AMERICAN PSYCHOLOGICAL ASSOCIATION

Journal of Experimental Psychology: General

ISSN: 0096-3445

2020, Vol. 149, No. 9, 1778–1787
http://dx.doi.org/10.1037/xge0000748

When Actions Go Awry: Monitoring Partner Errors and Machine Malfunctions

Roland Pfister, Lisa Weller, and Wilfried Kunde
University of Würzburg

Human actions often aim at triggering certain responses from social interaction partners, but these responses do not always come as expected. Here we show that unexpected partner errors trigger sustained monitoring and that this monitoring exceeds the level that is observed if participants are faced with a machine malfunction rather than an error of an interaction partner (Experiment 1). Critically, this pattern of results emerged even though both types of errors were signaled by physically identical events in an oddball task, ruling out alternative explanations in terms of differential bottom-up factors. Unexpected delays in the action-effect sequence, however, did not trigger increased monitoring for social as compared to nonsocial situations (Experiment 2). These results indicate that mechanisms of performance monitoring might be recruited especially when facing the variability that is inherent in social interactions.

Keywords: sociomotor actions, social action effects, monitoring, post-oddball slowing

Everyday interactions are often initiated by one agent who seeks to evoke a certain response of another person—such as opening a window, handing over an object, or simply orienting to the first agent. Research on human action control indicates that such social effects of own actions become integrated into basic mechanisms subserving action control (Wolpert, Doya, & Kawato, 2003), and this theoretical notion takes a prominent spot in the sociomotor framework (Kunde, Weller, & Pfister, 2018). The sociomotor framework assumes that agents acquire bidirectional associations between own actions and the predictable consequences that these actions evoke at other people, so that own body movements can become represented, selected, and initiated in terms of the social consequences that these actions will likely produce (for corresponding empirical findings, see Flach, Press, Badets, & Heyes, 2010; Herwig & Horstmann, 2011; Kunde, Lozo, & Neumann, 2011; Müller, 2016; Pfister, Dignath, Hommel, & Kunde, 2013).

The sociomotor framework is derived from general effect-based, ideomotor theorizing (Hommel, 2009; Kunde, 2001), an area that has previously focused on body-related action effects (Pfister, 2019; Pfister, Janczyk, Gressmann, Fournier, & Kunde, 2014; Thébault, Michalland, Derozier, Chabrier, & Brouillet, 2018; Wirth, Pfister, Brandes, & Kunde, 2016) and on effects in the agent's inanimate environment (Elsner & Hommel, 2001; Gozli, Huffman, & Pratt, 2016; Pfister, Janczyk, Wirth, Dignath, & Kunde, 2014; Pfister, Kiesel, & Hoffmann, 2011; Wolfensteller & Ruge, 2011). Action effects in the social environment, however, come with several unique peculiarities as compared to nonsocial action effects, and their impact on effect-based action control remains to be studied empirically. These peculiarities include specific mechanisms for processing biological stimuli, such as faces (Farah, Wilson, Drain, & Tanaka, 1998) or biological motion (Thompson, Clarke, Stewart, & Puce, 2005), and unique effector systems that are specifically attuned to social interactions such as facial expressions (Crivelli & Fridlund, 2018; Neumann, Lozo, & Kunde, 2014). At the same time, however, social action effects also come with increased uncertainty as compared to action effects in the inanimate environment. Social partners may fail to perceive the initial request, may misunderstand the sender's intentions or he or she might plan to respond as intended but commit an error in the process. In other words, imperfect contingencies are the norm for social action effects.

In the present experiments, we tested whether agents respond differently to expectancy violations—that is, unexpected noncontingent responses (Exp. 1) and unexpectedly delayed responses (Exp. 2)—for social versus nonsocial action effects. To measure the impact of such expectancy violations, we capitalized on a reliable signature in performance, that is, post-oddball slowing (Houtman, Castellar, & Notebaert, 2012; Notebaert et al., 2009; Saunders & Jentzsch, 2012), and compared post-oddball slowing for expectancy violations regarding the responses of a social partner versus expectancy violations regarding the functioning of an inanimate machine.

Experiment 1: Errors and Malfunctions

Participants were assigned to one of two groups and worked either with a social partner (the social group) or operated a com-

---

Roland Pfister, Lisa Weller, and Wilfried Kunde, Department of Psychology III, University of Würzburg.

The work reported in this article was supported by a grant of the German Research Council (Deutsche Forschungsgemeinschaft; DFG) to Roland Pfister (PF 853/2-1) and Wilfried Kunde (KU 1964/14-1). Raw data and syntax files for reproducing all statistical analyses are publicly available on the Open Science Framework (https://osf.io/sd7a8/).

Roland Pfister and Lisa Weller contributed equally.

Correspondence concerning this article should be addressed to Roland Pfister or Lisa Weller, Department of Psychology III, University of Würzburg, Röntgenring 11, 97070 Würzburg, Germany. E-mail: roland.pfister@psychologie.uni-wuerzburg.de or lisa.weller@uni-wuerzburg.de

puter without any other individual involved (the nonsocial group). Participants in both groups were to trigger specific feedback from the social partner or the computer, and we inserted error feedback in a fraction of the trials. This feedback indicated an error of the social partner for the social group and a machine malfunction for the nonsocial group. Crucially, timing and frequencies of these errors were held constant in the social and the nonsocial group. To that end, participants in the social group were led to believe that they saw the feedback from a social partner, even though the feedback was in fact controlled by the computer. We expected post-oddball slowing following error feedback in both groups (cf. Saunders & Jentzsch, 2012). Crucially, we compared post-oddball slowing between groups, that is, post-oddball slowing after observing either partner errors or machine malfunctions. The previous literature houses two opposing predictions for this comparison. For one, human agents are accustomed to imperfect contingencies already in early ontogeny and expect other humans to respond noncontingently at times (Bigelow, 1998; Striano, Henning, & Stahl, 2005). Accordingly, research on automation bias has shown that participants expect machines to work more predictable than human interaction partners; machine malfunctions therefore represent salient events, which are detected and remembered better than errors of human agents (Dzindolet, Beck, Pierce, & Dawe, 2001; Dzindolet, Pierce, Beck, & Dawe, 2002; Manzey, Reichenbach, & Onnasch, 2012). These findings suggest that occasional noncontingent responses should not attract as much attention for partner errors as for machine malfunctions, predicting *smaller* and possibly even absent post-oddball slowing in the social group than in the nonsocial group. By contrast, the relatively high prevalence of noncontingent responses in social settings may call for enhanced monitoring by default in this case, suggesting *larger* post-oddball slowing in the social group than in the nonsocial group (Weller, Schwarz, Kunde, & Pfister, 2018).

