was a significant main effect of GoalConstellation, $F(2, 38) = 13.84$, $p < 0.001$, $\eta_p^2 = 0.42$. RMT in the one goal constellation ($M = 1071$ ms) did not differ significantly from RMT in the same goals constellation ($M = 1137$ ms), but RMT in the different goals constellation ($M = 1415$ ms) was significantly higher than in both other constellations ($p < 0.05$). This finding can be attributed to the presence of a more difficult spatial goal in this constellation than in the other constellations. A significant main effect of GoalCondition, $F(1, 18) = 9.54, p < 0.006, \eta_p^2 = 0.33$, indicated that RMT was higher in effect-directed movements ($M = 1245$ ms) than in target-directed movements ($M = 1171$ ms; see **Table 1**).

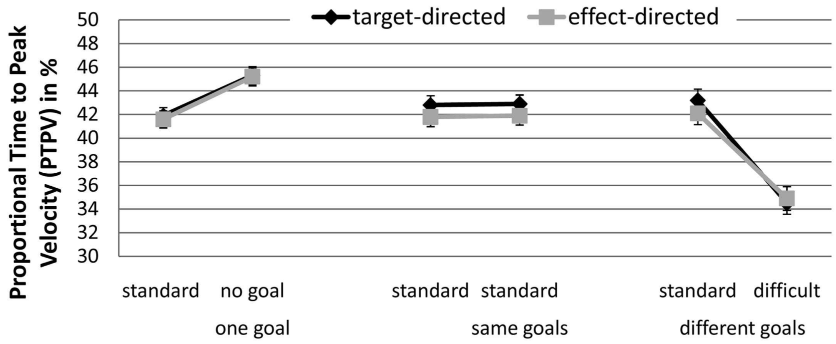

FIGURE 2 | Experiment 1: means and confidence intervals of Proportional Time to Peak Velocity in % (PTPV).

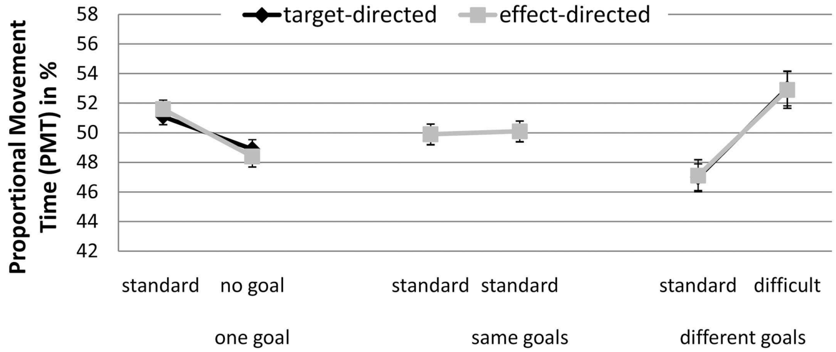

FIGURE 3 | Experiment 1: means and confidence intervals of Proportional Movement Time in % (PMT).

Table 1 | Experiment 1: variables describing temporal and spatial performance.

	One goal *M* **(CI)**		**Same goals** *M* **(CI)**		**Different goals** *M* **(CI)**	
Reversal movement time in ms (RMT)						
Target-directed	1045 (391)		1103 (588)		1365 (892)	
Effect-directed	1097 (495)		1172 (601)		1465 (472)	
	Standard	**Manipulated**	**Standard**	**Manipulated**	**Standard**	**Manipulated**
Endpoint variability in cm (EP_V)						
Target-directed	0.53 (0.1)	0.85 (0.1)	0.6 (0.1)	0.6 (0.1)	0.54 (0.1)	0.43 (0.1)
Effect-directed	0.56 (0.1)	0.82 (0.1)	0.57 (0.1)	0.59 (0.1)	0.56 (0.1)	0.51 (0.1)
Movement distance on the *x*-axis in cm (Dist_X)						
Target-directed	10.8 (0.15)		10.9 (0.12)		10.9 (0.11)	
Effect-directed	10.6 (0.15)		10.8 (0.12)		10.9 (0.13)	

Means and confidence intervals (in parenthesis) of Reversal Movement Time in ms (RMT), Endpoint Variability in cm (EP_V), and Movement Distance on the x-axis in cm (Dist_X).

Spatial performance

Endpoint variability. There was a significant GoalConstellation × BoxType interaction, $F(2,\ 38) = 14.84$, $p < 0.001$, $\eta_p^2 = 0.44$, that indicates that in the one goal constellation movements toward the side with the standard box ($M = 0.54$ cm) had a lower EP_V than movements to the no box side ($M = 0.84$ cm; see **Table 1**). In contrast, in the different goals constellation lower EP_V was observed in movements toward the more difficult box ($M = 0.47$ cm) in comparison to movements toward the standard box ($M = 0.55$ cm; all $p < 0.05$).

Movement amplitude on the x-axis. There was a main effect of GoalCondition, $F(1, 19) = 5.9$, $p < 0.025$, $\eta_p^2 = 0.24$.Target-directed movements ($M = 10.9$ cm) had higher MA than effect-directed movements ($M = 10.7$ cm; see **Table 1**).

Functions according to Fitts' Law. The correlation eID and MT was $r = 0.30$ in the effect conditions and $r = 0.38$ in the target condition (see **Figure 4**). These correlations did not significantly differ from each other, $t(19) = 1.12$, $p > 0.05$, indicating that the amount of variance explained by a linear relationship between eID and MT did not significantly differ between both types of movement. Fitting functions were also similar: β values, $t(19) = -0.74$, $p > 0.05$, and intercepts, $t(19) = 0.82$, $p > 0.05$, did not significantly differ between the target condition [$R^2 = 0.46$, $p < 0.05$; M (β) = 208, SD = 160; M (intercept) = 41, SD = 341] and the effect condition [$R^2 = 0.54$, $p < 0.05$; M (β) = 302, SD = 514; M (intercept) = −319, SD = 1850].

DISCUSSION

We conducted Experiment 1 in order to find out whether similar mechanisms of action control underlie movements toward presented visual-spatial targets and self-produced visual-spatial effects. Overall the data show that the movement kinematics are very similar in target- and effect-directed actions. We observed no main effect of GoalCondition and no interactions with the factor GoalCondition in PTPV and PMT. Both movement types can be equally well described by a linear Fitts' function, and the functions were not significantly different from each other. Moreover, no differences in EP_V between both movement types were observed. A typical relative spatial kinematic pattern was obtained in the one goal constellation: when moving toward the standard box PTPV was lower and PMT was higher than when moving to the no box side. This pattern reverses in the different goals constellation: here PTPV was lower and PMT was higher when moving toward the manipulated (more difficult) box side than when moving toward the standard box side. Spatial variability as described by EP_V follows the same pattern: in the one goal constellation movements toward the manipulated box side (no box) have higher EP_V, in the different goals constellation movements toward the standard box side have higher EP_V. In the different goal constellation movements have also a longer RMT. Small differences between target-directed and effect-directed movements were also obtained. Effect-directed movements have higher RMT and smaller movement amplitudes on the x-axis than target-directed movements.

As expected, target-directed and effect-directed movements are performed in a similar way. When comparing movements toward a spatial goal with movements toward a side without a goal a typical spatial kinematic pattern (low PTPV, high PMT) emerges no matter if aiming toward a spatial target or producing a spatial effect. For both types of movement it can therefore be assumed that this kinematic pattern reflects the specific goal characteristics (here: spatial characteristics) and helps to achieve the goal of the movement (to perform movements spatially accurate). It has been speculated that the additional time in the target area at the end of the movement helps to improve spatial accuracy (Novak et al., 2000; Elliott et al., 2001; Rieger, 2007). Another hint for this assumption comes from studies showing that the skewness in velocity profiles increases as spatial accuracy demands increase and/or targets are small (Hogan and Flash, 1987; MacKenzie et al., 1987; Helsen et al., 1998; Elliott et al., 2001). Whereas this kinematic pattern has previously been observed in studies in which target-directed movements were investigated (Elliott et al., 2001; Rieger, 2007), we were able to demonstrate that it also occurs with effect-directed movements. The observation that both target-directed and effect-directed movements can be equally well described by a linear Fitts' function, and that the functions do not significantly differ from each other, also points to a functional similarity of both as goals of an action. Surprisingly, no differences in EP_V between both movement types were found. Thus, even though participants have to remember location and width in effect conditions they seem to fulfill this task quite well. In the different goals condition they show lower EP_V toward the more difficult goal side in both conditions. This result, together with the data on the shape of the trajectories suggests, that participants do not only represent target location but also target width in effect conditions.

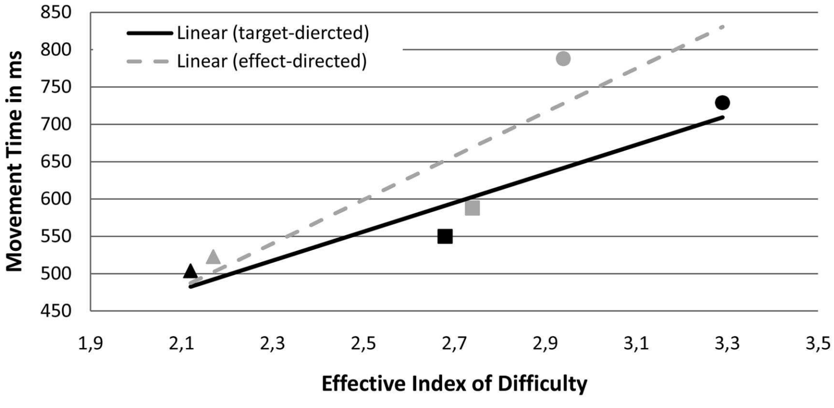

FIGURE 4 | Experiment 1: means and linear functions of the relation between effective Index of difficulty (eID) and movement time (MT in ms) for target-directed and effect-directed movements toward the manipulated goal. Triangles symbolize the one goal constellation, squares the same goals constellation, and circles the different goals constellation. Black markers indicate target conditions, gray markers indicate effect conditions.

Differences between both types of movement were also found: effect-directed movements have higher RMT and slightly shorter amplitudes (0.2 cm) than target-directed movements. Thus, even though the general movement *pattern* is the same as in target-directed movements, the data also point to differences between targets and effects. Those differences probably arise from higher cognitive demands in effect conditions: the need to remember the location of the effects, which may result in less precise goal representations. Those less precise goal representations may be compensated by longer reversal movement times and slightly shorter amplitudes.

To sum up, target-directed and effect-directed movements seem to be controlled in a similar manner. Movement execution is thereby influenced by the upcoming goal *before* the effect appears or the target is reached, indicating that goal anticipations are important for the way how a movement is executed. Differences between target- and effect-directed actions can be attributed to higher cognitive demands in effect conditions.

EXPERIMENT 2

Results of Experiment 1 indicated that spatial kinematics are comparable in target- and effect- directed movements to visual-spatial goals, pointing to similarities in their control mechanisms. However, data also indicated that effects are represented less precisely, probably due to higher cognitive demands. Whereas in Experiment 1 we compared movements toward targets and effects performed in different trials, in Experiment 2 we combined target-directed and effect-directed movements within trials (a target on one side of the reversal movement, an effect on the other side of the reversal movement). We expected that a direct comparison of target- and effect-directed movements within one trial may enhance differences between them. When participants are asked to move to targets and effects within one goal constellation, one of those goals may be dominant (i.e., result in a more pronounced representation) over the other goal. Further, this setup prevents that participants move at different overall speed levels and also prevents shorter MAs in effect-directed than in target-directed movements (as it was the case in Experiment 1).

Participants again performed continuous reversal movements on the medial-lateral axis to visual-spatial goals. There were four conditions: (a) target-directed movements on both reversal sides, (b) effect-direct movements on both reversal sides, (c) target-directed movements to the left side and effect-directed movements to the right side, and (d) target-directed movements to the right side and effect-directed movements to the left side.

Our hypotheses concerning the conditions with different goals on both sides of the reversal movement were undirected. On the one hand, the goal representation for the spatial target may be more pronounced than for the spatial effect, because the target is constantly visible. If this is the case, a more pronounced spatial kinematic pattern for the target side should be observed (higher PMT, lower PTPV in target-directed movements). On the other hand, as effect conditions seem more difficult, participants may devote more of their cognitive resources to the effect and thus, the effect representation may be more pronounced than the target representation. If this is the case, effect-directed movements should show a more pronounced spatial kinematic pattern (higher PMT, lower PTPV in effect-directed movements). We further expected, based on the results of the same goals constellation condition in Experiment 1, that no differences in movement kinematics between targets and effects occurs when the same type of movement is conducted toward both sides.

METHOD

Participants

Twenty healthy participants (11 female; mean age = 23.7 years, SD = 3.0) took part. According to the Edinburgh Inventory (Oldfield, 1971) all of them were right-handed (mean laterality quotient = 94, SD = 10). All of them reported normal or corrected-to-normal vision. They gave informed consent and received 7 Euro for participation. None of them had participated in Experiment 1.

Materials and apparatus

The experimental setup was the same as in Experiment 1. Therefore only differences are reported here. Visual stimuli consisted the standard boxes of Experiment 1 (black boxes, width: 2 cm, height: 9 cm, ID = 2.7, presented 5.3 cm to left and to the right of the middle of the screen).

Procedure and design

Visual-spatial goals were presented in four different goal combinations: two with same goals which were (a) target-directed movements on both reversal sides (target condition), and (b) effect-direct movements on both reversal sides (effect condition), and two with different goals which were (c) target-directed movements to the left and effect-directed movements to the right side, and (d) target-directed movements to the right and effect-directed movements to the left side (see **Figure 1**, right panel).

As in Experiment 1 participants were instructed to perform target-directed and effect-directed movements. In conditions in which targets and effects were combined participants were asked to reverse the endpoints of their movements within the constantly presented black box on one side. When performing effect-directed movements, participants were asked to produce such boxes themselves as in Experiment 1. Each condition was preceded by instructions and an 8 s demonstration phase of the widths and positions of the boxes. Participants were instructed to keep those vividly in mind and to produce them in the effect conditions during the experimental trials. Trial duration was always 40 s.

Each of the four goal combinations was conducted five times resulting in 20 experimental trials. Before the experimental trials were conducted participants performed four training trials, one in each condition. Trials were presented in random order with the exception that not more than three trials of the same condition were performed consecutively.

Data analyses

Data preparation was conducted as in Experiment 1. The first 10 s of the each experimental trial were excluded from further analyses. As again displacements on the y-axis were small ($M = 0.43$ cm, SD = 0.41 cm) only displacements on the x-axis were analyzed. The same exclusion criteria as in Experiment 1 were applied, leading to exclusion rates of less than 1% in each condition. Because the data patterns for movements to the left and right side were similar, data were collapsed over this factor. PTPV and PMT were analyzed using 2 × 2 repeated measurement ANOVAs with the factors GoalConstellation (same goals, different goals) and GoalCondition (targets, effects). RMT and Dist_X were subjected to ANOVAs with the factor GoalConstellation (same targets, same effects, different goals).

RESULTS

Shape of trajectory

Proportional time to peak velocity. There was a significant interaction between GoalConstellation and GoalCondition, $F(1, 19) = 12.1$, $p < 0.003$, $\eta_p^2 = 0.34$. In the same goals constellation target- and effect-directed movements did not significantly differ in PTPV, whereas in the different goals constellation PTPV was significantly lower for effect-directed ($M = 44.5\%$) than for target-directed ($M = 48.1\%$; $p < 0.05$) movements (see **Figure 5**).

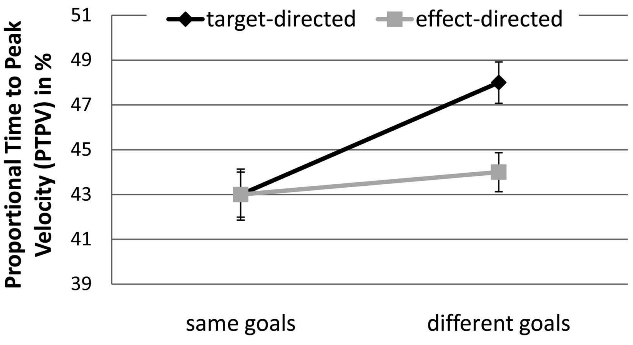

FIGURE 5 | Experiment 2: means and confidence intervals of Proportional Time to Peak Velocity in % (PTPV).

Proportional movement time. A significant interaction between GoalConstellation and GoalCondition, $F(1, 19) = 8.0$, $p < 0.011$, $\eta_p^2 = 0.3$, indicated that target-directed movements ($M = 49\%$) had lower PMT than effect-directed movements ($M = 51\%$) in different goals constellation, whereas no difference between the two types of movement was observed in same goals constellation (see **Figure 6**).

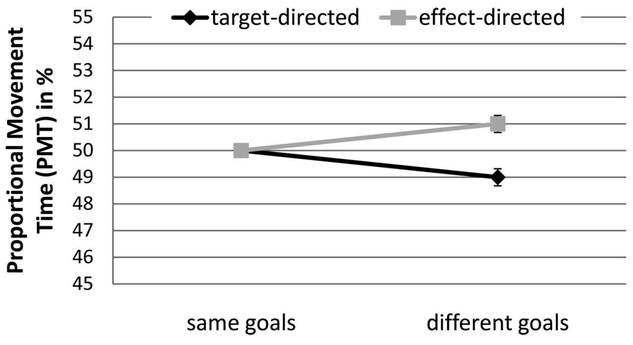

FIGURE 6 | Experiment 2: means and confidence intervals of Proportional Movement Time in % (PMT).

Temporal performance

Reversal movement time. The main effect of GoalConstellation was significant, $F(2, 38) = 4.1$, $p < 0.024$, $\eta_p^2 = 0.18$. Results were intransitive, only reversal movements in the same effects constellation took significantly longer ($M = 947$ ms) than movements in the different goals constellation ($M = 809$ ms, $p < 0.05$), whereas movements in the same targets constellation did not significantly differ from the other two conditions (see **Table 2**).

Spatial performance

Endpoint variability. There were no significant main effects or interactions (see **Table 2**).

Movement distance on the x-axis. There were no significant main effects or interactions, showing that participants moved comparable distances in all conditions (see **Table 2**).

Table 2 | Experiment 2: variables describing temporal and spatial performance.

Same targets *M* (CI)	**Same effects *M* (CI)**	**Different goals: targets *M* (CI)**	**Different goals: effects *M* (CI)**
Reversal movement time (RMT)			
895 (30)	947 (31)	809 (21)	
Endpoint variability in cm (EP_V)			
0.58 (0.006)	0.57 (0.006)	0.57 (0.005)	0.55 (0.006)
Movement distance on the *x*-axis in cm (Dist_X)			
10.8 (0.12)	10.7 (0.12)	10.8 (0.12)	

Means and confidence intervals (in parenthesis) of Reversal Movement Time in ms (RMT), Endpoint Variability in cm (EP_V), and Movement Distance on the x-axis in cm (Dist_X).

DISCUSSION

In order to enhance differences between effect-directed and target-directed movements, they were executed within the same reversal movement in one of the goal constellations of Experiment 2. Results of variables describing the shape of trajectory show that a more pronounced spatial kinematic pattern emerged in the different goals constellation toward effect-directed movements (lower PTPV, higher PMT). As expected, no significant differences were found in the same goals constellation. However, in the same effects constellation higher RMT were observed than in the different goals constellation. No significant effects were found in variables describing the spatial performance (EP_V and Dist_X).

As expected, based on the results of Experiment 1, no significant differences in shape of trajectory between target and effect conditions in the same goals constellation were observed. This provides further evidence for the functional equivalence of targets and effects as action goals. Interestingly, combining target- and effect-directed movements in one reversal movement enhanced differences between them: a more pronounced spatial kinematic pattern for effect-directed in comparison to target-directed movements was observed. Results of Experiment 1 suggested that effects have a less precise internal representation than targets. Thus, not the goal information provided by the experimental context (more precise in targets than in effects), but rather the cognitive resources devoted to the goal (more effortful for effects than targets) results in a more pronounced goal representation. This is in line with assumptions that movement kinematics are chosen in order to fulfill the task goals as well as possible (Rieger, 2007). In the same effects constellation significantly higher reversal movement time was observed, again underpinning the assumption that effects are represented less precise and are therefore more difficult to perform, which is then compensated with higher reversal movement time.

In summary, results of Experiment 2 again indicate that targets and effects are represented as action goals. However, less precise representation of effects is compensated by devoting more cognitive resources to effects, resulting in a more pronounced spatial kinematic pattern.

GENERAL DISCUSSION

We conducted the present study in order to investigate whether spatial targets and spatial effects play a comparable role in action control as action goals. This was done by analyzing *how* participants execute movements toward visual-spatial targets and visual-spatial effects. In two different experiments participants performed continuous reversal movements toward targets, effects or no goals. In Experiment 1 target-directed and effect-directed movements were compared across conditions in three constellations with varying goal features. In Experiment 2 both movement types were combined within one condition to enhance differences between them. Results indicated that the same mechanisms of action control underlie movements toward targets and effects, and that they are therefore equally represented as action goals. When compared across conditions no significant differences between targets and effects were observed in the shape of the trajectory (Experiment 1, and Experiment 2, same goals constellation) and in spatial variability (Experiment 1 and 2). Further, target- and effect-directed movements both show a more pronounced spatial kinematic pattern toward a goal than toward a no-goal (Experiment 1, one goal constellation). Similarly, both show a more pronounced spatial kinematic pattern toward a more difficult than toward an easier goal (Experiment 1, different goals constellation). In addition, both target-directed and effect-directed movements can be equally well described by Fitts' Law (Experiment 1). Differences between target- and effect-directed movements were observed when compared within conditions. Here effect-directed movements showed a more pronounced spatial kinematic pattern (Experiment 2). Effect-directed movements require that participants remember the effect location and use the remembered information to plan, initiate, and execute their aiming movement. To compensate for this less precise representation participants devote more cognitive resources to the effects. The higher cognitive demands also result in longer reversal movement times toward effects (Experiment 1, and Experiment 2, same effects constellation).

One may argue that participants simply produced repetitive movements of similar amplitudes toward the same locations in both, target and effect conditions. We intentionally designed target and effect conditions as similar as possible, as we wanted to avoid that other differences in the characteristics of targets and effects (apart from being a target or a effect) can account for the results. Thus, targets and effects only differed in one decisive aspect: targets did not depend on the action of the participant (i.e., they were always visible), whereas effects dependent on the action of the participant (i.e., appeared when participants reached the target area). As the target stimulus and the effect stimulus were physically the same, and due to experiencing the stimulus as a target in 50% of trials, one may be concerned that participants' experience of the effect as being self-produced may be reduced. This may have been the case if participants had repeatedly switched between target and effect conditions. However, in our experiments one trial always lasted for 40 s, which resulted in a stable current context (target or effect context) for the stimulus. Moreover, when combined within one trial (Experiment 2) differences between target-directed and effect-directed movements were enhanced. This indicates that participants indeed experienced target and effect conditions as different.

One may also be tempted to compare the visual effects in our study with what is termed visual feedback in other studies (e.g., Saunders and Knill, 2004; Roerdink et al., 2005; Thaler and Goodale, 2011). From a theoretical viewpoint, this is valid, because feedback certainly is an action effect. However, action effects in our study (appearance of a visual stimulus) were operationalized as the major goal of one reversal movement. In other studies investigating visual feedback the main purpose of a task is often not to "produce" the visual stimulus, but the visual feedback provides additional information about the current position. In addition to visual effects, participants also received visual feedback in our study: their current movement position was represented as a blue dot on the screen. Even though "effects" and "feedback" theoretically represent action effects, one may thus argue that the visual effects in our study (appearance of the boxes) reside on a higher level in the goal hierarchy of the task than visual feedback (cursor representing the current hand position), as it is the main purpose of the movement (or more specifically: the endpoint of the movement) to produce the effect which thus is the distal goal representation. It should be noted that in target conditions, participants also received visual feedback (cursor representing the current hand position). In target and effect conditions participants also received the same proximal effects/feedback (i.e., proprioceptive, kinesthetic). However, in target conditions participants received no visual effect. Rather, here the distal goal representation was to be at a certain position at a certain time.

Our results support the assumption that effect-directed movements are more difficult due to higher cognitive demands and that this is compensated by devoting more cognitive resources toward effects leading to a pronounced spatial kinematic pattern toward them. In line with this assumption are findings which indicate that (perceived) task difficulty influences movement kinematics. For example, Park and Kim (2008) manipulated target-size and movement amplitudes in a Fitts' task separately such that both manipulations resulted in the same indices of difficulty. They investigated self-terminated horizontal elbow-extension movements. The authors found different mechanisms of movement control leading to an increase of MT in both conditions. In the target-size condition a decrease in triceps and biceps muscle activation, and a decrease in movement velocity with increasing index of difficulty was observed in both, the acceleration and the deceleration phase. In the movement amplitude condition triceps activation after movement onset and biceps activation during deceleration increased with increasing index of difficulty, resulting in a higher peak velocity, even though MT also increased with increasing index of difficulty. Thus, they conclude that perceived task difficulty influences movement control, but not *de facto* task difficulty (held constant across conditions). Further, in a spatial aiming task reaction time and MT to a first target increased as a function of the number of elements only when either the full response or the number of elements that have to be performed were specified in advance of the starting stimulus (Khan et al., 2007). Khan et al. conclude that when the number of to be performed elements is known in advance more complex movement integration strategies are preprogramed, which leads to increased executive control and in turn results in longer reaction times as well as longer MTs. Along these lines we assume that higher cognitive demands in effect-directed movements are compensated by devoting more cognitive resources toward effects. This results in a more careful strategy of movement execution and leads to a more pronounced spatial kinematic pattern in effect-directed movements when they are combined with target-directed movements.

Besides that effect-directed movements are more difficult to perform, the here presented experiments show that both target-directed and effect-directed movements show a typical spatial kinematic pattern toward visual-spatial goals. We take this as evidence that both targets and effects can be viewed as goals of an action. In the case of effects the goal of the action is the production of the effect itself and in the case of targets the goal is "to be at a certain place." We assume that the representation of these goals shapes movement kinematics in the observed typical manner. As these goal representations are being formed *before* the movement is actually conducted and then influence its execution this is in accordance with ideomotor principles of action control, claiming that the anticipation of the intended consequences of an action influences movement selection (Knuf et al., 2001), initiation (Kunde, 2003), and also movement execution (Kunde et al., 2004). So far ideomotor theories mainly deal with action effects as action goals. Besides the possibility that proximal effects are produced at action targets (e.g., tactile sensations or sensations related to body postures) targets are neglected. In contrast, our study shows that both targets and effects may equally serve as action goals, evoking visual-spatial event anticipations. Ideomotor theories should thus be expanded to cover goal-based (including target- and effect-based), rather than only effect-based action control.

Both the here presented study and our study conducted with auditory-temporal goals (Walter and Rieger, 2012) show that the same mechanisms of action control underlie movements toward targets and effects as they can both be seen as goals of an action. This comparable result presented here is not obvious, as differences in the way spatially and temporally restricted movements are controlled are observed in some studies (e.g., Heuer, 1993; Franz et al., 1996; Maslovat et al., 2011). The findings of Walter and Rieger (2012) as well as the current study indicate that the equivalence of targets and effects as action goals holds for spatially as well as temporally restricted movements. This may also be the case in other modalities.

Note that the interpretation of our data relies partly on non-significant results. However, traditional null hypothesis testing does not tell us the probability that the null hypothesis is true (Cohen, 1994). Thus, drawing strong conclusions from non-significant results may be problematic. However, the very small confidence intervals, which indicate that the true deviation from H0 is unlikely to be large, an *a priori* hypothesized pattern in the data, and the high average correlations between target and effect conditions in the variables describing the shape of the trajectory in Experiment 1 render our explanation, that similar mechanisms of action control underlie target- and effect-directed actions, very likely.

Besides this general similarity in spatially and temporally restricted movements there is also a difference in the results from both studies: combining targets and effects within one reversal movement increased differences between effect- and target-directed movements toward spatial goals in the present study, whereas the same manipulation enhanced similarities between effect- and target-directed toward temporal goals in the previous study (Walter and Rieger, 2012). A reason for this can be that spatial targets and effects and temporal targets and effects may pose different demands on the cognitive-motor system. Spatial targets can be perceived all the time during a movement, whereas spatial effects cannot. In contrast, temporal targets and effects both only occur for a limited amount of time. Updating of timing in temporal targets can only occur at those points in time, whereas updating of the position of spatial targets can occur at any time. Thus, temporal targets and effects may be more alike in their degree of difficulty than spatial targets and effects. Consequently, when combined within one condition differences between temporal targets and effects are diminished as their similarity is then emphasized, whereas differences between spatial targets and effects are enhanced as they become more obvious, resulting in a more pronounced spatial kinematic pattern toward effects.

To conclude, movement kinematics toward spatial targets and spatial effects are shaped in a typical manner showing that both targets and effects can equally serve as action goals. Moreover, both target-directed and effect-directed movements can be described by Fitts' Law in a similar manner. Only small differences are found between target-directed and effect-directed actions. When combined within one condition more cognitive resources are devoted to effect-directed than to target-directed movements leading to a more pronounced representation of effects. The influence of the anticipation of upcoming events on movement execution is in accordance with ideomotor theories of action control. Ideomotor theories should be expanded to include action targets as action goals similar to action effects and consequently cover goal-based, rather than effect-based action control.

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The activation of effect codes in response preparation: new evidence from an indirect priming paradigm

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Evidence for the anticipation of environmental effects as an integral part of response planning comes mainly from experiments in which the effects were physically presented. Thus, in these studies it cannot be excluded that effect codes were activated during response preparation only because the effects were displayed as external stimuli before response execution. In order to provide more clear-cut evidence for the anticipation of response effects in action planning, we performed a series of three experiments using a new paradigm, where displaying effect codes before the response was avoided. Participants first learned arbitrary effects of key-pressing responses. In the following test phase they were instructed to execute a response only if a Go stimulus was presented after a variable stimulus onset asynchrony (SOA). The Go stimulus was either compatible or incompatible with the effect, but independent of the response. In Experiment 1 we tested the paradigm with two responses and two effects. We found a significant compatibility effect: If the Go stimulus was compatible with the response effect, responses were initiated faster than in incompatible trials. In Experiment 2 response effects were only present in the acquisition phase, but not in the test phase. The compatibility effect disappeared, indicating that the results of Experiment 1 were indeed related to the anticipation of the forthcoming response effects. In Experiment 3 we extended this paradigm by using a larger number of stimuli and response alternatives. Again we found a robust compatibility effect, which can only be explained if the effect representations are active before response execution. The compatibility effects in Experiments 1 and 3 did not depend on the SOA. The fact that the Go stimulus affected response preparation at any time indicates that the role of effect anticipation is not limited to response selection.

Keywords: action planning, effect anticipation, ideomotor theory, motor control

INTRODUCTION

Human motor behavior, when it is not reflexive, is typically carried out to achieve goals. When we plan a movement we normally have an environmental or sensory effect in our mind that we want this movement to produce. This can either be in form of the physical movement itself, like in floor exercises or in dancing, or a more distal effect like the creation of a new environmental state, or the avoidance of an unpleasant situation. This leads to the question how the environmental effects, or their representations, are involved in the planning and control of movements.

Early theories of movement control indeed considered the anticipation of the sensory effects of a movement as a prerequisite to perform the movement. By randomly executing a movement, the performer learns which sensations are connected with this movement. The re-activation of the sensations will then produce the same movement, or at least a corresponding movement tendency (Herbart, 1824; Lotze, 1852; Harleß, 1861; James, 1890; Münsterberg, 1888). James (1890) called this the ideomotor principle. More modern versions of the ideomotor principle follow this suggestion by assuming that voluntary responses or actions are centrally represented by the sensory feedback that they produce, i.e., by their effects (Greenwald, 1970; Prinz, 1983, 1997; Hommel et al., 2001; Hommel, 2009; see also Stock and Stock, 2004; Pfister and Janczyk, 2012 for an overview).

Also other theories of motor control consider the anticipation of action effects as an important component of the control process, e.g., Schmidt's (1975, 1988) Schema Theory and the more recent concept of forward models in motor control (Davidson and Wolpert, 2005; Wolpert and Flanagan, 2009). These theories basically assume that that the sensory effects of a selected motor response are anticipated in order to allow for the internal testing of the programmed response, the monitoring of execution, and the related error detection and correction.

Thus, theoretically it has been well established that action effects play a crucial role in the selection, preparation, and execution of motor actions. This theoretical emphasis has led to numerous experiments trying to find evidence for the anticipation of effects for the selection, internal test, and monitoring of motor responses (see Nattkemper et al., 2010; Shin et al., 2010 for reviews). However, it should be noted that a considerable part of this evidence comes from experiments in which the effects were presented prior to response execution. Obviously, an experimental setting in which response effects are physically present in the environment does not have high ecological validity. Under normal conditions the appearance of the effect in the environment would indicate the successful completion of the response but not trigger its execution. The fact that advance presentation of an effect facilitates the response could therefore be an artifact of the experimental situation. It could be that effect representations are only activated because of the external stimulation and that participants only use the effect information to select and prepare the required response because it is already available.

In particular, this criticism applies to all paradigms in which the response effects were used as imperative stimuli, or where the response effects were presented together with the imperative stimuli. A first paradigm of this kind is based on the theoretical assumption that the acquisition of goal-directed actions follows a two-stage process (Lotze, 1852; Hommel, 1996; Elsner and Hommel, 2001). In the first stage, randomly executed movements are associated with their effects. The association is assumed to be bi-directional, so that in a second stage the activation of effect representations automatically leads to the activation of the corresponding movement that is needed to produce the effect. In their experiments, Elsner and Hommel (2001) first let their participants execute two key-presses that were followed by a low pitched or a high pitched tone. In the second phase of the experiment the tones were used as imperative stimuli in a forced-choice task. Responses to the tone stimuli were faster if the tone-response assignment corresponded to the response effect relation from the first part of the experiment. Similarly, if the second phase of the experiment was a free-choice task, participants responded to a given tone more often with the response that produced this tone beforehand, rather than with the alternative response (see also, Pfister et al., 2011).

In a second paradigm to which our criticism also applies the effects were presented simultaneously with the imperative stimulus, and participants were instructed to ignore them. For example, in some of our own experiments (Ziessler and Nattkemper, 2002, 2011; Ziessler et al., 2004) we adapted the flanker task (Eriksen and Eriksen, 1974) to investigate the integration of effect anticipation in action planning. In an initial acquisition phase participants performed key-pressing responses to letters that were followed by other letters as effects. In the test phase participants performed the same forced-choice reaction task, but now the effect letters were presented as flanking stimuli on both sides of the imperative stimulus. Responses were facilitated if the flanking letters were the effects of the correct response to the imperative stimulus.

A third paradigm, which is less affected by our criticism, provides evidence that responses are facilitated if there is an overlap between features of the response and features of the effect (Greenwald, 1970; Kunde, 2001, 2003; Kunde et al., 2004). The experiments by Kunde and collaborators show, for example, that spatial compatibility between the location of the response and the location of the effect in a forced-choice reaction task facilitates the response. Also, if the intensity of the response (e.g., soft or forceful key-presses) was compatible with the intensity of the effect (e.g., loudness), or the durations of responses and effects corresponded to each other, responses were facilitated. Kunde and collaborators interpreted their findings of response effect compatibility, in analogy to activation models of stimulus-response compatibility (Kornblum et al., 1990), via the assumption that the feature overlap between responses and their effects leads to a facilitation of the responses. In line with the ideomotor principle, if there is dimensional overlap between the response and the effect, features of the effect can directly activate features of the response. The advantage of this paradigm is that the effects are not physically presented before response execution. However, the evidence is limited to situations in which responses and their effects show a dimensional overlap. There are indeed instances outside of the laboratory setting for such dimensional overlaps, for example, the correspondence between the duration of a key-press and the duration of a tone, or between the force of a response and the force of the effect. But in many other instances our movements do not exhibit any feature overlap with the distal effects that they produce in the environment. For example, the movements that we execute to bring up the letters on the computer screen have actually nothing in common with the letters; the movement to turn a light switch has no features in common with the light that is switched on.

The most convincing evidence for effect anticipation so far comes from experiments that adopted a fourth paradigm. Kunde et al. (2002) instructed their participants to prepare a response, but in a number of critical trials they had then to switch to another response. This switch could be performed faster when both the originally cued response and the switched response produced the same effect. Kunde et al. (2002) argued that their results show that effect codes become endogenously activated in response preparation and that the activated effect representations are capable of also activating other responses that produce these effects. To our knowledge, however, the advantage of switching between responses that produce the same effect, as compared to responses producing different effects, was only shown where tones were used as effects. The production of tones might be a special case, and so it is still unclear if those findings could be generalized to other response effects.

In summary then, in a considerable number of the available experiments on effect anticipation, the response effects themselves were used as imperative or as flanker stimuli, which compromises the interpretation of the results (see above). The two other experimental paradigms used to date are less affected by our circularity argument, but generalization of those findings is limited to situations where responses and effects show dimensional overlap (third paradigm), or, at least to date, to tones as environmental effects (fourth paradigm).

With the present series of experiments we devised a new paradigm, which neither includes the physical presentation of response effects nor relies on a feature overlap between responses and their effects. Basically, we used an indirect priming paradigm in which a stimulus presented during response preparation has the potential to interact with the effect of the response, provided the response effect is indeed activated at this point in time. The experiments started with an acquisition phase, where participants learned that their responses produced particular, arbitrarily assigned effects. The following test phase was designed as a Go/No-Go task. Participants were instructed to prepare a response, but they should only perform the response after a Go stimulus was presented. The identity of the Go stimuli had no relationship to the responses, but they were either compatible or incompatible with the response effect. Because the Go stimuli do not provide any information regarding the required response, they can only affect the response via their relationship to the response effects. Therefore, if the effect compatibility of the Go stimuli would affect the reaction times (RTs), this would convincingly demonstrate that effect codes have functional relevance for the preparation of the responses.

The aim of the first experiment was to test this paradigm using just two responses, two effects, and two Go stimuli. We predicted that effect-compatible Go stimuli would support the activation of the response-related effect code and that this would, in turn, facilitate the response, in comparison to effect-incompatible Go stimuli. There are at least two different hypotheses how a facilitation of the effect anticipation could result in faster responses. If effect codes were used for response selection (Elsner and Hommel, 2001; Hommel et al., 2001; Kunde et al., 2002) an early activation of the effect representation should lead to a faster response. If the activation of effect representations is part of the preparation of an already selected response (Schmidt, 1975, 1988; Ziessler and Nattkemper, 2011), then the additional activation of the effect by the compatible Go stimulus would shorten the process of response preparation, and consequently reduce the RTs. In an attempt to distinguish between these two hypotheses, we varied the stimulus onset asynchrony (SOA) between the imperative stimulus and the Go stimulus in the test phase. The first hypothesis would predict stronger compatibility effects for shorter SOAs, whereas the second hypothesis would either predict the opposite effect, indicating an increasing role of effect anticipation in later stages of response preparation, or no interaction between compatibility and SOA.

With the second experiment we wanted to assess whether the presence of the response effects in the test phase was crucial for the observed differences between effect-compatible and effect-incompatible trials, or whether the association between responses and effects, which was established during the practice phase, would be sufficient for compatibility effects to emerge in the test phase, even if the effects were no longer present. Disappearance of the compatibility effect would indicate that, in our paradigm, effects are only anticipated if they actually continue to appear after the response. Such a result would further validate Experiment 1 by showing that the observed effect was indeed due to the processing of the effect information during response preparation, and not due to the imperative stimuli or Go stimuli and their assignment to the responses.

In Experiment 3 we explored a wider range of SOAs than in Experiments 1 and 2. In addition, we used a larger number of response alternatives in order to extend the duration of response preparation, and introduced a second type of effect-incompatible Go stimuli, which were not related to any of the response effects. The latter variation was intended to provide more information about facilitating and inhibiting mechanisms contributing to the effects of compatible and incompatible Go stimuli on response preparation.

EXPERIMENT 1

The acquisition phase consisted in a free-choice task. According to previous experiments, the free-choice task provides optimal conditions for response effect learning (e.g., Elsner and Hommel, 2001; Herwig et al., 2007; Pfister et al., 2010). Participants learned that the execution of each of two key-presses was followed by a particular effect; one of the responses produced a picture of a car steering wheel on the computer screen, the other response a picture of a beach ball. In the test phase, an imperative stimulus defined which of the responses participants should execute. Response execution was only to be carried out after presentation of one of the two Go stimuli. The Go stimuli consisted either of a picture of hands in the posture of catching a beach ball or hands in the posture of holding a steering wheel (**Figure 1**). Obviously, the hands in the posture of holding a steering wheel were compatible with the steering wheel and incompatible with the beach ball and vice versa. The question was: would the effect compatibility of the Go stimuli affect the RTs? Effect-compatible Go stimuli should support the activation of the effect codes, effect-incompatible Go stimuli should interfere with the activation of the effect codes.

METHOD

Participants

Forty-two undergraduate students from the Department of Psychology at the University of Sunderland served as participants. All students were first-year students and received course credit for their participation. Their mean age was 21.3 years (SD = 4.9). Thirty-five of the participants were female, nine male. Participants had either normal or corrected to normal vision. Ethical approval was obtained from the departmental ethics committee.

Material and apparatus

Stimulus presentation and response recording were controlled by a standard PC. The computer was situated in a sound-proof booth. In the acquisition phase of the experiment the letter O was presented in the middle of the computer screen. Responses consisted of a key-press with either the left or right index finger. Participants were instructed to use the X-key of a standard QWERTY keyboard with their left index finger and the M-key with their right index finger. Both fingers were located on the corresponding keys throughout the experiment. After a left key-press a picture of a steering wheel appeared in the middle of the computer screen. After a right key-press a picture of a beach ball was presented as response effect (see **Figure 1**).

In the test phase, a yellow square required a response with the left index finger and a yellow triangle a response with the right index finger. Two hands in the posture of holding a steering wheel and two hands in the posture of catching a beach ball were used as Go stimuli. A No-Go trial was indicated by two hands with the palms turned outwards (see **Figure 1** for illustration of the Go and No-Go stimuli). The high similarity between Go and No-Go stimuli forced the participants to identify these stimuli if they did not want to produce a high number of false alarms in the No-Go trials.

Design and procedure

The experiment was divided into two phases: an acquisition phase and a test phase. In the acquisition phase the participants performed a free-choice reaction task. Upon presentation of an "O" in the middle of the screen participants were asked to perform either

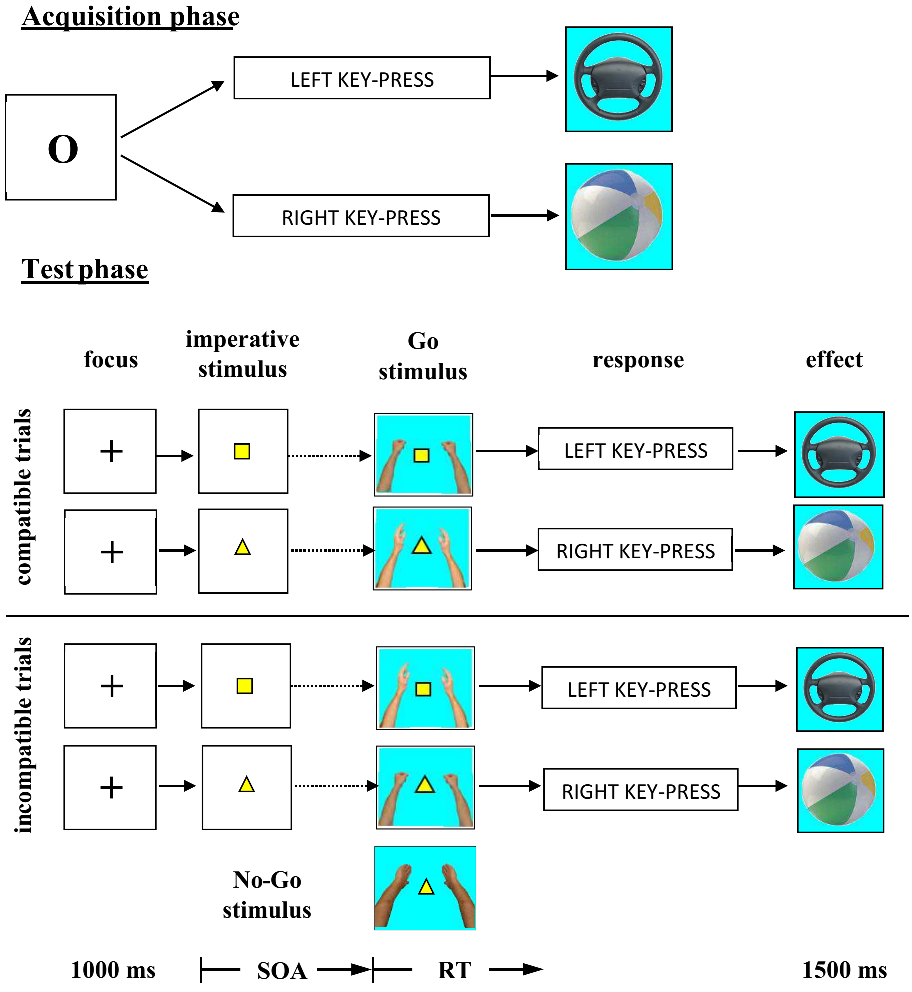

FIGURE 1 | Material and design used in Experiment 1. The SOA was 0, 100, or 200 ms.

a left or a right key-press depending on their choice. They were instructed to use both key-presses with about the same frequency. The key-press deleted the "O" from the screen and triggered the presentation of the response effect. After a left key-press the steering wheel appeared on the screen whereas the right key-press was always followed by the beach ball. The effect stimuli remained on the screen for 1500 ms. After an inter trial interval (ITI) of 500 ms the next trial began with the presentation of the "O." The acquisition phase consisted of 100 trials. After 50 trials and at the end of the acquisition phase participants were informed about the frequency of their use of each key.

The subsequent test phase was designed as a forced-choice reaction task. Each trial started with the presentation of a "+" sign for 1000 ms followed by the imperative stimulus, either a square or a triangle, which determined the response. Participants were instructed to withhold the response until the presentation of a Go stimulus, after which they should execute the correct response as quickly as possible. In case of the No-Go stimulus participants should not respond. The pictures of the two hands constituting the Go or No-Go stimulus appeared on the left and right sight of the imperative stimulus. The SOA between the onset of the imperative stimulus and the onset of the Go stimulus was either 0, 100, or 200 ms. Immediately after a correct response, the effect stimulus assigned to that response replaced the imperative stimulus and the Go signal. As in the acquisition phase, the effect stimulus remained on the screen for 1500 ms and, after an ITI of 500 ms, the next trial started with the "+" sign. In the case of an incorrect response, instead of the effect stimulus appearing, the word "incorrect" was presented for 1500 ms. No-Go trails were terminated after 2000 ms by the presentation of a blank screen for 500 ms. If participants performed a response in No-Go trials the response was followed by a written reminder that they should not respond in such trials.

The most important independent variable of the experiment consisted of the construction of the Go trials. Both imperative stimuli could be followed by both Go stimuli. This meant there was no fixed relationship between the two Go stimuli and the two responses. However, because there was a fixed relationship between responses and effect stimuli, in half of the Go trials the Go stimuli were compatible with the response effect and in the other half incompatible (cf. **Figure 1**).

Altogether the test phase consisted of 360 trials, divided into six blocks of 60 trials with short rest periods between blocks. Of all trials, 80% were Go trials and 20%were No-Go trials. The three SOAs were applied with equal frequencies to Go and No-Go trials. All experimental conditions were randomly mixed across the test phase. Responses and RTs were measured from the onset of the Go stimulus. **Figure 1** summarises the experimental procedure. Including the acquisition phase the complete experiment lasted about 50 min.

RESULTS

The aim of the acquisition phase was plainly to familiarize the participants with the response effects. There was no further data analysis. The data of interest regarding our research question were collected in the Go trials of the test phase. Only RTs from correct responses were taken into account. RTs longer than 2000 ms were considered as outliers. Overall, only 3.6% of all responses in Go trials were erroneous responses or outliers.

Individual mean RTs for each participant were calculated depending on the SOAs and the effect compatibility of the Go stimuli. The individual means were subjected to a 3 (SOA) × 2 (compatibility) repeated-measures Analysis of Variance (ANOVA). In this and all following analyses, sphericity was tested for all repeated-measures factors with more than two levels. If sphericity could not be assumed, the degrees of freedom and subsequently the *p*-values were corrected using the Greenhouse–Geisser correction.

Figure 2 presents the mean RTs for all experimental conditions. The ANOVA yielded a significant main effect of the SOA, $F(2, 82) = 308.71$, $p < 0.001$, $\eta_p^2 = 0.88$. Whereas at the 0 ms SOA the mean RT was 612 ms, at the longest SOA of 200 ms the RTs were about 135 ms shorter. This indicates that participants used the SOA interval to prepare the response. More importantly, there was also a significant compatibility effect, $F(1, 41) = 5.14$, $p = 0.029$, $\eta_p^2 = 0.11$. If the Go stimulus was compatible with the effect of the to-be-prepared response, RTs were on average 9 ms faster than in the incompatible condition. According to Cohen (1988), $\eta_p^2 = 0.11$ indicates a medium effect size. This is first evidence for an activation of effect codes during preparation of the motor response. The interaction between both variables was not significant, $F(2, 82) = 0.06$, $p = 0.946$, $\eta_p^2 = 0.001$.

Given numerically small compatibility effect we checked carefully if this difference could be explained by a speed-accuracy trade-off. However, this was not the case. An ANOVA conducted on the number of correct responses only found a significant effect of SOA, $F(2, 82) = 11.77$, $p < 0.001$, $\eta_p^2 = 0.22$. The number of correct responses increased with the prolonged SOA from 94.5 to 97.2%. However, there was no effect of compatibility, $F(1, 41) = 0.56$, $p = 0.460$, $\eta_p^2 = 0.01$. On average, 95.9% of the responses in compatible trials were correct. For incompatible trials the figure increased to 96.3%. There was also no interaction between SOA and compatibility, $F(2, 82) = 1.34$, $p = 0.268$, $\eta_p^2 = 0.03$. Therefore, we can exclude a speed-accuracy trade-off as a cause for the compatibility effect observed in the RT data.

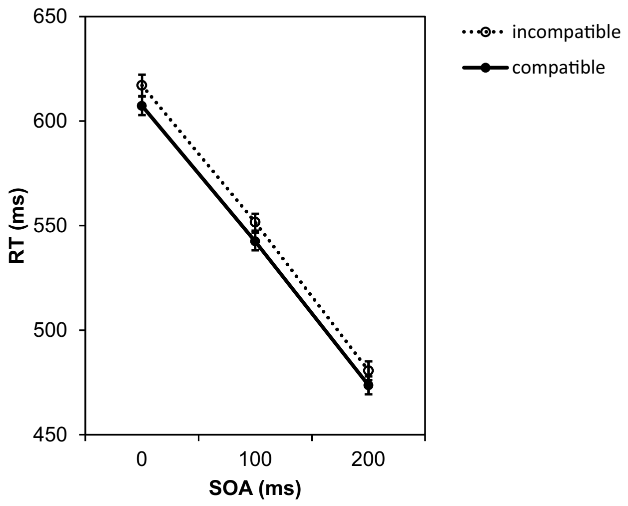

FIGURE 2 | Mean reaction times depending on SOA and compatibility conditions in Experiment 1. Error bars in this and all following graphs represent the standard error of the means calculated for the within-participant design following the procedure suggested by Cousineau (2005).

In a further step of the analysis we checked the participants' false alarm rates (i.e., responses in No-Go trials). A high number of false alarms indicates that the participant did not follow the instructions and did not distinguish the Go stimuli from the No-Go stimulus. In that case the compatibility effect should disappear. On average the false alarm rate amounted to 17.46%, i.e., on average in 12.6 out of the 72 No-Go trials a response was wrongly executed. The inter-individual variance was large. Whereas 29 participants showed false alarms in less than 20% of the No-Go trials, some of the remaining 13 participants had false alarm rates of 40% and above, and one participant responded in all No-Go trials. Therefore, we recalculated the compatibility effects separately for participants with less than 20% false alarms (low false alarm rate) and participant with more than 20% false alarms (high false alarm rate; **Figure 3**).

A 3 × 2 × 2 ANOVA with SOA (0, 100, 200 ms) and compatibility (compatible/incompatible) as within-participant factors and the false alarm rate (low/high) as between-participants factor revealed that the effect of false alarm rates was significant, $F(1, 40) = 18.44$, $p < 0.001$, $\eta_p^2 = 0.32$. Participants with a high false alarm rate responded considerably faster (mean RT = 458 ms) than participants with a low false alarm rate (mean RT = 584 ms). This underlines our assumption that participants with a high false alarm rate did not wait until they had fully identified the Go signal before they performed their response. As expected, the compatibility effect depended on the false alarm rate, $F(1, 40) = 4.34$, $p = 0.044$, $\eta_p^2 = 0.10$. Participants with a low false alarm rate showed a compatibility effect of 14 ms, and those with a high false alarm rate did not show any compatibility effect (their responses to compatible trials were on average 3 ms slower than their responses to incompatible trials). Apart from the main effect of false alarm rates and the interaction of false alarm rates with compatibility, only the main effect of SOA was significant, $F(2, 80) = 261.79$, $p < 0.001$, $\eta_p^2 = 0.87$. The main effect of compatibility did not reach significance, $F(1, 40) = 1.92$, $p = 0.174$, $\eta_p^2 = 0.05$. All other interactions were not significant (F-values < 1).

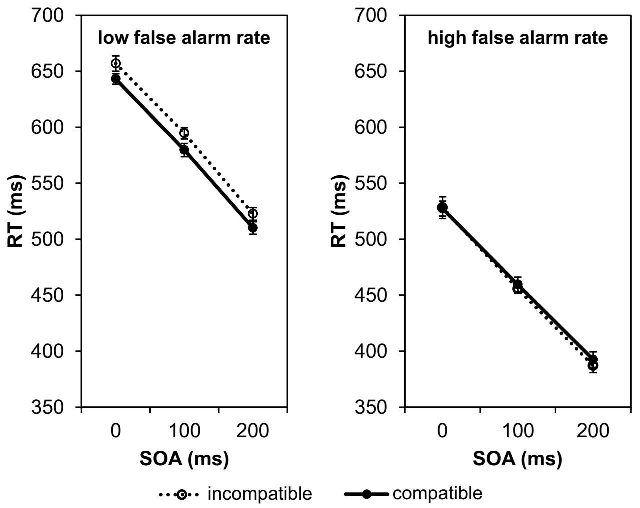

FIGURE 3 | Mean reaction times depending on SOA and compatibility conditions in Experiment 1. The left graph shows the result for participants with a low rate of false alarms (less than 20% responses in No-Go trials), the right graph for participants with a high rate of false alarms (more than 20% responses in No-Go trials).

In a further analysis, we tested if the compatibility effect developed with practice. To this end, we split the test phase in two halves. Only the data of the participants with a low false alarm rate were taken into account and were entered into a 2 (halves) × 3 (SOAs) × 2 (compatibility) ANOVA. We found a main effect of practice, $F(1, 32) = 54.51$, $p < 0.001$, $\eta_p^2 = 0.63$. Responses in the second half were 79 ms faster than responses in the first half. Also the main effect of SOA, $F(2, 64) = 238.01$, $p < 0.001$, $\eta_p^2 = 0.88$, and the main effect of compatibility could be confirmed, $F(1, 32) = 8.40$, $p = 0.007$, $\eta_p^2 = 0.21$ (large effect size according to Cohen, 1988). However, none of the interactions was significant. Most importantly, the compatibility effect was not affected by practice, $F(1, 32) = 2.27$, $p = 0.14$, $\eta_p^2 = 0.07$. There was no evidence for an increase of the compatibility effect from the first to the second half. On the contrary, numerically the compatibility effect was larger in the first half of the test phase (18 ms) as compared to the second half (7 ms). For all other interactions the F-values were <1.

DISCUSSION

Experiment 1 clearly shows that the Go stimuli affected the preparation of the responses depending on their compatibility with the response effects. As long as participants had sufficiently processed the Go stimuli before executing the response, their RTs in effect-compatible trials were significantly shorter than in effect-incompatible trials. The compatibility effect disappeared for participants who ignored the Go stimuli. It is important to remember that the Go stimuli itself did not have any relationship to the two responses. Both stimuli appeared with equal frequency for each of the two responses. What made the Go stimuli compatible or incompatible was only their relationship to stimuli appearing after the execution of the responses. Therefore, the compatibility effect can only be explained by assuming that participants anticipated the effects of their responses during response preparation. Only if effect codes were active before response execution the compatibility between the Go stimuli and the future effects could affect the RTs. Consequently, dropping the effects in an additional experiment should abolish the compatibility effect. This was tested in Experiment 2.

In the experiment we could only find a main effect of SOA, confirming that participants indeed prepared the response during the SOA. The imperative stimuli informed the participants about the required response, and they used the time until presentation of the Go stimulus for response selection and programming. However, contrary to our original expectations, the SOA did not affect the compatibility effect. The compatibility effect did neither decrease nor increase with increasing SOA. Whereas a decrease of the compatibility effect would have indicated the involvement of effect anticipation in response selection, an increase of the compatibility effect would have supported the assumption that effects were anticipated for an already selected response. Both expectations were not confirmed by the data. A preliminary explanation might be that the present, rather simple task was not sensitive enough to provoke any interaction between SOA and compatibility. We therefore tested this further in Experiment 3.

A further interesting result was that practice during the test phase did not increase the compatibility effect. Apparently, then, the compatibility effect was fully developed from the beginning of the test phase. This supports the idea that the effect is based on the response effect relations as learned in the acquisition phase and not on relations that are only present in the test phase, such as the relationship between the imperative stimulus and the Go stimulus or between the Go stimulus and the response.

EXPERIMENT 2

With the second experiment we wanted to assess whether the presence of the response effects in the test phase was crucial for the observed differences between effect-compatible and effect-incompatible trials. Alternatively, the association between responses and effects that was established during the practice phase might be sufficient for compatibility effects to emerge in the test phase, even if the effects are no longer present. We predicted that the compatibility effect would disappear when effects are no longer presented in the test phase. This result would further validate Experiment 1 by showing that the results were indeed due to the processing of the effect information during response preparation and not, e.g., to the imperative stimuli or Go stimuli and their assignment to the responses.

METHOD

Participants

Thirty-six undergraduate students (29 female, 7 male) from the Department of Psychology at the University of Sunderland took part in the experiment. Participants had a mean age of 20.9 years (SD = 3.4). All participants received course credit for their participation. They had either normal or corrected to normal vision. Ethical approval was obtained from the departmental ethics committee.

Material and apparatus

These were identical to Experiment 1.

Design and procedure

The acquisition phase was the same as in Experiment 1. In the test phase, the only change consisted in the replacement of the effect stimuli by the word "correct." Incorrect responses were followed by the word "incorrect."

RESULTS

Data were analyzed in the same way as for Experiment 1. Two of the participants exhibited exceptionally high error rates (about 50%, indicating random responses), and their data were thus discarded from the analysis. The remaining 34 participants exhibited an error rate of 3.7% which corresponds to the error rate in Experiment 1. The mean false alarm rate for the 34 participants was 10.17%. Only one participant had a false alarm rate above 20%, and we decided to also discard this participant's data so that only those participants were included for which a compatibility effect would be most likely to occur. **Figure 4** shows the mean RTs for effect-compatible and incompatible trials for each SOA.

Data were subjected to a 3 (SOA) × 2 (compatibility) ANOVA. The analysis only confirmed a significant effect of the SOA, $F(2, 64) = 140.98$, $p < 0.001$, $\eta_p^2 = 0.82$. As in the first experiment, RTs decreased with the prolonged SOA. The RT difference between the 0 and the 200 ms SOA amounted to 124 ms. As expected, no significant effect of compatibility was found, $F(1, 32) = 0.22$, $p = 0.643$, $\eta_p^2 = 0.01$. Also the interaction between both variables was not significant, $F(2, 64) = 1.94$, $p = 0.152$, $\eta_p^2 = 0.06$. On average, for compatible trials responses were just 2 ms faster than for incompatible trials. Separate paired-samples t-tests for the three SOAs did not find any significant differences, 0 ms: $t(32) = 0.28$, $p = 0.779$, 100 ms: $t(32) = 1.25$, $p = 0.220$, 200 ms: $t(32) = 1.72$, $p = 0.095$.

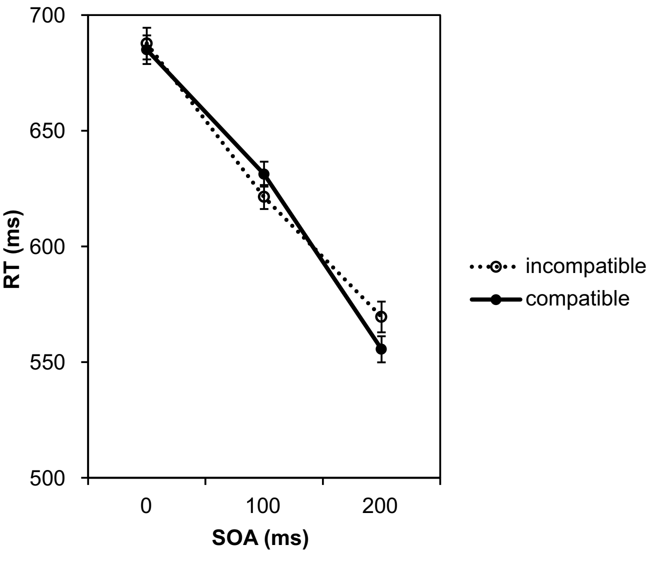

FIGURE 4 | Mean reaction times depending on SOA and compatibility conditions in Experiment 2. Compatibility is defined in relation to the response effects presented in the acquisition phase for each of the responses.

In an additional analysis we split the test phase into two halves in order to identify a possible residual compatibility effect at the beginning of the test phase. The three-way ANOVA (test half, compatibility, SOA) only confirmed the results regarding compatibility and SOA reported above. Most importantly, there was no interaction between half and compatibility, $F(1, 32) = 0.004$, $p = 0.950$, $\eta_p^2 = 0.88 < 0.001$. In fact, the difference between compatible and incompatible trials was only about 2 ms in both halves of the experiment.

DISCUSSION

Experiment 2 clearly confirms that the compatibility effect observed in Experiment 1 depended on the response effects. It only occurred in Experiment 1 where the response effects were present, but not in Experiment 2 where the differentiating effect stimuli were replaced by a single effect, the word "correct." Up to response execution, everything was exactly the same in both experiments. Therefore, we can be very certain that the compatibility effect in Experiment 1 was not caused by any unknown relationship between the imperative stimuli, Go stimuli, and responses. This underlines that the compatibility effect observed in Experiment 1 is reliable evidence for the activation of effect codes during the preparation of a motor response.

We did not find any evidence that the compatibility effect would fade out in course of the test phase. The compatibility of the Go stimuli with the originally learned response effects did not affect the RTs from the beginning of the test phase. There was not even a numerical difference of the compatibility effects in the first and the second half of the test phase. Even when we only analyzed the first 30 responses of the test phase, no hint of a compatibility effect was found. Unfortunately, however, it is impossible to get reliable data for such small parts of the test phase because the number of repetitions per condition becomes too low. Within the first 30 trials there were only four responses per condition for each participant, and some of these data were missing due to errors and delayed responses. These factors might have obscured a possible residual compatibility effect at the beginning of the test phase. Nevertheless, it appears that participants realized very quickly that their responses only produced an unspecific effect in the test phase, and consequently they stopped anticipating the specific effects learned in the test phase.

The missing compatibility effect in Experiment 2 questions the interpretation of experiments in which the response effects did not appear after the responses in the test phase. For example, in Elsner and Hommel's (2001) experiments as described above, the response effects were used as imperative stimuli in the test phase, but after the response no effect was presented. If participants stop anticipating effects if these effects are no longer appear after the response (as indicated by the present experiment), than the observed advantage of effect-compatible stimulus-response mappings might require a different interpretation. In another experiment, Cardoso-Leite et al. (2011) presented in half of the trials in the test phase no effects, and in the other half the effect stimuli appeared randomly after the responses. Their test phase was designed as detection task, and participants had to report if an effect stimulus was presented after the response. The authors found reduced sensitivity for response effects learned in the acquisition phase and explained this by effect anticipation; sensitivity for the expected stimulus is reduced, whereas the unexpected stimulus requires further processing. However, an alternative interpretation might be that participants stopped anticipating the previously learned effects and tried to learn and anticipate the new response effects, which could not be successful because of the random response effect assignment. The attempt to learn new effects might go along with the suppression of the old effects resulting in the reduced sensitivity.

EXPERIMENT 3

In Experiment 1 we did not find any interaction between the SOA and the compatibility effect. As already mentioned, this might have been due to the simplicity of the task with just two responses. Under such conditions selection of responses and activation of effect codes might go too fast to find a compatibility × SOA interaction with our paradigm. In Experiment 3 we therefore used a more complex task with four responses and four effects. The larger number of response alternatives should slow down response selection and prolong the preparation period. In addition we extended the duration of the SOA up to 450 ms.

The second aim of Experiment 3 was to investigate the mechanisms that induce the compatibility effect in greater detail. The difference between compatible and incompatible trials could be caused by a facilitation of the response in compatible trials, an inhibition of the response in incompatible trials, or both. To differentiate between these alternatives, we introduced a new variation of the effect-incompatible Go stimuli: There were incompatible Go stimuli related to one of the response effects (incompatible/related) as in Experiment 1, as well as incompatible Go stimuli that had no relationship to any of the response effects (incompatible/unrelated). Assuming that the Go stimulus activates its corresponding representation in memory, in compatible trials the Go stimulus would activate a representation that is compatible with the anticipated response effect. In incompatible trials the Go stimulus would either activate a stimulus representation that is compatible with the effects of an alternative response (incompatible/related) or a stimulus representation which does not have any relationship to one of the response effects (incompatible/unrelated).

According to the ideomotor principle activation of effect codes leads to an automatic activation of the responses to produce these effects. Therefore, for compatible trials the compatible Go stimulus should eventually facilitate the response activation via the effect, while incompatible/related Go stimuli should result in the activation of competing responses and inhibit the required response. Compared to that, incompatible/unrelated Go stimuli should not facilitate or inhibit a competing response via the response effects. Hence, RTs should be shortest in compatible trials and longest in incompatible trials with incompatible/related Go stimuli, whereas incompatible/unrelated Go stimuli should result in intermediate RTs, due to reduced or absent response competition.

No difference between related and unrelated incompatible Go stimuli would be expected if there was only facilitation in compatible trials but no inhibition in incompatible trials. In both types of incompatible trials the Go stimulus would activate its representation in memory. However, that would not further activate codes of a related response effect in the case of incompatible/related Go stimuli and therefore would not lead to response competition.

In addition, we used Experiment 3 to test our paradigm further. Whereas in Experiment 1 objects were used as effects and hand postures as Go stimuli, in Experiment 3 hand postures were the effects, while objects were used as Go stimuli. If we found compatibility effects equivalent to Experiment 1, then we can conclude that these effects do not depend on the particular sequence of the Go and effect stimuli. Note also that the particular setting might increase the size of compatibility effect since, in line with the affordance concept (Gibson, 1979; Tucker and Ellis, 1998), the presentation of the objects as Go stimuli would activate the actions depicted in the effect stimuli.

METHOD

Participants

Thirty undergraduate students of psychology at Liverpool Hope University took part in the experiment. Their mean age was 22.1 years (SD = 4.1). Fifteen of the participants were male and 15 female. Participants received course credit for their participation. Participants had either normal or corrected to normal vision. The experiment was approved by the departmental ethics committee.

Material and apparatus

Participants had to choose between four responses. These were key-presses with the left and right middle and index fingers. The fingers were placed on the keys "Z," "X," "N," and "M" on a standard QWERTY keyboard. Response effects were pictures of a right hand in the position of holding a coffee mug, a computer mouse, a pen, and a spoon assigned to the left middle finger, left index finger, right index finger, and right middle finger, respectively. In all cases, the hand was only shown without the corresponding objects. In the test phase the corresponding pictures of a coffee mug, a computer mouse, a pencil, and a spoon served as Go stimuli. Whereas these four Go stimuli had a relationship to one of the aforementioned hand postures, a fifth Go stimulus was a picture of a hand brush, which did not fit with any of the effect stimuli (see **Figure 5**). The No-Go stimulus was a picture of a hammer. As imperative stimuli in the test phase little squares in the colors orange, green, red, and blue were assigned in order to the left middle finger, left index finger, right index finger, and right middle finger.

Design and procedure

Basically, design and procedure of Experiment 3 were identical to that for Experiment 1. The acquisition and test phases followed exactly the same pattern as described above. There were again only 100 acquisition trials. That is, participants experienced on average only 25 instances of each response effect pairing. In the test phase, the colored squares served as imperative stimuli and the SOA between imperative stimulus and Go stimulus was 0, 150, 300, or 450 ms. The imperative stimuli (colored squares) were not occluded by the Go stimulus but stayed in the foreground. No-Go trials were indicated by the picture of a hammer. The No-Go stimulus always appeared after 600 ms in order to reduce the number of trials. All other details were identical to Experiment 1. **Figure 5** illustrates the procedure.

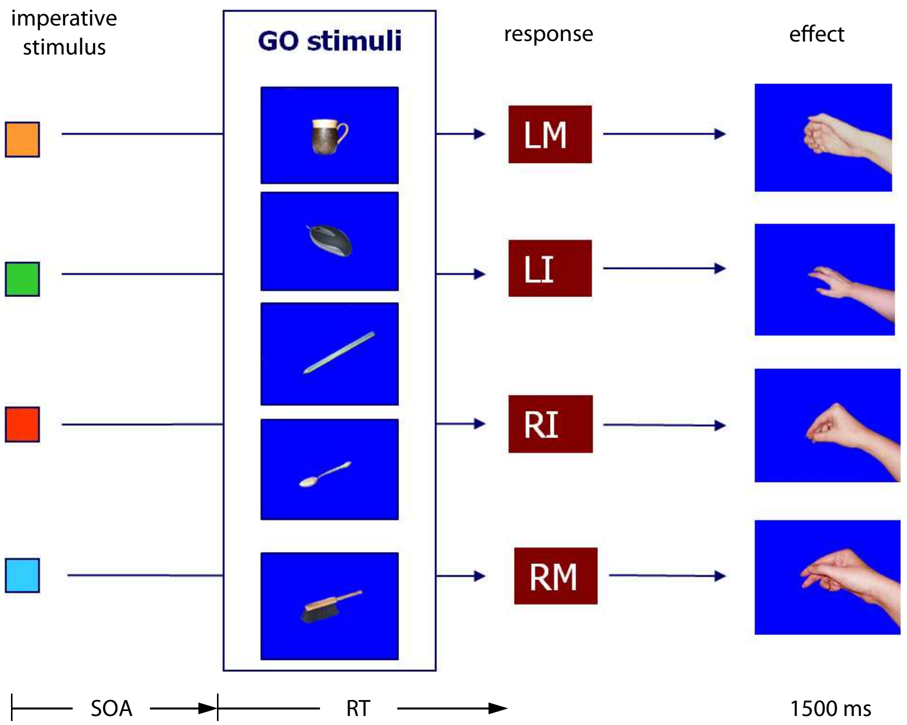

FIGURE 5 | Material and design of Experiment 3. Cup, mouse, pen, and spoon are related to one of the hand postures used as effect stimuli. The pictures were used as compatible and incompatible Go stimuli. The hand brush is unrelated to all effect stimuli and was therefore incompatible to all of them. LM, left middle finger; LI, left index finger; RI, right index finger; RM, right middle finger. The SOA was 0, 150, 300, or 450 ms.

In the Go trials, three types of Go stimuli were used. Go stimuli were either compatible with the effect (e.g., response with the left finger to produce a hand holding a coffee mug – picture of a coffee mug is a compatible Go stimulus), incompatible/related (e.g., response with the left finger to produce a hand holding a coffee mug – picture of a pencil is an incompatible Go stimulus, but related to the response with the right index finger producing a hand holding a pencil), or incompatible/unrelated (e.g., response with the left finger to produce a hand holding a coffee mug – picture of a hand brush is an incompatible Go stimulus that is unrelated to all effect pictures used in the experiment). In principle, four incompatible Go trials could have been generated, one with the hand brush and three with objects that corresponded to the effects of the other three responses. However, we decided not to use all possible combinations to avoid a bias in the experiment. If, on the one hand, all responses were combined with all Go stimuli with the same frequency, this had left us with 20% compatible trials and 80% incompatible trials, and the low frequency of compatible trials had likely worked against the compatibility effect. If, on the other hand, we had designed 50% of the trials as compatible and 50% as incompatible, each object picture had been combined more often with the response followed by the compatible effect than with all other responses. For compatible trials the Go stimulus had then biased the selection of the correct response, which had made it impossible to explain RT differences in terms of compatibility effects. Therefore, we limited the possible combinations of Go stimuli with the responses so that for each response there was exactly one compatible Go stimulus, one incompatible/related Go stimulus, and one incompatible/unrelated Go stimulus. For example, for a response with the left index finger only the coffee mug (effect-compatible), the pencil (effect-incompatible, related to the effect of the response with the right middle finger) and the brush (incompatible, unrelated to any of the other response effects) served as Go stimuli with equal frequency. In turn, for responses with the right index finger the coffee mug was the incompatible/related Go stimulus and the pencil the compatible stimulus. Following this design each of the four effect-related Go stimuli appeared with equal frequency for one response as effect-compatible Go stimulus and for another response as incompatible stimulus. The hand brush, as unrelated effect-incompatible Go stimulus, was used with the same frequency for all four responses.

The test phase consisted of 600 trials, with 480 Go trials and 120 No-Go trials. Among the Go trials, 160 trials were effect-compatible and 320 effect-incompatible (160 for each type of incompatible Go stimuli). All types of Go trials were presented at each SOA (0, 150, 300, 450 ms). Go trials and No-Go trials were randomly mixed. The full experiment lasted about 75 min.

RESULTS

Data from the acquisition phase were analyzed in order to check if participants had about the same learning experience with all responses and their effects. Given the total number of 100 trials, each response should have been executed 25 times. For each participant we calculated a response-use index, which was defined as the sum of the squared differences between 25 and the actual use for each of the four responses. An index of 0 would indicate that all responses had been used exactly 25 times. Indices varied between 20 and 1208. Whereas the small index of 20 showed that this participant deviated only by two to three responses from the ideal of 25 for each response, the highest index was due to using one of the responses only two times and another one 48 times. The mean response-use index was 182.1 (SD = 217.4). We decided to discard the two participants with the highest indices (1208 and 454) from the analysis since both had only rarely used at least one of the responses. In the test phase, the numbers of false alarms and erroneous responses were relatively low so that no other participants had to be excluded from further analysis: on average participants responded in 14.2% of the No-Go trials. Including outliers (RTs above 2000 ms) only 7.3% of the responses were erroneous responses.

In analyzing the data of the test phase, first the number of correct responses per participant and condition was subjected to a 4 (SOAs) × 3 (compatibility conditions) repeated-measures ANOVA. There was only a significant main effect of SOA, $F(3, 81) = 5.96$, $p = 0.001$, $\eta_p^2 = 0.18$, indicating that the number of correct responses increased with longer SOAs (**Figure 6**). For effect-compatible trials, the number of correct responses was slightly higher than for incompatible trials. The lowest number of correct responses was observed for incompatible/unrelated trials (hand brush as Go stimulus). However, the compatibility effect was not significant, $F(2, 54) = 1.30$, $p = 0.280$, $\eta_p^2 = 0.05$, nor was the interaction between SOA and compatibility, $F(6, 162) = 0.12$, $p = 0.994$, $\eta_p^2 = 0.004$.

In a second step the RTs were analyzed. As in the previous analyses, we split the experiment into two halves to check for possible practice effects on the RTs (see **Figure 7**).

The individual means per experimental condition were entered into a 2 (halves) × 4 (SOAs) × 3 (compatibility conditions) repeated-measures ANOVA. Practice from first to second half led to a small, significant facilitation of responses by 31 ms, $F(1, 27) = 4.50$, $p = 0.043$, $\eta_p^2 = 0.14$. There was also a significant effect of SOA, $F(3, 81) = 391.22$, $p < 0.001$, $\eta_p^2 = 0.94$. As in the first two experiments, and in line with the error analysis above, the responses became not only more accurate but also faster with increasing SOA. At the 450 ms SOA responses were 289 ms faster as compared to the 0 ms SOA. Again, this reflects that participants used the SOA to prepare the response. The SOA effect was 35 ms smaller in the second half as confirmed by the significant interaction between SOA and the halves of the experiment, $F(3, 81) = 2.94$, $p = 0.038$, $\eta_p^2 = 0.1$. Most importantly, we found a significant effect of compatibility, $F(2, 54) = 8.45$, $p = 0.001$, $\eta_p^2 = 0.24$ (large effect size according to Cohen, 1988). Pairwise comparisons using the Bonferroni correction revealed that RTs under the effect-compatible condition were significantly shorter than under both incompatible conditions (incompatible/related: difference = 19 ms, $p = 0.026$; incompatible/unrelated: difference = 25 ms, $p < 0.001$). The difference between the two incompatible conditions was not significant, $p > 0.999$. Thus, as in Experiment 1, the relationship between Go signal and response effect clearly modulated the RTs. Taking the analysis of the number of correct responses into account we can exclude a speed-accuracy trade-off in explaining the observed difference. However, similar to Experiment 1, the compatibility effect did not depend on the SOA, $F(6, 162) = 0.09$, $p = 0.997$, $\eta_p^2 = 0.003$. Furthermore, the compatibility effect did not change with practice, $F(2, 54) = 0.41$, $p = 0.665$, $\eta_p^2 = 0.02$. The numerical data in **Figure 7** suggest that in the second half of the experiment the compatibility effect increased with increasing SOA. However, this was not confirmed by the statistical analysis as

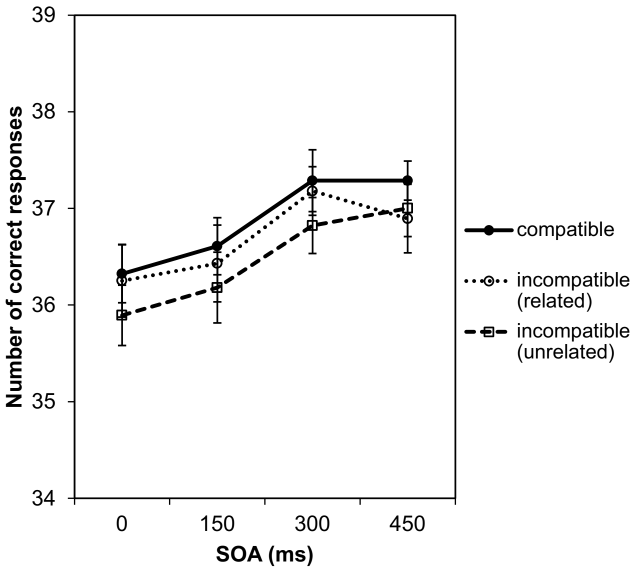

FIGURE 6 | Number of correct responses depending on SOA and compatibility. The maximum number of correct responses for each experimental condition was 40. In counting correct responses, erroneous responses and slow responses with RTs over 2000 ms were excluded.

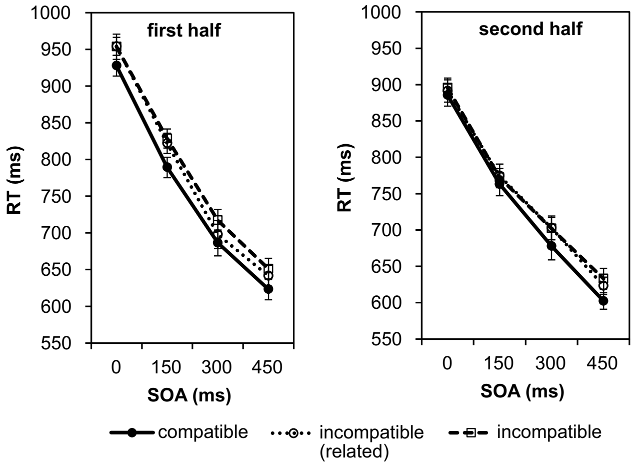

FIGURE 7 | Mean reaction times depending on SOA and compatibility in Experiment 3. The data are shown separately for the first half of the test phase (left graph) and the second half of the test phase (right graph).

the three-way interaction was not significant, $F(6, 162) = 0.36$, $p = 0.905$, $\eta_p^2 = 0.01$. Also a separate ANOVA for the second half of the test phase did not reveal an interaction between compatibility and SOA, $F(6, 162) = 0.21$, $p = 0.972$, $\eta_p^2 = 0.01$. Only the main effects of compatibility, $F(2, 54) = 3.24$, $p = 0.047$, $\eta_p^2 = 0.11$, and of SOA, $F(3, 81) = 156.67$, $p < 0.001$, $\eta_p^2 = 0.85$, were significant.

DISCUSSION

The third experiment demonstrates three important points: First, the compatibility effect between the Go stimulus and the response effect, as observed in Experiment 1, did not depend on the specific stimulus material. Under more complex conditions, and using different stimuli, we have observed the same effect again. Whereas we had originally expected a larger compatibility effect than in Experiment 1, this was not the case. This might be due to the fact that with the chosen design we had partially worked against an increase of the compatibility effect. Compatible trials had a lower frequency than incompatible trials (one-third against two-thirds of trials) whereas in Experiment 1 compatible and incompatible trials were equally frequent.

Second, also under the more complex conditions and extended SOAs, the compatibility effect was not affected by the SOA. This does not correspond to our original predictions. However, the present results make it unlikely that effect anticipation is the prerequisite for response selection, at least under the conditions of our experiment. Had this been the case, then in particular early compatible Go stimuli should have facilitated the responses, and the compatibility effect should have decreased with increasing SOA. The data do not show any tendency for such a pattern.

Third, it is important to note that there was no difference between the two types of incompatible trials. This indicates that only compatible Go stimuli facilitated the anticipation of the response effect, but incompatible Go stimuli did not seem to inhibit the anticipation of the effect or to activate competing responses. We assume that the representations activated by the Go stimuli do not directly activate representations of corresponding response effects. However, if representations of the response effects are anticipated depending on a selected response, compatible Go stimuli might contribute to further activation of these representations whereas incompatible Go stimuli remain without effect. This resembles very much our findings with the flanker task (Ziessler and Nattkemper, 2002, 2011; Ziessler et al., 2004). Compared to neutral stimuli, the presentation of response effects flanking the imperative stimulus facilitated the response only if the flanking stimuli were the effect of the required response. But also in that study, no inhibition was found when the effects of competing responses were presented.

GENERAL DISCUSSION

The present experiments provide evidence for effect anticipation using a new paradigm that does not require the presentation of the effects itself in the planning phase of the response. Participants were instructed to prepare a particular response, but to withhold its execution until the stimulus configuration would allow it. This is a relatively natural situation. Very often we are prepared to do something, but we have to wait for the suitable conditions before we can actually do it, for example waiting for the green traffic light before we can move the car forward. In the present experiments the execution conditions were defined by Go stimuli. The Go stimuli themselves had no relationship to the responses. Therefore, if the selection of a response and its preparation would depend only on the stimuli presented before response execution, the Go stimuli should not make any difference between the responses and should not affect response execution. In contrast to this assumption, Experiments 1 and 3 showed that events which occurred after the execution of the response also played a role in the reaction time interval. Depending on their relationship to the learned effects of the responses, the Go stimuli were either compatible or incompatible with the given response. The small, but consistent RT differences between effect-compatible and incompatible trials can only be explained by assuming that the Go stimuli interacted with the effect codes. In other words, effect codes must have been active in advance of response execution. Thus, we have found clear evidence for effect anticipation as part of response preparation without presenting the effects themselves during response preparation and also without any feature overlap between responses and effects.

Interestingly, in the present experiments the effects were completely irrelevant for the responses. In the test phases the identity of the required response was fully determined by the imperative stimulus. Participants could execute fast and accurate responses without taking the effects into account. However, the results show that the response effects were anticipated. Apparently, the anticipation of effects is a very basic process. When we plan a motor response or motor action we anticipate the effects that the response or action will cause in the environment, at least if the effects are attended to (Ziessler et al., 2004).

The experiments also show that the cognitive systems associated with response planning are very flexible in learning response effects and in applying this knowledge in motor control. The effects that we examined in the present experiments were completely arbitrary. A small number of acquisition trials was sufficient to create response effect relations that affected the RTs in the following test phase. In Experiment 3 participants had only about 25 repetitions of each response for acquiring the response effects in the acquisition phase. Note that the participants were not explicitly instructed to learn the effects. Nevertheless, we found compatibility effects early on in the test phase and, as the comparisons between the first and second half of the test phase indicated, these compatibility effects did not increase with further practice. Thus, on the one hand it seems that participants learn response effect relations very quickly (see also Wolfensteller and Ruge, 2011). On the other hand, our experiments also show that participants drop their response effect knowledge immediately if the effects are no longer valid. In Experiment 2, participants were given the same opportunity as participants in Experiment 1 to learn the response effects. However, in the test phase they clearly did no longer anticipate the learned effects after they detected that the effects would not appear after the responses.

The compatibility effect did not interact with the SOA. Originally we had expected that the compatibility effect should either decrease or increase with the SOA between the imperative stimulus and the Go stimulus. The data show that this was not the case. In Experiments 1 and 3 the compatibility effect was very stable over the SOA variation. Only the second half of the test phase of Experiment 3 showed a numerical increase of the compatibility effect with the SOA. However, this increase was not statistically significant. In two additional experiments not reported here we repeated Experiments 1 and 3 without the acquisition phase. Participants could only learn the response effects in the test phase. The idea behind this variation was that the process of effect anticipation might change with increasing practice from a more goal-oriented to a more stimulus-driven mode (Ruge et al., 2012). Thus, after extensive practice participants might just respond to the stimuli but not use effect anticipation for response control. The results confirmed that participants learned the effects very quickly. However, also if the participants could only learn the response effects in the test phase, the compatibility effect did not depend on the SOA.

There are two ways to interpret the general stability of our compatibility effects across different SOAs. First, it could be assumed that response selection in the present experiments indeed required the activation of effect codes as assumed by strong versions of the ideomotor theory. Following this approach, response selection occurs through the activation of effect codes (Lotze, 1852; Harleß, 1861; Hommel et al., 2001). Effect codes would then be activated early in the process and would remain active throughout response preparation. Consequently, the Go stimulus in our paradigm could in principle interact with the anticipated effect at all SOAs. However, one might expect the strongest compatibility effects for the shortest SOA, that is, when imperative and Go stimuli are presented together: In this case the presentation of an effect-compatible Go stimulus could prime the anticipation of the response effect and facilitate the response as observed in Experiment 3. **Figure 8A** illustrates this scenario.

Whilst this explanation works very well for the short SOAs, it is more difficult to explain why the compatibility effect would not decrease at longer SOAs: when the effect codes are already fully activated and responses are selected, the impact of the Go stimulus should become weaker. One way to explain compatibility effects at longer SOAs within this first framework might be to assume an additional mechanism: Based on the anticipated effect, participants might facilitate the processing of effect-compatible Go stimuli presented at long SOAs, compared to trials where the Go stimulus is different from the predicted. This means, not only

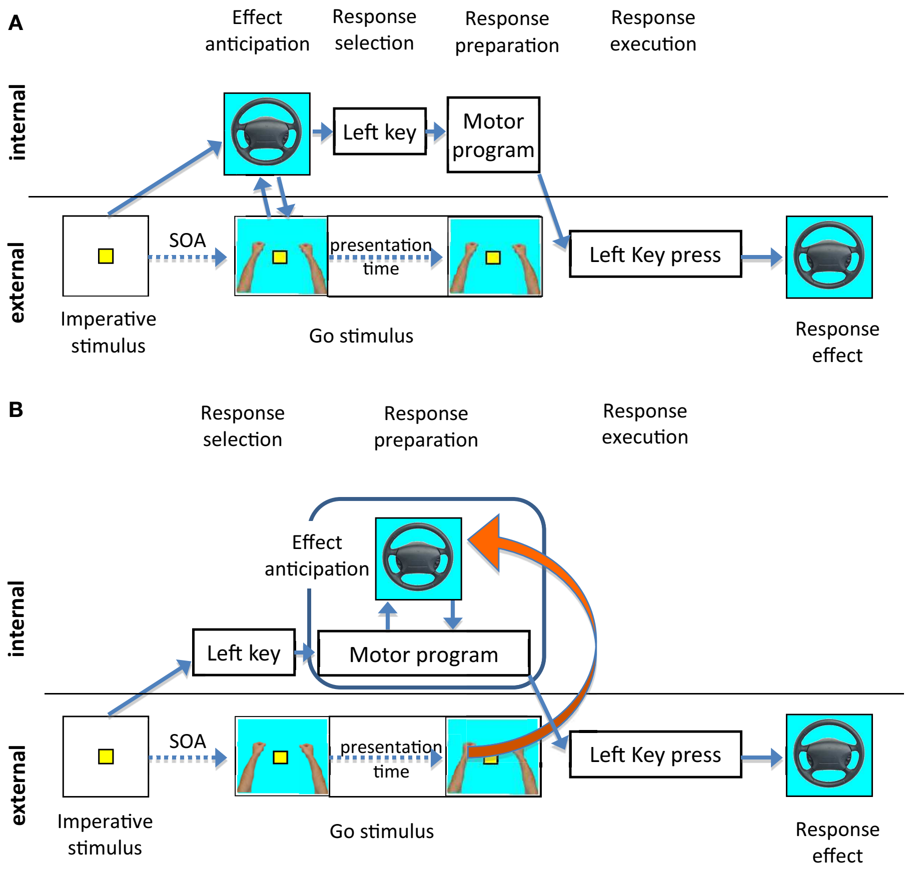

FIGURE 8 | Illustrations of alternative explanations of the compatibility effect and its relationship to the SOA. The dotted line to the right of the Go stimulus illustrates the presentation of the Go stimulus. **(A)** The imperative stimulus directly activates the representation of the effect. Depending on the activated effect codes the response is selected. A compatible Go stimulus would contribute to the activation of the same effect code and therefore facilitate the selection of the response, provided the Go stimulus was presented before the completion of the response selection. **(B)** The response is selected depending on the imperative stimulus. After response selection the effect of the response is anticipated to enable the monitoring of the response and error detection. The anticipation is supported by a compatible Go stimulus throughout response preparation.

would the Go stimulus prime effect anticipation, but also the anticipated effect would prime the processing of the effect-compatible Go stimulus.

A second explanation assumes that effects are anticipated based on an already selected response. **Figure 8B** illustrates this scenario, which is also supported by a number of findings in other experiments. Using a flanker task, we found evidence that effect codes were activated about 150–300 ms after presentation of the imperative stimulus (Ziessler and Nattkemper, 2011). In these experiments the effects of the response were presented as flanking stimuli together with the imperative stimulus. The strongest flanker effects were found if the effect flankers followed the imperative stimulus whereas effect flankers presented in advance of the imperative stimulus had no effect. Further, Nikolaev et al. (2008) could confirm the assumption that effect anticipation takes place after response selection using event related potentials (ERPs) in the same paradigm. In their experiment, ERPs evoked by effect-incompatible flankers differed from those evoked by other flankers in an early perceptual component indicating an inhibition of perceptual representations incompatible with the response, and in later components related to stimulus evaluation and response detection. In addition, the time difference between the lateralized readiness potentials and the onset of the response indicated that also the processes of motor execution were affected by incompatible flankers.