Method

Participants. Participants were recruited in pairs. We initially recruited a sample size of 96 participants, to allow for a comparison of 48 participants (24 pairs) per group. However, 10 participants had to be excluded from the social group and four participants had to be excluded from the nonsocial group because they either did not believe the cover story or because they did not pay attention to the responses of their partner/the computer (see the Data Treatment section for details). To equalize the sample size in the both groups, six additional participants were recruited for the social group so that the final sample size for statistical analysis comprised 88 participants (mean age = 25.0 years, range: 18–65 years; 26 men, six left-handed). This sample size ensured a power of $1\text{-}\beta = .80$ to detect an effect size of $d_s = 0.60$ for the between-groups comparison and it further ensured a similar power to detect within-subjects effects of at least $d_z = 0.43$ in each group. Participants gave informed consent prior to the experiment and received course credit or monetary compensation.

Apparatus and stimuli. The two participants of a pair were invited to the same laboratory room which contained a cubicle and a workspace outside the cubicle. One participant was seated in the cubicle, the other participant sat outside (see Panel A of Figure 1). Each participant operated a standard German QWERTZ keyboard and observed a 17” monitor with a refresh rate of 75 Hz.

The door of the cubicle was left open at the beginning of the session, and participants could see and talk to each other while acquainting themselves with the setup. In the experiment, the door was closed so that participants could not see or hear what the other person was doing. Each participant operated four keys in a row of the keyboard with the index and middle finger of each hand. The relevant keys were marked with color labels (the keys R, T, U, and I for the participant outside the cubicle; C, V, N, and M for the participant inside the cubicle). All target stimuli appeared in white against a black background. Target stimuli for the participant outside the cubicle were four complex, star-like geometrical shapes whereas simple shapes (a circle, a triangle, a square, and a hexagon) served as target stimuli for the participant inside the cubicle. In addition, the participant inside the cubicle saw a horizontal row of four blue squares (2 cm × 2 cm), which were displayed throughout the experiment in the lower part of the screen. These blue squares were arranged in the same way as the participants’ response keys and thus each square corresponded to one key. Only the reactions of the participant in the cubicle were of interest. The task for the participant outside the cubicle only served to increase the plausibility of the cover story. After half of the experiment, participants switched seats and thus tasks.

Procedure. Participants were informed that the purpose of the experiment was to investigate how people would work together if they could not see each other. In the social group, participants were informed that they would work together on one task, but that each of the participants had a different role in the task. They were further instructed that the responses of each person would influence what the other person would see on the screen. Thus, the participant inside the cubicle could control which target stimulus was displayed for the participant outside the cubicle. In turn, the participant outside the cubicle could control the blue squares on the screen inside the cubicle, which turned green whenever the participant outside pressed a key. That is, participants were instructed that the visual feedback displayed inside the cubicle indicated the action of the participant outside the cubicle. Because participants outside the cubicle always had to react with the same response key as the participant inside the cubicle, a spatially compatible green rectangle signified a correct partner response to the participant inside the cubicle, whereas a spatially incompatible green rectangle indicated a partner error. Participants were given ample time to observe this mechanism in action at the beginning of the experiment.

In the nonsocial group, by contrast, participants were informed that they would each complete an own task, independently of the other person. The only relation between the participants was that the timing of the tasks would be aligned; therefore, they were instructed that they would sometimes have to wait for the other person to complete their task.

The basic trial structure was identical in the social and the nonsocial group. Each trial started with the display of a white fixation cross for both participants. After 500 ms, the target stimulus was displayed on the screen inside the cubicle for 300 ms (the screen outside still displayed the fixation cross). As soon as the participant inside had reacted, the target stimuli was displayed on the outside screen for 200 ms. Importantly, not only the timing but also the identity of that target stimulus depended on the reaction of the participant inside (for both the social and the nonsocial group), so that the target stimulus prompted a reaction with the same finger