If effects are anticipated for an already selected response, then one might expect that, in the present experiments, the interaction between the Go stimuli and the anticipation of the response effects should be most pronounced at later stages of response preparation, i.e., for the longer SOAs. However, given that in the present experiments, the Go stimulus remained on the screen until the response key was pressed, even at short SOAs the Go stimulus could have interacted with the anticipation of response effects at any time before execution. In a third additional experiment not reported here, we had therefore presented the Go stimulus for a 100 ms period only. Again, there was a main effect of compatibility, but even with this restricted duration of the Go stimulus, the compatibility effect did not reduce at the longer SOAs. It is thus conceivable that the present methodology does not allow to narrow down the precise time point at which the compatibility effects emerge. The paradigm indeed requires that participants process the Go stimulus and keep its representation active throughout response preparation because this is the information indicating that they should finally execute the key-press. If this is correct then the Go signal could affect the anticipation of the response effect at any stage of response preparation.

Even though both interpretations are not exclusive, we consider our findings more in line with the second interpretation. In particular the fact that the early Go stimuli did not induce stronger effects than the late Go stimuli is difficult to align with the idea that effect anticipation is a prerequisite of response selection. This is, of course, not an argument against the strong version of the ideomotor principle as such. We fully agree with Herwig et al. (2007) and Keller et al. (2006) who distinguished between stimulus-based ("response mode") and intention-based actions ("intention mode"). In the intention mode, participants develop stronger action effect bindings and might use the effects in turn to select the actions. The test phase of our experiments was a choice reaction task, i.e., participants were in the response mode and did not need the response effects for response selection. Presumably they only used the effect anticipation for subsequent processing. This might also explain why we could not find any evidence for effect anticipation in Experiment 2 where the response effects were no longer presented, whereas other authors reported effect anticipation without physical presence of the response effects in the test phase. For example, Kühn et al. (2010) used a free-choice task in both, the acquisition and the test phase. Under this condition response effects learned in the acquisition phase led to differences in brain activations in the test phase where the effects were not presented. If we assume that participants in the free-choice task are more likely to act in the intention mode, then the different outcomes of their study and of our Experiment 2 become intelligible. To make sense of the free-choice task participants begin to intend the production of one of the response effects. This does not necessarily change when the effects do not appear in the test phase. Therefore, with the present experiments we do not want to exclude that the anticipation of response effects plays a role in response selection. However, in our paradigm the other functions of effect anticipation were likely more prominent.

In conclusion, with the present experiments we present a new paradigm providing more clear-cut evidence for the anticipation of response effects during response preparation than previously available. Effect anticipation was demonstrated via priming by another stimulus presented during response preparation. The paradigm does not include any direct activation of effect codes by external stimuli and therefore overcomes a possible objection to earlier studies.

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Action-effect bindings and ideomotor learning in intention- and stimulus-based actions

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According to ideomotor theory, action-effect associations are crucial for voluntary action control. Recently, a number of studies started to investigate the conditions that mediate the acquisition and application of action-effect associations by comparing actions carried out in response to exogenous stimuli (stimulus-based) with actions selected endogenously (intention-based). There is evidence that the acquisition and/or application of action-effect associations is boosted when acting in an intention-based action mode. For instance, bidirectional action-effect associations were diagnosed in a forced choice test phase if participants previously experienced action-effect couplings in an intention-based but not in a stimulus-based action mode. The present study aims at investigating effects of the action mode on action-effect associations in more detail. In a series of experiments, we compared the strength and durability of short-term action-effect associations (binding) immediately following intention- as well as stimulus-based actions. Moreover, long-term action-effect associations (learning) were assessed in a subsequent test phase. Our results show short-term action-effect associations of equal strength and durability for both action modes. However, replicating previous results, long-term associations were observed only following intention-based actions. These findings indicate that the effect of the action mode on long-term associations cannot merely be a result of accumulated short-term action-effect bindings. Instead, only those episodic bindings are selectively perpetuated and retrieved that integrate action-relevant aspects of the processing event, i.e., in case of intention-based actions, the link between action and ensuing effect.

Keywords: feature binding, event file, action-effect, sensorimotor integration, ideomotor learning

INTRODUCTION

Humans either carry out actions to achieve desired effects in the environment or to accommodate to environmental demands. For instance, pressing the cappuccino button on a coffee dispenser is primarily based on the agent's intention to have a hot cup of coffee. In contrast, flooring the brake pedal at a red traffic light is chiefly performed in response to a prior stimulus event. These two types of action have been labeled voluntary, operant, or *intention-based*, on the one side, and reaction, response, or *stimulus-based*, on the other side.

Neuroscientific evidence suggests that intention- and stimulus-based actions have distinct neural bases (e.g., Goldberg, 1985; Passingham, 1993; Praamstra et al., 1995; Deiber et al., 1996; Waszak et al., 2005, 2012; Mueller et al., 2007; Haggard, 2008). This distinction is further supported by clinical observations showing a selective impairment of one type of action and thereby implying dissociation between intention- and stimulus-based action control (e.g., Lhermitte, 1983; Shallice et al., 1989).

However, the actual processes that guide these two types of actions are still not well understood. One obvious functional difference between intention- and stimulus-based actions is the role of external stimuli either preceding or following the action. According to ideomotor reasoning (e.g., Harleß, 1861; James, 1890; for recent reviews, see Nattkemper et al., 2010; Shin et al., 2010; Pfister and Janczyk, 2012) intention-based actions are primarily directed at and selected by the effects following the action whereas there is a less obvious connection to preceding stimuli. On the contrary, stimulus-based actions, by definition, crucially depend on preceding stimuli whereas stimuli following the action are often less important. Thus, it has been suggested that intention-based actions rely more strongly on action-effect associations specifying which action produces which effect, whereas stimulus-based actions rely more strongly on stimulus-response associations specifying which motor routines action-relevant stimuli habitually require (Waszak et al., 2005, 2012; Herwig et al., 2007, 2013; Pfister et al., 2010).

IDEOMOTOR LEARNING

The purported functional difference is supported by a number of recent studies directly comparing the long-term consequences of actions carried out in response to exogenous stimuli (stimulus-based) with actions selected endogenously (intention-based). For instance, Herwig et al. (2007) investigated ideomotor learning, that is, the spontaneous acquisition of action-effect associations, in intention- and stimulus-based actions. Ideomotor learning can be assessed in a paradigm conceived by Elsner and Hommel (2001). These authors made participants first undergo an acquisition phase, in which a self selected key press always produced a particular tone (e.g., left key press/high-pitch tone; right key press/low-pitch tone). After having performed about 200 key presses, the same tones were presented as imperative stimuli for a speeded choice response in a subsequent test phase. Elsner and Hommel observed that the speeded choice responses were faster in response to the tone that the action had previously produced (e.g., compatible group: low-pitch tone/right key press) than to a tone that had been produced by the alternative action (e.g., incompatible group: high-pitch tone/right key press). This result demonstrates that, during the acquisition phase, participants acquire long-lasting bidirectional associations between the motor code of the action and the perceptual code of the auditory effect (i.e., action-effect associations). Presenting the effects as imperative stimuli in a later test phase leads to the retrieval of the previously acquired action-effect association which either speeds up or slows down the speeded choice task depending on whether the retrieved actions are compatible or incompatible with the instructed response.

Importantly, the effect of action-effect associations on a subsequent speeded choice task depends on the action mode during acquisition (Herwig et al., 2007; Herwig and Waszak, 2009; Herwig and Horstmann, 2011; but, see Pfister et al., 2011, for different results with a free choice test). That is, in the studies of Herwig and colleagues, a compatibility effect only occurred if, in the acquisition phase, participants freely selected between left and right key presses (intention-based acquisition), whereas there was no compatibility effect if the actions were triggered by external stimulus events (stimulus-based acquisition). This dependency on the action mode holds true for such different effect- and action-modalities like auditory effects and manual actions (Herwig et al., 2007; Herwig and Waszak, 2009) as well as visual effects and oculomotor actions (Herwig and Horstmann, 2011). Moreover, guiding participants' attention away or toward the effect did not influence the pattern of results (Herwig and Waszak, 2009). Thus, the observed differences between intention- and stimulus-based actions are not simply due to differences in allocation of attention to the action-effect event. Instead, the results suggest that one and the same action-effect event results in different long-term consequences depending on the action mode: if actions are performed in the intention-based mode, ideomotor learning occurs, that is new action-effect associations are acquired and later on retrieved upon effect presentation. In contrast, if actions are selected in the stimulus-based mode, sensorimotor learning occurs, that is stimulus-response associations are established while action-effect associations are much harder to detect subsequently.

It has to be noted that to date it is still under debate why action-effect associations are much harder to detect following stimulus-based actions and different hypotheses have been proposed. Herwig et al. (2007) suggested that the action mode affects the acquisition of action-effect associations. Accordingly, action-effect associations are weaker following a stimulus-based acquisition compared to an intention-based acquisition which in turn hampers their later detection. However, the different-acquisition hypothesis was recently put into question by two studies showing ideomotor learning also following stimulus-based actions (Pfister et al., 2011; Wolfensteller and Ruge, 2011). In the study of Pfister et al. (2011) ideomotor learning was assessed in a free choice test phase, in which participants were presented with randomly selected action-effects, which merely served as a trigger to carry out a self-chosen response. Under these test conditions participants preferred the selection of the action that was previously producing the effect regardless of the action mode during acquisition. To account for the differences between their own results and the results of Herwig et al. (2007), the authors proposed the different-application hypothesis (for converging evidence that the action mode can affect the application of action-effect associations, see Pfister et al., 2010; Herwig and Horstmann, 2011). According to this hypothesis, action-effect associations are acquired irrespective of the action mode, but are applied during the test phase only if an intention-based mode is adopted. Importantly, adopting an intention- or a stimulus-based mode depends not only on the current task in the test phase (free choice vs. forced choice) but also on the previous task in the acquisition phase (free choice vs. forced choice). However, the relationship between these two determining factors seem to be quite complex. According to Pfister et al. (2011) the intention-based mode is quickly adopted if participants carry out self-chosen responses (either during acquisition or test) and once adopted they will stick to this action mode even in a forced choice test phase. In contrast, participants slowly adopt a stimulus-based mode during a forced choice acquisition phase but remain in this mode only if they continue to perform forced choice actions in the test phase. Finally, Wolfensteller and Ruge (2011) suggested a third hypothesis to explain the observed effect of the action mode on ideomotor learning. In their study participants had to constantly switch between stimulus-based acquisition phases of varying lengths and forced choice test phases in which the effects were presented together with the imperative stimuli[1]. The results showed a small but reliable compatibility effect after only 12 action-effect episodes which seems to depend on contextual stability (i.e., on a consistent stimulus-response mapping). Therefore Wolfensteller and Ruge proposed the different-context hypothesis which states that action-effect associations following a stimulus-based acquisition are contextualized by means of their imperative stimuli (i.e., stimulus-action-effect episode). Such a contextualization can in principle hamper the retrieval of action-effect associations if the context (i.e., the imperative stimuli) changes between acquisition and test (cf., Godden and Baddeley, 1975).

Unfortunately, to date none of the three hypotheses, that is the different-acquisition, the different-application, and the different-context hypothesis, can satisfactorily explain all of the divergent results concerning the effect of the action mode on ideomotor learning. Thus, one main aim of the present study was to take a closer look at the emergence of action-effect associations against

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[1] Contrary to previous studies (e.g., Elsner and Hommel, 2001; Herwig et al., 2007; Pfister et al., 2011), Wolfensteller and Ruge (2011) made their participants switch repeatedly between acquisition and test phases in one experimental session. It is possible that this procedure has replaced incidental ideomotor learning by intentional learning since participants might have noticed that the effects will be relevant in the following test phases.

the background of the different-acquisition hypothesis proposed by Herwig et al. (2007).

ACTION-EFFECT BINDING

Up to now, we focused on the influence of the action mode on the compilation of action-effect associations that may be retrieved at least a couple of minutes after the acquisition (i.e., long-term associations or *learning*, hereafter). However, the build-up of long-term memory traces is not the only type of perceptuomotor integration that takes place when humans interact with the environment. The other type refers to a much shorter timescale and is related to one of the main characteristics of the primate brain: distributed coding (i.e., short-term integration or *binding*, hereafter)[2]. Distributed coding refers not only to features in the visual domain (e.g., shape, color, and location, see Cowey, 1985; Felleman and van Essen, 1991) and in the auditory domain (e.g., periodicity, location, and spectral shape, Brown and Wang, 2006) but also as regards the features of to-be-performed actions (e.g., direction, amplitude, and duration, Wickens et al., 1994).

Importantly, distributed coding creates numerous binding problems (Treisman, 1996), which call for some kind of integration mechanism that binds together the distributed codes belonging to the same object (e.g., color, shape, and motion of an object). Hommel (1998) argued that the binding problem holds for perceptuomotor processing as well. That is, perceptual and motor codes belonging to the same event need to be integrated, too (Hommel, 2004). Following previous work addressing the creation of "object files" (Kahneman et al., 1992), the temporarily stored outcome of this integration process was termed "event file" (Hommel, 1998).

Bindings of stimulus and action features can be assessed in the prime-probe stimulus-response task of Hommel (1998). In this paradigm each trial comprises two subtasks. In the first subtask, participants perform simple, precued left- or right key presses (R1) to the mere presence of a "Go" signal (S1) that varies randomly in form, color, and location. The effects of bindings created between S1- and R1-features on later performance are assessed in a second subtask, which is a binary-choice reaction (R2) to a pre-instructed feature (e.g., color) of a second stimulus (S2). The typical result of this type of paradigm is that performance is impaired in partial repetition trials, that is, if only the stimulus (or only the response) is repeated, compared to when both stimulus and response are repeated or when both change. This pattern of results suggests that a temporary binding of the respective codes is compiled when stimuli and actions co-occur. Repeating one feature reactivates also the associated fellow code, which, in partial repetition trials, creates a mismatch and, therefore, induces a time-consuming re-binding process (for a review, see Hommel, 2004).

Transient perceptuomotor bindings have been shown to emerge quickly (after 300 ms or less) and to remain intact for at least 4 s (Hommel and Colzato, 2004). Moreover, the temporal order of S1 and R1 does not seem to be important for perceptuomotor binding. Hommel (2005, Experiment 2) showed that stimulus features were bound to response features even if S1 follows R1 which suggests that the temporal time window for feature integration might be rather broadly defined. Thus, temporary feature binding across perception and action may take place not only in events, where the perceptual stimulus triggers the action (stimulus-based actions), but also in events, where the action triggers the perceptual event (intention-based actions). This opens up the possibility to investigate the immediate binding between actions and their effects in stimulus- and intention-based actions.

THE PRESENT STUDY

As outlined above, Herwig and colleagues (Herwig et al., 2007; Herwig and Waszak, 2009; Herwig and Horstmann, 2011) proposed that the acquisition of action-effect associations (i.e., ideomotor learning) is affected by the action mode. The present study investigates whether the action mode also influences temporary feature bindings. Although there is already some evidence that action-effect bindings can be observed following intention-based actions (Dutzi and Hommel, 2009) and stimulus-based actions (see Hommel, 2005; Experiment 2), a direct comparison of the strength and durability of action-effect bindings following intention- and stimulus-based actions is lacking. As a consequence, it is utterly unknown whether temporary action-effect bindings, too, are affected by the action mode and one main aim of the present study was to address this gap in the literature.

We ran three experiments that compare strength as well as durability of action-effect bindings between the two action modes. Experiments 1 and 2 were designed to test how the two types of integration, that is, binding and learning, are related. Based on the different-acquisition hypothesis proposed by Herwig et al. (2007) we, see three possible relationships (see Colzato et al. (2006), for similar considerations). First, binding and learning are tightly linked (*strong dependence hypothesis*). Binding via synchronization may cause long-term modifications of synaptic efficacy as suggested by Fell et al. (2003). In this scenario, temporary bindings strengthen the association between two features mediated through Hebbian learning (i.e., neurons that fire together, wire together; Hebb, 1949), each time making the memory trace more durable. The strong dependence hypothesis assumes that the difference in ideomotor learning between intention- and stimulus-based actions, as shown by Herwig et al. (2007), is due to a difference in action-effect binding between the two modes of movement. That is, if action and effect do not wire together (ideomotor learning) in stimulus-based actions, then this might be due to the fact that action and effect features do not always fire together (temporary bindings) in the first place.

Second, ideomotor learning is completely independent of the formation of temporary action-effect bindings. Although such a *non-dependence hypothesis* is rather radical, it is not so unlikely, since binding and learning act on different time-scales and are thought to solve different problems, with bindings being involved in the problem of distributed coding and ideomotor learning being involved in the control of voluntary actions. Under this view temporary feature binding represents a representational level which is mainly used for the perception of the current event. Action-effect associations underlying ideomotor learning, however, represent a

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[2] It should not go unnoticed that the divide between learning and binding is, at the same time, a divide between short-term and long-term memory. The question how binding and learning are related is thus also a question about how short-term memory representations are consolidated and translated into long-term memory. We will come back to this question in the General Discussion.

different representational level at which integrated feature assemblies are stored for the purpose of future guidance of behavior. Accordingly, there might be two crucial distinctions between both levels of representation. First, bindings as part of short-term memory depend on the actual presentation of an external effect, whereas action-effect associations as part of long-term memory depend on the internal generation of the effect. As a consequence, both representational levels might fundamentally differ in the level of detail and concreteness they are able to provide. Second, while action-effect associations underlying ideomotor learning presuppose contingent action-effect relationships (Elsner and Hommel, 2004), short-term bindings are also engaged in the perception of ever-changing action-effect relationships – just think of the different sounds one produces while talking with the mouth empty vs. full or the different ball trajectories one produces while playing pinball. Accordingly, both levels might fundamentally differ in the range of events they are able to incorporate. The non-dependence hypothesis thus assumes that the difference in ideomotor learning between intention- and stimulus-based actions (Herwig et al., 2007) do not have to be reflected in short-term bindings.

Third, binding and learning may not be as rigidly connected as assumed under the strong dependence hypothesis and not as independent as under the non-dependence hypothesis. In daily life, the particular effect that an action achieves depends tremendously on the current context. It would appear inefficient to perpetuate all episodes, that is, even those which are not needed anymore once the particular event is finished. This should especially hold true for non-contingent action-effects which cannot be reliable used for action planning. On the *weak dependence* view, binding and learning do not take place on fundamentally different levels. Instead, bindings are the building blocks for long-term associations, but only those bindings which are reliable and thus worthwhile to be preserved are further processed to form a more durable memory trace (see Colzato et al., 2006). The weak dependence hypothesis thus assumes that binding and learning are related only in case of contingency. Therefore, the difference in ideomotor learning between intention- and stimulus-based actions (Herwig et al., 2007), should only be reflected in short-term bindings of contingent action-effects, whereas it should not be reflected in short-term bindings of non-contingent action-effects.

Experiments 1 and 2 are designed to pit these three accounts against each other. The crucial difference between the experiments is that in Experiment 1 the features of the action-effect were not contingent on the action (as it is usually the case in this type of experiment), whereas in Experiments 2 they were contingent. The strong dependence hypothesis assumes that a difference in action-effect binding is the reason for the difference in ideomotor learning between intention- and stimulus-based actions. Consequently, this hypothesis predicts that intention-based actions result in both experiments in stronger binding effects than stimulus-based actions. The non-dependence hypothesis predicts that intention-based actions result neither in Experiment 1 nor in Experiment 2 in stronger binding than stimulus-based actions, since under this view learning and binding represent two different representational levels. Finally, the weak dependence hypothesis predicts that intention-based actions result only in Experiment 2 in stronger binding than stimulus-based actions, but not in Experiment 1. This is because contingent action-effects can be used only in Experiment 2 but not in Experiment 1 as building blocks for long-term associations. Experiment 3 complements Experiments 1 and 2 by directly comparing binding and ideomotor learning within one experiment.

To sum up, the present study addresses two research questions. First, are temporary bindings between action and effect features modulated by the action mode? Second, how are short-term bindings and long-term ideomotor learning related?

EXPERIMENT 1

To investigate the influence of the action mode on temporary action-effect bindings, we slightly modified the original prime-probe stimulus-response task comprising of two subtasks (see above; Hommel, 1998). In the first subtask, the first response (R1) to a neutral go signal was either freely selected (intention-based trials) or precued (stimulus-based trials). In both cases it triggered one out of four auditory effect stimuli (S1; see **Figure 1**). The second subtask was a speeded forced choice response (R2) to a second stimulus (S2). Moreover, we manipulated the stimulus-onset asynchrony (SOA) between S1 and S2 (1000 vs. 2000 vs. 6000 ms) to assess binding durability.

To assess the binding between features of R1 and S1, our focus was on interactions between stimulus and response repetition effects. On the basis of earlier findings regarding perceptuomotor binding (Hommel, 2005, Experiment 2), we expected that performance is impaired on partial repetition trials, in which either the response feature is repeated while the stimulus feature is alternated, or the stimulus feature is repeated while the response feature is alternated (partial repetition costs). By contrast, alternating both stimulus and response between the two subtasks of one trial should yield a performance level in the second subtask that is as good as when both are repeated. Such a pattern of results points to action-effect binding, since it implies that reactivating one feature tends to also activate the fellow feature. This, in turn, causes conflict in case of partial repetitions.

The crucial question was whether this interaction would be modulated by the action mode (intention- vs. stimulus-based). Under the strong dependence hypothesis of the relation between learning and binding, one would expect action-effect bindings to be weaker or less durable for stimulus-based than for intention-based actions. In this case the fragility of action-effect bindings in stimulus-based actions could be considered to be responsible for the effect of the action mode on ideomotor learning (see Herwig et al., 2007; Herwig and Waszak, 2009). Under the weak dependence hypothesis as well as the non-dependence hypothesis, binding should not be influenced by the action mode.

MATERIALS AND METHODS

Participants

Sixteen adults (mean age: 24.9 years) participated. They reported having normal or corrected-to-normal vision and audition and were not familiar with the purpose of the experiment. Informed consent was obtained from all subjects.

Apparatus and stimuli

The experiment was controlled by a standard PC, interfaced to a 17″ monitor. The viewing distance was about 70 cm. Visual

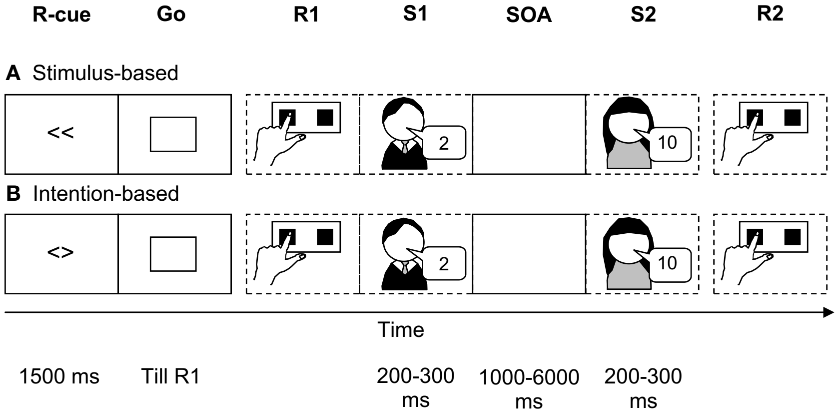

FIGURE 1 | Basic experimental setup to assess action-effect bindings. The first subtask consists of a simple go response (R1) either in a stimulus-based **(A)** or intention-based **(B)** action mode which triggered the auditory presentation of stimulus 1 (S1). The second subtask is a binary-choice response to the number feature of stimulus 2 (S2). Note that the stimulus-onset asynchrony (SOA) was varied in Experiments 1 and 2 (1000, 2000, 6000 ms) and was constant in Experiments 3 (1000 ms).

stimuli were displayed on a black background. In stimulus-based trials, two white left- or right-pointing arrows (mean extension: 0.4° × 0.7°) served as response cues and were presented in the center of the screen. In intention-based trials, the response cue was replaced by the free choice cue, i.e., two arrows pointing in different directions (<>) requesting participants to prepare a left or right key press depending on their own choice. A white rectangle (mean extension: 0.7° × 1.0°) served as a go signal for the execution of the precued/prepared response. Auditory stimuli were the English numbers "2" and "10" vocalized by a male or female voice (duration 200–300 ms). The words were presented simultaneously through the left and right speaker of a headphone. Responses were made by pressing the left or right of two keys mounted in a horizontal distance of 13.5 cm on a board with the left or right index finger.

Procedure and design

Each trial comprised two speeded responses. The first response (R1) was always a simple reaction to the go signal. The type of response (i.e., left or right key press) was either indicated by the response cue (stimulus-based trials) or depended on participants' own choice (intention-based trials). R1 triggered the presentation of the first auditory effect stimulus (S1). Whether the stimulus was the number 2 or 10 vocalized by a male or a female voice was determined randomly. The second response (R2) was always a binary-choice reaction to the number feature of the second stimulus (S2). S2 was again either the number 2 or 10 vocalized by either a male or a female voice, randomly determined. Half of the participants responded to the number 2 and 10 by pressing the left and right key, respectively, whereas the other half responded according to the opposite mapping.

The sequence of events in each trial is shown in **Figure 1**. Following an intertrial interval of 2000 ms, a response cue or a free choice cue was presented for 1500 ms, followed by the go signal that was presented until the first response was executed. R1 triggered the presentation of S1 (50-ms onset asynchrony between R1 and S1). If R1 was not executed within 1000 ms (counted as omission) a visual warning message (too slow) was presented for 800 ms and the trial started from the beginning. If R1 was incorrect (only possible in stimulus-based trials) or anticipatory (RT < 80 ms) a visual warning message (wrong key, too fast, respectively) was presented for 800 ms and the trial continued. S2 appeared 1000, 2000, or 6000 ms after the onset of S1. Responses to S2 that were incorrect, premature (RT < 80 ms) or omitted (RT > 2000 ms) triggered presentation of the corresponding visual warning message.

The experiment was divided into four parts which were done in 1 day. Two of the four parts consisted of 3 blocks of 96 randomly ordered intention-based trials each and the remaining two parts of 3 blocks of 96 randomly ordered stimulus-based trials each. The order of the four parts was counterbalanced across participants. Participants performed 24 randomly selected practice trials at the beginning of the experiment and prior to the first switch of the action mode. That is, all in all the experiment comprised 48 practice trials and 1152 experimental trials which took approximately 4 h. Each block was composed of a factorial combination of S2 number (2 vs. 10, corresponding to left vs. right R2) and S2 gender (male vs. female), the possible relationships between S1 and S2 (repetition vs. alternation) regarding number and gender, the SOA between S1 and S2 (1000 vs. 2000 vs. 6000 ms), and the two possible relationships between R1 and R2 (repetition vs. alternation). In intention-based blocks, in contrast, the relationship between R1 and R2 could not be determined *a priori* because R1 depends on participants' free choice. In these blocks participants were instructed to use the left and right key for the first response about equally often and in a random order. Participants could take a break after each block.

RESULTS

For the sake of clarity and according to our main question (i.e., action-effect bindings for intention- and stimulus-based actions), we present only the results of subtask 2 and, specifically only the reliable effects in the main text. The Appendix presents the results of subtask 1 as well as two tables which provide a detailed overview of the means (see **Table A1** in Appendix) and ANOVA outcomes (see **Table A2** in Appendix) for RTs and error rates obtained for subtask 2. After excluding trials in which R2 was anticipated or omitted (0.2%), R2 data were analyzed as a function of the action mode (intention- vs. stimulus-based), SOA (1000 vs. 2000 vs. 6000 ms), and repetition vs. alternation of stimulus number, gender, and response. Analyses of variance (ANOVA) with the factors Action mode (intention- vs. stimulus-based), Response (repetition vs. alternation), Number (repetition vs. alternation), Gender (repetition vs. alternation), and SOA (1000 vs. 2000 vs. 6000 ms) were performed on error rates and error-free RTs by using a five-way design for repeated measures. Violations of sphericity were corrected using the Huynh–Feldt ε. The significance criterion was set to $p < 0.05$ for all analyses.

Reaction times

The RT analysis yielded five reliable effects and importantly, none of these effects interacted with the action mode (*p*s > 0.24). There were main effects of SOA, response, and gender. These main effects indicated faster responses with increasing SOA (661, 637, and 603 ms for SOA of 1000, 2000, and 6000 ms, respectively), for response alternations (643 and 624 ms for response repetitions and alternations, respectively), and for gender repetitions (623 and 644 ms for gender repetitions and alternations, respectively). The main effect of gender was further modified by an interaction with number, indicating an integration of the auditory stimulus features number and gender.

More importantly, the main effect of response was modified by an interaction with number, indicating action-effect binding. **Figure 2** shows the relative repetition benefit for each stimulus dimension (i.e., the mean RT difference between number/gender alternation and number/gender repetition; note that the values depicted in **Figure 2** are differences of averaged values given in **Table A1** in Appendix) as a function of the relationship between R1 and R2 separated for intention- and stimulus-based trials and the three SOAs. A positive difference indicates that participants responded faster for stimulus repetitions than alternations, whereas a negative difference indicates faster reactions for stimulus alternations than repetitions. As **Figure 2** clearly shows, repeating stimulus number produces a benefit if, and only if, the response is

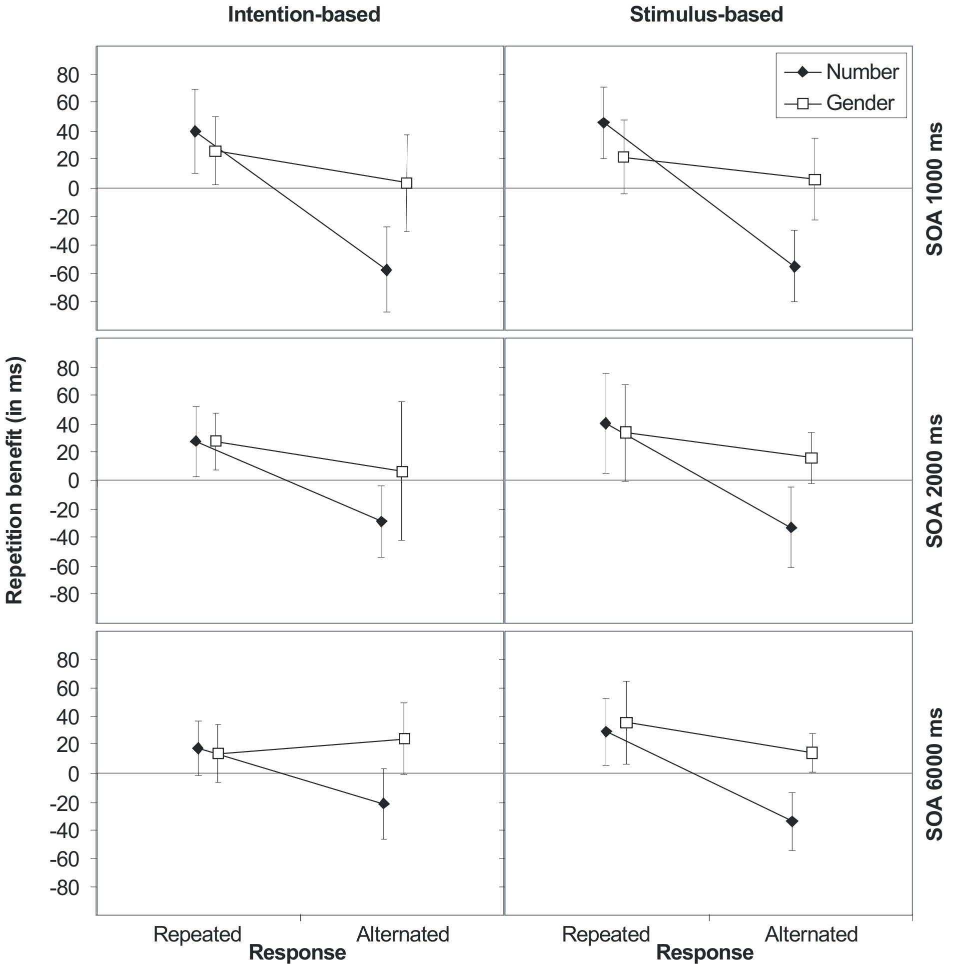

FIGURE 2 | Stimulus (S1–S2) repetition benefits ($RT_{alternation}$-$RT_{repetition}$) in Experiment 1 for stimulus features number and gender for intention-based (left panels) and stimulus-based trials (right panels) as a function of response relation (R1–R2 repetition or alternation) and stimulus-onset asynchrony (SOA 1000, 2000, and 6000 ms, from top to bottom). Error bars represent the 95% confidence interval of individual means. If error bars do not cross the midline, the repetition benefit (or cost) is significantly different from zero ($p < 0.05$).

also repeated. If the response is alternated, the repetition benefit turns into an alternation benefit. This was true for all three SOAs.

Error rates

The error rates overall mirrored the RTs but produced some additional effects. Importantly, once again none of the reported effects was modified by the action mode (ps > 0.27). As concerns the main effects, participants committed fewer errors with increasing SOA and response alternations. However, in contrast to the RT data, participants committed fewer errors with gender alternations (3.0 and 2.5% for gender repetitions and alternations, respectively). Thus a speed-accuracy trade-off can be excluded for the factors SOA and response, but not for gender. The main effect of response was modified by an interaction with SOA, indicating an increased alternation benefit with the medium SOA of 2000 ms.

Of importance, response interacted with number as well as with gender, indicating that each stimulus dimension was separately integrated with the response. Repeating both the number and the response or alternating both (1.8 and 0.9%, respectively) decreased the error rate, whereas the error rate increased if only one, but not the other, was repeated (number repeated: 3.5%; response repeated: 4.9%). Likewise, a response repetition was easier if gender was also repeated than alternated (3.2 and 3.4%, respectively), whereas a response alternation was easier if gender was also alternated than repeated (1.7 and 2.8%, respectively). Moreover, action-effect bindings for both effect features interacted with SOA. Separate ANOVAs for each SOA showed both interactions to be significant only for the SOAs of 1000 [response × number: $F(1,15) = 36{,}80$, $p < 0.001$, $\eta_p^2 = 0.71$, response × gender: $F(1,15) = 12.06$, $p = 0.003$, $\eta_p^2 = 0.45$] and 2000 ms [response × number: $F(1,15) = 24.30$, $p < 0.001$, $\eta_p^2 = 0.62$; response × gender: $F(1,15) = 4.83$, $p = 0.044$, $\eta_p^2 = 0.24$] but not for the SOA of 6000 ms (ps > 0.143).

DISCUSSION

As shown in **Figure 2**, the effect of stimulus repetition was clearly dependent on whether or not the response was also repeated. Thus, Experiment 1 suggests that the co-occurrence of action and auditory codes triggered by the action results in the temporary binding between the involved perceptual and motor features. Comparable to studies investigating perceptuomotor binding (e.g., Hommel, 1998), action-effect bindings were pronounced for the task relevant stimulus feature (i.e., number). Moreover, the analysis of RTs of Experiment 1 showed action-effect bindings to remain intact for at least six seconds – a finding that extends the results regarding the durability of perceptuomotor bindings by 2 s (Hommel and Colzato, 2004).

More importantly, Experiment 1 did not show any influence of the action mode on the strengths or durability of the action-effect bindings. That is, short-term action-effect bindings were comparably strong and durable following intention- and stimulus-based actions. This observation is in contrast to the predictions derived from the strong dependence hypothesis of binding and learning. Thus, the finding of Herwig et al., 2007; see also Herwig and Waszak, 2009; Herwig and Horstmann, 2011) that ideomotor learning is affected by the action mode does not seem to be due to an elementary difference in action-effect binding.

However, the dissociation of the effect of the action mode on binding and learning is in accord with the non-dependence as well as the weak dependence hypothesis. If binding and learning actually represent two independent representational levels (as suggested by the non-dependence hypothesis), one would not expect binding and learning to be influenced by the same factors. According to the weak dependence hypothesis action-effect binding is a necessary, but not a sufficient precursor for long-term ideomotor learning. In this scenario, the action mode might determine whether or not the repeated formation of identical transient bindings forms a memory trace. Metaphorically speaking, bindings may be regarded as building blocks that are constructed whenever an effect is produced in close temporal contiguity by an action regardless of whether the action was externally or internally selected. However, only intention-based actions, but not stimulus-based actions, may provide the glue necessary to agglutinate these building blocks to form a durable memory trace.

This notion can only be tested if one effect feature is produced contingently by one and not the other action. In Experiments 1 each effect feature was produced by each action with the same probability. Consequently, distinct action-effect relations could not be established. Therefore, we implemented contingent action-effect mappings in Experiments 2 and 3.

EXPERIMENT 2

As pointed out above, one reason for the missing influence of the action mode on the formation and durability of action-effect bindings might be related to the fact that each effect feature was produced by each action with the same probability. It is possible that due to this missing contingency between action and effect features binding and learning are unrelated as suggested by the weak dependence hypothesis. To address this issue, Experiment 2 was conducted, in which each action (R1) contingently produced one specification of the irrelevant effect feature of S1 (i.e., gender). For example, pressing the left key led to the auditory presentation of the number "2" or "10" spoken by a female voice, whereas pressing the right key resulted in the presentation of the number "2" or "10" spoken by a male voice.

Such a contingency manipulation should in principle enhance ideomotor learning (Elsner and Hommel, 2004). Importantly, if the weak dependence hypothesis holds (i.e., if binding and learning are only related in case of a contingent action-effect relationships), this enhancement should be reflected in partial repetition costs as well. This is because in Experiment 2 R2 may be affected by two factors: the *event file* compiled during the fist subtask of each trial and the *memory trace* emerging through the repeated experience of the contingent action-effect mapping. Both factors should entail RT costs if only the gender or the response is repeated while the other feature is alternated (i.e., partial repetition costs). Consider, for instance, an action-effect mapping for R1–S1 that links a left key press with a female voice (F) and a right key press with a male voice (M). Moreover, the stimulus-response mapping rule for S2–R2 be to respond to the number two (2) and ten (10) by pressing the right and left key, respectively. If S2 is the number two spoken by a female voice ($2F$), this might lead to a conflict in initiating R2 because female may automatically activate the left response due to the compiled memory trace, whereas 2 calls for a right response due to the instructed mapping. Likewise, if S2 is *10M*, 10 calls for a left whereas male calls for a right response. In contrast, no conflict arises if S2 is *2M* or *10F*, because the number as well as the gender feature call for the same response. Importantly, in the given example, *2F* and *10M* would also be the partial repetitions with respect to R1–S1, because a left R1 always triggers S1 spoken with a female voice and a right R1 always triggers S1 spoken with a male voice (leaving *2M* and *10F* as complete repetitions or complete alternations). Accordingly, if contingency determines whether binding and learning are related or not, one would expect R2 to be influenced by the previously compiled event file and the accumulating memory trace only for intention-based actions. In contrast, for stimulus-based actions R2 should be affected solely by the event file, resulting in a three-way interaction of response, gender, and action mode.

MATERIALS AND METHODS

Participants were 32 adults (mean age 24.2 years) who fulfilled the same criteria as those in Experiments 1. The method was the same as in Experiment 1, with the following exceptions. The gender feature of S1 depended on R1 (e.g., left key press/female voice, right key press/male voice), whereas there was, as in Experiment 1, no contingency regarding the number feature of S1. The action-effect mapping was counterbalanced across participants. Participants were not informed about the contingency manipulation[3]. Moreover, in contrast to Experiment 1, the action mode (intention- vs. stimulus-based) was manipulated between subjects to avoid transfer effects (i.e., to be sure that R2 following stimulus-based actions was not influenced by accumulated memory traces that were established following intention-based actions). The experiment consisted of 6 blocks of 96 randomly ordered trials and took approximately 2 h. Each block was composed of the possible combinations of two R1 alternatives (left vs. right), two S1 alternatives (2 vs. 10, either male or female voice depending on R1), four S2 alternatives (2-male, 2-female, 10-male, 10-female), three SOAs (1000, 2000, 6000 ms), and two repetitions of each combination. Importantly, the independence of repetition vs. alternation regarding number, gender, and response remained unchanged by the contingency manipulation, since R2 was always a reaction to the number feature (and not to the gender feature) of S2.

RESULTS

As for Experiment 1, we present only the reliable effects of subtask 2 in the main text. The Appendix presents the results of subtask 1 as well as the means (see **Table A3** in Appendix) and ANOVA outcomes (see **Table A4** in Appendix) for RTs and error rates obtained for subtask 2. After excluding trials in which R2 was anticipated or omitted (0.3%), R2 data were analyzed as in Experiment 1. ANOVA with the between subjects factor Action mode (intention- vs. stimulus-based) and the within subjects factors Response (repetition vs. alternation), Number (repetition vs. alternation), Gender (repetition vs. alternation), and SOA (1000 vs. 2000 vs. 6000 ms) were performed on error rates and error-free RTs.

Reaction times

The RT analysis produced various reliable effects. Importantly, action-effect bindings were not modulated by the action mode ($ps > 0.24$). Beside the main effects of SOA, response, number, and gender, indicating faster responses with increasing SOA (586, 566, and 544 ms), faster responses for response alternations (575 and 556 ms), number repetitions (560 and 571 ms), and gender repetitions (560 and 571 ms), all three two-way interactions between response, number, and gender reached significance, indicating stimulus feature as well as action-effect bindings.

As shown in **Figure 3**, repeating stimulus number or gender produce a benefit if, and only if, the response is also repeated, whereas the repetition benefit turns into an alternation benefit if the response is alternated. Although the response × number interaction was further modified by SOA, separate ANOVAs showed the interaction to be significant for all three SOAs. Noteworthy, there was a three-way interaction of response × number × gender that was due to a decrease of the response-by-gender interaction-effect if the number was alternated. However, separate ANOVAs showed the response-by-gender interaction to be significant for number repetitions [($F(1,30) = 40.91$, $p < 0.001$, $\eta_p^2 = 0.58$] as well as number alternations [$F(1,30) = 4.63$, $p = 0.040$, $\eta_p^2 = 0.13$].

Error rates

The error rates overall mirrored the RTs. Importantly, once again none of the reported effects was modified by the action mode. Beside the main effects of SOA and response, the two binary interactions between response and number and response and gender followed the same pattern as the RTs and indicated action-effect binding. That is, repeating both the number and the response or alternating both (2.2 and 0.7%, respectively) decreased the error rate, whereas the error rate increased if only one, but not the other, was repeated (number repeated: 4.4%; response repeated: 4.8%). In the same way, repeating both the gender and the response or alternating both (3.1 and 1.7%, respectively) decreased the error rate, whereas the error rate increased if only one, but not the other, was repeated (gender repeated: 3.4%; response repeated: 4.0%). Both action-effect bindings were further modified by SOA. Separate ANOVAs showed the response-by-number interaction to be significant for all SOAs, whereas the response-by-gender interaction was only significant for the SOAs of 1000 and 2000 ms but not for the SOA of 6000 ms ($p > 0.37$). The three-way interaction of response × number × gender followed the same pattern as the RTs and was due to a decrease of the response-by-gender interaction-effect if the number was alternated.

DISCUSSION

First of all, Experiment 2 yielded a reliable response-by-gender interaction, i.e., an interaction between the response and the contingent (first subtask), but task irrelevant (second subtask) effect feature. Although pronounced for repetitions of the task relevant feature this action-effect binding occurred also for alternations of the task relevant feature. This pattern of results suggests two things. First, each effect feature (i.e., the relevant but non-contingent

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[3] A post-test survey revealed that only 5 (all in the intention-based group) of the 32 participants recognized the correct action-effect mapping among four alternatives. The four alternatives they had to choose from were (1) left keypress = male voice; (2) left keypress = female voice; 3 = right keypress = male voice; 4 = right keypress = female voice.

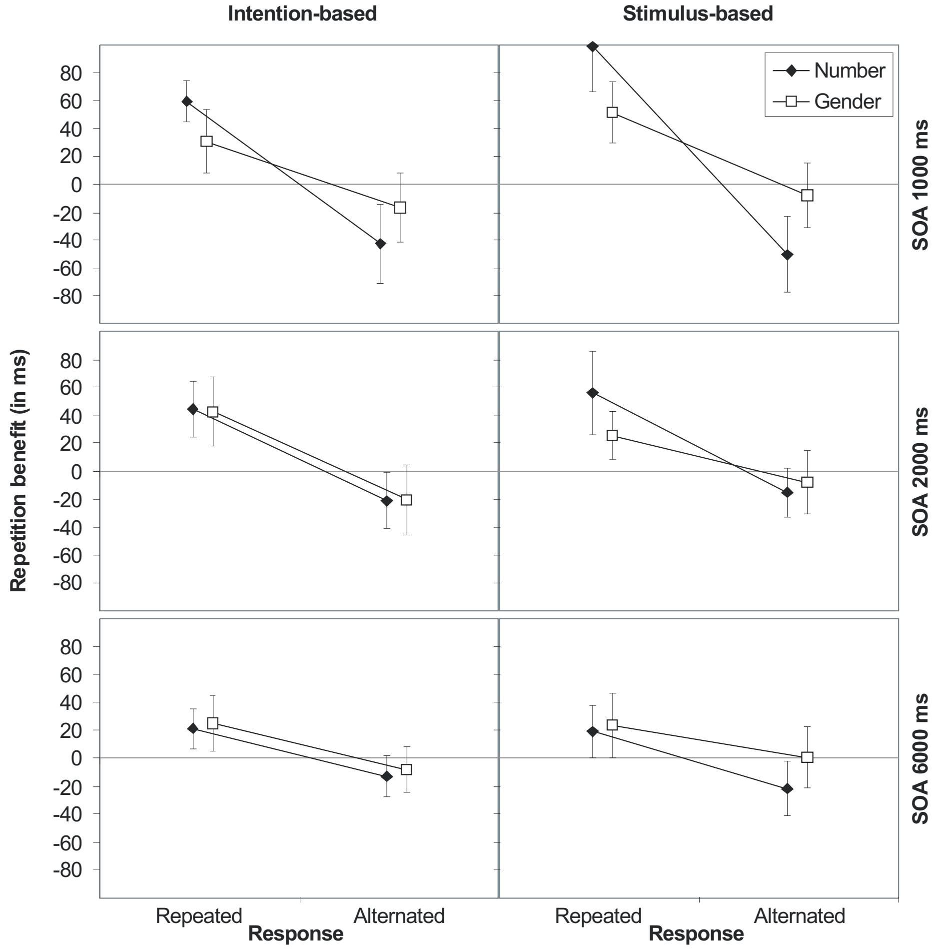

FIGURE 3 | Stimulus (S1–S2) repetition benefits ($RT_{alternation}$-$RT_{repetition}$) in Experiment 2 for stimulus features number and gender for intention-based (left panels) and stimulus-based group (right panels) as a function of response relation (R1–R2 repetition or alternation) and stimulus-onset asynchrony (SOA 1000, 2000, and 6000 ms, from top to bottom). Error bars represent the 95% confidence interval of individual means. If error bars do not cross the midline, the repetition benefit (or cost) is significantly different from zero ($p < 0.05$).

number feature as well as the irrelevant but contingent gender feature) is separately bound to the action. Second, in addition to these single feature bindings, there is also a binding between the action and a compound of both effect features.

More importantly, the action mode did not modify the bindings' strength or durability even under action-effect contingency. We also reran the ANOVA on RTs with the additional factor half of the experiment (first half vs. second half) to test whether the action mode modifies bindings only after some experience with the contingent action-effect mapping. This ANOVA also did not provide any evidence for an effect of the action mode (four-way interaction of Experiment half, Response, Gender, and Action mode, $p > 0.78$).

To sum up, the outcome of Experiment 2 failed to find an influence of the action mode on bindings between actions and their ensuing effect features, even though action and effect (gender) were contingent across the experiment. In light of previous studies showing that ideomotor learning can be affected by the action mode (Herwig et al., 2007), this finding is more in line with the non-dependence hypothesis than with the predictions derived from the weak dependence hypothesis of binding and learning. This is because the latter hypothesis assumes that binding and learning are related under action-effect contingency and thus should be influenced by the same factors.

However, two caveats impinging on the present data has to be taken into account before these results can be taken as evidence that ideomotor learning is more ore less independent of short-term action-effect bindings. First, intermingling effect-producing actions (subtask one) with choice responses to stimuli (subtask two) in Experiment 2 might have interfered with ideomotor learning. Second, up to now, we investigated binding (current study) and learning (Herwig et al., 2007; Herwig and Waszak, 2009; Herwig and Horstmann, 2011) in different experiments using different experimental designs.

EXPERIMENT 3

Experiment 3 was conducted to deal with these two caveats. To this end, we assessed both, bindings during an acquisition phase and ideomotor learning in a subsequent test phase. The acquisition phase was modeled after Experiment 2 so that once again the gender feature of S1 depended on R1. (We call the first part of the experiment acquisition phase, because in participants should acquire long-term memory traces. However, at the same time the acquisition phase served to test for temporary bindings, just as in Experiments 1 and 2.) In the additional test phase, participants were instructed to respond to the number feature of new stimuli (the English numbers "four" or "five") either with a left or a right key press. Importantly, the new stimuli were spoken either with a male or a female voice. If ideomotor learning occurs, than one would expect to find facilitation if the instructed stimulus feature number calls for the same response to which the task irrelevant feature gender is associated (compatible trials). In contrast, interference should occur if number and gender call for different responses (incompatible trials).

MATERIALS AND METHODS

Thirty-two adults (mean age 25.1 years) who fulfilled the same criteria as those in the previous experiments participated in this single session experiment of about 1 h. The experiment was divided in an acquisition and a test phase. The method used during acquisition was the same as in Experiment 2, with the only exception that there was no SOA manipulation and S2 always appeared 1000 ms after the onset of S1. The acquisition phase consisted of 2 blocks of 96 randomly ordered trials. Half of the participants executed R1 in an intention-based way, whereas the other half executed R1 in a stimulus-based way. After completing the acquisition phase, participants received an on-screen instruction of the required stimulus-response mapping for the test phase. In each test trial one out of four possible new stimuli ("four" or "five" vocalized by a male or a female voice) was presented. Half of the participants were instructed to respond to the number "four" with a left key press and to the number "five" with a right key press, whereas this mapping was reversed for the other half of participants. The next trial started 2000 ms after the response. The test phase comprised 200 randomly ordered trials (100 compatible and 100 incompatible trials).

RESULTS

Acquisition phase

The Appendix presents the results of subtask 1 as well as the means (see **Table A5** in Appendix) obtained for subtask 2. After excluding trials in which R2 was anticipated or omitted (0.1%), an ANOVA on R2 data was performed on error rates and error-free RTs with the between subjects factor Action mode (intention-based vs. stimulus-based) and the within subjects factors Response (repetition vs. alternation), Number (repetition vs. alternation), and Gender (repetition vs. alternation).

The RTs produced six reliable effects. Once again, action-effect bindings were not modified by the action mode (Fs < 1, ps > 0.441). Beside the main effect of gender [$F(1,30) = 7.80$, $p = 0.009$, $\eta_p^2 = 0.21$], indicating faster response if gender was repeated than alternated (496 and 511 ms, respectively), there was an interaction of number with action mode [$F(1,30) = 5.67$, $p = 0.024$, $\eta_p^2 = 0.16$]. This interaction was due to faster responses if number was repeated in the stimulus-based group (477 and 491 ms), whereas responses were slower if number was repeated in the intention-based group (530 and 516 ms). Of importance, all three binary interactions between response, number, and gender reached statistically significance. That is the interaction of number and gender [$F(1,30) = 41.32$, $p < 0.001$ $\eta_p^2 = 0.58$] indicated stimulus feature binding, whereas the interactions of response and number [$F(1,30) = 104.67$, $p < 0.001$, $\eta_p^2 = 0.78$] and response and gender [$F(1,30) = 5.68$, $p = 0.024$, $\eta_p^2 = 0.16$] indicated action-effect bindings. As depicted in **Figure 4**, repeating stimulus number or gender produce a benefit only if the response is repeated, whereas this benefit turns into in alternation benefit (as concerns the number feature) or vanishes (as concerns the gender feature) if the response is alternated. The sixth reliable effect was a three-way interaction of response × number × gender [$F(1,30) = 4.47$, $p = 0.043$, $\eta_p^2 = 0.13$]. Separate ANOVAs revealed that the response-by-gender interaction was pronounced for complete repetitions and alternations of response and number ($p < 0.001$), whereas it was absent for partial repetitions ($p = 0.462$).

The error rates mirrored both action-effect bindings observed in the RTs. Importantly, action-effect bindings were not modified by the action mode. The response-by-number interaction [$F(1,30) = 17.42$, $p < 0.001$, $\eta_p^2 = 0.37$] as well as the response-by-gender interaction [$F(1,30) = 7.66$, $p = 0.010$, $\eta_p^2 = 0.20$] reached statistical significance. Repeating both the number and the response or alternating both (2.5 and 1.1%, respectively) decreased the error rate, whereas the error rate increased if only one, but not the other, was repeated (number repeated: 5.4%; response repeated: 6.1%). Likewise, complete repetitions or alternations of response and gender decreased the error rate (3.1 and 2.4%, respectively), whereas the error rate increased with partial repetitions (gender repeated: 4.0%; response repeated: 5.5%). The ANOVA of error rates yielded no further reliable effects.

Test phase

Error rates and error-free mean RTs of the test phase were analyzed by mixed ANOVAs as a function of the action mode during the acquisition phase (between subjects factor) and compatibility (within-subject factor). The analysis of RTs yielded a significant interaction of the action mode and compatibility [$F(1,30) = 5.41$, $p = 0.027$, $\eta_p^2 = 0.15$]. None of the main effects reached statistical significance (Fs < 1, $p < 0.430$). As shown in **Figure 5** and as revealed by separate t-test, participants responded significantly faster on compatible (482 ms) than incompatible trials (495 ms) in the intention-based group [$t(15) = -2.16$, $p = 0.047$, $d = 0.54$, two-tailed], whereas there was no compatibility effect for the stimulus-based group [$t(15) = 1.10$, $p = 0.287$, $d = 0.28$, two-tailed]. The ANOVA on error rates did not yield any reliable effect. Suffice it to say that errors did not counteract the RT data, and thus, a speed-accuracy trade-off can be excluded.

DISCUSSION

Experiment 3 perfectly replicated the finding that the action mode affects binding and learning differently: action-effect bindings were unaffected by the action mode (replicating Experiments 1 and 2), whereas ideomotor learning was observed for intention-based actions only (replicating Herwig et al., 2007; Herwig and

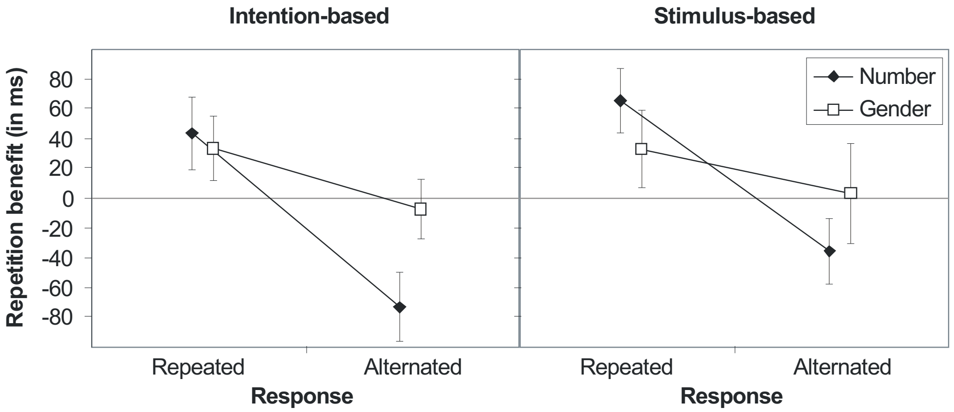

FIGURE 4 | Stimulus (S1–S2) repetition benefits ($RT_{alternation}$-$RT_{repetition}$) in Experiment 3 for stimulus features number and gender for intention-based (left panel) and stimulus-based group (right panel) as a function of response relation (R1–R2 repetition or alternation). Error bars represent the 95% confidence interval of individual means. If error bars do not cross the midline, the repetition benefit (or cost) is significantly different from zero ($p < 0.05$).

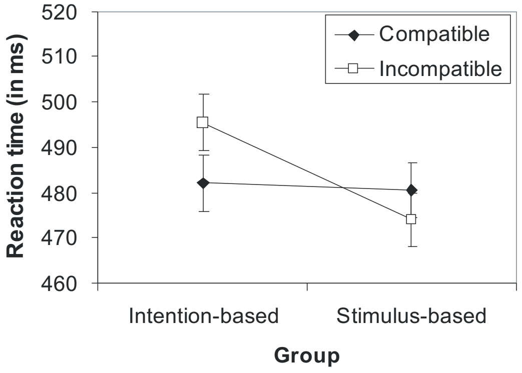

FIGURE 5 | Mean reaction times in the test phase of Experiment 3 as a function of group and compatibility. Error bars represent within-subject standard errors, calculated separately for each group (Loftus and Masson, 1994).

Waszak, 2009; Herwig and Horstmann, 2011). However, there are some points that need to be mentioned. First, unlike previous findings the compatibility effect for the intention-based group in Experiment 3 was rather small (13 ms instead of 30–70 ms). This might be due to the fact that in the present study the contingent effect feature was either task irrelevant in the test phase or had to compete against another task relevant effect feature during acquisition (or even a combination of both). Second, the interaction-effect seems to be driven more by interference than facilitation in the intention-based group as indicated by elevated reaction times to incompatible effects but comparable reaction times to compatible effects. Moreover, comparable to Experiment 2 action-effect bindings for gender were pronounced for complete repetitions and alternations of response and number, whereas, contrary to Experiment 2, they were not reliable for partial repetitions. This may indicate that there was a binding between the action and a compound of both effect features only. If correct, this interpretation would cast doubt on the notion that the results unequivocally support the non-dependence hypothesis. This is because we did not test for ideomotor learning of the action-compound association. However, Experiment 2 revealed that participants actually bind response and gender features even for number alterations. Thus, the difference between both experiments might be rather due to differences in power (576 vs. 200 trials in Experiment 2 and 3, respectively) than to qualitative differences underlying action-effect binding. To sum up, different effects of the action mode on short-term bindings and ideomotor learning were replicated within one experiment which rule out that intermingling effect-producing actions (subtask one) with choice responses to stimuli (subtask two) in Experiment 2 might have interfered with ideomotor learning.

GENERAL DISCUSSION

The present study aimed at addressing two research questions: are temporary bindings between action and effect features modulated by the action mode? How are temporary bindings and long-term ideomotor learning related?

Concerning the first research question, all experiments reported above showed strong and long-lasting (up to 6 s) action-effect bindings, not only for intention-based but also for stimulus-based actions. Importantly, we found no indication for the strength and durability of these bindings being dependent on the action mode. Hence, in contrast to ideomotor learning, temporary action-effect bindings are not modulated by the action mode. This finding is corroborated by the results of Janczyk et al. (2012). They used a different approach to assess the strength (but not durability) of short-term bindings following intention- and stimulus-based actions which required free- instead of forced choice responses in subtask 2 (Dutzi and Hommel, 2009). Accordingly, repetition rates were analyzed instead of RTs and error rates. With this slightly different experimental approach, Janczyk and colleagues also found immediate action-effect bindings for both types of actions. Interestingly, stimulus-based actions in their study even increased the bias to repeat the response if the stimulus was also repeated. However, they also pointed out that this observation does not necessarily imply stronger action-effect bindings for stimulus-based action.

Concerning our second research question about the relationship of binding and learning, the results of the present study are in line with the non-dependence hypothesis and suggest that binding and learning take place at different representational levels. Moreover, the results are in line with the study from Colzato et al. (2006). These authors manipulated the conjunction learning strength of a particular shape-color conjunction (i.e., their study dealt with bindings between stimulus features). They observed that bindings were not affected by previous learning (hence, Colzato et al., 2006 investigated the impact of learning on binding, whereas the present study focused on the impact of binding processes on learning). Colzato and colleagues concluded that learning is not a direct consequence of temporary bindings accumulating through Hebbian learning (i.e., neurons that fire together, wire together; Hebb, 1949).

It seems, thus, that different neural mechanisms mediate binding and learning. It has been proposed that temporary feature binding can be accomplished by selective synchronization of the firing patterns of feature coding neurons (e.g., von der Malsburg, 1999; Engel and Singer, 2001). Because temporal synchronization avoids combinatorial problems that are inherent in the principle of higher-order representations (e.g., cardinal cells, Barlow, 1972), it is well suited to flexibly represent a large number of frequently changing feature combinations. In contrast, learning seems to rely on strengthening of connections between neuron populations via long-term potentiation (e.g., Zalutsky and Nicoll, 1990; Bliss and Collingridge, 1993). Consequently, there has to be an additional process of consolidation that transforms transient bindings into durable memory traces (for discussions of consolidation processes in motor learning, see McGaugh, 2000; Robertson et al., 2004; Hotermans et al., 2006) since otherwise the brain is left without a trace when synchronization is gone (Wagner, 2001).

This brings us back to the question of why the action mode can affect the detection of action-effect associations that may be retrieved at least a couple of minutes after the acquisition. From a functional perspective, it seems to be crucial that consolidation is selective, i.e., irrelevant aspects of what binding processes integrate have to be filtered out while relevant aspects that might be needed on a later occasion have to be transformed into durable memory traces. Such filtering might be achieved by redundancy compression which is proposed to be regulated by the hippocampus (Gluck and Myers, 1993; Gluck et al., 2003). Redundancy compression assures that coincidental context information will come to be represented by a decreasing amount of neurons, whereas the relevant elements of the task remain well represented. In case of stimulus-based actions, the relevant element of the task is the stimulus-response rule specifying which motor routines action-relevant stimuli habitually require. In case of intention-based actions, in contrast, it is the action-effect rule that is used to control behavior (Pfister et al., 2010; Herwig and Horstmann, 2011). Accordingly, differences in ideomotor learning might be due to the (redundant) effect stimuli being compressed in stimulus-based actions but not in intention-based actions. A crucial question that needs to be addressed is whether short-term bindings are actually immune to the proposed filtering operations accompanying learning. Although to date there is no direct evidence validating or refuting this claim in the domain of action-effect learning and binding, there are already some tentative hints. These hints can be drawn from studies investigating stimulus-outcome learning and stimulus-response binding. For instance, Kruschke and Blair (2000) suggested that the learning phenomenon of conditioned blocking is driven by shifting attention away from the redundant information. Blocking refers to a situation in which stimulus-outcome learning is apparently reduced for a new cue accompanying an old cue that was already learned to perfectly predict an outcome. On the contrary, bindings seem to be not much affected by attention (Hommel, 2005) which suggests that bindings might be immune to redundancy compression. This claim would also fit to our observation, that even in the second half of Experiment 2 (i.e., when redundancy compression was probably at work) the action mode did not modify binding strength or durability.

With the refinement presented above the different-acquisition hypothesis put forward by Herwig et al. (2007) is also capable of explaining the divergent results of Wolfensteller and Ruge (2011) who observed action-effect learning following only 12 stimulus-based actions-effect episodes. Given their short acquisition phases it is not unlikely that during the test phase, the effect stimuli were not yet fully compressed leading to a small but reliable compatibility effect. However, based on the present data we cannot determine whether the action mode affects the acquisition of long-term action-effect associations prior to their retrieval (as suggested by the different-acquisition hypothesis of Herwig et al., 2007) or whether the action mode solely affects the application of these associations during the test phase (as suggested by the different-application hypothesis of Pfister et al., 2011). A satisfactory answer to this question will require future research directly testing whether intricate differences in the time course of adopting and switching action modes, as assumed by the different-application hypothesis, are actually responsible for the divergent results of Herwig et al. (2007) and Pfister et al. (2011). Obviously, the same holds true for alternative assumptions attributing the divergent results for instance to differences in the detection sensitivity of free choice and forced choice test phases which might be responsible for the detection or non-detection of weak action-effect associations following a stimulus-based acquisition.

In summary, the present experiments clearly show that transient action-effect bindings are unaffected by the action mode. At the same time, durable memory traces linking actions and their effects were detected only following intention-based but not stimulus-based actions. As a consequence the effect of the action mode on ideomotor learning cannot merely be a result of accumulated action-effect bindings. Instead, we suggest that only those episodic bindings are selectively perpetuated and retrieved that integrate action-relevant aspects of the processing event, that is, in case of intention-based actions, the link between action and ensuing effect.

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APPENDIX

EXPERIMENT 1

Subtask 1

The first response (R1) to the onset of the go signal was correctly carried out in stimulus-based trials in 330 ms, on average, and in intention-based trials in 375 ms, on average [$t(15) = 3.49$, $p = 0.003$, $d = 0.87$, two-tailed]. Errors of R1 in stimulus-based trials were rare (0.2%), as were anticipations and response omissions (stimulus-based: 0.4 and 1.4%, respectively; intention-based: 0.5 and 1.3%, respectively). The distribution of left-hand vs. right-hand key presses in intention-based trials was nearly equal (49.1 vs. 50.9%; average of absolute difference between left- and right-hand key presses = 5.9%) and provided a comparable amount of response repetitions and alternations (50.7 vs. 49.3%).

Subtask 2

Table A1 | Means of mean reaction times (RTs, in ms) and error rates (ER) for R2 in Experiment 1 as a function of the relationship between S1 and S2 and between R1 and R2 and stimulus-onset asynchrony for intention-based and stimulus-based trials.

Stimulus feature repeated	Response							
	Intention-based				Stimulus-based			
	Repeated		Alternated		Repeated		Alternated	
	RT	ER	RT	ER	RT	ER	RT	ER
SOA 1000 ms								
Neither	673	8.8	607	0.5	701	5.0	630	0.8
Number	669	1.0	679	3.8	683	1.1	706	3.5
Gender	684	6.5	613	0.8	708	5.8	641	0.8
Both	601	0.5	664	6.6	630	1.3	684	9.2
SOA 2000 ms								
Neither	658	4.8	608	0.6	673	5.3	615	0.3
Number	657	2.7	646	1.3	657	4.0	668	2.4
Gender	657	6.0	608	1.6	663	4.7	615	0.5
Both	598	2.4	635	4.8	593	0.8	636	4.5
SOA 6000 ms								
Neither	624	1.4	577	1.6	658	3.2	581	1.0
Number	602	2.4	615	1.9	629	1.3	630	2.1
Gender	607	3.8	564	0.8	622	3.0	578	1.3
Both	589	2.5	574	1.5	589	1.6	604	0.5

SOA, stimulus-onset asynchrony.

Table A2 | Results of analysis of variance on mean reaction times of correct responses (RT) and error rates (ER) for Experiment 1.

Effect	***df***	**RT_{R2}**			**ER_{R2}**		
		F	**η_p^2**	***p***	***F***	**η_p^2**	***p***
SOA	2,30	8.89	0.37	**	8.95	0.37	**
Am	1,15	0.46	0.03		0.64	0.04	
Res	1,15	11.09	0.43	**	8.42	0.36	*
Num	1,15	0.72	0.05		0.60	0.04	
Gen	1,15	15.23	0.50	**	4.54	0.23	*
SOA × Am	2,30	1.09	0.07		0.02	0.00	
SOA × Res	2,30	0.50	0.03		3.43	0.19	*
SOA × Num	2,30	1.06	0.07		0.07	0.00	
SOA × Gen	2,30	0.49	0.03		1.21	0.07	
Am × Res	1,15	0.03	0.00		0.79	0.05	
Am × Num	1,15	0.06	0.00		2.04	0.12	
Am × Gen	1,15	0.21	0.01		0.37	0.02	
Res × Num	1,15	46.54	0.76	**	35.18	0.70	**
Res × Gen	1,15	3.17	0.17		5.81	0.28	*
Num × Gen	1,15	16.66	0.53	**	2.13	0.12	
SOA × Am × Res	2,30	0.10	0.01		0.96	0.06	
SOA × Am × Num	2,30	0.11	0.01		3.21	0.12	
SOA × Am × Gen	2,30	0.20	0.01		3.97	0.21	*
SOA × Res × Num	2,30	2.96	0.17		22.11	0.60	**
SOA × Res × Gen	2,30	0.50	0.03		6.17	0.29	**
SOA × Num × Gen	2,30	3.24	0.18		4.12	0.22	*
Am × Res × Num	1,15	1.37	0.08		0.00	0.00	
Am × Res × Gen	1,15	0.30	0.02		0.68	0.04	
Am × Num × Gen	1,15	0.04	0.00		0.02	0.00	
Res × Num × Gen	1,15	1.01	0.06		7.14	0.32	*
SOA × Am × Res × Num	2,30	0.34	0.02		1.34	0.08	
SOA × Am × Res × Gen	2,30	1.50	0.09		0.42	0.03	
SOA × Am × Num × Gen	2,30	0.05	0.00		0.18	0.01	
SOA × Res × Num × Gen	2,30	2.10	0.12		1.57	0.09	
Am × Res × Num × Gen	1,15	0.35	0.12		0.04	0.00	
SOA × Am × Res × Num × Gen	2,30	0.25	0.02		2.57	0.15	

*SOA, stimulus-onset asynchrony; Am, action mode; Res, response; Num, number; Gen, gender. *$p < 0.05$, **$p < 0.01$.*

EXPERIMENT 2

Subtask 1

R1 was correctly initiated in the stimulus-based group in 269 ms, on average, and in the intention-based group in 331 ms, on average [$t(30) = 3.72$, $p < 0.001$, $d = 1.32$, two-tailed]. Errors of R1 in the stimulus-based group were rare (0.1%), as were anticipations and response omissions (stimulus-based: 0.6 and 0.7%, respectively; intention-based: 0.2 and 1.2%, respectively). The distribution of left-hand vs. right-hand key presses in intention-based trials was nearly equal (49.7 vs. 50.3%; average of absolute difference between left- and right-hand key presses = 2.9%) and provided a comparable amount of response repetitions and alternations.

Subtask 2

Table A3 | Means of mean reaction times (RTs, in ms) and error rates (ER) for R2 in Experiment 2 as a function of the relationship between S1 and S2 and between R1 and R2 and stimulus-onset asynchrony for intention-based and stimulus-based group.

Stimulus feature repeated	Response							
	Intention-based				Stimulus-based			
	Repeated		Alternated		Repeated		Alternated	
	RT	ER	RT	ER	RT	ER	RT	ER
SOA 1000 ms								
Neither	600	6.9	538	1.5	691	10.9	560	0.8
Number	573	3.8	566	4.2	602	1.6	614	3.7
Gender	602	6.1	540	0.0	648	4.5	572	0.3
Both	510	2.8	597	8.8	542	0.5	618	9.5
SOA 2000 ms								
Neither	581	3.8	518	0.3	621	2.9	562	0.3
Number	573	4.9	547	2.7	591	1.6	579	1.1
Gender	574	8.3	546	0.3	622	3.7	572	0.5
Both	494	2.4	560	6.8	540	0.5	585	7.3
SOA 6000 ms								
Neither	558	3.3	507	1.8	583	2.9	540	0.3
Number	543	3.1	518	2.1	583	1.8	564	1.6
Gender	539	2.6	514	1.0	579	2.1	542	1.0
Both	511	3.5	528	2.2	541	0.0	561	2.8

SOA, stimulus-onset asynchrony.

Table A4 | Results of analysis of variance on mean reaction time of correct responses (RT) and error rates (ER) for Experiment 2.

Effect	*df*	RT_{R2}			ER_{R2}		
		F	η_p^2	*p*	*F*	η_p^2	*p*
BETWEEN SUBJECTS							
Am	1, 30	1.40	0.04		1.60	0.05	
WITHIN SUBJECTS							
SOA	2, 60	14.57	0.33	**	17.95	0.37	**
Response (Res)	1,30	15.40	0.34	**	4.90	0.14	*
Number (Num)	1,30	9.29	0.24	**	1.98	0.06	
Gender (Gen)	1,30	19.25	0.39	**	3.76	0.11	
SOA × Am	2,60	0.08	0.00		1.34	0.04	
SOA × Res	2,60	0.23	0.01		0.11	0.00	
SOA × Num	2,60	3.47	0.10	*	0.79	0.03	
SOA × Gen	2,60	0.34	0.01		2.86	0.09	
Am × Res	1,30	0.62	0.02		2.21	0.07	
Am × Num	1,30	0.77	0.03		1.01	0.03	
Am × Gen	1,30	1.02	0.03		0.36	0.01	
Res × Num	1,30	90.43	0.75	**	53.15	0.64	**
Res × Gen	1,30	24.98	0.45	**	11.58	0.28	**
Num × Gen	1,30	15,81	0.35	**	7.79	0.20	**
SOA × Am × Res	2,60	0.93	0.03		1.30	0.04	
SOA × Am × Num	2,60	1.46	0.05		0.90	0.03	
SOA × Am × Gen	2,60	1.09	0.04		0.22	0.01	
SOA × Res × Num	2,60	26.94	0.47	**	25.44	0.46	**
SOA × Res × Gen	2,60	1.56	0.05		3.75	0.11	*
SOA × Num × Gen	2,60	1.65	0.05		7.30	0.20	**
Am × Res × Num	1,30	1.40	0.04		1.16	0.04	
Am × Res × Gen	1,30	0.30	0.01		3.72	0.11	
Am × Num × Gen	1,30	0.01	0.00		1.99	0.06	
Res × Num × Gen	1,30	16.61	0.36	**	17.19	0.36	**
SOA × Am × Res × Num	2,60	1.86	0.06		1.77	0.06	
SOA × Am × Res × Gen	2,60	1.13	0.04		0.23	0.01	
SOA × Am × Num × Gen	2,60	0.68	0.02		2.22	0.07	
SOA × Res × Num × Gen	2,60	0.61	0.02		5.85	0.16	**
Am × Res × Num × Gen	1,30	1.46	0.05		1.10	0.04	
SOA × Am × Res × Num × Gen	2,60	1.78	0.06		0.92	0.03	

SOA, stimulus-onset asynchrony; Am, action mode; Res, response; Num, number; Gen, gender. $^{*}p < 0.05$, $^{**}p < 0.01$.

EXPERIMENT 3

Acquisition phase – subtask 1

Participants of the stimulus-based group executed R1 correctly with a mean RT of 290 ms, whereas participants of the intention-based group executed freely selected actions with a mean RT of 334 ms [$t(30) = 1.94$, $p = 0.06$, two-tailed]. Errors of R1 in the stimulus-based group were rare (0.03%), as were anticipations and response omissions (stimulus-based: 0.4 and 0.8%, respectively; intention-based: 0.1 and 0.8% respectively). The distribution of left-hand vs. right-hand key presses in the intention-based group was nearly equal (49.4 vs. 50.6%, respectively; average of absolute difference between left- and right-hand key presses = 5.6%) which provided a comparable amount of response repetitions and alternations (49.6 vs. 50.4%).

Acquisition phase – subtask 2

Table A5 | Means of mean reaction times (RTs, in ms) and error rates (ER) for R2 in the acquisition phase of Experiment 3 as a function of the relationship between S1 and S2 and between R1 and R2 for intention-based and stimulus-based group.

Stimulus feature repeated	Response							
	Intention-based				Stimulus-based			
	Repeated		Alternated		Repeated		Alternated	
	RT	ER	RT	ER	RT	ER	RT	ER
Neither	543	8.5	482	0.5	514	6.1	452	0.8
Number	534	3.7	557	4.2	494	3.7	510	4.0
Gender	544	5.9	495	2.1	524	3.9	473	0.8
Both	471	1.7	557	8.2	423	1.1	482	5.0

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Instant attraction: immediate action-effect bindings occur for both, stimulus- and goal-driven actions

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Flexible behavior is only possible if contingencies between own actions and following environmental effects are acquired as quickly as possible; and recent findings indeed point toward an immediate formation of action-effect bindings already after a single coupling of an action and its effect. The present study explored whether these short-term bindings occur for both, stimulus- and goal-driven actions ("forced-choice actions" vs. "free-choice actions"). Two experiments confirmed that immediate action-effect bindings are formed for both types of actions and affect upcoming behavior. These findings support the view that action-effect binding is a ubiquitous phenomenon which occurs for any type of action.

Keywords: ideomotor theory, action planning, free-choice, forced-choice, action-effects, binding

INTRODUCTION

Human behavior other than unconditioned reflexes is characterized by enormous flexibility. In many situations, humans decide what to do and when to act to achieve their current goals. Such behavior has been investigated thoroughly by researchers of various disciplines and it has been distinguished on several grounds. For the present purpose, we focus on one specific distinction, i.e., that of stimulus- vs. goal-driven actions, and relate it to the central aspect of the present study: the question of whether or not similar action-effect associations are formed for these different kinds of actions. Here we focus explicitly on short-term associations of actions and effects, and our results suggest that short-term associations are formed for both kinds of actions (see also Herwig and Waszak, 2012, for similar conclusions with a different approach).

STIMULUS- AND GOAL-DRIVEN ACTIONS

In the following, we will distinguish actions by their more or less apparent cause. On the one hand, behavior can be exhibited as a response to environmental demands, such as when hitting the brake pedal upon the perception of a red traffic light. On the other hand, humans often act simply when they decide to do so, i.e., they exhibit instrumental behavior to pursue a self-determined goal. Importantly, this behavior may even be initiated in the absence of any explicit external stimulus demanding for it. Throughout this paper, we refer to these types of actions as "stimulus-" vs. "goal-driven," respectively[1].

This distinction highlights the criterion that determines whether an action was executed correctly or not: on the one hand it is the stimulus prompting a specific action; on the other hand it is the goal whose pursuit requires an instrumental action. Furthermore, it should be noted that this dichotomy rarely applies to realistic behavior. Rather, both aspects usually play a role in any given action – yet to a varying degree (see also Passingham et al., 2010). Hence, the dominant aspect must be used to pigeonhole the respective action.

In the laboratory, stimulus-driven actions are typically investigated with forced-choice tasks (Berlyne, 1957): a stimulus appears and entirely determines the appropriate response. Berlyne contrasted this task with free-choice tasks, where a stimulus simply prompts to choose between one of several possible response alternatives. These two types of tasks have been employed widely to investigate stimulus- and goal-driven actions (e.g., Waszak et al., 2005; Keller et al., 2006; Herwig et al., 2007; Pfister et al., 2010, 2011; Wolfensteller and Ruge, 2011), and the present experiments also draw on these methods.

THE ROLE OF ACTION-EFFECTS IN ACTION CONTROL

Numerous studies across the last years targeted the role of action-effects in action planning and/or execution. The term of action-effects encompasses any contingent sensory changes that are produced by the action. Regarding the conceptual distinction into stimulus- and goal-driven actions, the role of such action-effects was (and still is) subject to discussion (Herwig et al., 2007; Herwig and Waszak, 2009; Pfister et al., 2010, 2011; Herwig and Horstmann, 2011; Wolfensteller and Ruge, 2011). The theoretical background of this debate is mostly related to ideomotor theory – a general framework of human action control that we summarize

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[1] These types of behavior have, among others, been termed "stimulus-based" or "externally triggered" vs. "intention-based" or "self-initiated," respectively (e.g., Waszak et al., 2005; Herwig et al., 2007; Brass and Haggard, 2008; Passingham et al., 2010). However, if people show a particular behavior in response to a given stimulus (i.e., "stimulus-based"), the relevant association between stimulus and behavior has to be established beforehand. This, in turn requires intention as well. Exner (1879; cited in Hommel, 2000) described this kind of behavior as the "prepared reflex." Thus, stimulus-based actions clearly require intentions as well, at least when setting up the association between stimulus and response. We therefore use the labels stimulus- vs. goal-*driven* throughout this article.

in the following (Herbart, 1825; Harleß, 1861; James, 1890/1981; Hommel et al., 2001; for historical papers and reviews, see Stock and Stock, 2004; Shin et al., 2010; Pfister and Janczyk, 2012).

In a nutshell, ideomotor theory assumes that (1) actions are represented by their contingent sensory consequences, i.e., action-effects, and that (2) an action is selected and initiated by mentally anticipating these sensory consequences. These assumptions imply that there are stable and bidirectional associations of actions and their effects. For goal-driven actions, such long-term associations between actions and their contingent effects were demonstrated numerous times in the literature (e.g., Elsner and Hommel, 2001; Hommel et al., 2003; Rieger, 2004; Hoffmann et al., 2009). The respective experiments typically employed two distinct experimental phases. In the *acquisition phase*, participants performed freely chosen actions that were followed by contingent action-effects. For example, participants pressed one of two response keys at their choice and each key press reliably produced a low- or high-pitch tone effect (e.g., left key → low tone, right key → high tone). In the subsequent *test phase*, these tones were then presented as stimuli to probe the assumed action-effect association. For example, in forced-choice test phases, participants react to the effects either in an acquisition-compatible (i.e., low tone → left key, high tone → right key) or in the reversed mapping (i.e., high tone → left key, low tone → right key). Such studies consistently found reaction times (RTs) to be faster in the acquisition-compatible mapping than in the reversed mapping (the "non-reversal advantage"), and these results clearly indicate that action-effect associations were built up for the freely chosen actions in the acquisition phase.

As noted above, most studies in this design employed free-choice actions in the acquisition phase (but see Elsner and Hommel, 2004). A systematic comparison of both, free- and forced-choice actions was reported by Herwig et al. (2007). In this study, participants learned action-effect associations for either free- or forced-choice actions. A subsequent forced-choice test phase then probed for resulting action-effect associations. With a free-choice acquisition phase, they replicated the non-reversal advantage of previous studies (e.g., Elsner and Hommel, 2001). In contrast, for the forced-choice acquisition phase, this effect was absent (and in some conditions even numerically reversed). This finding was later shown not to depend on attentional factors (Herwig and Waszak, 2009) and to occur also for eye-movements as response modality (Herwig and Horstmann, 2011).

These findings were taken to suggest that action-effect associations are not built up for stimulus-driven actions. This conclusion, however, is at odds with several findings related to ideomotor theory. For instance, slight variations of the design of acquisition and test phase yielded reliable signs for action-effect learning in forced-choice tasks (Hommel, 1996; Elsner and Hommel, 2004; Pfister et al., 2011), already after very few pairings of actions and effects (Wolfensteller and Ruge, 2011). Furthermore (arbitrary) action-effects were shown to have a pronounced impact in a huge variety of entirely forced-choice tasks (e.g., Hommel, 1993; Ziessler, 1998; Kunde, 2001, 2003; Koch and Kunde, 2002; Rieger, 2007; Janczyk et al., 2009, 2012a; Hubbard et al., 2011).

THE PRESENT APPROACH: SHORT-TERM ACTION-EFFECT ASSOCIATIONS

In sum, the evidence whether or not associations of actions and their effects are acquired for stimulus-driven actions is somewhat mixed, yet with a trend toward a positive answer. So far, however, we have only dealt with long-term associations. On a shorter time-scale, features of a particular action (e.g., stimulus, response, and effect) are assumed to be bound into an event-file (Hommel, 1998; Hommel et al., 2001). Although there is evidence that such short-term associations are not necessarily the same as, or a precondition for long-term associations (Colzato et al., 2006), it is still important to know whether a putative difference between stimulus- and goal-driven actions is present in the short-term domain.

In the first study on such short-term action-effect associations, Dutzi and Hommel (2009) reasoned that a sufficiently co-activated (free-choice) response and its contingent effect should be integrated readily into an event-file (Hommel, 1998; Hommel et al., 2001). Encountering the effect again after a short period of time should thus prime the associated response (Hommel, 2007). This *response-repetition bias* was indeed found in four experiments. Thus, action-effects seem to be bound into event-files instantaneously (Dutzi and Hommel, 2009; see also Pfister et al., 2012, for the integration of effects that have been associated with a response on a long-term time scale). Additional evidence from a different paradigm suggests that stimuli occurring after a forced-choice response are similarly bound to the responses (Hommel, 2005, Experiment 2). As there are no direct comparisons in this context, it is unclear whether short-term associations occur similarly for both types of actions. To this end we (1) replicate earlier findings for free-choice responses (Dutzi and Hommel, 2009) and (2) show similar associations for forced-choice actions.

EXPERIMENT 1

Participants performed a task in which each trial consisted of two stages. A first response produced one of two auditory action-effects in a non-predictable manner. Importantly, this response was either a free-choice action (Experiment 1a, replicating the paradigm of Dutzi and Hommel, 2009) or a forced-choice action (Experiment 1b). Shortly thereafter, the same or the other tone was presented, prompting a free-choice response. For the free-choice actions of Experiment 1a, we expected to replicate the response-repetition bias when the effect tone was repeated. If the same binding mechanism operates for forced-choice actions, a similar bias should be observed in Experiment 1b. In contrast, this bias should be absent if action-effect binding does not take place under these circumstances.

METHOD

Participants

Seventeen participants performed in Experiment 1a (mean age = 27.8 years, 12 female), and another 16 participants performed in Experiment 1b (mean age = 21.6 years, 13 females). Participants were undergraduate students from the University of Würzburg and were naïve regarding the hypotheses underlying this experiment.

Apparatus and stimuli

Visual stimuli were presented in white against a black background. The imperative stimulus in the first stage of each trial was a string of 13 centrally presented asterisks in Experiment 1a (see Dutzi and Hommel, 2009) and a small white square presented below and to the left or right of a fixation cross in Experiment 1b (see Herwig et al., 2007; Wolfensteller and Ruge, 2011). Tones were 50 ms sinusoidal tones of either 300 or 900 Hz presented via headphones. Responses were given via the left and right control key of a standard computer keyboard using the index-finger of the left or right hand.

Procedure

The trial procedure is illustrated in **Figure 1**. In Experiment 1a, each trial began with the presentation of the asterisks for 300 ms (Stimulus 1). Participants were to freely choose from both responses at leisure (Response 1). This key press triggered one of the two tones at random (Effect). After 1000 ms, either a second tone occurred (Stimulus 2; go trials) or not (no-go trials). In go trials, the second tone was either the previous effect tone or the alternative tone. Participants were then to freely choose one of the response keys within a time window of 1500 ms. Following late responses or responses in no-go trials, visual error feedback was provided for 1000 ms, and the next trial started after an inter-trial interval (ITI) of 3000 ms. The trial procedure of Experiment 1b was identical except for the presentation of Stimulus 1. Here, each trial started with a fixation cross for 500 ms. Then, Stimulus 1 was presented to the left or right of fixation (300 ms) prompting a speeded response with the corresponding key. Wrong responses to Stimulus 1 prompted an error feedback (1000 ms) and the trial was canceled afterward. For correct responses, the trial continued just as in Experiment 1a.

Participants completed three experimental blocks with 64 trials each. Of these trials, 16 trials were no-go trials. In 24 trials the effect tone was repeated as Stimulus 2 (congruent go trials), and in the remaining 24 trials the other tone was played as Stimulus 2 (incongruent go trials). Ten practice trials were completed prior to the experimental blocks. Participants were tested individually in a single session of about 20 min, and they received written instructions prior to the experiment. For free-choices, participants were instructed to decide on the response as spontaneously as possible and not to pursue any specific strategies. Furthermore, they were encouraged to produce both response alternatives about equally often throughout the experiment.

RESULTS

The main dependent variable was the proportion of go trials with response-repetitions (*repetition rate*; see **Figure 2**, left panel)[2]. For Experiment 1a, we excluded go trials with late responses (0.9%) and for Experiment 1b we excluded trials with wrong responses to Stimulus 1 (1.0%) and go trials with late responses (0.8%). We then compared the repetition rates for congruent and incongruent go trials with separate one-tailed t-tests. Erroneous responses in no-go trials were given in 3.9 and 2.1% of the trials in the free- and forced-choice task, respectively.

For *Experiment 1a (free-choice)*, we excluded two participants from the analysis because they had chosen only a single key as Response 1 in 91.6 and 99.5% of the trials, respectively. The remaining 15 participants chose both keys about equally often for Response 1 [48.8 vs. 51.2%, $\chi^2(1) = 1.61$, $p = 0.205$].

The results are visualized in **Figure 2** (left panel). Crucially, the repetition rate was significantly higher in congruent as compared to incongruent trials, $t(14) = 2.58$, $p = 0.011$, $d = 0.94$. The same results emerged for *Experiment 1b (forced-choice)*, $t(15) = 3.29$, $p = 0.002$, $d = 1.16$.

To test for differences between free- and forced-choice actions, we performed an additional *between experiment analysis* by means

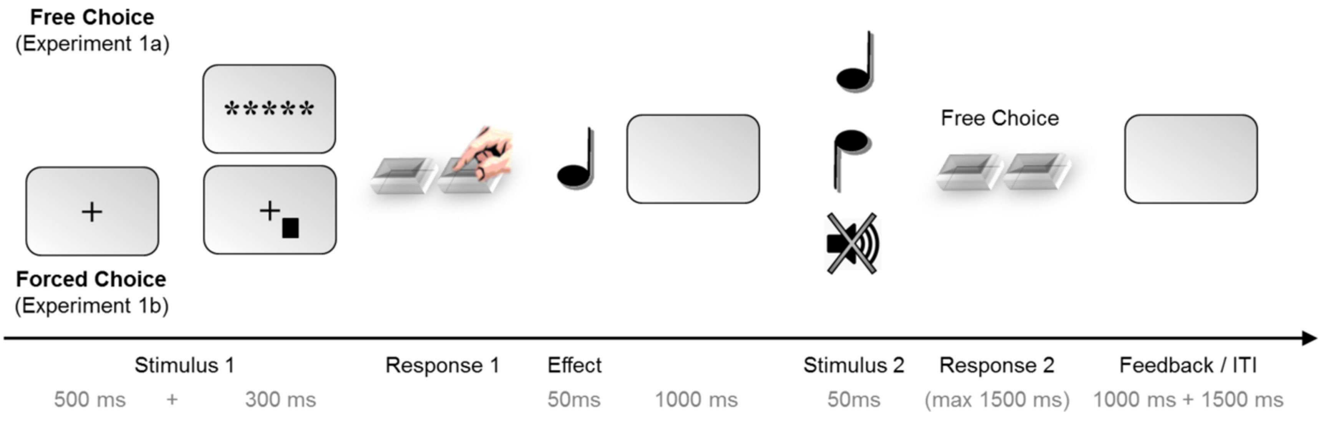

FIGURE 1 | Trial structure for Experiments 1a and 1b, where the task (free-choice vs. forced-choice) was manipulated between-subjects. Each trial consisted of two stages: first, a free- or forced-choice response randomly produced one of two possible effect tones. Then, a second tone appeared and prompted the participants to choose a second response; this tone was either the previous effect tone or the alternative tone. In 25% of the trials, no tone appeared as Stimulus 2, indicating a no-go trial. Experiment 2 employed the same trial structure but the task (free- vs. forced-choice as Response 1) was now varied within-subjects.

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[2] One could in principle also analyze RTs as the dependent variable. However, it is rather difficult to interpret any pattern that might emerge from such an analysis, rendering RTs an inappropriate measure in this context, as has been argued in detail by Dutzi and Hommel, 2009, Footnote 1). Exploratory analyses of the present data yielded no systematic pattern across both experiments. Descriptive RTs to Stimulus 2 are summarized in the Appendix.

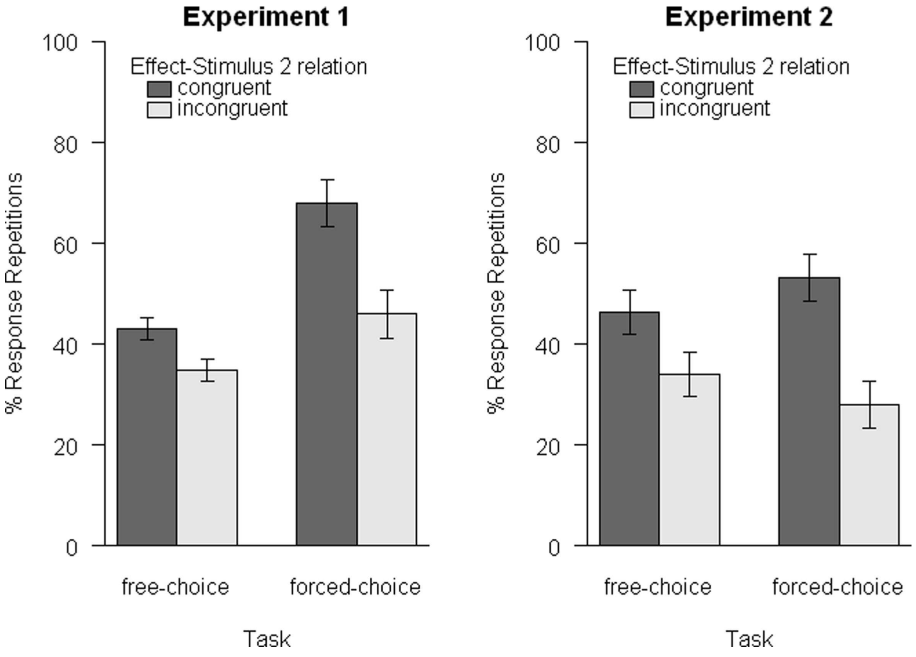

FIGURE 2 | Mean response-repetition rates as a function of task (free-choice vs. forced-choice) and relation of Effect and Stimulus 2 (congruent vs. incongruent). A repetition bias for congruent trials emerged consistently for both tasks in both experiments. Error bars are within-subjects standard errors, calculated separately for each comparison of congruent and incongruent trials (Loftus and Masson, 1994).

of a 2 × 2 ANOVA with trial-type (congruent vs. incongruent) as a within-subjects factor and experiment (1a vs. 1b) as a between-subjects factor. This analysis yielded a significant main effect of trial-type, $F(1, 29) = 15.82$, $p < 0.001$, $\eta_p^2 = 0.35$, confirming the higher repetition rate for congruent as compared to incongruent trials. Secondly, a significant main effect of experiment, $F(1, 29) = 6.52$, $p = 0.016$, $\eta_p^2 = 0.18$, indicated a generally higher repetition rate in Experiment 1b. Finally, the interaction of both factors approached significance, $F(1, 29) = 3.48$, $p = 0.072$, $\eta_p^2 = 0.11$, suggesting a larger effect of trial-type in Experiment 1b than in Experiment 1a.

DISCUSSION

The purpose of Experiment 1 was to replicate and extend previous findings of immediate action-effect binding and its impact on subsequent free response choices (Dutzi and Hommel, 2009). To this end, a free-choice response (Experiment 1a) or a forced-choice response (Experiment 1b) produced an auditory action-effect. Shortly after this action-effect, a second tone prompted a free-choice response. This tone was either the previous action-effect or another tone. As predicted, tone repetitions biased participants to repeat the previous response. For Experiment 1a (free-choice), mean repetition rates were in the range reported earlier by Dutzi and Hommel (2009). For Experiment 1b (forced-choice), overall repetition rates were higher and – at least numerically – the bias was even larger than in Experiment 1a. Therefore, and because of the between-subject manipulation in Experiment 1, we conducted Experiment 2 where both, free- and forced-choice responses were implemented within-subjects.

EXPERIMENT 2

In Experiment 1, we replicated the response-repetition bias reported by Dutzi and Hommel (2009), when participants performed their first response in a free-choice task. More importantly, we found the same pattern when this response resulted from a forced-choice situation. This indicates that short-term action-effect binding takes also place in this situation and is not restricted to free-choice actions. Somewhat unexpected, this bias was numerically even larger for the forced- compared to the free-choice situation. In Experiment 2 we strived to replicate this finding using a within-subjects design.

METHOD

Twelve new participants from the city of Würzburg (mean age = 25.0 years; 8 females) performed in this experiment. Procedural details were as in Experiment 1 with one exception: all participants performed in both, the free- and the forced-choice variant. Accordingly, task (free-choice vs. forced-choice) was introduced as a second repeated measure. The order of these tasks was counterbalanced across participants.

RESULTS

In the free-choice part, 1.0% of the go trials were excluded because of late responses. In the forced-choice part, trials with errors in response to Stimulus 1 were excluded (1.8%) as well as late responses to Stimulus 2 (2.0%). Participants chose both keys about equally often for Response 1 in the free-choice task [each 50.0%, $\chi^2(1) < 0.01, p = 0.967$]. Mean response-repetitions in correct trials were submitted to an ANOVA with trial-type (congruent vs. incongruent) and task (free-choice vs. forced-choice) as repeated measures. Results are illustrated in **Figure 2** (right panel). Erroneous responses in no-go trials were given in 5.6 and 2.3% of the trials in the free- and forced-choice task, respectively.

Response-repetitions were again significantly more likely for congruent than for incongruent trials, $F(1, 11) = 10.15$, $p = 0.009$, $\eta_p^2 = 0.48$. Importantly, this main effect was qualified by the significant interaction of trial-type and task, $F(1, 11) = 8.72$, $p = 0.013$, $\eta_p^2 = 0.44$. The main effect of task was not significant, $F(1, 11) = 0.02$, $p = 0.880$, $\eta_p^2 < 0.01$. One-tailed t-tests showed significantly more repetitions in congruent trials for both, the free-choice task, $t(11) = 1.99$, $p = 0.036$, $d = 0.81$, and the forced-choice task, $t(11) = 3.92$, $p = 0.001$, $d = 1.60$. Additionally, the size of these biases was strongly correlated across participants, $r(12) = 0.75$, $p = 0.005$.

DISCUSSION

The results of Experiment 2 replicated the pattern already observed in Experiment 1. A reliable response-repetition bias emerged for congruent trials for both tasks, and was again larger in the forced-choice task than in the free-choice task. Thus, again short-term action-effect associations were observed for both, free- and forced-choice actions. The positive correlation of the biases also indicates that the associations are built up to a similar degree not only on the group level, but also on the level of the individual.

GENERAL DISCUSSION

In the present study we investigated whether stimulus- and goal-driven actions differ with regard to short-term action-effect associations. In general, there is good evidence that stimuli and their corresponding responses are integrated into event-files (Hommel, 1998; Hommel et al., 2001). Similar integrations have previously been shown for the effects of goal-driven (free-choice) actions (Dutzi and Hommel, 2009), but only indirectly for stimulus-driven actions (Hommel, 2005, Experiment 2).

In the two present experiments, participants performed one of two manual responses, either as determined by an imperative stimulus (forced-choice task; stimulus-driven actions) or freely chosen by the participant (free-choice task; goal-driven actions). Each response randomly triggered one of two tones as an action-effect. Briefly thereafter, the same, the other, or no tone was played. In case of a second tone, participants freely chose between both responses again. Participants tended to repeat the first response more often when the effect tone was repeated to signal the (second) response than when it was not. Thus, short-term action-effect associations, i.e., the integration of actions and their effects into an event-file (Hommel, 1998), were evident in our experiments. Crucially, this was true for both, goal-driven, free-choice actions (Dutzi and Hommel, 2009) and stimulus-driven, forced-choice actions. Indeed, the observed bias was even larger for the stimulus-based actions.

SHORT-TERM AND LONG-TERM ACTION-EFFECT ASSOCIATIONS

These results suggest that actions and their effects are integrated regardless of whether the action is classified as stimulus- or goal-driven. Herwig and Waszak (2012) tackled a similar question with a slightly different experimental approach. They also employed two responses in each trial with the first response being either free- or forced-choice. Again, this response produced an action-effect and a second stimulus prompted the second response. This second stimulus could share features with the previous effect or not. In contrast to the present setup, however, the second response was forced-choice throughout. Accordingly, RTs and percentage errors were analyzed instead of repetition rates. Still, their results mostly converge with the present findings. However, in contrast to the present results, Herwig and Waszak did not find any hints toward a larger bias for forced-choice actions and they accordingly conclude that short-term binding results equally for free- as well as forced-choice actions.

In light of these differences, we are cautious about drawing definite conclusions from the observed differences between the present free- and forced-choice tasks in terms of stronger binding for forced-choice actions. Instead, passing control to the environment in a forced-choice task might simply have rendered the participants more susceptible for other environmental influences such as response tendencies invoked by the perception of previous action-effects. Thus, the more pronounced effects for the present forced-choice tasks need not necessarily imply a genuinely stronger binding. Additionally, this case required continuous task switches from forced- to free-choices and back. This may have increased attention to task-related stimuli and might have led to a stronger effect in the forced-choice condition, too. Regardless of this interesting difference, these results converge in the notion that short-term action-effect associations are built for both, stimulus- and goal-driven actions. Thus, on this level there is little reason for the assumption of profound and qualitative differences between these actions.

However, our data do not speak toward differences in long-term associations. Colzato et al. (2006) preferred the interpretation that short-term and long-term associations are not necessarily dependent on each other, although the existence of stable long-term memory representations appears to affect the time-course of short-term associations over a series of trials. Herwig and Waszak, 2012, Experiment 3) complemented their approach by testing additionally for long-term associations that may have evolved from an acquisition phase, where their participants showed reliable short-term associations. In line with their previous findings (Herwig et al., 2007; Herwig and Waszak, 2009; Herwig and Horstmann, 2011), a reliable long-term association of actions and corresponding effects was only found for those participants who performed the free-choice version of their acquisition task. Herwig and Waszak (2012) suggest that redundant information might be represented less and less strongly during repeated occurrences, and eventually is thus not integrated into long-term associations. Thus, in the case of forced-choice actions, action-effects are more and more identified as redundant; consequently their representation should diminish and eventually do not leave a long-term trace. Herwig and Waszak also suggest that such additional assumptions can explain the results of Wolfensteller and Ruge (2011). The latter authors observed reliable "long-term" associations after only a few forced-choice acquisition trials. According to Herwig's and Waszak's reasoning then, the brevity of the acquisition phases simply may not have allowed to firmly identifying the redundancy of the effects. As a consequence, they were still well represented and integrated with the responses – as became evident in their test phases.

Nevertheless, this hypothesis cannot explain the whole range of findings. It is difficult to see why free-choice test phases reliably reveal long-term action-effect associations, even when acquired during a long acquisition phase (Kühn et al., 2009, Experiment 3; Pfister et al., 2011). Furthermore, it cannot explain why slight variations of the experimental design (e.g., including more response alternatives) do yield reliable effects also for purely forced-choice settings (Hommel, 1996; Elsner and Hommel, 2004).

A different perspective that might account for these findings is based on the *intentional weighting* of feature codes (Hommel, 1996). According to this view, any feature code that covaries with the response is represented automatically. Intentional control over the available codes is possible by assigning different weights to the represented features. This account is in line with a variety of findings relating to perception and action (Memelink and Hommel, in press). The absence of experimental evidence for action-effect associations in forced-choice test phases following prolonged forced-choice acquisition phases (Herwig et al., 2007; Herwig and Waszak, 2009) might thus indicate that small weights are assigned to the represented action-effects even though an action-effect association does exist. This process might be supported by the fact that action-effects were explicitly rendered task-irrelevant in these studies.

In this view, free-choice (i.e., goal-driven) actions simply increase the tendency to use action-effects for action control by assigning a stronger weight to the relevant codes (Pfister et al., 2010, 2011). Furthermore, free response choices are not the only way to increase the weight that is assigned to action-effects. Other relevant factors might be instructions (Hommel, 1993), task-relevance (Ansorge, 2002), or saliency of action-effects that are relevant for the task at hand (e.g., Kunde et al., 2007, 2012; Janczyk et al., 2012a,b). Most importantly, however, this account does not assume qualitative differences between stimulus- and goal-driven actions regarding the underlying learning mechanisms. This implication is further supported by the present results which suggest similar mechanisms to mediate short-term bindings for both, stimulus- and goal-driven actions.

CONCLUSION

The present study investigated short-term associations between actions and their following effects. More precisely, we addressed the formation of such short-term bindings for stimulus-driven (forced-choice) and goal-driven (free-choice) actions. Results indicate that ensuing action-effects are readily associated to both types of actions. On a broader scale, these findings are also in line with the common definition of “actions” for which “goal-directedness” is a key element, independent of the more or less apparent cause/motivation of an action.

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APPENDIX

Table A1 | Mean RTs (ms) to Stimulus 2 from Experiments 1 and 2 as a function of Effect – Stimulus 2 relation.

Effect – S2 relation	Experiment 1		Experiment 2	
	Free-choice	Forced-choice	Free-choice	Forced-choice
Congruent	530	530	525	557
Incongruent	512	525	493	568

Note that task (free- vs. forced-choice) was varied between-subjects in Experiment 1 and thus refers to Experiment 1a and 1b, respectively. RTs are based on correct trials only and RTs deviating from the respective cell mean by more than 3SDs were considered outliers.

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On the influence of reward on action-effect binding

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Ideomotor theory states that the formation of anticipatory representations about the perceptual consequences of an action [i.e., *action-effect (A-E) binding*] provides the functional basis of voluntary action control. A host of studies have demonstrated that A-E binding occurs fast and effortlessly, yet little is known about cognitive and affective factors that influence this learning process. In the present study, we sought to test whether the motivational value of an action modulates the acquisition of A-E associations. To this end, we linked specific actions with monetary incentives during the acquisition of novel A-E mappings. In a subsequent test phase, the degree of binding was assessed by presenting the former effect stimuli as task-irrelevant response primes in a forced-choice response task, absent reward. Binding, as indexed by response priming through the former action-effects, was only found for reward-related A-E mappings. Moreover, the degree to which reward associations modulated the binding strength was predicted by individuals' trait sensitivity to reward. These observations indicate that the association of actions and their immediate outcomes depends on the motivational value of the action during learning, as well as on the motivational disposition of the individual. On a larger scale, these findings also highlight the link between ideomotor theories and reinforcement-learning theories, providing an interesting perspective for future research on anticipatory regulation of behavior.

Keywords: reward, motivation, ideomotor theory, action-effects, inter-individual differences

INTRODUCTION

The vast majority of actions we perform in everyday life are directed at producing a particular outcome in the environment. For instance, we may press a light switch because we want to illuminate the room, or boil water because we want to drink a cup of tea. In doing so, we effortlessly select actions that are appropriate for achieving a desired outcome. Accordingly, the ability to associate actions with their immediate and long-term consequences is a key mechanism for learning, and thus for flexible and adaptive control of behavior.

Ideomotor theory (IMT) constitutes the prevailing theoretical approach toward the role of effect anticipation in action control. The earliest versions of IMT can be traced back to the nineteenth century (Lotze, 1852; Harleß, 1861; James, 1890), and these ideas have undergone a renaissance in experimental psychology over the last decades (for recent reviews see Nattkemper et al., 2010; Shin et al., 2010; Pfister and Janczyk, 2012). In a nutshell, the core assumption of IMT is that actions and their perceptual outcomes are cognitively bound together. Performing an action (A) that produces a particular environmental effect (E) is assumed to lead to the formation of a common representation of the two events ("A-E binding"). Importantly, these bindings are conceived as bi-directional. Thus, internally anticipating a desired environmental effect directly activates the associated motor program, thereby promoting goal-directed behavior.

In the laboratory, this cardinal assumption of IMT is commonly assessed with so-called *induction paradigms* (Elsner and Hommel, 2001). Typically, participants first complete an acquisition phase to establish a novel association between simple actions and arbitrary sensory effects. For instance, participants may perform left- and right-hand button presses, each of which is contingently followed by a specific stimulus (e.g., left button → low-pitch tone, right button → high-pitch tone). In a subsequent test phase, the same responses are performed in a speeded forced-choice response task while the learned action-effects are presented as primes. Presupposing that participants have acquired bi-directional A-E bindings in the learning phase, the perception of a learned action-effect should directly activate the associated response, causing facilitation when the prime was previously the effect of the required response (compatible primes) and interference when the prime was previously the effect of a different response (incompatible primes). Over the last decade, this prediction has been confirmed in numerous studies employing a variety of response and effect modalities (e.g., Hommel, 1996; Elsner and Hommel, 2001; Beckers et al., 2002; Ziessler and Nattkemper, 2002; Kunde, 2004; Ziessler et al., 2004; Herwig et al., 2007).

Interestingly, once A-E knowledge has been acquired, the priming of a response via the activation of an associated perceptual representation seems to occur highly automatically, without requiring further cognitive mediation. For instance, it also occurs in conditions in which effect primes are entirely task-irrelevant (Hommel, 1996) and even when the primes are presented subliminally so that they cannot be consciously perceived (Kunde, 2004). On the other hand, relatively little is known about the factors that contribute to the *acquisition* of this kind of knowledge. Elsner and Hommel (2004) have investigated situational determinants of A-E binding, demonstrating that it critically depends on the temporal contiguity and the probabilistic contingency between actions and their effects. In other words, A-E binding diminishes with increasing delays between the two events, as well as with reduced predictability of a unique effect. Other studies have shown that cognitive factors such as the internal selection of an action may influence the strength of A-E binding during the acquisition phase (Ziessler et al., 2004; Herwig et al., 2007; Herwig and Waszak, 2009; Kühn et al., 2009; but see Pfister et al., 2011).

Here, we wanted to examine whether the acquisition of A-E bindings can moreover be modulated by factors related to the *motivational value* of an action. It is well established that monetary incentives can be used to modulate a wide range of human cognitive functions including visual discrimination, conflict resolution, and long-term memory encoding (Wittmann et al., 2005; Engelmann and Pessoa, 2007; Padmala and Pessoa, 2008; Krebs et al., 2012). In these paradigms, reward is typically associated with specific trial types, stimulus types, or entire task blocks, in such a way that the participant is rewarded for correct and/or fast executions of the required response. As such, these stimulus-reward associations are in most cases compatible with the task goal, which generally results in a facilitation of response execution. However, we recently showed that reward associations can also have detrimental effects upon response execution if they trigger specific response tendencies that are incompatible with the task goal (Krebs et al., 2010, 2011). Another line of research has demonstrated that not only perceptual but also affective features of outcomes are bound to the actions that produce them. Specifically, in a study by Beckers et al. (2002), one of two responses in a free-choice task was always associated with an electrocutaneous stimulation (negative valence), while the other was not (positive valence). In the subsequent test phase, responses to target words were facilitated if their semantic valence was compatible with the effect previously associated with this response (Beckers et al., 2002). Similar effects of "affective compatibility" have been observed in a recent study by Eder et al. (2012). The authors showed that preparing a response to a picture of positive or negative valence interfered with the actual execution of a subsequent response to a word of similar valence. This suggests that action planning involves the activation of associated affective features, making them less accessible to other responses that share this feature.

While these findings highlight that affective codes are a part of the mental representation of an action, we wanted to further investigate whether motivational values of an action would modulate the degree of A-E binding – a notion which has not yet been tested. To this end, we associated two out of four actions with monetary incentives during the acquisition phase of an induction paradigm. In the subsequent test phase, we assessed the influence of compatible and incompatible effect primes, which could be related to former reward or to no reward, in the absence of any further monetary reinforcement. Considering previous evidence that affective feedback stimuli can strengthen sensorimotor integration (Colzato et al., 2007a; Waszak and Pholulamdeth, 2009), and that reward-related stimuli can prime response tendencies even if they are task-irrelevant (Krebs et al., 2010), we predicted that binding would be stronger for rewarded A-E mappings as compared to unrewarded mappings. This should be reflected in increased compatibility effects for primes that were previously related to a rewarded action, and would provide direct evidence that the acquisition of action-effect knowledge can be modulated by changes in the motivational value of an action and its consequence.

MATERIALS AND METHODS

PARTICIPANTS AND PROCEDURE

Twenty-six undergraduate students from Ghent University (eight male, four left-handed) participated in the study (mean age = 18.72 years; SD = 1.02). They all had normal or corrected to normal vision, gave written and informed consent to participate, and were naive to the rationale of the experiment. Stimuli were presented on a PC with a 17" monitor and responses were given with both index and middle fingers using the buttons "A," "S," "K," and "L" on a QWERTY computer keyboard. Following the experiment, participants completed the Behavioral-Inhibition and Behavioral-Activation Scales (BIS/BAS; Carver and White, 1994) to assess individual sensitivity to reward. The whole procedure lasted approximately 30 min. All participants received a basic compensation of 4 euro and an average performance-related bonus of 2.5 euro.

EXPERIMENTAL DESIGN

In line with previous research on A-E binding, the experiment consisted of two phases. First, participants completed an acquisition phase to establish learning of novel A-E mappings. For the given purpose, we manipulated the reward value of these mappings by associating half of them with monetary incentives. In the subsequent test phase, in which participants could no longer earn bonuses, the degree of A-E binding was assessed by presenting the previous action-effects as task-irrelevant response primes. Based on our assumption that reward would modulate the binding between actions and their effects during the acquisition phase, we predicted that reward-related primes would induce greater incompatibility effects as compared to reward-unrelated primes in the test phase.

ACQUISITION PHASE

The acquisition phase consisted of a forced-choice reaction time (RT) task with four different responses. Within a given block, each response was consistently mapped onto one specific picture (response cue) taken from a set of line drawings (Snodgrass and Vanderwart, 1980). At the beginning of each block, the four specific response cues were presented on the screen along with their associated responses. In each trial, after a variable intertrial interval (ITI) of 800–1000 ms, one of the cues was centrally presented for the maximum duration of 1500 ms (**Figure 1A**, left panel). Immediately after a response was given, or the maximum duration was reached, a colored square was displayed for 500 ms in the background of the cue, serving as a visual action-effect (see Wolfensteller and Ruge, 2011 for a similar procedure). In case of correct responses, the background color was response-specific (red, green, blue, or yellow), and in case of incorrect or late responses (>1500 ms) the background square turned gray. Participants were instructed to respond to the cues as quickly and as accurately as possible. Furthermore, they were told that the background color would indicate if their response on a given trial was correct and within the critical time window. Importantly, the picture category of the current cue (living animals vs. non-living objects) indicated whether a correct response (action, A) would be rewarded (reward action, RA) or not (no-reward action, NA).

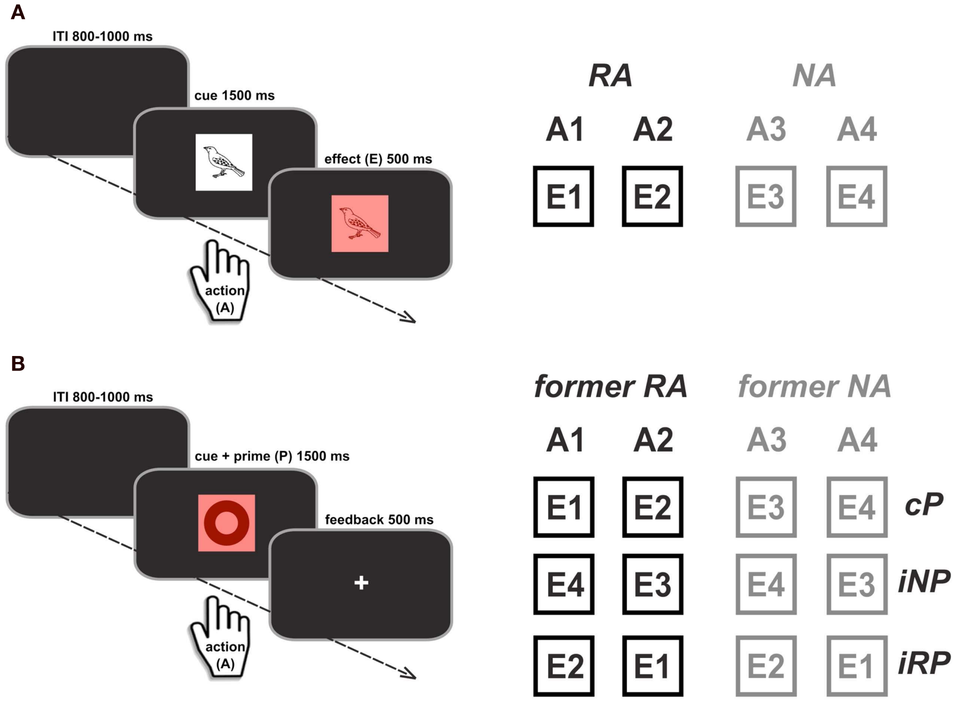

FIGURE 1 | Illustration of the experimental paradigm in the acquisition phase (A) and test phase (B). During acquisition, two out of the four actions were associated with reward (RA vs. NA). The unique effects (E1–E4) that were produced by specific actions (A1–A4) were used as response primes in the subsequent test phase. Primes could be either compatible with the required response (cP) or incompatible (shown for one exemplary A-E mapping). Due to the reward manipulation during acquisition, incompatible primes in the test phase could be either related to reward (iRP) or to no-reward (iNP) effects. The primes, however, were entirely irrelevant to the task and no longer predictive of reward in the test phase.

For each correct response that was given within the maximum time window of 1500 ms, 10 points were automatically added to the participants' score, which determined the total gain in Euro cents (0.5 euro per 200 points). The cue-category association with reward was counterbalanced across participants and cue categories were equally assigned to both hands and to index and middle fingers. In each block, a novel set of cue pictures was introduced in order to keep the task at a constant level of difficulty. However, mappings between cue categories and responses, and between responses and effect colors were constant for each participant (counterbalanced across participants). Overall, participants worked through four blocks of 60 trials, resulting in 120 reward trials and 120 no-reward trials performed with two fingers each.

TEST PHASE

In the test phase, participants completed a similar RT task using the same responses as before. They were told that there was no longer anything to win, but that they should continue to respond as quickly and accurately as possible. Importantly, responses were cued by a new set of pictures that were not associated with the previous cue categories (abstract symbols from the creative symbol collection of Matton images[1]). The new cue-category was introduced to eliminate a potentially confounding influence of stimulus-effect associations on task performance in the test phase (cf. Wolfensteller and Ruge, 2011). To probe the degree of A-E binding, the previous action-effects were now presented as response primes (i.e., displayed as squares in the background at 100 ms prior to cue onset until the offset of the cue). Participants were instructed that the colors were irrelevant for the task at hand and should thus be ignored. Analogous to the acquisition phase, cues remained on the screen for a maximum duration of 1500 ms. After a response was given or the maximum duration was reached, performance feedback was presented centrally for 500 ms, with a "+" indicating correct and fast responses and a "−" indicating response errors or omissions (**Figure 1B**). All possible combinations of response cues and primes were presented equally often, resulting in three types of primes: (1) compatible primes (cP, compatible to previous A-E mapping), (2) incompatible reward-related primes (iRP, effect of a different previously reward-related response), and (3) incompatible no-reward primes (iNP, effect of a different previously reward-unrelated response). Moreover, responses themselves could be distinguished based on whether they had been related to reward in the acquisition phase (former RA) or not (former NA). Altogether, participants completed eight trials of each prime response combination, resulting in a total of 128 trials (32 cP, 48 iRP, 48 iNP).

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[1] http://www.mattonimages.de/bilder/cd/ingram_publishing/creative_symbol_collection

RESULTS

ACQUISITION PHASE

As expected, participants' responses were faster on trials with RA than on trials with NA (RA < NA; $t = 6.58$, $p < 0.001$; **Table 1**), confirming that cue-reward associations facilitated performance in the respective trials. Overall, participants responded highly accurately with a small numerical but non-significant difference between reward and no-reward trials (96.8 vs. 95.4%; $p > 0.1$).

TEST PHASE RESPONSE TIMES (RTs)

Mean RTs of correct responses in the test phase were analyzed using a 2 × 3 repeated-measures analysis of variance (rANOVA) with reward-relatedness of the action (RA vs. NA) and prime compatibility (cP vs. iNP vs. iRP) as within-subject factors (**Figure 2A**; **Table 1**). The assumption of sphericity for the rANOVAs was tested using Mauchley's method. Since no significant violations were observed (all W-values > 0.8, $p > 0.2$), uncorrected F statistics are reported in the results. There was neither a main effect of reward-relatedness [$F(1,25) = 0.44$, $p = 0.512$, $\eta_p^2 = 0.017$] nor a main effect of prime compatibility [$F(2,50) = 1.24$, $p = 0.298$, $\eta_p^2 = 0.017$] alone, but a significant interaction of the two factors [$F(2,50) = 3.58$, $p = 0.035$, $\eta_p^2 = 0.125$]. Post hoc t-tests employed to test the nature of this interaction revealed that for former NA, RTs were significantly slower when primed with incompatible reward-related effects compared to incompatible reward-unrelated effects [iRP > iNP; $t(25) = 2.18$, $p = 0.031$, Cohen's $d = 0.225$], as well as compared to compatible primes [iRP > cP; $t(25) = 3.12$, $p = 0.005$, $d = 0.341$]. The difference between iNP and cP was not significant [$t(25) = 0.966$, $p = 0.344$; $d = 0.122$]. By contrast, for former RA, RTs did not differ at all across prime conditions (all p-values > 0.4).

TEST PHASE ACCURACY

An identical rANOVA on the response accuracy revealed no main effects of reward-relatedness of the action or prime compatibility, and no interaction of the two factors (all p-values > 0.1). This indicates that the conditions did not differ with regard to the *absolute* percentages of errors. We conducted an additional analysis of the *relative* percentages (i.e., ratios) of different error types across conditions to explore whether the ratio of prime-consistent errors would be increased in iRP-trials. This would support the notion that the perception of former reward-related effects indeed induced a specific, albeit false, action in the test phase (see Schmidt and De Houwer, 2011 for a similar analysis of different error types). To this end, we distinguished between *prime-consistent errors*, defined as erroneous responses that were consistent with the incompatible prime on a given trial, and *prime-inconsistent errors*, defined as erroneous responses that were not consistent with the incompatible prime, i.e., random errors. Observed ratios for prime-consistent errors were compared with a baseline of 33.3% that would be expected under a random error distribution with only one out of three possible false responses being prime-consistent. It should be noted that this analysis is limited in two ways, and must hence be considered exploratory: first, due to the nature of the paradigm, only incompatible conditions could be included, as no prime-consistent errors could be made on compatible trials. Second, the analysis could only be performed on a subset of participants, i.e., those who committed errors in the respective conditions (former RA trials: $N = 13$; former NA trials: $N = 11$). Ratios of prime-consistent errors were significantly increased in only one condition, namely on trials in which former no-reward responses were primed with incompatible reward-related effects [iRP: 62 vs. 33.3%, $t(10) = 2.3$, $p = 0.042$].

Table 1 | Behavioral performance in acquisition and test phase.

	RT ms (SE)		Accuracy% (SE)	
Acquisition	**RA**	**NA**	**RA**	**NA**
	553.1 (10.0)	608.2 (13.9)	96.8 (0.6)	95.4 (0.7)
Test	**Former RA**	**Former NA**	**Former RA**	**Former NA**
cP	580.6 (14.6)	573.2 (10.6)	96.5 (1.0)	98.3 (0.6)
iNP	575.7 (11.0)	580.0 (10.9)	97.2 (0.6)	96.8 (1.0)
iRP	574.0 (10.9)	593.5 (12.7)	95.6 (1.2)	97.5 (0.6)

RA, reward action; NA, no-reward action; cP, compatible prime; iNP, incompatible no-reward prime; iRP, incompatible reward prime; SE, standard error of the mean.

INDIVIDUAL REWARD RESPONSIVENESS

Our final analysis was concerned with the relation of participants' task performance to inter-individual differences in reward responsiveness. If the observed priming effect indeed reflects reward-driven strengthening of A-E bindings, then the size of this effect may be related to participants' dispositional sensitivity to rewarding events. To this end, we correlated individual RT-differences between iRP-trials and iNP-trials with the individual scores on the reward responsiveness subscale of the BIS/BAS (Carver and White, 1994), which is thought to reflect an individual's dispositional responsiveness to rewarding events. In the present sample, individual reward responsiveness scores varied between 14 and 20 (mean score = 17, SD = 1.74). We observed a significant correlation between RT difference values (NA-iRP minus NA-iNP) and the reward responsiveness subscale across all 26 participants [$r(24) = 0.42$, $p = 0.030$, two-tailed], indicating that those participants who reported being more responsive to reward in general showed a greater slowing on NA-iRP-trials compared to NA-iNP-trials (**Figure 2B**).

DISCUSSION

The present study investigated the influence of reward on A-E binding. We hypothesized that the intrinsic tendency to associate actions with their contingent outcomes could be influenced by assigning motivational values to specific actions. Following an acquisition phase in which half of the applied A-E mappings were related to monetary incentives, the strength of A-E binding was assessed in a test phase by presenting the former action-effects as task-irrelevant primes.

Altogether, three major findings were evident, all of which confirmed our prediction. First, and most importantly, induction effects were only found for primes that had been associated with reward during acquisition, providing direct evidence that reward strengthens the association between actions and their outcomes. Note that these differential effects occurred although the primes were entirely irrelevant to the task at hand and they were no longer

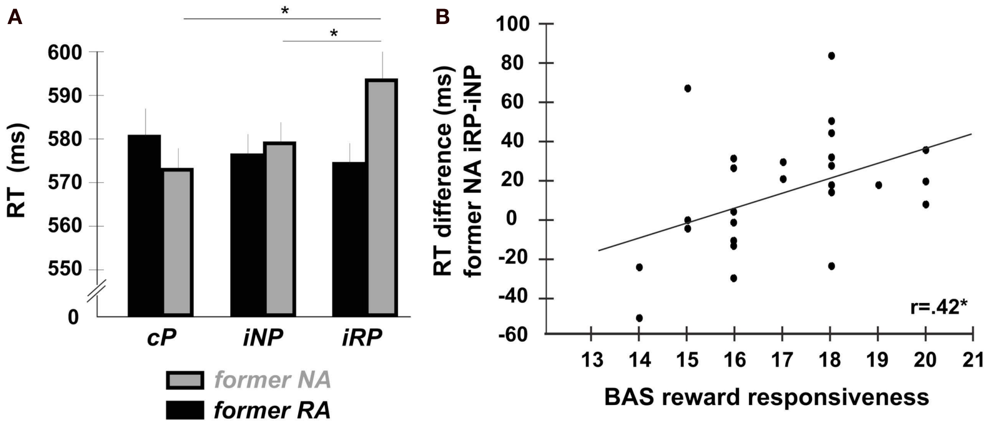

FIGURE 2 | Influence of reward-related primes in the test phase. (A) Despite being entirely irrelevant to the task and being no longer predictive of reward, incompatible reward-related primes (iRP) differentially increased RTs to new cues in the test phase. This effect was unique to *former* NA responses, in which the required action was never associated with actual reward. Error bars depict the standard error of the mean (SE) for within-subject comparisons. **(B)** The size of the RT-differences on trials with incompatible reward-related primes compared to trials with incompatible reward-unrelated primes (iRP > INP) on *former* NA trials correlated with participants dispositional responsiveness to reward.

predictive of any reward, which highlights the automatic nature of the binding process. Second, besides slower RTs on trials with correct responses, the same incompatible reward-related primes also increased the ratio of prime-consistent errors compared to a random distribution. This finding nicely illustrates the specificity of the interference effect at the response level and thus directly mirrors the concept of bi-directional action-effect representations in the framework of IMT. Third, inter-individual differences in reward responsiveness predicted the size of differential binding effects for reward-related and reward-unrelated primes. This finding further supports the idea that the observed induction effect with reward-related primes is related to incentive value representations of specific A-E bindings, which likely vary across individuals. Such a pattern is highly consistent with previously reported correlations between reward-sensitivity traits and actual behavioral responsiveness to reward (Kambouropoulos and Staiger, 2004), as well as between reward-related performance facilitation and neural activity in brain regions implicated in reward processing (Locke and Braver, 2008).

It is, however, important to consider to what extent the observed induction effect with reward-related primes indeed reflects a modulation of A-E binding in the acquisition phase. It could be argued that the influence of former reward effects arises from prioritized processing of a salient stimulus. Several possible outcomes are possible: for instance, stimulus processing could be generally facilitated by the salient effect, similar to effects of reward-related colors in a visual search array (Kiss et al., 2009). Such facilitation should, however, result in faster rather than slower response execution due to the advanced access to stimulus information. The salient effect color could also lead to a general distraction form the main task. Such effects have been demonstrated by using salient stimuli as irrelevant flankers in a target-discrimination task (Serences et al., 2005), as well as for reward-related colors that were presented at irrelevant positions in a visual search task (Hickey et al., 2010). Finally, participants could have experienced some kind of frustration in trials displaying former reward-related effects in the test phase, as they could no longer earn bonus money. In turn, frustration could cause unspecific attentional distraction. Importantly, however, all these forms of attentional distraction are unlikely to trigger specific erroneous response tendencies, which is suggested by the result of the exploratory error types analysis in the present study.

It is moreover key to exclude the possibility that the observed differential effect in the test phase is an artefact of the individuals' performance during the acquisition phase. As noted above, there was no difference in performance accuracy between reward-related and unrelated trials. Thus, participants experienced a similar number of A-E couplings in both conditions. Furthermore, participants responded faster in reward-related trials in the acquisition phase. This nicely illustrates that participants were indeed motivated by the prospect of reward and optimized their performance accordingly (Krebs et al., 2010; Schmidt et al., 2012). It could thus be argued that the observed binding for reward-related A-E mappings is a mere consequence of participants allocating more attention to the reward-related color effects during acquisition. Although recent evidence indicates that directing the focus of attention toward action outcomes during the acquisition phase does not automatically facilitate A-E binding (Herwig and Waszak, 2009), future research should certainly specify the mechanisms by which reward modulates A-E binding and to what extent it relies on the modulation of attentional mechanisms.

An additional interesting observation was that responses that had been associated with reward during acquisition were unaffected by prime compatibility in the test phase. Considering that reward-predictive stimuli have not only been shown to increase attention but also to strengthen the associated response pathways (e.g., Krebs et al., 2011; Schmidt et al., 2012), it is feasible to assume that former reward-associated responses in the current study are less prone to interfering information, namely incompatible effect primes.

Another noteworthy finding in the present study was the absence of significant compatibility effects with reward-unrelated primes. This non-finding is rather surprising since binding for unrewarded effects has already been demonstrated frequently in the literature (e.g., Hommel et al., 2003 or Hoffmann et al., 2009). However, the absence of compatibility effects for reward-unrelated primes may be associated with methodological aspects of the present experimental design. First, our study employed visual action-effects, which have been shown to be less salient than auditory action-effects, thereby leading to weaker A-E binding (Kunde, 2001; Dutzi and Hommel, 2009). Moreover, the paradigm was designed to minimize the influence of possibly confounding factors that could artificially inflate the size of induction effects. For instance, we excluded an influence of cue-effect associations by introducing a novel set of pictures as cues in the test phase. Furthermore, the present study employed a full combination of primes and responses, i.e., each effect occurred multiple times both as compatible and as incompatible prime. By using this design, the influence of each particular effect is necessarily weakened in comparison with classical paradigms that present effect stimuli as either only compatible or only incompatible primes in the test phase (cf. Elsner and Hommel, 2004; Wolfensteller and Ruge, 2011). A final paradigmatic aspect relates to the timing of prime presentation relative to the onset of the response cues. Recently, Ziessler and Nattkemper (2011) employed a systematic manipulation of the stimulus-onset asynchrony (SOA) between effect primes and response cues. Effects of prime compatibility were only observed when the primes were presented *after* cue onset. Thus, the absence of priming effects for reward-unrelated effects in the present study could be partly due to the fact that the primes may not have been presented at the time of their maximal effectiveness.

From a more general perspective, it is moreover a common observation that the introduction of reward signals not only modulates performance in those trials that are subject to actual reward, but it also modifies the general task context, resulting in altered performance on the no-reward trials, as compared to a "neutral" task-contexts without reward (e.g., Braem et al., 2012). Thus, in the present study, the presence of reward in the acquisition phase may have influenced participants' experience of the unrewarded A-E mappings as well. It could be argued that unrewarded effects in a reward context may be perceived as less significant. Specifically, it has been demonstrated that behavioral and neural influences of high-reward vs. low-reward stimuli critically depend on the overall context, i.e., the differences between trial types become more distinct in a general reward context (Delgado et al., 2004). Such a relative "devaluation" of unrewarded effects may counteract A-E binding in the present paradigm, such that for an action which does not produce an explicitly positive outcome, a bi-directional binding of the two events might be attenuated. Future research could explore this question by explicitly introducing reward as well and punishment signals during the acquisition of A-E associations.

Future research should also specify the precise mechanisms by which reward enhances the association strength of motor representations and representations of the respective sensory outcomes. It is known from numerous studies employing reward-modulated paradigms that reward associations can influence cognitive functions and behavior via diverse mechanisms (Pessoa, 2009; Pessoa and Engelmann, 2010). Among them are the prioritization of perceptual processing and the enforcement of specific response tendencies, as well as the increase of cognitive and physical effort to perform the task and the change of long-term stimulus representations. While conclusive statements about the underlying mechanism may not be warranted based on the present data, it appears likely that reward modulates the behavioral relevance of both an action and its consequence, which may in turn enforce the joint coding of the two events.

With regard to the neural level, dopamine has been proposed to underlie the formation of sensorimotor associations (Colzato et al., 2007a). Considering that reward-predicting stimuli are known to trigger dopaminergic activity (Knutson and Gibbs, 2007; Schott et al., 2008), it is likely that the reward-related effect in our own study is mediated by dopamine as well. Future studies will be needed to illuminate this relationship further, e.g., by assessing markers of individual dopamine levels, such as the spontaneous eye-blink rate, as covariates (Colzato et al., 2007b; Aarts et al., 2012), or by employing a similar paradigm in individuals with specific genotypes or clinical conditions promoting differential striatal dopamine levels (Schott et al., 2007; Yacubian et al., 2007).

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Early markers of ongoing action-effect learning

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Acquiring knowledge about the relationship between stimulus conditions, one's own actions, and the resulting consequences or effects, is one prerequisite for intentional action. Previous studies have shown that such contextualized associations between actions and their effects (S-R-E associations) can be picked up very quickly. The present study examined how such weakly practiced associations might affect overt behavior during the process of initial learning and during subsequent retrieval, and how these two measures are interrelated. We examined incidental (S-)R-E learning in the context of trial-and-error S-R learning and in the context of instruction-based S-R learning. Furthermore, as a control condition, common outcome (CO) learning blocks were included in which all responses produced one common sound effect, hence precluding differential (S-)R-E learning. Post-learning retrieval of R-E associations was tested by re-using previously produced sound effects as novel imperative stimuli combined with actions that were either compatible or incompatible with the previously encountered R-E mapping. The central result was that the size of the compatibility effect could be predicted by the size of relative response slowing during ongoing learning in the preceding acquisition phase, both in trial-and-error learning and in instruction-based learning. Importantly, this correlation was absent for the CO control condition, precluding accounts based on unspecific factors. Instead, the results suggest that differential outcomes are "actively" integrated into action planning and that this takes additional planning time. We speculate that this might be especially true for weakly practiced (S-)R-E associations before an initial goal-directed action mode transitions into a more stimulus-based action mode.

Keywords: instrumental learning, goal-directed action, differential outcomes, anticipation, instruction

INTRODUCTION

Common sense as well as an extensive body of literature suggests that higher organisms can learn to associate perceived changes in the environment with their own actions and use this acquired knowledge to actively pursue these environmental effects (E) by choosing the right action in a given context. In the simplest choice situation successful behavior requires response R1 under stimulus context S1 but response R2 under stimulus context S2. In other words, an organism needs to be able to discriminate between "good" and "bad" outcomes (O) of action (e.g., under S1: R1 returns good outcome; R2 returns bad outcome). This discrimination can be based on some form of performance feedback or based on instruction (Doll et al., 2009; Ramamoorthy and Verguts, 2012; Wolfensteller and Ruge, 2012). There is ample evidence that such outcome discrimination is indeed an integral part of the associational structure controlling action selection (Urcuioli, 2005; Balleine and Ostlund, 2007; de Wit and Dickinson, 2009; Nattkemper et al., 2010). That is, different from the classical Thorndikian view, performance feedback or "reinforcement" does not only serve the imprinting of stimulus-response (S-R) associations, but is in fact becoming part of a triple S-R-O or S-R-E association (Silvetti and Verguts, 2012). This is the associational basis of goal-directed action, enabling an agent to select an action based on anticipating the likely outcome this action would entail under a certain stimulus context. To disentangle S-R imprinting and S-R-O learning, the use of "differential outcomes" (DO) has been adopted in a wide range of different paradigms, including the term-defining "*DO paradigm*" (Trapold and Overmier, 1972), the *selective outcome devaluation paradigm* (e.g., Colwill and Rescorla, 1985), the *selective outcome priming paradigm* (e.g., Elsner and Hommel, 2001; Ziessler et al., 2004), and the *natural outcome compatibility paradigm* (e.g., Hommel, 1993; Kunde, 2001). In all these paradigms different actions do not only entail a common positive/negative feedback but additionally each action entails a unique outcome. This can be different types of rewards (e.g., sucrose liquid, food pellet, etc.) as in the outcome devaluation paradigm or different types of non-incentive perceptual events (sounds, colors, etc.) as in the selective outcome priming paradigm. Each paradigm has shown unique effects after the introduction of DO which support the notion of truly goal-directed action representations. In the DO paradigm, the trial-and-error learning rate of novel S-R mappings is higher under DO conditions as compared to common outcome (CO) conditions, especially early during learning (i.e., when error rates are still high). In the outcome devaluation paradigm, actions that have been learned to produce a certain outcome are less frequently chosen under extinction after this outcome has been selectively devaluated. In the selective effect priming paradigm, presentation of DO as response primes has shown to selectively activate those actions that have produced these effects in a preceding acquisition phase. Finally, in the natural outcome compatibility paradigm actions that produced the naturally expected effects (e.g., forcefully pushing a button leads to loud tone) were faster as compared to actions that produced the naturally incompatible effect.

Notably, these paradigms fall into one of two research traditions which share a common perspective on goal-directed action in terms of the DO rationale, but differ decisively in certain procedural aspects. One important difference is the amount of practice. Paradigms following the ideomotor learning tradition (i.e., selective effect priming and natural effect compatibility) typically investigate the impact of (S-)R-E associations after quite extended R-E acquisition periods typically amounting to more than 100 pairings of a response with its effect (amounting to a virtually infinite number of pairings for natural R-E mappings used in the compatibility paradigm). By contrast, paradigms following the instrumental learning tradition typically examine the impact of S-R-E associations early *during* (DO paradigm) or *after* (outcome devaluation) a rather limited number of S-R-E pairings well below 100 R-E pairings. Considering evidence mainly from brain research that too much practice diminishes the influence of goal (i.e., effect or outcome) representations while habitual control based on S-R associations alone becomes increasingly dominant (Killcross and Coutureau, 2003; Atallah et al., 2004; Yin and Knowlton, 2006; Seger and Spiering, 2011), it seems likely that ideomotor paradigms might measure different aspects of goal-directed action than instrumental paradigms. One speculation is that *early during learning* the anticipation of a specific outcome might affect response selection in two different ways concurrently. First, outcome anticipation might activate associations between actions and rewards (i.e., retrieving the information that one but not another response will yield reward or success in a given stimulus context). Second, outcome anticipation might directly activate the associated response, yet without any reference to its incentive value. By contrast after *extended practice*, only this latter "non-incentive" path might still be impacting behavior. This distinction might explain why extended practice reduces the impact of outcome devaluation (reference to incentive properties gets lost) while at the same time action effects are still able to prime the associated response directly via bi-directional R-E associations (for a recent review, see Wolfensteller and Ruge, 2012).

Before this background, we recently started conducting experiments within the ideomotor framework using the selective effect priming procedure, but different from previous studies we employed a comparably short R-E learning phase that is more similar to instrumental learning protocols in terms of the number of repeated R-E pairings (Wolfensteller and Ruge, 2011). In these initial experiments we found the typical effect priming results when re-using effect stimuli as response primes that were consistently produced by specific actions in a preceding acquisition phase. Specifically, test phase performance was impaired when the currently required response was incompatible vs. compatible with the response that had produced the current effect prime in the preceding learning phase. This clearly indicates that R-E associations were formed after very few (8–12) repeated pairings of R and E and, importantly, that these associations can be detected with the "passive" effect priming procedure. Hence, this demonstrates that the typical ideomotor mechanisms seem to operate even after very limited practice.

In the present study we aimed to link more directly performance measures associated with *initial ongoing* (S-)R-E learning with post-learning measures of R-E associational strength. Similar to instrumental learning protocols we implemented both, short DO learning blocks as well as short CO learning blocks. In both conditions, subjects had to learn novel S-R mappings by trial-and-error. The comparison of performance learning curves between DO and CO conditions thus allowed us to determine one index reflecting the "active" integration of goal information *during the initial acquisition* of S-R mappings. Additionally, similar to ideomotor learning protocols, after DO learning blocks were completed, an effect priming procedure was employed that allowed us to obtain a second, independent index of the strength of bi-directional R-E/R-O associations acquired beforehand. Based on these two behavioral indices, we aimed to determine how ongoing DO learning might be related to the test phase R-E compatibility effect. The rationale was that the size of the R-E compatibility effect serves as an index of R-E associational strength that can hence be used to determine the extent to which learning-related changes in performance might reflect the (increasing) incorporation of *anticipated* outcome information in action planning processes. This is particularly important in order to determine whether R-E associations are actually integrated during action planning when the natural order of events is preserved (i.e., S, then R, then E) as is the case during the initial learning phase in the present study. As of yet, evidence for active effect integration under the natural event order rests on studies involving *well practiced* associations during conditions of *R-E competition* (Kunde, 2001; Kunde et al., 2004). However, *weakly practiced* (S-)R-E associations have only been shown to "passively" impact action planning within the selective effect priming paradigm, that is, when the natural order of events is reversed (i.e., previous E, then S, then R; Wolfensteller and Ruge, 2011).

EXPERIMENT 1

MATERIALS AND METHODS

Subjects

Fifty subjects participated in this experiment and received monetary compensation or course credit. Data from one subject were lost due to logging errors. Hence, data analysis was based on a sample of 49 subjects (20 male, mean age24).

Design

The experiment comprised 22 experimental blocks, including 11 CO blocks and 11 DO blocks. CO and DO blocks were randomly intermixed. Each block comprised a learning phase in which subjects had to learn by trial-and-error novel 4:4 stimulus-response mappings. Stimuli were four abstract visual patterns (see **Figure 1** for an example) that were different for each block (i.e., 88 different visual stimuli overall). The 22 sets of four stimuli were compiled such that the four stimuli within each set were easily discriminable. The sequence of the 22 sets was randomized across subjects. Hence, across subjects, each set of stimuli was equally likely to be assigned

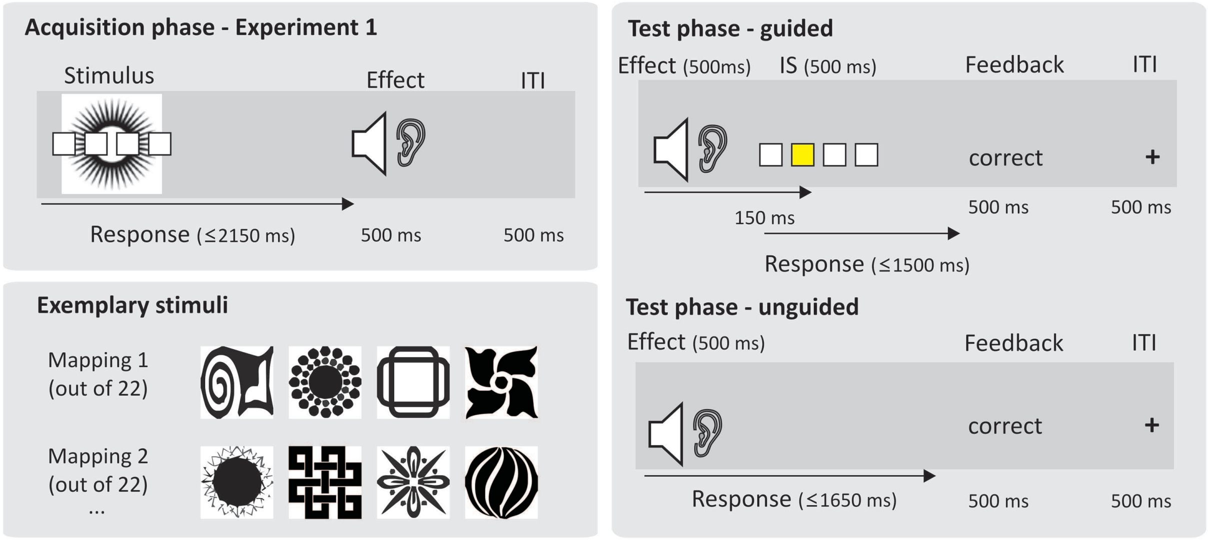

FIGURE 1 | Overview of the experimental procedures employed in Exp. 1. ITI refers to inter-trial interval. For Exp. 2A and Exp. 2B, this procedure was used in a modified form.

to the CO or DO condition. Responses were to be made with the left middle finger, the left index finger, the right index finger, and the right middle finger mapped to the keys "D," "F," "K," and "L" on a standard "QWERTZ" keyboard. In CO blocks a correct response was followed by a CO (natural sound). This CO sound (e.g., a ring tone, a dog's bark, a laugh, squeaking breaks, etc.) was different for each block (i.e., 11 different CO sounds overall). In DO blocks instead, correct responses were consistently followed by one of four different outcomes (again natural sounds). The four sounds were different for each block (i.e., 44 different DO sounds overall). As for the visual stimuli, we created 22 sets of four different sounds that were arranged to be easily discriminable (i.e., 88 different sounds overall). The sequence of the 22 sets was randomized across subjects. For the 11 DO blocks all four sounds were used whereas for the 11 CO blocks only one out of the four sounds was selected. Hence, across subjects, each set of sounds was equally likely to be assigned to the CO or DO condition. For both CO and DO conditions, a trial-and-error learning block was terminated when each response had been performed correctly eight times (not necessarily in a row). Alternatively, learning was terminated when a total of 70 learning trials were exceeded. For nine subjects this happened once for a single learning block each and for two subjects this happened twice. However, terminal error rates in each block were below 10% in all these cases. Since this indicates that the S-R mappings had been learned well, data were not excluded from the analysis.

A learning trial started with the presentation of the visual stimulus (S) in the center of the screen which remained on screen until response execution or time out after 2150 ms. The sound effect was presented immediately after correct response execution for 500 ms. In case of erroneous responses, error feedback was displayed for 500 ms in the center of the screen (German for "error" or "too slow"). The next trial started after a constant inter-trial interval of 500 ms.

Specific for the DO condition, the initial trial-and-error learning phase was directly followed by a "test" phase in which subjects were now required to respond to the previous effect sounds with the same set of four responses used during the acquisition phase, which could be either compatible or incompatible to the response that produced a specific effect during the preceding acquisition phase (see Elsner and Hommel, 2001, Exp. 2). The rationale is that the strength of bi-directional R-E associations acquired during the learning phase should be expressed in relatively impaired performance in incompatible vs. compatible test trials due to non-intentional response priming. Compatible and incompatible trials were randomly intermixed. The 4:4 sound-response mappings were explicitly instructed during an initial instruction phase spanning the first three presentations of each sound. During this initial "guided phase," the correct response was instructed via yellow squares appearing on the screen and localized spatially compatible with the four responses (see **Figure 1**). The initial three presentations of each of the four sounds were pseudo-randomly intermixed during the first 12 instruction trials such that each sound was exactly three times correctly responded to. Erroneous trials were immediately repeated. A guided trial started with the presentation of a fixation cross in the center of the screen for 500 ms, followed by of one of the previous effects sounds which lasted for approximately 500 ms. The instructional stimulus (IS) was presented 150 ms after sound onset until response execution or until time out after 1500 ms. Response execution was immediately followed by accuracy feedback presented centrally on the screen for 650 ms (German words for "correct," "error," or "too slow"). For the next 24 "unguided" trials six presentations of each sound were again pseudo-randomly mixed such that each sound was correctly responded to exactly six times. The timing of trial events was exactly the same as in the guided phase. The only difference was that the IS was not displayed, hence there was a response deadline of 1650 ms relative to sound onset (instead of 1500 ms relative to IS onset).

During both the guided and the unguided test phase, two sounds were paired with the response that had produced that sound during the preceding learning phase (R-E compatible condition). The two other sounds were paired with responses that had produced a different sound in the preceding learning phase (R-E incompatible condition). Compatible and Incompatible trials were randomly intermixed. The assignment of fingers and response hands was counterbalanced across test blocks such that compatible and incompatible responses were always only partly assigned to different hands. For instance, when the index finger of one hand was assigned to the compatible condition the middle finger of the same hand was assigned to the incompatible condition. The four different assignment schemes conforming to this rule were pseudo-randomly assigned to subjects and test blocks such that each assignment scheme was realized approximately equally often. The effect of R-E compatibility was computed for the 24 test trials following the instruction phase. Note that in the CO condition, in which all responses were associated with one common effect, no test phase was administered.

Analysis

In the initial trial-and-error acquisition phase, the progress of learning was analyzed with regard to error rates and response times as a function of correctly implemented distinct stimulus repetitions (SRep) one through eight. For instance, SRep level 1 comprised the performance data from the first correct implementation of each of the four different stimuli occurring in a given learning block. Response times were based on the arithmetic mean across distinct stimuli and learning blocks. Analogously, error rates were expressed in terms of the percentage of errors committed. These learning curves were separately computed for CO and DO learning blocks. To assess the effect of CO vs. DO on learning performance we run two separate repeated measures ANOVAs under SPSS (version 18), one for error rates and one for response times, each with the two within-subject factors SRep and OUTCOME. To account for possible non-sphericity in the 8-level SRep factor, significance tests were based on the multivariate analysis output.

In DO blocks, the test phase R-E compatibility effect was computed both for mean response times and mean error rates based on the 24 unguided test trials. Statistical significance was assessed via paired t-tests.

The central analysis targeted the correlation between learning phase performance dynamics and test phase compatibility effect in DO blocks. The rationale was that the size of the R-E compatibility effect serves as an index of R-E associational strength that can hence be used to determine the extent to which learning-related changes in performance might reflect the (increasing) incorporation of outcome information in action planning processes. To this end, we computed a series of across subjects correlations between "ongoing learning" as derived from all DO and CO blocks, respectively and the mean R-E compatibility effect derived from all test phases following DO blocks. To capture "ongoing learning" we used performance at SRrep level 2 as reference for performance at the six subsequent SRep levels 3 through 8. That is, for each subject we obtained a series of mean difference values (i.e., SRep2 – Srep3, SRep2 – SRep4, etc.). Note that we decided against SRep 1 as reference even though it might appear especially well suited due to its neutral status with respect to R-E associational strength (equaling zero). However, general considerations and the actual data pattern observed at SRep level 1 suggest potential problems with this approach. Generally, it should be kept in mind that the correlational analysis aims to identify learning-related performance indices related to the active integration of learned (S-)R-E associations into action planning by exploiting inter-subject variability specifically linked to that process. In this respect, SRep1 is not an ideal reference as it comprises especially strong "nuisance" variance components related to stimulus novelty or related to confusion due to the intermixing of CO and DO blocks. Hence, irrespective of CO or DO condition, the variability caused by such nuisance variables might mask a systematic, but comparably weak variability component induced by the process of interest. Second, even though associations between stimuli and DO are by definition non-existing at SRep1, we observed a highly significant response slowing for DO trials relative to CO trials at SRep1. This effect strongly indicates the presence of DO-related processes at SRep1 that cannot be due to the active integration of learned (S-)R-E associations in action planning.

We computed the correlations between DO learning performance slope (i.e., SRep2 – Srep3, SRep2 – SRep4, etc.) and test phase compatibility. Analogously, we computed the correlations between CO learning performance slope and test phase compatibility. Although compatibility was not defined for CO blocks (and hence no test phase was implemented), this analysis was nevertheless important as a control procedure to exclude the possibility that correlations observed for the DO condition might reflect unspecific effects. For instance, participants with weak trial-and-error learning performance (indicated by small differences between the SRep2 reference and subsequent SRep levels) might also be those that are more strongly affected by R-E compatibility. In such a case we would expect a negative correlation between learning performance slope and RE compatibility for both DO and CO learning performance slope although there is no direct link in terms of R-E associations. In contrast, a specific link between DO learning and RE compatibility directly related to the acquisition of R-E associations would be expressed in a correlation exclusively for DO learning but not for the CO learning condition. To explicitly test whether these correlations were significantly different between DO and CO, we correlated the compatibility effect with the difference between DO-related learning performance slope and CO-related learning performance slope [e.g., $(\mathrm{SRep2} - \mathrm{SRep3})_{\mathrm{DO}} - (\mathrm{SRep2} - \mathrm{SRep3})_{\mathrm{CO}}$]. In other words, we tested whether the compatibility effect would be associated with DO vs. CO differences in performance *slope*. In addition, we also tested whether the compatibility effect would be associated with the difference between DO and CO conditions in terms of the respective absolute performance *levels* (e.g., $\mathrm{SRep3}_{\mathrm{DO}} - \mathrm{SRep3}_{\mathrm{CO}}$).

RESULTS

Learning performance

A summary of learning performance is depicted in **Figure 2**. There was a sharp decline across SRep 1 to SRep 8 in error rates ($F_{7,42} = 165.4$; $p < 0.001$; $\eta_p^2 = 0.965$). However, this main effect was not significantly modulated by OUTCOME condition ($F_{7,42} = 0.92$; $p = 0.501$; $\eta_p^2 = 0.133$). Also, there was no main effect of OUTCOME ($F_{1,48} = 1.7$; $p = 0.204$; $\eta_p^2 = 0.033$). For mean RTs there was also a significant decline across correctly implemented SRep levels 1 through 8 ($F_{7,42} = 64.3$; $p < 0.001$; $\eta_p^2 = 0.915$). Different from error results, there was a highly significant OUTCOME main effect, indicating slower responses in the DO as compared to the CO condition ($F_{1,48} = 11.8$; $p < 0.001$; $\eta_p^2 = 0.197$). Again, the interaction between SRep and OUTCOME failed to approach significance ($F_{7,42} = 1.6$; $p = 0.154$; $\eta_p^2 = 0.214$). However, numerically the OUTCOME response slowing effect seemed to follow a 3-phasic pattern (see **Figure 2**), being strong in the beginning (SRep1), then reduced (SRep 2 through 4), and increasing again (SRep 5 through 8). In an exploratory *post hoc* polynomial contrast analysis, this 3-phasic pattern was confirmed statistically by a significant third order (cubical) interaction ($F_{1,48} = 6.0$; $p = 0.018$; $\eta_p^2 = 0.111$). Furthermore, separate paired *t*-tests (two-sided) for each SRep level revealed significantly increased RT for DO vs. CO exclusively at SRep 1 (25 ms; $t_{48} = 2.8$; $p = 0.008$; $\eta^2 = 0.140$), SRep 5 (19 ms; $t_{48} = 2.6$; $p = 0.014$; $\eta^2 = 0.123$), SRep 6 (23 ms; $t_{48} = 3.5$; $p = 0.001$; $\eta^2 = 0.203$), SRep 7 (21 ms; $t_{48} = 2.8$; $p = 0.007$; $\eta^2 = 0.140$), and SRep 8 (17 ms; $t_{48} = 2.0$; $p < 0.057$; $\eta^2 = 0.077$).

R-E compatibility

We found significant compatibility effects (i.e., incompatible vs. compatible in the DO condition) for both mean RTs (545.4 ms vs. 531.5 ms; $t_{48} = 2.73$; $p = 0.009$; $\eta^2 = 0.134$) and mean error rates (12.6 vs. 9.4%; $t_{48} = 5.33$; $p < 0.001$; $\eta^2 = 0.372$).

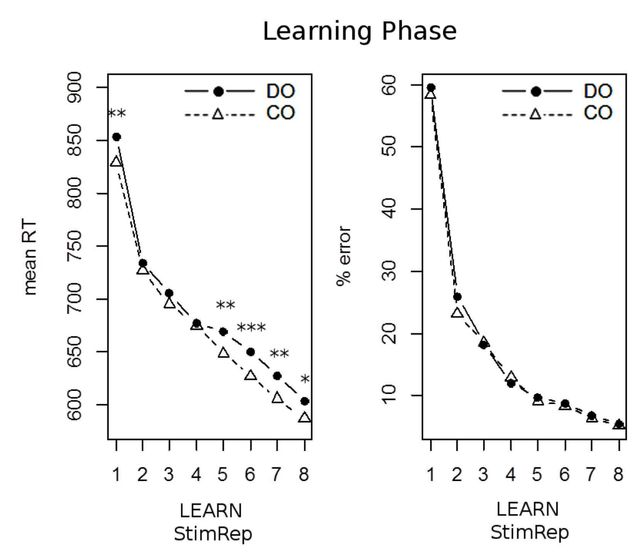

FIGURE 2 | Performance across the initial learning phase of Exp. 1 for mean response times (left panel) and mean% errors (right panel). DO denotes the Differential Outcome condition, CO denotes the Common Outcome condition. Learning is expressed in terms of correctly implemented stimulus repetitions (StimRep). Asterisks denote significant differences between DO and CO (*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$).

Correlations

We first analyzed correlations between R-E compatibility and ongoing learning in terms of performance *slope* (i.e., SRep2 – SRep3, Srep2 – SRep4, etc...) for all four combinations of RT and error rate in these two inter-dependent variables (i.e., $\mathrm{RT}_{\mathrm{learn}} \times \mathrm{RT}_{\mathrm{test}}$, $\mathrm{RT}_{\mathrm{learn}} \times \mathrm{errors}_{\mathrm{test}}$, $\mathrm{errors}_{\mathrm{learn}} \times \mathrm{RT}_{\mathrm{test}}$, and $\mathrm{errors}_{\mathrm{learn}} \times \mathrm{errors}_{\mathrm{test}}$). We first did this separately for both the DO and the CO learning conditions.

Generally, we only found significant results for $\mathrm{RT}_{\mathrm{learn}} \times \mathrm{RT}_{\mathrm{test}}$ correlations. Specifically, we found significant *negative* correlations between R-E compatibility and learning performance slope in the DO condition for all six slope levels (all $p < 0.05$; *two-sided*)[1]. As shown in **Figure 3** the correlations were strongest early during learning, peaking at SRep2-SRep4 ($r = -0.43$). Importantly, there were no significant correlations between R-E compatibility and performance slope during ongoing learning in the CO condition. These differential correlational patterns for DO and CO could be confirmed statistically for the early phase of learning by computing the correlation between the compatibility effect and the learning-related RT "difference of the difference" [e.g., $(\mathrm{SRep2\text{-}SRep3})_{\mathrm{DO}} - (\mathrm{SRep2\text{-}SRep3})_{\mathrm{CO}}$]. The respective correlations were $r = -0.36$ ($p = 0.014$; *two-tailed*) for SRep2-SRep3 and $r = -0.31$ ($p = 0.031$; *two-tailed*) for SRep2-SRep4.

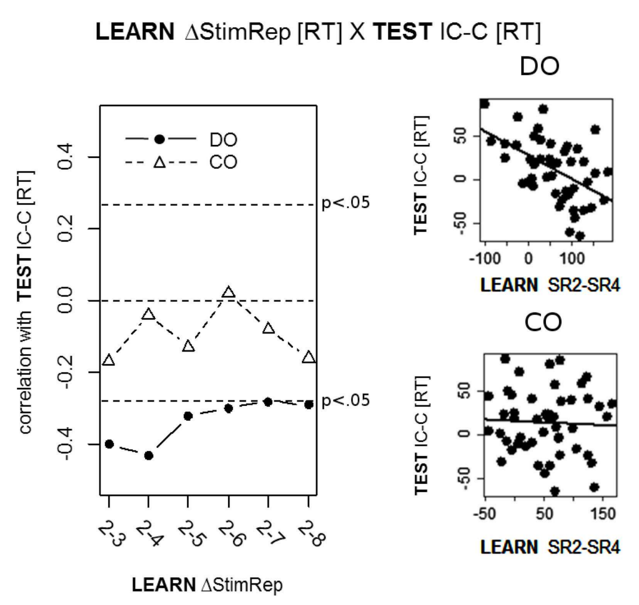

FIGURE 3 | Correlations between response times (RT) during ongoing learning and RTs during the subsequent response-effect (R-E) compatibility test in Exp. 1. Ongoing learning is expressed in terms of the RT difference between Stimulus Repetition (StimRep) 2 and the subsequent StimRep levels (i.e., ΔStimRep). R-E compatibility is expressed in terms of the RT difference between incompatible (IC) and compatible (C) test trials.

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[1] When using SRep 1 as reference for online R-E learning progress (SRep1 – SRep2, Srep1 – SRep3, etc...), there was an overall trend for negative correlations with the RE compatibility effect but none of them reached significance (neither for RTs nor for errors).

All correlations between R-E compatibility and absolute performance level differences between DO and CO ($\text{SRep1}_{\text{DO}} - \text{SRep1}_{\text{CO}}$, $\text{SRep2}_{\text{DO}} - \text{SRep2}_{\text{CO}}$, etc.) revealed no significant results (all $|r| < 0.23$; all $p > 0.122$).

DISCUSSION

As a first important result we found that trial-and-error S-R learning under DO conditions relative to CO conditions prolonged mean response times. At the same time we did not observe the typical DO-related relative reduction in error rates early during trial-and-error learning (e.g., Mok and Overmier, 2007; Noonan et al., 2011). That we failed to replicate this latter effect on error rates is not surprising given that the learning problem was comparably easy as indicated by the sharp drop of errors from SRep1 (60%) to SRep2 (25%). Logically, DO can only start contributing to response selection from SRep2 onward as subjects need to complete SRep level 1 to know which specific DO is produced by which specific response. Given the comparably low error rate at SRep2,it seems likely that the direct S-R link is already sufficiently strong on its own, hence reducing the potential contribution of DO for selecting the correct response. Notably, mean response slowing for DO vs. CO blocks was already present at SRep 1, that is, when DO could not be known prior to response execution and could thus not directly affect response selection. Instead, SRep1 response slowing might be related to the additional effort to encode (S-)R-E associations once a subject is realizing that the present block involves DOs instead of COs. Alternatively, it might indicate increased distraction due to the higher perceptual load in the DO condition. The absence of DO-related mean response slowing in the subsequent SRep levels 2 through 4 suggests that this initial effect is rather short-lived.

Importantly, neither the RT difference between DO and CO at SRep1 nor the DO-related learning performance slope from SRep1 to SRep2 (see footnote 1) yielded significant correlations with the R-E compatibility effect as a measure of R-E associational strength. This suggests that the initial DO-related mean RT slowing effect at SRep1 has no direct relevance for the formation and usage of (S-)R-E associations. This suggests that DO-related mean response slowing at SRep level 1 might rather reflect unspecific side effects possibly related to stronger distraction from the main S-R task by the higher perceptual load imposed by DOs as compared to COs.

By contrast, when considering DO-related learning performance from SRep level 2 onward we indeed found evidence for the integration of outcome information in action planning during the learning phase. Specifically, this was indicated by strong DO-specific negative correlations between learning performance *slope* and the R-E compatibility effect. In turn, this suggests that increasing outcome integration slows down (i.e., decelerates) the overall learning-related decrease in RT. Surprisingly, the strongest negative correlations between DO-related performance *slope* and R-E compatibility effect were observed early during learning (SRep2 through SRep4) where the *mean* RT difference between DO and CO was not significant. By contrast, later during learning (SRep5 through SRep8) the negative correlations with performance slope decreased considerably while at the same time mean RTs were now significantly slower for DO as compared to CO. Similarly surprising, absolute performance *level* differences between DO and CO conditions (i.e., $\text{SRep2}_{\text{DO}} - \text{SRep2}_{\text{CO}}$, $\text{SRep3}_{\text{DO}} - \text{SRep3}_{\text{CO}}$, etc…) did not correlate with the R-E compatibility effect – neither early during learning (i.e., SRep2 through 4) nor late during learning (i.e., SRep5 through 8). This seems particularly contradictory for the early phase of learning where performance slope exhibited the strongest correlation with R-E compatibility. For instance, a subject who exhibits a strong DO-specific decrease in performance slope between SRep2 and SRep4 should automatically also exhibit a relative RT slowing for DO vs. CO at SRep4 as a direct consequence of the decreased slope. Hence, both measures (performance slope and performance level) should similarly correlate with R-E compatibility. Yet, only slope but not level showed the correlation.

To account for both, the dissociation between slope-related correlations and level-related correlations and between slope-related correlations and mean RT differences, we need to consider the specific nature of correlations. First, the dissociation between slope-related correlations and mean RT difference might be due to the fact that we are dealing with correlations based on inter-individual *variability* on the one hand and *mean* differences on the other hand. Hence, strong correlations are likely to emerge when there is large inter-individual variability in a behavioral marker of interest while at the same time this marker might not be strongly expressed in mean differences between conditions exactly as a consequence of this variability. Accordingly, it seems reasonable to assume that variability in the learning and/or usage of (S-)R-E associations is stronger early in learning. This might explain why *mean* RT slowing for DO vs. CO conditions is maximal later during learning (i.e., SRep5 through SRep8) when most of the subjects might have learned the underlying (S-)R-E associations to a certain extent. By contrast, earlier in learning (i.e., SRep2 through SRep4) subjects might vary strongly in the (S-)R-E learning success which might give rise to the stronger correlation with the compatibility effect. In other words, a subject who starts earlier with (S-)R-E learning (i.e., implicating a strong decrease in performance slope between SRep2 and SRep4) will have acquired stronger R-E associations by the end of the learning phase, hence giving rise to a stronger compatibility effect. At the same time, early DO-related decrease in performance slope between SRep2 and SRep4 might be present only in a relatively small proportion of subjects which might imply that overall mean RT will not be strongly increased for DO vs. CO at this stage yet.

Alternatively – or additionally – it is well conceivable that the DO-related decrease in performance slope (linked to compatibility) is not the only factor that affects response times differently for DO and CO. If this additional factor X caused a speed-up of RT for DO relative to CO, this would counteract the opposite mean RT slowing effect caused by the DO-related decrease in performance slope associated with factor Y. Hence, in sum, DO-related RT increase due to factor Y and DO-related RT decrease due to factor X might cancel out. This could explain the absence of significant mean RT differences between DO and CO for SRep2 through SRep4. Moreover, if factor X was *not* correlated with R-E compatibility [i.e., not specifically related to S-(R-E) learning/usage], this could explain why the mean RT difference between DO and CO was not correlated with compatibility. The reason is that the additional source of DO-related variability caused by factor X would overshadow the variability component caused by factor Y (i.e., the component that is associated with R-E compatibility)[2]. An additional source of "factor X" variance that might cause a mean speed-up for DO vs. CO could be related to an unspecific phasic alerting of DOs relative to COs with respect to the subsequent learning trial. Phasic alerting is known to induce a response speeding and is stronger for more salient accessory stimuli (Stahl and Rammsayer, 2005; Jepma et al., 2009). Since each of the four DOs is occurring less frequently than a single CO, its salience should be stronger and hence its phasic alerting impact on the processing of the next trial should be stronger. Clearly, this DO-related phasic alerting should be unrelated to the learning/usage of (S-)-R-E associations. Hence, this variance component should be uncorrelated with the R-E compatibility effect.

Finally, we need to discuss whether the DO-related negative correlation between performance slope and R-E compatibility indeed reflects the active integration (i.e., "usage") of DOs in action planning processes as we had originally reasoned. Alternatively, this correlation might also be related to the learning of (S-)R-E associations itself. Maybe the most compelling scenario why (S-)R-E learning might be associated with a decrease in learning performance slope is based on the indirect influence of DO-related distraction from the main S-R task. Such distraction might increase attention toward the differential action effects which, in turn, might increase (S-)R-E strengthening. Hence, a subject who is more strongly distracted by the DOs as reflected by greater decrease in performance slope would form stronger (S-)R-E associations as reflected by a greater R-E compatibility effect. What speaks against this interpretation is that distraction should be maximal in the beginning of learning and decrease toward the end of the learning phase. Indeed, we observed a quite strong slowing effect already at SRep 1 which is likely due to unspecific distraction (see also point further above). However, this initial slowing effect did not correlate with the R-E compatibility effect, suggesting that associated initial DO-related distraction did not amplify (S-)R-E learning.

Together, we conclude that the DO-specific negative correlations between learning performance slope and R-E compatibility likely indicate the active integration or usage of newly acquired (S)R-E associations in action planning when the natural order of events is preserved (i.e., S, then, R, then E) – at least when performing in an early phase of practice as in the present study. However, it is also clear that the complex pattern of DO-related correlations and mean RT differences between DO and CO conditions suggests that learning under DO conditions involves additional unspecific processes (distraction and phasic alerting) that affect mean RT without impacting the strength of (S-)R-E associations. This latter conclusion in particular needs to be confirmed by future research that will need to disentangle the different DO-related processes that are strongly intermingled in the present study.

EXPERIMENT 2A AND 2B

To further validate and generalize the correlational results from Exp. 1, we conducted an analogous correlational analysis for two additional Exp. 2A and 2B. Different from Exp. 1 these additional experiments employed an "instruction-based" learning procedure for acquiring novel 4:4 S-R mappings instead of trial-and-error learning. Also, S-R learning took always place in a DO learning context.

SUBJECTS

Forty-five subjects were recruited that had not participated in Exp. 1. Twenty-five subjects participated in Exp. 2A (eight male, mean age 27) and 20 subjects participated in Exp. 2B (five male, mean age 24).

PROCEDURE

Learning phase

The instruction procedure for acquiring novel S-R mappings was structurally highly similar to the instruction procedure used for the R-E compatibility test in Exp. 1. That is, the to-be-acquired 4:4 stimulus-response mappings were explicitly instructed during an initial instruction phase spanning the first three presentations of each stimulus. Stimuli were drawn from the same set of abstract pictures as in Exp. 1 and were different for each learning block. As in the DO condition of Exp. 1 correct responses were consistently followed by one of four different natural sounds as outcomes drawn from the same set of sounds as in Exp. 1. The four sounds were different for each block. There were 20 different learning blocks, each followed by an R-E compatibility test phase.

During instructed S-R learning, the initial three presentations of each of the four stimuli were pseudo-randomly intermixed during the first 12 instruction trials such that each stimulus was correctly responded to exactly three times. During this initial "guided phase," the correct response was instructed differently in Exp. 2A and 2B. In Exp. 2A the correct response was indicated by a yellow square appearing on the screen and localized spatially compatible with the four responses (see **Figure 1**). In Exp. 2B the correct response was indicated by a letter (D, F, K, or L) presented in the center of the screen. Letters were mapped to fingers according to their standard QWERTZ keyboard position (see **Figure 1**). Prior to the start of the experiment, subjects were told to memorize this mapping (the actual results confirmed that this was sufficiently easy for all subjects as indicated by SRrep level 1 error rates of below 20% for each subject and an overall mean SRep 1 error rate of 8%). These two instructional mappings were designed to manipulate retrieval effort, an aspect that is not further elaborated on in the present paper. Here, we simply use these two conditions for cross-validation assuming that they are sufficiently similar with regard to the processes of primary interest in the present context. A guided trial started with the presentation of the visual stimulus in the center of the screen until response execution. The IS was presented 150 ms after stimulus onset until response execution or until time out after 1500 ms. The sound effect was presented immediately after correct response execution for 500 ms. In case of erroneous responses, error feedback was displayed for 500 ms in the center of the screen (German words

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[2] This holds under the assumption that factor Y and factor X impact RT along different timelines – otherwise factor X impact on performance slope should have a similar overshadowing effect regarding factor Y impact on performance slope.

for "error" or "too slow"). The next trial started after a constant inter-trial interval of 500 ms.

Following the first 12 guided trials, 24 unguided trials were presented comprising six presentations of each stimulus that were again pseudo-randomly intermixed such that each stimulus was correctly responded to exactly six times. During this phase no IS was presented, yet the overall timing remained exactly the same as in the guided phase, implicating a response deadline of 1650 ms relative to stimulus onset (instead of 1500 ms relative to IS onset). Erroneous trials were immediately repeated during all phases of the experiment.

Test phase

The R-E compatibility test procedure used in Exp. 2A was identical to the test procedure in Exp. 1. The test procedure used in Exp. 2B was the same, except that letters were used for instruction (as in the learning phase of Exp 2B).

ANALYSIS

The analysis was performed analogously to Exp. 1, with two exceptions. One difference was that we used SRep level 4 instead of SRep level 2 as reference for determining the progress of learning. This was done to adjust for the fact that the first 3 SRep levels in Exp 2A and 2B were guided and hence not easily comparable to the subsequent unguided trials. Accordingly we used the first unguided SRep level 4 as reference. The second difference was that we applied a one-sided instead of a two-sided significance test for the correlational analysis according to the *a priori* hypothesis derived from Exp. 1 that the correlation should be negative. Since one might argue that Exp. 2 is not sufficiently similar to Exp. 1 to justify a one-sided test, we additionally indicate whenever significance would be missed according to the more conservative two-sided criterion. We first analyzed data collapsed across Exp. 2A and 2B. For cross-validation of the correlational results, we performed the correlational analysis separately for each sub-experiment.

RESULTS

Learning phase

Learning performance was analyzed separately for the guided and unguided phase using separate repeated measures ANOVAs for RTs and error rates analogous to Exp. 1. As could be expected, error rates were constantly ($F_{2,43} = 1.4$; $p = 0.246$; $\eta_p^2 = 0.063$) low during the guided phase and jumped up with the start of the unguided phase reflecting that responses had to be generated without the help of the instructional stimuli (**Figure 4**). Across the unguided phase error rates declined considerably ($F_{4,41} = 18.0$; $p < 0.001$; $\eta_p^2 = 0.637$). Response times declined significantly during the guided phase ($F_{2,43} = 44.5$; $p < 0.001$; $\eta_p^2 = 0.674$) as well as during the unguided phase ($F_{4,41} = 10.8$; $p < 0.001$; $\eta_p^2 = 0.514$). Note that absolute RTs were referenced to the instruction stimulus in the guided phase and to the antecedent stimulus (i.e., the abstract picture) in the unguided phase[3].

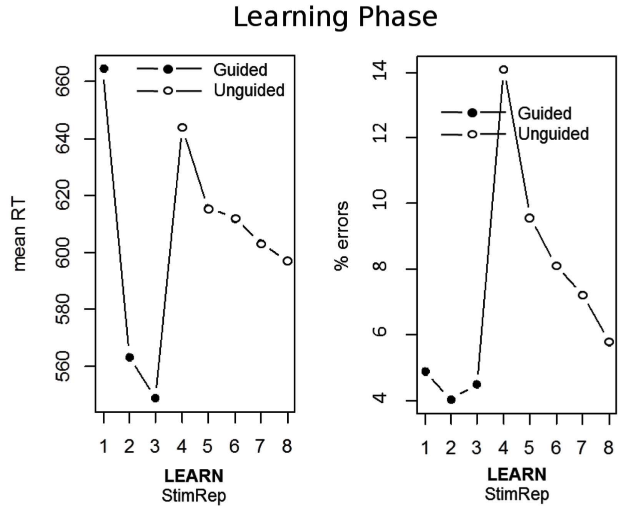

FIGURE 4 | Performance across the initial learning phase of Exp. 2A and Exp. 2B averaged together. The left panel depicts mean response times (RT) and the right panel depicts mean% errors. Learning is expressed in terms of correctly implemented stimulus repetitions (StimRep). The first 3 stimulus repetitions were guided by an instructional stimulus (IS) whereas the remaining 5 stimulus repetitions where unguided. RTs for StimRep 1–3 are measured relative to the IS onset whereas RTs for StimRep 4–8 are measured relative to the stimulus itself.

R-E compatibility

As in Exp. 1, R-E compatibility (i.e., incompatible vs. compatible test trials) was determined for the unguided phase. Again replicating the results from Exp. 1, there was a significant effect for RTs (522ms vs. 511 ms; $t_{44} = 2.7$; $p = 0.009$; $\eta^2 = 0.142$) as well as for error rates (14.8 vs. 11.3%; $t_{44} = 6.2$; $p < 0.001$; $\eta^2 = 0.466$).

Correlations

First, we computed the correlations between ongoing learning (i.e., SRep4 – SRep5, SRep4 – SRep6, etc.) and RE compatibility across Exp. 2A and 2B. To adjust for possible differences in the distributions of the two inter-dependent variables in each sub-experiment, we first *z*-standardized the values for each sub-experiment (mean = 0; SD = 1) before they were entered into the overall correlational analysis. We performed this analysis for all four combinations of RT and error rate in the interdependent variables (i.e., $\text{RT}_{\text{learn}} \times \text{RT}_{\text{test}}$, $\text{RT}_{\text{learn}} \times \text{errors}_{\text{test}}$, $\text{errors}_{\text{learn}} \times \text{RT}_{\text{test}}$, and $\text{errors}_{\text{learn}} \times \text{errors}_{\text{test}}$). As in Exp. 1 we found significant results only for correlations involving RTs in both inter-dependent variables. Replicating Exp. 1, the correlations between ongoing learning and RE compatibility were again all *negative.* These results are depicted in **Figure 5A** showing that negative correlations reached significance for SRep4 – SRep7 ($r = -0.33$; $p = 0.016$) and for SRep4 – Srep8 ($r = -0.38$; $p = 0.006$). Note that without prior experiment-wise normalization, the correlational pattern turns out to be highly similar ($r_{\text{SRep4-Srep5}} = -0.04$, $p = 0.403$; $r_{\text{SRep4-Srep6}} = -0.13$, $p = 0.203$; $r_{\text{SRep4-Srep7}} = -0.29$, $p = 0.030$; $r_{\text{SRep4-Srep8}} = -0.36$, $p = 0.009$). **Figure 5B** depicts the correlations based on non-standardized variables separately for each sub-experiment. Generally, both experiments yielded a similar

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[3] As a side note, it might be of interest that both error rates as well as RTs reached similar asymptotic levels for Exp. 1 (trial-and-error learning) and Exp. 2 (instruction-based learning).

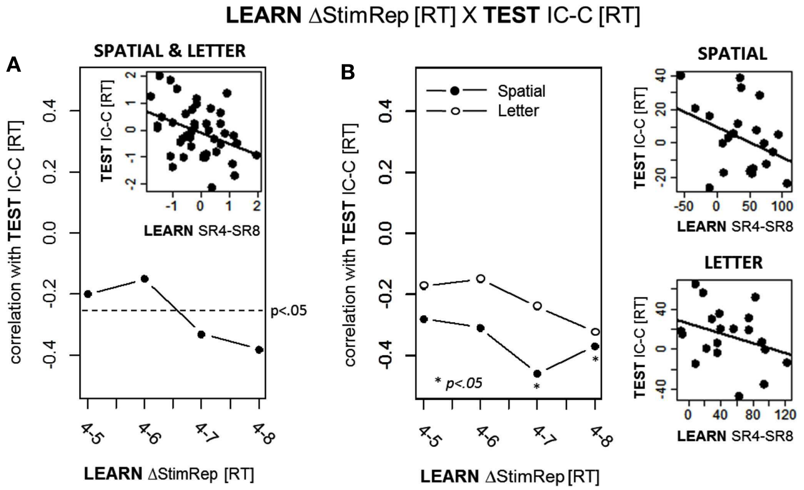

FIGURE 5 | Correlations between response times (RT) during ongoing learning and RTs during the subsequent response-effect (R-E) compatibility test in Exp. 2A and 2B. Ongoing learning is expressed in terms of the RT difference between Stimulus Repetition (StimRep) 4 and the subsequent StimRep levels (i.e., ΔStimRep). R-E compatibility is expressed in terms of the RT difference between incompatible (IC) and compatible (C) test trials. **(A)** depicts the results of the correlational analysis across both sub-experiments. **(B)** depicts the results of the correlational analysis separately for each sub-experiment (SPATIAL and LETTER).

pattern of negative correlations, reaching significance in Exp. 2A (spatial) for SRep4 – SRep7 ($r = -0.46$; $p = 0.0135$) and for SRep4 – SRep8 ($r = -0.37$; $p = 0.044$; note that this latter correlation would not reach significance with a two-sided test). However, a direct comparison of these correlations (after Fisher-*z* transformation) between Exp. 2A and 2B did not yield significant differences.

DISCUSSION

Generally, Exp. 2 replicated the correlation between learning-related response slowing and the R-E compatibility effect already observed in Exp. 1. However, the detailed time course of this correlation differed between experiments. Specifically, the size of the correlation *decreased* with learning in Exp. 1 whereas it *increased* in Exp. 2. While these different patterns might not be overly surprising given the procedural and analytical differences (i.e., trial-and-error vs. instructed; different reference SRep levels), some elaboration seems warranted. In particular, the diverging results might be suited to clarify whether learning-related response slowing directly indicates the strengthening of (S-)R-E associations (i.e., the process of association formation itself) or rather the "active" usage of increasingly stronger (S-)R-E associations. We propose that the results support the latter account for two reasons. First, the terminal associational strength after learning seems to be the same for Exp. 1 and Exp. 2 as suggested by the finding that the R-E compatibility effect did not differ between both experiments (tested via identical procedures). Second, the number of distinct learning trials (i.e., the number of co-occurrences for a particular S-R-E triple at each stimulus repetition level) was the same (i.e., 8) for Exp. 1 and Exp. 2 (as erroneous responses were never followed by an effect sound). Together this suggests that also the time course of associational strengthening across consecutive stimulus repetition levels can be expected to be similar in Exp. 1 and Exp. 2. Hence, the diverging time course of the correlations is unlikely to be associated with associational strengthening itself. Rather, it appears reasonable to assume that it reflects differences in the active usage of these associations. Why exactly the active usage might occur at different points in time in Exp. 1 and Exp. 2 remains unclear and requires additional experimental work. Finally, separate assessments of Exp. 2A and 2B revealed that both sub-experiments show a trend for negative correlations for later SRep levels, but this trend was numerically stronger for Exp. 2A and reached significance only for Exp. 2A. While this numerical difference between Exp. 2A and 2B could not be confirmed statistically, it still seems conceivable that the more demanding instructional S-R mapping used in Exp. 2B (i.e., letters) vs. Exp. 2A (i.e., spatial) might indeed absorb cognitive resources that could otherwise be devoted to the "active" incorporation of action effects during the learning phase. Hence, it might be worth pursuing this issue more systematically and with increased statistical power (which is clearly lacking for the between-subjects comparison of Exp. 2A and 2B).

GENERAL DISCUSSION

The present series of experiments aimed to establish whether, and if so, in which specific way response-contingent DO or effects might be "actively" integrated into action planning during an early phase of (S-)R-E learning (i.e., prior to considerable automatization or overlearning). We did that by investigating the relationship between performance indices of ongoing (S-)R-E learning and behavioral measures of post-learning "passive" R-E priming[4]. Exp.