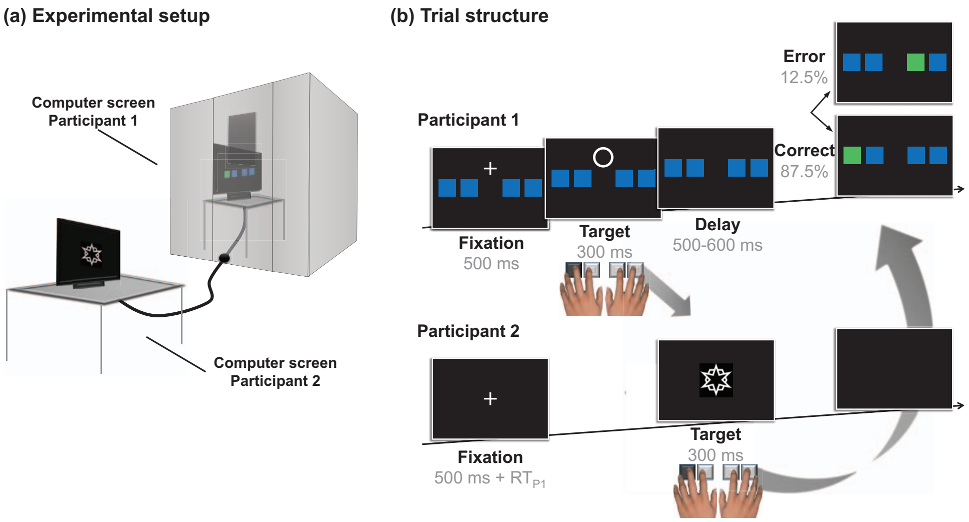

Figure 1. (a) Setup of Experiment 1. One participant sat in front of a screen inside a cubicle, the other participant sat outside. (b) Trials started with a fixation cross for both participants, followed by the stimulus for the participant inside the cubicle (Participant 1). After a certain delay, one of the blue rectangles on his or her screen was colored green (blue and green squares appear in dark and light grey in greyscale versions of this figure). In about 87.5% of the trials, the green rectangle corresponded to the correct keypress of Participant 1 (correct trials); in about 12.5% of the trials, the green rectangle did not correspond to this key keypress (error trials). The timing and identity of the stimulus for the participant outside the cubicle (Participant 2) was determined by the first participant's reaction: as soon as Participant 1 responded, a stimulus requiring the same reaction was presented to Participant 2. In the social group, Participant 1 was led to believe that onset and identity of the green rectangle were controlled by the keypress of Participant 2, even though the events were actually controlled by the computer. In the nonsocial group, participants were informed that the computer controlled the green rectangles. For Participant 1, the green rectangles would usually correspond spatially to the key pressed, while a noncorresponding rectangle lit up in case of partner errors or machine malfunctions. See the online article for the color version of this figure.

from the participant outside. Participants in the social group were informed about this relationship, but participants in the nonsocial group were not. For the participants outside, a trial was finished when they had responded to the target stimulus. For the participant inside, the display changed again after a delay of 500 to 600 ms following their reaction. Then, one of the blue squares on the screen turned green. In most cases (approximately 87.5% of the trials), this square corresponded to the key the participant had used in the trial; in rare cases, however, a noncorresponding square was colored green indicating a partner error for the social group and a machine malfunction for the nonsocial group (12.5% of the trials).

More precisely, each trial had a one in eight chance to be a noncorresponding trial, which was randomly determined at the beginning of each trial. Participants in the social group were instructed that this green square represented the key press of the participant outside the cubicle. Participants in the nonsocial group were instructed that the computer would color one of the squares green. All participants were informed that occasionally a noncorresponding square could turn green, representing an error of the other participant (in the social group) or a malfunction of the computer (in the nonsocial group), and that they should count these errors (while mean error frequency was identical for both groups). The respective square stayed green until 1,000 ms after the response of the participant outside the cubicle was registered (i.e., the square stayed green for at least 1,000 ms and longer if the participant outside the cubicle responded only after the square had turned green, i.e., if the response time [RT] was longer than 500 to 600 ms). Then, all squares were reset to blue and the next trial started after 1,000 ms.

If one of the participants pressed a key during the presentation of the fixation cross, an error message was displayed. In the social group this error message was displayed to both participants (indicating the person who had committed the error) and the trial was aborted. This was also the case if the participant inside the cubicle did not react. In the nonsocial group, these error messages were only displayed to the participants who had committed the error and the trial continued for the other participant. If the participant in the cubicle committed an error, the identity of the target stimulus for the participant outside the cubicle still depended on this erroneous reaction, but if the participant inside did not react, a random target stimulus was displayed. In both groups, commission errors triggered error messages only for the participant who had committed the error and the trial continued for the other participant.

We included a practice phase at the beginning of the experiment. To boost the participants' belief that the colored squares were controlled by the other participant in the social group, the squares were actually controlled by the other participant in this practice phase (i.e., timing and identity of the green square actually depended on the other participant's reaction during practice). During the practice trials, the door of the cubicle was left open, so that participants could see and hear each other. Participants completed 12 practice trials. Then, they switched seats (and tasks) and completed another 12 practice trials. Thus, participants knew each other's tasks. In the nonsocial group, participants completed the same practice phase, but the colored squares were controlled by the computer.

Participants performed a total of 14 blocks with 60 trials each and they switched seats after half of the experiment (i.e., after seven blocks). At the end of each block, the participant inside the cubicle were to report the number of trials with a noncorresponding green square, that is, the number of errors of the other participant (social group) or the number of malfunctions of the computer (nonsocial group).

Data treatment. In the debriefing, seven participants of the social group mentioned that they had not believed the cover story and were therefore skeptical whether the other participant had controlled the colored squares. These participants were excluded from all analyses. Furthermore, three participants in the social group and three participants in the nonsocial group were excluded because they missed more than 20% of the errors in the counting tasks. Finally, one participant of the nonsocial group was excluded because he or she committed errors in more than 75% of the trials.

Only data from the participant inside the cubicle was analyzed to compare participants' reactions in the two groups depending on the feedback in the previous trial, that is, depending on whether participants had observed an error of the other participant or the computer, as indicated by a noncorresponding green rectangle, or whether the other participant/the computer had responded correctly. For RT analysis of this data, all trials with errors of the participant themselves (rather than errors of their alleged interaction partner; 5.4%) and all trials following these erroneous trials were excluded, as well as trials that deviated more than 2.5 standard deviations from the cell mean of the remaining correct RTs (calculated separately for each participant and condition; 3.0%). For error analysis, we only considered trials in which the participant responded correctly and trials with commission errors, whereas trials with omissions, early responses (during the fixation cross) and trials with early responses of the other participant were excluded. Mean RTs and error percentages were analyzed with a mixed 2 × 2 analysis of variance (ANOVA) with the within-subjects factor previous feedback (error/malfunction vs. correct) and the between-subjects factor setting (social vs. nonsocial group). To follow up on the ANOVA, two-tailed, paired *t* tests were calculated comparing participants' reactions after observed error and correct trials in each group.

The data and syntax files for statistical analyses are publicly available on the Open Science Framework (https://osf.io/sd7a8/).