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[4] As a side note, it should be stressed that we consistently observed passive effect priming effects (i.e., R-E compatibility effects) in both experiments. This replicates an earlier observation made with a different priming procedure (Wolfensteller and Ruge, 2011) and provides further evidence that (i) R-E association can be rapidly learned when action selection takes place in a stimulus-based mode and (ii) the

1 compared S-R trial-and-error learning under DO conditions vs. CO conditions. The results suggest that DO are "actively" integrated into action planning and that this takes additional planning time as indicated by relative response slowing in terms of decreased learning performance *slope* in DO vs. CO learning blocks. This finding was replicated in Exp. 2A and 2B where novel S-R mappings were learned via instruction rather than by trial-and-error. Importantly, it seems important to emphasize that in Exp. 1 R-E compatibility was exclusively associated with a DO-specific decrease in learning performance *slope* but not with the relative DO vs. CO difference regarding absolute performance *level*. As elaborated extensively in the discussion of Exp. 1, this implies that mean RT differences between DO and CO conditions during learning might to some extent also reflect unspecific DO-related side effects. Specifically, increased perceptual load in the DO blocks might result in distraction from the primary task which might possibly result in mean response slowing for DO vs. CO blocks. Yet, this does not seem to be functionally related to the acquired strength of (S-)R-E associations[5].Additionally, DOs might be more salient than COs which might imply stronger phasic alerting effects in the subsequent trial which might cause faster mean RTs in DO than in CO trials. Again, this potential DO-related RT speeding effect does not seem to be functionally related to the acquired strength of (S-)R-E associations.

We propose that the correlational results can in particular potentially clarify an important theoretical issue. It is entirely unclear whether newly acquired (S-)R-E associations should affect overt choice behavior in situations where the primary S-R learning task is rather easy and hence, would not decisively benefit from additional action retrieval cues in form of anticipated effects (see further below). Importantly, this question cannot simply be answered by demonstrating post-learning passive priming effects as expressed in the R-E compatibility effect. While the compatibility effect shows that R-E associations were learned, it does not tell whether these associations were already integrated in action planning during initial learning. This differentiation is not trivial as the retrieval of R-E associations triggered by direct perceptual input (i.e., the former E serving as the imperative stimulus in the test phase) does not automatically also imply that effect representations are activated through *anticipation* during the preceding learning phase (de Wit et al., 2009). By relating learning-related and test-related behavioral indices, the present study addressed and positively answered this question. Moreover, we think that it is also not trivial to show that newly acquired (S-)R-E associations affect response times negatively rather than positively. Possible theoretical implications of this aspect are discussed further below.

Next, we will critically evaluate these findings with regard to the existing literature. First we will discuss how our results relate to previous findings that also support the notion that anticipated outcomes or effects play an active role during action planning. The classical DO paradigm has demonstrated – mostly in lower animals, young children, and mentally handicapped persons – that the rate of trial-and-error S-R learning is initially higher under DO vs. CO conditions (Trapold, 1970; Mok and Overmier, 2007; Noonan et al., 2011) suggesting an active role of stimulus-based effect anticipation early in learning. The present study differs in three important aspects from this classical approach. First, the typical DO results have been obtained with incentive outcomes as compared to non-incentive outcomes used in the present study. Second, the typical DO results refer to error rates rather than response times. Notably, this also includes the few DO studies conducted in healthy adult human subjects (Mok and Overmier, 2007; Noonan et al., 2011). This exclusive focus on error rates might be related to the choice of quite challenging learning problems. While this is suited to create a slow and gradual decrease in error rates – hence increasing the potential benefit of DOs – it might at the same time imply rather noisy RT data especially in the initial learning phase where DOs have been shown to exert strongest impact on error rates. By contrast, the primary S-R learning task in the present study was comparably easy resulting in an atypically rapid decline in error rates. Not surprisingly, under these circumstances we could not detect a significant impact of DOs on error rates. Instead, DOs affected RTs. This opens a question that has not been directly addressed before, namely whether the presence of DOs should be expected to exert a positive or negative impact on RTs (see further below).

But why, in the first place, would we be interested in examining how DOs affect performance in the context of atypically easy learning problems, and hence, evaluate response times instead of error rates? The reason is that we wanted to make sure to examine the impact of DOs before any considerable automatization or overlearning of (S-)R-E associations could be expected. Therefore we restricted the number of specific S-R-E pairings to no more than eight, which is well below the number occurring for difficult learning problems. This decision was partly led by the suspicion that the typical DO studies might fail to observe significant RT effects not only in early, highly error-prone phases of learning where the accuracy-related DO effect is maximal, but also in later phases where error rates have stabilized at low asymptotes and do no longer differ between DO and CO conditions. While early in learning high RT noise levels due to high error rates might easily mask potential DO-related RT effects, the same does *not* hold for later learning phases. Hence, the suspected absence of DO-related RT effects after more extended practice (together with the typically reported absence of effects in accuracy) might in fact suggest a diminishing engagement of goal-directed control with extended practice. In the light of an extensive body on instrumental learning literature, such a conclusion is consistent with the notion that goal-directed control of action is transitioning into stimulus-based control of action already after comparably modest amounts of practice (Killcross and Coutureau, 2003; Atallah et al., 2004; Seger and Spiering, 2011). Accordingly, by strongly limiting the learning duration in the present experiments, we reasoned that the newly formed (S-)R-E associations would be actively used for action planning. Indeed, our results did confirm this expectation, as detailed above. However, it would be

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successful acquisition of R-E associations can be detected via the effect-priming rationale when test phase actions are selected in a stimulus-based action mode. A possible reason for different findings (Herwig et al., 2007; Pfister et al., 2011) might be that we investigate (S-)R-E learning in a very early phase of practice, hence precluding strong habitualization effects.

[5] See Discussion of Exp. 1 for an account that would assume amplified (S-)R-E learning due to increased unspecific DO-related distraction.

premature to extrapolate that the RT slowing effect would have vanished after more extended practice in the present experimental paradigm, as predicted by the instrumental learning literature. Interestingly though, Exp. 1 indeed suggests a decline of (S-)R-E usage already across the rather short learning phase as indicated by a decreasing correlation between RT slowing and RE compatibility effect. By contrast, Exp. 2 seems to suggest exactly the opposite. Hence, this issue needs to be clarified by future experiments. This seem particularly warranted in the light of results from the natural R-E compatibility paradigm (Hommel, 1993; Kunde, 2001; Kunde et al., 2004) that seem to directly contradict the hypothesis that only weakly practiced (S-)R-E associations are integrated into active action planning. Specifically, it has been shown that maximally over learned R-E associations (e.g., forceful button press – loud sound) interfere with modestly practiced R-E associations acquired within a session that are incompatible with the natural mapping (e.g., forceful button press – low sound). These and results by Ziessler and Nattkemper (Ziessler et al., 2004; Ziessler and Nattkemper, 2011) have been interpreted to reflect the active integration of *anticipated* action effects into action planning. A possible re-conciliation might be that the experienced incompatibility between natural effects and newly introduced reversed effects triggers a switch back to a goal-oriented action mode. Note that outcomes are always compatible in the classical DO paradigm, hence precluding the "forced" adoption of a goal-oriented action mode. However, this hypothesis still needs to be directly tested.

Next we discuss possible explanations for why active effect-based action planning was associated with response slowing instead of response facilitation in the present study. Intuitively and contrary to the actual results, a speed-up of response times under DO vs. CO conditions might seem more plausible. Such an expectation might be implied by the idea that the anticipation of a specific DO provides just another valid retrieval cue for the currently required response in addition to the antecedent stimulus cue. Hence, the correct response code is activated "twice" which implies that the response threshold is reached earlier than under CO conditions where this additional retrieval cue is absent. This would be consistent with results from the passive effect priming procedure suggesting that RTs are shorter for compatible effect primes as compared to neutral primes (Ziessler et al., 2004). Clearly, however, at least for weakly practiced (S-)R-E associations, this scenario is not supported by the present results. Instead, the observed DO-related response slowing might indicate that effect-based action selection should be conceptualized as an additional time-consuming process which delays response execution. Importantly though, this scenario only makes sense under the assumption that response execution is waiting for this additional process to transmit its output. Otherwise, based on stimulus-based response selection alone, the response threshold would be reached at exactly the same time for DO as for CO conditions. A parsimonious explanation for this additional "waiting time" could be that the response threshold is elevated under DO conditions because the "system" resides in a more controlled "goal-directed" mode under DO conditions (cf., Botvinick et al., 2001).

In conclusion, we speculate that effect anticipation plays an integral part in action planning even when it could solely rely on the antecedent stimulus. Importantly, this may be especially true early in practice, that is, before habitualization kicks in. Consistent with this view, relative response slowing under these circumstances indicates that effect-based action planning is a non-automatic process that may be different from the mechanisms that mediate the influence of effect representations after intensive practice. Furthermore, it will be especially important for future research to decide more clearly whether learning-related response slowing under DO conditions reflects either (S-)R-E learning itself or rather the active usage of these newly formed associations, as we would tentatively propose.

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What I say is what I get: stronger effects of self-generated vs. cue-induced expectations in event-related potentials

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Expectations regarding future events enable preparatory processes and allow for faster responses to expected stimuli compared to unexpected stimuli. Expectations can have internal sources or follow external cues. While many studies on expectation effects use some form of cueing, a direct comparison with self-generated expectations involving behavioral and psychophysiological measures is lacking. In the present study we compare cue-induced expectations with self-generated expectations that are both expressed verbally in a within-subjects design, measuring behavioral performance, and event-related brain potentials (ERPs). Response time benefits for expected stimuli are much larger when expectations are self-generated as compared to externally cued. Increased amplitudes in both the N2 and P3 components for violations of self-generated expectations suggest that this advantage can at least partially be ascribed to greater perceptual preparation. This goes along with a missing benefit for stimuli matching the expected response only and is mirrored in the lateralized readiness potential (LRP). Taken together, behavioral and ERP findings indicate that self-generated expectations lead to increased premotoric preparation compared to cue-induced expectations. Underlying cognitive or neuronal functional differences between these types of expectation remain a subject for future studies.

Keywords: self-generated expectations, cue-induced expectations, event-related brain potentials, N2, P3, lateralized readiness potential

INTRODUCTION

Expectations play a crucial role in action control. Research on effect-based action control has stressed that representations of anticipated action effects play a role when performing an action (e.g., Nattkemper et al., 2010). According to the ideo-motor principle (see Shin et al., 2010, for a recent review) the mental representation of an anticipated action effect triggers the action (similar to forward and inverse computational models of motor control, e.g., Wolpert and Ghahramani, 2000). For instance, the representation of an open drawer might help us to initiate the pulling action. By choosing actions according to the anticipated effects, people can gain intentional control over their behavior (e.g., Kunde, 2001; Pfister et al., 2010). They can consider expectations about upcoming action effects for choosing between actions depending on which effects they desire or not. As such, expectations about effects stem from goals of the actor. They might not be directly caused by current external stimulation, but rather be self-generated by integrating goals and past external stimulation. Interestingly, this view often does not directly translate to the methodology of experiments on the role of action effect anticipation in action control. For instance, the role of anticipated effects has been studied by presenting action effects additionally as subliminal stimuli (e.g., Kunde, 2004) or irrelevant flankers (e.g., Zießler and Nattkemper, 2002). One could argue that presenting to-be-expected effects as stimuli might trade experimental control against external validity, as such a situation is not closely resembling action preparation driven by self-generated expectations. Conceivably, intentional action control supposes self-generated expectations. These are likely to interact with stimulus-based preparation but are unlikely identical to this. For instance, according to Kunde et al. (2007) actors use anticipated action effects based on internal goals. Yet, stimuli have an important role in this view, too. They disambiguate situations as to whether or not an effect can be brought about by an action. As many actions only lead to the desired outcomes in highly specific contexts, the role of a stimulus is to signal that in the current context the link between expected effect and action is valid.

Taken together, this reasoning might suggest that the presumed equivalence between self-generated expectations and cue-induced expectations cannot be taken for granted. It is also conceivable that self-generated expectations differ from expectations that are directly triggered by external stimuli or cues. A similar distinction has been discussed with respect to internally triggered vs. externally cued task switching (Arrington and Logan, 2005). Differences between expectations based on external cues and internal sources are also conceivable given the long history of debates concerning motor patterns that are predominantly stimulus-triggered vs. predominantly driven by a response goal. For instance, the Baldwin–Titchener debate at the end of the nineteenth century (e.g., Baldwin, 1895; Titchener, 1895) centered around the question of whether or not response times (RTs) are regularly shorter when people concentrate on the response rather than on expecting the stimulus. An important insight of that debate was that people can apparently choose between different modes for controlling the same motor pattern.

In line with these precursors, recent results point to differences between intentional vs. reactive action. Surprisingly, a motor pattern already triggered by an internal goal is incompatible with the execution of the very same motor pattern in response to a stimulus presented while the intentional action is in preparation (e.g., Astor-Jack and Haggard, 2005; Pfister et al., 2012). If an internally prepared action is truncated by a stimulus that requires the same action that was intentionally prepared, RT costs result in comparison to a situation where the response could be executed without concurrent intentional preparation. The authors interpret their results as evidence for distinct action systems that are triggered either endogenously by intention or exogenously by an imperative stimulus. Presenting the stimulus during intentional action preparation therefore results in interference between both systems and delays the action. In line with these results, Herwig et al. (2007) have differentiated two types of action control modes, a stimulus-based action control mode and an intention-based action mode. Pfister et al. (2011) have shown that previously acquired action effect associations either impact performance or not, depending on which of these two modes is operating. One can of course debate what exactly differentiates the intention-based from stimulus-based action mode (e.g., Neuringer and Jensen, 2010), however, empirical data highlights that different paths to action do exist.

While our current study is inspired by recent work on effect-based action control, we focus on distinguishing between self-generated vs. cue-induced stimulus expectations. Such a focus is feasible given that theories on integration of perception and action (e.g., Hommel, 2009; Magen and Cohen, 2010) suggest that action effects and stimuli share the same representational basis. Studying self-generated vs. cue-induced expectations is driven by the conjecture that anticipating appropriate environmental conditions in order to prepare for efficient goal-directed actions is one of the core abilities of our neurocognitive system (e.g., Kunde et al., 2007). Anticipation, prediction, and expectancy are only some of the labels used to discuss such mechanisms (e.g., Sutton and Barto, 1981; Elsner and Hommel, 2001; Jentzsch and Sommer, 2002). Here we use the term expectation in a broad sense, encompassing both the process of expecting as well as the object of this process. Expectations can originate from prior experience, when events occurring with a high frequency in the past are expected to be more likely to occur again in the future (e.g., Fitts et al., 1963). Expectations may as well rest upon situational cues that provide advance information about upcoming events (e.g., Posner and Snyder, 1975). Whatever the source, performance is usually boosted when the expected event occurs, whereas unexpected events impair performance (e.g., Acosta, 1982).

Previous studies of expectation have often exclusively relied on the use of external cues (e.g., Shulman et al., 1999; Oswal et al., 2007). Cueing allows a more rigid experimental manipulation of the induced expectations as compared to a setup with self-generated expectations. However, before jumping to the conclusion that cueing should be used to study expectation in general, potential functional differences between endogenous and exogenous expectations should be scrutinized. To our knowledge, the only direct comparison of self-generated and cue-induced expectations was carried out by Acosta (1982). In a series of experiments he pitted predictions verbalized by participants against cues (words that announced a certain stimulus and were to be read aloud). As he included neutral expectations as a control, he could differentiate the facilitation of correct expectations from the cost of a wrong expectation. Furthermore, he manipulated the expectation-target interval and found effects of the interval duration in the prediction condition for both benefits of matches and costs of mismatches. Benefits increased with longer expectation-target intervals while costs were highest at the shortest intervals. The effects were generally much smaller in the cue condition. Mismatch costs were also highest at the shorter intervals while no significant benefits for matches of cue-induced expectations were found. In a second experiment he manipulated the number of the response alternatives by mapping more than one stimulus to a response. The expectation effect did not increase linearly with the number of alternative responses, indicating that the process responsible for expectation effects is not just a scaling effect in choosing between the possible alternatives to predict. Moreover, his findings suggested that expectation effects were bound to stimulus processing rather than to response processing. As multiple stimuli were mapped to the same response, an expectation concerning a stimulus could be violated while the response to be executed was the same that would have been appropriate in case of a stimulus matching the expectation. Responses in such trials were as slow as those to unexpected stimuli with a different response. This suggests that the expectation effect is not (solely) a part of response execution.

Comparing different behavioral effects of self-generated vs. cue-induced expectation, Acosta (1982) concluded that the types of expectation differed only in the magnitude of their effects but not qualitatively. It therefore appears expedient to study self-generated vs. cue-induced expectations with respect to their effects on action preparation including neural measures that are more independent of the overt responses and could better differentiate quantitative from qualitative effects. In the current study we aimed to replicate the behavioral findings of Acosta (1982), showing stronger effects of self-generated compared to cue-induced expectations. Moreover, we used event-related brain potentials (ERPs) to further distinguish the contribution of different cognitive processes to expectation effects in these two conditions. This includes potential differences between the two types of expectation prior to stimulus presentation. Qualitative differences in preparatory activity would be in accordance with theories that assume different routes to action (e.g., Astor-Jack and Haggard, 2005; Kunde et al., 2007; Pfister et al., 2011).

Explicit self-generated expectations about upcoming stimuli measured on a trial-by-trial basis (through verbalization) have not been a focus of recent research. To analyze the processes during the build-up of the expectations and response preparation, we used EEG recordings. There are two main questions we wanted to address with this study. First, do differences between the expectation types already exist prior to stimulus presentation? Second, which cognitive processes (perception, action selection, motor preparation) are influenced by expectation? More specifically, do self-generated expectations affect other processes than cue-induced expectations (qualitative differences between the expectation types) or affect the same processes but with a different magnitude (quantitative differences)?

We manipulated the type of expectation within-subjects. In the prediction condition participants had to verbally express their expectation regarding the upcoming stimulus, in the cue condition they had to read aloud a word naming the upcoming stimulus. Stimuli were simple shapes or colors. The task was then to react as fast as possible to the imperative stimulus with the right or left index finger. Since there were four stimuli, with two mapped to each finger, three types of matches or mismatches existed. First, for *stimulus matches* the expected (cued or predicted) stimulus matched the upcoming stimulus. Second, for *response matches* the expected stimulus did not match the upcoming stimulus but required the same response. Third, for *mismatches* the expected stimulus and the upcoming stimulus were different and did not require the same response either.

In addition, we included a manipulation of stimulus frequency. The two stimuli mapped to each finger were shown with different frequencies, at either 33 or 17% of all trials. Both hands had to respond equally often. The frequency manipulation was included to guide the participants' predictions and to provide a measure indicating whether participants base their predictions on their experience (instead of random guessing). In a similar paradigm, Umbach et al. (2012) have shown that participants closely match their stimulus predictions to the observed frequencies. Even though expectations in their study were not valid in predicting the stimulus (similar to the current study) participants nonetheless used these expectations in preparing their responses.

The role of expectation in action preparation can be studied by comparing trials in which upcoming stimuli fulfill vs. do not fulfill expectations in behavioral measures (RTs and errors, e.g., Acosta, 1982) or with regard to effects in the brain that can for instance be measured by EEG (e.g., Matt et al., 1992; Jentzsch and Sommer, 2002). There are multiple processes that can lead to the expectation mismatch effects. It is possible that a correct expectation (a) facilitates the encoding of the stimulus, (b) the response selection, (c) response execution, or a combination of these. It is also possible that an expectation that does not match the stimulus delays one of these processes, or else that both – fulfilled and unfulfilled expectations – have opposing effects. Time differences in RTs and the latencies of the different ERPs which occur during the different stages prior to the response can help to show the stage(s) where the expectations exert their influence. ERP amplitudes can provide information about the magnitude of the involved processes in the different conditions.

CONTINGENT NEGATIVE VARIATION

To investigate whether there is a difference of cue-induced vs. self-generated expectation even before the stimulus is shown, we charted the contingent negative variation (CNV). This is a slow negative potential following an event cueing the upcoming target stimulus (inducing expectations in our case). The CNV develops in the cue-target interval and its amplitude is most pronounced directly before onset of the imperative stimulus. Depending on task demands, the late phase of the CNV reflects sensory, cognitive, or motor preparation (Damen and Brunia, 1994; Fan et al., 2007). Acosta (1982) has shown stronger RT effects in self-generated as compared to cue-induced expectations. A possible cause of this difference may be that the internal generation of expectations results in a larger amount of specific preparation that could, consequently, show up in a more pronounced CNV in the prediction condition.

N2

The N2 is an ERP characterized by a larger amplitude in cases where the stimulus deviates in form or context from the prevailing stimulus (for a review, see Patel and Azzam, 2005). The N2 is also larger in response conflict trials as evoked by incongruent flanker or no-go trials (Kopp et al., 1996). Therefore, we explored whether mismatch between either kind of expectation and the upcoming target would result in an enlarged N2 amplitude. Larger interference effects in the N2 have been demonstrated in the Eriksen flanker task with a greater proportion of incongruent trials (Tillman and Wiens, 2011). As the interference effect on RTs was smaller in this condition, the N2 might reflect endogenous attention processes. If we assume that self-generated expectations have a stronger influence on preparatory processes (e.g., attention), the violation of an expectation might result in a larger N2 effect in the prediction condition compared to the cue condition.

P3

Matt et al. (1992) and Jentzsch and Sommer (2002) differentiated between passive and active forms of expectations. While passive expectations automatically affect behavior, active expectations act in a rather controlled manner (Kahneman and Tversky, 1982). Matt and colleagues induced active expectations through instruction ("Expect stimulus repetitions!" "Expect stimulus alternations!") in a blockwise manner. P3 amplitude as well as RTs revealed the higher order repetition effects typically found in simple reaction time tasks (stimulus repetitions benefit if they continue a run of repetitions, alternations if they continue a run of alternations). Importantly, the RT effect but not the P3 effect was modulated by the instructed expectation (expecting repetitions reduced the sequential effect for repetitions and increased that for alternations, and vice versa). This dissociation suggests that active and passive forms of expectation differentially affect processing stages involved in performing the task but might not show up in the P3.

However, operationalization of active and passive forms of expectation differed between Matt et al. (1992) and the current study. In contrast to their experimental approach, self-generated expectations in the current study were allowed to change on a trial-by-trial basis and were induced by stimulus frequency. Self-generated expectations might lead to stronger P3 effects as compared to cue-induced effects, because generating expectations internally trial-by-trial might lead to more pronounced processing of the expectation as compared to reading a cue. If one considers the relation of stimulus to expectation (rather than considering the stimulus alone), there are various possibilities for P3 effects. On the one hand, it is possible that the P3 relates to expectation by capitalizing on stimulus probability. In the current design, expectations often mismatch the actual stimuli. Even if a participant exclusively relies on the frequent stimulus, expectation matches are rare. Therefore, upon stimulus presentation, a P3 could follow in case of matches. On the other hand, P3 may reflect the accuracy of a concrete expectation on a single-trial basis rather than reflecting the past frequency of this expectation being fulfilled. In this case, a stimulus mismatching the expectation should elicit the higher P3 amplitude.

LATERALIZED READINESS POTENTIAL

At the other end of the processing stream, the lateralized readiness potential (LRP) can be used to infer the role of response preparation in expectation effects (e.g., Jentzsch and Sommer, 2002). The LRP is a difference waveform that arises with a higher activity in the motor area of the brain hemisphere contralateral to the responding hand as compared to the ipsilateral hemisphere (Coles, 1989). The onset of the stimulus-locked LRP (S-LRP) provides a chronometric index of premotor processing stages (Leuthold et al., 1996) while onset differences in the response-locked LRP (LRP-R) indicate processing differences at late motor-related stages (Hackley and Valle-Inclán, 1998). Jentzsch and Sommer (2002) found that S-LRP was significantly influenced by the expectations, while the LRP-R was not. This shows that the instructed expectation influenced a process after early stimulus processing (as P3 was not affected in this study) but prior to the response initiation. Accordingly, we assumed expectation effects specifically on the S-LRP that should be particularly strong in case of self-generated expectations.

INFLUENCES OF STIMULUS FREQUENCY

While the main focus of our experiment lay on the comparison of cue-induced vs. self-generated expectations, the variation of stimulus frequency we applied also needs to be briefly summarized. Obviously the experimenter has little control over expectations self-generated by participants. By varying stimulus frequency it should be possible to partly shape self-generated expectations and to be able to explore how self-generated expectations accommodate to the task environment (see Umbach et al., 2012). Specifically, reliance on stimulus frequency can be considered a sign of subjective validity of the self-generated expectations that participants are asked to verbalize. Furthermore, the more frequent stimuli should lead to faster responses as compared to less frequent stimuli. Potential effects of stimulus frequency may in part be independent of expectation match effects in the current trial (compare Jiménez and Méndez, 2012). Conceivably, stimulus frequency leads to a sustained effect more similar to the passive form of expectation that Matt et al. (1992) found reflected in the P3. We expected larger P3 and N2 components for infrequent as compared to frequent stimuli.

Furthermore, the CNV is seen to reflect preparatory processes and the amplitude is, for example, modulated by cue validity (if the upcoming stimulus is specified with different probabilities). CNV amplitude is larger the more valid the cues (and thus, the more expected the stimuli) are (Scheibe et al., 2009). We therefore expect a larger CNV for the expectations of frequent stimuli since these are more likely to be fulfilled (33 vs. 17% validity).

MATERIALS AND METHODS

PARTICIPANTS

Eighteen participants (four men) with a mean age of 24.7 years took part in the experiment. All Participants were right-handed and had normal or corrected-to-normal vision. The participants were either psychology students at Humboldt-Universität zu Berlin and participated in exchange for course credit or received a compensation of €20 for the experiment with a duration of approximately two and a half hours. Participants gave their informed consent prior to the experiment.

APPARATUS AND SOFTWARE

The Experiment was programmed with MathWorks MATLAB and the Psychophysics Toolbox (Brainard, 1997; Pelli, 1997) and presented on a Windows computer. The participants' expectations were recorded using a table microphone and played to the experimenter who coded the predictions on a separate computer outside the EEG booth. Error feedback after erroneous responses was given via tabletop speakers.

STIMULUS MATERIAL AND EXPERIMENTAL MANIPULATION

The stimuli were either simple shapes (house, star, cross, and gate) or colored circles (blue, red, green, and yellow) presented on a computer monitor with a light gray background. Stimuli were 22 mm in diameter, corresponding to a visual angle of about 2.1° at a viewing distance of approximately 60 cm. The experiment consisted of two parts: a cue-induced (cue condition) and a self-generated expectation variant (prediction condition). One of these parts was performed with colors as stimuli and the other with shapes. The order of the expectation variants as well as the assignment to the two types of stimuli was randomized across participants.

In trials of the cue condition, the participants were presented with the one-syllable word for one of the stimuli, which they were instructed to read aloud (the German equivalents for house, star, cross, and gate in the shape condition, or the German equivalents for blue, red, green, and yellow in the color condition). In the prediction condition they saw a prompt – the German equivalent for "color?" or "shape?" – to which they should respond by naming the stimulus they expected to appear in the current trial. Thus, verbal output consisted of the same words in both expectation conditions.

Participants had to react to the stimuli by pressing one of two buttons with either the left or the right index finger. Depending on the current type of stimuli, each button corresponded to two forms or two colors. The mapping was randomized, shown before the experiment and was trained during two training blocks. Of the two stimuli per hand, one was presented in one out of three trials (33% = frequent stimuli) and the others in one out of six (17% = infrequent stimuli; half as often as the frequent stimuli), together resulting in the same frequency (50%) of responses with each hand. The order in which the stimuli were presented was randomized. In the cue condition, the frequency of the cues was matched to the frequency of the stimuli (cues for frequent stimuli were shown in 33% of the trials, cues for infrequent stimuli in 17%). The task is shown in **Figure 1**.

TASK PROCEDURE AND INSTRUCTIONS

After being introduced to the lab and the experimental procedure, participants provided their consent to participate and were seated in a one person lab room and prepared for the EEG measurements. Next a detailed explanation of the task in the following

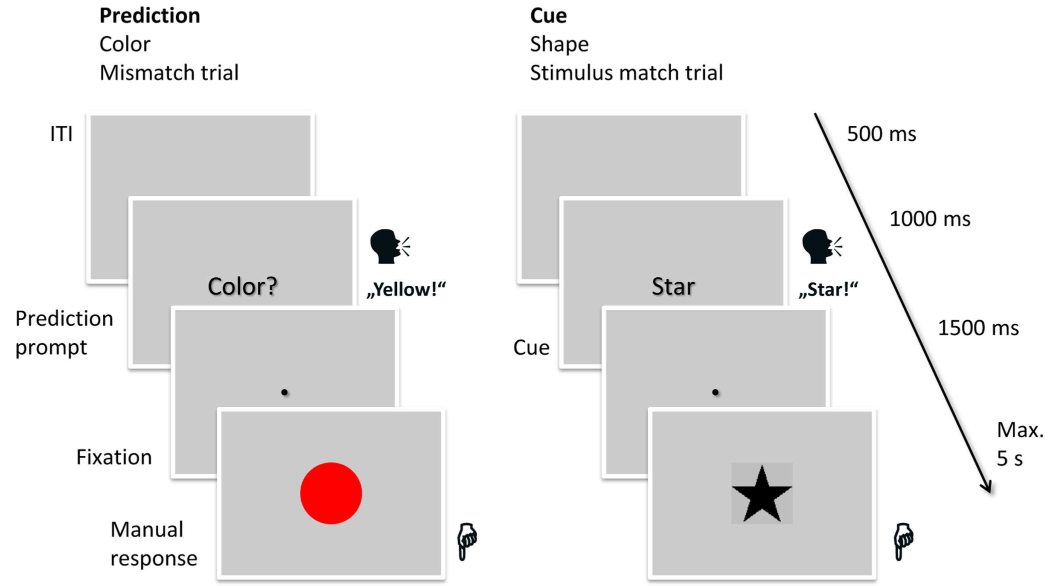

FIGURE 1 | Task used in the experiment. On each trial, participants either had to read aloud the cue (in this case "star") or to verbalize their prediction for the upcoming stimulus (in this case "yellow"). After 1,000 ms the fixation point appeared on screen and after another 1,500 ms the stimulus appeared on screen (in this case red in the prediction trial, signifying a mismatch, and a star in the cued trial, signifying a match). The participants manually responded to the stimulus by pressing one of two keys. The next trial started 500 ms after the response. The mapping of the stimulus types (shape, color) to the expectation conditions and the order of the expectation conditions was balanced over participants.

experiment and the stimulus-response mapping was presented on the screen and also explained by the experimenter. Instructions explained the course of the trials, the response mappings and the request to relax the mouth as soon as possible after pronouncing the expectation (i.e., as soon as the fixation point presented in response to the registration of the expectation). This was included to ensure minimized muscle artifacts in the EEG measurements.

The first training block of eight trials followed. After that, a shorter version of the instructions was presented and any questions that arose during the first training block could be clarified with the experimenter. This was followed by another training block, after which the experimenter left the room and the participant could start the experiment by pressing a button. The experiment consisted of two parts, each containing five blocks of 108 trials. The length of the breaks between the blocks could be controlled by the participants. The second half of the experiment contained a switching of the stimuli and expectation condition. There were again two training blocks of eight trials each preceded by instructions explaining the new task. To minimize mistakes, the stimulus-response mapping was shown before every block. If the wrong button was pressed an acoustic error feedback was given; it was also given when no button had been pressed within 5 s following stimulus presentation.

Each trial in the experimental blocks began with the presentation of either the cue or the prompt for the expectation in the middle of the screen. After 1,000 ms, the fixation point was shown at the same point. After another 1,500 ms, the stimulus was shown until a button press was registered or for 5 s if no reaction followed during that time. This was followed by an intertrial interval of 500 ms before the next trial started with the presentation of a cue or prediction prompt.

At the end of the session participants were asked to estimate the frequency of the characteristic stimulus values.

ELECTROPHYSIOLOGICAL RECORDINGS

Recordings were made from Ag/AgCl electrodes mounted in an electrode cap (Easy-Cap) at 25 scalp positions (FP1, FP2, F3, F4, F7, F8, C3, C4, T7, T8, P3, P4, P7, P8, O7, O8, O1, O2, FPz, Fz, FCz, Cz, CPz, Pz, Oz) according to the extended 10–20 system. AFz served as ground electrode. In addition, external electrodes were used for recording the vertical and horizontal electrooculogram as well as for the mastoids. The electrodes were referenced to the linked mastoids. Electrode impedance was kept below 5 kΩ. The EEG was recorded with a sampling rate of 1,000 Hz and no online filters were applied. Blink artifacts were corrected semi-automatically by independent component analysis (ICA) using the ICA algorithm integrated in the BrainVision Analyzer 2.0 (Brain-Products GmbH). Offline, the continuous EEG was separated into individual trials with 300 ms pre- and 2,700 ms post-cue epochs (cue-locked data, in the prediction condition they were locked to the presentation of the prompt), and 200 ms pre- and 800 ms post-stimulus epochs (stimulus-locked data), and with 1,000 ms pre- and 200 ms post-response epochs (response-locked data).

DATA ANALYSIS

For data analysis, only trials with correct key presses were considered. For the CNV, the cue-locked segments were averaged according to the expectation condition (cue vs. prediction) and frequency condition (expectation corresponded to frequent or infrequent stimulus) and 30 Hz low-pass filtered. For the statistical analysis the difference between the mean voltage around the visual potential of the fixation point (1,400–1,200 ms prior to stimulus presentation) and the mean voltage 200 ms before the stimulus onset at electrode Cz was used with the baseline 200 ms before the onset of the cue or the prediction prompt. For the N2 and P3, the stimulus-locked segments were averaged according to the expectation conditions (cue vs. prediction) and match types (mismatch, response match, and stimulus match) and 30 Hz low-pass filtered (Butterworth, slope 12 dB/oct). The N2 amplitude was the mean amplitude measured at Fz between 250 and 350 ms after stimulus onset. P3 latency was measured as the time of the positive maximum at the Pz electrode during the time range of 250–550 ms following stimulus onset. The P3 amplitude was measured as the mean amplitude measured at Pz between 250 and 550 ms after stimulus onset. For both N2 and P3 the baseline was taken during a 200 ms pre-stimulus interval.

For the LRP, EEG was 5 Hz low-pass filtered (Butterworth, slope 12 dB/oct). The LRP was derived by computing difference waves for the C3 and the C4 electrodes between the electrode contralateral to the corresponding hand in a given trial and the ipsilateral electrode. Then the two types of difference waves (C3–C4 for right-hand response trials and C4–C3 for left-hand response trials) were averaged within each of the experimental conditions (cue mismatch, cue response match, cue stimulus match, prediction mismatch, prediction response match, prediction stimulus match). LRP onsets were analyzed using a jackknife-based procedure for factorial designs (Ulrich and Miller, 2001). Eighteen different grand average LRPs for each of the experimental conditions were computed by omitting the ERP data of one participant from each grand average. This allowed to measure the usually noisy LRP onsets much more precisely than on a single participant. LRP onsets were measured in the waveform of each grand average and submitted to an ANOVA with F-values corrected as $F_c = F/(n-1)^2$, with F_c as the corrected F-value and n as the number of participants. S-LRP onsets were measured with a 200 ms pre-stimulus baseline and LRP-R with a 100 ms baseline, starting 100 ms after the responses were made. As Miller et al. (1998) recommended, we used a relative criterion of 50% of the maximal LRP amplitude during the recording epoch for determining the LRP onsets for both the S- and the R-locked LRPs.

SPEECH ARTIFACTS AND VERBALIZATION LATENCY

The participants were asked to verbalize their expectation as soon as the prompt or cue was shown and to relax their facial muscles again as soon as the fixation point was shown. The EEG data acquired during the time of speech was not analyzed. The earliest data points used in the analysis were in the Cz amplitude (CNV), starting 100 ms after the presentation of the fixation point, which should render enough time for artifacts from muscles involved in the prior speech production to subside. Visual inspection of the microphone recordings showed activations in the frequency range of speech primarily prior to the presentation of the fixation point. In addition, the stimulus types and their mapping to the expectation condition and frequencies were randomized; thus their verbalization should not have been able to systematically influence any EEG measurements. Furthermore, participants were instructed to use the relatively long interval between the prompt or cue and the fixation point for blinking if necessary.

Analyzing processing differences with chronometric measures (as comparing ERP latencies) presumes equivalent starting points of the processes of interest. In our case it is assumed that possible preparatory processes start with the verbalization of either the prediction or the cue, respectively. Possibly, however, it is harder to generate a prediction than to read a cue. If, because of this, predictions are verbalized later than cues that have simply to be read aloud, preparation, on the one hand, may start later in the prediction condition and, on the other hand, the distance in time between the verbalization and the imperative stimulus would be shorter for predictions than for cues. Both influences would make a comparison of the time courses of the prediction and cue conditions problematic. Being aware of these difficulties we conducted a behavioral pilot study with the same materials that allowed a precise measurement of voice onset times. Moreover, anticipating possible differences in verbalization latency, we locked the time of stimulus presentation in this pilot study to voice onset time rather than using a fixed interval between prompt/cue and stimulus as in the main study reported here. The stricter controlled pilot study revealed the same behavioral effects of expectation as the EEG study. Importantly, we found no difference in verbalization latency between predictions and cues (though the different standard deviations may mirror a processing difference between producing one and the same word as a prediction or by reading)[1] and decided for a fixed interval between prompt/cue and stimulus in the main study in order to avoid problems with incompatibilities between speech recognition and precise EEG recording.

RESULTS

EXCLUSION OF DATA

Training blocks were not analyzed. Furthermore, error trials were excluded from the RT and EEG analyses. Trials in which the participants had not reacted after 5 s were counted as error trials. According to this criterion 2.6% of all trials were excluded. Response time analyses were based on medians per participant and condition. Due to the experimental design, roughly twice as many mismatch trials went into the analysis compared to response matches and stimulus matches; this proportion was similar for both expectation conditions[2]. Predictions were matched by the correct stimulus in 25.9% of trials while cues were valid in 27.7%.

BEHAVIORAL DATA

Response times and error rates can be seen in **Figure 2**. RT was on average 72 ms longer for mismatches than for stimulus matches. This slowing was about the same for response match and complete mismatch trials. Match trials were also more accurate than mismatch trials. The advantage of stimulus matches was larger for the prediction condition (**Figure 2**, left; 113 ms) than for the

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[1] Voice onset time results from the behavioral pilot study; predictions: $M = 467$ ms (SD = 117.03), cues: $M = 465$ ms (SD = 49.05), $t(9) = 0.08$, ns.

[2] Number of trials, excluding errors: predictions: mismatch $M = 257$ (SD = 13); response match $M = 127$ (SD = 11); stimulus match $M = 138$ (SD = 11); cues: mismatch $M = 268$ (SD = 7); response match $M = 119$ (SD = 5); stimulus match $M = 149$ (SD = 4).

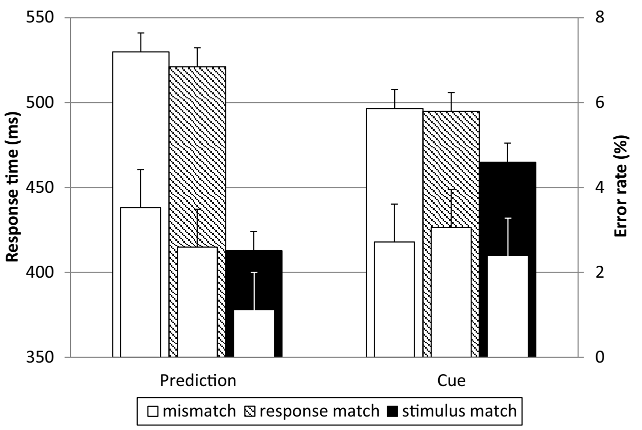

FIGURE 2 | Response times and error rates. Response times (outer bars) exhibit an effect of match (with faster responses to stimulus matches than to response matches and mismatches) and an interaction with expectation condition (with a stronger effect of match in the self-generated predictions, left). The same pattern is visible in the error rates (inner bars). There were less errors made in the match trials, thus the effect in response times cannot be explained by a speed-accuracy trade-off. Error bars represent confidence intervals (95%) for repeated-measures designs according to Loftus and Masson (1994) and Jarmasz and Hollands (2009).

cue condition (**Figure 2**, right; 31 ms). Additionally, the RTs were 17 ms shorter for the frequent stimuli compared to infrequent stimuli. A three-way repeated-measures ANOVA with expectation condition, match and frequency as within-subjects factors on the median RTs rendered a significant main effect of frequency, $F(1, 17) = 32.96$, $p < 0.001$, $\eta_p^2 = 0.66$, and of match, $F(2, 34) = 316.38$, $p < 0.001$, $\eta_p^2 = 0.95$; there was no main effect of expectation condition, $F(1, 17) = 0.06$, ns. Importantly, there was a significant interaction of match and expectation condition, $F(2, 34) = 36.78$, $p < 0.001$, $\eta_p^2 = 0.68$, with a larger difference between the two types of mismatch and the stimulus match for the prediction condition than for the cue condition. T-tests revealed that for both expectation conditions there was no significant difference between mismatch and response match [both $t(17) < 1.46$, ns], while the stimulus match was significantly faster than both [all $t(17) > 6.69$, $p < 0.001$, all $d > 3.38$]. The effect of match on the error rates was in the same direction, $F(2, 34) = 7.13$, $p = 0.003$, $\eta_p^2 = 0.30$, with less errors for stimulus matches as compared to mismatches. The effects can therefore not be explained by a speed-accuracy trade-off.

The frequency manipulation was reflected in the prediction behavior, as participants predicted the more frequent stimuli on a larger proportion of trials, $\chi^2(1) = 7.39$, $p = 0.007$. The *post hoc* estimates of stimulus occurrence in % made by the participants also provide a good approximation of the actual frequencies, with the frequent stimuli at 59%, and the infrequent stimuli at 41% (for comparison, real presentation frequencies: 66 and 33%, respectively).

CONTINGENT NEGATIVE VARIATION

The CNV was neither influenced by the expectation condition nor by the frequency. A repeated-measures ANOVA for the influence of frequency and expectation condition revealed no main effect of expectation condition, $F(1, 17) = 1.29$, ns, or of frequency, $F(1, 17) = 1.64$, ns, and no interaction, $F(1, 17) = 0.92$, ns.

N2

Figure 3 (top) shows the N2 for the prediction and the cue condition at electrode Fz. The N2 amplitude was larger for the cue condition than for the prediction condition, and in both expectation conditions the N2 was larger for mismatches and response matches than for stimulus matches. The amplitude difference of response matches and mismatches compared to stimulus matches was larger for the prediction than for the cue condition. A repeated-measures ANOVA for the effects of match type and expectation condition on the mean amplitude of the N2 measured at Fz between 250 and 350 ms revealed a main effect for match, $F(2, 34) = 15.52$, $p < 0.001$, $\eta_p^2 = 0.48$ and a main effect for expectation condition, $F(1, 17) = 39.14$, $p < 0.001$, $\eta_p^2 = 0.70$. The interaction was based on a larger amplitude difference between the different match types for the prediction condition compared to the cue condition, $F(2, 34) = 6.79$, $p = 0.003$, $\eta_p^2 = 0.29$. A three-way repeated-measures ANOVA that also included the influence of frequency on the N2 peak amplitude rendered no main effect of frequency, $F(1, 17) < 0.01$, ns.

P3

The P3 (**Figure 3**, bottom) had a larger amplitude for predictions compared to cues and for mismatches compared to stimulus and response matches. In the cue condition the full stimulus match exhibited the smallest P3 amplitude, with a higher amplitude for response matches and the highest amplitude for mismatches. In the prediction condition the pattern was more complex, with stimulus matches showing a much shorter peak latency of the P3 compared to all other conditions. A repeated-measures ANOVA for the effects of match type and expectation condition on the mean amplitude of the P3 revealed a main effect for match, $F(2, 34) = 14.16$, $p < 0.001$, $\eta_p^2 = 0.45$, a main effect for expectation condition, $F(1, 17) = 16.23$, $p < 0.001$, $\eta_p^2 = 0.49$, and a significant interaction, $F(2, 34) = 6.83$, $p < 0.003$, $\eta_p^2 = 0.29$. A three-way repeated-measures ANOVA that also included the influence of frequency on the P3 mean amplitude rendered no effect of frequency, $F(1, 17) = 0.23$, ns. There was a significant effect of match on the peak latency, $F(2, 34) = 17.20$, $p < 0.001$, $\eta_p^2 = 0.50$. A t-test revealed that this was due to the earlier P3 for stimulus matches in the prediction condition. The P3 for stimulus matches in the prediction condition began on average 85 ms earlier than for mismatches, $t(17) = 5.57$, $p < 0.001$, $d = 2.70$.

LATERALIZED READINESS POTENTIAL

The onset of the S-LRP was earlier for stimulus matches than for response matches and mismatches, mirroring the RT results (**Figure 4**, top). A repeated-measures ANOVA for the influence of match and expectation condition on the S-LRP onset rendered a main effect of match, $F(2, 34) = 24.33$, $p < 0.001$, $\eta_p^2 = 0.59$, but not of expectation condition. There was a trend toward an interaction, $F(2, 34) = 2.58$, $p = 0.090$, $\eta_p^2 = 0.13$, with a larger difference between the S-LRP onset latency for the stimulus match compared

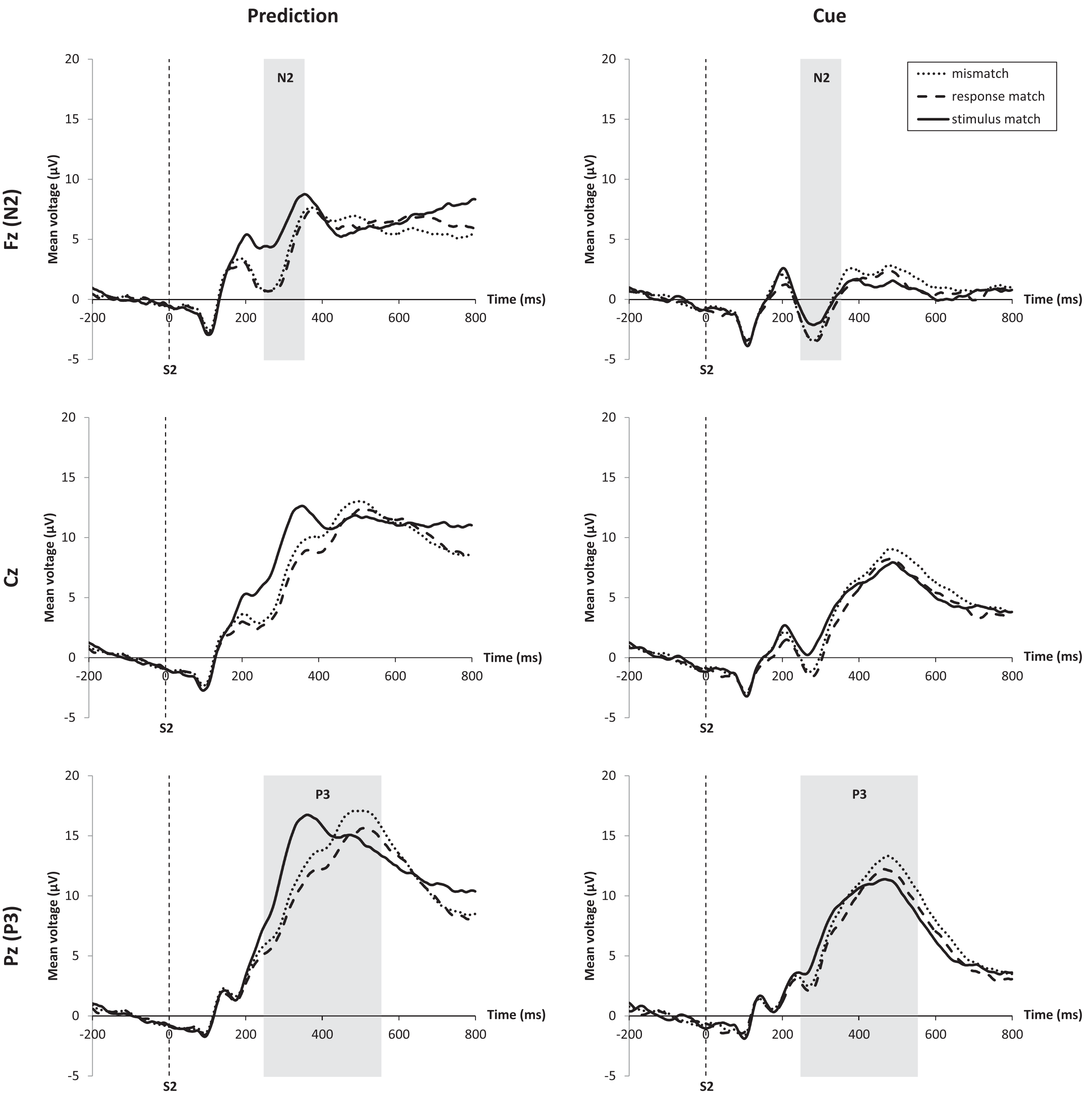

FIGURE 3 | ERPs at midline electrodes Fz, Cz, and Pz time-locked to stimulus onset. Prediction condition is shown on the left, cue condition on the right. For each electrode the different waveforms for the three match types are shown. Analysis windows for N2 and P3 components are highlighted in gray. Stimulus matches are marked by the solid line, response matches by the dashed line, and mismatches by the dotted line. The interaction of match and expectation condition can best be seen at the Fz electrode for the N2 and at the Pz electrode for the P3.

to the response match and mismatch in the prediction condition compared to the cue condition.

As can be seen in **Figure 4** (top) there was an early rise of the response match S-LRP (especially in the prediction condition) which then soon aligned with the mismatch S-LRP. According to this visual inspection we also analyzed the average S-LRP amplitude 150–250 ms after stimulus onset. A repeated-measures ANOVA for the influence of match and expectation condition on the S-LRP amplitude 150–250 ms after stimulus onset revealed a main effect of match, $F(2, 34) = 19.44$, $p < 0.001$, $\eta_p^2 = 0.53$, but not of expectation condition. There was a significant interaction of expectation condition and stimulus match condition, F(2,

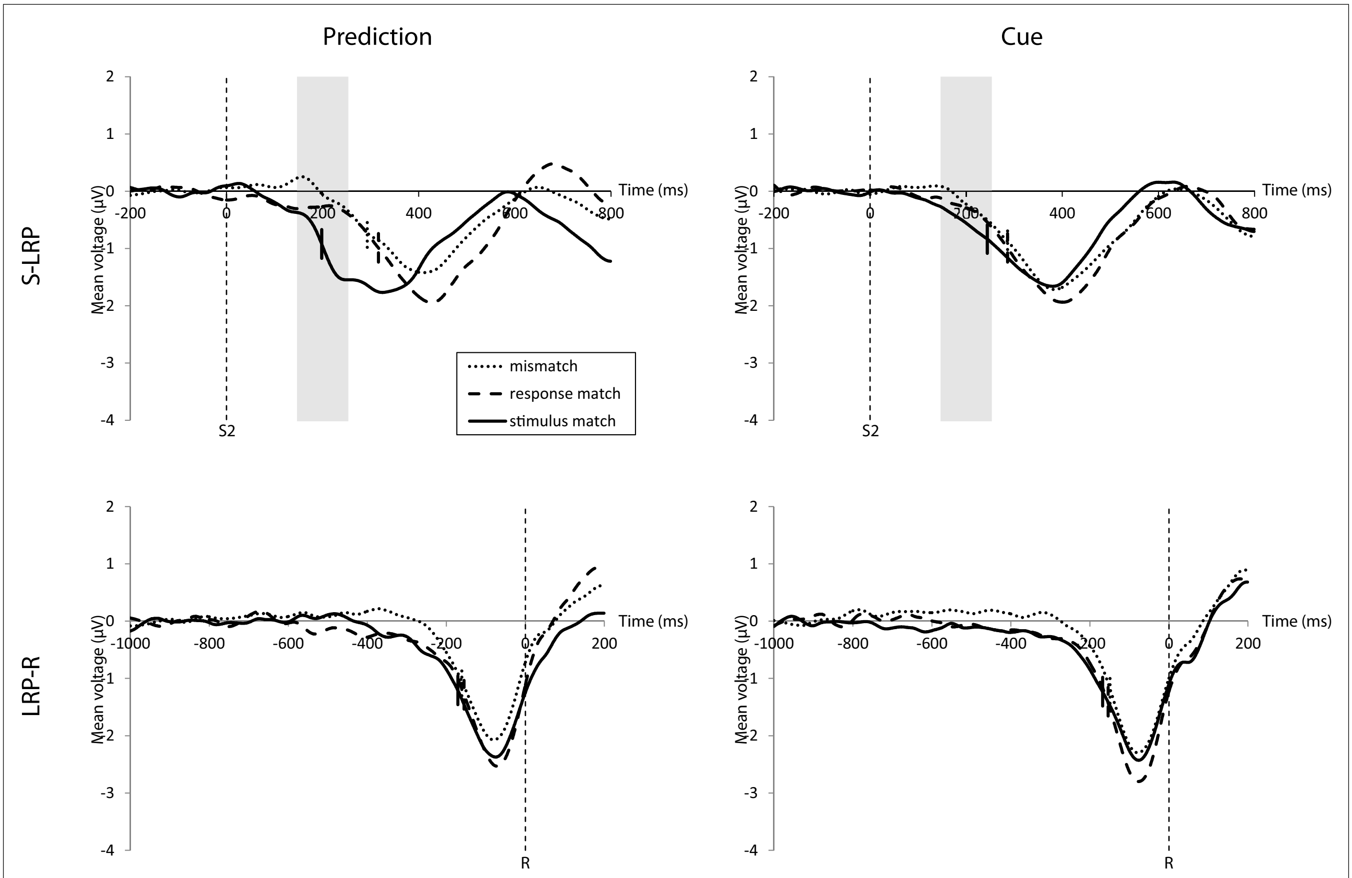

FIGURE 4 | Top: stimulus-locked LRP waveforms for the prediction condition (left) and cue condition (right). There was an earlier S-LRP onset for stimulus matches than for mismatches and response matches. (Onsets are marked by the short vertical lines intersecting the waveforms.) This onset difference was, in trend, larger for the prediction condition. Although the response match S-LRP onset is as late as for mismatches, they differ in their amplitude before S-LRP onset (50% of the maximum amplitude) in the time interval 150–250 ms following stimulus onset (highlighted in gray). The response match amplitude rises in the correct direction as with the stimulus match and is significantly higher than the mismatch amplitude, but only in the prediction condition. Bottom: response-locked LRP waveforms for the prediction condition (left) and cue condition (right). There is only a significant effect of match with an earlier LRP-R onset for stimulus matches compared to response matches and mismatches.

$34) = 3.92$, $p = 0.029$, $\eta_p^2 = 0.19$. The average S-LRP amplitude in the prediction condition in this interval was 0.29 µV higher for response matches than for mismatches, $t(17) = 2.20$, $p = 0.042$, $d = 1.06$ but there was no such difference in the cue condition, $t(17) = 0.65$, ns. Even though it was not reflected in the response time this finding indicates an early correct motoric activation for response matches in the prediction condition.

The onset latency of the LRP-R was influenced only by match, $F(2, 34) = 5.21$, $p = 0.011$, $\eta_p^2 = 0.24$ but not by the expectation condition; there was no interaction (**Figure 4**, bottom).

DISCUSSION

The aim of the present study was to shed some light on the basic processes that underlie the effects of expectation on the control of performance. We were especially interested in distinguishing between the consequences of self-generated expectations (predictions) vs. cue-induced expectations. On each trial participants verbalized an expectation prior to stimulus onset in a two-choice discrimination task. The expectation was either freely generated by the participants (prediction) or specified by an external cue (a word denoting the discriminating stimulus feature). Our results suggest that when investigating effects of explicit expectation one should be aware of possible differences between internally and externally triggered anticipation processes: predictions showed stronger behavioral effects and stronger effects on most ERP components after stimulus presentation that are related to expectation. The two types of expectation showed different aftereffects once a matching or mismatching stimulus was presented. Predictions, therefore, differed substantially from cue-induced expectations.

Direct comparisons of behavioral and neuronal indicators between expectations induced by cues vs. self-generated expectations have been lacking so far. With respect to behavioral differences between the two types of expectation we replicated Acosta (1982). RTs were slower when the stimulus did not match the expectation as compared to a match. This difference was larger in the prediction than in the cue condition. Moreover, as in Acosta's study, we found no benefit of response match trials over complete mismatch trials, suggesting that the expectation exerts its influence before response preparation. The results of error rates reflected RTs, contradicting a speed-accuracy trade-off. Additionally, as a consequence of the frequency manipulation in our experiment, participants also responded faster to the more frequent stimuli.

In the following we shall first discuss the relevant aspects of the ERP results on self-generated vs. cue-induced expectations. We shall then discuss how type of expectation might relate to similar distinctions in other aspects of action control.

STRONGER ERP EFFECTS FOR PREDICTIONS VS. CUES

The CNV did not reveal any differences between predictions and cues. If at all, differences in the cue-target interval between both conditions showed up in an early time window starting 450 ms after cue onset. This was, however, the time window comprising the speech artifacts. Furthermore, participants were instructed that blinks should be synchronized with speaking aloud. Although the time window of this cue-related positive deflection resembled those found in task switching paradigms (e.g., Nicholson et al., 2005; Li et al., 2012) we refrain from further interpretation until this positivity is replicated in a design excluding artifacts.

In the ERPs related to stimulus processing we found differences with respect to expectation match that were modulated by the source of expectation (prediction vs. cue). The N2 amplitude for response match and complete mismatch trials was larger than for stimulus match trials, and this difference was significantly larger in the prediction condition. The N2 has been reported to be larger for incompatibly cued stimuli (Kopp et al., 1996) and interpreted as reflecting cognitive control functions concerning incorrect response preparations. Thus, our results might reflect the need to control the prepared incorrect responses for stimulus mismatch trials. However, in case of a response match the response associated with the unexpected stimulus is correct in our experiment. Our finding of equal N2 amplitudes for response match and complete mismatch trials indicates that the control processes are triggered by the pure stimulus mismatch. This corresponds to the view that interprets the N2 as a sign of mismatch or conflict detection (e.g., Folstein and Van Petten, 2008; Nigbur et al., 2011). Our data suggests that the effect is elicited by the stimulus violating the expectation rather than by the response associated with a different stimulus than the one presented.

Expectation effects on the N2 are larger in the prediction condition. As the probability of a violation of the expectation was comparable for the prediction and the cue conditions it is unlikely that the match effect in the N2 mirrors just the probability of conflict. This finding corresponds to the view that preparing for a self-generated prediction involves endogenous attention processes to a greater degree as preparing for a cued stimulus. Furthermore, the N2 amplitude was generally higher in the cue condition. Though this main effect does not relate to our hypotheses, one might speculate that it possibly also reflects the "expectation mode" (self-generated vs. externally triggered). Presumably, expectations were weaker in the cue condition so that stimuli were generally "less expected" as compared to the prediction condition. This corresponds to the smaller expectation effects we found for the cue condition in the behavioral data and the other ERPs.

We obtained an interaction of match and expectation condition for the P3 amplitude. While usually higher P3 amplitudes have been found for infrequent stimuli (Fabiani et al., 1987), we were able to demonstrate a frequency-independent influence of subjective expectation on the P3. Our results differ from those of Jentzsch and Sommer, 2002, who did not find an influence of explicit expectation on the P3. A possible reason for this discrepancy may lie in methodological differences. In contrast to Jentzsch and Sommer, 2002; see also Matt et al., 1992), we allowed expectations to fluctuate on a trial-by-trial basis instead of manipulating them by instruction at a block-level. Inducing an expectation at the beginning of a block of trials might lead to a situation where this expectation is implemented for action preparation early on and afterward might be effective in action preparation on lower levels of representation while no longer being strongly represented as an expectation proper (compare e.g., Wenke et al., 2009, for a similar argument with respect to the implementation of instructed stimulus-response links). Furthermore our experimental approach differed from the one in the above studies in that we required participants to generate explicit expectations themselves instead of being asked to hold a specific expectation given by instructions. As a consequence, the design of the present study might have been more sensitive to detecting small effects on P3 amplitudes. Concluding from our data, we suggest that explicit self-generated expectation indeed affects early stimulus processing stages, even stronger so than cue-induced expectations.

There was a much earlier P3 peak for stimulus matches as compared to mismatches in the prediction condition. Though the component was similar in its form to the other experimental conditions, conceivably, some kind of signal of prediction success or affirmation might have played a role if the self-generated expectation proved to be correct. Usually, the latency of the P3 peak reflects the time of uncertainty resolution. Sutton et al. (1967) showed this for match trials in an experiment with explicit self-generated expectations about upcoming auditory stimuli (either single or double clicks). They analyzed match trials in which single clicks were expected. The P3 latency depended on the latency of the possible (unexpected) second click. In the conditions with earlier second clicks the P3 was also earlier because the uncertainty about whether the expectation matched could be resolved earlier. This does not explain why in our study the P3 is so much earlier for stimulus matches only in the prediction condition, while in the cue condition the P3 is as late for stimulus matches as for mismatches. In the cue condition, uncertainty regarding the correctness of preparation should be resolved similarly early as in the prediction condition. However, in accordance with the idea that self-generated expectations result in more preparation than cue-induced expectations, a stronger impact of uncertainty resolution in the prediction condition seems plausible. We looked at the scalp distribution for this component in order to check if there is an additional process responsible for the latency difference, but the distribution did not differ from the distributions around the P3 for the other conditions.

Furthermore, we found no frequency effect for the N2 or P3. Even though frequency affected RTs, these effects do not seem to stem from the processes involved in the generation of the N2 or P3. In contrast to our hypothesis and the results from Jentzsch and Sommer (2002), the more passive form of expectation generated by the stimulus frequency had no effect on the ERPs. This could be due to the relatively small frequency differences of the four stimuli. As the expectations for the more frequent stimuli in our experiment happened to be matched by the stimulus more often than for the infrequent stimuli, an effect of frequency or an interaction of frequency and condition could also have been expected to influence the CNV. Expectation validity has been shown to affect CNV amplitude (Scheibe et al., 2009). However, there were no effects of the frequency manipulation on the CNV in our data, perhaps due to the relatively small differences in stimulus frequency that resulted in equally small differences in expectation validity. Although two of the four possible stimuli were shown twice as often as the other two, the absolute difference in validity between frequent and infrequent stimuli amounted to only 17% (as compared to 25% differences and an overall higher validity, 50 vs. 75 and 100%, in Scheibe et al., 2009).

The LRP results only partially reflect our predictions. As expected, the S-LRP onset reflected the RT results for the different match types, showing that these effects are the result of premotoric processing stages. The interaction with the influence of the expectation condition only approached significance. In contrast to our hypothesis and the results of Jentzsch and Sommer (2002) there was a significant effect of match type on the LRP-R onset, similar to the S-LRP onset and the RT, with an earlier onset for stimulus matches than for the two mismatch types. That is, motor preparation started earlier in those cases with fast response selection. The expectation condition, however, did not affect motor preparation as measured by the LRP-R.

Response matches did not differ from complete mismatches in behavioral performance. Although response matches call for the same response as indicated by the cue or prediction, we did not find any benefit compared to complete mismatches. This finding suggests that response preparation depends on the imperative stimulus. Similarly, the N2 and P3 amplitude did not differ between response matches and mismatches whereas response matches differed significantly from stimulus matches in N2 and P3 amplitude measures. The facilitation of stimulus matches is reflected in the S-LRP onset and can, therefore, be attributed to perceptual and/or central parts of the preparation process. There was no difference between response matches and complete mismatches in the S-LRP and the LRP-R onset was similarly late for response match and mismatch. This is partly in line with what the theory of event coding (TEC; Hommel, 2009) would predict. Event codes are abstract codes encompassing features of perceived stimuli and (to be) produced actions. According to TEC, stimulus and response features are integrated into one event code. Event codes might be formed and retrieved both during prediction/cue processing and when the stimulus is presented and responded to (compare e.g., Wenke et al., 2007). Connecting and disconnecting features in an event code takes processing time. Thus, if we assume that explicit expectation provides some form of "preparative" event code, response matches, and complete mismatches should take longer than stimulus matches, in which all links set up by the expectation can be kept. This prediction is met by our data. However, TEC further predicts that complete mismatches are faster than response matches because a new event code is formed instead of disconnecting old and connecting new features as in the case of a response match (in a response match trial the predicted response has to be kept, but in combination with another stimulus). This prediction is not met because complete mismatches behaviorally do not differ significantly from response matches, and, in tendency, are rather slower than response matches.

Overall, S-LRP results mostly reflected behavioral performance. However, with self-generated predictions, both stimulus and response matches lead to an initial rise in the S-LRP, indicating an activation of the corresponding response. In the later course a faster rise for stimulus matches results in the S-LRP passing the onset threshold (defined at 50% of the peak amplitude) much earlier, while response matches do not pass this threshold before mismatches. This pattern suggests a preactivation of the correct response that was then inhibited due to the reevaluation after a different stimulus was shown. Presumably, inhibition seems to commence in response matches as soon as the mismatch between expected and presented stimulus is detected. This is interesting with regard to the role of stimuli in goal-directed action that Kunde et al. (2007) offer. They suggest that actions are generally goal-oriented and stimuli primarily serve to disambiguate between two variants: (1) a specific effect can be expected to follow an action in the current context, or (2) a goal is likely unattainable in the current context. Even simple actions such as button presses or operations of switches can have different effects depending on context factors. Presumably, the early S-LRP in response matches is indicating that action preparation, turning the expectation into an action goal, is no longer fostered (or even inhibited) once the stimulus signals a mismatch with the expectation.

DIFFERENTIATING TYPES OF EXPECTATION

We suggest that it is necessary to differentiate between self-generated and cue-induced expectations. This might be informative for research proposing similar distinctions with respect to other aspects of action control. For instance, in research on effect-based action control the role of action mode (free choice vs. stimulus-driven) in the acquisition (e.g., Herwig et al., 2007; Herwig and Waszak, 2012; Janczyk et al., 2012) or application (Pfister et al., 2011; Gaschler and Nattkemper, submitted) of action effect associations is under current debate. We suggest that effect anticipation might have an especially strong impact on action control if it is based on expectations about effects that stem from goals of the actor rather than being directly caused by current external stimulation. Expectations that are generated internally by integrating goals and past external stimulation might be represented more strongly as compared to cue-induced expectations, as the former need to be shielded against competing external stimulation (compare e.g., Dreisbach and Haider, 2008). When relying on cues that are present on each trial, a strong representation is not established as it is not necessary (compare e.g., Ballard et al., 1995).

We explain our results by a difference between self-generated and cue-induced expectations. A reviewer suggested an alternative account according to which the response time and ERP differences might be based on just one kind of expectation that plays out differently in these two experimental conditions. For instance, one could assume that the participant's expectation is in most cases validly reflected in the prediction condition. Thus, in most trials the participant would be expecting exactly what she or he verbally indicates. In contrast, a randomly presented cue might mirror the expectation on just some of the trials. While the cue suggests the expectation of a specific stimulus, the participant might not always follow this suggestion and often expect a different stimulus instead. By this account, expectation effects in the cueing condition might be as strong as in the prediction condition for the subset of trials in which participants expect what the cue suggests. It would be even conceivable that in this subset of trials of the cueing condition expectation effects might be stronger than those of the prediction condition, as potentially cues and internally generated predictions could be combined. However, as there is possibly a substantial proportion of trials in which participants do not follow the cue, one could expect that effects are on average smaller in the cueing condition as compared to the prediction condition. Though our experiment was not designed to test this alternative account, we analyzed reaction time data to evaluate this idea. According to the above view there should be no (or even a reversed) difference between the cueing and the prediction condition in the subset of trials in which there was likely a match between cue and internally generated expectation. This should be the case for the fastest 10% of match trials in the cueing condition. Percentile analyses did not support this conjecture. The 10% fastest match trials in the cue condition were *slower* than the 10% fastest match trials in the prediction condition [$\Delta = 27.39$ ms; $t(17) = 2.77$, $p = 0.013$].

A second possibility to address this concern is to scrutinize the influence of stimulus lag on the match effect in the cue condition. A typical fallacy often underlying predictions is the tendency to increasingly expect a stimulus alternation after longer runs of repetitions, also known as the "gambler's fallacy" (Ayton and Fischer, 2004). If a cue-independent internally generated expectation is effective in the cueing condition, a stimulus should be increasingly expected the longer it has not been presented. Indeed, in our sample the mean prediction probability for a stimulus increased from 16% when it had been presented two trials before to 30% when the last presentation was five or more trials back. The probability to predict a first-order repetition was on average 25%. All contrasts between the prediction probabilities for a stimulus presented at lag 1 (repetition prediction) to lag 5 or more were statistically significant. So, the predictions of our participants seem to reflect a mixture of a "gambler's fallacy"-like alternation bias and a first-order repetition bias. Therefore, if the cue matches a stimulus that has not been presented for several trials, the likelihood for the cue matching the "real" expectation should be highest. Consequently, one would expect the largest match effect at the longest lag of trials. We reanalyzed RTs of stimulus match and complete mismatch trials (there were not enough data points in some cells for response matches) of the cue condition. We found an effect for match, $F(1, 17) = 38.75$, $p < 0.001$, $\eta_p^2 = 0.70$, with no differences between lags [interaction match × lag: $F(1, 17) = 1.43$, $p = 0.232$], while RTs generally increase over lags for match and mismatch trials (main effect of lag: $F(1, 17) = 8.88$, $p < 0.001$, $\eta_p^2 = 0.34$). In the case of a stimulus repetition the effect tended to be larger (41 ms), rather than smaller, compared to the effect at longer lags (22, 20, 25, and 20 ms, for lags 2, 3, 4, and more than 4, respectively). Currently, our data does not support the view that there is only one kind of expectation effective in both the cueing and prediction condition. Rather, the data suggests that expectation in the cueing condition is different from expectation in the prediction condition. As these *post hoc* analyses provide only preliminary arguments, the task to disentangle the interactions between internal and externally motivated expectation remains open to future research.

One can further argue that self-generated expectations can not be controlled experimentally to the same extent as cue-induced expectations. Yet we suggest that it is warranted to (also) use self-generated expectations for studying effects of expectation on goal-directed action. Research on task switching has witnessed a similar case where presumably external validity and experimental control have to be balanced. It could be shown that a voluntarily initiated task choice results in different behavioral effects as compared with the situation where the task set to be implemented is triggered by a cue: voluntary task switches lead to much smaller task switching costs than cued task switches (Arrington and Logan, 2005). Thus, not only in the preparation of simple actions but also at the superordinate level of task sets there are differences between self-initiated and externally triggered processes. Participants in the Arrington and Logan (2005) study were instructed to choose freely between two possible tasks (with about the same frequency and in an approximately random manner). Thus, they decided on a task to prepare for, or, to put it differently, they expected to execute the chosen task as soon as the stimulus appeared (cf. Kunde et al., 2007). Accordingly, after being cued, they prepared to execute the task given by the cue. This situation, therefore, is similar to the approach of the current study: performance differences are observed as a consequence of preparation determined by internal or external sources. However, it is not clear if the differences are based on qualitative differences between internally or externally initiated task preparation processes, or if it may already be the source of expectation generation (i.e., before any preparation starts) that affects the consecutive task processes.

The findings from voluntary task switching suggest that the two paths to action might already differ prior to stimulus presentation. Accordingly, expectations prior to stimulus presentation may vary and differently affect action preparation depending on whether they are cue-induced or self-generated. Moreover, the idea of stimulus-based and intention-based action control modes (e.g., Herwig et al., 2007) can be mapped to what is (not) necessary to build-up explicit stimulus expectations in cueing vs. self-generation: while cues can potentially act as rather automatic triggers for a specific expectation (e.g., Bargh and Chartrand, 1999), the requirement to generate predictions can only be fulfilled intentionally (compare e.g., Jahanshahi et al., 2006). As expectations are a part of the action it seems plausible that participants are more likely to be in an intention-based mode if they generate expectations themselves. Moreover, expected or unexpected stimuli in this context represent feedback (i.e., action effects) to the expectations, and the contingency between expectations and stimuli should impact performance to a larger extent if it is acquired in an intention-based mode (Pfister et al., 2011). This could explain the performance differences between prediction and cue trials in Acosta's (1982) and our study.

The difference between self-generated and cue-induced expectations and their role in action control requires further study. We have demonstrated that these types of expectation differ in a situation in which both are explicitly verbalized using the same words as output (naming the predicted differentiating stimulus feature vs. reading the cue word of this feature). A study trying to generalize the different expectation effects beyond this specific verbal task seems promising. Furthermore, it is necessary to test accounts of how and why self-generated and cue-induced expectations differ. As of yet, it is not clear whether the two types of expectation differ qualitatively or quantitatively. Self-generated expectations might either show stronger and/or qualitatively different effects on action preparation and performance. For instance, one could argue that a difference in the results might simply be due to an artifact in the methods used to induce the two types of expectation. On the one hand, reading aloud the cues does not enforce deep processing. In an implicit sequence learning study with a repeating sequence of to be read words, Hartman et al. (1989) demonstrated a surprising lack of explicit and even implicit learning. Generating the predictions, on the other hand, might enforce deeper processing for various reasons. For instance, participants were instructed that expectations should not be the same all the time. The experimenter was present outside the EEG booth coding the expectations online. Thus, the self-generated expectations were constrained such that they should be somewhat variable from trial-to-trial, avoiding perseverance and obvious patterns. This likely enforced that participants allocated a substantial part of their resources to the expectations in the prediction version of the task (compare e.g., Rapoport and Budescu, 1997).

Looking for functional differences between different types of expectation, Bubic et al. (2009, 2010) employed EEG and fMRI to investigate involved brain structures and processes. Violations of sequential regularities were accompanied by increased activity in premotor and cerebellar components of the "sequencing network," presumably reflecting a mismatch between expectations generated by a forward model (cf. Wolpert and Ghahramani, 2000) and the observed stimuli – and an adjustment of the model. In addition, lateral prefrontal areas were engaged when a sequence violation required a boost in cognitive control. Stimuli deviating from a context of standard stimuli by a certain feature (as in an oddball paradigm), on the other hand, triggered responses in bilateral posterior temporal and parietal areas, reflecting increased attention and perceptual processing (Bubic et al., 2009). Interestingly, they also report differences in both the N2 and P3 components between their expectation conditions. While the N2 exhibited a shorter latency for sequential deviants compared to feature deviants, the P3 peaked later in the first condition and had a smaller amplitude. In line with the activation pattern reported in their imaging study, both components had a more posterior distribution for feature deviants. Additionally, they identified an enhanced N1 component for feature deviants, suggesting an early sensory registration of the irregularity (Bubic et al., 2010). The authors take these findings as indication for distinct functional networks involved in the processing of different types of expectation. It remains an interesting question whether similar functional differences also apply to the distinction between self-generated and externally cued expectation studied here.

CONCLUSION

Self-generated expectations differ from cue-induced expectations on a range of cognitive processing stages and result in stronger behavioral effects. Response time benefits for expected stimuli are much larger when expectations are self-generated as compared to externally cued. Higher amplitudes in both the N2 and P3 components for violations of self-generated expectations indicate increased premotoric preparation compared to cue-induced expectations. This goes along with a missing benefit for stimuli matching the expected response only and is mirrored in the LRP. Underlying cognitive or neuronal functional differences between these types of expectation remain a subject for future studies.

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Action intentions modulate allocation of visual attention: electrophysiological evidence

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In line with the Theory of Event Coding (Hommel et al., 2001), action planning has been shown to affect perceptual processing – an effect that has been attributed to a so-called *intentional weighting* mechanism (Wykowska et al., 2009; Hommel, 2010). This paper investigates the electrophysiological correlates of action-related modulations of selection mechanisms in visual perception. A paradigm combining a visual search task for size and luminance targets with a movement task (grasping or pointing) was introduced, and the EEG was recorded while participants were performing the tasks. The results showed that the behavioral congruency effects, i.e., better performance in congruent (relative to incongruent) action-perception trials have been reflected by a modulation of the P1 component as well as the N2pc (an ERP marker of spatial attention). These results support the argumentation that action planning modulates already early perceptual processing and attention mechanisms.

Keywords: action-perception links, intentional weighting, visual attention, ERPs

INTRODUCTION

Being active agents in the world, humans must have developed means to optimize their interaction with the environment through efficient action planning. What does action planning consist in? Several researchers postulate that actions are represented as action goals and these, in turn, are represented as sensory effects of planned actions (e.g., James, 1890; Greenwald, 1970; Prinz, 1987, 1997; Hommel et al., 2001). Imagine you are planning to hit a tennis ball with your racket. Your brain presumably represents that action in the form of a somatosensory feedback of how it will feel on your arm to hit the object with a given force. According to Hommel et al. (2001), the action representation will also involve more "distal" sensory effects, such as visual perception of a motion trajectory of the hit ball as well as the sound of the ball struck by the racket. Such a way of representing planned action might indeed prove efficient, as it entails that consequences of actions which do not match expected effects need to be corrected. Humans must, therefore, learn given consequences of their actions through life-long experience with those actions (e.g., Hommel, 2010). Similar ideas are also implemented in forward models of motor control (e.g., Wolpert and Ghahramani, 2000).

COMMON-CODING OF ACTION AND PERCEPTION CHARACTERISTICS

If actions are represented in form of sensory consequences of the planned actions, action planning and perception need to be tightly coupled (e.g., Prinz, 1997; Hommel et al., 2001). The idea of close action-perception coupling is in line with ideomotor views (e.g., James, 1890; Greenwald, 1970; Prinz, 1987, 1997; Hommel et al., 2001) as well as common-coding perspectives, such as the Theory of Event Coding (TEC: Prinz, 1987, 1997; Hommel et al., 2001) that clearly speak against traditional views postulating linear stage models of processing (e.g., Sternberg, 1969; for a discussion see Hommel et al., 2001). Such traditional views state that processing takes place in sequential stages, i.e., for example, perceptual processing, memory, action selection, action execution that can be studied autonomously without taking other stages – especially the later ones – into account.

In the theoretical framework proposed in TEC, and in line with earlier ideomotor perspectives, perception, and action share a common representational code, which allows for efficient action planning. This common code consists in a network of features distributed across domains (such as action or perception) that can be bound together to represent common sensorimotor events.

A common code implies bi-directional links between action and perception. Such links and mutual influences have been supported by a growing body of empirical evidence, where the findings showed interference effects in situations when a code for action and perception has been occupied and needed updating (e.g., Müsseler and Hommel, 1997; Hommel, 1998).

Evidence for close coupling between action and perception has been brought forward also by imaging techniques. For example, Schubotz and von Cramon (2002) carried out a series of fMRI-studies in which sequences of stimuli were presented. The data showed that when participants were judging whether certain sequences of stimuli are in accordance with a rule (either increasing size of visually presented disks or increasing pitch of a sequence of auditory tones) the respective areas of premotor cortex were activated: that is, hand-related areas were activated when the rule was related to the size of the disks and articulation areas were activated when the rule was related to tone pitch. These results showed an automatic activation of motor areas when action-relevant perceptual attributes were processed, speaking in favor of strong action-perception coupling.

Similarly, Grèzes and Decety (2002) or Grafton et al. (1997) showed automatic activation of motor areas when objects bearing certain affordances (Gibson, 1977) were only viewed. Studies by Kiefer and colleagues using an action priming paradigm have also shown that perceptual processes such as object recognition can be modulated by action-object congruency (e.g., Helbig et al., 2006, 2010), and that these effects may be rather early in perceptual processing (Kiefer et al., 2011; see also Humphreys et al., 2010). Moreover, Tucker and Ellis (2001) observed the effects of object affordances on motor responses in a visual categorization task. In their study, participants were asked to discriminate objects as being artificial (e.g., hammer, nail) or natural (e.g., cucumber, grape). Participants responded with either a power- or precision grip dependent on the category of objects (artificial vs. natural). Size of objects was completely irrelevant and orthogonal to the task. Yet, precision responses were facilitated if the object was smaller and power grips were made faster in response to larger objects. Results of the study by Tucker and Ellis have been interpreted in line with the idea of object affordances (Gibson, 1977), which, even if irrelevant to the task, activate certain motor responses that would be compatible with the object properties. Consequently, if a required action is incongruent with the afforded one, impaired performance is observed, relative to congruent scenarios.

The concept of affordances not only implies automatic activation of a motor program through perceiving action-affording objects but can also have consequences in opposite direction, i.e., action-related bias on attentional processes. Evidence for the latter has been found in neuropsychological case studies (e.g., Humphreys and Riddoch, 2001; di Pelligrino et al., 2005). In the study of Humphreys and Riddoch (2001), a patient suffering from visual extinction was better in detecting objects on the neglected side when the objects were defined by their action affordances, as compared to other characteristics. di Pellegrino et al. (2005) reported that visual extinction patients showed a behavioral benefit for the extinction site when the presented objects had characteristics affording an action on that site (e.g., a cup with a left handle). Another piece of evidence for a bias of spatial attention through action-affording characteristics of perceived objects has been brought forward by an ERP/fMRI study of Handy et al. (2003), in which a sensory ERP component (P1) has been modulated by (implicit) action-relevance of stimuli. Pictorial action-congruency effects were also reported in a recent study by Kiefer et al. (2011), where ERP-modulations in the P1 latency range were observed for stimuli that afforded the same action as an earlier presented prime. These effects were, however, prominent over central electrode sites and were related to activity of motor areas.

INTENTIONAL WEIGHTING MECHANISM AND ATTENTIONAL SELECTION

The above-described studies focused mainly on the evidence for a close coupling between action and perception based on the concept of affordances. However, this concept does not determine the underlying mechanism of the observed action-perception coupling. If spatial attention is biased with respect to action-related attributes of the environment, then what sort of mechanism is employed by the brain to impose such a bias? A postulate of a common code for action and perception implies similar selection mechanisms in both domains.

Research in the area of visual attention has established that attentional selection is a result of a biased competition (e.g., Bundesen, 1990; Desimone and Duncan, 1995; Reynolds et al., 1999) or weighted processing of perceptual features and/or dimensions (e.g., Wolfe, 1994; Müller et al., 2003, 2009; Wolfe et al., 2003). If action and perception share a common code, then similar weighting mechanism should operate with respect to action planning. This has indeed been postulated through the idea of the *intentional weighting* mechanism (Hommel et al., 2001; Hommel, 2010). According to the authors, the intentional weighting mechanism prioritizes processing of those perceptual characteristics that are relevant for intended actions. Hommel (2010) claims that such a mechanism has developed in order to provide information for open parameters of online action control. However, once it developed to serve such a function, it became also available for other processes, also in the absence of planning of overt action.

Craighero et al. (1999) observed effects that might be interpreted in line with such an idea of an intentional weighting mechanism: in their study, latencies of a grasping movement toward a particular object were reduced when a visually presented go-signal was congruent with to be grasped object (a left- or right- oriented bar). Craighero et al. concluded that planning a given action (e.g., grasping) biased visual detection (of the go-signal).

Fagioli et al. (2007) directly tested the idea of intentional weighting, using an oddball paradigm in which a sequence of stimuli was presented on a computer screen. The oddballs were either size or location oddballs and were to be detected. At the same time, participants were asked to either grasp a white cube or point toward a white dot. The authors found that when participants were preparing for a grasping movement they detected size oddballs faster than luminance oddballs whereas location oddballs were detected faster than size oddballs in the pointing condition. The authors concluded that perceptual dimensions were weighted with respect to action planning, which resulted in such differential pattern of behavior.

Wykowska et al. (2009) conducted a series of experiments along similar lines. In this series, a more classical attention task (a visual search task) was used to investigate whether intentional weighting modulates visual attention. The paradigm consisted of two tasks: a visual search for size or luminance pop-out targets presented on a computer screen, and a movement task: pointing or grasping of items placed on an especially designed device below the computer screen. Importantly, the two tasks were completely unrelated both perceptually (different objects to be detected in the visual search task and different objects to be grasped/pointed to), and motorically: the visual search task was performed with mouse key presses with the dominant hand (target present: one key vs. target absent: the other key) whereas the grasping/pointing action was performed with the non-dominant hand on the items of the device (for details of the design, see Wykowska et al., 2009). The authors observed that size detection was better when participants were preparing for a grasping action (*congruent* condition) as compared to pointing (*incongruent* condition) whereas luminance detection was improved when participants were preparing for a pointing movement (*congruent* condition), relative to grasping (*incongruent* condition). The authors termed these effects action-perception *congruency effects* as they manifested the idea of facilitated processing for congruent pairs (e.g., grasping and size) relative to incongruent pairs (e.g., grasping and luminance). Similarly to Fagioli et al. (2007), Wykowska et al. concluded that processing of perceptual dimensions seem to be biased (weighted) by action planning – thanks to the *intentional weighting* mechanism (e.g., Hommel, 2010; Memelink and Hommel, 2012). Importantly, Wykowska et al. (2009) observed that such a bias can already be observed at early stages of processing that are manifested in a simple task of search for pop-out. The authors concluded that action planning might be another source of a top-down control over bottom-up perceptual processing in a similar way as a task-related weighting mechanism weighs task-relevant perceptual dimensions higher than the irrelevant dimensions (e.g., Müller et al., 2009).

AIM OF THE PRESENT STUDY

The aim of the present study was to investigate the intentional weighting mechanism with the EEG/ERP methodology. Attentional theories (Wolfe, 1994; Müller et al., 1995, 2003) postulate that processing certain characteristics of the environment can be weighted pre-selectively, and that this weighting affects visual attention. If so, then intentional weighting (given that it is similar to other weighting mechanisms) should influence attentional selection processes. In order to test this, we used the ERP technique, which allows focusing on the correlates of attentional selection (the N2pc component) and early sensory pre-selective processes (P1 or N1 components).

Wykowska et al. (2009) suggested that perceptual *dimensions* are weighted with respect to action planning. Hence, intentional weighting should occur pre-selectively (e.g., Müller et al., 2003), and through pre-selective bias that should influence attentional focus. If that were to be the case, then early sensory ERP components, such as P1 and/or N1, around the time window of 100 ms post-stimulus, should be modulated by action intentions. Although P1 and N1 components are traditionally interpreted as reflecting effects of spatial attention (e.g., Luck et al., 1993; Luck and Hillyard, 1995; Hillyard et al., 1998; Hopfinger and Mangun, 1998; Wykowska and Schubö, 2010, 2011), recent data suggest that P1/N1 components might also reflect a biasing mechanism that operates at the early level of feature/dimension weighting, not necessarily being restricted to *spatial* attention (see Zhang and Luck, 2009 for a discussion on feature-based attention effects on P1). Hence, we hypothesized that pre-selective weighting of dimensions should be observable at early stages of processing (as reflected by the P1/N1 components), i.e., before attention allocation (as reflected by the N2pc).

At the same time, however, such a weighting mechanism should also affect focal attention. It is postulated (Müller et al., 2003; Wolfe et al., 2003) that in a visual search for a feature target, attention is allocated to a location on the basis of a master map of activity that exhibits the highest signal. This signal is a result of a weighted sum of signals coming from various dimension maps. To be more specific, if there is a size pop-out target in the visual field, a strong signal will be elicited in the size dimension map. A weighting mechanism might modulate this signal – either decrease or increase it, dependent on the relevance of the given dimension. In effect, deployment of attention to a location on a master map can be modulated accordingly. Therefore, if action planning weighs perceptual dimensions in a similar manner (Wykowska et al., 2009), it might result in modulation of not only early stages of processing, as reflected by the P1/N1 ERP components, but as a consequence, also an attention-related ERP component, namely, the N2pc. The N2pc is measured at posterior sites within the time window of ca. 180–300 ms and is more negative on contralateral electrode sites compared to ipsilateral electrode sites relative to an attended object presented in the left or right visual hemifield (e.g., Luck and Hillyard, 1994; Eimer, 1996). Although it is not entirely clear whether N2pc reflects a filtering process in the presence of distractors (Luck and Hillyard, 1994) or attentional selection process *per se*, i.e., enhanced processing even in the absence of distractors (Eimer, 1996), N2pc is generally assumed to reflect deployment of attention to objects in the visual field (Eimer, 1996; Woodman and Luck, 2003; Luck, 2005; Jolicoeur et al., 2006). Therefore, observing action-related modulation of the N2pc would indicate that the focal attention is biased by action planning, presumably due to a weighting mechanism that operates at perceptual dimensions.

To meet the aim of the present study, we introduced a paradigm similar to the experimental design of Wykowska et al. (2009). Participants had to perform a visual search task for size and luminance targets and responded with the dominant hand on mouse keys. Additionally, participants were asked to perform a grasping or pointing action (with the other hand) on three linearly aligned cups positioned under the computer screen (as Wykowska et al. (2011) have shown, the congruency effects can be observed even with completely reduced perceptual similarity between action and perception contexts). With the two types of target dimensions (size vs. luminance) and two types of actions (grasping vs. pointing) we created two action-perception congruency pairs (in line with Wykowska et al., 2009, 2011). That is, size was assumed to be a congruent dimension for grasping (during grasping one needs to specify size of grip aperture, among other parameters) and luminance was assumed to be a relevant dimension for pointing (luminance targets enable efficient localization of an object with a pointing movement response (e.g., Graves, 1996; Anderson and Yamagishi, 2000). While participants were performing the task, the EEG signal was recorded. We expected to replicate the behavioral results of Wykowska et al. (2009) and hypothesized that the congruency effects should be observed in the form of modulation of either the P1/N1 ERP complex, the N2pc, or both.

MATERIALS AND METHODS

STIMULI AND APPARATUS

Stimuli were presented on a 17-inch computer screen with a 100 Hz refresh rate placed at a distance of 100 cm from an observer. The movement cues consisted in black-and-white pictures of a left hand grasping or pointing to a paper cup (**Figure 1**) presented in the middle of the computer screen covering 11.8° × 17.7° of visual angle.

The items of the search display were positioned on three imaginary circular arrays with diameters of 4.2°, 9.9°, and 15.3° of visual angle on a light-gray background. Sixteen elements were positioned on the outermost circle; eight elements were presented on the middle circle; and four elements on inner circle. All elements were dark gray (22 cd/m^2) except for the luminance target (53 cd/m^2). Size of elements covered 1.5° of visual angle in diameter, except for the size target, which was larger: 2° of visual angle. There were two possible display types: a target present display (50% of trials), **Figures 2A,B**; and a blank display, **Figure 2C**. The target could appear at one of six positions (upper/middle/lower and left/right to the fixation point, on the middle circular array).

The go-signal for movement execution consisted in a yellow asterisk of 0.6° in diameter, CIE L*a*b color coordinates: 87/5/82. It was presented 4.5°, 11.3°, or 17.7° from the left border of the screen signaling the to be grasped/pointed to paper cup situated beneath the computer screen, each cup being situated directly below one of the asterisk positions, see **Figure 3**.

The to be grasped/pointed to cups were placed on a table below the computer screen 70 cm in front of the observers, to allow for easy reach. There were three cups: a small white (3 cd/m^2) cup, 5 cm (2.8°) in diameter in the middle point; a middle gray (1.8 cd/m^2) cup, 6.5 cm (3.7°) in diameter in the middle point; and a large dark gray (0.43 cd/m^2) cup, 8 cm (4.5°) in diameter in the middle point. They were all equal in height (4.5°) and weight (2 g).

PARTICIPANTS

Eighteen participants (13 women) aged from 21 to 30 years (mean age: 24.3) took part; 8 participants took part in the experiment for course credit, 10 were paid volunteers. Five participants were left-handed, all had normal or corrected to normal vision. Visual acuity was tested with a Rodenstock R12 vision tester (stimuli 112). The experiment was conducted with the understanding and consent of each participant. None of the observers had taken part in an experiment with such a paradigm before.

FIGURE 1 | Movement cues: grasping movement cue (left) and pointing movement cue (right). The cues were made to be as physically similar as possible, so that they would not elicit different brain response related to their physical characteristics.

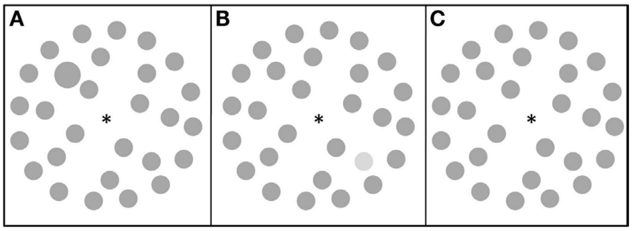

FIGURE 2 | Visual search stimuli: (A) a size target display; (B) a luminance target display; (C) a blank display.

PROCEDURE

A trial started with a 300 ms fixation display (a black asterisk of 0.5 cm in diameter in the center of the screen). Subsequently, a movement cue was presented for 100 ms (see **Figure 3**) followed by another fixation display presented for 200 ms. Next, a search display was presented for 100 ms. Upon response to the search task and a blank screen (400 ms), the go-signal asterisk was presented for 300 ms. The asterisk indicated which of the three cups should be grasped/pointed to. At this point, participants executed the prepared movement, which was registered by an experimenter (who observed performance with a camera outside of the chamber) with a mouse key press. Following the experimenter's button press, a blank screen was presented for 100 ms, which constituted the inter-trial interval.

In order to be able to perform a subtraction of ERP potentials and extract only search-locked ERPs without the overlapping cue-locked ERPs, catch trials were introduced in the design (30% of all trials, randomly intermixed with standard trials). These differed from the standard trials only in that in place of a search display, another fixation display was presented for 100 ms. As participants did not need to perform a search task, a blank display was presented for 500 ms during the time they would respond to the search display in case of trials of interest. The rest of the trial following the blank display was identical to the actual trials of interest.

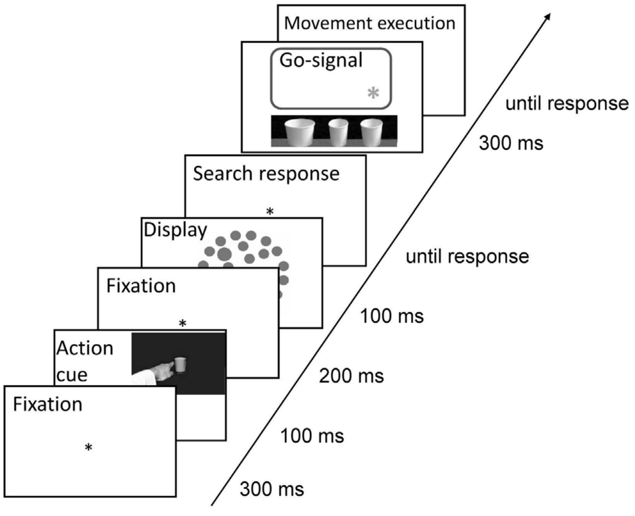

FIGURE 3 | Trial sequence. First, a movement cue was presented. Participants were asked to only prepare for the movement but not execute it at this stage. Subsequently, after a short presentation of a display with fixation asterisk, a visual search display was presented. Participants were asked to respond to the search task immediately and be as fast and as accurate as possible. Upon completion of the search task, a yellow asterisk presented on the screen signaled which of the three cups placed below the computer screen in a horizontal line should be grasped or pointed to (dependent on the cue presented at the beginning of the trial). Only at this point, participants executed the prepared movement. In this task, accuracy, but not speed was stressed.

Response assignment in the search task was counterbalanced, participants were asked to press one of the mouse keys for target present, the other for target absent, with index and middle fingers of their right hand. Speed and accuracy was stressed in the search task whereas only accuracy was stressed in the movement task.

There were altogether 504 trials for each of the tasks. The target type (size or luminance) was blocked (task order was counterbalanced across participants), whereas the movement type (grasp vs. point) and display type (target present vs. blank) were randomized within a block. Short breaks were introduced after each 63 trials so that participants could move their eyes, blink, and relax. Otherwise, participants were asked to reduce blinking and movement not to introduce excessive movement and eye artifacts.

Before the experimental session proper, participants took part in a practice session (without EEG recording) on a separate day, in which they practiced first only the movement task, without the visual search task, and then 270 regular trials for each of the target type (size vs. luminance). The practice session was scheduled minimum 1 day and maximum 2 days before the experimental session proper. During the experimental session, before the actual start of the experiment, participants did 18 warm-up trials with movement only and 18 trials with search + movement.

EEG RECORDING

EEG was recorded with Ag-AgCl electrodes from 37 electrodes (Fp1, Fp2, F3, F4, Fz, F7, F8, F9, F10, FC1, FC2, FC5, FC6, C3, C4, CP1, CP2, CP5, CP6, T7, T8, TP9, TP10, P3, P4, Pz, P7, P8, PO3, PO4, POz, PO7, PO8, O1, O2, Oz, VEOG). The electrodes were mounted on an elastic cap (EASYCAP, GmbH, Germany), according to the International 10-10 System. Horizontal and vertical EOG were recorded bipolar from the outer canthi of the eyes and from above and below the observer's left eye, respectively. All electrodes were referenced to Cz and re-referenced offline to the average of all electrodes. Electrode impedances were kept below 5 kΩ. Sampling rate was 500 Hz with a High-Cutoff Filter of 125 Hz.

DATA ANALYSIS

EEG data

EEG was averaged offline over 600-ms epoch including a 200-ms pre-stimulus baseline with epochs time locked to search display onset. Trials with eye movements and blinks on any recording channel (indicated by any absolute voltage difference in a segment exceeding 80 μV or voltage steps between two sampling points exceeding 50 μV) were excluded from analyses. Additionally, channels with other artifacts were separately excluded if amplitude exceeded ±80 μV or any voltage was lower than 0.10 μV for a 100 ms interval. Raw data was filtered offline 40-Hz high-cutoff filter (Butterworth zero phase, 24 dB/Oct). Only trials with correct movement and correct search responses were analyzed. Responses in the search task deviating more than ±3 SD from mean RT (calculated separately for each participant and target type) were categorized as outliers and excluded. One participant was excluded from analyses due to extensive eye blinks, two due to extensive alpha waves and one due to poor performance in the movement task (14% of errors in the pointing condition; other participants did not exceed 7%). The analyses focused on O1, O2, PO7, PO8 electrodes, where early visual processing is most pronounced.

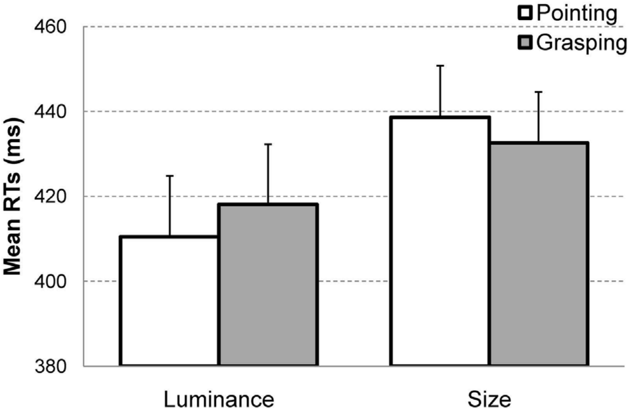

FIGURE 4 | Mean reaction times (RTs) in target present trials as a function of task type (luminance or size) and movement type (pointing or grasping). The *congruency effect* consists in shorter RTs for the congruent action-perception pairs, i.e., size-and-grasping and luminance-and-pointing as compared to incongruent pairs, i.e., size-and-pointing and luminance-and-grasping. Error bars represent the standard errors of the mean.

Behavioral data

Error rates were computed for each participant in both the search task and the movement task. Similarly as in the case of EEG data analysis, prior to RT analysis in the search task, errors in any of the two tasks as well as outliers in the search task were excluded (±3 SD from mean RT for each participant and each target type separately). Error rate analyses in the search task were conducted on correct movement trials. Participants excluded from the EEG data analyses were also excluded from the behavioral analyses.

RESULTS

BEHAVIOR

Reaction times

A 2 × 2 × 2 analysis of variance (ANOVA) on mean RTs with the within-subject factors display type (target present vs. target absent), task type (size vs. luminance), and movement type (pointing vs. grasping) as well as order (size first vs. luminance first) as between-subjects factor showed a main effect of task type, $F(1, 12) = 16.2$, $p < 0.005$, $\eta_p^2 = 0.57$ indicating faster RTs in the luminance task ($M = 419$ ms, SEM = 16) relative to the size task ($M = 436$ ms, SEM = 15). This effect did not interact with order, $p > 0.5$. The main effect of order also did not reach significance, $p > 0.7$. Most importantly for the purposes of this experiment, the interaction of display type, task type and movement type was significant, $F(1, 12) = 6$, $p < 0.05$, $\eta_p^2 = 0.33$. This interaction reflected the congruency effect for target present trials: when participants searched for size targets, performance was faster in the grasping condition ($M = 433$ ms, SEM = 12) relative to pointing ($M = 439$ ms, SEM = 12) whereas in search for luminance targets, the effects were in the opposite direction, i.e., pointing condition yielded faster RTs ($M = 410$ ms, SEM = 14) than grasping ($M = 418$ ms, SEM = 14), see **Figure 4**. This effect did not interact with the order factor, $p > 0.8$.

Subsequent analyses conducted for target present and absent trials separately showed that the interaction of movement type and task type was significant for target present trials $F(1, 12) = 16$, $p < 0.005$, $\eta_p^2 = 0.58$, but not for target absent trials, $p > 0.7$ (Luminance task, pointing: $M = 422$ ms; SEM = 20 vs. grasping: $M = 425$; SEM = 20; Size task, pointing: $M = 433$ ms; SEM = 19 vs. grasping: $M = 439$ ms; SEM = 17). In neither target present or absent trials, was the interaction of task type and movement type modulated by order, both $p > 0.5$.

Finally, planned comparisons between grasping and pointing conditions for size and luminance tasks separately (target present trials) revealed that the difference between those two conditions was significant in the luminance task, $t(13) = 2.1$, $p < 0.05$ (one-tailed) and marginally significant in the size task, $t(13) = 2.1$, $p = 0.06$ (one-tailed).

Error rates

Analogous analysis on error rates revealed no significant results except for the main effect of display type, $F(1, 12) = 6.7$, $p < 0.05$, $\eta_p^2 = 0.36$, showing that more errors were committed in target present trials (M = 3.6%, SEM = 0.8) as compared to target absent trials (M = 1.4%, SEM = 0.4), which suggests that participants adopted a rather conservative strategy in the visual search task by avoiding committing false alarms. Lack of congruency effects for error rates parallels previous results (Wykowska et al., 2009, 2011) and might be due to an overall small error rate (<7%).

EVENT-RELATED POTENTIALS

As the action-related effects on visual search task were observed in RTs for target present trials and not target absent trials, the ERP analyses focused on target present trials (see **Figure A1** in Appendix for analyses of target absent trials). Catch trials (no search display; movement only trials) were subtracted from "actual" trials which allowed for elimination of overlapping potentials related to presentation of the movement cues and for the extraction of potentials related to search display presentation. The subtraction was conducted on epoched data, separately for each cue type, time locked to search display onset.

Early sensory ERP components

A 2 × 2 × 4 ANOVA with the factors task type (size vs. luminance), movement type (grasping vs. pointing) and electrode (O1, O2, PO7, PO8) conducted on the mean amplitudes of the ERP waveform within 70–130 ms time window [representing the latency of the P1 component, determined around (±30 ms) the grand average peak latency] revealed a significant interaction of task type and movement type, $F(1, 13) = 6.2$, $p < 0.05$, $\eta_p^2 = 0.32$ indicating a more enhanced positivity for the pointing movement ($M = 1.9$ μV, SEM = 0.6) relative to grasping ($M = 1.5$ μV, SEM = 0.7) in the luminance condition but not in the size condition (pointing: $M = 2.1$ μV, SEM = 0.7; grasping: $M = 2.2$ μV, SEM = 0.7), see **Figure 5**. This effect did not interact with electrode, $p > 0.7$.

The analysis for luminance and size task separately showed that this difference was indeed significant for the luminance targets, $t(13) = 2$, $p < 0.05$, one-tailed (**Figure 5A**) but not for size targets, $p > 0.25$, one-tailed (**Figure 5B**). As such, the behavioral congruency effect for luminance dimension was reflected in a P1 modulation in the ERPs. The scalp distribution of the mean amplitude of the ERPs within the 70–130 ms time window (P1) is shown in **Figure 5**, right. See **Figure A2** in Appendix for separate analyses of trials in which targets were presented in the left vs. right hemifield.

Attention-related ERP – the N2pc

In order to investigate the congruency effects on the lateralized N2pc component, the EEG signal was epoched separately for left and right targets for the PO7/PO8 electrode pairs. Subsequently, the left/right targets were averaged together for respective ipsi- and contralateral electrodes resulting in two waveforms (contralateral vs. ipsilateral) for each of the task types and movement types (see **Figure A3** in Appendix for separate analyses of left- and right hemifield targets, which reveal that N2pc was not modulated by hemifield of presentation and thus left- and right targets were averaged together for the analysis of interest). A 2 × 2 × 2 (ANOVA) was performed on the N2pc mean amplitudes obtained in the 230–300 ms time window, around (±35 ms) the grand average peak latency of the difference wave between contra and ipsilateral channels with the factors laterality (contralateral vs. ipsilateral), task type (size vs. luminance), and movement type (grasping vs. pointing) for the electrode sites PO7/PO8 (**Figure 6**, solid boxes).

The analysis showed a main effect of laterality, $F(1, 13) = 5.3$, $p < 0.05$, $\eta_p^2 = 0.3$, an interaction of laterality and task type, $F(1, 13) = 10$, $p < 0.01$, $\eta_p^2 = 0.4$, and most importantly, an interaction of laterality, task type, and movement type, $F(1, 13) = 4.5$, $p = 0.05$, $\eta_p^2 = 0.26$, see **Figure 6**. The interaction of laterality and task type showed that in this time window, N2pc was more pronounced for size targets (contralateral: $M = -2.5$ μV, SEM = 1; ipsilateral: $M = -1.2$ μV, SEM = 0.9) than for luminance targets (contralateral: $M = -1.2$ μV, SEM = 0.8; ipsilateral: $M = -0.8$ μV, SEM = 0.7). Therefore, subsequent analyses were conducted separately for each task type. The analysis on size targets revealed a main effect of laterality, $F(1, 13) = 9$, $p < 0.05$, $\eta_p^2 = 0.4$, and a significant interaction of laterality and movement type, $F(1, 13) = 5.2$, $p < 0.05$, $\eta_p^2 = 0.28$ indicating that the N2pc was more pronounced in the grasping condition (contralateral: $M = -2.4$ μV, SEM = 1.2; ipsilateral: $M = -0.9$ μV, SEM = 0.9, see **Figure 6A**, left, solid gray box) as compared to pointing (contralateral: $M = -2.6$ μV, SEM = 1; ipsilateral: $M = -1.5$ μV, SEM = 0.8, see **Figure 6A**, right, solid gray box). Scalp distribution of the ERP waveforms in the N2pc time window of 230–300 ms for size targets in the grasping (congruent) and pointing (incongruent) conditions, separately for targets presented in the left and right visual hemifields is shown in **Figure 7A**.

For luminance targets, no effects reached the level of significance, all $p > 0.15$ (see **Figure 6B**, solid gray box). Scalp distribution of the ERP waveforms in the N2pc time window of 230–300 ms for the luminance targets in the grasping (incongruent) and pointing (congruent) conditions separately for targets presented in the left and right visual hemifields is shown in **Figure 7B**. Note that no clear difference in negativity was observed for contralateral and ipsilateral sites for luminance targets in the incongruent movement condition (grasping, left lower part of the **Figure 7**) while a slight difference (statistically non-significant) is visible in

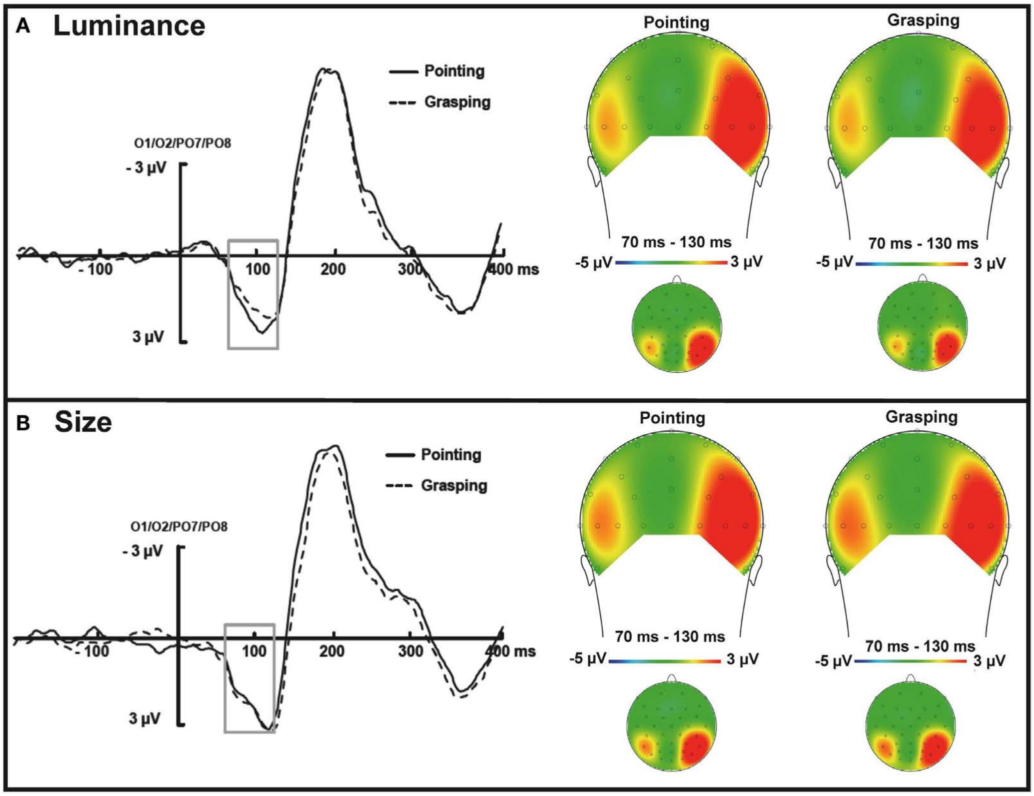

FIGURE 5 | *Left*: Grand average ERP waveforms within the P1 time window of 70–130 ms for luminance targets (A) and size targets (B); targets presented in the left and right visual hemifield are averaged together. The grand average waveforms are pooled across occipital electrodes (O1/O2 and PO7/PO8) locked to the search display. Solid lines represent the pointing movement condition whereas the dashed lines represent the grasping condition. Gray outline boxes indicate the P1 effect (70–130 ms) for luminance targets and lack thereof for size targets. *Right*: Topographical maps of voltage distribution for the same time intervals, presented from posterior view (larger images) and top view, all channels (smaller images, front plotted upwards). Note that the scalp distribution of the mean amplitude within the P1 component time window indicates a larger positivity on the right electrode sites, independent of condition. This might be related to the fact that attentional networks are located mostly in the right cerebral hemisphere (e.g., Heilman and Van Den Abell, 1980; Mesulam, 1981; Sperry, 1974; Thiebaut et al., 2011), and is in line with previous findings on attentional orienting that showed validity effects in a cueing paradigm also predominantly on right lateral electrodes (e.g., Mangun and Hillyard, 1991).

the congruent condition (pointing), targets presented in the right hemifield (lower right part of **Figure 7**).

As no pronounced N2pc was observed for the luminance targets in the time window of 230–300 ms, an additional analogous analysis was performed in the earlier time window of 160–230 ms (see **Figure 6**, dashed boxes) with the factors laterality, task type and movement type. This analysis revealed a main effect of laterality, $F(1, 13) = 11$, $p = 0.01$, $\eta_p^2 = 0.45$ (contralateral: $M = -5.2\ \mu V$, $SEM = 1.1$; ipsilateral: $M = -4.2\ \mu V$, $SEM = 0.9$), and no interactions with task type or movement type, all $p > 0.6$. This effect indicated a pronounced N2pc for both size and luminance targets in this earlier time window but no modulation thereof by movement type.

Control experiment

In order to examine if the action-perception congruency effects are indeed due to action preparation and not a result of mere perceptual priming related to the pictorial cues themselves, we conducted an experiment in which participants (17 in total, 7 women, mean age: 23.2; age range: 20–28) were asked to perform a visual search task for size or luminance, with the visual search displays following photographs depicting either a pointing or a grasping movement. In this experiment, however, no movement execution was required. Otherwise, the design remained identical to that of Experiment 1. The experiment consisted of 384 trials, with 192 trials in which participants searched for luminance, and 192 trials in which size was the target-defining dimension. Target dimensions were blocked, and the order was counterbalanced across participants. Pictures of a grasping or pointing movement were randomized within each block of trials. Data of 3 participants were excluded from further analyses due to large error rates (>20%). Mean RTs were subject to statistical analyses after exclusion of erroneous trials and trials on which RT exceeded ±3 SD from the mean of each participant and each task separately.

An ANOVA with the within-participants factors of *display type* (target present vs. target absent), *task type* (luminance vs. size), and *picture type* (grasping vs. pointing) as well as a between-subjects factor of *task order* (luminance first vs. size first) revealed no significant interaction of task type and picture type, $F < 2.7$ $p > 0.12$, and no significant interaction of task type, picture type and display

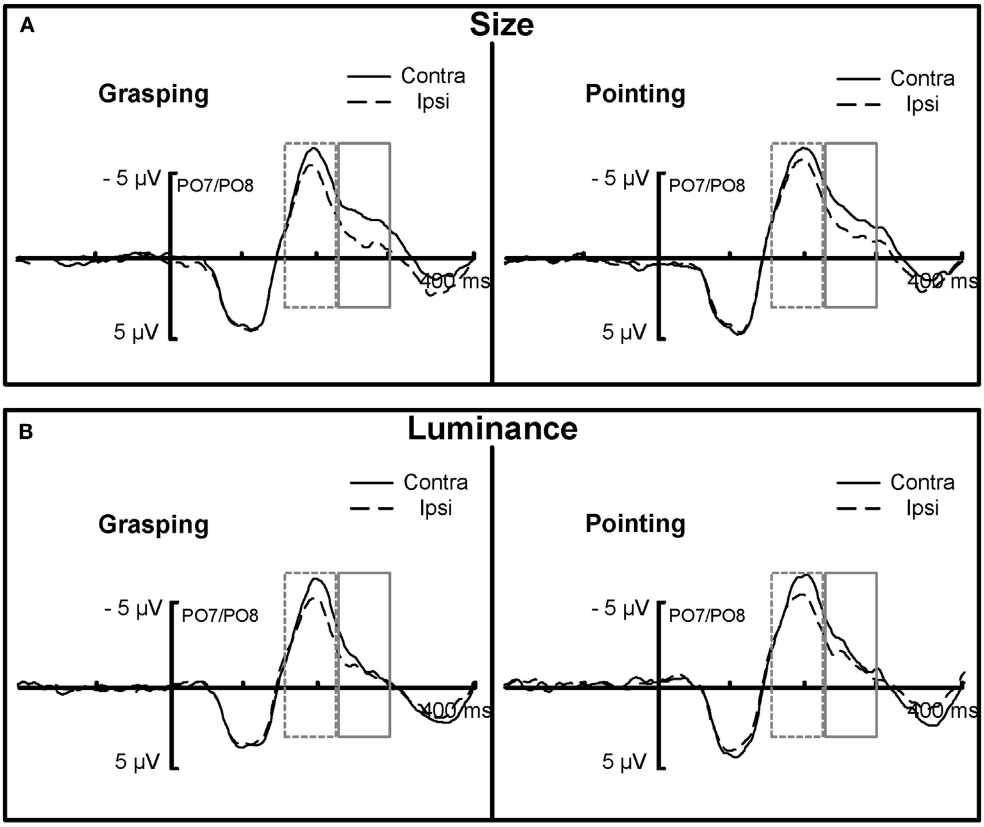

FIGURE 6 | Grand average ERP waveforms, locked to the search display plotted for ipsilateral (dashed lines) and contralateral (solid lines) electrode sites separately, pooled across PO7/PO8 electrodes for size (A) and luminance (B) targets separately as a function of grasping (left) and pointing (right) conditions. The difference between the contralateral and ipsilateral curves, at around 180–300 ms indicates the N2pc. Solid gray boxes mark the time window (230–300 ms) in which an action-related modulation was observed for size targets: N2pc was larger for size-and-grasping as compared to size-and-pointing. No such differential effect was observed for luminance targets in this time window. The dashed boxes represent the earlier time window in which a general N2pc was observed for both luminance and size targets.

type, $F < 1$ $p > 0.35$. When only target trials were analyzed, also no interaction between task type and picture type was observed, $F < 0.35$, $p > 0.59$, and so was the case for target absent trials, $F < 2.5$, $p > 0.15$. Finally, task order had no effect on any other effects or interactions of interest, all $ps > 0.35$.

DISCUSSION

The aim of this study was to investigate electrophysiological correlates of the intentional weighting mechanism observed in the form of action-perception *congruency effects* (Wykowska et al., 2009): better performance in search for size targets when a grasping movement was prepared as compared to a pointing movement; and better detection of luminance targets when pointing, as compared to grasping. In the present experiment, we replicated the behavioral congruency effects for target present trials. Lack of effects in target absent trials might indicate that intentional weighting operates on perceptual processing more prominently when a given signal (size or luminance) is present or when attention is more focused. In general, it is not surprising to observe different effects for target present and absent trials in a visual search task (see Chun and Wolfe, 1996 for discussion on differential processing of target present and absent trials, as well as Schubö et al., 2004b, 2007). The control experiment, in which pictures of movement cues and visual search displays were presented, but no movement was required, confirmed that the observed congruency effects are indeed due to action preparation. The fact that in this control experiment the interaction between picture type and task type was neither observed for target present nor target absent trials indicates that congruency effects do not result from some sort of low-level sensory priming related to the cue stimuli.

Importantly for the aims of the present study, we observed a modulation of early visual ERPs and the N2pc that was related to action intentions. These effects were in line with our hypotheses: if action planning biases processing of perceptual dimensions through intentional weighting, it should be possible to observe such weighting effects on pre-selective processes, reflected by P1 or N1 and, as a consequence, on attentional selection, as mirrored by the N2pc. Interestingly, we observed intentional weighting effects on early sensory P1 component (70–130 ms) for luminance targets whereas for size targets, this effect was reflected in a modulation of attention-related N2pc (230–300 ms).

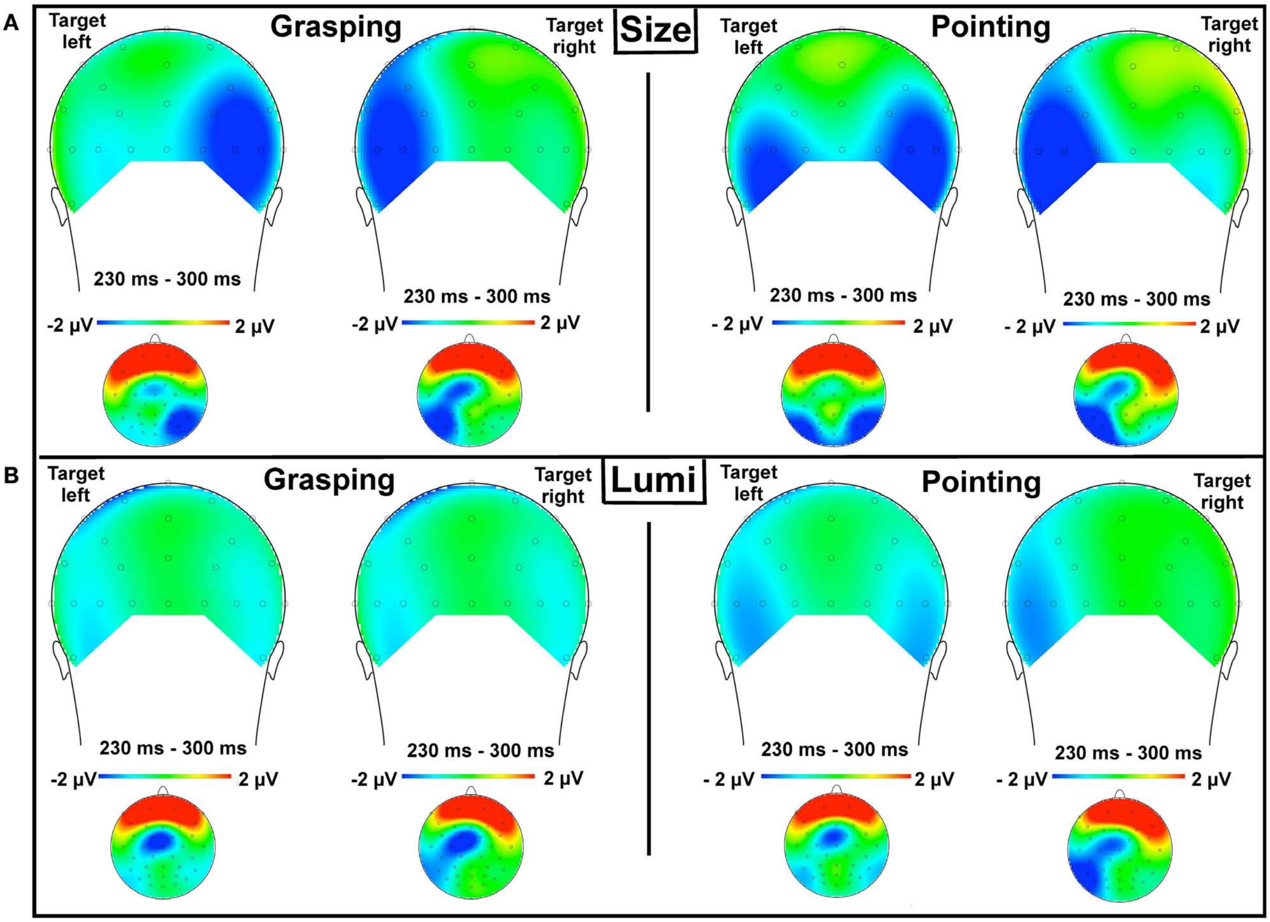

FIGURE 7 | Topographical maps of the ERP voltage distribution for the N2pc time window (230–300 ms) for size targets (A) and luminance targets (B) in the grasping condition (left) and pointing condition (right) presented from posterior view (larger images) and top view, all channels (smaller images, front plotted upwards). The voltage distribution maps represent un-subtracted waveforms in the respective conditions for targets presented in the left and right visual hemifields. The maps show clear target-related laterality effects (that is, enhanced activity contraleratal to the target: the N2pc) for size targets in the grasping condition [**(A)**, left], while laterality was present but less pronounced in the pointing condition [**(A)**, right]. The enhanced negativity on the ipsilateral side for the left size targets in the pointing condition [**(A)**, right] might be a slight indication of not entirely successful disengagement of attention from the right visual hemifield when targets were presented on the left. This may have resulted from the pointing cue that may have acted as a directional cue to the right hemifield. This effect, however, needs to be taken with caution, as it has not been supported by the behavioral results, see Appendix. In the luminance condition **(B)**, negativity was less pronounced in the grasping condition compared to pointing. In grasping trials, there was no difference in negativity for contra- and ipsilateral sites **(B)**, left) yet a slight difference is observed in the pointing condition for target presented in the right hemifield [**(B)**, right].

More specifically, for luminance targets the P1 was more positive in the pointing movement condition relative to grasping (see **Figure 5A**) while for size targets there was no differential effect on P1 (see **Figure 5B**). The opposite pattern of results was observed for N2pc in the 230–300 time window: for size targets, the N2pc was larger in the grasping condition relative to pointing (see **Figure 6A**, left) and there was no effect for the luminance targets (see **Figure 6B**).

Interestingly, for luminance targets no pronounced N2pc was observed in this time window. It might be the case that the action-related bias of perception and attention is observable on those stages of processing that are more crucial for successful completion of a task. That is, if the task requires focal attention, then the effects might be better observed as modulation of focal attention. Similarly, if a task can be completed with mere detection of salience signals, then effects of intentional weighting can be observed already on sensory stages of processing. As behavioral results revealed that size targets were more difficult to detect than luminance targets, search for size might have been less efficient (see Wolfe, 2003 for a discussion on search efficiency). Thus, to detect size targets, more attentional focus might have been required, and hence intentional weighting effects were observed on the attention-related ERP (N2pc). On the contrary, luminance target might have been detected only based on their saliency signal and hence the stage of attentional focusing might have been less pronounced in solving the luminance detection task. Hence, in case of luminance targets, intentional weighting could be observed at the earlier ERP component, namely the P1.

THEORETICAL CONSIDERATIONS

Results of the present study support the idea that perceptual processes can be biased by action planning – an idea that has been put forward within the TEC and supported empirically (Müsseler and Hommel, 1997; Craighero et al., 1999; Hommel et al., 2001; Fagioli et al., 2007; Wykowska et al., 2009 and many others).

According to TEC, perception and action share a common representational code, which entails bi-directional influences between perception and action planning. To date, such influences have been observed mostly behaviorally (Müsseler and Hommel, 1997; Craighero et al., 1999; Bekkering and Neggers, 2002; Schubö et al., 2004a; Fagioli et al., 2007; Wykowska et al., 2009), although several studies have been conducted with neuroimaging techniques (e.g., Grafton et al., 1997; Grèzes and Decety, 2002; Schubotz and von Cramon, 2002; Handy et al., 2003). Moreover, existing research with the EEG/ERP method has shown that an ERP correlate of deviance detection (P3a) was modulated by participants' anticipatory mechanisms related to acquired links between certain actions and their perceptual consequences (Waszak and Herwig, 2007); or that action observation influenced the N2 component dependent on whether the performed action was compatible with the observed one or not (Press et al., 2010). Action observation was shown to affect even earlier ERP components when action observation was congruent with prepared action (Bortoletto et al., 2011). Furthermore, results showed that the N1 component was modulated by action-object congruency when participants judged whether objects were real or not (Humphreys et al., 2010); or that spatial attention was shifted to the side where movement was being prepared (Eimer et al., 2005).

However, the present study is the first to show ERP correlates of a more general mechanism that biases perceptual processing toward those perceptual characteristics that can potentially be action-relevant – the *intentional weighting* mechanism. Therefore, the present results extend earlier findings of action-related bias on perception of *action*/action observation (Press et al., 2010; Bortoletto et al., 2011); and they also go beyond the idea of selection-for-action in a spatial manner (e.g., Rizzolatti et al., 1994; Deubel and Schneider, 1996). Deubel and Schneider, for example, showed that perceptual processing is facilitated in the position toward which a saccade is planned, even before the saccade is executed. In a similar line, Rizzolatti postulates the idea of premotor attention. According to Rizzolatti, spatial attention is a consequence of neuronal activity related to preparation of goal-directed, spatially organized movements.

Although other researchers have already investigated the effects of action-perception links on feature- or dimension-based selection (e.g., Craighero et al., 1999; Fagioli et al., 2007; Wykowska et al., 2009), the present results indicate the ERP correlates of such an action-related intentional weighting mechanism that operates on perceptual dimensions. It is important to note that although the action-related modulations were found on an ERP component that reflects spatial attention, the modulation was not spatial in nature. That is, the *type* of action (grasping or pointing) modulated spatial attention, and not the location of an eye or arm movement. Therefore, modulation of an ERP marker of spatial attention (N2pc) might have been a consequence of an earlier weighting mechanism that weighs perceptual dimensions according to their (action) relevance. This suggestion is plausible especially due to the fact that we observed also action-perception links imposing bias on perceptual processing at even earlier stages than allocation of spatial attention. That is, action-related effects were observed earlier than the N2pc: already at around 70 ms post-stimulus presentation. This effect is in line with the postulates put forward in Wykowska et al. (2009) as well as Hommel (2010) stating that action planning influences perception through *intentional weighting* (Hommel et al., 2001; Hommel, 2010) which operates at the level of perceptual dimensions and biases neural responses toward dimensions that are potentially action-relevant. In the case of the present experiment, luminance dimension was weighted higher for pointing actions whereas size dimension was prioritized for grasping.

We postulate that the intentional weighting mechanism is similar to other task-related biasing mechanisms (e.g., Eimer and Kiss, 2008; Lien et al., 2008; Zhang and Luck, 2009; Töllner et al., 2010; see also Desimone and Duncan, 1995; Reynolds et al., 1999 as well as Bundesen, 1990; Müller et al., 2003, or Wolfe et al., 2003 for non-ERP research on mechanisms that bias visual perception) as it is not dependent on spatial- or action-compatibility (Wykowska et al., 2011). In line with Hommel (2010), we believe that intentional weighting is a mechanism that originally developed in order to provide information for open parameters of online action control. Hence, the function of attention is not to reduce the abundance of input for further processing that has limited capacity (e.g., Broadbent, 1958; Kahneman, 1973), but rather to provide parameters for online adjustment in action control. In particular, Hommel specifies two processing pathways in action planning: an offline pathway where invariant characteristics of an action are planned and an online pathway in which particular variable parameters of a given action are specified (a particular size or location of an object). The original function of attention, therefore, has been to prepare the system for delivering the online parameters. This idea is supported by the present data, which show that attentional processes, as measured by the N2pc, are tuned to intended actions.

Finally, the observation that action-related influences reach early stages of processing is an important result, given how far action planning brain areas, i.e., premotor areas, supplementary motor areas (preSMA), parietal areas (intraparietal sulcus), and cingulate cortex (e.g., Rizzolatti et al., 1998; Rizzolatti and Luppino, 2001; Mueller et al., 2007), are located from the early visual areas, i.e., extrastriate cortex – which is claimed to be the source of the P1 component (e.g., Luck et al., 2000). Therefore, results of the present study support the idea of broad interactions between various brain regions, including action-related, and visual areas and far-reaching connections.

The present findings may also be discussed in relation to the attentional sensitization model (Kiefer and Martens, 2010; Martens et al., 2011; Kiefer, 2012), which was developed to account for various top-down controlled influences on unconscious information processing. This model claims that task representations configure the cognitive system in such a way that processing streams are modulated ("sensitized") in accordance with the respective active task set. Attentional sensitization is supposed to enhance the sensitivity of task-relevant and attenuate the sensitivity of task-irrelevant pathways. In their experiments, Kiefer and colleagues used an induction task (either semantic or perceptual stimulus classification) in combination with a masked priming task. The induction task was presented before the masked prime and was supposed to activate either a semantic or a perceptual task set. Results showed that processing of the prime presented after the induction task was modulated by the nature of the activated task set: processing of an unconsciously perceived word prime was enhanced after a semantic induction task but not after a perceptual induction task and vice versa. Thus prime processing benefited from previous sensitization when the priming task matched the pathways sensitized by the induction tasks (Kiefer and Martens, 2010; Martens et al., 2011; Kiefer, 2012). Although there are some differences between the induction task and the present paradigm, the attentional sensitization model may also be used in order to describe the present findings. In the context of the present experiment, one may assume that action planning, which was induced by the presentation of the action cue, was accompanied by the activation of a respective task set. This task set may have modulated processing within the respective perceptual pathways. Thus a grasping cue may have modulated ("sensitized") the action-congruent perceptual size pathway while the pointing cue may have done so for the luminance pathway. Thus, similar to differential sensitization within the perceptual domain in the experiments by Kiefer (2012), the action cue may have differentially modulated the sensitivity of size and luminance processing in the search task of the present experiment.

CONCLUSION

In summary, results reported in this study revealed that visual perception and selection are influenced by action intentions. That is, what we humans focus on – among the abundance of input reaching the sensory apparatus – is already biased by how we intend to act. Through a life-long experience with our actions, we have learned what perceptual characteristics are important for a given action type. Therefore, when planning to act in that particular way, we tune our perception to what is action-relevant. This mechanism needs to be taken into account in research on selection mechanisms that is usually conducted in artificial laboratory setup. In other words, one needs to remember that people select not only what is asked from them to select but also what is related to how they intend to act.

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APPENDIX

TARGET ABSENT TRIALS

In order to test whether the different movement types of pointing vs. grasping affected the P1 in any systematic fashion during the target absent trials, we conducted a 2 × 2 × 4 ANOVA with the factors *movement type* (grasping vs. pointing), *task type* (luminance vs. size), and *electrode* (O1, O2, PO7, and PO8) on target absent trials in the time window of the P1 (70–130 ms). This was done to ensure that the ERP effects in the target present trials truly reflected interactions of intentions and target processing rather than a general influence of intentions. This ANOVA could only be conducted for the P1 because the N2pc is always calculated relative to the target position. The analysis showed no main effect of movement type or interaction of movement type and task type, all $F\text{s} < 1.3$, all $p\text{s} > 0.23$, see **Figure A1** in Appendix. There was only a significant interaction of electrode, and movement type, $F(1.3, 16.9) = 6.9$, $p < 0.02$. Subsequent analyses for each of the electrodes showed that the effect of movement type was significant only on the PO7 electrode, $F(1, 13) = 5.3$, $p < 0.05$, $\eta_p^2 = 0.3$ with grasping movement eliciting more positive amplitude ($M = 2.1$, $\text{SEM} = 0.8$) than the pointing movement ($M = 1.5$, $\text{SEM} = 0.8$). This positivity effect of intention to grasp on the P1 cannot account for the strong overall P1 effect at all electrodes that was found in the analysis of the target present trials, i.e., the effect of more enhanced P1 for pointing vs. grasping in the luminance task condition.

In a subsequent time window (130–300 ms) there was a main effect of movement type $F(1, 13) = 12.5$, $p < 0.01$, $\eta_p^2 = 0.5$ with grasping movement eliciting less negative amplitude ($M = -2.3$, $\text{SEM} = 0.9$) than the pointing movement ($M = -3$, $\text{SEM} = 0.8$), all other effects and interactions were non-significant, all $F\text{s} < 0.9$, all $p\text{s} > 0.4$.

More enhanced negativity for the pointing condition in target absent trials, independent of the task type might indicate that when there was no signal to be processed in the visual search display, more neuronal resources were employed when a pointing movement was prepared, relative to a grasping movement. This might be due to pointing being in general a simpler movement than grasping. Hence, more resources could have been devoted to the visual search task. This interpretation, however, needs to be taken with caution, as no corresponding effect was observed in behavior (main effect of movement type in target absent trials was not significant, $F < 1.5$, $p > 0.25$). Importantly, the differential effect of movement type in target absent trials was observed for a different time window than the effect of interest (P1) observed in target present trials in luminance task.

CATCH TRIALS

In order to examine the ERPs in the catch trials, i.e., baseline trials, which were subtracted from the trials of interest, we conducted a 2 × 4 ANOVA with the factors *movement type* (grasping vs. pointing) and *electrode* (PO7, PO8, O1, O2) over the time window of the P1 component (70–130 ms). This was done in order to examine whether the effects of interest are not due to the subtraction procedure. The analysis revealed that the main effect of movement type was significant, $F(1, 13) = 5.1$, $p < 0.05$, $\eta_p^2 = 0.28$ with pointing movement yielding a more negative amplitude ($-3.9\ \mu\text{V}$) as compared to grasping ($-3\ \mu\text{V}$). This difference in the baseline trials cannot however explain the significant *interaction* between movement type and task type in the P1 time window for the trials of interest, as the very same catch trials were subtracted from the luminance and size condition.

LEFT- AND RIGHT HEMIFIELD OF TARGET PRESENTATION

In order to test whether the cues differentially influenced target detection on right and left sides of the visual field, we analyzed detection of right- and left-presented targets separately. Note that even though the arm presented in the cues always extended from the lower left corner of the photograph to the middle (and not to the right), attention might have been guided to search for the target first to the right side, as the arms might have acted as directional cues for attention. This could be the case for the pointing movement in particular, as an extended finger in pointing gestures is a salient directional cue.

P1 component

A 2 × 2 × 4 ANOVA with the factors *movement type* (grasping vs. pointing), *target position* (left vs. right), and *electrode* (O1, O2, PO7, and PO8) for the time window 70–130 ms (P1) in the *luminance* task condition showed that the main effect of movement type in the luminance condition on the P1 component was not dependent on whether the target was presented in the left or in the right visual hemifield: interaction of movement type and side of the target was not significant, $F < 0.01$, $p > 0.9$, see **Figure A2** in Appendix.

N2pc effect

A 2 × 2 × 2 ANOVA with the factors *movement type* (grasping vs. pointing), *target position* (left vs. right), and *laterality* (contra- vs. ipsilateral) for the time window of 230–300 ms (N2pc) in the *size* condition showed that the side in which target was presented had no general effect on the N2pc, interaction of laterality and target position, $F < 0.05$, $p > 0.58$. Only the interaction of laterality and movement type slightly depended on the hemifield in which the target was presented: interaction of laterality, movement type and side of target was marginally significant, $F(1, 13) = 3.6$, $p = 0.08$, $\eta_p^2 = 0.2$, see **Figure A3** in Appendix. For targets presented in the left hemifield, the interaction of laterality and movement type was significant, $F(1, 13) = 7.25$, $p < 0.02$, $\eta_p^2 = 0.36$ while for targets presented in the right hemifield this interaction was not significant, $F < 0.4$, $p > 0.54$. Analogous analysis for the luminance targets showed no significant effects or interactions, all $F\text{s} < 2.8$, $p\text{s} > 0.12$. More pronounced movement-related effect on N2pc for targets presented in the left hemifield speaks against the interpretation that the effect is due to cues guiding attention to the right side of the visual field. Rather, this might be due to preferential processing of the left hemifield related to attentional networks being located in the right cerebral hemisphere (please see below). Alternatively, analyzing left- and right hemifield targets separately might have reduced the statistical power, and hence the effects reached the level of significance only in one of the conditions.

BEHAVIOR

An additional analysis on behavioral data in target present trials with the factors *movement type* (grasping vs. pointing), *target*

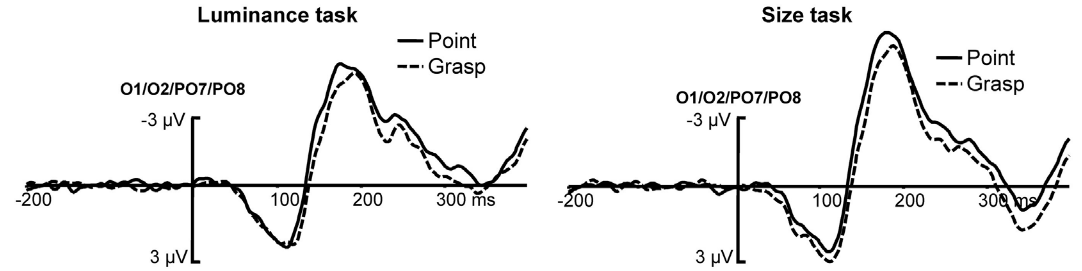

FIGURE A1 | Grand average ERP waveforms for target absent trials in luminance task (left) and size task (right). Solid line represents the pointing condition while the dashed line depicts the grasping condition.

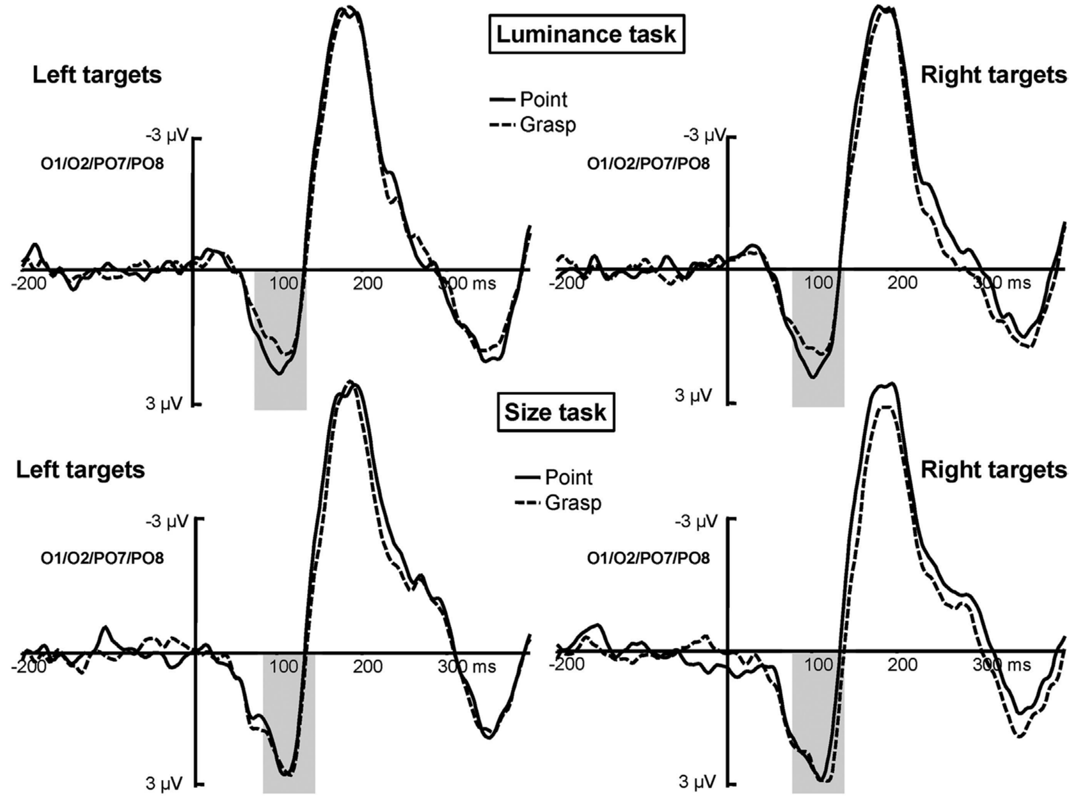

FIGURE A2 | Grand average ERP waveforms for left and right targets separately in the luminance and size task conditions. The dark gray box depicts the P1 time window (70–130 ms) for which statistical analyses have been conducted. Solid line represents the pointing condition while the dashed line depicts the grasping condition.

position (right vs. left), and *task type* (luminance vs. size) showed that there was no main effect of target position, $F < 0.05$, $p > 0.85$, no interaction of movement type and target position, $F < 0.05$, $p > 0.85$, and revealed also that the interaction of interest (movement type and task type) did not depend on the hemifield in which the target was presented, $F < 1$, $p > 0.3$.

Also in the control experiment, an analogous ANOVA with the factors *picture type* (grasping vs. pointing), *target position* (left vs. right), and *task type* (luminance vs. size) revealed no significant effect of target position, $F < 1.5$ $p > 0.25$; no interaction of picture type and target position, $F < 0.05$, $p > 0.8$ and no interaction between picture type, target position, and task type $F < 0.6$, $p > 0.45$.

Taken together, the results of separate analyses for trials in which targets were presented in the left vs. right hemifields indicate that targets presented in the right hemifield were not processed preferentially. Therefore, the cues have not cued participants' attention to the right side of the visual field, which could potentially influence the congruency effects.

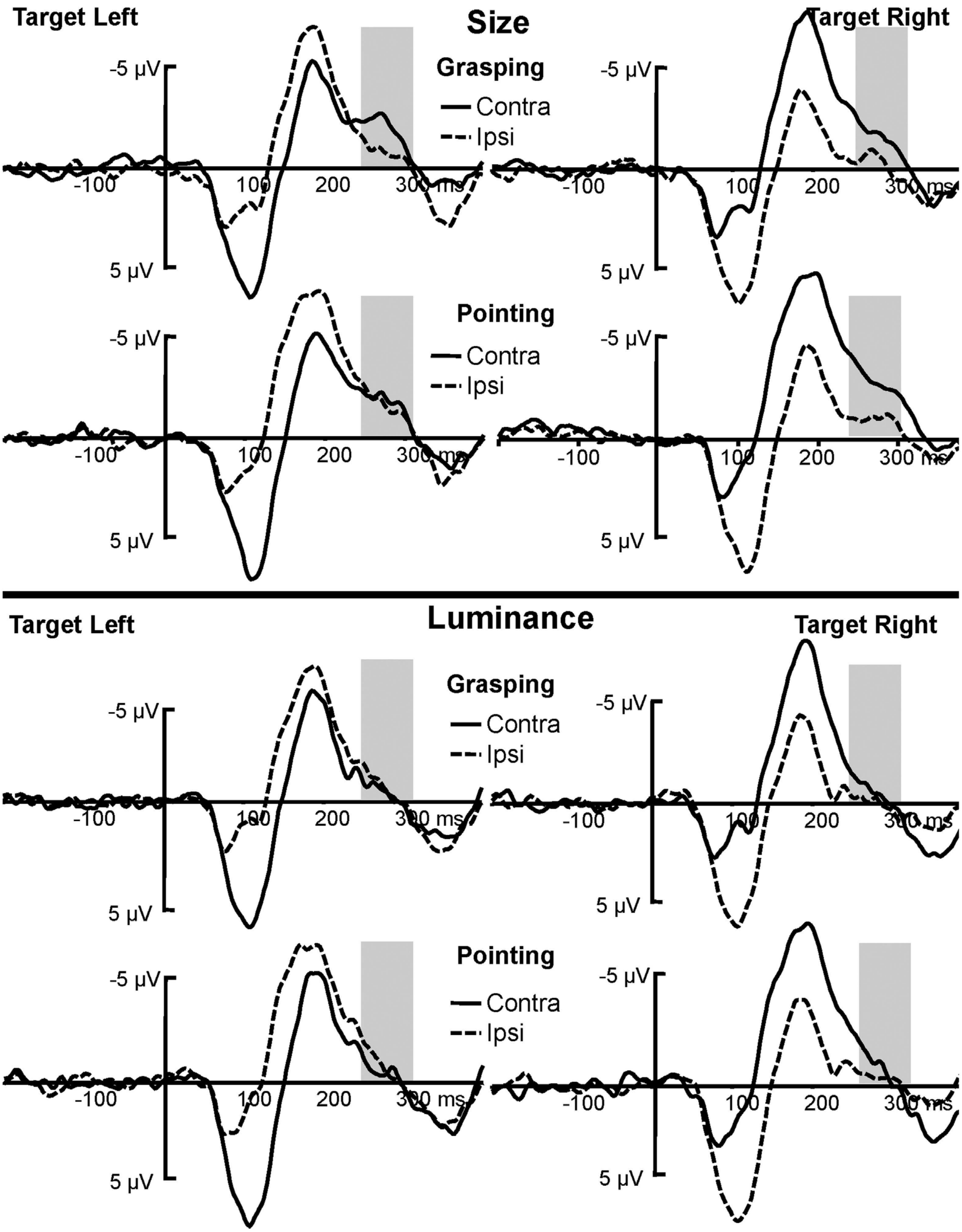

FIGURE A3 | The N2pc for left and right targets separately for PO7/8 electrode pair. In the size and luminance tasks and grasping and pointing condition, respectively. The dark gray box depicts the N2pc time window (230–300 ms) for which statistical analyses have been conducted. Please note that PO8 is the contralateral electrode for targets on the left (solid lines in the graphs on the left), while it is ipsilateral for the targets on the right (dashed lines in the graphs on the right side). Therefore the visible difference between contra- and ipsilateral waveforms on the P1 and N1 components is due to more positive amplitude on the PO8 electrode, independent of condition. This might be related to attentional networks being located in the right cerebral hemisphere (e.g., Heilman and Van Den Abell, 1980; Mesulam, 1981; Sperry, 1974; Thiebaut et al., 2011).

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Age effects on controlling tools with sensorimotor transformations

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Controlling tools in technical environments bears a lot of challenges for the human information processing system, as locations of tool manipulation and effect appearance are spatially separated, and distal action effects are often not generated in a 1:1 manner. In this study we investigated the susceptibility of older adults to distal action effects. Younger and older participants performed a Fitts' task on a digitizer tablet without seeing their hand and the tablet directly. Visual feedback was presented on a display in that way, that cursor amplitude and visual target size varied while the pre-determined hand amplitude remained constant. In accordance with distal action effects being predominant in controlling tool actions we found an increase in hand movement times and perceptual errors as a function of visual task characteristics. Middle-aged adults more intensely relied on visual feedback than younger adults. Age-related differences in speed-accuracy trade-off are not likely to account for this finding. However, it is well known that proprioceptive acuity declines with age. This might be one reason for middle-aged adults to stronger rely on the visual information instead of the proprioceptive information. Consequently, design and application of tools for elderly should account for this.

Keywords: distal action effect, ideomotor principle, perception, proprioception, proximal action effect, sensory integration, tool use, vision

INTRODUCTION

Controlling tools in technical environments bears a lot of challenges for the human information processing system, as locations of tool manipulation and effect appearance are spatially separated, and distal effects are often not generated in a 1:1 manner. When processing discordant information – for instance – from the moving hand (proximal action effect) and a moving cursor on a monitor (distal action effect) diverse crosstalk between proximal and distal action effects affect short-term compensation for as well as long-term adaptation to changes in sensorimotor transformations (e.g., Rieger et al., 2005; Heuer and Hegele, 2009; Ladwig et al., 2012; Sülzenbrück and Heuer, 2012; for recent overviews of empirical evidence see, e.g., Sutter and Sülzenbrück, 2012; Sutter et al., in press). Ladwig et al. (2012) let participants perform hand movements on a covered digitizer tablet while different gain factors varied the cursor amplitude, so that the cursor amplitude was shorter, equal or longer than the hand amplitude. Participants had to replicate the formerly performed hand amplitude as exactly as possible (but now without visual feedback) after finishing a trial. Drawn hand amplitudes were very exact when hand and cursor amplitude of the former movement corresponded. But, they were shorter (longer) when the former cursor amplitude was shorter (longer) than the former hand amplitude. That means performing hand movements while perceiving perturbed visual feedback originates after-effects in a subsequent movement. These findings speak in favor of a common representation of proximal and distal action effects, as proposed by the theory of event coding (Hommel et al., 2001). And furthermore (tool), actions are controlled with regard to their distal action effects. An increasing number of studies provides evidence for the dominance of distal action effects: for tool use (e.g., Kunde et al., 2007; Sutter, 2007; Massen and Prinz, 2008; Müsseler et al., 2008; Sutter et al., 2008, 2011; Lukas et al., 2010; Janczyk et al., 2012).

Studies by Sutter and colleagues (Sutter, 2007; Sutter et al., 2011) demonstrated for tools with different sensorimotor transformations the dominance of visual action effects in motor control. They varied amplitude and size of visual targets and found a log linear increase in movement times as a function of movement difficulty. The fit of the data with Fitts' law (Fitts, 1954) held for cursor movements controlled by a motion-transforming tool – a touchpad that translates finger movement on the pad surface into cursor movement on the monitor. But more interestingly, Fitts' law held in a comparable way for cursor movements produced by a force-driven tool (isometric joystick). Note that controlling an isometric joystick does not require any ballistic hand movement. The cursor movements result from finger force applied on the joystick alone. Consequently, the findings support the action effect account claiming that Fitts' law holds for action effect movements (i.e., the cursor movements), irrespective of the sensorimotor transformation of a tool. Or in other words, what counts most in this situation is the representation of the distal action effect (i.e., the cursor movement toward the stimulus), not the proximal effect (i.e., the hand manipulating the tool). As predicted by the ideomotor principle (James, 1890; Greenwald, 1970; for recent overviews of empirical evidence see, e.g., Hommel et al., 2001; Nattkemper and Ziessler, 2004), any intentional act requires a goal, that is, some anticipatory representation of the intended action effect. The anticipation of these action effects may fulfill a generative function in motor control: actors select, initiate, and execute a movement by activating the anticipatory codes of the movement's effects. These may be representations of body-related effects, like the proprioceptive/tactile feedback from the moving hand manipulating a tool (proximal effects). However, the intended action effects when using a tool are the movements of the effective part of the tool displayed on the monitor (distal effects). If both, proximal and distal action effects were equally important for controlling tool actions then any discrepancy between them would be a constant source of interference. The human information processing system apparently solves this conflict by favoring the distal action effects – as has been supported by an increasing body of research – while the proximal action effects are attenuated (e.g., Fourneret and Jeannerod, 1998; Knoblich and Kircher, 2004; Müsseler and Sutter, 2009; Wang et al., 2012). And it seems, that with increasing age distal action effects become even more influential in perception and action. This demonstrated a recent study by Wang et al. (2012): younger and older adults sat in front of a robot-arm and placed their dominant hand on a handle attached to the tip of the robot-arm. The hand of participants and the robot-arm were covered from view. The robot produced one of six pre-defined trajectories in the shape of an acute ($\gamma = 45°$ or 63° or 81°) or an obtuse triangle ($\gamma = 99°$ or 117° or 135°). All triangles were isosceles and had a constant horizontal base of 26 cm. Participants were instructed to follow the movement of the robot-arm with their hand on the handle and to monitor their hand movement very carefully. During the movement participants received no feedback (condition 1) or perturbed visual feedback on a display (condition 2). In the latter condition the cursor produced a static equilateral right-angled triangle with a horizontal base of 26 cm. The cursor movement was synchronized with the robot-arm's movement. After the completion of the movement participants were asked to evaluate the shape of their hand trajectory (acute or obtuse) by giving a verbal response. The results showed that participants were more uncertain about the shape of their hand trajectory when perturbed visual feedback was presented than when no feedback was present. This effect was more pronounced in older adults than in younger. The authors conclude that perturbed visual feedback attenuates the perception of hand movements and that older adults are more susceptible to distal action effects than younger adults.

When looking at developmental changes in motor behavior several studies demonstrate an increase of the amount of visual control in goal-directed aiming for the elderly: for instance, Pratt et al. (1994) investigated the impact of practice on movement kinematics of younger and older adults in a rapid aiming task. Participants manipulated a handle to perform aiming movements with a cursor on a display. Target amplitude and target size were always the same. The amount of practice varied between 100 trials (exp. 1) and 200 trials (exp. 2). For younger adults they found a modification in movement kinematics as a function of practice (exp. 1): the amount of visually controlled fine adjustments decreased (distance and time of secondary submovement decreased), so that movements became more pre-programmed (distance and time of primary submovement increased). These changes in movement kinematics represent an optimization in motor control according to Meyer et al., 1988; for an overview of empirical evidence see Elliott et al., 2001). Older participants did not adjust their motor behavior in such a way and remained controlling movements mostly visually. Even an extension of the practice phase (exp. 2) did not show any further adjustments of motor behavior.

Seidler-Dobrin and Stelmach (1998) varied the amount of visual feedback (movement execution with vs. without visual feedback) and investigated its impact on movement kinematics. Younger and older participant manipulated a lever to perform constant aiming movements with a cursor on a display. In the first and third block (40 trials each) visual feedback of the cursor movement was omitted in 10 of the trials after movement onset. The second block was the practice block (100 trials) in which visual feedback was constantly available. When visual feedback was omitted in the pre-practice block (first block), the distance of the primary submovement decreased (and consequently the distance of the secondary submovement increased) and endpoint accuracy decreased for both age groups. In the post-practice block (third block) young participants had been able to optimize movement control in the practice block – so that the primary submovement generally lengthened, independently of whether the visual feedback was present or not. In contrast, movement control in older participant did not benefit from practice with visual feedback, but remained the same and continued to highly depend on visual feedback.

In this context the present paper aims to explore age-related changes in distal action effect control. Participants were seated in front of a display and a digitizer tablet. A cover screened the tablet and the participant's hand, so that participants received proprioceptive/tactile feedback from their moving hand without seeing the hand itself. We presented two horizontally arranged target boxes on the display and asked participants to move the cursor (via pen on the digitizer tablet) several times per trial back and forth between the boxes. We varied the relationship between hand amplitude and cursor amplitude by introducing different gain factors (1:1.22; 1:2.44; 1:4.88), and we varied visual target size. Consequently, the pre-defined hand amplitude remained constant within a block, and the cursor amplitude and the visual target size randomly varied from trial to trial. After the completion of a trial, participants were asked to evaluate the length of their hand amplitude. The experimenter recorded the verbal response, after that the next trial appeared. In line with the previously mentioned studies (Pratt et al., 1994; Seidler-Dobrin and Stelmach, 1998) we held the pre-defined hand amplitude constant, so that participants were asked to produce the very same hand movement throughout a block. Yet, the perturbation of the visual feedback by introducing different gain factors led to varying cursor amplitudes and visual target sizes. This procedure is not trivial, since although there are a number of studies using Fitts' law to evaluate human-computer interaction (e.g., MacKenzie, 1992; Armbrüster et al., 2004; Sutter, 2007), previous studies often varied both hand movements and cursor movements with regard to amplitude and target size.

To disentangle action effects, we varied only distal action effects and kept the proximal action effects constant (cf. Rieger et al., 2005). The ideomotor principle (James, 1890; Greenwald, 1970) holds that any intentional action is controlled by anticipatory representations of the intended action's effect. These anticipations may be directed toward the goal of the hand's actions: if intended body-related effects control tool actions then movement times should remain constant, as the pre-defined hand amplitude remains constant within a block. However, and more likely as an increasing number of studies demonstrates, actors represent (tool) actions in terms of their intended distal action effects. In the present study, these are the effects of the cursor's movements on the display. Thus, we hypothesize that if task difficulty of cursor movements varies in terms of cursor amplitude and visual target size (Fitts, 1954 law) then hand movement times should increase as a function of task difficulty. We predict an increase of hand movement times with increasing cursor amplitude and decreasing visual target size, even though the pre-determined hand amplitude remains constant. This effect was recently found in our lab for a young population (Sutter et al., 2011, exp. 2), and should be replicated in this study.

Furthermore, the theory of event coding (Hommel et al., 2001) proposes a common representation of proximal and distal action effects, i.e., the event code. Consequently, both action effects may interact in action control and action perception: for instance, it has been found that if proximal and distal action effects do not correspond distal action effects predominated action control while proximal action effect were attenuated (e.g., Fourneret and Jeannerod, 1998; Knoblich and Kircher, 2004; Müsseler and Sutter, 2009; Wang et al., 2012). More important, there is also some crosstalk between proximal and distal action effects affecting action perception (e.g., Ladwig et al., 2012). Participants performed a discrete aiming movement and received perturbed visual feedback on the display. When asked to replicate the formerly performed hand amplitude (now without visual feedback), replicated amplitudes were longer (shorter) when the formerly seen cursor amplitude was longer (shorter) than the formerly performed hand amplitude. In the present study we asked participants to judge their hand amplitude by giving a verbal response instead of a motor response. In line with the findings of crosstalk between proximal and distal action effects we hypothesize that if distal action effects are predominant and therefore superpose proximal action effects then amplitude judgments of one's own hand movements should be more affected by (varying) cursor amplitudes than by (constant) hand amplitudes. We predict an increase in amplitude judgments of one's own hand movements with increasing cursor amplitude, although the pre-determined hand amplitude remains constant.

Finally, the present study investigated younger and middle-aged adults and expects age-related changes in distal action effect control. The optimized submovement model (Meyer et al., 1988) defines optimization of motor control when motor execution shifts over time on task from being more visually controlled to being more pre-programmed. Evidence has been presented above that older adults do not optimize movement execution in the same way as younger adults (e.g., Pratt et al., 1994; Seidler-Dobrin and Stelmach, 1998). Thus, while the amount of pre-programming increased in motor control of younger adults, older adults remained controlling movements visually. In line with these findings we hypothesize that middle-aged adults rely more on visual feedback than younger adults, and therefore the impact of distal action effects should more intensively unfold on hand movement times and judgments of hand amplitude of middle-aged participants than on the younger adults. We predict a stronger increase of hand movement times with increasing cursor amplitude and decreasing visual target size for middle-aged adults than for younger adults. For judgments of hand amplitude we also predict an interaction between age and cursor amplitude: for middle-aged adults we predict a stronger increase in amplitude judgments with increasing cursor amplitude than for younger adults.

MATERIALS AND METHODS

PARTICIPANTS

Fifteen students from the RWTH Aachen University (seven female, 17–34 years of age, mean age 24 years) participated for pay or course credit. Another 15 adults (eight female, 55–67 years of age, mean age 59 years) followed a call in a local newspaper and participated for pay. Thirteen of fifteen younger adults were graduate students, one participant was a high school-student and one participant had a profession based on an apprenticeship. Thirteen of fifteen middle-aged participants had professions based on a university degree (equivalent to a master degree), two of them had professions based on an apprenticeship. All participants reported to use a computer and a computer mouse daily (younger adults: $M = 2.7$ h/days; SD = 2.2; middle-aged adults: 1.4 h/days; SD = 1.4; $F(1, 25) = 3.12$; $p = 0.089$; $\eta^2 = 0.11$). All participants had normal or corrected-to-normal vision.

APPARATUS, TASK, AND STIMULI

Participants sat in front of a digitizer tablet (WACOM Intuos2 A3) that was operated with a pen (WACOM Intuos2 Grip Pen). Experimental tasks were presented on a 17″ CRT display (EIZO F563-T) with a 1024 × 768 resolution. A cover screened the digitizer tablet and the moving hand from the view. On top of the cover a measuring tape was fixated. Participants were only able to see the display on which the cross-hair cursor (length 0.8 cm × 0.8 cm) and the target boxes were presented. The task involved moving the cursor back and forth between two horizontally arranged target boxes. Each trial lasted until 25 error-free movements occurred. This task design was adapted from Rieger et al. (2005). After finishing a trial participants were asked to estimate the average hand amplitude of the successful 25 movements.

The movement distance of the hand (hand amplitude) was the same for all trials within a block (20, 40, or 60 mm). Within each block movement distance of the cursor (cursor amplitude) varied as a result of gain factor [1.22 (low gain), 2.44 (middle gain), and 4.88 (high gain)]: the cursor amplitude was 24 (low gain), 48 (middle gain), and 97 mm (high gain) within the 20-mm block, 48, 97, and 195 mm within the 40-mm block, and 73, 146, and 292 mm within the 60-mm block. Additionally, within each block the target sizes varied randomly with 5, 10, 20, and 40 mm. The combination of 24-mm cursor amplitude and 40-mm target size in the 20-mm block was skipped from the procedure, as overlapping target boxes resulted.

PROCEDURE AND DESIGN

Participants were instructed to continuously move the cursor back and forth between the two target boxes. As soon as they reached one target box the movement direction should be reversed without pausing in the target box. The instruction stressed the need to move continuously, and to move as fast and turn as accurately as possible. When 25 error-free movements were performed the screen turned blank. Participants then made a verbal judgment about the average length of their hand amplitude on the tablet (in cm), which was recorded by the experimenter.

The participants worked throughout the three blocks of hand amplitudes. The order of blocks was counterbalanced across participants. Within a block, cursor amplitude and visual target size were randomly varied. Each block consisted of 11 amplitude/size combinations (20-mm block) or 12 amplitude/size combinations (40- and 60-mm block) with 25 repetitions and additional 3 × 25 training trials in advance of the experimental trials. In total, the experiment lasted about 45 min.

RESULTS

For each block of hand amplitude data were separately analyzed. The *mean deviation* between the pre-determined hand amplitude and the estimated hand amplitude was analyzed with ANOVAs with the within-subject factors gain (low, middle, and high) and the between-subject factor age (young and middle-aged). Due to technical failure the verbal judgments of one middle-aged participant were not recorded. The *mean movement time* (the interval between a target-to-target movement, averaged across a successful 25 movement cycle) was calculated for error-free trials and analyzed with ANOVAs with the within-subject factors gain (low, middle, and high) and target size (5, 10, 20, and 40 mm), and the between-subject factor age (young and middle-aged). *Mean error rates* were calculated on the number of trials, where the reversal point of movement was outside the target box (averaged across a successful 25 movement cycle). A reversal point between start box and target box represents an undershoot, a reversal point behind the target box represents an overshoot. Before further analysis error rates were arc sin transformed. Then, data were analyzed with ANOVAs with the within-subject factors gain (low, middle, and high) and target size (5, 10, 20, and 40 mm), and the between-subject factor age (young and middle-aged). Additionally, for error trials we calculated the mean deviation between the pre-determined hand amplitude and the actual hand amplitude (=over-/undershoot). *Mean over-/undershoots* were analyzed with ANOVAs with the within-subject factors gain (low, middle, and high) and the between-subject factor age (young and middle-aged).

MEAN DEVIATION OF JUDGMENTS

Figure 1 depicts the results for the mean deviations of judgments.

For all hand amplitudes the analyses revealed a significant main effect for the factor age [20 mm: $F(1, 27) = 4.10$; $p = 0.053$; $\eta^2 = 0.13$; 40 mm: $F(1, 27) = 6.83$; $p = 0.014$; $\eta^2 = 0.20$ and 60 mm: $F(1, 27) = 4.77$; $p = 0.038$; $\eta^2 = 0.15$]. The factor gain reached significance for the 40 mm and 60 mm amplitude, while for the 20 mm amplitude a corresponding trend was observed [20 mm: $F(2, 54) = 2.81$; $p = 0.69$; $\eta^2 = 0.09$; 40 mm: $F(2, 54) = 11.21$; $p < 0.001$; $\eta^2 = 0.29$; 60 mm: $F(2, 54) = 7.46$; $p = 0.001$; $\eta^2 = 0.22$]. Furthermore, the factors age and gain interacted significantly for the 40 mm amplitude, a corresponding trend was observed for the 60 mm amplitude [20 mm: $p = 0.364$; 40 mm: $F(2, 54) = 3.63$; $p = 0.033$; $\eta^2 = 0.12$; 60 mm: $F(2, 54) = 2.44$; $p = 0.097$; $\eta^2 = 0.08$].

Across amplitudes middle-aged adults generally overestimated hand amplitudes stronger than younger adults (20 mm: 4.0 vs. 2.3 cm; 40 mm: 5.3 vs. 2.9 cm; and 60 mm: 6.4 vs. 3.7 cm). Concerning the impact of distal action effects results showed that although the pre-defined hand amplitude remained constant judgments increased as a function of gain (40 mm: by 2.4 cm and 60 mm: by 3.2 cm).

Post hoc tests with Bonferroni adjustments were carried out on judgments of younger and middle-aged adults separately. All judgments significantly deviated from zero ($ps < 0.05$). Considering the interaction between age and gain (40 mm hand amplitude) *post hoc* tests confirmed an increase in judgments as a function of gain for middle-aged adults ($p < 0.05$), but not for younger adults. Consequently, distal action effects – in terms of gain – mainly affected judgments of middle-aged adults (40 mm amplitude).

MEAN MOVEMENT TIME

Mean movement times as a function of age and gain are depicted in **Figure 2**. For all hand amplitudes analyses showed a significant main effect for the factor gain [20 mm: $F(2, 56) = 43.97$; $p < 0.001$; $\eta^2 = 0.61$; 40 mm: $F(2, 56) = 31.68$; $p < 0.001$; $\eta^2 = 0.53$; 60 mm: $F(2, 56) = 38.42$; $p < 0.001$; $\eta^2 = 0.58$]. The main effect of the

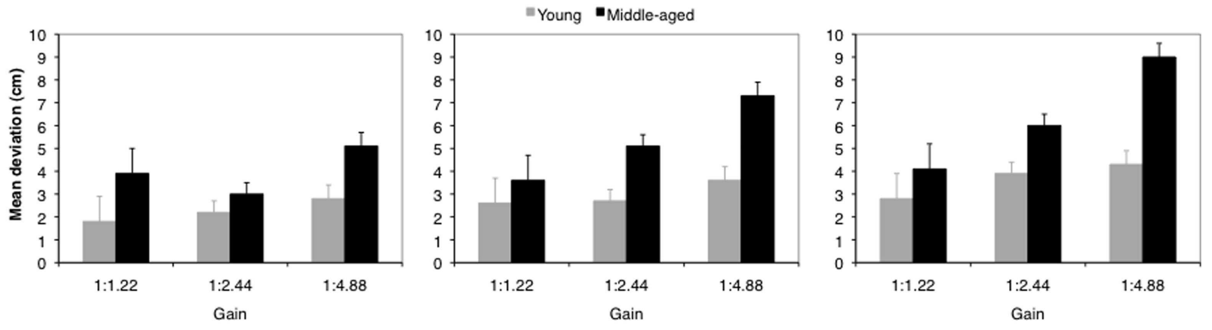

FIGURE 1 | Mean deviation (cm) between pre-determined hand amplitude and estimated hand amplitude for a pre-determined hand amplitude of 20 mm (left), 40 mm (middle), and 60 mm (right). Error bars represent the standard error of the mean.

factor age was significant for the 60 mm hand amplitude only [$F(1, 28) = 4.03$; $p = 0.054$; $\eta^2 = 0.13$]. Analyses further revealed significant interactions between the factors age and gain for the 40 and 60 mm hand amplitudes [40 mm: $F(2, 56) = 4.0$; $p = 0.024$; $\eta^2 = 0.13$; 60 mm: $F(2, 56) = 6.51$; $p = 0.003$; $\eta^2 = 0.19$] and a corresponding trend for the 20 mm hand amplitude [20 mm: $F(2, 56) = 2.73$: $p = 0.074$; $\eta^2 = 0.09$].

As depicted in **Figure 2** movement times generally increased with a high gain compared to the low and middle gain. This is insofar remarkable, because hand movement times arising from constant hand amplitudes rose as a result of the increasing cursor amplitude. For longer hand amplitudes (40–60 mm) the impact of gain stronger affected middle-aged adults than younger adults.

The factor target size revealed significant main effects across all hand amplitudes [20 mm: $F(3, 84) = 113.70$; $p < 0.001$; $\eta^2 = 0.80$; 40 mm: $F(3, 84) = 97.47$; $p < 0.001$; $\eta^2 = 0.78$; 60 mm: $F(3, 84) = 69.64$; $p < 0.001$; $\eta^2 = 0.71$]. Target size significantly interacted with age [20 mm: $F(3, 84) = 3.73$; $p = 0.014$; $\eta^2 = 0.12$; 40 mm: $F(3, 84) = 2.74$; $p = 0.048$; $\eta^2 = 0.09$; 60 mm: $F(3, 84) = 6.10$; $p = 0.001$; $\eta^2 = 0.18$]. Finally, gain and target size interacted significantly for the 20 mm hand amplitude [$F(6, 168) = 7.78$; $p < 0.001$; $\eta^2 = 0.22$]. Other main effects or interactions did not reach significance (ps > 0.222).

In all gain conditions movement times were lowest for the largest target and increased as a function of target size. Concerning the factors age and target size, movement times increased from 406 to 952 ms for younger adults and from 390 to 1304 ms for middle-aged adults for the 20 mm amplitude. For the 40 mm (60 mm) hand amplitudes movement times increased from 497 to 1260 ms (702 to 1363 ms) for younger adults and from 589 to 1724 ms (789 to 2171 ms) for middle-aged adults. This shows that target size had a stronger impact on movement times of middle-aged adults than on movement times of younger adults.

MEAN ERROR RATE AND MEAN OVER-/UNDERSHOOT IN ERROR TRIALS

Figure 3 depicts the mean error rate as a function of age and gain. Across all hand amplitudes a significant main effect of the factor gain was found [20 mm: $F(2, 56) = 13.80$; $p < 0.001$; $\eta^2 = 0.33$; 40 mm: $F(2, 56) = 24.68$; $p < 0.001$; $\eta^2 = 0.47$; 60 mm: $F(2, 56) = 15.71$; $p < 0.001$; $\eta^2 = 0.36$]. The main effect of the factor age was significant for the 20 mm hand amplitude only [$F(1, 28) = 5.84$; $p = 0.022$; $\eta^2 = 0.17$]. For the 20 mm hand amplitude error rates increased as a function of gain from 2 to 6% (young: $M = 2\%$; middle-aged: $M = 5\%$). For the 40 mm (60 mm) hand amplitude gain alone mediated error rates with a general increase from 4 to 9% (9–17%).

The factor target size revealed significant main effects for all hand amplitudes [20 mm: $F(3, 84) = 43.67$; $p < 0.001$; $\eta^2 = 0.61$; 40 mm: $F(3, 84) = 96.79$; $p < 0.001$; $\eta^2 = 0.78$; 60 mm: $F(3, 84) = 66.05$; $p < 0.001$; $\eta^2 = 0.70$]. The interaction between gain and target size was significant for the 40 mm amplitude [$F(6, 168) = 2.38$; $p = 0.031$; $\eta^2 = 0.08$]. A corresponding trend was

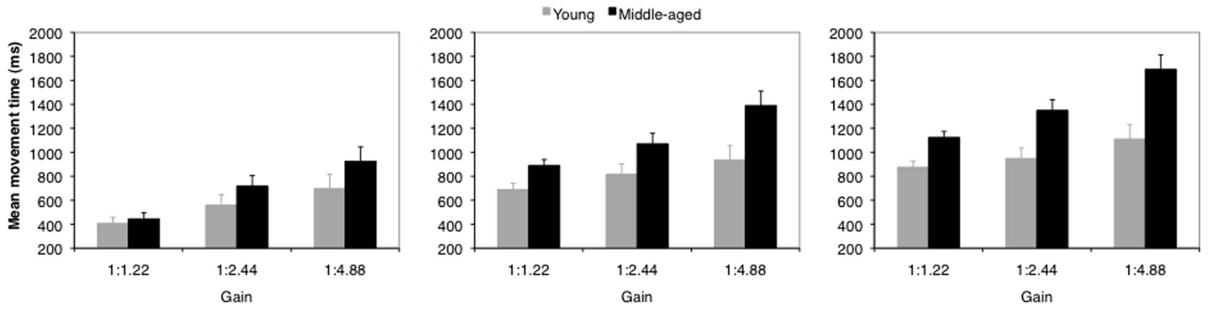

FIGURE 2 | Mean movement times of younger (gray) and middle-aged (black) adults as a function of gain for a hand amplitude of 20 mm (left), 40 mm (middle), and 60 mm (right). Error bars represent the standard error of the mean.

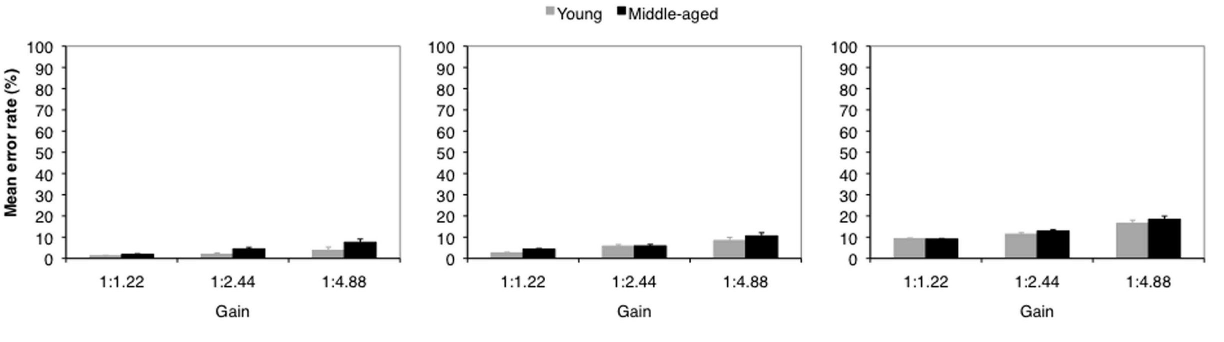

FIGURE 3 | Mean error percentages of younger (gray) and middle-aged (black) adults as a function of gain for a hand amplitude of 20 mm (left), 40 mm (middle), and 60 mm (right). Error bars represent the standard error of the mean.

found for the 20 mm amplitude [$F(6, 168) = 1.92$; $p = 0.079$; $\eta^2 = 0.06$]. For the 60 mm hand amplitude a significant interaction between the factors age and target size was observed [$F(3, 84) = 2.86$; $p = 0.041$; $\eta^2 = 0.09$]. Other main effects or interactions were not significant ($ps > 0.121$).

Error rates increased stronger as a function of target size in the high gain condition than in the low gain condition. The pattern of results in error rates resembles that of movement times, so that data were not confounded by a speed-accuracy trade-off.

Error trials were further analyzed with regard to the mean over-/undershoots. The ANOVAs with the within-subject factors gain (low, middle, and high) and the between-subject factor age (young and middle-aged) showed significant main effects of the factor gain for all hand amplitudes [20 mm: $F(2, 56) = 29.89$; $p < 0.001$; $\eta^2 = 0.52$; 40 mm: $F(2, 56) = 17.19$; $p < 0.001$; $\eta^2 = 0.38$; 60 mm: $F(2, 56) = 18.80$; $p < 0.001$; $\eta^2 = 0.40$]. All other effects or interactions did not reach significance ($ps > 0.134$).

For the 20 mm hand amplitude with low gain we observed a small overshoot (+1.54 mm), for all other conditions participants undershot the target area by −2.52 mm (min.) to −16.56 mm (max.). Undershoots increased as a function of gain. That means, although actual hand amplitudes in error trials deviated from the pre-defined hand amplitude, the observed undershoots can not account for overestimations observed in hand amplitude judgments.

GENERAL DISCUSSION

In this study we investigated the susceptibility of older adults to distal action effects. Younger and older participants performed a Fitts' task on a digitizer tablet without seeing their hand and the tablet directly. Visual feedback was presented on a display in that way, that cursor amplitude and visual target size varied while the pre-defined hand amplitude remained constant. In accordance with distal action effects being predominant in controlling tool actions (e.g., Kunde et al., 2007; Sutter, 2007; Massen and Prinz, 2008; Müsseler et al., 2008; Sutter et al., 2008, 2011; Lukas et al., 2010; Janczyk et al., 2012) we proposed an increase in hand movement times as a function of cursor amplitude and visual target size (Fitts, 1954). Since pre-defined hand amplitudes remained constant, any changes in distal action effects should be mirrored by variations in movement times if actions were predominantly distally controlled. And indeed, this is what we found. Hand movement times were strongly determined by cursor amplitude and target size. Both visual task characteristics accounted for most variance in movement times (η^2 between 0.53 and 0.80). This strong impact of distal action effects on tool actions is a successful replication of a recent finding (Sutter et al., 2011, exp. 2). And to further this, a similar and even stronger influence of visual task characteristics on movement times was found for middle-aged adults. This result was being hypothesized on the fact, that the elderly control manual actions to a larger amount visually than the younger (e.g., Haaland et al., 1993; Pratt et al., 1994; Seidler-Dobrin and Stelmach, 1998). To rule out that any speed-accuracy trade-off may have mediated the age effect, we looked at error rates in detail: across age groups we found that error rates were strongly moderated by gain and visual target size. However, they followed the same pattern of results as found in movement times: both dependent variables rose due to extensions in gain and to reductions in visual target size. Between age groups error rates only differed significantly for the 20 mm hand amplitude. In that case error rates were 3% higher for middle-aged adults than for younger adults (5 vs. 2%). But, at the same time middle-aged adults were also slower than younger adults. Thus, the often found strategic differences (cf. Pachella, 1974; Welford, 1976) between younger adults – emphasizing speed and neglecting accuracy – and older adults – emphasizing accuracy and neglecting speed – could not account for the age differences found in this experiment. Thus, we can preclude speed-accuracy trade-off as confounding factor.

However, another possible confound may emanate from the methodology we used. The study was designed as a cross-sectional study. We controlled age groups for specific demographic factors, like gender and daily computer usage. The latter factor might be critical for the present experiment: we observed a trend for middle-aged adults spending less time per day in front of a computer compared to younger adults. The relationship between computer mouse movements and cursor movements is basically similar to the transformation scaling gain used in the present experiment. So it could be that less computer usage led to a disadvantage for the middle-aged and that this accounted for the age differences found in the present experiment.

However, on the one hand, we asked participants about their daily computer usage, but not about their exposure to computers and computer input devices over their life span. This should be done in future studies, because it seems to be very likely, but we can only speculate about this point at the moment, that middle-aged adults spend more years (but less time per day) using a computer, and vice versa for younger adults. Consequently, the amount of life time exposure might even the amount of daily usage out. On the other hand, it is known that transformations scaling gain are very easy to learn and adaptation occurs very fast (Bedford, 1994; Bock and Burghoff, 1997; Seidler et al., 2001; Rieger et al., 2005; Sutter et al., 2008). Moreover, middle-aged and younger adults comparably improve performance by practice (e.g., Armbrüster et al., 2007) and adapt to gain transformations (e.g., Heuer and Hegele, 2007). Thus, although we can not fully rule out a possible confound, previous studies give reason that the group differences found in our cross-sectional study relate to developmental changes. However, further light could be shed on this point by conducting a longitudinal study.

Our second hypothesis was concerned with the impact of distal action effects on the proprioceptive/tactile perception. There is evidence for actors being less aware of what they do with their hands when there is a discrepancy between proximal and distal action effects (e.g., Fourneret and Jeannerod, 1998; Knoblich and Kircher, 2004; Müsseler and Sutter, 2009; Wang et al., 2012). In the experiment we asked participants to estimate the average amplitude of their hand movement after finishing 25 error-free movements. In general, judgments were most precise when the low gain only slightly perturbed the relation between hand and cursor amplitude and it became increasingly imprecise with higher gain factors. That means, although the pre-determined hand amplitude remained constant, participants were not really aware of what they were doing with their hand. Moreover, middle-aged participants stronger overvalued their hand amplitude than younger participants. This was particularly the case for longer hand amplitudes.

A critical point is, that error rates increased with increasing gain and longer hand amplitudes, too. It could have been that participants systematically overshot the target area in error trials and that this biased their judgments. Fortunately, the data revealed a contrary pattern of results, and confirms distal action effects mediated the perceptual bias.

To shed further light on these findings age-related changes in sensory performance will be discussed in more detail. It is well known that proprioceptive acuity declines with age (e.g., Cooke et al., 1989; Darling et al., 1989; Adamo et al., 2007; Boisgontier et al., 2012). For instance, Adamo et al. (2007) compared the proprioceptive acuity between younger and older participants in an elbow-position matching task. Having only proprioceptive information to match a former position increased matching errors for the older adults more intensely than for younger. Concerning the present study, judgment errors resembles this pattern of results. Although participants were instructed to monitor their hand movement carefully, judgments were quite inaccurate across all conditions. Judgment errors were more pronounced in middle-aged adults than in younger adults, and inaccuracy more strongly increased in middle-aged adults when cursor amplitude increased. The same impact of visual feedback was also found in motor behavior. Thus, middle-aged adults obviously rely on visual feedback in perceiving and controlling actions. One could argue that the tendency of older adults to allocate more resources on controlling movements visually (see also Haaland et al., 1993) might be a useful (compensation) mechanism against increasing neural noise in the motor system (Welford, 1984). Quantitative models of multisensory integration (Ernst and Banks, 2002) assume that information from all involved senses contribute to a percept in an optimized fashion, so that the reliability of the percept is maximized. Concerning motor actions, the proprioceptive information of limb movements, however, is highly variable (e.g., Cooke et al., 1989; Darling et al., 1989; Fourneret and Jeannerod, 1998; Knoblich and Kircher, 2004; Adamo et al., 2007; Müsseler and Sutter, 2009; Boisgontier et al., 2012; Wang et al., 2012), and less reliable than the visual perception (e.g., van Beers et al., 1998). Aging, however, increases the variance in the proprioceptive sense (Welford, 1984). And consequently, it makes perfect sense that the visual information becomes even more important with increasing age. Further experiments are needed to substantiate this interpretation. One way to investigate the integration of visual and proprioceptive information is to add noise to either the visual or the haptic sense (e.g., Serwe et al., 2009; Sutter and Ladwig, 2012). For instance applying vibration to a moving hand adds noise to the afferent information of the proprioceptive sense. Consequently, actions that were controlled by their proximal action effects when vibration was not present shifted to be visually controlled when the vibration was in effect (Sutter and Ladwig, 2012).