Results

Figure 2 shows RTs and error percentages for the social group and the nonsocial group as a function of previous feedback—that is, feedback indicating either correct or erroneous responses of the partner in the social group and of the computer in the nonsocial group.

RTs. The ANOVA revealed a main effect of previous feedback, $F(1, 86) = 48.86$, $p < .001$, $\eta_p^2 = .36$, indicating that participants slowed down after having observed an error or a malfunction compared to a correct trial (i.e., post-oddball slowing). There was no main effect of setting, $F < 1$, but an interaction of previous feedback and setting, $F(1, 86) = 6.56$, $p = .012$, $\eta_p^2 = .07$, indicating that post-oddball slowing was more pronounced in the social group compared to the nonsocial group. Pairwise comparisons within groups still showed significant post-oddball slowing in both the social group ($M_{\text{correct}} = 634$ ms, $M_{\text{error}} = 671$ ms), $t(43) = 6.95$, $p < .001$, $d = 1.05$, and the nonsocial group ($M_{\text{correct}} = 637$ ms, $M_{\text{error}} = 655$ ms), $t(43) = 3.05$, $p = .004$, $d = 0.46$.

Error rates. The ANOVA revealed a main effect of previous feedback, $F(1, 86) = 5.29$, $p = .024$, $\eta_p^2 = .06$, indicating worse performance after error feedback than after correct feedback. The main effect of setting was not significant, $F < 1$, and neither was the interaction of previous feedback and group, $F(1, 86) = 1.15$, $p = .286$, $\eta_p^2 = .01$.

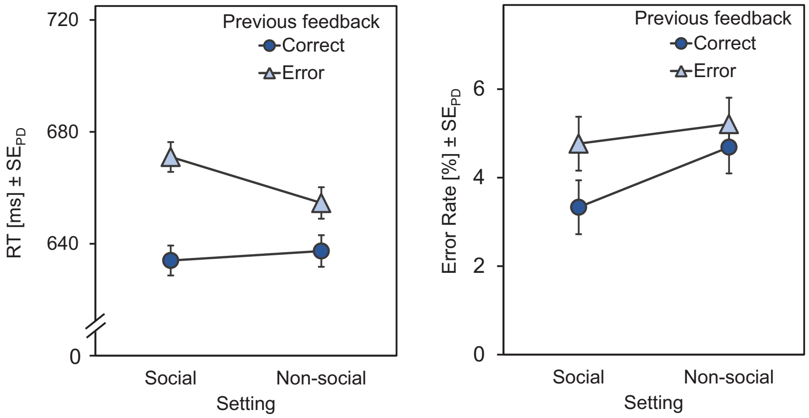

Figure 2. Results of Experiment 1. Response times (RTs) and error percentages for the social group and the nonsocial group for trials that were preceded by correct feedback and for trials that were preceded by error feedback. Participants were led to believe that error feedback indicated partner errors in the social group whereas it indicated machine malfunctions in the nonsocial group. Error bars indicate standard errors of paired differences for the effect of previous feedback in each group (Pfister & Janczyk, 2013). See the online article for the color version of this figure.

Discussion

Experiment 1 yielded increased post-oddball slowing for unexpected partner errors as compared to unexpected machine malfunctions. Crucially, these differences in post-oddball slowing cannot be attributed to physical or statistical differences between both types of oddballs (Notebaert et al., 2009), because oddball events looked identical and occurred with an equal probability in both groups. Between-groups differences in post-oddball slowing can thus not be attributed to general attentional lapses following unexpected events (Houtman & Notebaert, 2013; Van Der Borght, Schevernels, Burle, & Notebaert, 2016).

But which mechanisms are responsible for the pronounced slow-down after partner errors? Own errors typically lead to attentional focusing on own behavior and a readjustment of speed–accuracy trade-offs, which often results in posterror increases of accuracy (Danielmeier, Eichele, Forstmann, Tittgemeyer, & Ullsperger, 2011; Danielmeier & Ullsperger, 2011). This was clearly not the case here, because post-oddball accuracy was worse than postcorrect accuracy. The findings of Experiment 1 therefore suggest that partner errors triggered sustained attentional orienting toward the social partner, that is, sustained monitoring of the corresponding social action effects.

These findings fit well with a recent study which compared posterror slowing for errors of a social partner that occurred either in response to an action of the participant (as in the present Experiment 1) or that were merely observed without intervention on part of the participant (Weller et al., 2018). Here, posterror slowing was more pronounced if partner errors occurred in response to the participant's action as compared to merely observed errors.

A potential reason for increased monitoring for social as compared to nonsocial effects is that the common state of imperfect contingencies in social environments renders such monitoring especially relevant. Building on numerous encounters of imperfect contingencies, humans might thus habitually engage in monitoring processes when acting to trigger responses from other people. In addition to imperfect contingencies, sustained monitoring might also be especially useful in social interactions rather than interactions with the inanimate environment, because even correct, intended partner responses are often more variable than correct responses of a machine. For instance, when asking whether a social partner remembered to bring a certain object, the answer might be the expected "yes," but it might also be another affirmative response ("I did") or even a more subtle nod. Even though these considerations are suggestive of increased monitoring in social situations, the results of Experiment 1 might also be explained by assuming that the chosen experimental setup diminished monitoring for nonsocial action effects. More precisely, participants are likely to expect most machines to work deterministically, especially for a simple task such as the present one. Noticing several malfunctions could therefore lead the participants to classify the computer or the computer program as defective and therefore not attend its feedback as strongly anymore. Such an explanation would suggest, however, that participants in the nonsocial group should show strong posterror effects specifically for the very first encounters of a computer malfunction. To address this potential alternative explanation, we reanalyzed the data and considered only the first 10 errors in the social group and the first 10 malfunctions of the nonsocial group. We then compared the participants' RTs for these trials with the RTs of the immediately preceding correct trial (social group: $M_{\text{correct}} = 680$ ms, $M_{\text{error}} = 727$ ms; nonsocial group: $M_{\text{correct}} = 659$ ms, $M_{\text{error}} = 669$ ms). For this restricted analysis, there was still a sizable effect of post-oddball slowing, $F(1, 86) = 10.29$, $p = .002$, $\eta_p^2 = .11$, which was again larger in the social group than in the nonsocial group, $F(1, 86) = 4.23$, $p = .043$, $\eta_p^2 = .05$. These results suggest that enhanced monitoring of social effects is indeed the driving force behind the observed differences in post-oddball slowing.

Experiment 2: Unexpected Delays

The unexpected oddball events of Experiment 1 indicated that the intended action-effect sequence was disrupted. Such disruptions caused especially strong and extended monitoring when they were due to a social partner. Experiment 2 aimed at extending this approach to oddball events that did not endanger task completion by introducing unexpected delays between own actions and following effects (i.e., unexpected reductions of action-effect contiguity).

We therefore replicated the basic setup of the Experiment 1 but now displayed the partner response in the social group and the computer response in the nonsocial group after a substantial delay of about 2,000 ms in a fraction of the trials (while also omitting the error counting task). Delays of this length have been shown to yield a noticeable impact on action control when encountered predictably (Dignath, Pfister, Eder, Kiesel, & Kunde, 2014; Elsner & Hommel, 2004), whereas their impact on performance monitoring has not been scrutinized for either social or inanimate settings. We again analyzed the participants' performance as a function of previous feedback (immediate vs. delayed). Following the results of Experiment 1, we expected post-oddball slowing after delays as compared to immediate responses, and we expected this effect to be more pronounced in the social group as compared to the nonsocial group.

Method

Participants. We aimed at a sample size of 88 participants, with 44 participants (22 pairs) per group, as in Experiment 1. A considerable share of the participants in the social group did not believe the cover story that the green squares had been controlled by the other participant, however, and several participants in the social and nonsocial group did not pay attention to the green squares (see the section on Data Treatment for details). The data of these participants were replaced as in Experiment 1 so that we tested 108 participants (62 for the social group, 46 for the nonsocial group) in total. The final sample size for statistical analysis comprised 86 participants (43 per group; mean age = 24.4 years, range: 18–51 years; 25 men, nine left-handed; note that participants were recruited in pairs, but individual participants were replaced so that we could not opt for an exact number of 44 participants per group). Participants gave informed consent prior to the experiment and received course credit or monetary compensation.

Apparatus, stimuli, and procedure. Stimuli and apparatus were identical to Experiment 1 whereas the experimental procedure was slightly adapted. Participants inside the cubicle no longer observed commission errors of the other participant or the computer. Instead, they observed a delayed response in those trials. To that end, the corresponding square was now colored green in oddball trials, but this color change was delayed compared to correct trials (1,850 to 2,150 ms after the participant inside the cubicle had responded instead of 500 to 600 ms). Participants were not informed about these delays and they no longer had to count the errors of the other participant or the malfunctions of the computer. Instead, they were encouraged to carefully attend to the blue and green squares, in case there were any irregularities like errors of the other participant (social group) or malfunctions of the computer (nonsocial group). To investigate whether participants had actually attended to the green squares, they completed a short questionnaire at the end of the experiment. This questionnaire also allowed us to probe whether participants had believed the cover story that the green squares had been controlled by the other participant (in the social group).

Data treatment. In the follow-up questionnaires, 17 participants of the social group expressed doubts that the other participant had controlled the colored rectangles. These participants were excluded from all analyses. Furthermore, two participants in the social group and three participants in the nonsocial group indicated that they had not paid any attention to the green rectangle. These participants were also excluded.

The data of the remaining participants was treated as in Experiment 1. For RT analysis, we again excluded all trials with errors of the participant (5.1%) and all trials following these erroneous trials, as well as trials that deviated more than 2.5 standard deviations from the cell mean of the remaining RTs (calculated separately for each participant and previous feedback; 2.6%). Mean RTs and error percentages were analyzed with a mixed 2 × 2 ANOVA with the within-subjects factor previous feedback (delayed vs. immediate) and the between-subjects factor setting (social vs. nonsocial group).

Results

Figure 3 shows RTs and error percentages for both groups as a function of previous feedback—that is, feedback indicating either immediate or delayed responses of the partner in the social group and of the computer in the nonsocial group.

RTs. The ANOVA revealed a main effect of previous feedback, $F(1, 84) = 25.99$, $p < .001$, $\eta_p^2 = .24$, indicating that participants reacted slower after having observed a delayed compared to an immediate response. There was also a main effect of setting, $F(1, 84) = 5.17$, $p = .026$, $\eta_p^2 = .06$, but no interaction of previous feedback and setting, $F < 1$.

Error rates. The ANOVA showed no main effect of previous feedback, no main effect of setting and no interaction, all Fs < 1.

Discussion

The results of Experiment 2 yielded post-oddball slowing after unexpected delays for both the social and the nonsocial group, and the effect size for postdelay slowing was numerically almost identical. Together with the results of Experiment 1, these results suggest that unexpected social action effects recruit increased monitoring only if the unexpected event disrupts an intended action-effect sequence but not if the sequence results in the intended (albeit delayed) effects.