In conclusion, based on the cognitive account of action effect control (James, 1890; Greenwald, 1970; Hommel et al., 2001) our results demonstrated that distal action effects predominantly determined how actors perceive and interact with tools. Movement times varied as a function of gain while the pre-determined hand amplitude remained constant. This is insofar surprising, as it represents a highly demanding and resource-limiting behavior. If participants had been able to ignore the visual feedback completely, then the very same motor program (e.g., Schmidt, 1988; Elliott et al., 2001) would have fitted for all movements within a block. Whereas the younger participants (according to their judgments) seemed to have realized that they were performing the same movements all the time, movement times nevertheless increased due to the cursor amplitude. The influence of the visual feedback unfolded even more intensely in middle-aged adults. On the one hand age-related limits in cognitive processing capacities can be assumed, since increasing task difficulty extended the performance gap between younger and middle-aged adults. On the other hand and more likely, age-related changes in the proprioceptive acuity play the crucial role in this context. Thus, design and application of tools for the elderly should account for this.

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The influence of action effects in task-switching

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According to ideomotor theories, intended effects caused by a certain action are anticipated before action execution. In the present study, we examined the question of whether action effects play a role in cued task-switching. In our study, the participants practiced task-response-effect mappings in an acquisition phase, in which action effects occur after a response in a certain task context. In the ensuing transfer phase, the previously practiced mappings were changed in a random, unpredictable task-response-effect mapping. When changed into unpredictable action-effects, RT as well as switch-costs increased, but this occurred mainly in trials with short preparation time and not with long preparation time. Moreover, switch costs were generally smaller with predictable action-effects than with unpredictable action-effects. This suggests that anticipated task-specific action effects help to activate the relevant task-set before task execution when the task is not yet already prepared based on the cue.

Keywords: cued task-switching, action control, task set, task selection, preparation time

INTRODUCTION

The question of how human actions are mentally controlled is one that has been investigated since the early beginnings of cognitive psychological research in the nineteenth century (for a historical review see Stock and Stock, 2004; Shin et al., 2010, for a more recent review). According to the ideomotor principle, actions are controlled by internal processes – the anticipation of the intended action-effect (James, 1890). That is, the intended result of the action is already mentally represented before the action execution and controls the action.

A challenge for action control is to select and execute the appropriate action among a variety of feasible actions in a certain situation and to ignore or inhibit possibly competing actions. This implies that the system of action-selection processes has also to be very flexible. The context in which an action is performed is highly important (Brass et al., 2003; Kunde et al., 2007; Braverman and Meiran, 2010) and can change easily. This context and all other important components that are necessary to perform an action (or a task) are assumed to be mentally represented in a *task set* (see, e.g., Prinz et al., 2009). A task set contains information for example about the appropriate class of task-relevant stimuli, the stimulus-dimension, and the response mode (e.g., Philipp and Koch, 2010). The nature of a task set and its flexibility has been investigated in a variety of studies using the task-switching paradigm (see, e.g., Monsell, 2003; Kiesel et al., 2010; Koch et al., 2010; Meiran, 2010, for reviews).

In a typical task-switching experiment, subjects are instructed to switch between two reaction tasks that appear in a random order. It is usually found that performance [reaction time (RT) and error rate] is worse after a task switch than after a task repetition. These *switch costs* are supposed to reflect, at least partly, "executive" processes that are needed for switching between tasks. These executive processes imply on the one hand the maintenance of a mental representation of the current task. On the other hand, flexibility is required and the ability to inhibit the just executed task set. This includes the ability to overcome the persisting task activation (which is called *task-set inertia*), to shift attention to the new, currently relevant task-set, and to activate the new task-set (e.g., Allport et al., 1994; Rogers and Monsell, 1995; Meiran, 1996; Kiesel et al., 2010).

Action effects, that is, effects that happen as a consequence of the specific task execution, might help to reduce the conflict of ambiguous task-sets. When referring to action effects, we are referring to effects or events that take place after the response, as explicit consequence of the response. Examples for action effects are the tone that is heard after pressing a piano's keyboard or the light that is turned on after pressing the light switch.

The influence of action effects that occur after response execution can be realized by the *anticipation* of the effect, which happens temporally before response execution, as assumed in the ideomotor principle. For example, when playing a piano, the pianist anticipates already the tone he/she wants to produce before pressing the key (see for empirical evidence e.g., Keller and Koch, 2008). That this kind of anticipation not only takes place in humans but also in animals was shown in so-called "differential outcome" studies (for a review, see Urcuioli, 2005). In these studies, animals learn different responses to different stimuli that lead to different outcomes. It is assumed that in learning of stimulus-response-outcome contingencies, the outcomes (or action effects in our terminology) are part of what is learned and are not merely "reinforcers." That is, they serve as anticipatory cue to guide behavior by adding up to discrimination of the possible action alternatives (see Urcuioli, 2005).

When we assume that action effects influence response selection, action effects might also play a crucial role in task-switching. However, as far as we know, the impact of action effects have been neglected in task-switching research, even though in the literature, the concept of task-set appears often in conjunction with action control (e.g., Kunde et al., 2007; Hommel, 2009; Prinz et al., 2009). However, only few studies indeed combined a task-switching paradigm with effects that took place after the response.

Kiesel and Hoffmann (2004) reported one of the few studies that added action effects to a task-switching paradigm. They used this paradigm to create two different contexts (horizontal vs. vertical arrangement of a target) in which the same action (a key stroke) was performed, leading to two different action-effects (short/fast vs. long/slow movements of the target in the horizontal and vertical arrangement, respectively). Reactions were slower in the slow-movement context and faster in the fast-movement context, although these target movements occurred *after* the response. Hence, it was shown that action-effect associations are acquired according to the context and that the basically same actions are influenced by different (context-dependent) effect anticipation.

In another study, Ruge et al. (2010) provided task-related action effects in one condition and task-unspecific feedback about the correct execution of the task in another condition. Two target stimuli were horizontally *and* vertically aligned. A cue indicated if the position of the horizontal or the vertical target should be determined. The task-related action effect was a red light-up of the target in the position which indicated the correct response. This means, if the correct response was "right," the target on the right side appeared red as consequence of the response, whereas the vertical target stayed colorless; and if the correct response was "up," the target turned red on the up-position, whereas the horizontal target stayed colorless. The action effects were thus semantically associated with the correct response. The task-unspecific effect was just a feedback with the information whether the task was executed correctly or not. The authors found a significant two-way interaction of task transition and type of effect for trials with a long cue-target interval (CTI; i.e., 1500 ms). In the task-specific effect condition, residual switch costs were reduced compared to the unspecific effect condition. That is, anticipating task-specific action effects might help to discriminate the task sets of the upcoming trial and select the appropriate one. However, in this study, only trials with a long CTI were analyzed so that no statement can be made about how action effects influence task performance in trials with a short CTI.

Moreover, the question of whether the anticipation of action effects indeed influences the task activation process was not in the focus of these studies. According to ideomotor theories, action effects are mentally represented *before* response execution (e.g., James, 1890; Greenwald, 1970). That is, if action effects are mentally represented before response execution, the representation of action effects should influence the activation of a task set, for example by helping to discriminate the task sets.

The aim of the present study was to examine the role of action effects in task-switching. To this end, we devised a novel transfer paradigm. In order that intended effects can trigger actions, the regularities between the action and the following effect have to be acquired, so that stable action-effect associations result in effect anticipation prior to action execution (e.g., Elsner and Hommel, 2001, 2004; Dutzi and Hommel, 2009; cf. also Zießler et al., 2004). For this reason, we divided our experimental paradigm in two phases. In the first phase, the participants had to learn the task-response-effect contingencies so that task-response-effect associations could be established. These effects can then be anticipated after the cue is presented. In the second, transfer phase, the previously practiced mappings were changed into a random mapping, so that valid anticipation of action effects was no longer possible.

We assumed that in the acquisition phase the expected action-effects are anticipated after the presentation of the cue before task execution, probably during action planning (see Zießler et al., 2004), thus helping to further disambiguate the task set relative to the currently irrelevant, competing task set. We would like to note here that with the cue, both possible action-effects for the appropriate task (i.e., that occurring after the left or right response key-press) are anticipated. If the previously experienced task-response-effect mappings, however, are not valid anymore, as in the transfer phase, the implementation of the task-set should take more time because the effects as additional cues for task-set disambiguation cannot be utilized any longer prior to task activation. This prolongation, if observed, might be due to additional monitoring processes that "double-check" if the implemented task-set is appropriate.

If the influence of anticipated action-effects needs time to build up, we would assume an influence of task preparation time. To examine the influence of preparation time on the impact of action effects in task-switching, we manipulated the cue-target interval. If the task-response-effect association becomes stronger with more time to activate the task set, then we would expect a more pronounced increase in RT and switch costs for long preparation intervals compared to short preparation intervals. However, with the presentation of the cue, the cue-task association is as well retrieved, entailing a sustained bias to the relevant task components (Meiran, 2000; Koch and Allport, 2006; Meiran et al., 2008), so that the influence of anticipated action-effects becomes smaller when the task is already well prepared based on the cue. Given that the action effects are nominally task-irrelevant (and occur only *after* task execution), we assume that they play a stronger role primarily when the task is not yet fully prepared (i.e., with short CTI).

MATERIALS AND METHODS

PARTICIPANTS

Twenty-four students of the RWTH Aachen university (19 female, 5 male; mean age = 23 years) took part in the experiment. They received partial course credit or 8€. The participants were equally and randomly assigned to the experimental vs. the control group. Informed consent was obtained from all subjects, and the experiment was performed according to the ethical standards of the Declaration of Helsinki.

APPARATUS, STIMULI, AND TASKS

The experiment was programmed with the experimental runtime system ERTS (Version 3.33e, BeriSoft Cooperation, Frankfurt am Main, Germany). Participants sat in front of a screen with a viewing distance of approximately 60 cm. The stimuli consisted of digits ranging from one till nine, without the five. They appeared in white on a black background at the center of the screen with a height of 1.4 cm (vertical visual angle: 1.34°).

The two tasks were two numerical judgment tasks. In one task, the participants had to decide if a presented number was greater or less than five (i.e., the magnitude task). In the other task, the participants had to decide if the presented number was odd or even (i.e., the parity task). The tasks switched randomly and were indicated by a cue. Each number was framed by either a diamond or a rectangle. If the number was framed by a diamond, the magnitude task was required; if the number was framed by a rectangle, the parity task was required. The diamond was 3 cm high and 3 cm wide (vertical and horizontal visual angle: 2.86°). The rectangle was 3.6 cm high and 3.5 cm wide (vertical visual angle: 3.44°; horizontal visual angle: 3.34°). Responses were to be made by manually pressing one of two response keys (i.e., the left and right Alt-key) with the left or right index finger. Participants were instructed to press left for a smaller or an odd number and to press right for a greater or an even number. If the response given by the participants was correct, an action-effect occurred. The effects were assigned to the task and the response in the experimental group in the first eight "acquisition blocks." For example, for the magnitude task, visual action-effects and for the parity task, auditory action-effects occurred. If the response was "less" (left key-press), the background of the screen turned yellow and if the response was "greater" (right key-press), the background of the screen turned green. For the parity task, a honking tone was presented after the correct response for "odd" (left key-press) and a ringing tone after the correct response for "even" was presented (right key-press). The mapping between task, response, and action-effect was counterbalanced across participants in the experimental group. For the control group, the action effects were completely random, that is, participants could not establish an association between task, response, and action-effect and thus could not anticipate the action effect. If the response was wrong, nothing happened, the background just turned black in both groups. Participants were instructed to react as fast and as correct as possible. They were informed in both groups that the effects happening after the key-press should be used as feedback whether the task was performed correctly.

PROCEDURE

Each trial started with a cue, which appeared in half of the trials 100 ms prior to the target stimulus (CTI of 100 ms) and in the other half of the trials 900 ms prior to the target stimulus. The CTI duration varied randomly. Cues and stimuli stayed on the screen until a response was given. For both CTI levels, an action effect was presented for 200 ms immediately after a correct response. That is, the response-cue interval (RCI) was held constant at 400 ms and the response-stimulus interval (RSI) was 500 ms for trials with a short CTI and 1300 ms for trials with a long CTI.

Before the experiment started, the participants performed a practice block with 16 trials. The experiment consisted of eight acquisition blocks and one transfer block. The acquisition blocks as well as the transfer block comprised 96 trials each plus four warm-up trials that were not recorded. Altogether, one session lasted about 45 min.

In the experimental group, the task-response-effect associations were predictable so that a mental task-response-effect association could be established by the participants. In the transfer block, the learned associations were changed into random action-effects as in the control group. There was no difference between the acquisition blocks and the transfer block in the control group. To analyze the influence of the change in the action-effect assignment, performance in the last acquisition block (Block 8) was compared with that in the transfer block (Block 9).

DESIGN

Task transition (switch vs. repetition), block (Block 8 vs. Block 9), and CTI (short vs. long) were used as independent within-participants variables; group (experimental vs. control) was used as independent between-participants variable. RT and error rate were measured as dependent variables. Significance was tested at $\alpha = 0.05$.

RESULTS

In addition to the four non-registered warm-up trials, the first recorded trial of each block was excluded from analysis because it could not be categorized as a switch or repetition trial. All trials with RT less than 200 ms or exceeding 3 SD of each participant's mean were discarded as RT outliers (i.e., 2%). Furthermore, all incorrect trials and that following an incorrect trial were not included in the analysis. Errors occurred in 4.8% of the trials.

RT

The data were submitted to a four-way mixed analysis of variance (ANOVA) with the independent variables task transition, block, CTI, and group. To forestall the most important result: the four-way interaction of task transition, block, CTI, and group was significant, $F(1, 22) = 5.49$, $p = 0.029$, $\eta^2 = 0.2$ (see **Figure 1**). That is, the increase of switch costs in the transfer block with a short preparation interval takes place only for the experimental group, whereas there were no effects of block in the control group. To better understand this four-way interaction, below we split the analysis in the two experimental groups and analyzed them separately with two three-way ANOVAs. In the overall, four-way ANOVA, the main effect of group was not significant, $F = 1.2$. The only other interaction with the variable group was the two-way interaction of group and block, $F(1, 22) = 4.98$, $p = 0.036$, $\eta^2 = 0.185$. Only in the experimental group, RT increased in Block 9 (from 817 to 882 ms), but not in the control group (776 vs. 775 ms). This interaction is also reflected in the split ANOVA, as described below.

For the *experimental group*, the three-way interaction of task transition, block, and CTI was significant, $F(1, 11) = 12.34$, $p = 0.005$, $\eta^2 = 0.529$. Switch-costs increased significantly from Block 8 (75 ms) to Block 9 (226 ms) in short CTI trials, whereas they even somewhat decreased numerically from Block 8 (61 ms) to Block 9 (49 ms) in long CTI trials. Further, the two-way interaction of task transition and CTI was significant, $F(1, 11) = 6.7$, $p = 0.025$, $\eta^2 = 0.378$, showing that switch-costs decreased from 150 ms for short CTIs to 56 ms for long CTIs. The main effects of task transition, $F(1, 11) = 30.03$, $p < 0.001$, $\eta^2 = 0.732$, block, $F(1, 11) = 10.3$, $p = 0.008$, $\eta^2 = 0.484$, and CTI, $F(1, 11) = 11.05$,

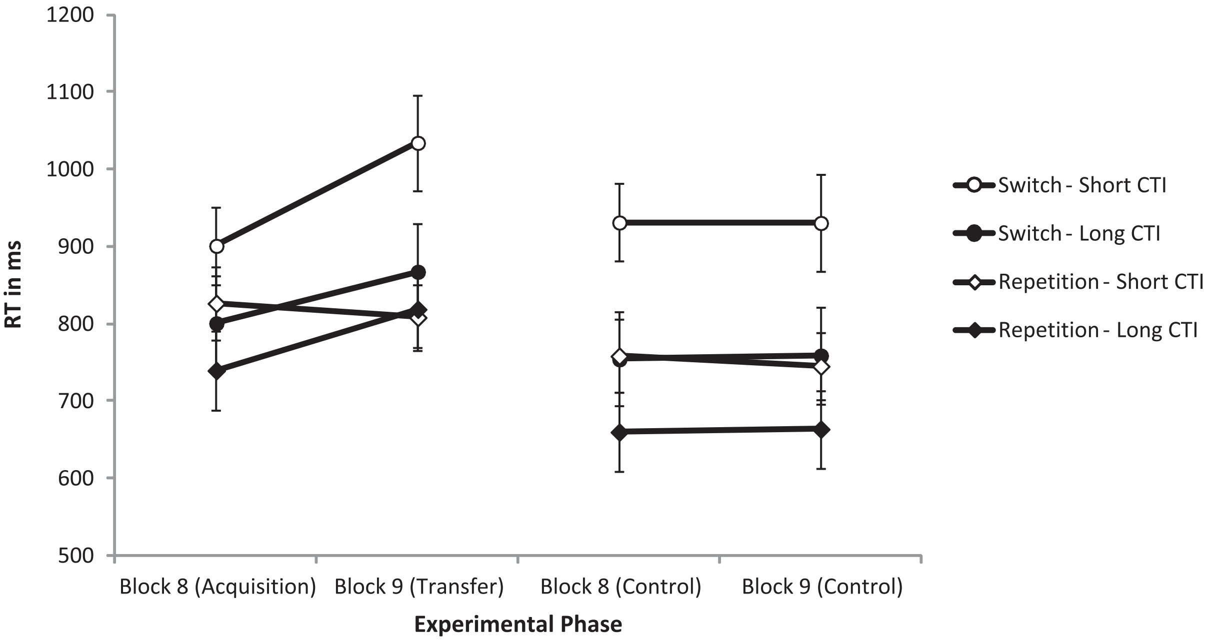

FIGURE 1 | Mean reaction times (RT) in ms as a function of task transition, experimental phase, group, and cue-target interval (CTI).

$p = 0.007$, $\eta^2 = 0.501$ were significant, too. RT was shorter for repetition trials (798 ms) than for switch trials (901 ms), for Block 8 (817 ms) than for Block 9 (882 ms) and for long CTIs (807 ms) than for short CTIs (893 ms). To go more into detail, and understand better the three-way interaction, we further split the analysis in two two-way ANOVAs for short and long CTI trials.

The trials with a short CTI showed a significant two-way interaction of task transition and block, $F(1, 11) = 6.75$, $\mathrm{p} = 0.025$, $\eta^2 = 0.380$, indicating that switch-costs increased in the transfer block. Further, the main effect of task transition was significant, $F(1, 11) = 35.38$, $p < 0.001$, $\eta^2 = 0.763$ whereas the main effect of block was just not significant, $F(1, 11) = 4.13$, $p = 0.067$, $\eta^2 = 0.273$. For the trials with a long CTI, the two-way interaction of task transition and block was not significant, $F < 1$. The main effect of task transition failed to reach significance, $F(1, 11) = 4.07$, $p = 0.069$, $\eta^2 = 0.270$, but the main effect of block was significant, $F(1, 11) = 11.26$, $p = 0.006$, $\eta^2 = 0.506$. RT was increased from Block 8 (770 ms) to Block 9 (843 ms).

In contrast to the experimental group, in the *control group*, the three-way interaction of task transition, block, and CTI was not significant, $F < 1$. The well-known two-way interaction of task transition and CTI was significant, $F(1, 11) = 11.03$, $p = 0.007$, $\eta^2 = 0.501$. It reflects a reduction of switch costs with a long preparation interval (short CTI: 179 ms, long CTI: 95 ms). The main effects of task transition, $F(1, 11) = 20.74$, $p = 0.001$, $\eta^2 = 0.653$, and CTI, $F(1, 11) = 47.69$, $p < 0.001$, $\eta^2 = 0.813$ were significant. RTs were faster in repetition trials (707 ms) than in switch trials (844 ms) and faster with a long preparation interval (709 ms) than with a short preparation interval (841 ms). There was neither a main effect of block, nor an interaction including this variable ($F\mathrm{s} < 1$).

In the analysis of trials with a long preparation interval in the experimental group, we found small and non-significant switch-costs, like in the study of Ruge et al. (2010). Hence, we further investigated if we could also find diminished residual switch-costs with predictable action-effects compared to unpredictable action-effects already in the acquisition phase. In order to do this, we analyzed RTs averaged across the first eight acquisition blocks (see **Table 1**). These data were submitted to a three-way ANOVA with a 2 (task transition) × 2 (CTI) × 2 (group) design. Noteworthy, the interaction of transition and group was significant, $F(1, 22) = 20.02$, $p < 0.001$, $\eta^2 = 0.476$. Switch costs were higher in the control group (190 ms) than in the experimental group (50 ms). Also, the three-way interaction of transition, CTI, and group was significant, $F(1, 22) = 4.51$, $p = 0.045$, $\eta^2 = 0.17$. Switch costs were especially high in the control group in short CTI trials (250 ms), but could be reduced with a long CTI (130 m), which is a reduction of 48%. However, in the experimental group, switch costs were even more reduced with a long CTI (from 75 to 24 ms), which is a reduction of 68%. In addition, also the expected interaction of transition and CTI was significant, $F(1, 22) = 28.29$, $p < 0.001$, $\eta^2 = 0.563$. Switch costs were higher in trials with a short CTI (162 ms) than with in trials with a long CTI (77 ms). As expected, the main effect of transition, $F(1, 22) = 58.41$, $p < 0.001$, $\eta^2 = 0.726$, and of CTI were significant, too, $F(1, 22) = 105.26$, $p < 0.001$, $\eta^2 = 0.827$. Neither the main effect of group was significant, $F = 1.3$, nor the interaction of CTI and group, $F = 2.29$.

To check at which point of time the switch-costs decreased for the experimental group, we additionally took a look at the switch costs in the first eight blocks for the experimental and the control group. The main effect of group was not significant, $F = 1.2$.

Table 1 | Mean RT in ms (and SE) of the first eight blocks (acquisition phase) as a function of task transition (repetition vs. switch), CTI (short vs. long), and group (Experimental group vs. Control group).

			Cue-target interval	
		Task transition	**Short**	**Long**
Group	Experimental group	Switch	989 (60)	855 (56)
		Repetition	913 (43)	831 (41)
	Control group	Switch	1017 (60)	811 (56)
		Repetition	767 (43)	681 (41)

Already in the first two blocks, the switch costs were smaller in the experimental group (Block 1: 81 ms; Block 2: 47 ms) than in the control group (Block 1: 267 ms; Block 2: 280 ms). The interaction of transition and group in the first two blocks was accordingly significant, $F(1, 22) = 20.02$, $p < 0.001$, $\eta^2 = 0.476$.

ERRORS

For the error rate, the four-way ANOVA for Block 8 and 9 was conducted. It revealed only a main effect of task transition with fewer errors in repetition trials than in switch trials (3.9 vs. 5.97%), $F(1, 22) = 7.86$, $p = 0.01$, $\eta^2 = 0.263$. No other main effect or interaction was significant, Fs < 1.9.

Also in the first eight blocks, no difference between group was shown, $F < 1$, nor an interaction with switch costs.

DISCUSSION

The aim of the present study was to investigate the role of action effects in task-switching. In order to examine this question, we designed a task-switching paradigm in which task-irrelevant, but predictable action-effects occurred. The task-response-effect mappings were practiced in eight acquisition blocks, so that action effects could be reliably anticipated. In a ninth (transfer) block, the action effects were random, so that the effects could no longer be anticipated in a task- and response-specific way. Additionally, the CTI was manipulated to examine effects of task preparation. The results showed that going from predictable (i.e., anticipated) to unpredictable action-effects increased both RT and switch costs. However, this influence of anticipated action-effects was found only in trials with a short CTI.

The fact that task-switching performance is impaired when previously learned action-effects cannot be anticipated anymore in a task-specific way relative to when action effects are unpredictable throughout the experiment shows that the predictable effects have been *anticipated* because otherwise an influence of stimuli that occur after response execution would be quite inexplicable. Hence, this influence of action effects is consistent with the already existing literature on effect anticipation (e.g., Elsner and Hommel, 2001; Koch and Kunde, 2002; Kunde et al., 2007). Moreover, the finding that the influence of action effects depends on both CTI and task transition (i.e., is largest on switch trials with short CTI) rules out a general "surprise" effect because this should be comparable across conditions. Note that even the concept of surprise presupposes that an expectation, that is, anticipation is violated, so that surprise actually assumes anticipation. However, we assume that the role of anticipated action-effects is more specific and lingering, because it cannot be ignored easily. A mere surprise effect should be transient and easier to overcome.

To account for the data, we assume that in short CTI trials, the task set of the previous task is still activated, so that a new implementation is not necessary in task repetition trials. But for switch trials, a new task-set has to be activated, which is a time-consuming process. In implementing the relevant task-set, also the task-response-effect associations are activated, which helps in further disambiguating the task set so that less interference between competing task-sets occurs. That is, for the magnitude task, the two visual action-effects and for the parity task, the two auditory effects are anticipated. The anticipation helps to activate and implement the correct task-set, reducing the switch costs. However, if the action effects are not predictable anymore, they cannot facilitate selection of task or response any longer, yielding higher RT in switch trials. In contrast, in trials with long CTIs, participants have enough time to activate the relevant task-set based on the cue, so that the switch-specific component of the facilitative influence of anticipated action-effects disappears. However, anticipated action-effects still show a general beneficial influence in task switches and repetitions alike, probably because the task-specific action-effect anticipation in the response selection process helps keeping the task sets better separate and thus counters stimulus-based task interference (e.g., Rogers and Monsell, 1995; Koch and Allport, 2006). Note that the idea that not the stimuli themselves are made more distinguishable from each other, but the task context or the required action is also endorsed by studies examining the differential outcome effect in animals (e.g., Honig et al., 1984; cf. also Urcuioli, 2005).

In explaining our data, we would like to point out again that the action effects are completely irrelevant to the task. Keep in mind that the predictable action-effects occur *after* the response and could therefore easily have been ignored. Nevertheless, they influenced task-switching performance, and specifically switch-costs, when the preparation time was short. The increased switch-costs as well as the increased RTs for long CTIs are an indication that the action effects could not be ignored, and thus affected task performance.

Action-selection can be externally controlled by stimuli or internally by goals and/or intentions. In earlier studies, it was shown that learning of task-effect associations only took place in an intention based experimental setting (Herwig et al., 2007; Herwig and Waszak, 2009). Thus, it is noteworthy that in our study, task-effect associations were built up even though learning was rather stimulus-based on the cue than intention based. However, Herwig and Waszak (2009) also stated that under certain experimental conditions, like more complex S–R mappings, action effects might become more important, thus allowing also "ideomotor" learning for stimulus-based actions. Moreover, it was recently shown that also stimulus-based settings can yield response-effect expectancies (Pfister et al., 2010) or effect-response preferences after a forced-choice acquisition phase (Pfister et al., 2011). Our study provides additional evidence that under forced-choice, stimulus-based action-effect learning takes place and influences the response behavior.

One would assume that the action-effect associations built up in the acquisition phase is weakened in the transfer phase. Consequently, the increase in switch costs should be higher in the beginning of the transfer block than at a later point of time. However, there is an alternative explanation: one could argue that participants realize that the action effects are not useful anymore to add up to activate a task set and adopt a task-processing strategy like in the control condition. As we have seen, switch costs are higher in the control group than in the experimental group. Consequently, an increase of switch costs at a later point of time in the transfer block should be assumed. We checked which of the two explanations holds by comparing the switch costs in the first half of the transfer block to the switch costs in the second half of the block for each group. However, we could not find an interaction of task transition, block half, and group. Switch costs changed neither in the control group nor in the experimental group significantly from the first to the second half of the last block. As already mentioned before, this finding speaks additionally against a mere "surprise effect" as surprise should have only a transient effect.

Our results also showed a difference in the switch costs between groups already in the acquisition phase. The experimental group with the predictable action-effects revealed smaller switch costs than the control group with the random action-effects, corroborating the results of the study of Ruge et al. (2010). However, the switch costs were mainly reduced because the repetition trials in the experimental group showed higher RT than the repetition trials in the control group. The RT in the control group and the resulting switch costs of about 190 ms are to be expected for a parity-magnitude task-switching experiment (cf. e.g., Arrington and Logan, 2005). That is, providing action-effects might lead to an aggravation of performance in repetition trials. It is possible, that the higher information content with predictable action-effects leads to longer task-processing time, during which the action effects are anticipated. But this is only observed in repetition trials because action-effect anticipation can be done simultaneously in the additional time in which a task switch is prepared. As soon as this additional information processing is done, the anticipated action-effects help to disambiguate different task-sets: In the experimental group, switch costs were not additionally increased in general, but they were proportionally more strongly reduced by a long preparation interval. In contrast to the action effects in the study of Kiesel and Hoffmann (2004) and Ruge et al. (2010), the action effects in our study were arbitrary and task-irrelevant. Nevertheless, they can easily be integrated in the task set. It can be assumed that, for example, visual action-effects, like the yellow or green background for the magnitude task, are incorporated as component to the task-set. This would imply that action effects within one task should be similar and easily distinguishable from the action effects of the alternative task, so that they have a positive effect in task performance (cf. Honig et al., 1984). However, action effects do not have to be similar to the respective response, concerning for example an ideomotor-compatible response-modality/effect-modality mapping (Greenwald, 1972), spatial compatibility (Ansorge, 2002; Pfister et al., 2010), numerical magnitude compatibility (Badets et al., 2012), verbal response-effect compatibility (Koch and Kunde, 2002), or the compatibility between key-alignment and respective tone production (Keller and Koch, 2008) in order to become associated with a task. This conclusion is also corroborated by studies of Hommel (2009), in which he investigated the influence of irrelevant action features. He found reliable correspondence effects for irrelevant action-effects. He concluded that not only intended, but also non-intended action effects are automatically integrated in the action code (*automatic integration hypothesis*). Although intended action-effects may be weighted more in the action code (Hommel, 2009; cf. Herwig and Waszak, 2009), having more influence on response selection, our results show that also non-intended, task-irrelevant action effects can show an important influence in task performance (cf. also Pfister et al., 2010, 2011).

This association seems to occur very fast, as the switch costs were already smaller in the first two blocks for the experimental group compared to the control group, although participants had only little opportunity to experience the predictable task-response-effect mapping. There is also evidence in the study of Kiesel and Hoffmann (2004) that the mental association between the predictable mappings is built up quickly: after the first half of the experiment, they interchanged the task context-action effect assignment. Performance did not differ in the two halves of the experiment, demonstrating that participants learned the new assignment very fast, even though the old assignment could have interfered. If action effects play a role in task implementation – and our results argue for it – only few encounters with the predictable mapping might be enough to build up an association (cf. also Dutzi and Hommel, 2009, who also argue for a fast response-effect binding).

Taken together, our data support the hypothesis that action effects play an important role in task implementation. Further, we can conclude that the learned associations are task-specific, that means task-response-effect associations instead of stimulus-response-effect associations, because the switch costs are affected. In this regard, one might ask if action effects do not only influence task-set activation but are even a part of the task set as a distinct task-set component. With respect to our study, this assumption is not yet necessary, but this may be a theoretical option that should be investigated in future studies. Either way, one should not forget that in our daily life, performing tasks has mostly external effects. Hence, to really understand how humans are dealing with multiple tasks in their daily life, action effects should be considered as an important influence in task performance in future studies.

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Up to "me" or up to "us"? The impact of self-construal priming on cognitive self-other integration

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The degree to which people construe their perceived self as independent from or interdependent with their social environment can vary. We tested whether the current degree of social self-construal predicts the degree to which individuals integrate others into their self-concept. Participants worked through tasks that drew attention to either personal interdependence (e.g., by instructing participants to circle all relational pronouns in a text, such as "we," "our," or "us") or independence (by having them to circle pronouns such as "I," "my," or "me") and were compared with respect to the social Simon effect (SSE) – an index of the degree to which people co-represent the actions of a co-actor. As predicted, the SSE was more pronounced in the interdependence group than in the independence group, suggesting that self-other integration varies dynamically as a function of the relative saliency of the other.

Keywords: self-construal priming, SSE, self-other integration

INTRODUCTION

Western societies take it commonly for granted that people own some sort of "self," a concept that refers to the phenomenal and social identity of a person over time and his/her responsibility for his/her actions. Eastern cultures are often more skeptical; e.g., Buddhism considers the self as only apparent and seeks to overcome it through systematic mental training (the anatta doctrine). Even though there is no agreed-upon definition of the concept (Neisser, 1988), authors often distinguish between what has been called the "minimal self" (Gallagher, 2000) and the "narrative/diachronic self" (Dennett, 1992). While the latter refers to the social identity people construct by actively creating their (ideally coherent) autobiography, the former refers, among other things, to the phenomenal experience that one has a body that is different from others' and that can be employed to actively change one's environment. How much that experience is fueled by, and thus depending on self-perception has been emphasized by Hume (1739/1978, p. 252): "when I enter most intimately into what I call myself, I always stumble on some particular perception or other, of heat or cold, light or shade, love or hatred, pain or pleasure," an observation that led Hume to conceptualize the self as a bundle of perceptions (a construction that roughly corresponds to James' concept of "me"; James, 1890). Hence, the cognitive system may represent oneself as just another event, that is, as an integrated network of codes representing one's own perceptual features (Hommel et al., 2009). Along the same line, very recently, it has been shown that Buddhist practice, which is assumed to "remove the barriers between oneself and others" (Dogen, 1976, p. 39), which should lead to a loss of discrimination between the representation of oneself and the representations of others, enhances self-other integration (Colzato et al., 2012).

The present study tested whether the degree of self-other integration is not only determined by such slow learning processes but also depends on more situational, dynamic factors. Previous research suggests that the degree to which individuals perceive themselves as dependent on, or independent from their social environment might vary rather quickly. For instance, Kühnen and Oyserman (2002) showed that having participants to circle all relational pronouns in a text, such as "we," "our," or "us," induces a global, context-sensitive processing strategy, while having them to circle pronouns referring to the self independent from others, such as "I," "my," or "me," induces a local, context-insensitive processing strategy. Even though this observation does not prove that priming can produce long-lasting modifications of the basic structure of self-perception, it does suggest that task and context can temporarily affect people's attention in such a way that they perceive themselves either as a part of a social context (as interdependent) or more in isolation (as independent). If so, one would expect that interdependence priming would lead them to integrate others into their own self-concept to a greater degree than independence priming. We assessed this hypothesis by testing whether self-construal priming modulates the social Simon effect (SSE; Sebanz et al., 2003).

The classical Simon effect shows that left and right actions are carried out faster if they spatially correspond to the stimulus signaling them (Simon, 1969). Recent studies revealed that this is the case even when the two actions are carried out by different people (i.e., the SSE), which has been taken to imply that task representations are socially shared (for overviews, see Sebanz et al., 2006). Very recently Hommel and colleagues (2009) provided evidence that the SSE occurs only if actor and co-actor are involved in a positive relationship (induced by a friendly acting, cooperative confederate) but not if they are involved in a negative relationship (induced by an intimidating, competitive confederate). Hence, the mere presence of another person is insufficient for the SSE to occur if this person is not involved in the task (Sebanz et al., 2003) or is perceived as intimidating and unfriendly (Hommel et al., 2009). This suggests that people consider the other person's action in their own representation of the current task and that the SSE can be considered to indicate the degree to which the participant has integrated another person's actions into his or her own task representation (Sebanz et al., 2003; Hommel et al., 2009). If drawing people's attention to personal interdependence or independence affects the degree to which people integrate others into their own self-concept, one would expect a more pronounced SSE with the former than with the latter.

MATERIALS AND METHODS

Forty-four healthy young adults, with a mean age of 22.5 years (SD = 2.4, range 18–30), participated for partial fulfillment of course credit or a financial reward. Written informed consent was obtained from all participants after a detailed explanation of the study procedures. The protocol was approved by the local ethical committee (Leiden University, Faculty of Social and Behavioral Sciences).

In the social Simon task participants made speeded discriminative responses to the color (green or blue) of circles by pressing one of two keys while the other key was operated by another participant (see **Figure 1**). Circles (diameter of 43 pixels) were equiprobably presented to the left or right (at a distance of 50 pixels) of a central fixation point (12 pixels) until the response was given or 1,500 ms has passed. Intervals between subsequent stimuli varied randomly but equiprobably, from 1750–2250 ms in steps of 100 ms. Participants were to ignore the location of the stimulus and to base their response exclusively on its color. Responses were to be given as fast as possible while keeping error rates below 15% on average; feedback about general speed (averaged between the RT of the two participants) was provided at the end of a trial block. The task consisted of one practice 60-trial block and three experimental 60-trial blocks. Just like in the original version of the task (Sebanz et al., 2003), the participants sat next to each other, attended to the same screen, and responded both with their dominant hand.

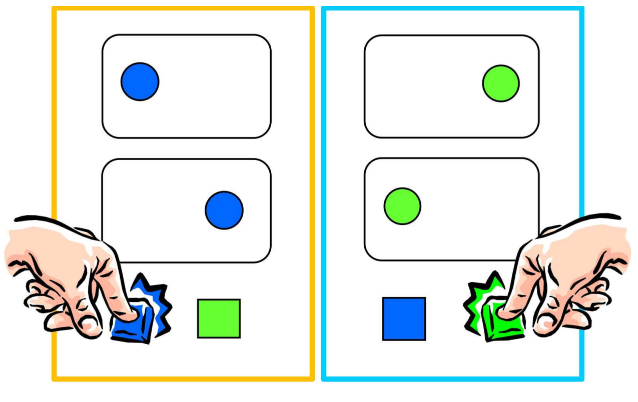

FIGURE 1 | Setting in the social Simon task: the task was distributed among two individuals. Each person responded to only one of the two colors.

Eleven pairs of participants, randomly determined, were asked to constantly switch between circling the independent (e.g., I, mine) pronouns in an essay for 2 min (independent self-construal priming) and completing a block of the social Simon task. The other 11 pairs of participants constantly switched between circling the interdependent (e.g., we, ours) pronouns in an essay for 2 min (interdependent self-construal priming) and performing a block of the social Simon task. Given that the experiment was composed of one practice and three experimental blocks, participants were to switch between the prime and the probe task four times in total. Participants were naïve to the experiment. When debriefed after the study, all participants thought that the study was about cooperation. None pointed out the possible relation between the social Simon task and the fact they had to circle the relational pronouns.

RESULTS

A significance level of $p < 0.05$ was adopted for all tests. Mean reaction times (RTs) from correct trials and error rates were analyzed by means of ANOVAs as a function of Priming Group (independence vs. interdependence) as between-participants factor and spatial stimulus-response Correspondence (correspondence vs. non-correspondence) as within-participants factor.

The reaction time analysis showed no evidence of a group effect, $F < 1$, but a main effect of correspondence, $F\ (1,\ 42) = 40.19$, $p < 0.001$, MSE = 50.777, $\eta_p^2 = 0.49$, indicated that responses were generally faster with stimulus-response correspondence than with non-correspondence (322 vs. 332 ms). More importantly, a significant interaction indicated that the correspondence effect on RT differed between groups, $F(1{,}42) = 4.65$, $p = 0.037$, MSE = 50.777, $\eta_p^2 = 0.10$, Even though the correspondence effect was reliable in both, the interdependence, $F(1{,}21) = 40.31$, $p < 0.001$, MSE = 45.472, $\eta_p^2 = 0.66$, and the independence group, $F(1{,}21) = 7.91$, $p = 0.010$, MSE = 56.083, $\eta_p^2 = 0.25$, the SSE was significantly more pronounced in the interdependence group (see **Figure 2**). Moreover, follow-up analyses showed that the two groups did not differ in the corresponding trials and that the size of SSE did not change over time adding block as additional factor in the ANOVAs, F's < 1.

Overall, error percentages on corresponding trials (0.06%) and non-corresponding trials (0.1%) were comparable and did not differ between Groups (F's < 1).

DISCUSSION

As expected, the SSE was more pronounced in the interdependence group than in the independence group. This finding suggests that having people work through tasks that draw attention to either personal interdependence or personal independence affects the degree to which people integrate other people with their own self-concept.

Hommel et al. (2009) suggested the Theory of Event Coding (TEC; Hommel et al., 2001) as theoretical framework to explain

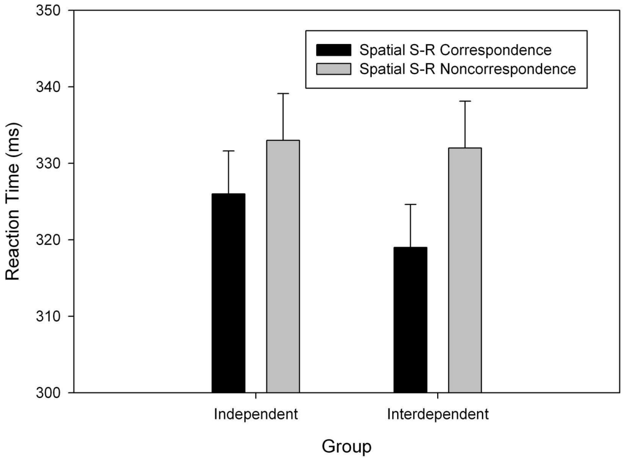

FIGURE 2 | Mean reaction time as a function of group (Independent vs. Interdependent) and spatial stimulus-response (S-R) correspondence. Error bars show standard errors of the means.

the mechanism underlying the SSE. TEC assumes that both perceived events and produced events (i.e., actions) are cognitively represented by codes of their perceptual features (such as color and shape of objects, the sensory feedback and affective consequences of actions, etc.). Along these lines, other people can be considered as just another type of event, which would be cognitively represented by codes of the features that describe what the given individual looks like, which perceivable action effects he or she is currently producing, which affective states are triggered by this person, and so forth. And the same would hold for the perceiving person him- or herself: one might represent oneself, including one's body parts, just as any other event and code oneself in terms of one's perceptual attributes and perceivable action effects. Self-other integration is, then, assumed to be a function of the overlap between the features bound to, and thus constituting self and other.

From this perspective, independence priming along the lines of Kühnen and Oyserman (2002) might be expected to operate by drawing attention to features that distinguish between me and other, while interdependence priming would draw attention to features that me and other are sharing. As suggested by Hume's (1739/1978) bundle theory of the self, self-perception (i.e., the current construal of one's minimal self) would not only be a function of the stimulus features characterizing me and other but also by the attentional weight each feature receives. Accordingly, weighting shared features more strongly would increase the perceived overlap between me and other while a stronger weighting of discriminating features would decrease the overlap. As suggested by Hommel et al. (2009), greater me-other overlap will increase the likelihood that the action of the other is considered in one's own task representation, which again increases the SSE.

What might be the mechanism responsible for this increase? There is increasing evidence that the SSE is sensitive to both social and non-social factors. For instance, Dolk et al. (2011, submitted) and Dittrich et al. (2012) showed that even non-social events can produce an SSE if they are sufficiently salient. And this is indeed what our theoretical framework suggests: social and non-social events are represented alike, even though there is evidence that social events are more salient and attract more attention (e.g., Friesen and Kingstone, 1998; Langton and Bruce, 2000). Dolk et al. (2011, submitted) suggest that the presence of another salient event in addition to the participant's own action induces uncertainty about agency, that is, it is no longer clear which of the two events is representing the participant's own action. Resolving this uncertainty requires the emphasis on features that discriminate between the action of the participant and the action of the co-actor. The most obvious and most salient feature in the standard task setup is relative location (Guagnano et al., 2010), which means that participants will attend more to, and code more strongly the location of their response (relative to the response of the other), thus creating the SSE. From this perspective, increasing self-other overlap (as by means of interdependence priming) is not the only way to increase the SSE but a particularly effective one.

With regard to cultural variations in the degree of self-other integration, our findings would be consistent with the assumption that culture-specific reward schedules operate on developing individuals. As we have argued elsewhere (Hommel and Colzato, 2010), individuals are likely to acquire preferences for particular control styles through selective reward from their peers. In particular, perceptual, attentional, and action-related processes are under the control of executive functions that specify control parameters (such as speed vs. accuracy, local vs. global processing, or inclusive vs. exclusive decision-making; see Logan and Gordon, 2001; Hommel, 2012), and it makes sense to assume that social reward can bias individuals toward particular ranges of parameter values (Hommel and Colzato, 2010; Hommel et al., 2011). Even though these biases or default values would be acquired in social situations, they are likely to generalize to any situation that is affected by the same executive control function. This would explain why the preference for a high degree of self-other integration in Asian cultures comes along with a more pronounced tendency for integration in non-social perceptual tasks (Boduroglu et al., 2009) and in Social Simon tasks (Colzato et al., 2012).

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Pointing hand stimuli induce spatial compatibility effects and effector priming

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The present study investigated the automatic influence of perceiving a picture that indicates other's action on one's own task performance in terms of spatial compatibility and effector priming. Participants pressed left and right buttons with their left and right hands respectively, depending on the color of a central dot target. Preceding the target, a left or right hand stimulus (pointing either to the left or right with the index or little finger) was presented. In Experiment 1, with brief presentation of the pointing hand, a spatial compatibility effect was observed: responses were faster when the direction of the pointed finger and the response position were spatially congruent than when incongruent. The spatial compatibility effect was larger for the pointing index finger stimulus compared to the pointing little finger stimulus. Experiment 2 employed longer duration of the pointing hand stimuli. In addition to the spatial compatibility effect for the pointing index finger, the effector priming effect was observed: responses were faster when the anatomical left/right identity of the pointing and response hands matched than when the pointing and response hands differed in left/right identity. The results indicate that with sufficient processing time, both spatial/symbolic and anatomical features of a static body part implying another's action simultaneously influence different aspects of the perceiver's own action. Hierarchical coding, according to which an anatomical code is used only when a spatial code is unavailable, may not be applicable if stimuli as well as responses contain anatomical features.

Keywords: spatial compatibility, perception and action, inter-individual interaction, hierarchical coding, effector priming, pointing

INTRODUCTION

Other's actions affect one's own cognitive processing and task performance. For example, the perceived direction of another's eye gaze is widely known to elicit reflexive attentional shifts, even when the gaze direction is non-predictive or counter-predictive (Friesen and Kingstone, 1998; Driver et al., 1999; Ristic and Kingstone, 2005; Galfano et al., 2012). Eye gaze has also been shown to activate responses on the side of its direction, eliciting the spatial compatibility effect (Ansorge, 2003; Zorzi et al., 2003; Ricciardelli et al., 2007): the phenomenon in which performance is better when a spatial stimulus feature (such as a location or symbolic spatial meaning) is congruent with a spatial response feature than when they are incongruent, irrespective of whether the spatial stimulus feature is relevant to the task (Kornblum et al., 1990; Simon, 1990; Umiltà and Nicoletti, 1990; Lu and Proctor, 1995; Hommel and Prinz, 1997; Proctor and Vu, 2006). Additionally, humans tend to imitate the gaze direction of other individuals (Ricciardelli et al., 2002).

In daily life, referential pointing with an extended index finger is ubiquitous. The index finger pointing gesture is used as a social cue to communicate spatial information; the performer's intent to indicate spatial direction and/or location might be stronger than his or her eye gaze (Burton et al., 2009). Recently, Ariga and Watanabe (2009) reported reflexive attentional shifts elicited by pointing pictures. Participants localized a target that appeared to the left or right. Before the presentation of the target, a leftward or rightward hand stimulus, whose direction was non-informative, was briefly presented. A larger attentional cueing effect was observed for hand stimuli with the index finger extended than for hand stimuli with the little finger extended, with the index and middle fingers extended, or with no finger extended (i.e., a fist) during short stimulus onset asynchronies (SOAs; 107 ms). In addition, the attentional cueing effect was smaller for hand stimuli with the index finger shortened to the length of the little finger, or with the little finger lengthened to the length of the index finger, than for the normal index finger pointing stimuli. These findings suggest that directional body parts affect a viewer's attention, and that the index finger pointing gesture is selectively strong during this process.

Perceiving a pointing hand stimulus would affect not only attention but also action. The spatial compatibility effect is one such case. Studies have found that the spatial compatibility effect (Eimer, 1995; Ansorge, 2003; Zorzi et al., 2003; Wühr and Kunde, 2006; Ricciardelli et al., 2007; Nishimura and Yokosawa, 2010a) can be induced by lateral and central stimuli that induce automatic attentional shifts, such as the sudden appearance (Posner, 1980) or disappearance (Theeuwes, 1991) of stimuli, eye gaze (Friesen and Kingstone, 1998; Driver et al., 1999; Ristic and Kingstone, 2005; Galfano et al., 2012), and arrows (Hommel et al., 2001b). Likewise, the pointing hand stimuli are expected to prime actions on the side of pointing direction and to elicit the spatial compatibility effect.

Another possible influence of the perception of a pointing hand stimulus on action is effector priming: perceiving another's body parts could prime an observer's action using the same body part. Recent studies have shown that left/right anatomical identity of the presented hand stimulus affects responses using the left or right hand (Ottoboni et al., 2005; Vainio and Mustonen, 2011).

Thus, the pointing hand stimulus could potentially affect manual responses in two ways: the spatial compatibility effect by its *spatial* meaning and effector priming by its *anatomical* hand identity. However, whether these two effects could emerge simultaneously is unclear. Concerning the horizontal (i.e., left/right) response coding in the spatial compatibility effect with spatial stimulus features, Heister et al. (1990) proposed the hierarchical coding hypothesis of the horizontal spatial response. According to the hierarchical coding hypothesis, the spatial code of the response location (left button vs. right button) is ranked higher than, and is used in priority to, the anatomical code of the effector identity (left hand vs. right hand) to represent the response as left or right. The lower-ranked internal coding of anatomical identity is influential only when a higher-ranked external positional coding could not be used. Evidence for hierarchical action coding is reported in a wide range of interactions between spatial/spatially-associated stimulus features and manual responses (Klapp et al., 1979; Müller and Schwarz, 2007; Nishimura and Yokosawa, 2010b).

However, there might not be a hierarchical relationship between spatial and anatomical coding themselves, if stimulus as well as action properties are considered. Hierarchical coding might not be applicable when a stimulus also has anatomical features. Automatic imitation – a tendency to perform the same movement using the body part corresponding to the perceived body movement – emerges simultaneously with the spatial compatibility effect (Bertenthal et al., 2006; Catmur and Heyes, 2011). This suggests that both the spatial and the effector-based effects could be simultaneously observed in an appropriate situation. Action coding along multiple dimensions (Hedge and Marsh, 1975; Nicoletti and Umiltà, 1984; Rubichi et al., 2006), based on multiple action effects (Hommel, 1993, 1996; Grosjean and Mordkoff, 2002), and based on both vertical spatial and horizontal anatomical features (Nishimura and Yokosawa, 2010b) also supports the potential availability of multiple levels for action coding.

The present study investigated the influence of perceiving a pointing hand stimulus on one's own manual response action in terms of spatial compatibility based on symbolic/spatial features and effector priming based on anatomical features. We used a task similar to the Simon task (Simon, 1990; Lu and Proctor, 1995) in which the compatibility-related and effector-related stimulus features were task-irrelevant, in order to test automatic influences (see also Ottoboni et al., 2005). Participants were required to make left or right button press responses based on the color of a centrally presented target patch while ignoring a task-irrelevant hand picture. The hand pictures displayed a left or right hand with the index or little finger extended. The direction of the extended finger was either left or right. Participants pressed the left and right button with their left and right hands, respectively. Spatial compatibility was based on the relationship between the pointing direction and the response location (left vs. right): compatible when the pointing direction and the response location corresponded and incompatible when they were opposite. On the basis of Ariga and Watanabe's (2009) findings regarding attentional shift, we predicted that both the index- and little-finger pointing stimuli should elicit the spatial compatibility effect, and that the compatibility effect should be larger for the pointing index finger than for the pointing little finger.

Effector priming was based on the relationship between the anatomical identity (left hand vs. right hand) of the pointing hand and the response hand. If symbolic/spatial feature coding based on the environmental reference frame is ranked higher than, and is used in priority to, anatomical feature coding in the cognitive processing hierarchy (Heister et al., 1990), then pointing direction alone should affect performance: the spatial compatibility effect should be present but the effector priming effect should be absent. In contrast, if the symbolic/spatial and anatomical properties could simultaneously influence cognitive processing, then both pointing direction and hand identity should affect performance: both the spatial compatibility effect and the effector priming effect should be observed. To further investigate whether effector priming is modulated by postural congruency between the presented body part and the body part used for responding, we asked participants to press the response buttons by using their extended index fingers for one block and their extended little fingers for another block, while the other fingers were folded. The participants' hand posture was congruent with the observed hand posture if the index (little) finger pointing was displayed while the participants used their index (little) fingers for responding. The participants' hand posture was incongruent with the observed hand posture if the index (little) finger pointing was displayed while the participants used their little (index) fingers for responding.

EXPERIMENT 1

In Experiment 1, we examined the automatic influence of briefly presented task-irrelevant hand stimuli depicting leftward or rightward pointing with the little or index finger on a manual horizontal button-pressing task, from the perspective of spatial compatibility between the pointing direction and the location of the response button and effector priming between the pointing hand and the identity of the response hand. The pointing hand picture was briefly presented 160 ms prior to the presentation of the target. We used a short SOA because attentional cueing effects were observed with an SOA of 107 ms but not with an SOA of 1,000 ms in the study by Ariga and Watanabe (2009).

METHOD

Participants

Twenty-four volunteers (20 females; mean age = 24.1 years; all right-handed) participated in this experiment. All participants reported normal or corrected-to-normal vision.

Apparatus and stimuli

Experiments were controlled by MATLAB 7.2 (MathWorks). Visual stimuli were presented on a 24″ LCD display (Diamondcrysta RDT241WEX, Mitsubishi). The left and right shift keys were used as response keys. Participants pressed the left and right response keys with their left and right index fingers, respectively, in one block and with their left and right little fingers in another block. The experiment was conducted in a darkened room.

Visual stimuli were presented at the center of the display on a gray background. The fixation point consisted of a white dot (3 mm in diameter). Target stimuli were a green or red dot (11 mm in diameter). The pointing hand stimuli (92–101 mm width × 39–51 mm height for the index finger pointing; 75–76 mm width × 40–50 mm height for the little finger pointing) were grayscale palm or back hand images with the index or little finger extended while the other fingers were clenched (**Figure 1**). Hand stimuli were obtained from three females and three males. Eight types of finger pointing stimuli, a combination of view (back or palm), pointing finger (index or little finger), and pointing direction (left or right; mirror-reversed images were used), were used for each model. Thus, 48 images in total were used as pointing hand stimuli. A chin rest maintained a viewing distance of 60 cm.

Task and procedure

Participants were instructed to press the left or right response key based on the color of the target as quickly and accurately as possible. Half of the participants were required to press the left key for green targets and the right key for red targets. The other half was required to press the left key for red targets and the right key for green targets.

Each trial started with the presentation of the white fixation point. The duration of the fixation point ranged from 1,000 to 2,000 ms. Then, a pointing hand was presented for 60 ms. After an inter-stimulus interval (ISI) of 100 ms, the central target dot was presented until the response was made. The inter-trial interval (ITI) preceding the next trial was 1,000 ms. During the ISI and the ITI, the gray background was presented on the screen.

The experiment had two experimental blocks. Each block contained 288 trials of 3 replications for each combination of pointing hand view (2; back, palm), pointing finger (2; index finger, little finger), pointing direction (2; left, right), model of the pointing hand (6), and target stimulus color (2; green, red). Trial order was randomized. Participants were given a short break every 48 trials, after which they restarted the experiment with a left or right shift key press. They used their left and right index fingers to press the left and right response keys in one block and their little fingers in another block. Response finger order was counter-balanced across participants. A practice block of 16 trials preceded each experimental block.

RESULTS

Trials in which reaction times (RTs) were less than 100 ms or more than 1,000 ms (<1% of all trials) were excluded as outliers from the RT analyses. The overall error rate was low (2.5%) and therefore immaterial to our discussion. The error rate pattern was generally consistent with RT results (**Table 1** and **Figure 2**). Mean RTs for correct responses were submitted to an analysis of variance (ANOVA) with pointing finger of the hand stimulus (index finger, little finger), spatial compatibility between the left/right pointing direction and the left/right response key position (compatible,

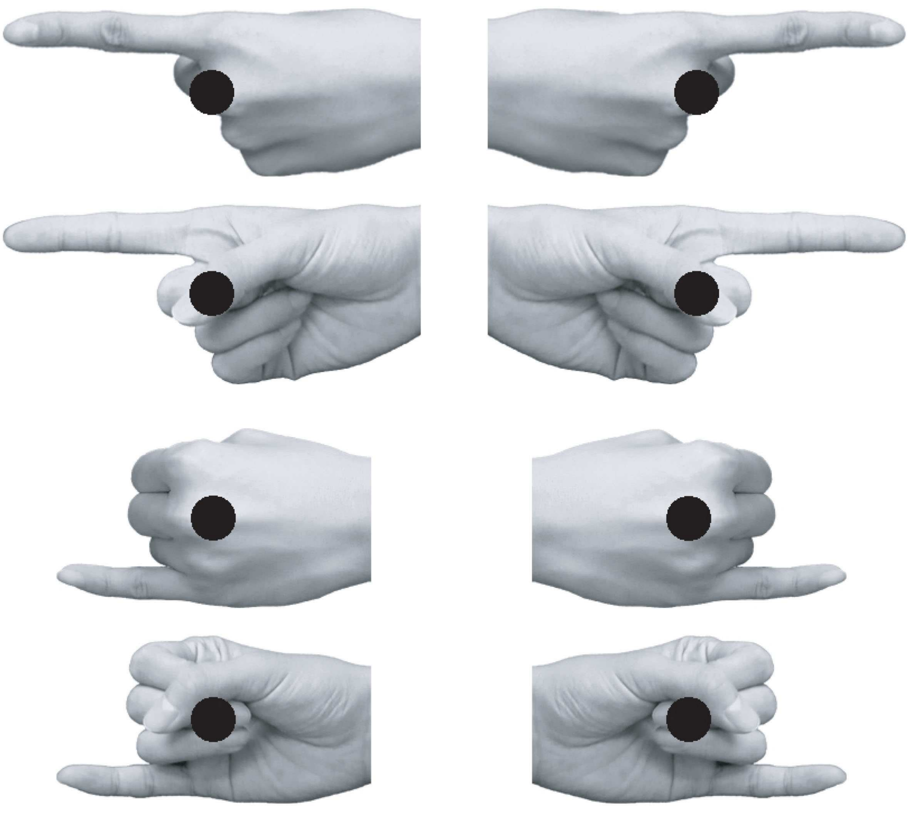

FIGURE 1 | Examples of pointing hand stimuli. The black dot superimposed on each hand represents the position of the target relative to the hand. Note that the target was superimposed on the hand stimulus only in Experiment 2. In Experiment 1, the hand stimulus disappeared before the onset of the target dot.

Table 1 | Mean reaction time (ms) and error rate (%; in parentheses) for Experiments 1 and 2 as a function of pointing finger, spatial compatibility, effector compatibility, and postural congruency.

Pointing finger		**Index**		**Little**	
Spatial compatibility		**Compatible**	**Incompatible**	**Compatible**	**Incompatible**
Experiment 1					
Postural congruency	Effector compatibility				
Congruent	Compatible	412 (1.0)	428 (3.6)	423 (2.2)	436 (2.7)
	Incompatible	409 (2.0)	430 (2.8)	427 (2.2)	432 (4.5)
Incongruent	Compatible	421 (1.4)	443 (3.4)	412 (1.5)	426 (3.0)
	Incompatible	420 (1.3)	440 (3.6)	412 (1.6)	420 (3.7)
Experiment 2					
Congruent	Compatible	402 (1.7)	410 (2.5)	398 (1.7)	396 (0.9)
	Incompatible	406 (1.7)	418 (1.6)	401 (1.6)	404 (2.5)
Incongruent	Compatible	393 (1.2)	408 (2.4)	404 (1.4)	403 (1.9)
	Incompatible	401 (0.9)	409 (4.7)	407 (1.5)	412 (3.0)

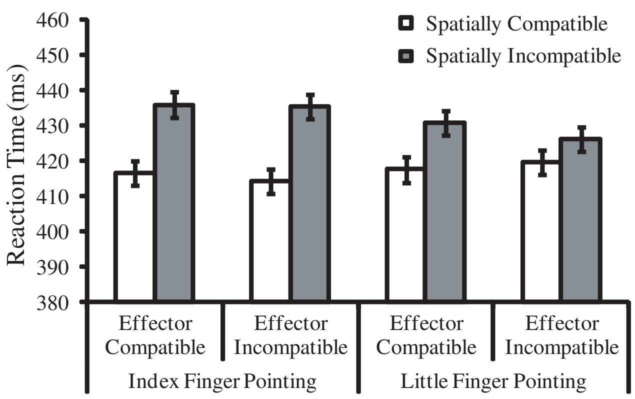

FIGURE 2 | Mean reaction times (in milliseconds) for Experiment 1 as a function of pointing finger, effector compatibility, and spatial compatibility. Error bars represent within-subjects standard errors pooled from the three factors.

incompatible), effector compatibility between the left/right identity of the pointing hand stimulus and the left/right identity of the response hand (compatible, incompatible), and postural (i.e., extended finger) congruency between the pointing hand stimulus and the responding hand (congruent, incongruent) as within-subjects factors.

The main effect of spatial compatibility was significant, $F(1, 23) = 25.89$, $p < 0.001$, $\eta_p^2 = 0.53$, indicating a 15 ms spatial compatibility effect. Responses were faster when the pointing direction and the response key position were congruent ($M = 417$ ms) than when they were incongruent ($M = 432$ ms). The two-way interaction between spatial compatibility and pointing finger of the hand stimulus was significant, $F(1, 23) = 9.62$, $p = 0.005$, $\eta_p^2 = 0.29$. The spatial compatibility effect was larger for the pointing index finger (20 ms, $p < 0.001$) than for the little finger (10 ms, $p = 0.002$). The two-way interaction between pointing finger and postural congruency was significant, $F(1, 23) = 7.12$, $p = 0.014$, $\eta_p^2 = 0.24$, reflecting faster index finger key presses ($M = 419$ ms) than little finger key presses ($M = 430$ ms). Other main effects or interactions, including the main effect of effector compatibility (−1 ms effector priming effect), were not significant ($ps > 0.05$).

DISCUSSION

We found evidence of the spatial compatibility effect. Performance was better when the pointing direction of the task-irrelevant pointing hand stimulus was consistent with the response location than when the pointing direction was opposite to the response location. The spatial compatibility effect was larger for the pointing index finger than for the little finger. These findings are consistent with the previous evidence demonstrating superiority of the pointing index finger as an attention-directing pointing gesture (Ariga and Watanabe, 2009).

In contrast to the spatial compatibility effect, the effector priming effect was not observed in Experiment 1. This might indicate that when the spatial compatibility effect due to symbolic/spatial feature overlap emerges, no effector-based effect due to anatomical feature overlap emerges. However, recent studies have shown that time is needed for the effects related to body parts to develop (Catmur and Heyes, 2011; Vainio and Mustonen, 2011). Therefore, the brief presentation of the pointing hand stimulus might be responsible for the absence of the effector priming effect in Experiment 1. To test this issue, we extended the duration of the pointing hand stimulus in Experiment 2.

EXPERIMENT 2

In Experiment 2, to maximize the possibility of observing the effector priming effect (see Vainio and Mustonen, 2011), the SOA between the pointing hand stimulus and the target dot was extended to 560 ms (from 160 ms in Experiment 1). Additionally, duration of the pointing hand stimulus was extended until a response was made. Therefore, the pointing hand stimulus remained present even after onset of the target dot. If symbolic/spatial feature coding based on the environmental reference frame is ranked higher than, and used in priority to, anatomical feature coding in the cognitive processing hierarchy (Heister et al., 1990), only the spatial compatibility effect should be observed.

However, if the symbolic/spatial and anatomical properties can simultaneously influence cognitive processing, but effector priming needs some time to occur (Vainio and Mustonen, 2011), then both the spatial compatibility effect and the effector priming effect should be observed in this experiment.

METHOD

Participants

Twenty-four undergraduate and graduate students (14 females; mean age = 22.4 years; 22 right-handed) participated in this experiment. All participants reported normal or corrected-to-normal vision. None of them had participated in Experiment 1.

Apparatus, stimuli, task, and procedure

Stimuli and procedures were the same as Experiment 1 except for the following: the pointing hand stimuli were presented until a response to the red or green dot was made. The target stimulus was superimposed upon the pointing hand stimulus (see **Figure 1**). The SOA between the pointing hand stimulus and the target was 560 ms.

RESULTS

Outliers (<1% of all trials; defined using the same criteria as in Experiment 1) were excluded from the analyses. As in Experiment 1, the overall error rate was low (2.0%) and therefore immaterial to our discussion. The error rate pattern was generally consistent with RT results (**Table 1** and **Figure 3**). Mean RTs for correct responses were submitted to an ANOVA with the pointing finger (index finger, little finger), spatial compatibility (compatible, incompatible), effector compatibility (compatible, incompatible), and postural congruency (congruent, incongruent) as within-subjects factors.

The main effect of spatial compatibility was significant, $F(1, 23) = 12.61$, $p = 0.002$, $\eta_p^2 = 0.35$, indicating a 6 ms spatial compatibility effect. Responses were faster when the pointing direction and the response key position were congruent ($M = 401$ ms) than when they were incongruent ($M = 407$ ms). The main effect of effector compatibility was also significant, $F(1, 23) = 19.95$, $p < 0.001$, $\eta_p^2 = 0.46$, indicating a 5 ms effector priming effect. Responses were faster when the pointing hand and the response hand had the same left/right identity ($M = 402$ ms) than when the identity was reversed ($M = 407$ ms). The two-way interaction between spatial compatibility and pointing finger was significant, $F(1, 23) = 14.79$, $p < 0.001$, $\eta_p^2 = 0.39$. The spatial compatibility effect was larger for the pointing index finger (11 ms, $p < 0.001$) than for the little finger (1 ms, n.s.). Other main effects or interactions were not significant ($ps > 0.05$).

DISCUSSION

With longer presentation of the task-irrelevant pointing hand stimuli than in Experiment 1, both the spatial compatibility and the effector priming effects emerged in Experiment 2. As in Experiment 1, the spatial compatibility effect between the pointing direction of the hand and the response location was observed. However, the spatial compatibility effect was significant only for the pointing index finger. In contrast to Experiment 1, the effector priming effect was observed in Experiment 2. Performance was better when the task-irrelevant pointing hand was anatomically identical to the hand used for the button press response (e.g., left hand pointing and a key press with the left hand) than when they were different (e.g., left hand pointing and a key press with the right hand). The effector priming effect was not modulated by postural congruency between the pointing hand and the responding hand.

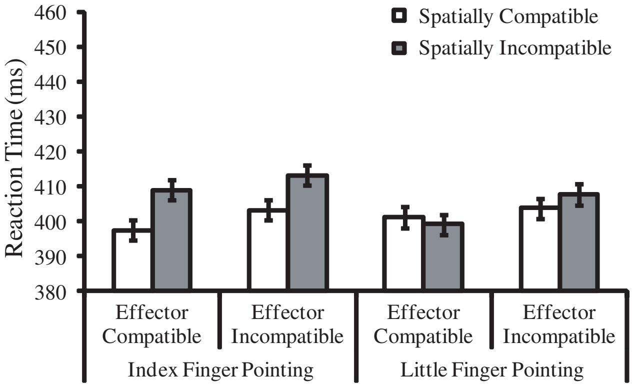

FIGURE 3 | Mean reaction times (in milliseconds) for Experiment 2 as a function of pointing finger, effector compatibility, and spatial compatibility. Error bars represent within-subjects standard errors pooled from the three factors.

Experiment 2 revealed that with sufficient duration, both pointing direction and anatomical identity of a task-irrelevant pointing hand stimulus could simultaneously affect an observer's action. This finding is consistent with the notion that the absence of effector priming effect in Experiment 1 was due to insufficient time to develop rather than hierarchical coding of symbolic/spatial and anatomical features.

GENERAL DISCUSSION

The present study investigated the automatic influence of symbolic/spatial and anatomical features of task-irrelevant pointing hand stimuli on manual key press responses in terms of spatial compatibility between pointing direction and response location and of effector compatibility between the pointing hand and the response hand. Although only the spatial compatibility effect was observed in Experiment 1 (brief presentation of the pointing hand stimuli prior to target onset), both the spatial compatibility and the effector priming effects emerged in Experiment 2 (longer SOA and duration of the pointing hand stimuli). Thus, we revealed that with sufficient processing time, pointing hand pictures could automatically affect observer's actions based on both symbolic/spatial and anatomical features. An imitative compatibility effect between viewing and doing *dynamic* manual actions emerges simultaneously with the spatial compatibility effect (Bertenthal et al., 2006; Catmur and Heyes, 2011). The present study showed the simultaneous occurrence of spatial compatibility effect and effector priming effect based on left/right anatomical identity even when stimuli (pointing hand pictures) and responses (manual button presses) were both *static*.

Hierarchical horizontal action coding, in which spatial coding is superior to anatomical coding, has been repeatedly confirmed in interactions between spatial/spatially-associated stimulus features and horizontal manual actions (Klapp et al., 1979; Heister et al., 1990; Müller and Schwarz, 2007; Nishimura and Yokosawa, 2010b). However, the simultaneous occurrence of the spatial compatibility and effector priming effects suggests that hierarchical coding is not applicable, and that action coding in terms of both spatial (location) and anatomical (effector identity) features can be simultaneously influential, when the stimulus also has anatomical feature. We conclude that there is no hierarchical relationship between spatial coding and anatomical coding themselves. Both the spatial and effector-based effects could simultaneously emerge in appropriate situations (see also Bertenthal et al., 2006; Nishimura and Yokosawa, 2010b; Catmur and Heyes, 2011). The present findings suggest the importance of considering stimulus properties in addition to action properties to understand the action coding in interaction between perception and action.

No interactions involving both spatial compatibility and effector compatibility were significant in the analyses. The spatial compatibility effect was larger in Experiment 1 than in Experiment 2, whereas the effector priming effect was observed only in Experiment 2. Furthermore, the size of the spatial compatibility effect did not correlate with the size of the effector priming effect ($r = 0.00$ for Experiment 1 and $r = 0.02$ for Experiment 2). Although inconclusive, these results suggest that the spatial compatibility and effector priming effects emerge independently.

Although the spatial compatibility and effector priming effects may emerge with independent processing, these effects are commonly explained in terms of ideomotor theory. According to this theory (James, 1890; Prinz, 1997; Hommel et al., 2001a), actions are represented and controlled by their perceptual consequences (action effects). This notion implies that perception/cognition and action control use common representation. Therefore, the stimulus features should activate the actions with corresponding features through the action effect codes. Manual button presses accompany perceptual events within the visual, auditory, proprioceptive, and tactile senses at the response location with the effector used for that response (see Hoffmann et al., 2009). In the present study, spatial information conveyed by the pointing direction of the hand stimulus should activate the corresponding spatial code of left or right, also associated with action on that side. As a result, a response was facilitated when the pointing direction was on the side of the correct response, but response conflict emerged when the pointing direction was opposite the correct response because the pointing direction activated the code representing the incorrect response. Similarly, a stimulus depicting left (or right) hand should activate representation of that hand, which is also used to control action with the hand. This activation facilitated the actions using the left (or right) hand, inducing the effector priming effect.

SPATIAL COMPATIBILITY EFFECT

The spatial compatibility effect was observed in the present study. Performance was better when the direction of the pointing hand stimulus and the response location were on the same side than when they were on opposite sides. In Experiment 1, with a brief presentation of the pointing hand stimulus and a short SOA of 160 ms, the spatial compatibility effects for both the index and the little finger pointing hand stimuli were significant, but the former was larger than the latter. Ariga and Watanabe (2009) revealed larger reflexive shifts of attention for the pointing index finger than for the pointing little finger following a brief presentation of a pointing hand stimulus with SOA of 107 ms. Thus our spatial compatibility findings add further evidence that stimuli eliciting reflexive shifts of attention also elicit the spatial compatibility effect (see the Introduction for other examples), and are consistent with the notion of a close relationship between attention and action (e.g., Rizzolatti et al., 1987; Rubichi et al., 1997; Deubel et al., 1998; Humphreys and Riddoch, 2005).

In Experiment 1, the spatial compatibility effect was larger for the pointing index finger than for the pointing little finger. In Experiment 2, the spatial compatibility effect was significant for the pointing index finger but not for the pointing little finger. The superiority of the index finger in the spatial compatibility effect was constant across both experiments (10 ms). One possible cause of the larger spatial compatibility effect in index finger pointing is intentionality. Previous studies suggest that in the interaction between individuals, the effect of the perceived action of another on one's own task performance should be larger when the action is recognized as intentional (Tsai and Brass, 2007; Liepelt et al., 2008; Liepelt and Brass, 2010; Atmaca et al., 2011). The index finger pointing hands may be special stimuli from which the spatial intention/meaning of the actor is automatically extracted, probably because an index finger pointing gesture is widely used to indicate spatial position or direction, whereas a hand with a little finger extended is rarely used for such purposes.

Another possible cause of the difference in spatial compatibility effects between the index and little finger pointing is morphological differences between the index and little finger pointing stimuli such as finger length, extended position, or size. Our study alone cannot distinguish these two possibilities, but Ariga and Watanabe (2009) shed some light on this issue. Although their paradigm differed from the present one, they obtained cueing effects of similar magnitude for hand stimuli with the little finger extended, with the index and middle fingers extended, with the index finger shortened to the length of the little finger extended, or with the little finger lengthened to the length of the index finger extended. These effects were smaller than that of the normal index finger pointing stimuli. This finding suggests that the larger spatial effect related to the index finger pointing hand stimuli is specific to normal index finger pointing and cannot be explained by position or length of the extended finger or by the overall size of the picture. Taken together, we tentatively conclude that the spatial compatibility effect specific to the pointing index finger should be based on the spatial intention/meaning automatically extracted from a picture of a body part that implies an action. However, a further experiment would be required to rule out the possibility that similar effects would be seen with non-social stimuli of similar shape and size.

EFFECTOR PRIMING

An effector priming effect between the left/right anatomical identity of the presented task-irrelevant pointing hand and that of the hand used for the button press was absent with a 160-ms SOA and brief presentation of pointing hand stimuli (Experiment 1). However, effector priming effect was present with a 560-ms SOA and longer presentation of hand stimuli until response (Experiment

2). Thus, the present study showed that the effector priming effect required additional time to develop. This finding suggests that it takes a certain amount of time for the anatomical left/right feature of the perceived hand to be identified, and/or that it takes time for that feature to affect manual action using the left/right hand. In addition, absence of the influence of postural congruency indicates that feature codes that represent effector identity and control manual movements are posture-free, at least in the range of those used in the present study.

Vainio and Mustonen (2011) reported the effector priming effect with manual button press responses according to a target superimposed on centrally presented task-irrelevant hand stimulus. Their results were similar to those obtained in the present study. In their study, the effector priming effect was present with SOAs of 400 ms and 700 ms, but was not reliable with 100 ms SOA. Most hand postures of the stimuli used in their studies elicited similar effector priming effects. However, the direction of the hand modulated effector priming: positive effector priming effect emerged for the upward hand (wrist on the bottom), whereas negative effector priming effect was found for the downward hand (wrist on the top). We obtained a positive effector priming effect with in-between hand direction (i.e., leftward/rightward hand). Identification of boundary condition(s) of positive/negative effector priming in future research will support further understanding of the automatic influence of perceiving body parts on viewer's actions in various ways and with various functions, such as the integration of perceived information and motor processes for action control, action mirroring, and communication (e.g., Liepelt et al., 2010; Vainio and Mustonen, 2011).

The present study obtained evidence for effector priming based on anatomical identity with static stimuli and responses. Properties of the effector priming effect, such as the requirement of adequate time to emerge (see also Vainio and Mustonen, 2011) and simultaneous occurrence with spatial compatibility effects, were consistent with those of imitative compatibility effects based on the correspondence of body parts and their movements (Bertenthal et al., 2006; Catmur and Heyes, 2011). Further research is needed to determine the elements specific to movement in the interaction between the perception of body parts and use of corresponding body parts.

CONCLUSION

The present study investigated the influence on one's own task performance of perceiving body parts that imply another's action using a presentation of task-irrelevant pointing hand stimuli and manual button press responses. A spatial compatibility effect between the pointing direction and response location and an effector priming effect between the left/right anatomical identity of the pointing hand and response hand simultaneously emerged. For example, when a right hand with an index finger pointed to the left was presented, response on the left side and response using the right hand were activated. Our findings on spatial compatibility effects and on effector priming were consistent with the literature on each topic. We conclude that even when spatial directional information is delivered by a stimulus implying another's action, anatomical features of the action are also automatically extracted, and that both the spatial/symbolic and the anatomical features simultaneously influence different aspects of one's own action. The hierarchical coding account (Heister et al., 1990), according to which anatomical features are used only if spatial features are unavailable, was not supported when stimuli as well as responses involved corresponding anatomical features.

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The end-state comfort effect in 3- to 8-year-old children in two object manipulation tasks

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The aim of the study was to compare 3- to 8-year-old children's propensity to anticipate a comfortable hand posture at the end of a grasping movement (*end-state comfort effect*) between two different object manipulation tasks, the bar-transport task, and the overturned-glass task. In the bar-transport task, participants were asked to insert a vertically positioned bar into a small opening of a box. In the overturned-glass task, participants were asked to put an overturned-glass right-side-up on a coaster. Half of the participants experienced action effects (lights) as a consequence of their movements (AE groups), while the other half of the participants did not (No-AE groups). While there was no difference between the AE and No-AE groups, end-state comfort performance differed across age as well as between tasks. Results revealed a significant increase in end-state comfort performance in the bar-transport task from 13% in the 3-year-olds to 94% in the 8-year-olds. Interestingly, the number of children grasping the bar according to end-state comfort doubled from 3 to 4 years and from 4 to 5 years of age. In the overturned-glass task an increase in end-state comfort performance from already 63% in the 3-year-olds to 100% in the 8-year-olds was significant as well. When comparing end-state comfort performance across tasks, results showed that 3- and 4-year-old children were better at manipulating the glass as compared to manipulating the bar, most probably, because children are more familiar with manipulating glasses. Together, these results suggest that preschool years are an important period for the development of motor planning in which the familiarity with the object involved in the task plays a significant role in children's ability to plan their movements according to end-state comfort.

Keywords: end-state comfort effect, anticipatory planning, child development, motor development, action effects to investigate end-state comfort performance

INTRODUCTION

Adults typically grasp objects by anticipating what they are intending to do with that object. For example, when intending to get a drink, a glass that is placed upside-down on a table is first grasped thumb-down, rotated by 180°, and then placed thumb-up. That is, adults start the movement with an uncomfortable thumb-down grip, in order to end the movement in a comfortable thumb-up grip. This so-called end-state comfort effect has generally been taken as evidence for the influence of optimization constraints in motor planning in a variety of object manipulation tasks (e.g., Rosenbaum et al., 1990; Rosenbaum and Jorgensen, 1992; Fischman, 1997, 1998; for a recent overview see Rosenbaum et al., 2012). In contrast to adults' success in manipulating different objects according to end-state comfort, findings were inconsistent with regard to children's performance as a function of age and type of task. Therefore, the general aim of the present study was to investigate end-state comfort performance in children across different ages in two different object manipulation tasks.

Most previous studies applied only a single task, specifically the bar-transport task, in order to investigate the end-state comfort effect in children. For example, in a study conducted by Manoel and Moreira (2005) 3- to 6-year-old children had to insert either the right or the left end of a horizontally resting bar into a box with either a cylindrical hole (low-precision condition) or with a semi-cylindrical hole (high precision condition). In both conditions, a right-end insertion required a comfortable overhand grip (uncritical trials), whereas a left end insertion required an uncomfortable underhand grip (critical trials) for right-handed children. Results revealed little evidence of end-state comfort, independent of age, and condition, with even the oldest children tending to grasp the bar with an overhand grip in the majority of cases, even if this meant to finish the maneuver in an uncomfortable posture. However, using a similar bar-transport task with 3- to 5-year-old children, Weigelt and Schack (2010) found an increase in end-state comfort performance from 18% in the 3-year-olds to 70% in the older children (see also Stöckel et al., 2011, for evidence of an increase of end-state comfort performance from 50 to 92% in 7- to 9-year-old children). Thibaut and Touissant (2010) also used the bar-transport task in 4-, 6-, 8-, and 10-year-old children. Whereas end-state comfort performance rose from 40% in the 4-year-olds to 70% in the 6-year-olds, performance dropped in the 8-year-olds to 50% and rose again to 80% in the 10-year-olds.

Interestingly, when the bar was replaced by a two-colored pencil, and children were asked to pick up the pencil to trace an alley on a sheet of paper (high precision requirements), the 8-year-olds now performed better than the 6-year-olds. This suggests that precision requirements together with the familiarity of the object involved in the task might have helped these children to plan their movements more efficiently. Adalbjornsson et al. (2008) also investigated end-state comfort performance in a task that involved familiarity with the object used. They employed the overturned-glass task by asking two groups of preschool children (2–3 years and 5–6 years) to pick up an upside-down glass and to pour water into it, both with their preferred hand. However, only 20% of the 2- to 3-year-olds and 35% of the 5- to 6-year-olds grasped the glass according to end-state comfort.

Although, these studies generally suggest an increase in end-state comfort performance with age, they also show that the propensity to use end-state comfort in object manipulation tasks differs in children of comparable age within and across tasks. For example, whereas Manoel and Moreira (2005) found only little evidence of the end-state comfort effect in 6-year-old children in the bar-transport task, children of the same age showed end-state comfort 70% of the time in a bar-transport task as reported by Thibaut and Touissant (2010). Likewise, whereas Weigelt and Schack (2010) found 70% end-state comfort performance in 5-year-old children in the bar-transport task, Adalbjornsson et al. (2008) found only 35% end-state comfort performance in 5- to 6-year-old children in the overturned-glass task. These conflicting results might be due to differences within the tasks, such as precision requirements and task demands, as well as differences across tasks, such as familiarity with the object to be manipulated. Therefore, it would be interesting to compare children of the same age for two different tasks within one single study. To the best of our knowledge, this was only done once before by Smyth and Mason (1997), who examined the performance of children from 4 to 8 years of age in the bar-transport task and the handle-rotation task. The latter required children to rotate a handle on a disk in order to cover pictures printed at different degrees on the disk (see also Crajé et al., 2010; van Swieten et al., 2010; for different versions of the handle-rotation task with children). According to the results, however, the end-state comfort effect was not present even in the oldest children, no matter which task was used. Hence, it is still an open question whether the presence of the end-state comfort effect differs between two tasks for children of the same age. If one would find different developmental patterns of the end-state comfort effect between tasks, then this would be evidence for the strong role of task constraints on the emergence of anticipatory planning skills in young children.

Another factor that might lead to different results in end-state comfort performance in young children might be the particular set-up used. Young children might find it easier to plan their actions according to end-state comfort, if their movements lead to interesting effects in the environment, such as a light that turns on as a consequence of their movement. This was the case in a recent study by Jovanovic and Schwarzer (2011), who used a modified version of the bar-transport task with 18-, 24-, and 42-month-old children. Instead of presenting the bar horizontally, the bar used by Jovanovic and Schwarzer had a small platform on one end, which held the bar in a vertical position. By way of demonstration, the bar stood with its platform on its top (requiring a comfortable thumb-up grip) and children were shown that when the bar was inserted into the cylinder, lights lit up that were built into the cylinder. The experimenter modeled the thumb-up grip twice for the child (*baseline condition*) and subsequently, the bar was returned to its starting position and the child was encouraged to perform the same action as the experimenter. Then, the bar was returned to its starting position, but this time standing on its platform (*reverse condition*) and children were asked to switch on the lights (now requiring an uncomfortable thumb-down grip). Only 8% of the 18-month-olds and none of the 24-month-olds grasped the bar with an uncomfortable thumb-down grip. In contrast, a comparatively high percentage of 60% of the 42-month-olds showed the end-state comfort effect. From the latter observation, the question arises whether the high percentage of children showing the end-state comfort effect at this age is a result of the action effects presented at the end of the object manipulation.

There is ample evidence, that action effects, such as lights (Paulus et al., 2011), sounds (Hauf et al., 2004; Paulus et al., 2012), or both (Elsner and Aschersleben, 2003; Hauf and Aschersleben, 2008) play an important role in how infants control their actions. According to the common coding theory, which is based on the ideomotor theory proposed by James (1890), actions are planned and selected by anticipating the corresponding action effects (Prinz, 1997; Hommel et al., 2001). Through repeated co-occurrences of particular actions and their effects, action–effect associations are established. Planning an action is therefore assumed to activate the representation of the desired action effect (e.g., making a light occur), which then results in a priming of the corresponding movement (e.g., pressing a button; Kunde, 2001; Pfister et al., 2010). Action-effect associations can either be learned by ways of instrumental learning (e.g., DeCasper and Fifer, 1980; Elsner and Hommel, 2004) or by observation (e.g., Elsner and Aschersleben, 2003; Paulus et al., 2011). In the context of observational learning of action-effect associations it is not only necessary to represent the particular action-effect, but also to relate the other's action to one's own motor repertoire. Elsner and Aschersleben (2003) have shown that 15-month-old infants indeed already expect their own actions to produce the same effects as the observed action. Similarly, if 14-month-old infants see a model touch a lamp with her forehead, they imitate this action significantly more often if it was followed by a light effect than when it was not (Paulus et al., 2011).

However, although in the study reported by Jovanovic and Schwarzer (2011) action effects were involved, the experimental situation was somewhat different to the typical set-up used in the imitation studies testing the role of action effects reported above. First of all, the lights always lit up when the bar was inserted into the cylinder. That is, the same action effect always followed the action, irrespective of the grip selected. Moreover, in the *reverse condition* tested by Jovanovic and Schwarzer (2011), a thumb-down grip was never demonstrated to the children. Instead, children where only shown the starting state (bar resting on its platform) and the end-state (bar in the lit cylinder; shown in the preceding baseline condition) and children had to infer the movement in order to switch on the lights. That is, rather than being able to rely on established action-effect associations, children in this study had to emulate the action necessary to reach the goal. In contrast to goal imitation, goal emulation has been described as being a case where an observer attempts to reproduce a completed goal (e.g., bar in a lit cylinder) by whatever means seem suitable, without having observed the exact action used by the actor to reach the goal (Tomasello, 1999). Studies on goal emulation in infancy suggest, that the ability to make inferences from the observed goal to the required movement emerges by the end of the second year (e.g., Bauer et al., 1999; Huang et al., 2002; see Elsner, 2007, for a review of the role of movements and their effects in infants' emulation of goal-directed actions). Even though the common coding theory does not make predictions about the influence of action effects on goal emulation, it is still possible that the light effects used in the study by Jovanovic and Schwarzer (2011) did help the 42-month-old children to plan their movements more efficiently by indirectly enhancing children's motor planning by, for example, affecting attentional or motivational processes. That is, the light effects might have rendered the goal more salient and therefore the light effects might have motivated children to accomplish the task more accurately.

THE PRESENT STUDY

The first aim of the present study was to compare the presence of the end-state comfort effect in children of different ages between two different object manipulation tasks. The second aim was to investigate, whether the light effects in the study by Jovanovic and Schwarzer (2011) was the determining factor with regard to the comparatively high percentage of end-state comfort shown by the 42-month-old children. To this end, we investigated end-state comfort performance in six age groups of children from 3 to 8 years, as well as a control group of adults, in the bar-transport task following Jovanovic and Schwarzer (2011) and in a version of the overturned-glass task adapted from Adalbjornsson et al. (2008). In both tasks, half of the participants in each age group experienced action effects as a consequence of their movements (AE groups) while the other half of the participants did not (No-AE groups). In neither of the two tasks was a thumb-up or a thumb-down grip demonstrated to the participants. Based on the literature discussed above, we expected to find a general increase in end-state comfort performance across age in both tasks. In addition, if the presence of an interesting action effect was the determining factor with regard to the comparably high percentage of end-state comfort shown in children aged 3–4 years in the study by Jovanovic and Schwarzer (2011), especially children at the younger ages, during which end-state comfort is still developing, should benefit from the presence of an action effect.

MATERIALS AND METHODS

PARTICIPANTS

Six age groups of 16 participants each and an adult control group ($n = 20$) took part in the study (3-year-olds: nine female, *M* age = 41.6 months, SD = 2.87, 15 right-handed; 4-year-olds: five female, *M* age = 55.6, SD = 2.55; 15 right-handed; 5-year-olds: eight female, *M* age = 66.2, SD = 3.31, all right-handed; 6-year-olds: eight female, *M* age = 75.5, SD = 2.89, 13 right-handed; 7-year-olds: nine female, *M* age = 89.2, SD = 3.21, all right-handed; 8-year-olds: seven female, *M* age = 102.44, SD = 3.21, 13 right-handed; and adults: 11 female, *M* age = 25.6 years, SD = 5.2, 18 right-handed). Participants were recruited and tested in different kindergartens in the Saarbrücken area of Germany and in the Developmental Psychology Unit, Saarland University, Germany. The adult control group was not included in the analyses, because all of the participants showed the end-state comfort effect in both tasks (see Thibaut and Touissant, 2010, for similar results). Ten additional children were tested, but their data were excluded from further analyses, because they did not understand German (one 3-year-old) or did not understand the task (one 3-year-old), due to an experimenter error (three 3-year-olds, two 4-year-olds, one 5-year-old), or because the child was unwilling to finish the task (two 3-year-olds). In each age group, there were eight children in the AE group and eight children in the No-AE group, except for the 7-year-old group with nine children in the AE group and seven children in the No-AE group.

APPARATUS

In both tasks, the bar-transport task and the overturned-glass task, materials were placed on a white wooden board (40 cm × 66 cm) on a table. A starting line was marked on the floor at approximately 70 cm in front of the table. The material of the bar-transport task consisted of a white box (13 cm × 13.5 cm × 11.5 cm) with an insertion hole (diameter: 3- to 6-year-olds: 2 cm, 7- and 8-year-olds: 2.5 cm, adults: 3 cm) on its top and a smiley configuration of LEDs inserted in its front side facing the child. Twenty-three centimeters to the left and to the right of the box a bar holder was placed, which held the bar in an upright position. Pilot data had revealed that mainly the young children were uncomfortable with handling a rather thick bar. Therefore, the bar used (and the corresponding hole in the box) was of different size for the kindergarten children, the school children, and the adults, in order to adjust for different hand sizes and to ensure precision requirements (3- to 6-year-olds: diameter bar = 1.6 cm, platform: 4 cm × 4 cm; 7- and 8-year-olds: diameter bar = 2 cm, platform: 4.5 cm × 4.5 cm; adults: diameter bar = 2.5 cm, platform: 5 cm × 5 cm; bar length: all 20 cm). For the AE groups, a point-light-smiley lit up on the front of the box when the bar was inserted (see **Figure 1**, top right). The point-light-smiley consisted of 16 LED lights (Homefit lightning, 20 LEDs, 3.3V/0.066W) arranged in an outer circle (diameter: 8.6 cm) of 8 LEDs with a distance of 3.2 cm between each LED light and additionally, 2 LEDs for the eyes (distance: 3.4 cm), 1 for the nose and 5 for the mouth (distance: 1 cm).