This interpretation suggests that monitoring should be tuned toward task-relevant aspects of an action effect. Even though action-contingent changes in the environment are also processed if they are not relevant to the task at hand (Hommel, 2005) it seems plausible to assume that active monitoring targets especially those aspects that are currently intended (Logan & Crump, 2010). Unexpected delays could therefore incur stronger monitoring in social as compared to nonsocial situations when the timing of own and partner actions matters for success of the action sequence. Such situations are plausible in many everyday scenarios that involve close temporal coordination of individual agents—a topic that has received considerable interest from research on joint action (Valdesolo, Ouyang, & DeSteno, 2010; Vesper, van der Wel, Knoblich, & Sebanz, 2011). A related situation that could boost effect monitoring after delayed effects occurs when action-effect delays allow for predicting the success of an action sequence. Human agents have indeed been shown to acquire such delay-event associations (Thomaschke & Dreisbach, 2015; Thomaschke, Hoffmann, Haering, & Kiesel, 2016; Thomaschke, Kunchulia, &

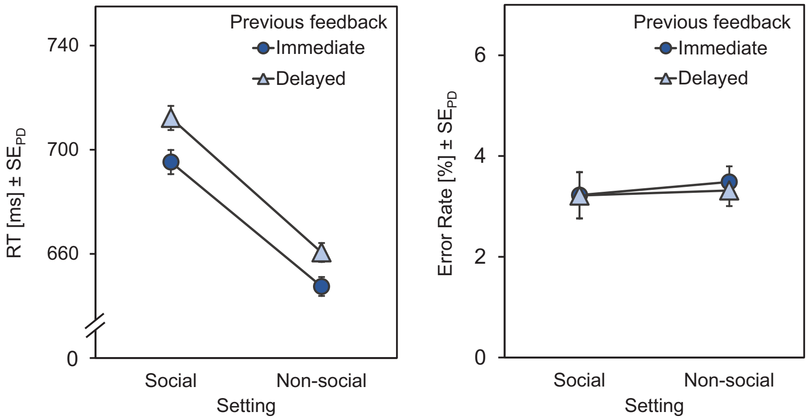

Figure 3. Results of Experiment 2. Response times (RTs) and error percentages for the social group and the nonsocial group for trials that were preceded by immediate feedback (87.5% of the trials) and for trials that were preceded by feedback that indicated delayed responses (12.5%). Error bars indicate standard errors of paired differences for the effect of previous feedback in each group (Pfister & Janczyk, 2013). See the online article for the color version of this figure.

Dreisbach, 2015) and such associations could be exploited to trigger monitoring proactively (e.g., when long delays are predictive of error commission).

Finally, the RT results of Experiment 2 indicated generally slower responses of the social group compared to the nonsocial group while the nonsocial group performed at about the same level as both groups of Experiment 1. This finding may be taken to suggest that participants of the social group adapted to the average speed of their social partners whereas no such adaptation took place regarding the speed of the computer responses for the nonsocial group. Such spontaneous synchronization to social partners has been reported in a range of studies (Konvalinka, Vuust, Roepstorff, & Frith, 2010; Lelonkiewicz & Gambi, 2017; Richardson, Marsh, Isenhower, Goodman, & Schmidt, 2007). This finding is again suggestive of a somewhat greater reactivity of human agents to social as compared to nonsocial action effects. We will get back to this issue in the following discussion.

General Discussion

The present experiments targeted the processing of unexpected interruptions of action-effect sequences for social relative to nonsocial action effects. In Experiment 1, we created situations of reduced contingency by introducing partner errors in a social group and machine malfunctions in a nonsocial group and observed larger post-oddball slowing for the social than for the nonsocial group. In Experiment 2, we created situations of reduced contiguity by delaying social and nonsocial action effects in an unpredictable manner; here, robust post-oddball slowing emerged to a similar degree for both groups.

Together, these results indicate that participants recruit monitoring mechanisms especially when observing a social partner responding erroneously to their actions. These results extend recent studies on effect-monitoring for nonsocial action effects such as visual or auditory changes in the participants' environment (Wirth, Steinhauser, Janczyk, Steinhauser, & Kunde, 2018) to the domain of sociomotor actions. Previous work on nonsocial action effects has suggested that actions evoke a short-lived monitoring process which serves at least two functions. First, sustained monitoring helps to ascertain that an action has been carried out as intended and that it has produced the anticipated effects in the environment. Second, monitoring allows for detecting unforeseen but potentially relevant effects of own movements (Wirth, Janczyk, & Kunde, 2018). Both functions may be required to a larger degree in social as compared to nonsocial context (Kunde et al., 2018; Müller & Jung, 2018).

Not surprisingly, monitoring processes for action effects are similar to error detection and error monitoring processes and share critical functional and neural characteristics with them (Band, van Steenbergen, Ridderinkhof, Falkenstein, & Hommel, 2009; Steinhauser, Wirth, Kunde, Janczyk, & Steinhauser, 2018; Weller et al., 2018). The close connection of monitoring of own performance and monitoring of ensuing effects may also inform how interrupted action-effect sequences are represented. More precisely, the failure to produce an intended behavior of a social interaction partner (i.e., an interrupted sequence of action and social action effect) essentially constitutes an action slip albeit at the level of environment-related action effects rather than at the level of own body movements (Logan & Crump, 2010; Steinhauser & Kiesel, 2011). This interpretation is corroborated by previous findings suggesting that participants experience agency over social responses to their own actions (Caspar, Cleeremans, & Haggard, 2018; Pfister, Obhi, Rieger, & Wenke, 2014; Recht & Grynszpan, 2019; Stephenson, Edwards, Howard, & Bayliss, 2018).

These effects of increased monitoring arose even though we carefully controlled for low-level perceptual features of social as compared to nonsocial events. This was achieved by having participants work on a computer-mediated task in which the behavior of one's partner could only be assessed through its consequences on the computer screen. We believe that direct, face-to-face interactions provide additional cues that are likely to boost monitoring even further. Social stimuli are processed particularly efficiently so that deviations from an expected course of events are likely to be especially salient (Farah et al., 1998; Neumann et al., 2014; Thompson et al., 2005). Moreover, many social responses will map directly onto the agent's own behavioral repertoire ("sociomotor compatibility"; Kunde et al., 2018) so that they should become deeply integrated into the corresponding action representations. Both factors are likely to boost monitoring efforts in such direct, unmediated interactions.

The present results further suggest that monitoring processes for social and nonsocial effects are not qualitatively different; rather, the employed monitoring processes seem to be more similar than dissimilar while they are recruited to a larger degree in social as compared to nonsocial contexts. This conclusion mirrors findings on attentional orienting to exogenous cues that are assumed to originate either from the agent's physical environment or from a social partner (Gobel, Tufft, & Richardson, 2018). Both types of cues were found to capture attention, but the attentional capture effect was more pronounced for allegedly social stimuli. That is, despite possible peculiarities of sociomotor actions (Kunde et al., 2018; see also Sahaï, Pacherie, Grynszpan, & Berberian, 2017) and possible influences of social contexts on cognition in action (Capozzi, Becchio, Garbarini, Savazzi, & Pia, 2016; Khalighinejad, Bahrami, Caspar, & Haggard, 2016), similar monitoring processes seem to be recruited irrespective of whether an action aims at changing the animate or the inanimate environment. At the same time, these mechanisms can come with genuinely social consequences. For instance, it has been observed that noncontingent partner responses reduce social affiliation as compared to contingent partner responses (Dignath, Lotze-Hermes, Farmer, & Pfister, 2018). Given that monitoring is effortful in the sense that it is a bottleneck process (Foerster, Wirth, Berghoefer, Kunde, & Pfister, 2019; Jentzsch, Leuthold, & Ulrich, 2007) and takes longer with feedback that is spatially or otherwise incompatible rather than compatible to the response (Wirth, Janczyk, et al., 2018; Wirth, Steinhauser, et al., 2018), it seems plausible that the stimuli causing this increased effort are evaluated less favorably than stimuli that can be monitored efficiently. An alternative view would hold that partner errors reduce the flow (or: fluency) of the interaction (Georgescu et al., 2014). It is an open question, however, whether monitoring efforts or flow disruption effects scale with the level or the variability of contingency and contiguity. In other words: Is monitoring increased or decreased for higher as compared to lower contingency of (social) action effects? Is there a lower bound of contingency, and thus predictability, at which monitoring is no longer recruited (e.g., when the performance of the social partner approaches chance level)?

Further open questions relate to interactions in which agents are able to adapt their own behavior to prevent future malfunctions or errors. Psycholinguistic studies, for instance, have shown that participants adapt instantly to misunderstandings, that is, machine malfunctions in human-computer dialogs by choosing expressions they expect the computer to understand (Koulouri, Lauria, & Macredie, 2016; Zoltan-Ford, 1991; for similar findings on human-human dialogs, see Brennan & Clark, 1996; Pickering & Garrod, 2006). Research on such adaptation effects has shown that the degree of adaptation depends on the participants' beliefs about their virtual partner, a factor that we did not manipulate or control in the present experiment (Branigan, Pickering, Pearson, McLean, & Brown, 2011; Kennedy, Wilkes, Elder, & Murray, 1988). The explicit manipulation of this potential moderator appears a promising avenue for further research, as are higher-level moderators such as trust in humans and machines (Madhavan & Wiegmann, 2007).

Conclusions

The present results indicate that erroneous responses of a social interaction partner trigger sustained monitoring processes that affect subsequent interactions. Even though these processes are likely mediated by general mechanisms of performance monitoring, these mechanisms are still recruited to a stronger degree if an unexpected event in an action-effect sequence represents a partner error as compared to a machine malfunction.

Context of the Research

The main idea behind the framework of "sociomotor action control" is that one's own actions can become represented in terms of the behavior they elicit at social interaction partners. The three authors have been intrigued by this possibility for several years, and accumulating evidence from different groups lends strong support for this claim by now. It is much less clear, however, whether such social action representations differ in any way from actions that aim at affecting the inanimate environment. One of the main factors that are typically evoked when describing the peculiarities of social interactions is the natural variability of social responses—which results in rather limited contingency and contiguity between one's own actions and the following responses of a social partner. Studying these two variables thus was a natural go-to. Finding an experimental paradigm to implement these variables efficiently was a much more difficult task, especially when aiming to control for (psycho)physical differences on the stimulus side as best as possible. The work reported here represents a major outcome of a joint, DFG-funded research project between 2016 and 2019 and is also a key element of Roland Pfister's habilitation project. We hope to use the experimental design of the described studies to work toward a comprehensive take on monitoring of social action effects in the future.

References

Band, G. P. H., van Steenbergen, H., Ridderinkhof, K. R., Falkenstein, M., & Hommel, B. (2009). Action-effect negativity: Irrelevant action effects are monitored like relevant feedback. *Biological Psychology, 82,* 211–218. http://dx.doi.org/10.1016/j.biopsycho.2009.06.011

Bigelow, A. E. (1998). Infants' sensitivity to familiar imperfect contingencies in social interaction. *Infant Behavior & Development, 21,* 149–162. http://dx.doi.org/10.1016/S0163-6383(98)90060-1

Branigan, H. P., Pickering, M. J., Pearson, J., McLean, J. F., & Brown, A. (2011). The role of beliefs in lexical alignment: Evidence from dialogs with humans and computers. *Cognition, 121,* 41–57. http://dx.doi.org/10.1016/j.cognition.2011.05.011

Brennan, S. E., & Clark, H. H. (1996). Conceptual pacts and lexical choice in conversation. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 22,* 1482–1493. http://dx.doi.org/10.1037/0278-7393.22.6.1482

Capozzi, F., Becchio, C., Garbarini, F., Savazzi, S., & Pia, L. (2016). Temporal perception in joint action: This is MY action. *Consciousness and Cognition: An International Journal, 40,* 26–33. http://dx.doi.org/10.1016/j.concog.2015.12.004

Caspar, E. A., Cleeremans, A., & Haggard, P. (2018). Only giving orders? An experimental study of the sense of agency when giving or receiving commands. *PLoS ONE, 13*(9), e0204027. http://dx.doi.org/10.1371/journal.pone.0204027

Crivelli, C., & Fridlund, A. J. (2018). Facial displays are tools for social influence. *Trends in Cognitive Sciences, 22,* 388–399. http://dx.doi.org/10.1016/j.tics.2018.02.006