The material used in the overturned-glass task consisted of an OSRAM LUX pod coaster (outer diameter: 9 cm; inner diameter: 6.5 cm) and a transparent, plastic glass (height: 9.5 cm, diameter: 5.5 cm on the bottom, and 7.5 cm on the top). The glass could be grasped near its narrow bottom by children with small hands. Alternatively, the glass could be grasped near its wide end by children with comparatively bigger hands, therefore adjusting for different hand sizes of the different age groups. Note, that in this task precision requirements were comparable to the bar-transport task, since the diameter of the bottom of the glass just fitted the inner diameter of the coaster. A white cardboard circle (diameter: 6 cm) was glued on the board

23 cm centimeters to the left and to the right of the coaster, in order to keep the starting position constant and to prevent the glass from slipping when being grasped. For the AE groups, the coaster lit up when the glass was placed on top of it (see **Figure 1**, bottom right). In contrast, for the No-AE groups, the batteries were removed in both set-ups and light effects never occurred. A camera was positioned behind the table, facing the participant, and all sessions were videotaped for future reference.

TASKS AND PROCEDURE

Participants were tested individually with three experimenters in the room. Only Experimenter 1 interacted with the participant and gave instructions. Experimenter 2 prepared the set-up between trials and tasks and Experimenter 3 took note of the participant's grip. Since adults follow instructions more readily, they were tested with only two experimenters in the room. Both tasks consisted of six trials and each trial began with the participant standing at the starting line. Before the first trial of each task, the starting state (bar resting on its platform, upside-down glass; see **Figure 1**, left column) and the desired end-state (bar in the box, glass on the coaster, see **Figure 1**, right column) was shown to the participant by Experimenter 2. However, how Experimenter 2 grasped the bar/glass was never demonstrated to the participant, neither during the demonstration nor before the first trial or in between trials. Experimenter 2 always covered the set-up with her body, and additionally, she covered her movements with a clipboard when grasping and moving the bar/glass. The starting position of the bar/glass was always opposite to the participants' to-be-used hand (e.g., for a right-hand-trial the bar/glass was placed to the left of the box/coaster). This was done in order to keep the movement required to grasp the bar/glass (moving the arm diagonally across the body's midline) constant across both tasks.

THE BAR-TRANSPORT TASK

In the bar-transport task, participants were asked to insert the bar into the opening of the box with their preferred hand and to put the non-preferred-hand behind their back. The bar-transport task was always performed with the preferred hand. Half of the trials were critical trials, which started with the bar being placed on its platform next to the bar holder (see **Figure 1**, top left). In critical trials, a thumb-down grip was required, followed by a 180° rotation to end in a comfortable thumb-up position. The other half of the trials were uncritical trials, which started with the bar being placed in the bar holder, requiring a thumb-up grip with no rotation of the bar.

THE OVERTURNED-GLASS TASK

In the overturned-glass task participants were asked to put the glass right-side-up on the coaster. In order to see if handedness has an impact on end-state comfort performance, half of the trials were preferred-hand-trials, in which participants had to use their preferred hand and to put their non-preferred-hand behind their back. The other half of the trials were non-preferred-hand-trials, in which participants had to use their non-preferred-hand and to put their preferred hand behind their back. In both, preferred- and non-preferred-hand-trials, a thumb-down grip of the glass was required followed by a 180° rotation of the glass to reach end-state comfort. In both tasks, if the child had difficulties using only one hand, the experimenter took the child by their not-to-be-used hand, walked them to the table, and kept hold of their hand until they had completed the trial.

For each age group, the order of the tasks was counterbalanced. That is, half of the participants received the bar-transport task first and the other half of the participants received the overturned-glass task first. The trial order of the two sorts of trials in each task (critical/uncritical in the bar-transport task;

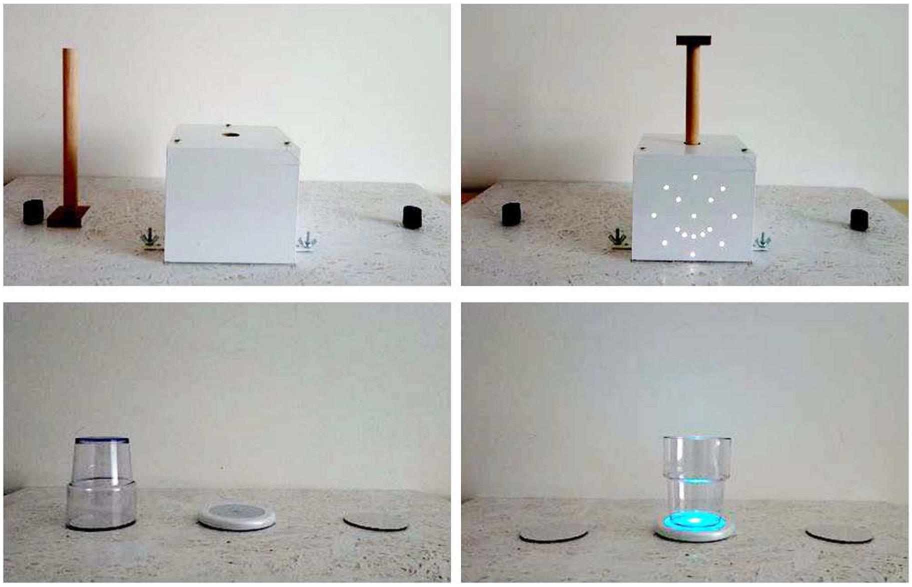

FIGURE 1 | Left column: starting position of the bar and the glass in the critical trial in the bar-transport task and right-hand-trial in the overturned-glass task. Right column: final position of the bar and the glass for the AE groups in the bar-transport task and the overturned-glass task.

preferred/non-preferred-hand in the overturned-glass task) was randomized such that (a) half of the participants started the task with a critical (preferred hand) trial and the other half of the participants started the task with an uncritical (non-preferred-hand) trial (b) the same sort of trial was administered maximally two times in a row. A trial was repeated if (a) both hands were used, (b) the wrong hand was used, (c) the glass/bar was grasped on its top, (d) if the glass was not turned, (e) if the glass/bar was turned on the participant's chest, or (f) an experimenter error occurred (such as indicating the wrong hand). Handedness of all child participants was determined before the start of the experiment by registering the participant's preferred hand when throwing a ball, holding a spoon, and drawing a face (one trial per task). The participant's preferred hand was determined by the hand that was used in at least two out of the three activities.

CODING

For each trial, participants' grip was coded. In the bar-transport task, the score 1 was given if participants grasped the bar thumb-down followed by a 180° rotation in critical trials and if participants grasped the bar thumb-up followed by no rotation in uncritical trials. The score 0 was given in all other cases. In the overturned-glass task, score 1 was given if participants grasped the glass thumb-down followed by a 180° rotation. The score 0 was given in all other cases. In accordance with Adalbjornsson et al. (2008) and Weigelt and Schack (2010), the end-state comfort effect was considered to be present if the score 1 was given in at least two out of three trials. All six trials of a randomly chosen set of 25% of the participants of each age group were coded by a second coder, blind to hypotheses of the study. Inter-rater reliability was perfect, Cohen's $\kappa = 1$.

RESULTS

In the following, the results regarding children's performance in the presence or absence of an action effect are reported first. Then, the results on the influence of age on end-state comfort performance are reported for (1) the bar-transport task, (2) the overturned-glass task, and (3) the comparison between the two tasks. For all analyses, non-parametric tests were used with a significance level of $\alpha = 0.05$ and with p-values between 0.05 and 0.10 considered as marginally significant. All p-values reported are two-tailed.

ACTION EFFECTS

There was no difference in end-state comfort performance between the AE and the No-AE groups in critical trials in the bar-transport task (Chi-square exact, $p = 0.394$; Fisher's exact tests per age group, all $ps > 0.467$) and neither in preferred-hand-trials (Chi-square exact, $p = 0.326$, Fisher's exact tests per age group, all $ps > 0.585$) or in non-preferred-hand-trials (Chi-square exact, $p = 1$, Fisher's exact tests per age group, all $ps > 0.438$) in the overturned-glass task (see **Table 1**). For the following analyses we therefore pooled the data of the AE groups and the No-AE groups in both tasks.

INFLUENCE OF AGE ON END-STATE COMFORT PERFORMANCE

Bar-transport task

The percentage of participants using a thumb-down grip in critical trials and the percentage of participants using a thumb-up grip in uncritical trials (in at least two out of three trials) in the bar-transport task for each age group are depicted in **Figure 2**. Almost all participants in all age groups grasped the bar thumb-up in uncritical trials with no significant difference between the age groups (Chi-square exact, $p = 0.93$). However, in the critical trials end-state comfort performance differed significantly between the age groups (Chi-square exact, $p < 0.001$) and increased with age: 3-year-olds 13%, 4-year-olds 38%, 5-year-olds 81%, 6-year-olds 75%, 7-year-olds 88%, 8-year-olds 94%. When compared separately, end-state comfort performance was significantly different between the 3- and the 5-, 6-, 7-, and 8-year-olds (Fisher's exact, all $ps < 0.001$), the 4- and the 5-, 7-, and 8-year-olds (Fisher's exact test, $ps < 0.029$). There was no effect of task order (bar-transport task first or second; Fisher's exact test over all groups, $p = 0.135$, Fisher exact tests per age group, all $ps > 0.262$), trial order (critical trials first or second; Fisher's exact test, $p = 0.832$), or gender (Fisher's exact test, $p = 0.202$) on end-state comfort performance in critical trials.

Table 1 | Percentages of end-state comfort in action effect (AE) groups and no-action effect (No-AE) groups in the bar-transport task and the overturned-glass task.

Age (years)	Bar-transport task				Overturned-glass task			
	Critical		Uncritical		Preferred		Non-preferred	
	AE	No-AE	AE	No-AE	AE	No-AE	AE	No-AE
3	0	25	100	75	75	50	75	63
4	38	38	88	100	75	63	63	75
5	75	88	100	100	75	88	100	100
6	75	75	88	100	88	75	88	75
7	86	89	86	86	86	86	86	100
8	88	100	100	88	100	100	100	88

All percentages are based on n = 8, with an except for the 7-year-olds (AE group n = 9, No-AE group n = 7).

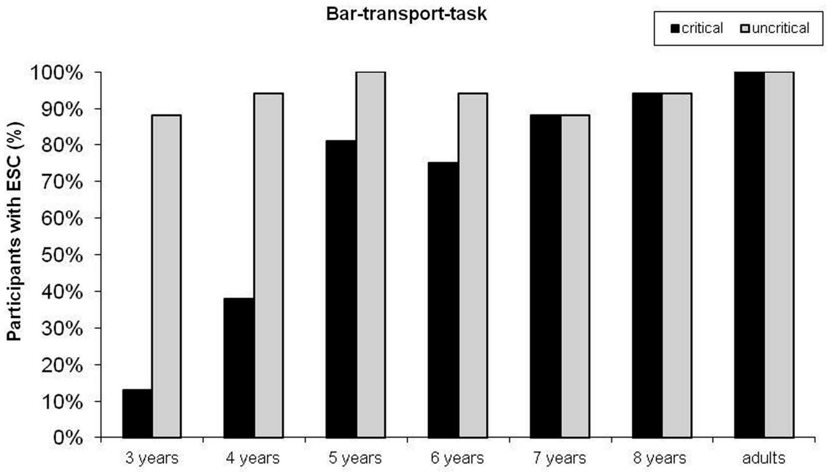

FIGURE 2 | Percentages of participants showing the end-state comfort effect (ESC) in critical and uncritical trials across age in the bar-transport task.

Trial repetitions were unlikely to influence end-state comfort performance. In each age group, participants performed a total of 96 trials and the total number of trial repetitions per age group was 25 for 3-year-olds, 19 for 4-year-olds, and 7 for each of the remaining older age groups.

Overturned-glass task

As depicted in **Figure 3**, the percentages of end-state comfort performance in preferred-hand-trials and in non-preferred-hand-trials did not differ significantly in neither of the age groups (McNemar, all $ps > 0.250$). In the following only analyses on preferred-hand-trials are reported in order to directly compare children's performance in the two tasks.

There was a significant increase in end-state comfort performance with age (Chi-square exact, $p = 0.006$): 3-year-olds 63%, 4-year-olds 69%, 5-year-olds 82%, 6-year-olds 82%, 7-year-olds 75%, 8-year-olds 100%. When compared separately, end-state comfort performance was significantly different between the 3- and the 8-year-olds (Fisher's exact test, $p = 0.018$) and the 4- and the 8-year-olds (Fisher's exact test, $p = 0.043$). There was no effect of task order (overturned-glass task first or second; Fisher's exact test over all groups, $p = 0.622$, Fisher's exact tests per age group, all $ps > 0.550$), trial order (preferred-hand-trials first or second; Fisher's exact test, $p = 1$), or gender (Fisher's exact test, $p = 0.34$) on end-state comfort performance.

Trial repetitions were unlikely to influence end-state comfort performance. Per age group participants performed a total of 96 trials and the total number of trial repetitions per age group was 26 for 3-year-olds, 12 for 4-year-olds, 11 for 5-year-olds, 12 for 6-year-olds, 11 for 7-year-olds, and 11 for 8-year-olds.

In order to investigate whether children may have learned to grasp the glass according to end-state comfort across trials, for each child, trial scores were summed across the first half (trials 1–3) and across the second half (trials 4–6) of the task, regardless of trials being performed with the preferred- or non-preferred-hand. When comparing children's end-state comfort performance in the first half with the second half of the task, there was no indication of learning (Wilcoxon, $p = 0.16$).

Performance across tasks

When comparing end-state comfort performance between critical trials in the bar-transport task and preferred-hand-trials in the overturned-glass task, differences were statistically significant only for the 3-year-olds (McNemar, $p = 0.008$), marginally significant for the 4-year-olds (McNemar, $p = 0.063$), and not significant for the other age groups (all $ps > 0.625$). Accordingly, a higher number of 3-and 4-year-olds showed the end-state comfort effect in the overturned-glass task, as compared to the bar-transport task.

DISCUSSION

The first aim of the present study was to compare the presence of the end-state comfort effect in children of different ages between two different object manipulation tasks, the bar-transport task, and the overturned-glass task respectively. In line with Weigelt and Schack (2010) and Stöckel et al. (2011), we found an increase in end-state comfort performance in the bar-transport task. Accordingly, children's propensity to use an uncomfortable thumb-down grip in critical trials rose from 13% in the 3-year-olds to 94% in the 8-year-olds. Interestingly, the number of children showing the end-state comfort effect in this task doubled from 3 to 4 years and from 4 to 5 years of age, whereas the older age groups differed only slightly in end-state comfort performance. This might suggest that the age between 3- and 5 years is an important period, in which children progressively become better in planning their movements.

However, when examining children's performance in the overturned-glass task, a different pattern of results was found. Here, end-state comfort performance also increased with age, but in contrast to the findings of Adalbjornsson et al. (2008), already 63% of the 3-year-olds in the current study grasped the glass according to end-state comfort. Thus, whereas only 13% of the 3-year-olds showed the end-state comfort effect in the bar-transport task, 63% of the 3-year-olds showed the end-state comfort effect

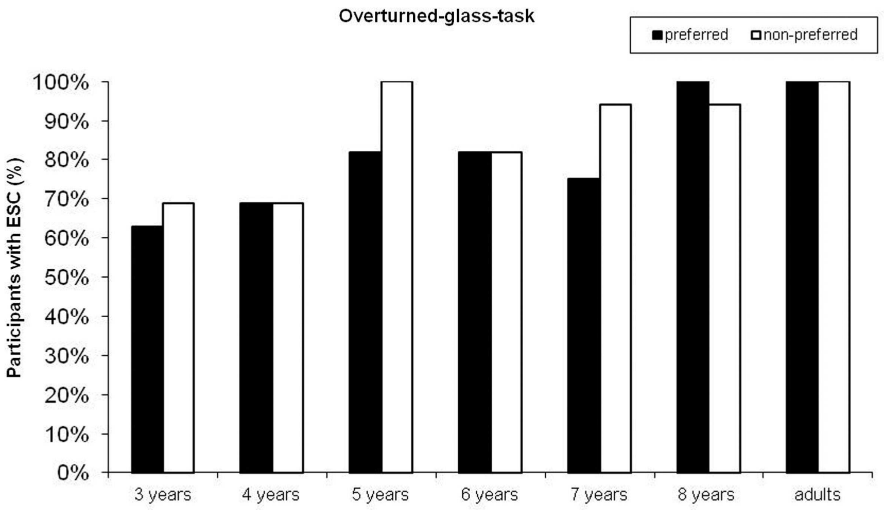

FIGURE 3 | Percentages of participants showing the end-state comfort effect (ESC) in preferred hand and non-preferred-hand-trials across age in the overturned-glass task.

in the overturned-glass task. This difference in end-state comfort performance at the younger ages might be explained by the child's familiarity with the object involved. The child's familiarity with the object involved pertains to the amount of prior experience children have gathered with that object throughout lifetime. Object manipulations with every day objects, such as glasses, are likely more familiar to preschoolers than object manipulations with novel objects, such as the bar used in the present study. For example, studies investigating tool-use in young children show that, around 1 year of age, the way children grasp objects is influenced by the intended future use. This is evidenced by distinct movement kinematics for actions on different objects (Claxton et al., 2003) or progressively more efficient grasping strategies used for a familiar tool, such as a spoon containing food across 9-, 14-, and 19-month-old children (McCarty et al., 1999). More specifically, Barrett et al. (2007) have shown that the familiarity with a tool (prior experience) influences its use in a novel task in 12- and 18-month-old children. In their study, children had to turn on a light inside a box by using either a familiar tool (spoon) or an unfamiliar tool (spoon-like object). In one condition, both tools fit in the box with their handle end only. Results revealed that children tended to grasp the familiar spoon by its handle even though children were shown to grasp the spoon by its bowl end for insertion. In contrast, the unfamiliar spoon was grasped much more flexibly and led to significantly more successes. Thus, children's familiarity with the object manipulations involved in this novel task likely biased their tool-directed actions.

This bias might be explained by a competition between the goal-directed and the habitual system as suggested by Herbort and Butz, 2011; see also Stöckel et al., 2011 for a similar interpretation). The goal-directed system selects grasping movements according to the intended future use of the object (insertion into the box), whereas the habitual system selects grasping movements that are habitually used to grasp the object (self-feeding). When presented with an unfamiliar tool, both systems select the same action (insertion into the box). However, when presented with a familiar tool, the habitual system trumps the goal-directed system and a grasping movement that has been used repeatedly in the past for that object is selected (self-feeding). Therefore, in the study by McCarty et al. (2001), the habitual system likely had hindered children in solving a novel task with a familiar object. In contrast, in the present study, the habitual system might have helped children in solving a familiar task (overturned-glass task) with a familiar object. That is, the greater familiarity with handling glasses in the present study might have helped children to plan their movements in the overturned-glass task more accurately as compared to the bar-transport task. In the bar-transport task, the habitual system likely did not help children in solving the task, since children may not have gathered sufficient experience with the object manipulations required. Indeed, when comparing end-state comfort performance between critical trials in the bar-transport task and preferred-hand-trials in the overturned-glass task, 3- and 4-year-old children were better in manipulating the glass according to end-state comfort in the overturned-glass task than they were able to manipulate the bar in the bar-transport task. The older children, in contrast, were able to manipulate both, the glass and the bar equally efficient. Therefore, when considering children's performance in both tasks, results show that the age between 3- and 5 years is an important period for motor planning in which the familiarity with the object involved in the task may play an important role in children's ability to plan their movements according to end-state comfort.

Given this finding, the question arises whether the bar-transport task and the overturned-glass task were truly comparable, since in the bar-transport task an uncomfortable thumb-down grip was required only in half of the trials, whereas in the overturned-glass task an uncomfortable thumb-down grip was required in all the six trials. Consequently, when considering that children in the overturned-glass task were given the opportunity to use end-state comfort twice as much as compared to the bar-transport task, children might have learned across trials to use a thumb-down grip. If so, one might expect children to perform better in the bar-transport task when the latter was administered after the overturned-glass task. However, there was no order effect of tasks found. Furthermore, when comparing end-state comfort performance across the first and second half of trials in the overturned-glass task, there was no indication of learning, suggesting that indeed familiarity with the object has a significant impact on young children's propensity to use end-state comfort in object manipulation tasks. This interpretation is supported by a recent study showing that preschoolers imitate familiar tool-use actions more correctly than unfamiliar tool-use actions (Wang et al., 2012).

It could also be argued that anticipatory motor planning for manual action is object or task specific for known objects or tasks, especially for younger children. This can be inferred from the different onsets of the end-state comfort effect for the two different tasks, the bar-transport task and the overturned-glass task, respectively. Younger children were more proficient in solving the overturned-glass task than the bar-transport task. This shows that the end-state comfort effect does not generalize simply from one task to another task. Importantly, adding an action effect at the end of the manipulatory maneuver did not make the end-state comfort effect to occur more often. This is an interesting aspect, since it has been shown elsewhere that action effect associations are formed by children of the same age or even younger (Verschoor et al., 2010). For example, Eenshuistra et al. (2004) showed that already 4-year-old children are able to acquire response-effect associations. At the same time, these children still display stronger stimulus-driven behavioral tendencies as compared to 7-year-old children, and they are less able to maintain the task goal. Thus, it seems reasonable to assume that anticipatory planning skills develop at different rates regarding object manipulation and the acquisition of response-effect associations. This assumption should be tested in future studies.

The second aim of this study was to investigate whether the action effects presented in the bar-transport task by Jovanovic and Schwarzer (2011) were the determining factor with regard to the comparatively high percentage of 60% end-state comfort shown by the 42-month-old children. The present results suggest that it is unlikely that the high percentage of end-state comfort performance of the 42-months-old children reported by Jovanovic and Schwarzer (2011) can be attributed to the presence of action effects. Children in the present study did not benefit from the action effects presented, neither in the bar-transport task nor in the overturned-glass task. This is most likely explained by one important aspect that makes this study different from other studies investigating the influence of action effects in infants (e.g., Hauf et al., 2004; Paulus et al., 2011). Children were never shown the uncomfortable thumb-down grip in order to make the light effects occur and, consequently, no action effect associations could have been formed by observation. Instead, children were only shown the starting state and the end-state of the action, without the corresponding action and children had to emulate, rather than imitate, the action. According to the common coding theory, however, the formation of action effect associations is crucial. Only through repeated co-occurrences of the uncomfortable thumb-down grip and the following light effect could children have associated the light effect with a thumb-down grip and hence, anticipating the light effect might have helped them plan their movements more readily according to end-state comfort. In addition, the light effects were presented independent of the initial grasp used by the child (thumb-down or thumb-up). That is, also thumb-up grips in critical trials (and in preferred- or non-preferred-hand-trials) were followed by a light effect. Consequently, one might expect that children in the AE groups, who used a comfortable thumb-up grip in the first critical trial will use a comfortable thumb-up grip in the following trials, independent of trial type, due to instrumental learning established by the rewarding light effect (smiley). However, inspection of the data revealed that all children changed grip posture in either task with an increase in thumb-down grips across age. Even in the 3-year-olds each child showed an uncomfortable thumb-down grip in the overturned-glass task at least once.

Still, it may be that the presence of an action affect may indirectly enhance young children's performance in these tasks by positively affecting their interest in performing the task. Already very young infants easily detect a contingent relation between their own movements and subsequent effects and greatly enjoy this experience of self-efficacy (Watson, 1972). Thus, the relatively simple and possibly boring actions of inserting a bar into a hole and putting a glass on a coaster may become more interesting once they are instrumental to self-produce a more interesting event. In a similar vein, the presence of an action effect adds a reason to performing the action itself, distinct from simple compliance with the experimenter's instructions. As there seems to be a general teleological bias in human reasoning, that is, a tendency to ask what objects and events *are for* (Kelemen, 1999), qualifying an action as a mean to achieve a goal might indirectly enhance children's motor planning in end-state comfort tasks by affecting, for example, attentional or motivational processes. In this sense, it might still have been possible that the comparatively high percentage of end-state comfort performance of the 42-month-old children reported by Jovanovic and Schwarzer (2011) was due to such attentional or motivational factors. The current findings, however, suggest that the action effects used by Jovanovic and Schwarzer did not play a role in end-state comfort planning, neither directly according to the principles of the common coding theory, nor indirectly via enhancing children's interest in performing the task.

Even though the present findings are in line with previous studies that found a general increase in end-state comfort performance over age, the results of this study are opposite to findings reported by Adalbjornsson et al. (2008) and Jovanovic and Schwarzer (2011). Adalbjornsson et al. (2008) found only little evidence for end-state comfort in the overturned-glass task in 2- to 6-year-old children, whereas in the present sample already 63% of the 3-year-olds grasped the glass according to end-state comfort. This discrepancy might be due to differences in the experimental set-up. In the study by Adalbjornsson et al. (2008) children were sitting in front of the table during test trials. This might have caused some motorical restrictions whereas in the actual study, children were standing during testing, and could therefore move their arms more freely. In addition, Adalbjornsson et al. (2008) asked the children not only to turn the glass but also to pour water from a pitcher into it. For both action parts, children were only allowed to use their preferred hand. Thus, children were not only asked to perform a rather complex action sequence, it might also be that the planning of the second action (grasping the pitcher by using a thumb-up grip and pouring water into the glass) influenced the way the first action (turning the glass) is performed. This explanation is supported by studies showing that later elements of an action sequence are already planned and specified at the beginning of the sequence (see, e.g., Inhoff et al., 1984).

Likewise, in the bar-transport task conducted by Jovanovic and Schwarzer (2011) already 60% of the 42-month-olds grasped the bar according to end-state comfort, whereas only about half as much 3- and 4-year-old children in the present study showed the end-state comfort effect in the bar-transport task. Although tasks were quite similar, still some differences in the procedure of the two studies might account for this inconsistent result. In the bar-transport task with 42-month-old children (Experiment 1) of Jovanovic and Schwarzer (2011) no care was taken to occlude the rotation of the bar when the experimenter returned the bar to its starting position in between trials. Even though 18- and 24-month-old children did not benefit from observing the experimenter performing the transport in the reverse condition as shown in Experiment 3 of the same study, the 42-month-olds tested in Experiment 1 might have benefited from observing the experimenter grasping the bar according to end-state comfort when returning the bar. In contrast, children in the current study never saw the experimenter grasping the bar.

There is also a notable difference in end-state comfort performance in the bar-transport task of children of comparable age found by Stöckel et al. (2011) and the present study. Stöckel et al. (2011) found that 50% of the 7-year-olds, 67% of the 8-year-olds, and 92% of the 9-year-olds used end-state comfort. In contrast, in the present study, comparable success rates shifted toward the younger age groups with already 81% of the 5-year-olds showing the end-state comfort effect. One possible explanation for this shift of success rate in age may be the different rotations of the bar required in each task. Whereas the bar-transport task by Stöckel et al. (2011) required only a 90˚ rotation, the bar-transport task in the present study required a 180˚ rotation. If the bar has to be rotated by 180˚ an initial comfortable grip would end in an even more uncomfortable grip (arm rotated counterclockwise 180˚ rather than 90˚) for a 180˚ rotation than for a 90˚ rotation, which children might have sought to avoid. However, in the bar-transport task reported by Weigelt and Schack (2010) also only a 90˚ rotation of the bar was required and their results are comparable to the results of the present study (18% end-state comfort in 3-year-olds, 47% in 4-year-olds, and 70% in the 5-year-olds). This suggests that other differences between the studies are more likely to account for the difference in end-state comfort performance shown by children of comparable age.

There are several methodological differences between the present study and previous work that might account for the inconsistent findings reported on the development of the end-state comfort effect in young children. These differences relate, for example, to the particular task and the version of the task used, the familiarity of the object to be manipulated, task complexity, and precision requirements. In addition, also the specific procedure used in a study might influence children's performance in motor planning tasks. For example, it might make a difference, if participants are standing or sitting when performing the task. In the majority of studies that found the end-state comfort effect in children, including the present work, the children were standing during testing and could therefore move their arms more freely (e.g., Weigelt and Schack, 2010; Stöckel et al., 2011). In contrast, in several studies that did not observe the end-state comfort effect, children were sitting during the testing session (e.g., Manoel and Moreira, 2005; Adalbjornsson et al., 2008; van Swieten et al., 2010).

Finally, the development of different cognitive abilities, such as executive functions, might explain some of the inconsistencies in the findings. It is interesting to note that around the same time during the preschool years when end-state comfort planning seems to develop, children also show a marked improvement in higher level cognitive processes, termed executive function, that are involved in planning and controlling goal-directed behavior (Zelazo et al., 1997). Therefore, differences in task demands that are due to differences in methodology, such as those mentioned above, may have an effect on task performance. This is especially true for the preschool age, which seems to be a period of marked development in end-state comfort planning. For example, the less complex a given task, the less executive control it may require, or the more familiar the child is with the to be manipulated object, the more cognitive resources may be available for motor planning and control. It is therefore possible, that differences in task demands which tap into executive control processes may have a greater effect on younger compared to older children's performance in end-state comfort tasks. Further research is needed to investigate whether executive function skills play a role in the development of end-state comfort planning.

To summarize, the current work provides three major findings. First, despite inconsistencies in the literature regarding the onset of end-state comfort planning in childhood and its prevalence at specific ages, several studies point to a general increase in motor planning skills as indicated by the end-state comfort effect across the preschool and school years. The results of the present study are in line with this observation. Second, results suggest that the high percentage of end-state comfort performance of the 42-months-old children reported by Jovanovic and Schwarzer (2011) cannot be attributed to the presence of action effects. Third, the comparison of two different object manipulation tasks within the same participants allowed us to investigate the influence of the particular task used as a possible contributing factor to the inconsistent results found across studies. Results suggest that the familiarity with the object involved in the task does play a significant role in at least the younger children's ability to plan their movements according to end-state comfort.

AUTHOR NOTE

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Investigating ideomotor cognition with motorvisual priming paradigms: key findings, methodological challenges, and future directions

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Ideomotor theory claims that perceptual representations of action-effects are functionally involved in the planning of actions. Strong evidence for this claim comes from a phenomenon called motorvisual priming. Motorvisual priming refers to the finding that action planning directly affects perception, and that the effects are selective for stimuli that share features with the planned action. Motorvisual priming studies have provided detailed insights into the processing of perceptual representations in action planning. One important finding is that such representations in action planning have a categorical format, whereas metric representations are not anticipated in planning. Further essential findings regard the processing mechanisms and the time course of ideomotor cognition. Perceptual representations of action-effects are first activated by action planning and then bound into a compound representation of the action plan. This compound representation is stabilized throughout the course of the action by the shielding of all involved representations from other cognitive processes. Despite a rapid growth in the number of motorvisual priming studies in the current literature, there are still many aspects of ideomotor cognition which have not yet been investigated. These aspects include the scope of ideomotor processing with regard to action types and stimulus types, as well as the exact nature of the binding and shielding mechanisms involved.

Keywords: motorvisual priming, dual task, ideomotor theory, binding, planning and control model, action-effect blindness, categorical perception

INTRODUCTION

Human behavior is to a large degree anticipative and goal-directed. That means most of our actions are not merely direct responses to environmental stimuli, but are selected with regard to an anticipated action goal. How anticipated action goals are cognitively processed in action selection is an extensively researched area in cognitive psychology (e.g., Nikolaev et al., 2008; Nattkemper et al., 2010; Pfister et al., 2012). Currently one of the most influential theories in this area is the ideomotor theory (Massen and Prinz, 2009; Shin et al., 2010). The fundamental claim of ideomotor theory is that anticipated action goals processed in action selection are represented as the *sensory* consequences of achieving those goals. To put it another way, action selection involves perceptual representations of action-effects (Kunde et al., 2007; Waszak et al., 2012). Various versions of ideomotor theory have emerged in the cognitive psychology literature during the last three decades (see Kunde et al., 2007; Nattkemper et al., 2010; Shin et al., 2010, for reviews). Despite some conceptual differences between these versions, all variations are based on two essential hypotheses: first, goal-directed behavior is achieved by goal representations which have a functional role in action selection. Second, the goal representations are represented in the same format as sensory input from these goal states would be represented (Prinz, 1997).

Although the ideomotor theory has a long history in philosophy and psychology (Stock and Stock, 2004; Pfister and Janczyk, 2012), it has evolved with increasing rapidity only since the late 1990s, owing to a growing number of empirical findings supporting the involvement of perception in action processing (see Nattkemper et al., 2010; Shin et al., 2010, for reviews). During this time a set of classical ideomotor paradigms has emerged.

One example is the response-effect-compatibility paradigm (Kunde, 2001, 2003, 2004; Koch and Kunde, 2002; Rieger, 2007; Janczyk et al., 2009; Pfister et al., 2010). In response-effect-compatibility experiments, participants provide free or forced choice responses, which have task-irrelevant effects. Effects can be compatible (i.e., naturally following on from the current response, e.g., a left stimulus following a left key press), or incompatible. Responses are on average faster when they are followed by compatible effects than by incompatible ones. A performance decrement when action and effect are constantly mismatched indicates that response processing is sensitive to action-effect matching, and involves, thus, some representations of effects (Hoffmann et al., 2001).

Another classical paradigm in ideomotor research is the effect-learning paradigm (Elsner and Hommel, 2001, 2004; Hommel et al., 2003; Kray et al., 2006; Hoffmann et al., 2009). The logic is similar to the response-effect-compatibility design, the only difference being that the action-effect associations are acquired only during the experiment, in an initial learning phase. In a seminal study by Elsner and Hommel (2001) participants pressed two keys in an arbitrary self-chosen sequence. The keys were contingently followed by tone effects. After that, either a forced or a free choice test phase was employed (differing between experiments and studies). In a forced choice test phase, the former action-effects now figured as action cues. Participants were faster when the cue response mapping matched the cue effect mapping experienced in the learning phase than when the mapping was reversed (see also Herwig et al., 2007; Herwig and Waszak, 2009). In a free choice test phase, where the former action-effects figured as Go-signals, participants chose more often than chance would suggest the response which had been followed, in the learning phase, by the current Go-signal (see also Hoffmann et al., 2009; Pfister et al., 2011).

Further prominent paradigms in the context of the ideomotor theory include versions of the Serial Response Time paradigm (Nissen and Bullemer, 1987) that emphasize the formation of R-S associations (Ziessler, 1998; Ziessler and Nattkemper, 2001), and studies where human movement stimuli induce compatible response tendencies in observers (Knuf et al., 2001; De Maeght and Prinz, 2004; Prinz et al., 2005; Häberle et al., 2008; Watanabe, 2008).

Although research with these paradigms has produced significant knowledge about ideomotor mechanisms, they provide only relatively indirect access to the processing of perceptual representations in action. In these paradigms, the process of action selection can only be primed *in advance* by perceptual activation. The effectiveness of perceptual effect-like primes on the consecutive action is interpreted as evidence for the involvement of perceptual representations in the selection of these actions. A more direct experimental access to ideomotor cognition would require measuring perceptual processing online, *during* action planning. This strategy is realized in motorvisual priming paradigms.

MOTORVISUAL PRIMING

In motorvisual priming paradigms, a response action (R1) is selected and executed in response to a perceptual cue (S1), while, concurrently, a stimulus (S2) has to be detected or identified (see **Figure 1**). The experimenter manipulates whether S2 is ideomotor-compatible with R1 (i.e., whether on any dimension it is similar to an effect of R1) or not. This compatibility usually affects the speed or accuracy of S2 perception. Such compatibility effects are commonly seen as originating from an involvement of perceptual representations of effect-compatible stimuli in action planning (Kunde and Wühr, 2004).

Motorvisual priming paradigms can, on the one hand, be realized as *single* task versions (Craighero et al., 2002). In this case, S2 mostly figures as a Go-Signal for R1 (see **Figure 1**). The identity of R1 is cued by S1, but R1 is withheld until presentation of S2. Although the identity of the Go-Signal is irrelevant to the task, its ideomotor-compatibility with the withheld response still has an effect on the response correctness and latency (Craighero et al., 1999). These effects are commonly interpreted as reflecting the processing of perceptual representations in action planning (Craighero et al., 2002; Bortoletto et al., 2011).

On the other hand, motorvisual priming paradigms have also been realized in *dual task* versions. In these studies R1 is planned according to S1 and executed either immediately (Müsseler and Wühr, 2002; Wühr and Müsseler, 2002), at the participant's own pace (Eder and Klauer, 2007; Oriet et al., 2007), or after a compatibility-neutral Go-Signal (Kunde and Wühr, 2004; Hommel and Müsseler, 2006). At different times during the preparation or execution of R1, a second stimulus S2 is displayed. S2, in contrast to single task versions, is not a Go-Signal for R1 here, but belongs to a second unrelated task instead (see **Figure 1**). The second task consists in reporting the identity of S2 by a secondary – either speeded (Zwickel et al., 2007; Pfister et al., 2012) or unspeeded (Müsseler and Hommel, 1997a; Stevanovski et al., 2002) – response R2. Although R1 and S2 belong to different tasks and are functionally unrelated, influences of ideomotor-compatibility between R1 and S2 on R2 performance have frequently been observed. These effects are commonly interpreted as being owed to the involvement of perceptual representations in R1 processing. In R1-S2 compatible trials, this involvement interferes with S2 perception, and this interference is reflected by R2 performance (Müsseler, 1999; Eder and Klauer, 2009).

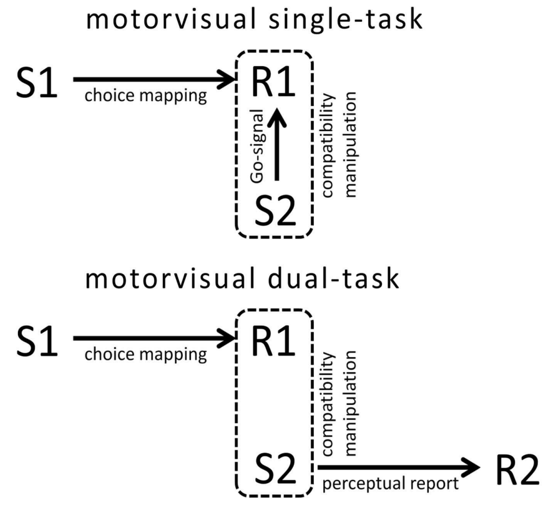

FIGURE 1 | Schematic illustration of motorvisual single tasks and motorvisual dual tasks. In both types of tasks R1 is selected according to a perceptual cue S1. During R1 preparation, a target stimulus S2 is presented. The experimenter manipulates whether R1 and S2 are ideomotor-compatible or not, that is, whether S2 resembles, in any respect, an effect of R1. In single tasks S2 figures as Go-stimulus for R1. In dual tasks S2 identity is reported by a secondary response R2. Thus, R1 and S2 are functionally unrelated. A motorvisual priming effect is present when R1-S2 compatibility affects R1 performance in single tasks, or when R1-S2 compatibility affects R2 performance in dual tasks.

Motorvisual priming is, of course, not the only way in which actions can affect perception of ideomotor-compatible stimuli. There is accumulative evidence that motor-expertise with certain movement types (e.g., athletics or dancing) can selectively improve the perception of movements of this type (Calvo-Merino et al., 2005; Craig et al., 2009; Hohmann et al., 2011; Cañal-Bruland et al., 2012; Diersch et al., 2012). A similar long-term motorvisual effect has been observed in experimentally controlled motorvisual learning studies. Motorvisual learning experiments typically include a motor-learning phase, where the participants acquire new movement patterns, and a visual test phase, where the participants have to observe similar movement patterns. Results typically show that movement practice selectively improves or biases perceptual capacities for stimuli similar to the motor-practiced ones (Hecht et al., 2001; Casile and Giese, 2006; Engel et al., 2008a,b; Beets et al., 2010; Glenberg et al., 2010). Both learning phenomena can be interpreted as evidence for the involvement of perceptual representations in motor execution (Vogt and Thomaschke, 2007).

Although such motorvisual learning transfer studies are of eminent importance in determining visuomotor processing in skill acquisition, they are, however, of limited value for the detailed investigation of ideomotor mechanisms. As in the aforementioned effect-learning paradigms, inferences are drawn from relations between different experimental phases. These conclusions are informative about how one acquires action-effect associations, but are less informative about the mechanisms by which the acquired action-effects bring about intended actions. For that reason, motorvisual priming studies pose a much more powerful and temporally precise tool, because these paradigms allow manipulation and measurement of ideomotor processes in an online, trialwise fashion. Consequently, the present review focused on motorvisual priming studies, although occasional evidence from learning studies is cited when directly related to the findings from priming studies.

MOTORVISUAL PRIMING AND IDEOMOTOR THEORY

Motorvisual priming studies have frequently been cited as support for the ideomotor theory (Stoet and Hommel, 1999; Kunde and Wühr, 2004; Shin et al., 2010). Actually, only rarely has any other explanation been offered for motorvisual priming than the processing of perceptual representations in action (see, however, Oades and Kreul, 2001, for an exception). Indeed it seems difficult to explain why actions should affect unrelated perceptual events, if perceptual representations are *not* involved in action processing. Thus, motorvisual priming can be regarded as one of the most convincing cases of empirical evidence for ideomotor theory's central claim that action planning processes perceptual action-effects. The reference to motorvisual priming in the ideomotor literature is quite general, however. It usually does not go beyond citing motorvisual priming as support for the theory in general. This is in stark contrast with the high informative value that motorvisual priming studies have for the understanding of many aspects of processing perceptual effect representations in action selection.

The aim of the present review is to show that previous motorvisual priming studies allow precise conclusions about the detailed functional role that perceptual representations play in action planning. Furthermore I discuss potential methodological pitfalls in designing motorvisual priming studies, and sketch directions for future motorvisual priming research which might further elucidate the mechanisms of ideomotor cognition.

HOW ARE PERCEPTUAL EFFECT REPRESENTATIONS PROCESSED IN ACTION PLANNING?

In the following two subsections, I review evidence from motorvisual priming studies on different aspects of ideomotor cognition. First, it will be argued that results from motorvisual priming studies allow the nature of perceptual representations processed in action planning to be determined, and that the findings are in accordance with predictions of current theories of ideomotor cognition. Second, motorvisual evidence for the binding of perceptual action-effects in action plans is discussed. Motorvisual priming studies have shown that action selection activates and binds effect representations throughout planning and execution, in order to stabilize the action plan against interferences.

IDEOMOTOR COGNITION PROCESSES CATEGORICAL REPRESENTATIONS OF ACTION-EFFECTS

As reviewed above there is plenty of evidence for the involvement of perceptual representations in action processing. One important question which has not been answered in the action-effect learning studies previously discussed is the format which these perceptual representations have. Are they perceptual representations of a categorical symbolic nature or are they metric spatial representations? In this subsection I will show that motorvisual priming studies can answer this question, and that it is in favor of categorical representations.

There is accumulative interdisciplinary evidence that human cognitive processing makes use of two fundamentally different kinds of mental representations (Kosslyn, 1987, 2006; Logan, 1995; Kosslyn et al., 1998; Okubo et al., 2010). One class of mental representations is commonly referred to as *categorical*. Categorical representations are of a relational nature. They are used to represent cognitive entities – like stimuli or responses – as members of categories. Typical examples include word identities, affective categories (positive/negative), or abstract propositional spatial categories (e.g., above/below, or left/right).

Categorical representations are usually defined in opposition to *metric* representations. Metric representations are coordinate representations of exact spatial relations. Examples include the coordinate location of a stimulus or an effector, or the exact size or rotational angle of an object (Kosslyn, 1994).

Categorical representations in planning and control

In order to answer the question of which representations are subject to ideomotor cognition, one must take into account that action processing is commonly conceived of as consisting of two different sub-processes, planning and control, and that these sub-processes differ with respect to the processed representations.

Most theories of motor cognition distinguish between movement planning processes and movement control processes (Elliott et al., 2001; Glover et al., 2012). Planning processes integrate motivational, environmental, and goal-anticipative factors in order to determine the appropriate action in a given situation. Action planning does not specify in advance the entire course of the movement until its completion, but instead determines only the gross parameters in order to initiate the movement (e.g., which effector, which initial direction, etc., see Schmidt, 1975; Hommel, 2005, 2010).

Movement control, on the other hand, comes into play once the movement is chosen and initiated. Movement control specifies the exact movement parameters online via fast feedback cycles. Control constantly compares predictions based on the actual state of the movement with incoming sensory information, in order to minimize mismatch between movement goal and prospective course of the movement (Wolpert et al., 1998; Wolpert and

Ghahramani, 2000; Bubic et al., 2010). Movement control works at high speed and can rapidly correct for movement disturbances and perturbations in target size, location, or orientation (Prablanc and Pélisson, 1990; Gosselin-Kessiby et al., 2008; Hesse and Franz, 2009; van de Kamp et al., 2009).

Glover (Glover, 2002, 2004; Glover and Dixon, 2002) has suggested that movement planning primarily processes categorical representations whereas control processes only metric information. This proposal has been supported by a considerable amount of evidence from empirical studies that compared the effects of categorical and/or metric manipulations on planning and control processes (see Glover, 2004; Thomaschke et al., 2012a, for reviews). For example, Keulen et al. (2004) have shown that priming a reaching task with the gross layout of the target distracter distribution, pertinent to categorical coding, affected only the early planning stage of grasping. The locational coordinate distance between distracter and target, likely to infer with metric processing, on the other hand, affected only later control stages of the grasping movements. In a dual task study, Liu et al. (2008) showed that a letter identification task affected a secondary pointing movement task only in RT (a measure of planning duration), but not in accuracy (a measure of control effectiveness). Likewise, Spiegel et al. (2012) found that planning a grasping movement impairs a secondary verbal working memory task, but grasping control does not. Hesse and Deubel (2011) showed that a digit identification task affected initial movement parameters in the early phase of a grasping movement, whereas the later, control-based, phase of the movement was unaffected. See Glover and Dixon (2002) and Thomaschke et al. (2012b) for further evidence that planning processes categorical information and control processes metric information.

These findings lead to a clear conclusion concerning the type of perceptual representations in ideomotor cognition. As the ideomotor principle – that action selection involves perceptual effect processing – concerns action selection, not action control, ideomotor theory is commonly assumed to relate exclusively to the action planning stage, not to action control (Kunde et al., 2007; Shin et al., 2010; Janczyk et al., 2012). Consequently, the ideomotor theory would suggest that the perceptual representations processed in motor cognition are of a categorical nature.

Types of mental representation in motorvisual priming

Motorvisual priming studies provide a simple straightforward test for the ideomotor theory's prediction that perceptual representations in action planning are categorical. When motorvisual priming effects are owed to perceptual processing in action selection, then one would assume that such effects can only be observed for dual tasks where the overlap between R1 and S2 is categorical rather than metric.

At first glance, the empirical findings are clearly at odds with this prediction. Motorvisual priming has been observed for categorical R1-S2 overlap (e.g., Kunde and Wühr, 2004), but also for instances where the overlap can unambiguously be described as metric (e.g., sharing a certain spatial location, see Deubel et al., 1998), and for stimuli where it is ambiguous whether the overlap is categorical or metric (e.g., biological stimuli, Jacobs and Shiffrar, 2005; Miall et al., 2006).

Closer inspection reveals, however, that the effect direction in motorvisual priming research systematically varies between different studies, depending on the type of the representations involved. Some have reported motorvisual impairment, whereas perception was facilitated by compatible action in other studies (Schütz-Bosbach and Prinz, 2007; see Muthukumaraswamy and Johnson, 2007; Press et al., 2009; Thomaschke et al., 2012a; and Zwickel and Prinz, 2012, for systematic discussions of this issue). The effect direction allows a clear distinction to be drawn between motorvisual effects owed to action planning and motorvisual effects owed to other action-related processes.

One important aspect of action planning processes is that they integrate and stabilize the features of a selected movement. This ensures that the basic features of the movement (e.g., which effector is moved) remain constant throughout its course. This, however, requires that movement features are shielded against access by other cognitive processes, including other action alternatives, including perceptual processes (Müsseler, 1999; Stoet and Hommel, 1999). As perceptual effect representations, according to ideomotor theory, are also features of an action plan, these features are also shielded against other cognitive processes, including perception. Thus, ideomotor theory would predict that action planning impairs the perception of effect-compatible stimuli, instead of facilitating it (Hommel et al., 2001; Hommel, 2009).

Types of mental representation in motorvisual interference.

When we consider only motorvisual impairment studies among the motorvisual priming studies – that is, studies where the motorvisual priming effect can unambiguously be ascribed to perceptual processing in action planning – the overlap between R1 and S2 is exclusively on a categorical dimension. Motorvisual impairment has been shown for speaking and identifying direction words (Hommel and Müsseler, 2006), or color words (Kunde and Wühr, 2004), and for writing and identifying letters of certain forms (James and Gauthier, 2009). Other examples include impairment from left/right key presses on left/right pointing arrow symbols (Müsseler and Hommel, 1997b), and impairment from positively/negatively charged lever movements on the detection of positive/negative words (Eder and Klauer, 2007, 2009). Hence, motorvisual priming studies have confirmed the ideomotor theory's prediction that ideomotor cognition relates exclusively to categorical representations (see also Zwickel et al., 2010a, for a discussion of this issue).

Some authors have divided categorical representations further into *verbal* categorical representations and *spatial* categorical representations (Kemmerer and Tranel, 2000; Tranel and Kemmerer, 2004; van der Ham and Postma, 2010; van der Ham and Borst, 2011). A motorvisual priming study by Hommel and Müsseler (2006), has shown that both kinds of categorical representations can figure as perceptual effect representations in action selection. In one experiment speaking the words left/right impaired the identification of the written words left/right in compatible trials compared with incompatible trials (Exp. 3B). The overlap in this experiment can clearly be regarded as categorical verbal. In an analogous experiment (Exp. 1A), with left/right key presses as R1, and left/right pointing arrow symbols as S2, the overlap was categorical spatial. Again, a motorvisual impairment effect was observed. In further experiments with the cross-conditions, spoken words with arrow heads (Exp. 2B), and key presses with written words (Exp. 3A), no motorvisual impairment effect was observed. The absence of an effect in the latter experiments confirmed that two different kinds of categorical representations were applied. Thus, action selection involves spatial and verbal categorical representations.

Further support for the strict confinement of ideomotor cognition to categorical representations comes from the finding that motorvisual priming effects – that are not related to action selection (see above) – have been observed *only* for metric spatial R1-S2 overlap on non-categorical dimensions. These studies can be classified in motorvisual priming studies with non-biological metric R1-S2 overlap, and with biological motion stimuli. I discuss each type in turn.

Motorvisual facilitation with metric representations. When R1 and S2 overlap on a metric dimension, effects are either absent (Bonfiglioli et al., 2002) or in a positive direction (Hommel and Schneider, 2002; e.g., Paprotta et al., 1999; Wykowska et al., 2009, 2011, 2012) in the sense of better performance when R1 and S2 are metrically compatible than when they are not. Motorvisual facilitation effects are thought to reflect action control, instead of action selection (Hommel, 2009, 2010; Thomaschke et al., 2012a). Action control is a closed loop process leading to rapid constant online updating of precise metric information in order to correct potential target movement mismatches (Wolpert et al., 1998; Wolpert and Ghahramani, 2000). This process benefits greatly from attentional pre-selection of movement-relevant areas in perceptual space. Thus actions, once selected, strongly facilitate processing of compatible metric representations like effector laterality (Hommel and Schneider, 2002; Koch et al., 2003; Müsseler et al., 2005; Press et al., 2009; Gherri and Eimer, 2010), goal location (Fischer, 1997; Linnell et al., 2005), orientation (Lindemann and Bekkering, 2009; Janczyk et al., 2012), or size (Fagioli et al., 2007a,b; Symes et al., 2008, 2010). This motorvisual facilitation effect is, in contrast to action planning, independent from action context (Fischer et al., 2007; Thomaschke et al., 2012b) but it is selective with regard to the specific control demands of different movement types (Fischer and Hoellen, 2004). For example, pointing movements particularly facilitate locational metric processing (Deubel and Schneider, 2004; Collins et al., 2008), whereas grasping facilitates metric size processing (Symes et al., 2008) and orientation processing (Lindemann and Bekkering, 2009; see Memelink and Hommel, 2012, for a review of motorvisual facilitation with different movement types).

Two recent works have directly compared motorvisual facilitation and impairment in one and the same study, and have confirmed that impairment is obtained with categorical R1-S2 overlap and facilitation can be observed with metric R1-S2 overlap. Koch (2009) found a motorvisual dual task facilitation effect in response times with a metric visual task (orientation judgment) and motorvisual interference with an analogous categorical visual task (object naming). Thomaschke et al. (2012b) found that left key presses impaired perception of symbols pointing to the left (categorical overlap), but facilitated stimulus perception in the left visual field (metric overlap).

Thus, evidence from motorvisual priming studies with metric R1-S2 overlap fully confirms the prediction of ideomotor theory. As metric representations are not involved in ideomotor cognition, these paradigms have not yielded motorvisual impairment effects (as would have been characteristic of perceptual effects from action selection), but have exclusively produced motorvisual facilitation effects.

At this point, two clarifications are needed to clearly distinguish between the processes related to action selection and those related to action control. One concerns feedback loop processing in motor cognition, and the other concerns categorical activation by relative location.

Closed loops in action planning. Note that some versions of the ideomotor theory also assume a kind of a feedback loop (see Nattkemper et al., 2010; Hughes et al., 2012; Waszak et al., 2012, for reviews). In particular, it is assumed that the action selection process is shaped by information about whether actions have successfully achieved their intended action-effect or not. Therefore perceptual representations of action-effects can be associated more precisely with appropriate generative actions, which, in turn, makes action selection more effective (Ziessler et al., 2004). These feedback loops do, however, relate rather to the acquisition of action-effect associations, instead of the actual online processing of perceptual representation in action-perception. Consequently, they are not likely to affect perception during action selection.

Categorical activation by relative location. Responses and stimuli always have metric representations of location, simply because stimuli and responses necessarily occupy a location in physical space. When metrical information is important for response control, processing of metrically compatible stimuli is facilitated (e.g., in the left visual field, for left pointing movements, see above). Depending on task context, however, the location of a stimulus can also activate a categorical, relational representation of its position, which is employed in action planning. When, for example, a stimulus frequently appears in different locations which are relative left/right to each other, it automatically activates categorical left/right representations, even when the stimulus location is task-irrelevant. Such effects have been observed in Simon effect (Proctor and Vu, 2006) studies, and in response-effect-compatibility studies (see introduction) with response compatible locations. Kunde (2001), for example has shown that horizontally arranged finger press responses to non-spatial imperative stimuli are facilitated more when the responses are followed by locationally compatible visual stimuli than when they are followed by incompatible stimuli (see above). In this study, response planning has activated categorical representations of response location, and categorical representations of effect location, which have been compatible in one condition and incompatible in the other. Consequently, in the former condition, response planning was facilitated. Note that the results from motorvisual facilitation studies reviewed here strongly suggest that (owing to the metric overlap and control relevance) effect perception was facilitated in the compatible condition relative to the incompatible one. This was, however, not measured in Kunde's study.

Motorvisual facilitation for human movement stimuli. There is accumulative behavioral and neuroscientific evidence that the rapid metric visuomotor feedback loops employed in action control also have a function in the perception and prediction of others' movements. Behavioral (Catmur and Heyes, 2011; Heyes, 2011; Martel et al., 2011; Boyer et al., 2012) and neuroscientific (Saygin et al., 2004) studies have shown that the observation of others' movements covertly activates the own motor system in a compatible way (see also the examples of ideomotor induction in section Introduction). The covert motor activation is likely to launch the same movement control processes as an active movement would have done (Wilson and Knoblich, 2005; Fagioli et al., 2007a). These control processes constantly produce predictions for the next immediately expected perceptual input caused by the movement, based on the current state of the motor system (Wolpert et al., 1995). For actually executed movements, such predictions have the function of detecting and correcting for mismatches between action goal and predicted course of the action (see above). Wilson and Knoblich (2005) have recently argued that these predictions are also employed in the observation of others' actions. They could serve to stabilize the ongoing percept by assisting perceptual disambiguation (Wilson and Knoblich, 2005). This visual function of motor control is reflected in motorvisual facilitation effects in dual tasks which apply biological motion displays (Miall et al., 2006). In particular, metric positional prediction of future visual movement states is facilitated when compatible movements are planned or executed (Graf et al., 2007; Springer et al., 2011, 2012; Saygin and Stadler, 2012; Stadler et al., 2012).

As this perceptual function of action is not dependent on action planning, however, and is thus not in the domain of ideomotor cognition, ideomotor theory would predict no motorvisual interference effects with biological stimuli. The data from motorvisual priming studies are in line with this prediction. The effects are mostly facilitative. Only when the temporal asynchrony between executed and observed movement is too extreme for predictions to be perceptually supportive have interferences been observed (see Christensen et al., 2011, for a review).

Conclusion

Evidence from motorvisual priming studies shows that ideomotor cognition is confined to categorical representations. Motorvisual priming has been shown for almost all kinds of representations. Effect direction, however, allows the motorvisual effect caused by ideomotor processes to be identified, because these processes typically lead to motorvisual impairment. Motorvisual impairment has only been observed with categorical stimuli. Motorvisual facilitation effects, on the other hand, have only been shown with metric representations and with biological stimuli. These effects are owed to motor control processes, and are, consequently, not in the domain of ideomotor theory.

ACTION PLANS BIND ACTIVE PERCEPTUAL REPRESENTATIONS DURING ACTION EXECUTION

Motorvisual priming paradigms are informative, not only about the nature of perceptual representation in action planning, but also about the way in which these representations are processed. The duration of motorvisual priming effects suggests that perceptual representations are bound in action plans to shield them from competing processes. Furthermore, the boundary conditions for motorvisual priming to occur suggest that action planning first activates perceptual representations before binding them. I discuss each issue in turn.

The duration of motorvisual interference

In early studies on motorvisual impairment priming, the effect was explained in terms of refractoriness of perceptual representation by action planning (Müsseler and Hommel, 1997a). These explanations assumed that perceptual representations are briefly activated during action selection, just at the point when they are employed to inform motor parameter choice in an ideomotor fashion. According to these explanations, the impaired availability of the action-effect representation for concurrent perceptual processes results from refractory inhibition of the representation following its brief ideomotor activation. Hence, the reduced availability of action-effect representations for other processes would have been only a byproduct of ideomotor cognition, without own functional value. This account of motorvisual impairment suggests a rather narrow time window for the effect, near the time of action execution (see Wühr and Müsseler, 2001, for a discussion).

Contrary to this prediction, however, in further investigations of motorvisual impairment, the effect has been observed during a relatively long time window, spanning from at least 2000 ms before action execution (Wühr and Müsseler, 2001, Exp. 2) until 1000 ms after action execution (Müsseler and Wühr, 2002; Stevanovski et al., 2002, Exp. 1; Oriet et al., 2003a,b; Wühr and Müsseler, 2002).

These findings have led to the interpretation of the motorvisual impairment effect as an indicator of something more essential in ideomotor cognition than a byproduct caused by refractoriness. Stoet and Hommel (1999) have suggested that action selection entails binding processes which connect all selection-relevant features of an action into a common event file (Hommel, 2004). Perceptual representations of action-effects are also features of an action and are, according to ideomotor theory, selection-relevant. Thus, these representations are also bound into event files. These binding mechanisms stabilize action plans through the course of their execution, and therefore shield the action plan against interferences from other cognitive processes, like, for example, other competing action plans. They can also prevent the same action being cyclically triggered again and again by the activated effect anticipations (Müsseler, 1999). Since the mid-2000s, a considerable amount of evidence has been accumulated in favor of event file binding in action planning (see, e.g., Colzato et al., 2006; Hommel, 2007; Mattson and Fournier, 2008; Wiediger and Fournier, 2008). Binding of features into action plans has also been referred to as "occupation" (Schubö et al., 2004) or "encapsulation" (Müsseler, 1999). Based on the prolonged time course of motorvisual interference, Wühr and Müsseler (2001) have concluded that motorvisual impairment is caused by the binding of perceptual event representations in compound representations of the action plan. This view has now become common sense in motorvisual interference research (Hommel, 2004; Nishimura and Yokosawa, 2010a; Thomaschke et al., 2012b).

The boundaries of binding

Stoet and Hommel (1999) suggest that action selection consists of two phases. First, the relevant action features are activated, and after activation they are quickly bound into a composite representation of the action. The binding of activated action features remains intact throughout the course of the movement. Motorvisual priming studies have supported this view with regard to perceptual representations of action-effects. Particularly informative are studies on situations where the second phase of Stoet and Hommel's model – the binding phase – had either not yet commenced, was already over, or was prevented by certain task demands. I will discuss each of these issues in turn.

Motorvisual priming before binding. According to Stoet and Hommel, action features are first activated and then bound. In order to investigate the transition between both phases directly one would have to measure motorvisual priming effects at the point when features are activated but not yet bound. In the majority of motorvisual interference studies, this condition is, however, not met. Usually R1 is executed at leisure after S1, and S2 perception is measured shortly before, shortly after, or during execution of R1 (e.g., Müsseler et al., 2000, 2001; Eder and Klauer, 2007; Nishimura and Yokosawa, 2010a). Under such a scenario, R1 features can be assumed to be long activated and bound when participants initiate the movement and S2 is presented.

A study by Müsseler and Wühr (2002) has, however, applied speeded R1 responses and has measured S2 perception almost immediately after S1. At this point it can be assumed that the S2 compatible perceptual representations are already activated by R1 selection but not yet bound. Müsseler and Wühr (2002, Exp. 2) applied a relatively difficult speeded four-choice task with intervals of 200, 400, or 1000 ms between S1 and S2. They observed the typical impairment effect at 400 and 1000 ms, but a motorvisual facilitation effect was observed at 200 ms. Participants needed on average around 600 ms for their speeded responses to the cues. This indicates that S1-R1 translation was particularly difficult in this task and binding followed activation after more than 200 ms.

Motorvisual priming after binding. Other studies have investigated motorvisual priming *after* the binding phase. When S2 is presented at increasing time intervals after R1 execution, the motorvisual interference effect gets significantly weaker (Oriet et al., 2003b, Exp. 1, 2007; Wühr and Müsseler, 2001, Exp. 1). In some studies the priming effect turned into facilitation at the longest interval. For example, Müsseler et al., 2001, Exp. 2) had three timing conditions. R1 was to be executed immediately after a self-paced neutral double key press. S2 was presented at the double key press, at R1, or 500 ms after R1. The typical motorvisual impairment effect was found at the former conditions. When S2 was presented 500 ms after R1, however, a motorvisual facilitation effect was observed (see Schubö et al., 2004, for a similar pattern).

These results can be seen as further support for the two-phase view of action planning. After action execution, binding is not required any longer and consequently released, but activation in the action features, including perceptual representations of action-effects, still persists, and consequently causes motorvisual facilitation, when S2 is presented late after R1 (see also James and Gauthier, 2009, for a related discussion).

Motorvisual priming without binding. Another important source of information concerning the activation/binding view of action planning is motorvisual priming studies with movement tasks that counteract the binding process. A study by Caessens and Vandierendonck (2002) has been particularly illuminating in this respect. They applied a Stop-Signal paradigm, where participants had to execute speeded lateral key presses as R1 in response to visual S1. In half of the trials, a stop-signal appeared 200 ms after S1. In the latter case participants had to refrain from executing R1. After a variable SOA, a masked arrowhead was presented as S2. In one experiment (Exp. 1A), the typical motorvisual impairment from R1 planning on the perception of compatible S2 was observed. In a further experiment (Exp. 1B), however, Caessens and Vandierendonck increased the difficulty of the Stop-Signal procedure. Again, in half of the trials, a stop-signal was presented but the interval between S1 and the stop-signal was individually adapted by a staircase procedure such that participants were only able to refrain from responding in half of the Stop-Signal trials. Thus, binding of the response features into a composite representation in order to shield them from other processes would have been counterproductive here. In half of the trials this action plan would have had to be abandoned in favor of a new plan to inhibit the prepared action. Release of action features would have taken time, hindering quick inhibition. Under these experimental conditions, a motorvisual facilitation effect was observed, reflecting feature activation, but not binding.

This finding suggests that binding only takes place when stabilization of a chosen action is of advantage. In situations with high action uncertainty, where action plans need to be quickly abandoned and rapidly replanned very often, action features are activated by ideomotor processes, but not bound.

Conclusion

Motorvisual priming studies have provided conclusive evidence about the processing of perceptual representations in action planning. When perceptual representations are employed to select actions in an ideomotor fashion, these representations are first activated, to the effect that compatible perceptual processes are facilitated. Then these representations are quickly bound, together with other action features, into a composite action representation, shielding them from involvement in other cognitive processes. The binding process is only abandoned in situations where one has to switch quickly between opposing action options.

METHODOLOGICAL CONSIDERATIONS

Despite the importance of motorvisual priming paradigms for investigating ideomotor processes, there is an inherent methodological difficulty in measuring such effects which requires careful consideration and control. Most behavioral cognitive psychology paradigms are visuomotor paradigms in a very general sense. The experimenter systematically manipulates the participant's perceptual stimulation as an independent variable and records the participant's responses. This basic logic of psychological experiments is designed to test hypotheses about causal effects from stimulus presentation on response production. Working in this intuitive way, stimulus manipulation and response measurement are thought to reveal regularities in mental processing from perception to action. Stimuli are perfectly controllable and directly affect perceptual processing, whereas responses are typically caused by internal mental processes. This experimental design appears intuitively feasible since it meets our everyday experiences with perceptions and actions. Perceptual stimulation is experienced as being largely caused by the environment. We usually have to change the environment (e.g., shifting objects into our visual field) to influence perceptual stimulation (yet, it has sometimes been argued that a scientific description of perception should not follow this intuition, e.g., Gibson, 1979; Noë, 2004; Bompas and O'Regan, 2006). Actions, on the contrary, are experienced as being produced or at least largely shaped by our own cognitive system. Motorvisual priming experiments have to reverse this highly intuitive causal direction (just as ideomotor theory does on a conceptual level). Such experiments aim at establishing a causal effect of response execution on stimulus perception. In order to do this, an experimenter would have to directly control the action intentions of the participants as an independent variable and directly measure the content or other features of their visual perception as a dependent variable. Both are practically impossible. Although one can induce involuntary movements by neural stimulation or by applying external forces to effectors, voluntary action planning (often of central interest in motorvisual research and constituting the central explanatory goal of ideomotor theory) cannot be directly physically controlled by the experimenter in a way comparable with stimulus manipulation in visuomotor experiments. Likewise visual perception is an event inside the participant's brain, which cannot directly be observed, and neuroscientific measurements are not precise enough to differentiate between perceptual states to a degree that could reasonably be assumed to be affected by action. Hence, motorvisual researchers have to apply indirect methods of response manipulation and indirect measures of visual perception. Both can lead to characteristic methodological problems, as will be discussed in turn.

In the remainder of this section, I discuss potential alternative non-motorvisual explanations for motorvisual priming studies arising from those methodological problems. I also show how these potential confounds have been dealt with in previous studies.

TRANSITIVITY OF STIMULUS SIMILARITY

The indirect manipulation of participants' action planning processes, as independent variable, is usually achieved by varying experimental instructions. In some paradigms, the instruction to prepare a certain type of action is blocked. In order to avoid learning effects, however, most motorvisual priming paradigms vary the response randomly from trial to trial. This is commonly done by displaying a response cue before each trial. The cue signals the response required in the current trial. In some trials the cued response is compatible with the observed visual stimulus, in others it is incompatible. A motorvisual interaction is detected by comparing visual performance for compatible and incompatible response-stimulus pairings. The compatibility relation between stimulus and response is usually a very natural one and is a salient feature of each (e.g., matching gestures, words, movement directions, or common spatial locations). The instructed mapping between cue and response, however, is also often a natural and intuitive one. This ensures that the cue response translation does not absorb too much cognitive capacity by requiring participants to memorize and apply complex rules, which could lead to a deficit in response correctness.

These requirements, to keep both the instructed cue response mapping and the evaluated response-stimulus compatibility relation simple and intuitive, makes it tempting to choose similar or even identical compatibility mappings for both. Doing so, however, leads to serious problems concerning the interpretation of a potential compatibility interaction, because in such situations compatibility between response and stimulus is always accompanied by compatibility between response cue and stimulus. When compatibility between cue and response and between response and stimulus are defined in the same terms, then any systematic compatibility effect of response-preparation on stimulus perception is indistinguishable from a compatibility effect of the cue on stimulus perception (see also Hommel and Müsseler, 2006, for a discussion of this issue).

Consequently, studies that apply analogous compatibility definitions for the cue response mappings and for response-stimulus matching cannot be regarded as unambiguous evidence of a motorvisual effect. Any compatibility effect could be owed to a causal response-preparation stimulus perception link as well as to a causal cue-perception stimulus perception link (the latter being a visuovisual interaction). The motorvisual priming literature has however suggested several strategies to control for this potential interpretation problem.

For example, Müsseler and Hommel (1997a, Exp. 1, 2), Müsseler and Hommel (1997b, Exp. 1, 2) used the same stimuli (arrow heads) for S1 cues and for S2 stimuli with identical cue response and response-stimulus compatibility definitions. The effect was also found, however, in motorvisual impairment experiments that applied more complex cue response mapping. Müsseler and Hommel cued the response with direction words instead of arrows (Müsseler and Hommel, 1997a, Exp. 4) and reversed the natural cue response mapping from the original experiment (Müsseler and Hommel, 1997a, Exp. 5), whereas Müsseler et al. used auditory cues (Müsseler et al., 2000, Exp. 1) and required the participants to generate responses endogenously in an alternating sequence (Exp. 2).

These findings show that one of the most extensively researched motorvisual priming paradigms (i.e., the priming of arrow perception by lateral key presses) cannot be explained by visuovisual effects.

TRANSITIVITY OF RESPONSE SIMILARITY

A comparable interpretation problem arises from the necessity to measure stimulus perception indirectly in motorvisual experiments. Perceptual performance is usually assessed by involving a secondary response in the design. The secondary response R2 is either a speeded detection/identification of the stimulus (e.g., Craighero et al., 2002; Pfister et al., 2012) or an unspeeded report of certain stimulus features (e.g., Müsseler and Hommel, 1997a) or a reproduction of the stimulus movement (Schubö et al., 2004). The speed or accuracy of R2 is a measure of the speed or accuracy of the perceptual process. As regards S1-R1 mapping, however, there are arguments for keeping the S2-R2 mapping relatively natural and intuitive. This is especially important for *speeded* secondary responses. A complex translation would be likely to require additional cognitive processing time and thereby add an additional source of variance to the response time, which would interfere with the statistical detection of any response-stimulus compatibility effects. Yet, when R1-S2 compatibility and S2-R2 compatibility are defined by the same mapping rules, the compatibilities cannot vary independently of each other. In such a situation a compatibility priming effect could not be assigned unambiguously to motorvisual priming since it would be indistinguishable from a primary-response secondary-response priming effect. Response–response priming effects have frequently been observed in dual tasks with compatibility relations between functionally unrelated responses (Schuch and Koch, 2004; Wenke and Frensch, 2005). This interpretability problem can also be controlled for, however. For example, Müsseler and Hommel (1997a, Exp. 1), Müsseler and Hommel (1997b, Exp. 1) used the same key pressing movements as primary and secondary response with the same compatibility definition but they also obtained a motorvisual interference effect when, in a control experiment, the secondary responses were verbal responses (direction words) instead of key presses (Müsseler and Hommel, 1997a, Exp. 2).

An analogous criticism applies to Schubö et al. (2001, 2004) motorvisual interference paradigm. The secondary response in their paradigm figures as primary-response in the subsequent trial. Thus, the compatibility mapping between response and stimulus is identical with the mapping between stimulus and secondary response. Schubö et al. (2004, Exp. 2) attempted to rule out a response secondary response explanation by including an additional motor task (drawing circles) between trials. They found comparable compatibility effects with and without such a task. According to their interpretation, the motor task would have interfered with, and thus eliminated, a response secondary response compatibility effect.

VISUOMOTOR EXPLANATIONS IN MOTORVISUAL PRIMING EXPERIMENTS

As reviewed in the introduction, visual processing can directly affect motor processing, evidenced by influences of task-irrelevant aspects of visual stimulation on motor action. When stimuli and responses are compatible, responses are faster and more accurate than with incompatible ones. Some of these visuomotor effects have been interpreted as evidence for the ideomotor theory. When the compatibility relation between stimulus and response is an action-effect relation – i.e., when response performance is better when responses are triggered by their typical perceptual effects than when they are triggered by non-effects – such findings can clearly be attributed to ideomotor processing, because they show that perceptual effect representations play a role in action selection.

There is, however, also plenty of evidence for visuomotor priming where the relation between stimulus and response is not one of effect but one of affordance. In such instances, the stimulus is not a typical effect of the action, but usually rather precedes the action in the sense of affording it. For example, the task-irrelevant side of a handle on a cup primes the ipsilateral response hand (Fischer and Dahl, 2007; Bub and Masson, 2010; Goslin et al., 2012). These kinds of visuomotor priming effects can also be explained by associative learning accounts (Heyes, 2001) instead of ideomotor theory, without assuming any perceptual processing in action selection. In some visuomotor priming studies it is fully apparent, whether the compatibility between stimulus and response rests on the stimulus typically being an external imperative cause of the response (affordance priming), or whether it rests on the stimulus typically being an external effect of the response (ideomotor priming).

For many other visuomotor studies, it is, however, unclear whether the relation between stimulus and response is one of affordance or one of effect. This has led to controversies about the appropriate interpretation of visuomotor effects with affordance/effect-ambiguous stimulus-response pairs.

For example, it has been debated whether visuomotor priming for biological motion stimuli, sometimes referred to as "imitation priming," is owed to associative learning (Heyes, 2001, 2003; Heyes and Ray, 2004; Bird and Heyes, 2005; Heyes et al., 2005; Wiggett et al., 2011) or to ideomotor principles (Brass et al., 2000; Stürmer et al., 2000), because in imitation a compatible stimulus can be an affordance cue from the perspective of the imitator *and* an effect from the perspective of the model (see, however, Leighton et al., 2010, for an integrative view). A similar interpretation ambiguity pertains for the Simon effect – a priming effect from irrelevant stimulus laterality on ipsilateral responses (Proctor and Vu, 2006). On the one hand, actions are often afforded by ipsilateral stimuli (Michaels and Stins, 1997), but, on the other hand, they equally often have ipsilateral effects (Greenwald and Shulman, 1973).

This issue is of particular importance for the interpretation of motorvisual priming paradigms, because for many types of S2 stimuli commonly applied in these paradigms, it is not apparent whether they are compatible with R1 in an affordance sense or in an effect sense. If, however, the designer of a motorvisual experiment with affordance/effect-ambiguous stimuli can make sure that the experiment really demonstrates an influence of action processing on perceptual processing, then this effect can definitely be ascribed to ideomotor processing, despite the ambiguity of the stimuli. The just described alternative non-ideomotor explanations for visuomotor priming with affordance/effect-ambiguous stimuli do not apply to motorvisual paradigms. These non-ideomotor accounts can easily explain why perceptions that usually trigger certain responses prime these responses, but they cannot explain why these responses should prime perceptions which usually trigger them. Thus, motorvisual paradigms are, for theoretical reasons, superior to visuomotor paradigms with regard to the investigation of ideomotor processing with rather ambiguous stimuli. This is an important advantage, because there are few stimuli which can be classified without doubt as effect, and not as affordance, of a response, unless they are associated with the response in a pre-experimental learning phase (as, e.g., in Cardoso-Leite et al., 2010; Pfister et al., 2012).

As mentioned above, however, this advantage is only realized when the experimental design of a motorvisual priming study does not allow an alternative visuomotor explanation. For some motorvisual priming studies this is not the case. When these studies apply affordance/effect-ambiguous stimuli, they cannot be definitively regarded as informative about ideomotor processing. This applies in particular to motorvisual single task paradigms and to concurrent motorvisual dual task paradigms. I will discuss each in turn.

Single tasks

For both affordance/effect-ambiguous stimulus classes discussed above (lateral stimuli and human movement stimuli), single tasks have been interpreted as evidence for motorvisual effects. Van der Lubbe and colleagues (Van der Lubbe and Abrahamse, 2011; Van der Lubbe et al., 2012), for example, have suggested a framework that explains the standard Simon effect in terms of motorvisual effects (see also Metzker and Dreisbach, 2009, 2011; Nishimura and Yokosawa, 2010b), and Stürmer et al. (2000) and Craighero et al. (2002) have interpreted imitation priming in terms of the ideomotor theory.

Craighero et al. (2002), for instance, primed stimulus perception by the preparation of compatible or incompatible grasping movements. The secondary response was the speeded execution of the previously prepared movement. They explained the effect as the effects of motor preparation on stimulus perception. The effect could also be interpreted, however, as an effect of stimulus perception on response execution, as Grosjean and Mordkoff (2001), Vogt et al. (2003) and Miall et al. (2006) have pointed out.

A strategy to avoid this interpretation ambiguity has been applied by Lindemann and Bekkering (2009). They investigated motorvisual effects by a series of single tasks, and protected the effects against visuomotor explanations with an additional motorvisual dual task.

Concurrent dual tasks

An alternative visuomotor explanation for motorvisual dual tasks is only possible when stimulus and response are cyclic, temporally extended, events (e.g., Hamilton et al., 2004; Schubö et al., 2004; Jacobs and Shiffrar, 2005; Miall et al., 2006; Zwickel et al., 2010b). From now on, I will refer to such tasks by the term *concurrent* motorvisual task.

Concurrent motorvisual priming effects are behaviorally indistinguishable from visuomotor effects. Several previous studies have shown that it is more difficult to perform compatible cyclic movements in synchrony with compatible stimulation than incompatible stimulation (Kilner et al., 2003; Bouquet et al., 2011; Capa et al., 2011; Press, 2011; Gowen and Poliakoff, 2012). This means that the difficulty of the motor task differs between compatible and incompatible trials in concurrent motorvisual priming studies. In compatible trials, the motor task is more difficult. Performing a more demanding task might lead to an *unspecific* impairment of general perceptual performance in incompatible trials. Unspecific means that the impairment is *per se* independent of the action's compatibility with the perceptual event, but would affect perception of any stimulus (see Müsseler and Wühr, 2002, for an analysis of specific and unspecific motorvisual interference). Unspecific motorvisual priming effects have often been demonstrated in dual tasks, where R1-S2 compatibility was either not manipulated or additive to unspecific impairment (Band et al., 2006; Johnston and McCann, 2006; Brisson and Jolicœur, 2007). Unspecific motorvisual impairment can, however, not be regarded as clear evidence for ideomotor processing. It can also be explained by limitations in either motor- or perceptually-related processes alone, such as transfer of information to visual short-term memory (Jolicœur and Dell'Acqua, 1998), or response selection (Pashler, 1994; Marois et al., 2006), owing to limited general processing capacities. Motorvisual evidence for the ideomotor theory requires that actions impair perception in a content-sensitive, compatibility-selective, manner, because only this shows that specific perceptual effect representations are processed in action planning.

The best strategy to ensure that a motorvisual priming effect can be explained by compatibility-specific motorvisual impairment, instead of a combination of compatibility-specific visuomotor impairment and unspecific motorvisual impairment, would be to have the S2 stimulus temporally follow the R1 response (e.g., Oriet et al., 2003b).

CONCLUSION

Although motorvisual priming studies are a powerful tool for investigating perceptual processing in motor cognition, they are sometimes susceptible to alternative explanations. This explanation ambiguity stems from the requirement to manipulate responses indirectly as independent variables, and to measure perceptual processes indirectly as the dependent variable. Alternative explanations can be excluded however, by using dual tasks, where response and stimulus do not temporally overlap, and where S1-R1 mapping is defined on another dimension as R1-S2 compatibility.

DIRECTIONS OF FUTURE RESEARCH

Although previous motorvisual priming studies have substantially extended our knowledge about ideomotor processing, many questions about perceptual processing in action planning are still unanswered, and there is enormous potential for future motorvisual priming research. In the following subsections I sketch some of the most urgent ideomotor issues that could be solved by motorvisual priming research.

THE FUNCTION OF BINDING

Motorvisual priming research has shown that perceptual features are bound into action plans, and are, consequently, not fully accessible to concurrent perceptual processes. The function of this binding process is, however, not clear yet. Some have suggested that binding of the perceptual effect representations keeps these representations from triggering the same action redundantly again and again by ideomotor mechanisms. In that case, execution would be blocked by a repetitive chain of triggering the same action (e.g., Müsseler, 1999). According to this account, the function of effect-binding would be the inhibition of outgoing activation from the perceptual effect representations toward other motor processes. Thus, the perceptual impairment would be merely a perceptual side-effect of inhibiting representations to shield them from actions.

Koch and Prinz (2002) suggested an account of effect-binding, which presents motorvisual impairment not as a side-effect but as the main function of binding. They say that "... the code subserving response execution is shielded against interference from visual input, which then leads to an impairment in perceiving compatible stimuli" (Koch and Prinz, 2002, p. 200). According to this view, R1 production is shielded against any interference from irrelevant visual information which might affect it. S2 is task-irrelevant for R1 production, but would be a potential ideomotor-trigger in R1-S2 compatible trials. Thus, shielding is particularly important in compatible trials and would produce the motorvisual impairment effect.

There is preliminary evidence for both accounts. The finding that binding can also affect compatible *responses* in dual tasks (e.g., Mattson and Fournier, 2008; Eder et al., 2012), rather supports the proposal that the function of binding is to avoid redundant repetitive response planning.

Support for the shielding account comes from studies on the modulation of shielding processes. According to Dreisbach (2012) the process of shielding responses against interference from irrelevant stimuli does depend strongly on the task set applied, that is, on how the response is cued. When it is a simple arbitrary S-R mapping, such shielding is virtually absent, whereas a constant (Dreisbach and Wenke, 2011) and rule-based (Dreisbach and Haider, 2008, 2009) mapping leads to substantial shielding effects.

This pattern is indeed also reflected in motorvisual impairment. Thomaschke et al. (2012b) and Wühr and Müsseler (2002) investigated the role of different S1-R1 mapping rules on a motorvisual impairment effect. Both studies compared motorvisual (R1-S2) impairment effects under compatible S1-R1 mapping rules with impairment effects under incompatible S1-R1 mapping rules. R1 were lateral key presses, and S2 were left/right pointing arrow heads. Both studies found the same pattern: when S1-R1 mapping was performed according to a simple, compatible mapping rule a substantial impairment effect was found, but the effect was absent when S1-R1 mapping required memorizing incompatible S1-R1 translations.

A definitive decision would require further research, in particular a more systematic investigation of the role of S1-R1 mapping rules in motorvisual priming.

S2 MODALITY IN MOTOR-PERCEPTUAL PRIMING

The motorvisual priming studies reviewed in this article were restricted to the visual domain. The ideomotor theory claims, however, that sensory effects in any modality can trigger actions. Previous action-perception studies have rarely applied other modalities (see Schütz-Bosbach and Prinz, 2007).

An interesting question for further research would be whether the temporal patterns found in motorvisual priming studies also obtain for motor-auditive or motor-tactile priming, or whether effect representations in different modalities are differentially involved in ideomotor cognition.

Another interesting issue is related to the interplay between different modalities. Research on multisensory interactions has shown that perceptual representations in one modality are tightly coupled to perceptions on other modalities when they frequently co-occur (Driver and Spence, 2000; Craig, 2006; Butz et al., 2010). Proponents of the ideomotor theory have often suggested that the perceptual representations involved in action selection are of a multisensory nature (e.g., Hommel, 2004).

This raises the question whether actions can also be *indirectly* triggered by, for example, auditory perceptual representation, when this representation is not a typical effect of the action but often co-occurs with its visual effects. This question could be answered by motor-perceptual priming. In particular, one would have to associate, in a learning phase, an action with an auditory effect that is compatible with a certain visual sensation, such as high-pitched tones with stimuli in the right visual field (see, e.g., Rusconi et al., 2006; Nishimura and Yokosawa, 2009; Eitan and Timmers, 2010). If this action, in a later dual task test phase, impaired perceptions in the other modality one could infer that ideomotor representations are multisensory.

R1 TYPE IN MOTORVISUAL PRIMING

The motorvisual priming studies reviewed in the present paper have been restricted to manual or verbal R1 responses, because these were predominant in the ideomotor-inspired literature on motorvisual effects.

A further well-researched motorvisual phenomenon is, however, the influence of eye movement planning on visual attention. Eye movements are very tightly coupled with vision, because they almost always have direct effects on visual input. It has long been known that the planning and execution of eye movements have a major impact on visual attention (Rizzolatti et al., 1987; Atabaki et al., 2009; Land and Tatler, 2009). The effects of eye movements on the perception of compatible stimuli can be facilitative (Shepherd et al., 1986; Sheliga et al., 1994; Hoffman and Subramaniam, 1995; Kowler et al., 1995; Smith et al., 2004), but also detrimental (Tibber et al., 2009). There is, however, already evidence from comparative studies that *eye* movements have qualitatively different effects on perception from *manual* movements (Fischer et al., 1999, 2003; Deubel and Schneider, 2003). It is an important task for future research to determine whether eye movements can be explained at least in part by ideomotor processes (see Herwig and Horstmann, 2011; Huestegge and Kreutzfeldt, 2012, for initial steps in this direction), and, if such an explanation is possible, why do ideomotor processes lead to different behavioral effects for eye and hand movements?

CONCLUSION

Previous research on ideomotor processing has shown that action planning binds perceptual representations into a stable compound representation of the action. It is, however, still unclear which cognitive function this binding fulfills. Other open questions are the degree to which ideomotor representations are multisensory, and which types of actions employ ideomotor processing. These issues can potentially be solved by future motorvisual priming studies.

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Hierarchy of idea-guided action and perception-guided movement

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The ideomotor theory of voluntary behavior assumes that the selection and control of a concrete goal-directed movement depends on imagining its direct perceptual consequences. However, this perception-guided assumption neglects the fact that behavioral control entails a hierarchical mechanism wherein conceptual expectations – action goals – can modulate lower level perceptuo-motor representations. In this paper, we focus on the hierarchical nature of voluntary behavior by distinguishing between perceptual representations of *images* that relate to attainment of concrete movement goals and conceptual representations of *ideas* that pertain to attainment of action goals. We review the dominant ideomotor principle of the direct perceptuo-motor relation and examine its limitation in the light of the proposed hierarchical view of voluntary behavior. Finally, we offer a revision of the ideomotor principle that allows extension of its explanatory domain from perception-guided movement to conceptual, idea-guided action.

Keywords: voluntary behavior, goal-hierarchy, ideomotor principle, idea-guided action

In his seminal work "The principles of psychology," William James popularized the notion that all voluntary behavior entails a fundamental principle of ideomotor action (James, 1890). The ideomotor principle states that selection and control of a particular movement depends on the anticipation of a sensory effect, which is normally experienced as its immediate product. This notion has been inherited by modern-day empirical psychology and has been translated into the experimental prediction that movement execution should be influenced by a perceptual image of the movement's effect. Proponents of the ideomotor principle have repeatedly employed stimulus-response paradigms to demonstrate that simple movements are directly linked to the perception of their effects. This work resulted in a plethora of reports in support of the prediction of "perception-guided" movement selection (Greenwald, 1970a; Chartrand and Bargh, 1999; Brass et al., 2000, 2001; Kunde, 2001; Kilner et al., 2003; Pfister et al., 2010), making the perception-guided ideomotor principle fundamental to various accounts of voluntary behavior (Greenwald, 1970b; Prinz, 1987; Hommel et al., 2001; Haggard, 2005; Kunde et al., 2007; Custers and Aarts, 2010).

In everyday goal-directed behavior, however, actors do not typically voluntarily decide which concrete movements to execute. Instead, decisions to move are contingent on the expectations regarding actor's higher-order, conceptual goal (Jacob and Jeannerod, 2005; Adolphs, 2009; Hauser and Wood, 2010). For example, guidance of everyday voluntary behavior, like getting in touch with a friend, entails both the expectation of the conceptual goal – choosing to make a phone call (or to send an email by using the laptop), as well as selection of concrete movement goals, for example grasping the phone (Searle, 1983; Mele, 1992; Grafton and de Hamilton, 2007; Kilner et al., 2007; Pacherie, 2008). Present versions of the ideomotor principle are not well suited to provide an understanding of these more complex real life behaviors. Understanding the mechanisms that underpin guidance of more complex behaviors requires explanation of the role of prior conceptual knowledge (Johnson-Frey, 2003; Jacob and Jeannerod, 2005), over and above the anticipation of movement's sensory consequences.

Whereas ample evidence supports "perception-guided" movement, at present few experiments have investigated the role of conceptual goals in selection and control of behavior. There are several reasons why this could be the case. First, the practical difficulties of empirically examining how individuals' conceptual goals influence their behavior are clear. Conceptual goals relate to actor's internal states that cannot be directly perceived by the observer's senses, but need to be inferred by recruiting their own conceptual knowledge to make sense of the observed behavior. Second, theories that are tailored to explain voluntary behavior often implicitly adopt versions of behaviorist (perceptuo-motor) principles, which inherently do not include the involvement of conceptual representations in explanations of guidance of voluntary behavior (Lashley, 1951; Jacob and Jeannerod, 2005). In this article we will review some new studies that investigated the role of higher-level conceptual action goals in selection and control of one's own behavior and in processing of other individuals' observed behavior. We conclude with a proposal for a revision of the ideomotor principle that allows extension of its explanatory domain from the perceptuo-motor level of perception-guided movement to the conceptual level of "idea-guided action."

PERCEPTION-GUIDED VOLUNTARY BEHAVIOR

Paradoxically, the term ideomotor action was first coined by the British physiologist William B. Carpenter (1852) to explain peculiar involuntary movements that are executed by individuals independent of their conscious action intention (for a historical review, see Stock and Stock, 2004). Later, the ideomotor action gained ground as the fundamental principle for the account of voluntary behavior (James, 1890). Carpenter, like James, was dedicated to demystifying the underlying neurocognitive mechanisms of movements, which can be observed in a variety of "psychic" phenomena (e.g., Ouija boards, moving tables, the divining rod, the magic pendulum). To provide a rational explanation for these peculiar phenomena, Carpenter and James proposed a principle of ideomotor action that assumes a direct link between perception and movement. Furthermore, in addition to an anticipation of the sensation of movement effects, ideomotor action is proposed to require a presence of a conceptual expectation (an idea) that a certain action will occur (Carpenter, 1852). In other words, hand movement over the Ouija board should be contingent to the idea (expectation) that some "psychic" force will generate movement. However, present day research on the ideomotor phenomena has mainly neglected the role of more abstract expectations in guidance of goal-directed movements.

More recently, selection and control of involuntary movements caused by the direct perception of similar movements of other individuals (i.e., mimicking) has received substantial interest from researchers in psychology (e.g., Prinz, 1997; Dijksterhuis and Bargh, 2001; Hommel et al., 2001). For example, Wolfgang Prinz and colleagues (Prinz, 1987, 1997; Hommel et al., 2001) proposed that perception, planning, and control of movements share a common representational domain. To find support for common representations of perception and movement, participants executed finger-movements in response to an arbitrary number while observing task-irrelevant images of movements, similar or dissimilar in terms of the movement direction (e.g., Brass et al., 2000; Brass et al., 2001). The findings showed that movements that were similar to the perceived image were executed faster, compared to the dissimilar ones; providing evidence for a direct "perception-guided" movement.

In a similar vein, an account of social behavior put forward by social psychologists Dijksterhuis and Bargh (2001) introduced the elegant notion of the "perception – movement expressway" to explain that people tend to copy the observed behaviors of other individuals. The core notion put forth by the authors is that people have a natural tendency to imitate their conspecifics, which in some social settings need to be inhibited in order to carry out volitional action. For example, Chartrand and Bargh (1999) have demonstrated that individuals copy movements of the coactors with whom they interact, without the presence of a conscious action intention to do so. These authors instructed participants to rate photographs together with a confederate coactor who either repeatedly shook their foot or rubbed their face. The results showed that participants shook their foot more often while working with the confederate who shook their foot and rubbed their face more often when they perceived face rubbing. This notion that movement execution is automatically governed by perception of similar movement is in agreement with the ideomotor proposal of "perception-guided" movement.

The neurocognitive mechanism that was proposed to account for the reported social and cognitive psychology findings relies on a direct coupling/common representation of observed and executed movement (Prinz, 1997; Hommel et al., 2001; Hommel, 2009). More recently, a useful distinction has been made between the "weak" ideomotor principle that entails an intermediate step between sensory prediction and movement execution and "strong" ideomotor principle, which assume no cognitive intermediation (Shin et al., 2010). For example, some authors consider current ideomotor theory to be "weak" due to the apparent perception-action duality that is maintained (Richardson and Michaels, 2001). Notably, although ideomotor accounts of voluntary behavior emphasize the role of abstract ideas (i.e., concepts) that represent action expectation, the empirical work is mainly focused on the associative mechanisms that underpin perception-guided movement. The bias toward the perception-movement coupling in behavioral control has led to the formation of associative perception-guided theories of voluntary behavior. These theories have mainly neglected the role of abstract ideas (i.e., expectations) that are present prior to any association of perception and movement (Lashley, 1951) and have been criticized for a number of reasons. For example, the theories are deemed limited in their focus on arbitrary perceptuo-motor mappings (i.e., simple button presses to arbitrary stimuli – associating left perceptual feature with a left motor response). Therefore, ideomotor theories are still limited in their capability of explaining everyday object-related action, which necessitates conceptual knowledge regarding functional properties of the used objects (e.g., Johnson-Frey, 2003).

IDEA-GUIDED VOLUNTARY BEHAVIOR

Following reasoning derived mainly from introspection (Lotze, 1852; James, 1890), research on voluntary behavior in cognitive and social psychology focused on the mechanism of direct coupling between perception and movement (Greenwald, 1970b; Prinz, 1997; Dijksterhuis and Bargh, 2001; Hommel et al., 2001), stripped away from the higher, conceptual levels of action control (Lashley, 1951). In contrast, recent theoretical and computational work proposes that the control and planning of simple bodily goal-directed movements depends on prior conceptual expectations that are related to achievement of a particular outcome (Searle, 1983; Wolpert et al., 2003; Grafton and de Hamilton, 2007; Kilner et al., 2007; Pacherie, 2008). For example, it has recently been proposed that a multitiered model underpins selection and control of one's own behavior (Grafton and de Hamilton, 2007), as well as predicting and understanding the behavior of other individuals (Kilner et al., 2007). Grafton and de Hamilton (2007) proposed a hierarchy of control which includes: (1) the conceptual level of action intention, (2) the concrete movement goal level needed to realize the intention, (3) motor commands that activate the muscles to attain the movement goal, and (4) body kinematics that entail a synergy of different muscles to produce movements in time and space. Also, a recently proposed active inference account suggests that a hierarchy of predictions underpins both observation and execution of movement, without distinguishing between sensory and motor representations (Clark, in press; Friston et al., 2012).

This seemingly opposing nature of the perceptuo-motor (associative) and conceptual (hierarchical) approaches of action control has created tension and opacity throughout the fields of psychology and cognitive sciences. In order to relieve the tension created between the perceptuo-motor and conceptual views of voluntary behavior, the field needs to focus on the interplay between prior conceptual knowledge about the world and the perceptuo-motor associations that are formed by our experiences (Ochipa et al., 1992; Hodges et al., 2000; Wolpert et al., 2003; Pacherie, 2008; Adolphs, 2009). For example, a strong line of evidence from neuropsychology suggests that selection and control of goal-directed behavior entails involvement of a conceptual system that includes world knowledge about objects and their abstract properties and a perceptuo-motor production system that includes information regarding object manipulation (Roy and Square, 1985; Ochipa et al., 1992). Crucially, Ochipa et al. (1992) showed that a damaged conceptual system causes impairments related to abstract knowledge about objects, referred to as conceptual or ideational apraxia, whereas damage to the perceptuo-motor system leads to ideomotor apraxia – impairments of concrete movement production (Roy and Square, 1985).

Carpenter (1852) has already pointed out that perception-guided movement is contingent on the conceptual expectation that a particular movement will occur. It is important to note that Carpenter's suggestion that multiple levels might be involved in selection and control of behavior resonates with the hierarchical accounts of action control (Lashley, 1951; Searle, 1983; Roy and Square, 1985; Grafton and de Hamilton, 2007; Kilner et al., 2007; Pacherie, 2008). Nevertheless, most psychological experiments throughout the last decades, even those investigating the ideomotor action (e.g., Brass et al., 2000), have neglected this notion of apparent hierarchy. Various paradigms had a limited focus on the relation between the execution of simple movements and the perceptual images that they produce. Important to note is that even though many experiments try to avoid addressing the conceptual level of action control, it is inevitably present in the participants' explicit or implicit expectations during selection and guidance of their voluntary behavior (Clark, in press).

THE HIERARCHY FROM PERCEPTION-GUIDED MOVEMENT TO IDEA-GUIDED ACTION

A number of recent experimental paradigms were employed to investigate the role of conceptual expectations that pertain to abstract ideas in governing selection and control of goal-directed movements (Massen and Prinz, 2007a,b; Liepelt et al., 2008; Ondobaka et al., 2011). For example, in a study by Ondobaka et al. (2011) participants were facing a coactor while sitting at the table with an integrated touchscreen on which, on each trial, four playing cards appeared – one in each corner. First, the coactor selected the higher or lower card of the two cards that were revealed in front of him. This action then led to the immediate revelation of the participant's cards. Participants were instructed to either match or mismatch the coactor's conceptual goal (i.e., to select the higher or lower card). During the experiment, regardless of the task (matching or mismatching the conceptual goal), participants received trials in which their response led to matched or mismatched movements with the coactor. For example, in the "match conceptual goal" case, on some trials, the conceptual goal (e.g., the higher card) required the same movement to be carried out by both participants and coactor (e.g., reach to the left). In other trials, the same conceptual goal match may lead to a mismatch of movement goals (e.g., the coactor reaches right for the higher card and the participant must then reach left for the higher card). Findings indicated that participants' movement execution was solely influenced by the perception of the coactor's movement (i.e., left or right) when their conceptual goals also matched the coactor's, suggesting that a hierarchy of intentions governs goal-directed behavior. That is, conceptual goals apparently sit atop the perception-guided level in the hierarchical control of overt behavior.

Interestingly, a similar hierarchy between perception-guided and idea-guided action control has been investigated in tool-use (Massen and Prinz, 2007a,b, 2009). In a series of experiments, the authors employed a joint tool-use paradigm wherein two participants, in consecutive order, touched one of two targets. Participants could touch the target by moving a lever that could translate around one of two activated pivotal points, resulting in the opposite movement-to-target action rule. The setup allowed the authors to manipulate congruency between coactors' kinematics (moving toward or away from the body), concrete movement goals (target location) and abstract action goals (action rules indicating target-to-movement mapping) in order to test which goal level most strongly influenced observers' performance. The results suggest that observer's performance accuracy was significantly higher when abstract action goals (rules) matched, compared to condition in which coactors' action goals mismatched. Moreover, Massen and Prinz (2009) reported that only when the two coactors adopted the same action goal (idea-guided action), was an effect of match in kinematics and movement goals (perception-guided movement) observed. In contrast, when their action goals mismatched, no perception-guided movement effect was observed. These results are in line with findings from the social card-selection study (Ondobaka et al., 2011) and indicate a guiding role of expectation regarding conceptual goals in the processing of concrete movement goals.

HIERARCHICAL IDEOMOTOR ACCOUNT OF VOLUNTARY BEHAVIOR

Results from these studies are in congruence with Carpenter's original proposal of the ideomotor action principle, which goes beyond perception-guided movement and stresses the importance of conceptual action expectation in the guidance of voluntary behavior. Recent findings show that in the social contexts wherein abstract ideas govern voluntary behavior, perception-guided effects are present only if coactors' ideas match (Massen and Prinz, 2009; Ondobaka et al., 2011). Likewise, findings are in agreement with the assumption that the selection and control of one's own voluntary goal-directed behavior entails a hierarchical mechanism wherein expectation of conceptual goals can modulate concrete bodily movements in space and time (**Figure 1**). For example, engaging in a goal-directed behavior like getting in touch with a friend entails idea-guided conceptual goal to make a phone call. Subsequently, selection and control of concrete image-guided movements involved in grasping the phone transporting it to the ear must depend on the expectation that the phone will be used in the first place (and not the laptop) and results in the inference

FIGURE 1 | Illustration of a hierarchical ideomotor principle of action in execution and observation. The actor generates action expectations (i.e., ideas) at the abstract conceptual level (action goal to make a phone call) as well as at the movement level (movement goal to transport the phone to the ear) and infers these by evaluating the sensory inputs. The observer needs to infer both aspects as well. First, is the actor going to make a phone call? This conceptual goal inference depends on recruiting the conceptual knowledge regarding the function of the object. Second, is the actor bringing the phone to the ear? This movement goal inference depends on the concrete perceptuo-motor associations that link phone to the ear.

of one's own behavior as getting in touch with a friend. Similarly, understanding whether the observed actor is getting in touch with a friend requires a parallel inference of both conceptual and movement goals (Kilner et al., 2007) by relying on the same hierarchical mechanism that is used for action execution in the observer (**Figure 1**).

The current proposal asks for an extension of the dominant approaches of voluntary behavior (Greenwald, 1970b; Hommel et al., 2001; Heyes, 2011), which mainly focus on the direct perception-movement links that underpin the generation of perception-guided movement. Consequently, we suggest that dominant views on the nature of perception-action coupling (Greenwald, 1970b; Hommel et al., 2001; Heyes, 2011) should be extended to allow the influence of idea-guided action on top of perception-guided movement (Lashley, 1951; Oztop et al., 2005; Grafton and de Hamilton, 2007). Following the active inference account (Clark, in press), the current proposal can be viewed as a strong version of the ideomotor account – a version that does not necessitate any intermediate cognitive steps in order to translate perceived input into movement. Our hierarchical ideomotor framework states that bodily movement fulfills conceptually guided proprioceptive and visual expectations, without the necessity of an intermediate cognitive process. However, the proposed action hierarchy implies that an antecedent state of expectation does play a fundamental role in shaping perception and action. Crucially, the addition of the conceptual-perceptual (i.e., idea-image) hierarchy and the extension of the anticipatory ideomotor mechanism to the conceptual level leads to significant theoretical advances. First, the incorporation of the conceptual level (Johnson-Frey, 2003) allows the current account to explain everyday object-related action. Second, the proposal maintains the indistinguishable nature of sensory and motor representations, but allows prior expectations to play a modulatory role in the anticipation of sensory consequences that are directly related to movement execution.

Collectively, we summarized recent studies that demonstrate the fundamental role of idea-guided behavior (Massen and Prinz, 2007a,b; Ondobaka et al., 2011) and proposed an extension of the ideomotor principle's explanatory domain from perception-guided movement to conceptual, idea-guided action. The proactive and hierarchical nature of the extensions accommodate the pivotal role of prior conceptual expectation (i.e., ideas) in providing a scaffold for direct perceptuo-motor coupling, thus maintaining the "strong" ideomotor character. At the same time, the proposal is in accordance with the origins of the ideomotor principle (Carpenter, 1852), in which a fundamental role for prior expectations in voluntary movement is already suggested. Adopting the hierarchical ideomotor view wherein action concepts and movement goals interact during selection and control of action could potentially unify the perceptuo-motor and conceptual frameworks of voluntary behavior.

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Too good to be true? Ideomotor theory from a computational perspective

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In recent years, Ideomotor Theory has regained widespread attention and sparked the development of a number of theories on goal-directed behavior and learning. However, there are two issues with previous studies' use of Ideomotor Theory. Although Ideomotor Theory is seen as very general, it is often studied in settings that are considerably more simplistic than most natural situations. Moreover, Ideomotor Theory's claim that effect anticipations *directly* trigger actions and that action-effect learning is based on the formation of direct action-effect associations is hard to address empirically. We address these points from a computational perspective. A simple computational model of Ideomotor Theory was tested in tasks with different degrees of complexity. The model evaluation showed that Ideomotor Theory is a computationally feasible approach for understanding efficient action-effect learning for goal-directed behavior if the following preconditions are met: (1) The range of potential actions and effects has to be restricted. (2) Effects have to follow actions within a short time window. (3) Actions have to be simple and may not require sequencing. The first two preconditions also limit human performance and thus support Ideomotor Theory. The last precondition can be circumvented by extending the model with more complex, indirect action generation processes. In conclusion, we suggest that Ideomotor Theory offers a comprehensive framework to understand action-effect learning. However, we also suggest that additional processes may mediate the conversion of effect anticipations into actions in many situations.

Keywords: ideomotor theory, associative learning, computational model, planning, consolidation

INTRODUCTION

Human beings are continuously confronted with change and novelty. Novel tools emerge, the environment changes, the social role of an individual changes, and the body grows and ages. Human beings can only deal with change and novelty because they can learn. *Ideomotor Theory* proposes a mechanism for learning to reach ones goals in novel situations. Ideomotor Theory is simple, old, elegant, and thus highly attractive (Herbart, 1825; Laycock, 1845; James, 1890; for a review of its history see Stock and Stock, 2004). It is a core element of many contemporary theories of goal-directed action (e.g., Hommel et al., 2001; Hoffmann, 2003) and has found considerable empirical support (e.g., Elsner and Hommel, 2001; Kunde et al., 2004, for a review see Shin et al., 2010). Its principles have also been picked up in other domains, such as social cognition (Paulus, 2012).

What follows is a brief summary of Ideomotor Theory. Whenever a movement is executed, the (mental representation of the) movement gets associated with (the mental representation of) its effects. This association between movement and effect is bidirectional. If the organism later wants to reach a goal state, the mere *anticipation* of this state may be sufficient to *directly* trigger the appropriate movement. This simple principle has been elaborated in more detailed theories of goal-directed action. For example, the theory of anticipatory behavior has put additional emphasis on the situation dependency of action-effect relationships (Hoffmann, 1993, 2003; Stock and Hoffmann, 2002). The Theory of Event Coding provides a sophisticated representational structure (Hommel et al., 2001).

Ideomotor Theory and many of its successors share three core assumptions. First, to trigger an action, the effects of the action are anticipated (effect anticipation). Second, this anticipatory image of action-effects directly activates an action by means of direct associations between actions and effects (direct-activation). Third, during learning these associations are acquired more or less independently of the actor's current intentions and possibly without the help of a teacher (associative learning rule). This formulation of Ideomotor Theory, especially the direct-activation claim, distinguishes it from other approaches and can be called the "strong" Ideomotor Theory (Shin et al., 2010).

The effect anticipation assumption is supported by a range of experiments. A common feature of these experiments is that they show that the selection, initiation, and control of an action is affected by the features of its effects. An example is an experiment in which the response-effect-compatibility was manipulated (Kunde, 2001). In each trial, participants were asked to press one of four horizontally arranged keys in response to a non-spatial color stimulus. Each key press was followed by an effect stimulus in one of four horizontally arranged positions on a screen. If the positions of the keys corresponded to the positions of their effects, responses were faster than when there was no such spatial response-effect compatibility. Similar effects have been reported for other kinds of actions and stimuli, including social behavior (Kunde et al., 2004, 2011).

Likewise, the direct-activation claim has found empirical support. For example, electrophysiological and neuroimaging studies have shown that the mere perception of stimuli that were used as action-effects in an acquisition phase activated motor areas (Elsner et al., 2002; Melcher et al., 2008; Paulus et al., 2012). However, it remains unclear whether this activation results from direct action-effect links, as suggested by Ideomotor Theory, or if the link is mediated by other, potentially automatic, processes. It also remains to be studied if such observations can be confirmed for action-effect learning in more complex tasks.

Finally, it is hard to test the associative learning rule claim empirically. Even though action-effect learning shares characteristics with associative learning (Elsner and Hommel, 2004), it is difficult to draw conclusions about the underlying learning mechanisms. To conclude, Ideomotor Theory offers an astonishingly simple and elegant mechanism to explain the acquisition and execution of goal-directed actions. However, although the theory found empirical support, it is surprising that the assumed mechanisms have barely been adopted in computational models or machine learning algorithms.

From a psychological point of view, it is suspicious that Ideomotor Theory has rarely found a way into computational models of human learning and goal-directed actions. For example, in the domain of motor learning and control, only a few computational models can be considered direct implementations of Ideomotor Theory (e.g., Herbort et al., 2005). Most approaches differ considerably (for reviews see Wolpert et al., 2001; Todorov, 2004; Butz et al., 2008).

From a functional point of view, it can be argued that Ideomotor Theory has mostly been studied in rather simple settings. In experiments the range of relevant actions and effects is constrained, the to be executed actions are usually simple, and the effects quickly follow actions. While these features are shared by some real-world learning tasks, many real-world situations have less clearly identifiable action and effect dimensions, require the execution of more complex actions, and provide delayed effects only. Thus, even though recent experiments progressed toward studying action-effect learning in more realistic settings (e.g., Paulus et al., 2012), it remains unclear to what extent Ideomotor Theory is applicable to more complex learning tasks. Doubt of the applicability of Ideomotor Theory in such situations is also raised by the fact that many machine learning techniques and artificial intelligence approaches have little in common with Ideomotor Theory.

The previous considerations show that Ideomotor Theory is a well-accepted framework. Nonetheless, there are reasons to question whether the theory fully lives up to its claims. Here, we adopt a computational perspective to put Ideomotor Theory to the test. To this aim, we cast Ideomotor Theory in a simple computational model that is based on the theories' basic claims. We then evaluate the performance of the model in a series of tasks to test if it reproduces empirical findings. Our goal is to test Ideomotor Theory with our model, rather than developing a model that strives to account all behavioral findings related to action-effect learning. Each task aims to capture the essence of a real-world challenge for any learning mechanism. During our exploration, we take two different perspectives. The psychological perspective considers whether Ideomotor Theory parallels human behavior, in both success and failure. The functional perspective considers which kinds of tasks can be mastered with Ideomotor Theory. This includes the question of whether it can account theoretically for learning to coordinate actions in tasks other than those previously studied in the lab.

Evidently, the brain relies on other learning mechanisms besides the one specified by Ideomotor Theory (Doya, 1999). Consequently, the failure or success of our model in specific tasks could be attributed to mechanisms other than the one proposed by Ideomotor Theory. Nevertheless, an isolated computational analysis of Ideomotor Theory will shed additional light on its validity. First, a computational analysis of various learning tasks allows us to test whether Ideomotor Theory specifies a basic learning mechanism that bootstraps the acquisition of goal-directed behavior. Second, even though other learning mechanisms may complement Ideomotor Theory, it is important to know how far one can go with Ideomotor Theory alone and under which conditions Ideomotor Theory fails or requires complementary mechanisms. Finally, a computational analysis of various learning tasks may help to identify potential challenges for Ideomotor Theory.

A COMPUTATIONAL MODEL OF STRONG IDEOMOTOR THEORY

In this section, we outline a simple computational model which strives to capture the core ideas of Ideomotor Theory without adding unnecessary features. **Figure 1A** depicts the general layout of the model. It is comprised of a simple, single-layered neural network containing two sets of nodes: action nodes and effect nodes ($A_1, A_2, \ldots, A_n$, and $E_1, E_2, \ldots, E_n$). For each action, there is one action node and for each effect, there is one effect node. If an action is executed or an effect is perceived, the respective nodes are active (i.e., activity is set to 1.0). If no action is executed or no effect is perceived, they are mute (activity is set to 0.0). The action-effect association w_{ij} between an action node A_i and an effect node E_j is strengthened when both nodes are active at the same time.

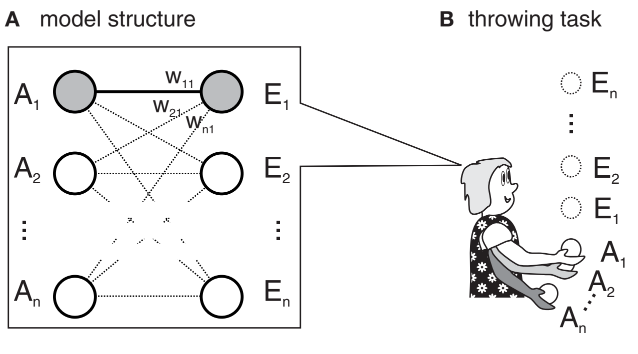

FIGURE 1 | (A) According to Ideomotor Theory, actions ($A_1, \ldots$) get associated to their effects ($E_1, \ldots$) when a person learns a novel task, such as throwing a ball. **(B)** In the example, each action node activates a throw of different strength and each effect node encodes the height of the resulting ball flight. If A_1 resulted in E_1 (gray circles), the link between both (w_{11}) is strengthened.

We apply this mechanism to a simple exemplar task: learning to throw a ball to particular heights. Each action node is associated to a throwing movement with a particular strength. In our example, activating A_1 causes a weak throw, activating A_2 causes a somewhat stronger throw, and so on. In this view, an action is defined as the production of a throw with specific strength, and throws of different strengths are considered different actions. Each effect node encodes a specific height. During learning, actions are randomly and individually executed[1]. Thus, there is only one active action node which gets associated to the activated effect nodes. In our model, the weight of the association between two active nodes is increased by one. To produce goal-directed behavior an effect node is activated and the activity is spread to the action nodes. If the goal is to produce the effect associated with the i-th effect node, each action node A_j is activated by the value w_{ji}. To select an action node, a winner-takes-all procedure is applied by selecting the action node with the highest activation. If there are several nodes with maximal activation, one of them is selected randomly. This formulation is fairly simple, but it captures Ideomotor Theory's three main assumptions: effect anticipation, direct-activation, and the associative learning rule.

MODEL FEATURES

Learning rule

Although we kept the model as simple and generic as possible, we want to explain some design decisions before proceeding. First, artificial neural networks are usually modeled with non-linear nodes (e.g., node activations are restricted to a range from 0.0 to 1.0 by a non-linear, sigmoidal input function) or include mechanisms to bind the associative strength between two nodes. Because we select the action node with the highest activation in a winner-takes-all mode, such algorithms would not affect the predictions of our model in the tasks we employ.

Situation and context

Obviously, Ideomotor Theory as it is formulated above is an oversimplification because it does not take into account that actions may have different effects in different situations. The model could be easily extended to encode action-effect associations situation-dependently. To keep our model simple, we do not account for the situation from the beginning but will introduce situation-dependencies later.

Representation

For the sake of the simplicity of our model, we consider only a single action dimension and a single stimulus dimension. Of course, it would be possible to integrate more than one stimulus dimension. Indeed, it has been suggested that a population-code like representational structure, as is employed in our model, is especially suited to allow the integration of multiple stimulus dimension (Ma et al., 2006). Moreover, in our model the representational structure does not change. Each node consistently encodes the same action or stimulus. Thus, the model does not implement any mechanisms for changing the receptive field of the present nodes or introducing new nodes. These simplifications are justified for three reasons. First, our tasks can be learned without adaptations of the representational structure. Second, adaptation is only possible once some skill is acquired in a given task. Because we also want to test the claim that Ideomotor Theory can bootstrap learning, we exclude such mechanisms. Third, no such processes are specified by Ideomotor Theory and we aim to provide a proof of principle of Ideomotor Theory. Nevertheless, future modeling might greatly benefit from integrating Ideomotor Theory with a richer, adaptive representational structure.

TASK

As a simple scenario for our model evaluation, we refer to the example of a child that is about to learn to throw a ball to various heights. In the example, actions are defined as throws of different strengths. The child can also perceive the position of the ball (**Figure 1B**). While we keep the task as simple as described here in the first test case, it is subsequently enriched. The task will be changed with respect to the action-effect mappings and the dynamics of actions and effects. However, some aspects of the task will stay constant. First, learning is always unsupervised. This means that the model receives neither reinforcement signals (such as "this action was good") nor corrective feedback (such as "next time better use action X") from an external teacher or from internal prior knowledge. This reflects the central claim of Ideomotor Theory that goal-directed actions can be acquired solely by observing the effects of own movements. Second, the same associative learning rule will be applied in all settings. Third, the representational structure will remain fairly constant, with the exception being that the number of action and effect nodes will be varied.

EVALUATION

The model can be tested by selecting a goal state and activating the associated effect node. The action then suggested by the model can be read out as described above. If the action produces the desired effect, it can be considered a success. To evaluate the performance of the model in various tasks, we generate a number of independent instances of the model and train them. At various time points during training we require each instance of the model to reach each possible goal state. If the model outcome is stochastic, each goal is presented repeatedly. As a measure of performance, we report the percentage of successful actions, averaged over all goals, repetitions, and model instances. Later, we distinguish between successful and optimal actions. Optimal actions are defined as successful actions that produce the goal in the most efficient way. When a model is tested, no novel action-effect associations are formed.

ROADMAP

In the following, we present five different scenarios in which we examine the performance of Ideomotor Theory in the face of different challenges imposed by many learning tasks. In Case 1, we show that the model is able to learn to control a task defined by a simple one-to-one mapping. Even if the number of actions or effects increases or noise is added, the model remains effective. In Case 2, we show that model performance degrades if multiple and potentially irrelevant actions can be executed in parallel. This

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[1] Actions are selected with equal probability and independent of action selections in previous learning episodes.

implies that Ideomotor Theory explains learning best in a task in which actions and effects are clearly defined. In Case 3, we show that the model is able to encode redundant action possibilities, which is a central problem in motor learning. In Case 4, we extend the model by allowing actions to trigger a chain of effects at various time points. This case shows that learning in our model depends critically on the close temporal proximity between action and effect. Finally, in Case 5, we examine the scenario that a sequence of actions is necessary to produce an effect. It is shown that Ideomotor Theory has difficulties in learning longer action sequences. It is suggested that this shortcoming can be overcome by introducing additional mechanisms which, however, go beyond some of the core assumptions of Ideomotor Theory.

MODEL EVALUATION

CASE 1: ONE-TO-ONE MAPPING BETWEEN ACTIONS AND EFFECTS

The simplest learning task is that of a one-to-one mapping between actions and effects. In this case, each action produces one specific effect and each effect is produced by one specific action. This case closely describes many experiments on Ideomotor Theory in which participants usually perform clearly defined actions (e.g., button presses) that are accompanied by clearly defined effects (tones, e.g., Elsner and Hommel, 2001). It seems obvious that Ideomotor Theory can account for learning when only few different actions and effects are involved. In many situations, however, a much greater number of actions and effects are possible, and as a result we tested our model with different numbers of actions and effects (2, 10, 50, 250). **Figure 2A** shows the results of 100 simulated runs for each number of actions and effects. If the number of possible actions and effects is low (e.g., 2 or 10), the model of Ideomotor Theory is able to produce different effects after very few trials. This corresponds to the results of Wolfensteller and Ruge (2011), who report action-effect learning after very few repetitions of different possible action-effect episodes. However, if the number of distinguishable actions and effects increases, learning takes longer but still results in a high success rate. The main reason that learning slows down with a growing number of actions is that for maximal performance, each action has to be executed at least once. In sum, Ideomotor Theory can successfully account for goal-directed behavior in one-to-one scenarios.

As a first step toward more realistic situations we wanted to test whether learning is robust to noise. To do this we ran the simulation with four actions and effects and added noise during learning. To compare the conditions, noise was switched off during testing. In the no noise condition, we did not include noise. In one condition, we set the initial action-effect association weights to Gaussian distributed random values ($m = 0.0$, $sd = 1.0$). In another condition, we added random Gaussian noise to each node in each learning episode ($m = 0.0$, $sd = 1.0$). This corresponds to a situation in which neither actions nor effects can be encoded noise-free by the neural apparatus. In a third condition, the selected action was replaced by one of the other actions in half of all learning episodes. This corresponds to a clumsy child with a very noisy motor system[2]. Finally, we combined all noise conditions. Each condition was simulated 1000 times. **Figure 2B** shows that even though noise slows down learning, the behavior is successful in the end. Comparing the simulation data with empirical results suggests that action-effect learning is subject to very little noise in common experimental setups (Wolfensteller and Ruge, 2011). This seems reasonable, as actions and stimuli are usually easily distinguishable in the lab.

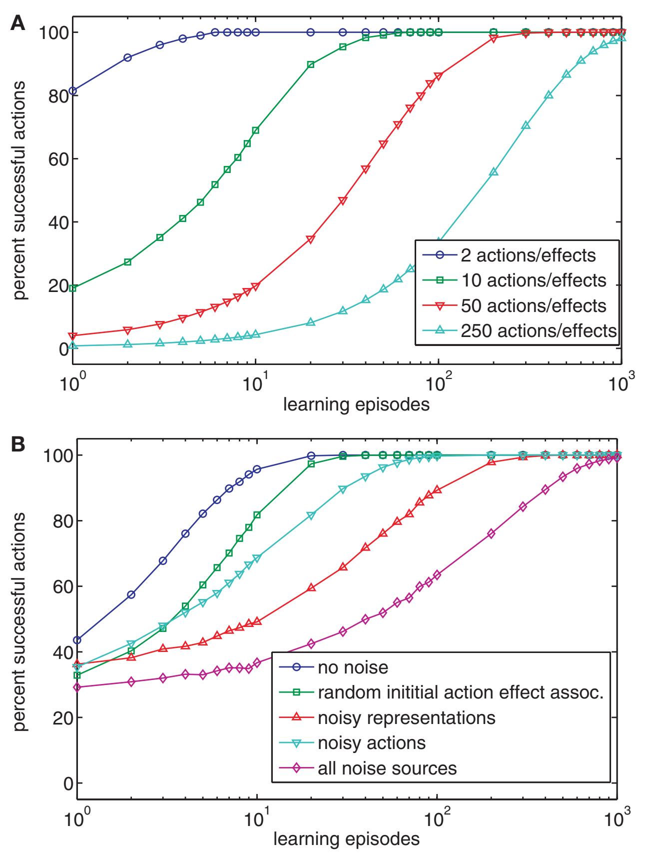

FIGURE 2 | (A) The chart shows the percentage of successful actions after various numbers of learning episodes for scenarios with different numbers of possible actions and effects. **(B)** The chart shows the impact of various kinds of noise on the performance of the model of Ideomotor Theory (using four actions/effects).

To conclude, the model accounts for action-effect learning in a simple task. If noise is low and the number of different actions and effects corresponds to the number used in experimental setups, the model requires about the same amount of training as humans do. When the number of potential actions and effects is high or when noise is present, learning is slower but still effective in the end.

CASE 2: ONE-TO-ONE MAPPING WITH IRRELEVANT ACTIONS

In the previous case, the child throwing the ball could only execute actions that were directly related to the task. However, while throwing, the child could have reoriented the head and the eyes,

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[2] In many real-world tasks this kind of noise can be controlled to some extent, e.g., by moving slowly (Fitts, 1954). Our exemplar task is too simple to offer the ability to model such strategic adjustments. However, it is conceivable that such strategic adjustments can be integrated in our model by including information about motor noise in effect representations.

tapped with a foot, swayed the body, or could have talked. Thus, besides actions that have an immediate impact on the effect, many other actions can be executed in parallel. Consequently, the effects of the novel task may get associated to other, irrelevant actions.

To test whether this poses a problem, we added 16 irrelevant action nodes to the four relevant nodes in our model. The activation of irrelevant action nodes did not yield any effects (at least in the effect nodes under consideration). Each of the irrelevant action nodes was activated randomly with a fixed probability during training. In addition, one actually relevant action node was activated in every learning episode. **Figure 3A** shows that learning slows down with increasing probability of irrelevant action nodes being active. Thus, even a moderate ratio of relevant to irrelevant actions could decrease the speed of learning by up to an order of magnitude. **Figure 3B** also shows that the ratio of relevant to irrelevant action nodes affects initial learning, even though a high performance level is reached after some time. In the analysis, irrelevant action nodes were activated with a probability of 0.25.

It seems reasonable to assume that in many situations and tasks, the ratio of task-relevant actions to task-irrelevant actions is much less favorable than assumed in our (noise-free) examples. Thus, on its own, Ideomotor Theory provides a rather slow and ineffective learning mechanism. We see three ways to deal with this limitation. First, it can be acknowledged that learning a novel task without a teacher takes time. We discuss this issue in more detail in the general discussion. Second, one could assume that an attentional mechanism constrains the range of possibly to-be-associated action and effect nodes. However, from a learning perspective, this assumption is problematic. It implies that a more fundamental learning mechanism than that proposed by Ideomotor Theory pre-structures the learning problem and that Ideomotor Theory is insufficient for bootstraping learning. Third, the sparse coding scheme results in a high number of different action and effect representations. Nodes with broad receptive fields might be employed to first home in on the relevant action and stimulus dimensions of a task. The resulting constrained space of task-relevant action-effect might then be subject to action-effect learning as described in our model. Indeed, it has been shown that executing actions primes stimulus dimensions that relate to this action (Fagioli et al., 2007), and that infants turn attention toward relevant stimulus dimensions when skills improve (Eppler, 1995). However, even if more sophisticated representational structures might facilitate learning in our model, it must be kept in mind that most tasks are also much more complex than our exemplar one.

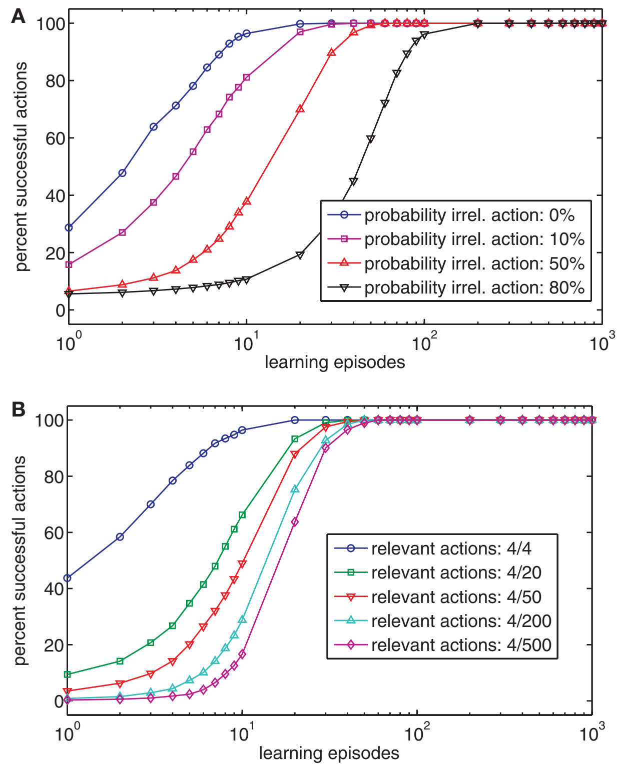

FIGURE 3 | (A) The chart shows the effect of the relative frequency of activating irrelevant action nodes on ideomotor learning. **(B)** The chart shows the effect of the ratio of relevant and irrelevant actions on ideomotor learning.

To conclude, Case 1 and Case 2 have shown that if the number of relevant actions and effect nodes is high and task-irrelevant actions can be executed during learning, the learning mechanism underlying Ideomotor Theory may be rather inefficient, even though it leads to an effective action selection in the end.

CASE 3: REDUNDANT ACTION POSSIBILITIES

Up until now we have considered cases with one-to-one mappings between actions and effects. However, most goals can be reached in numerous ways. To accommodate this, the ball-throwing example is modified. Consider that the child is now tossing a paper plane and not a ball. To make the paper plane fly as far as possible, just the right amount of force is needed. This means that some flying distances (effects) can be reached either with a strong or a mild throw. To include this into our model we extended the range of actions. For the milder throws (A_1–A_4), increasing throwing force result in increasing flying distances (E_1–E_4). However, for the stronger throws (A_5–A_7), increasing throwing force results in decreasing flying distance (E_3–E_1). Humans face similarly structured situations all the time. For example, a specific hand position in 3D space can be realized by an infinite number of arm postures. Likewise, most objects can be grasped in different ways. **Figure 4A** shows that two distinct action nodes get associated to each of the effects E_1–E_3 during learning. For example, it is encoded that E_1 can be realized by either executing A_1 or A_7. Thus, Ideomotor Theory is able to encode redundant action possibilities for each action.

This feature is not trivial, because many learning mechanisms (e.g., direct inverse modeling) can barely cope with similar problems (Jordan and Wolpert, 1999). The reason for this is that they cannot encode two or more distinct actions that result in the same effect. If several actions produce the same effect, these actions are blended into a single representation. Considering our example, a short flying distance would be associated with a mixture of weak and strong throws. Thus, a medium force throw would effectively be activated when striving for short flying distances, even if it effectively produces rather large flying distances. This problem is also referred to as the non-convexity problem (Jordan and Wolpert, 1999).

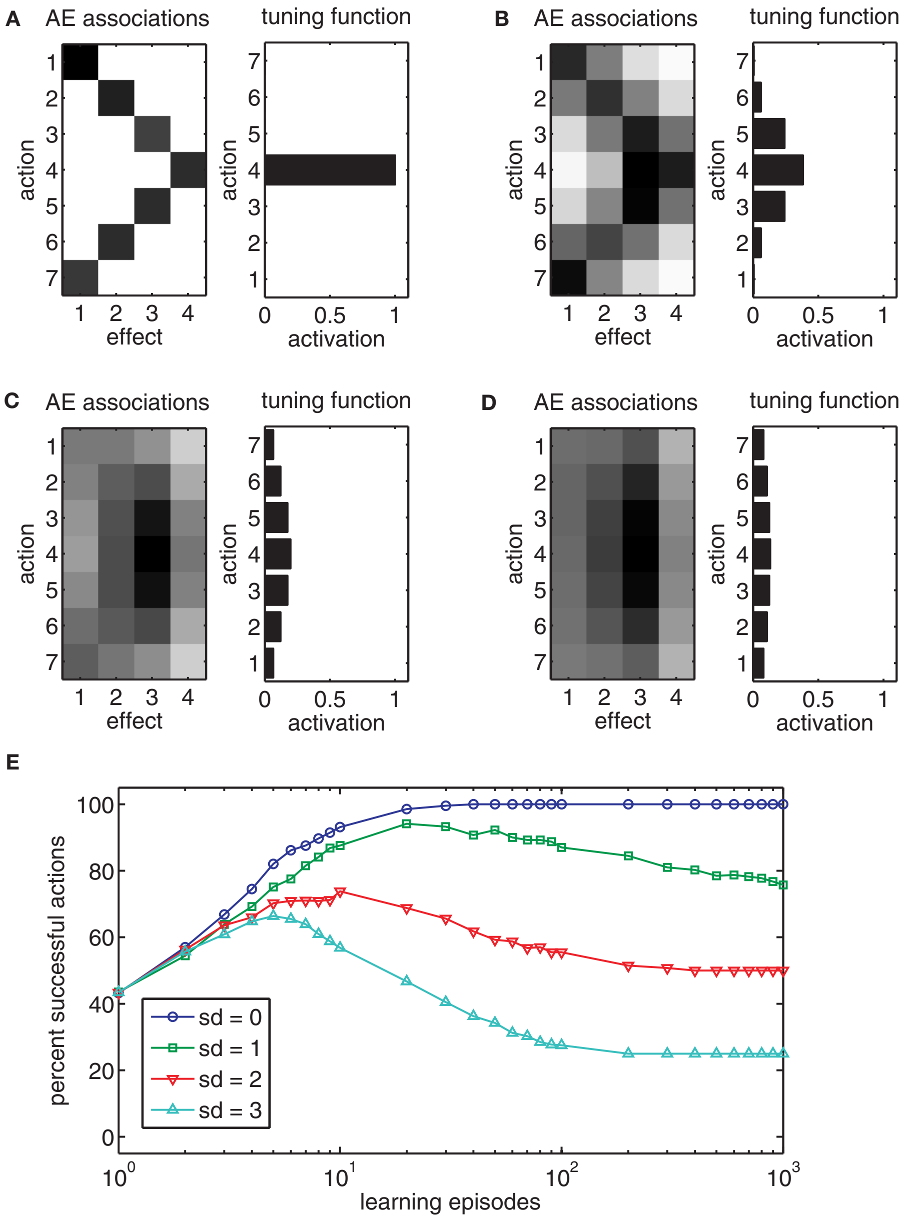

FIGURE 4 | (A–D) The left chart of each panel shows the action-effect (AE) associations after 1000 episodes (darker squares indicate stronger associations) with different Gaussian action turning functions (SD = 0.0, 1.0, 2.0, 3.0). The right chart of each panel shows the exemplar tuning functions for action 4. **(E)** The chart shows the effect of the action tuning function on the performance of the model.

Even though Ideomotor Theory is not subject to the non-convexity problem under ideal conditions, its performance may degrade under more realistic circumstances. In the example of **Figure 1A**, the action nodes were tuned very sharply to specific actions, resulting in the activation of a single node. This precise representation results in the likewise accurate representation of the action-effect structure of the task. However, in neural systems, nodes are frequently tuned much more broadly (Georgopoulos et al., 1983; Bastian et al., 2003). To implement this finding, an action is now encoded by all action nodes based on a Gaussian tuning function, where the i-th action activates each node A_j based on a Gaussian function with mean i[3]. Hence, when the i-th action is executed, not only is action node A_i active, but adjacent nodes are also active, albeit somewhat less so. To assess the effect of the breadth of the tuning function, we set its standard deviation to either 0, 1, 2, or 3. **Figures 4A–D** shows that the representation of redundant actions degrades with broader tuning curves. As a result, the model loses its ability to reproduce certain effects (**Figure 4E**). To conclude, Ideomotor Theory can be applied to

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[3] The activity of node A_j when executing the i-th action was defined as the integral of the Gaussian probability density function with mean i between $j - 0.5$ and $j + 0.5$.

some extent to redundant tasks if the tuning functions of actions nodes are sharp. Under more realistic conditions performance partially degrades.

CASE 4: DYNAMIC ACTION-EFFECTS

In the previous cases, the potential delay between actions and effects was not considered. However, timing is an important factor in action-effect learning (Elsner and Hommel, 2004; Haering and Kiesel, 2012). Moreover, in the real word, effects are not only delayed, but less clearly defined than in the lab. For example, throwing a ball results in the ball passing through a number of states on the way to the peak of the trajectory and then down again. Likewise, the activation of muscles causes the body to transition through a number of states.

To test whether Ideomotor Theory can explain learning in such tasks, we made our scenario more dynamic. Upon the execution of one of four actions, the ball moves up and down on a parabolic path. The trajectory of the ball was modeled so that the strongest throw propels the ball to the peak of its trajectory within 0.5 s. The ball then falls down again for 0.5 s[4]. Depending on the rate with which the child updates the position of the ball, each action causes a number of successive effects. To be able to associate an action to these effects, we include a trace conditioning mechanism in the model (Pavlov, 1927). Each action node remains active after action execution for a certain time interval. Thus, it can be associated to delayed action-effects. While such a mechanism seems to be a prerequisite for learning, the learning task gets considerably more difficult because the action-effect relation is less clear-cut than in the previous cases.

As a first step, we explored the sampling rate with which the effects are perceived. We used sampling rates of 2, 4, 10, and 100 Hz, meaning that the effect nodes are updated every 500, 250, 100, or 10 ms, respectively. Additionally, we included a condition in which the peak height of the ball was presented as a single effect. Under all conditions with dynamic effects, it was more difficult to learn to reproduce the different possible peak heights than under the single effect condition (**Figure 5A**).

These results suggest that a mechanism to single out the relevant event is crucial. In our example, this was difficult because the model was perceptually unable to distinguish between a ball on its way up, down, or at the peak of the trajectory. Indeed, from the perspective of the model, actions were mostly successful. As the model cannot perceive whether the ball is at its peak (e.g., has zero velocity) or not, it is sufficient – from the model's perspective – to make the ball pass through a specific height to reproduce the respective effect. Indeed, when considering this, the model is highly accurate. This was easy, however, because most actions reproduce several effects. For example, all actions are suitable to make the ball travel through the lowest position E_1.

Optimal actions

When many actions are suitable to reach a goal, one might ask which action should be selected. From a functional point of view, it is reasonable to select the most efficient action (Todorov, 2004). Because energetic costs and uncertainties are not included in our model, the most efficient or optimal action can be considered the action that produces an effect as quickly as possible. In our example, the optimal action is always the strongest throw (A_4), because this action propels the ball to all possible positions faster than any other action. However, when the model associates an action to everything that happens later, suboptimal actions are chosen in 75% of cases (**Figure 5B**, right black bar).

To improve efficiency, one could assume that actions are only associated to those effects that occur within a short time window after action execution. Experimental results suggest that this time window spans between 1 and 2 s (Elsner and Hommel, 2004). **Figure 5B** shows the percentage of successful and optimal actions for different time windows, using a sample rate of 10 Hz. For short time windows, successful actions are more frequently optimal than for longer time windows. Nevertheless, the model cannot always generate successful actions if the time window is short. The reason for this is illustrated in **Figures 5D–G**. The Figures show the strength of action-effect associations acquired with time windows of different lengths. If the time window is short (0.0–0.1 s, **Figure 5D**), the most effective action (A_4) is associated to E_1, but because the ball needs more than 0.1 s to move into the receptive field of nodes E_2–E_4, these effects never get associated to any action. If the time window is longer, all effect nodes get associated with action nodes. However, widening the time window removes the bias to associate effects with those actions that produce the effect quickly, yielding inefficient action choices.

To assess whether this trade-off can be avoided by a more sophisticated trace decay function, different decay functions were compared. **Figure 5C** shows the usage of different decay functions, which modulate the strength for temporally distant action-effect associations. Whereas an exponential decay function yielded the best result, a linear and an inverse proportional decay function were just as inefficient as a constant function.

To conclude, we applied a variety of sample rates, time windows in which actions and effects would be associated, and trace decay functions in the learning tasks with dynamic effects. Except for the shortest time windows, most goals could be reached but action selection was rather suboptimal. If the time window was short or an exponential decay function was applied, optimal actions were selected more frequently. Although this poses a functional limitation, it corresponds to human action-effect learning (Elsner and Hommel, 2004). Thus, from a psychological perspective, this property of the associative learning rule supports the model of Ideomotor Theory. Therefore, Ideomotor Theory is also supported as an account for action-effect learning.

CASE 5: STIMULUS DEPENDENCY AND SEQUENTIAL ACTIONS

In the previous cases the activation of a single action node resulted in some effects. However, many situations are more complex. Not

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[4] A ball thrown with the strongest action A_4 reached the peak of its trajectory after 0.5 s, about 1.2 m above the hand (gravity constant of $9.81\,\mathrm{ms}^{-2}$). The ball then accelerated down again. The peak height of throws with other actions A_i was proportional to the index i of the action: A_1, A_2, A_3 produced heights of 0.3, 0.6, and 0.9 m, respectively.

The four effect nodes covered the range from the height reached by the weakest throw A_1 to the strongest throw A_4 equidistantly. In each time step, the effect node encoding the height closest to the ball position was active.

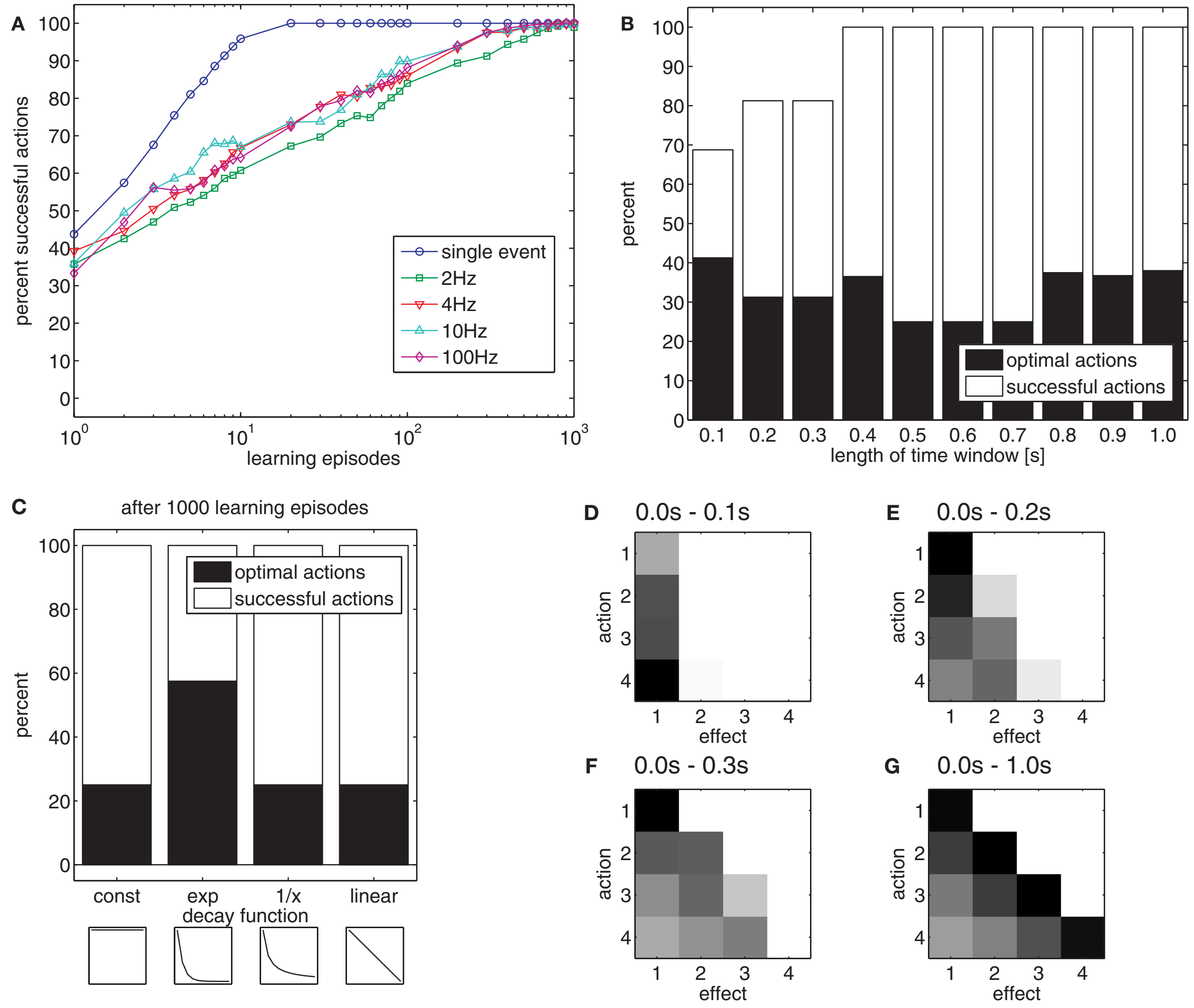

FIGURE 5 | (A) The chart shows the effect of the different sampling rates on learning if effects unfold dynamically in time. **(B)** The chart shows the effect of time windows of different length on the acquisition of an effective (white) and optimal (black) action-effect mapping. **(C)** The chart shows the effect of different trace decay functions. The insets show the decay functions between 0.0 and 1.0 s after action execution: const: constant value in time window 0.0–1.0 s; exp, exponential decay; 1/x, inverse proportional; linear, linear decrease. **(D–G)** The charts show the strengths of action-effect associations after learning of 1000 episodes with a time window of 0.1 s **(D)**, 0.2 s **(E)**, 0.3 s **(F)** and 1.0 s **(G)**. Black squares denote strong connections between actions and effects, white squares denote no connections.

only do action-effects depend on the current state of the body or the environment, but some effects may only be produced under conditions that need to be approached beforehand. For example, before a lifting movement of the arm causes the ball to fly, the ball needs to be grasped. In humans, even simple actions such as grasping a cup require the coordination of multiple movements (Herbort and Butz, 2011). Moreover, a concerted pattern of control signals needs to be sequenced to enable even simple arm movements (Gottlieb, 1996).

Direct and indirect (state-) action-effect associations

To test whether Ideomotor Theory is capable of sequencing actions, we altered our example in several ways. Consider for now that there are four actions, A_1–A_4. Each action moves the arm to a specific position. Action A_1 moves the arm to a low position, action A_2 to a higher position, and so on. We assume that the ball rests in the open hand. As long as the hand moves down or up slowly, the ball stays in the hand. In these cases, actions A_1 to A_4 result in effects E_1 to E_4, respectively. If the hand moves up quickly, the ball is thrown. The peak position of the ball trajectory depends on how far the hand has moved in the last step. If the hand starts from the lowest position, which is encoded as E_1, and A_3 is executed, the effect will be E_5. If A_4 is executed, E_6 will result. In this example, many effects can be reached by simply executing a single action. However, in some cases several actions need to be sequenced. For example, if the ball should be thrown as high as possible (E_6), one has to execute A_1, to move the arm and ball down, and then A_4, to propel the ball quickly upward. Likewise, if one has just generated E_1 and now wants to produce E_4, the arm needs to be brought up slowly by executing A_2, A_3, and then A_4 in succession. If action A_4 would be directly executed, the ball would be thrown in the air and E_4 would not be reached[5].

To be able to apply Ideomotor Theory to this example, it is necessary to make action-effect associations conditional on the current state. This conditionality is realized by adding a state layer. Kiesel and Hoffmann (2004) have provided empirical support for the state-conditionality of action-effect associations.

The state layer encodes the effect of the previous action and is otherwise functionally and structurally similar to the effect layer. State-action-effect associations are formed between active nodes of the three layers during learning, dependent on the state before the action, the action, and its effect. Unlike in the previous cases, a learning episode is now defined as a sequence of actions that lasts until the child produces a particular effect, which has been randomly determined before the episode. For goal-directed actions, the weights of those state-action-effect associations that match the current state and the desired effect are compared, and the action of the strongest state-action-effect association is executed.

When applying this model directly to the task, state-action-effect associations similar to those in **Figure 6A** are formed. An inspection of the chart reveals that several associations have been formed. However, some combinations of states and goals are not associated to any action nodes. For example, for goal E_6 no action is associated with the states 1, 2, and 3. This lack of associations is due to the fact that some goals just cannot be reached *directly* from some states, but only by sequencing several actions.

Hence, Ideomotor Theory needs to be extended in a way that enables such sequencing. To allow this, a trace conditioning procedure similar to that of Case 4 can be used. If each state-action pair is not only associated to its immediate, direct effects, but also, due to its trace, to subsequent, indirect effects – just as is done to enable learning in the previous case – a sufficient structure of state-action-effect associations might be built. **Figure 6B** shows the state-action-effect associations for a model in which the executed states and actions were associated to all subsequent effects, using an exponential trace decay function. For each state in which the ball is still in the hand (1–4) and for each goal, at least one action can be derived.

Trace decay

To test the importance of different ways to associate later effects with state-action pairs, different decay functions were evaluated. **Figure 6C** shows performance curves for different learning methods with respect to combinations of initial states and goals which require sequencing actions (average of 100 simulations for each condition). An exponential decay function yielded the best results (blue circles)[6]. In contrast, learning is considerably slower if no discounting function is used (green squares). If only direct state-action-effect associations are formed, performance is heavily impaired (red triangles) but still outperforms a baseline condition without any learning in which random actions were chosen (light blue triangles). The difference between the latter two conditions arises because there is some chance that random actions result in a state from which the goal can be directly reached in the direct association condition, but not in the baseline condition.

This example shows that Ideomotor Theory is theoretically able to account for action sequencing. However, the example of four different actions is fairly simple. To evaluate whether learning is still possible in a more complex scenario, we scaled the example up to 12 possible actions (100 simulation runs). **Figure 6D** (left bar) shows that the goal is reached in only about two of three cases, even after 10.000 learning episodes. Further analysis reveals that if a goal can be reached by a single action or very short sequences of actions, the model produces almost optimal behavior. However, if three or more actions need to be sequenced, behavior fails almost all of the time (**Figure 6E**, black bars).

Action selection during learning

This leads to the question of why it is so hard to generate longer action sequences. There are at least two potential reasons. First, it is possible that the structure of our model lacks the power to store the information that is necessary to sequence actions. Second, it is possible that the model is not able to extract the information from the training data. In the following, we argue that the latter aspect limits the performance of the model. In our example, learning is based on the random execution of actions. In the previous cases this did not pose any problem, because each action was more or less useful to generate some effects. This has changed in the current task – while some action sequences are useful to produce an effect, others are not. Moreover, the probability that a long, useful action sequence is produced by chance drops exponentially toward zero with growing sequence length. However, the model needs to experience a long useful action sequence at least once to be able to reproduce it.

Thus, one can now ask how critical performance depends on the actions that are executed during learning. To test this, we implemented three additional methods for action generation during learning and trained the 12 action node model for 10.000 episodes, using an exponential trace decay function (100 simulation runs for

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[5] The following relationship between situations, actions, and effects were used. Let n be the number of different actions, $e(t-1)$ the state before the execution of the action $a(t)$, $a(t)$ the action, $e(t)$ the effect after the execution of action $a(t)$, and t be a specific point in time. Action $a(t)$ was encoded by setting $A(t)_{a(t)}=1$ and all other nodes to $A(t)_i=0,\ i\neq a(t)$. Likewise, state $e(t)$ was encoded by setting $E(t)_{e(t)}=1$ and all other nodes to $E(t)_i=0,\ i\neq e(t)$.
To initialize an episode, time was set to $t=1$, $e(0)$ was either set to a random integer between 1 and n (learning) or to a specific value in that range (testing). In the case that $e(t-1)+1\geq a(t)$, the effect was $e(t)=a(t)$, i.e., ball and arm were moved together. Otherwise, the ball was thrown with $e(t)=n-1+a(t)-e(t-1)$ and the episode ended.
During learning, actions $a(t)$ were randomly set to a value between 1 and n. After each action, the state-action-effect weights m were updated, including a trace of past states and actions. For each time step u that has already passed in the episode ($u=1..t$), the following update rules were used: $m_{e(u-1),\,a(u),\,e(u)}=m_{e(u-1),\,a(u),\,e(u)}+f(t-u)$. If there was no memory trace, $f(x)$ was set to 1.0 when x was 0.0, and otherwise to zero. If the exponential function was used, $f(x)=e^{-x}$. If a flat function was used, $f(x)=1$.
During testing, actions were generated by the weights associated to the state, each potential action j, and the desired effect e^*, $a(t)_j=m_{e(t),j,e*}$. The action associated to the highest weight was then executed. If several actions had maximum weights, a random choice was made. An episode ended if the ball was thrown, if the desired effect was produced, or if more than twice as many steps elapsed as would be necessary to be able to reach either effect from either initial state. The evaluation was conducted 100 times for each initial state and each desired effect.

[6] State-action-effects weights at time t after action execution were increased by $e^{-0.2t}$.

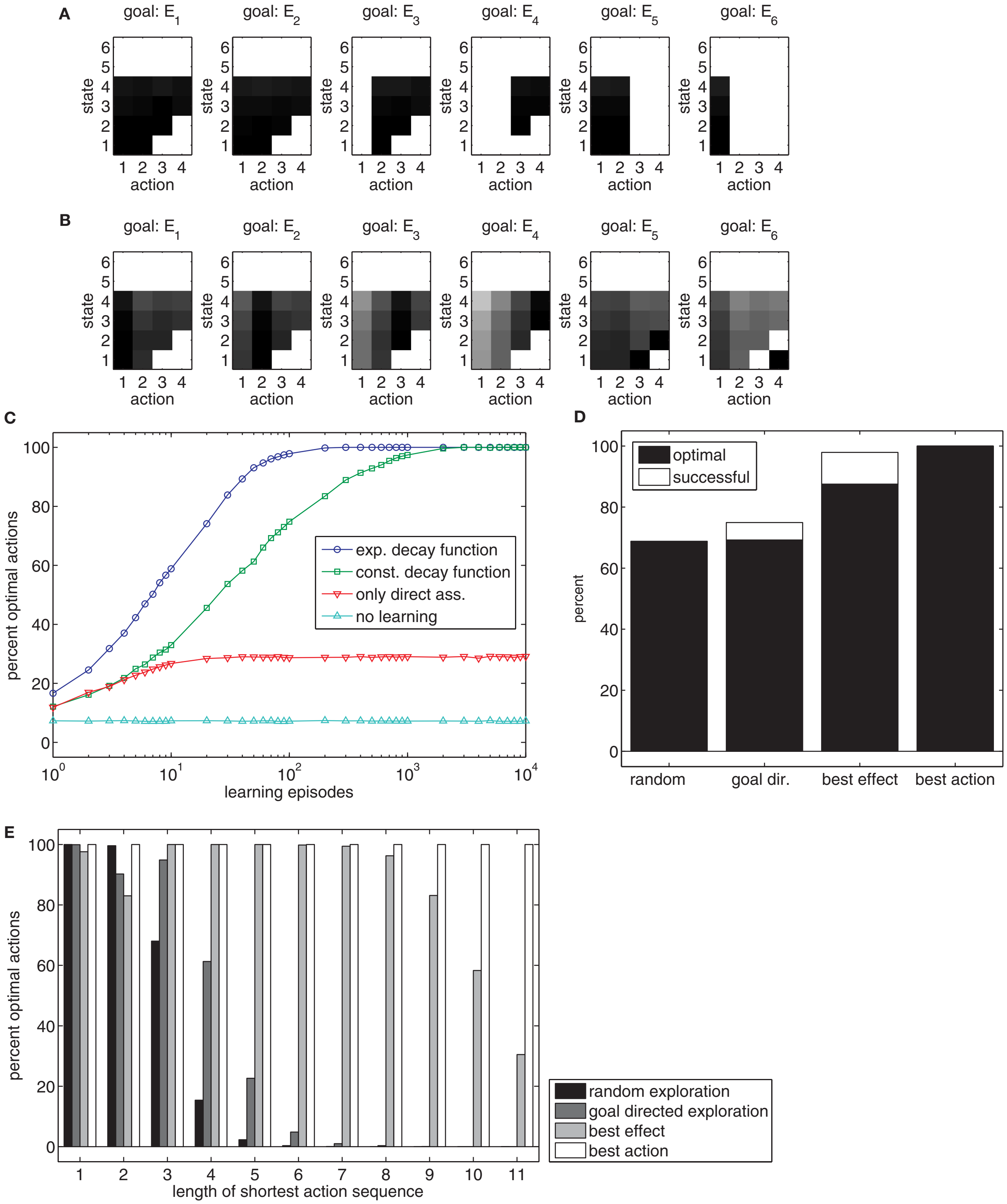

FIGURE 6 | (A) The chart shows the weights of state-action-effect associations after 10,000 learning episodes when acquired without a trace of past actions. Black squares denote large weights, white squares denote no associations. **(B)** The chart shows the weights' state-action-effect associations after 10,000 learning episodes when acquired with a trace of past actions. Please note, no actions are associated to states five and six because these are states in which the ball has already been thrown. **(C)** The chart shows how frequently goals are reached optimally (i.e., with a minimal actions sequence) with different trace decay functions. **(D)** The chart shows how efficient goals that require sequencing of at least two actions can be reached dependent on the learning method in a setup with 12 action nodes. **(E)** The chart shows how efficient goals can be reached, dependent on the minimal number of actions that need to be sequenced to reach the goal and dependent on the learning method.

each condition)[7]. These methods only affect the action choices; the learning mechanism for generating state-action-effect associations remains identical in all cases.

The *random exploration* method, which was used so-far, produced a new random action in each time step. Thus the child in the example just moves the arm to various positions, without trying to throw the ball in a specific way. As shown above, only mediocre results are achieved in this case, especially for longer action sequences (**Figures 6D,E**).

With the *goal-directed exploration* method, actions are generated by trying to reach an internally (randomly) determined goal (i.e., reach a specific height with the ball) based on already acquired state-action-effect associations. As long as the model does not know how to reach this ball-related goal it moves the arm to random positions. As soon as the model moves into a state that is associated with the goal, the action selection mode changes. In 50% of the cases, it approaches the goal of throwing the ball to a specific height directly, consequently facilitating the generation of useful long action sequences. In the other 50%, the arm is moved to a random position, as before, to be able to explore alternative action sequences. The value of 50% yielded the best performance in the current task in pilot simulations. This method corresponds to a situation in which the child tries to throw the ball to different heights completely on its own, and without any previous knowledge of the task. **Figures 6D,E** show that this action generation method is slightly superior to random exploration.

The *best effect* method assumes that the child knows through which sequence of states it has to travel in order to reach the goal. This corresponds to a situation where the sequence of states may have been shown to the child by a teacher. The teacher is able to tell the child to which position to move the arm next, but of course it cannot tell the child which action nodes to activate. In 50% of cases, the best effect method attempts to reach the next state in the sequence by executing the best action currently known for reaching that next state; otherwise, it activates a random action node. Again, the value of 50% yielded maximal performance in pilot simulations. This method can be considered to provide the maximum information that could be realistically obtained. Even though performance is high, it is unable to reach all goals. Moreover, actions are sequenced sub-optimally in more than 10% of the cases (**Figures 6D,E**).

Finally, the *best action* method is of rather theoretic interest. It randomly selects a goal and then produces the optimal action sequence to reach it. This allows us to test if the model is able to store longer action sequences, given that only optimal throwing movements serve as learning examples. **Figures 6D,E** show that this method results in perfect performances. This shows that the model is structurally able to store state-action-effect associations that enable perfect behavior. This implies that the model's performance is mostly limited by the necessarily suboptimal learning experience.

In conclusion, it seems reasonable to assume that natural behavior in learning a novel task is likely somewhat better than the goal-directed exploration method, but not as good as the best effect method. Thus, even a rather simple task with 12 different actions can only be partially mastered by the learning algorithm suggested by Ideomotor Theory.

Consolidation and planning

The previous section showed that the performance of our model of Ideomotor Theory depends critically on what actions are experienced during learning. Even if all the individual elements of an action sequence can be produced, they need to be executed in exact sequence during learning to be able to reproduce certain goals. Unfortunately, the probability that useful or even optimal action sequences are tried out during learning decreases exponentially with increasingly complex tasks.

While the basic state-action-effect triplets that constitute the elements of longer action chains can be easily acquired, learning entire sequences is difficult. This could be due to limitations in the information acquired during learning or the ineffective use of this information. To test the latter hypothesis, we tested whether reprocessing the acquired state-action-effect episodes could improve the performance of the model.

Two different modes of such reprocessing can be distinguished. First, the experiences of practice could be processed offline after learning. Indeed, it has been shown that performance in novel skills may increase after learning during times of rest (Brashers-Krug et al., 1996; Korman et al., 2007). This process is usually called consolidation.

Second, individual state-action-effect episodes may be sequenced before trying to reach specific goals, a process that might correspond to (motor) planning. In line with this reasoning is the finding that planning more complex or longer movements takes more time than planning simple movements (e.g., Rosenbaum et al., 1984; Munro et al., 2007). Note that if a consolidation mechanism, a planning mechanism, or both are necessary for successful learning this implies that associative learning alone is not sufficient and that effect anticipations do not always trigger actions directly.

For the sake of the example, we assume that the child practices 1000 ball throws a day. The consolidation mechanism of the model is invoked after daily practice and simulates another 1000 ball throws, based on the acquired state-action-effect links. This allows the formation of novel indirect state-action-effect links by learning from simulated action sequences that were not actually experienced.

The planning mechanism is invoked before generating actions in the test phase (but not for action generation in the acquisition phase). The planning mechanism systematically chains experienced state-action-effect links in order to reach remote goals. It is implemented by spreading activation repeatedly from the goal state or from states from which the goal can be reached to other states, thus creating new associations between actions and their indirect effects. This technique is known as dynamic programming (Bellman, 1957). Of course, both mechanisms could be realized in very different ways in the brain and we do not claim that our approach necessarily reflects these mechanisms in detail. The aim of the following simulations is to test whether consolidation or planning mechanisms can exploit the so-far learned state-action-effect associations more efficiently.

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[7] State-action-effects weights at time t after action execution were increased by $e^{-0.2t}$.

To evaluate the impact of consolidation and planning, we simulated the 12 action nodes ball-throwing example with an overall goal-directed training of 10.000 episodes. We used setups with and without planning or consolidation (100 simulation runs for each condition). Additionally, we simulated tasks with up to 96 nodes with and without consolidation and planning to see how these mechanisms affect performance in more complex tasks (20 simulation runs for each condition). During testing, each goal was pursued from each possible state 10 times.

Figure 7A shows that both planning and consolidation increase the percentage of goals reached. Whereas consolidation improves results only slightly, the planning mechanism yields a dramatic increase in performance. The success rate of about 90% after 10.000 trials of the models without planning is already surpassed after 200 trials if planning is employed. Whereas the overall success rate is little affected by the consolidation mechanism, **Figure 7B** shows that consolidation increases the percentage of optimally sequenced actions. This improvement is visible for models with and without planning. This suggests that planning and consolidation may play complementary roles. Whereas planning enables sequencing novel actions from acquired state-action-effect links, consolidation tends to improve the efficiency of these links.

Finally, we compare the pure Ideomotor Theory model against models that include consolidation and planning for up to 96 action nodes. **Figure 7C** shows that planning and consolidation enables the model to acquire much more complex tasks than would be possible without these mechanisms. **Figure 7D** charts model performance by the minimum length of action sequences required to reach a goal after 10.000 learning episodes. It can be seen that these additional mechanisms are pivotal to generate longer action sequences.

In sum, the planning mechanism especially enables effective sequencing of actions after comparatively little practice. Moreover, such a planning mechanism also allows adjusting action sequencing to situational constraints (Butz et al., 2007). For example, many throwing heights can be achieved from various initial positions of the hand. If some positions cannot be reached in a specific situation due to external obstacles or reduced mobility of the arm, for example, planning mechanisms might provide the flexibility to take such constraints into account.

To conclude, it seems plausible from a psychological and functional perspective that additional mechanisms play a crucial role for the acquisition and execution of goal-directed behavior. This implies that strong Ideomotor Theory and, in particular, that the claim that effect anticipations shall *directly* trigger actions, provides an insufficient account in more complex tasks.

DISCUSSION

In the previous section we developed a simple model of Ideomotor Theory. Following the example of a child learning to toss a ball, we extended our computational analysis beyond the learning challenges of common experiments on Ideomotor Theory. Given the simplicity of Ideomotor Theory, the model did rather well in many tasks. First, the model accounted for simple one-to-one mappings as used in many experimental setups. However, learning took considerably longer if many distinct actions and effects had to be considered, if the system was noisy, and if irrelevant actions possibilities shrouded the task-relevant actions. Nevertheless, the model was able to reach various goals after extended practice. Second, the model was capable of performing a task in which various actions or action sequences reached identical goals. Controlling such tasks is a non-trivial feature which cannot be accomplished by a range of learning mechanisms. However, this ability is impaired to some extent if action nodes have broad receptive fields. Third, the model was able to account for learning in dynamic environments if the delay between actions and their effects was small. A major challenge identified for Ideomotor Theory is the formation of links between actions and delayed effects. Likewise, while Ideomotor Theory can account for action sequencing in simpler tasks, the successful production of longer action sequences requires a rather lengthy acquisition phase. To conclude, our model suggests that Ideomotor Theory provides a good account for efficient unsupervised learning if (1) effects follow actions in close temporal proximity, (2) actions are simple movements that do not require intricate sequencing, and (3) the range of potentially relevant actions and effects is restricted. If these conditions are not met, implementations of Ideomotor Theory require extensive learning to approach reasonable performance.

Functionally, these preconditions could be said to limit the usefulness of Ideomotor Theory as a general mechanism for the acquisition of goal-directed behavior. However, these functional shortcomings support the model from a psychological perspective, because these limitations resemble those of humans in three ways. First, it has been shown that action-effect associations are only learned if both appear in a narrow time window (Elsner and Hommel, 2004). Second, skills in the "zone of proximal development," which refers to capabilities that are just a little more complex than those already possessed by a learner, can be readily acquired (Vygotsky, 1978). In contrast, human beings have difficulties in acquiring skills that go far beyond their current capabilities. In our model, this effect was mimicked in Case 5, when skill learning required sequencing actions. Whereas the model can easily learn novel skills that require the sequencing a small number of familiar actions, it is far more difficult to learn skills which require long action sequences. Third, action and stimulus dimensions need to be constrained for humans to acquire novel skills. This was also evident in our model. Learning benefited if a teacher provided information on the to be executed task and if the number of potential actions and effects was constrained (Cases 2 and 5). This also parallels learning in humans. When children learn new skills, parents, older children, or other persons often support learning (Rogoff, 1998) by, for example, guiding attention (Zukow-Goldring and Arbib, 2007). However, in some cases skill acquisition cannot be supported from the outside or is only supported to a limited degree. In these cases, skill acquisition requires a lot of time. For example, infant reaching movements converge toward an adult-like level only after about 2 years (Konczak and Dichgans, 1997).

Thus, in sum, it can be argued that the identified functional limitations of Ideomotor Theory resemble those of humans. To nevertheless enable learning, humans try to constrain the space of relevant actions and stimuli. If this cannot be achieved, this type of human learning is a time-consuming process.

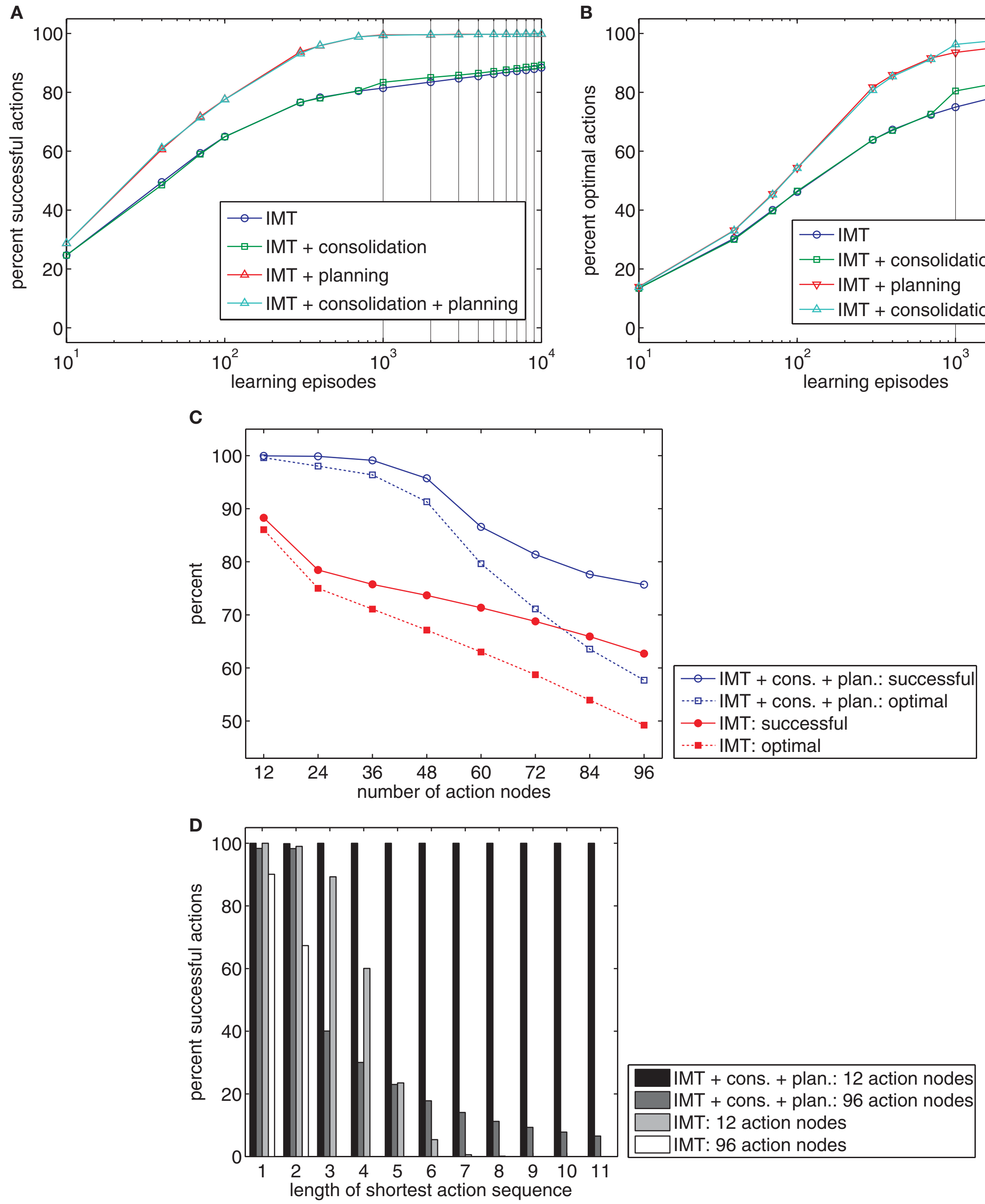

FIGURE 7 | (A,B) The charts show the percentage of goals reached **(A)** or reached in the most efficient way **(B)** after different numbers of learning episodes. Four models were tested: pure Ideomotor Theory (IMT), an extension of IMT with a consolidation mechanism, an extension of IMT with a planning mechanism, and an extension of IMT with both kinds of mechanisms. **(C)** The chart shows the percentage of successful and optimal actions for learning tasks with different numbers of action nodes for the pure IMT model or models with consolidation and planning. **(D)** The chart shows the percentage of successful actions by the shortest action sequence length required to reach a goal.

CHALLENGES FOR STRONG IDEOMOTOR THEORY

The various learning tasks could all be mastered to some extent. However, in some cases extensive learning experience was necessary to reach a high performance level. This suggests that the representational structure proposed by Ideomotor Theory, which was captured by our model, is adequate to control behavior. The major challenge stems from the difficulty to gather enough and good learning examples to fill the representational structure. While this was already evident from analyzing scenarios with an increasing number of action nodes, this problem was most dramatically exposed if a task required action sequencing. This reasoning is further supported by the fact that the exploration strategy during the acquisition phase had a very strong impact on performance. In the unrealistic case that perfect action sequences served as input for the ideomotor learning mechanism, the model learned to control actions optimally. However, if action selection during learning was less ideal, models of identical structure performed worse and were partially unable to sequence longer actions. Thus, even in the simple ball-throwing task in which the arm of the child could assume only 12 different postures, a random or goal-directed learning scheme results in mediocre performance, even after throwing the ball 10.000 times.

IMPLICATIONS

Our results have two implications that relate directly to Ideomotor Theory. First, they shed light on the potential role of intentionality. Second, they relate to the validity of the direct-activation claim.

Intentional actions

It has recently been debated if an intentional action mode is necessary to enable action-effect learning. It has been argued that freely chosen, intentional actions (but not reactions) on a stimulus are associated to their effects (Herwig and Waszak, 2009). However, action-effect learning in a forced choice acquisition phase has also been reported (Pfister et al., 2011). While intentional and stimulus-based actions may be functionally distinct (Waszak et al., 2005), our model is mute to the differences between these two modes. Nonetheless, our analysis hints at another facet of intentionality. In our example of sequencing actions, we contrasted a random and a goal-directed exploration method. The goal-directed exploration method mimicked the behavior of a person who moved the arm in order to reach the overarching goal of throwing the ball to a specific height. Such an overarching goal was not present in the random exploration method. Thus, the goal-directed exploration method is intentional whereas the random exploration method is unintentional. This suggests that, at least in tasks that require the execution of more complex actions, intentionality might affect the way actions are executed during early phases of learning. Whereas an intentional mode produces actions that are fairly well suited for ideomotor learning, purely random exploration is less efficient. Indeed, it has been shown that participants learning a novel sensorimotor task switch to successively more goal-directed action modes after having explored basic action-effect relationships (Sailer et al., 2005). However, it is questionable to what extent this reasoning can be applied to experimental tasks that frequently require minimal action coordination.

Planning

Strong Ideomotor Theory implies that effect representations directly trigger actions. This claim is central to strong Ideomotor Theory as it distinguishes it from many of its competitors (Shin et al., 2010). Our model suggests a more differentiated view on this topic. The analysis revealed that our model is able to account for learning short action sequences. However, when longer action sequences had to be generated, only a planning mechanism enabled effective goal-directed behavior. Moreover, computational models of motor planning have shown that such mechanisms provide a flexibility that cannot be accomplished by direct action-effect mappings (Kawato et al., 1990; Rosenbaum et al., 1995; Cruse, 2003; Butz et al., 2007; Herbort et al., 2010). Thus, our analysis suggests that the direct-activation claim may be justified if the effects can be realized by executing simple actions. However, if it is necessary to sequence longer chains of actions, indirect planning mechanisms, which mediate between goals and actions, seem to be employed.

COMPARISON OF IDEOMOTOR THEORY WITH MODELS OF MOTOR LEARNING AND CONTROL

The previous section has shown that action-effect learning and goal-directed behavior may not be as simple as predicated by Ideomotor Theory. Our critique focused mainly on Ideomotor Theory's claim that effect anticipations *directly* trigger actions. Setting this aside, the other claims of Ideomotor Theory seem generally feasible from a computational perspective. To illustrate this point, we want to discuss the relationship between Ideomotor Theory and (computational) models of motor learning. We focus on motor learning because motor learning can be considered to be one of the computationally most complex learning problems that human beings face. In the following we make two arguments. First, we show that Ideomotor Theory is an effective way to address learning without prior knowledge of sensorimotor contingencies. For this reason, we discuss other models only with respect to the issue of learning a novel task (for reviews discussing such models in more detail see: Todorov, 2004; Butz et al., 2007, 2008). Second, we argue that the mechanisms suggested by Ideomotor Theory need to be complemented with other approaches to account for human behavior.

Bootstraping action-effect learning

Several recent computational models of motor learning and control share the assumption that goals are represented in terms of sensory effects with Ideomotor Theory. Moreover, the acquisition of action-effect associations is central to these models (e.g., Kawato, 1999; Butz et al., 2007). However, note that the direction of the associations is emphasized. In the motor literature, the term "forward model" refers to a set of action-effect links. The term "inverse model" is usually used to describe a set of associations between effects and actions, which are the focus of the following discussion.

The most basic learning scheme to associate effects with the actions that cause them is *direct inverse modeling* (Kuperstein, 1988; Jordan and Wolpert, 1999). According to direct inverse modeling, one-to-one effect-action associations are extracted from random movements. Each time an action is executed and an effect is observed, direct inverse modeling updates the corresponding effect-action mapping. The updating is based on the difference between the action that has actually been executed and the action with which the acquired effect-action associations would have tried to realize the actual effect. Thus, direct inverse modeling seems closely related to ideomotor learning, as learning is possible without an external error signal. The key difference is that direct inverse modeling updates action-effect associations with a supervised learning mechanisms, such as the delta rule, whereas Ideomotor Theory suggests an updating according to an unsupervised Hebbian-like rule.

While this difference seems rather technical, it has considerable impact on the capabilities of the learning mechanisms. If multiple actions result in identical effects, direct inverse modeling may fail (Jordan and Rumelhart, 1992; Jordan and Wolpert, 1999). Furthermore, as direct inverse modeling associates each effect with a single action, it is impossible to associate an effect with potentially multiple traces of various previous actions. Hence, this scheme cannot be applied to tasks in which actions unfold in time. While this limitation can be circumvented to some degree by reformulating the learning problem (Bullock et al., 1993), the mechanism is considered to be rather ineffective (Jordan and Rumelhart, 1992; Jordan and Wolpert, 1999) in dynamic settings with redundant action possibilities.

More advanced learning mechanisms do not suffer from the limitations of direct inverse modeling (e.g., distal supervised learning, Jordan and Rumelhart, 1992; feedback error learning, Kawato and Gomi, 1992). However, such mechanisms require an external error signal (for a discussion see Butz et al., 2007). This implies that there is some additional knowledge source available that provides information on how to improve one's actions. Thus, these learning schemes may refine skills and improve performance but they cannot bootstrap action-effect learning. Moreover, these supervised learning schemes usually encode the single optimal action for each possible goal. This may be computationally efficient but may be disastrous if optimality criteria change. For example, an approximately straight movement path may be learned because it can be considered optimal for simple point-to-point movements (Flash and Hogan, 1985). However, straight movements are useless if there are obstacles in the way. Since other previously suboptimal actions are not encoded by the supervised learning schemes, alternative action sequences cannot be generated. In conclusion, supervised learning mechanisms have two limitations. First, action-effect learning in a novel situation is impossible. Second, behavior cannot be quickly adapted to changing task constraints.

Finally, Schmidt's (1975) Schema Theory is a prominent framework of motor skill learning. According to Schema Theory, motor skills are organized around schemata that map goals onto suitable actions and the sensory input that usually accompanies their execution. A key feature of these schemata is their ability to parameterize actions (e.g., the strength of a ball throw) with respect to a goal (e.g., the target height of the ball) and initial conditions. Thus, the Schema approach offers an account for how a single skill can be applied to different tasks, such as throws of different height. While this account is highly attractive, Schema Theory has been formulated on a rather structural level. The precise learning mechanism that enables the abstraction of schemata from individual experiences or the generation of new schemata has not been formulated (Schmidt, 1975, 2003). Thus, Schema Theory does not offer a learning mechanism itself but is built on the assumption that such a mechanism exists. Whether learning mechanisms similar to those suggested by Ideomotor Theory could offer an implementation for schema generation has yet to be evaluated.

Thus, given the failure of direct inverse modeling for action-effect learning in redundant or dynamic tasks, the failure to bootstrap action-effect learning with supervised learning schemes, and the general inflexibility of both approaches, one can ask whether Ideomotor Theory can make a contribution. We think the answer should be "yes." Our analysis has shown that ideomotor learning does not require an error signal or any prior information about the relationship between actions and effects. Moreover, ideomotor learning is able to cope with action redundancy, as has been shown by the paper plane example. Finally, ideomotor learning can handle situations in which actions and effects unfold in time. Thus, it seems that ideomotor learning could be a candidate to explain initial motor learning. Indeed, a simple computational model of Ideomotor Theory can account for learning to control a simple dynamic limb (Herbort et al., 2005). However, ideomotor learning can also lay the basis for more complex motor behavior. In our SURE_REACH model, we applied the principles of ideomotor learning to the control of a redundant arm (Butz et al., 2007; Herbort and Butz, 2007; Herbort et al., 2010). As in our last examples, the SURE_REACH model also deviates from pure Ideomotor Theory by including a planning mechanism. The model shows that ideomotor learning and a planning mechanism enables to explain highly adaptive behavior, such as the avoidance of obstacles, the reduction of the motion of injured joints, the integration of externally and internally defined constraints, and anticipatory adjustments of movements to subsequent actions. Thus, it can be concluded that the principles of ideomotor learning, as simple as they might be, can result in surprisingly adaptive and efficient behavior.

Complementary mechanisms

The previous section has shown that Ideomotor Theory, in contrast to many other theories, offers an effective unsupervised learning mechanism. In turn, future extensions of Ideomotor Theory could benefit considerably by adopting aspects of current models of motor learning and control.

Current models of motor learning and control distinguish clearly between predicting the consequence of an action (with forward models) and selecting an action to produce an effect (inverse models, Jordan and Rumelhart, 1992; Kawato and Gomi, 1992). This distinction is also made in Adams' (1971) closed loop theory and Schmidt's (1975) Schema Theory. The distinction is based on the findings that forward models are acquired faster than inverse models (Flanagan et al., 2003) and that both types of models assume different functions (Desmurget and Grafton, 2000). For example, in contrast to inverse models, which are primarily involved in control, forward models may help to cancel out noise, improve action selection by establishing an internal control loop, or even support inverse model learning (Karniel, 2002). In contrast to these considerations, Ideomotor Theorists describe the link between actions and effects as "bidirectional" (e.g., Elsner and Hommel, 2004; Shin et al., 2010), thereby neglecting potentially different mechanisms underlying mappings in different directions.

Moreover, in many models it is assumed that basic information is further refined during motor learning. One possible way to refine a motor skill is to use the output of the ideomotor model as a teaching signal for a secondary controller that improves performance and also mostly determines control signals (Kawato et al., 1987). In addition, basic acquired action-effect associations could be abstracted into schemata to form the basis of higher order motor skills (Schmidt, 1975). In this view, Ideomotor Theory could describe early processes of skill acquisition. It is likely to be complemented by other mechanisms later on.

Finally, the future extension of models of Ideomotor Theory should elaborate on mechanisms to model state- or context-dependent action-effect learning. In our Case 5 we introduced simple state-action-effect associations. However, it has been suggested that action-effect associations are only stored context-dependently if no contingent relationship between actions and effects can be otherwise established (Hoffmann, 2003). To accommodate such a process, it is likely that the representational structure of action nodes, effect nodes, and context nodes has to be adapted during learning. This, however, is out of the scope of our current model.

In conclusion, to model the acquisition of goal-directed behavior, Ideomotor Theory should be embraced as a core element. However, it needs to be integrated into larger frameworks to account for the control of actions, the refinement of movement skills, or abstraction processes.

SUMMARY AND CONCLUSION

Ideomotor Theory is a framework that explains action-effect learning without prior knowledge with an astonishingly simple mechanism. Whereas the claim that actions are triggered by the anticipation of desired effects has found considerable empirical support, the assumptions that actions are directly associated to their effects during learning and that effect anticipation *directly* trigger actions have been examined rarely and indirectly. Here we took a computational approach to evaluate whether these assumptions are theoretically suited to explain goal-directed action control and action-effect learning. We developed a simple computational model of Ideomotor Theory and subjected it to a number of different learning tasks. In general, the model operated successfully on a wide range of tasks. Similar to humans, the model had difficulties if the range of potentially relevant sensory and action nodes was very large. Also similar to humans, the model failed to associate actions with delayed effects. However, when learning tasks require sequencing motor commands, which is the case for even simple reaching or grasping movements, Ideomotor Theory failed. This limitation mainly arose due to the model's restriction that effect anticipations should directly trigger actions. When adding planning and possibly consolidation mechanisms and thus deviating from Ideomotor Theory's claim that effect anticipations directly trigger actions, effective goal-directed behavior was achieved even for tasks where a proper sequencing of motor commands needs to be learned.

To conclude, from a computational point of view Ideomotor Theory offers a surprisingly sound basis to understand the acquisition of goal-directed behavior. However, the assumption that effect anticipations directly trigger actions can only be upheld for learning tasks that require the learning of a mapping from sensory to motor space. If actions unfold in time, additional planning mechanisms are inevitable.

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