Danielmeier, C., Eichele, T., Forstmann, B. U., Tittgemeyer, M., & Ullsperger, M. (2011). Posterior medial frontal cortex activity predicts post-error adaptations in task-related visual and motor areas. *The Journal of Neuroscience, 31,* 1780–1789. http://dx.doi.org/10.1523/JNEUROSCI.4299-10.2011

Danielmeier, C., & Ullsperger, M. (2011). Post-error adjustments. *Frontiers in Psychology, 2,* 233. http://dx.doi.org/10.3389/fpsyg.2011.00233

Dignath, D., Lotze-Hermes, P., Farmer, H., & Pfister, R. (2018). Contingency and contiguity of imitative behaviour affect social affiliation. *Psychological Research, 82,* 819–831. http://dx.doi.org/10.1007/s00426-017-0854-x

Dignath, D., Pfister, R., Eder, A. B., Kiesel, A., & Kunde, W. (2014). Representing the hyphen in action-effect associations: Automatic acquisition and bidirectional retrieval of action-effect intervals. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 40,* 1701–1712. http://dx.doi.org/10.1037/xlm0000022

Dzindolet, M. T., Beck, H. P., Pierce, L. G., & Dawe, L. A. (2001). *A framework of automation use* (Report No. ARL-TR-2412). Aberdeen Proving Ground, MD: Army Research Laboratory.

Dzindolet, M. T., Pierce, L. G., Beck, H. P., & Dawe, L. A. (2002). The perceived utility of human and automated aids in a visual detection task. *Human Factors, 44,* 79–94. http://dx.doi.org/10.1518/0018720024494856

Elsner, B., & Hommel, B. (2001). Effect anticipation and action control. *Journal of Experimental Psychology: Human Perception and Performance, 27,* 229–240. http://dx.doi.org/10.1037/0096-1523.27.1.229

Elsner, B., & Hommel, B. (2004). Contiguity and contingency in action-effect learning. *Psychological Research, 68,* 138–154. http://dx.doi.org/10.1007/s00426-003-0151-8

Farah, M. J., Wilson, K. D., Drain, M., & Tanaka, J. N. (1998). What is "special" about face perception? *Psychological Review, 105,* 482–498. http://dx.doi.org/10.1037/0033-295X.105.3.482

Flach, R., Press, C., Badets, A., & Heyes, C. (2010). Shaking hands: Priming by social action effects. *British Journal of Psychology, 101,* 739–749. http://dx.doi.org/10.1348/000712609X484595

Foerster, A., Wirth, R., Berghoefer, F. L., Kunde, W., & Pfister, R. (2019). Capacity limitations of dishonesty. *Journal of Experimental Psychology: General, 148,* 943–961. http://dx.doi.org/10.1037/xge0000510

Georgescu, A. L., Kuzmanovic, B., Santos, N. S., Tepest, R., Bente, G., Tittgemeyer, M., & Vogeley, K. (2014). Perceiving nonverbal behavior: Neural correlates of processing movement fluency and contingency in dyadic interactions. *Human Brain Mapping, 35,* 1362–1378. http://dx.doi.org/10.1002/hbm.22259

Gobel, M. S., Tufft, M. R. A., & Richardson, D. C. (2018). Social beliefs and visual attention: How the social relevance of a cue influences spatial orienting. *Cognitive Science, 42*(Suppl. 1), 161–185. http://dx.doi.org/10.1111/cogs.12529

Gozli, D. G., Huffman, G., & Pratt, J. (2016). Acting and anticipating: Impact of outcome-compatible distractor depends on response selection efficiency. *Journal of Experimental Psychology: Human Perception and Performance, 42,* 1601–1614. http://dx.doi.org/10.1037/xhp0000238

Herwig, A., & Horstmann, G. (2011). Action-effect associations revealed by eye movements. *Psychonomic Bulletin & Review, 18,* 531–537. http://dx.doi.org/10.3758/s13423-011-0063-3

Hommel, B. (2005). How much attention does an event file need? *Journal of Experimental Psychology: Human Perception and Performance, 31,* 1067–1082. http://dx.doi.org/10.1037/0096-1523.31.5.1067

Hommel, B. (2009). Action control according to TEC (theory of event coding). *Psychological Research, 73,* 512–526. http://dx.doi.org/10.1007/s00426-009-0234-2

Houtman, F., Castellar, E. N., & Notebaert, W. (2012). Orienting to errors with and without immediate feedback. *Journal of Cognitive Psychology, 24,* 278–285. http://dx.doi.org/10.1080/20445911.2011.617301

Houtman, F., & Notebaert, W. (2013). Blinded by an error. *Cognition, 128,* 228–236. http://dx.doi.org/10.1016/j.cognition.2013.04.003

Jentzsch, I., Leuthold, H., & Ulrich, R. (2007). Decomposing sources of response slowing in the PRP paradigm. *Journal of Experimental Psychology: Human Perception and Performance, 33,* 610–626. http://dx.doi.org/10.1037/0096-1523.33.3.610

Kennedy, A., Wilkes, A., Elder, L., & Murray, W. S. (1988). Dialogue with machines. *Cognition, 30,* 37–72. http://dx.doi.org/10.1016/0010-0277(88)90003-0

Khalighinejad, N., Bahrami, B., Caspar, E. A., & Haggard, P. (2016). Social transmission of experience of agency: An experimental study. *Frontiers in Psychology, 7,* 1315. http://dx.doi.org/10.3389/fpsyg.2016.01315

Konvalinka, I., Vuust, P., Roepstorff, A., & Frith, C. D. (2010). Follow you, follow me: Continuous mutual prediction and adaptation in joint tapping. *Quarterly Journal of Experimental Psychology, 63,* 2220–2230. http://dx.doi.org/10.1080/17470218.2010.497843

Koulouri, T., Lauria, S., & Macredie, R. D. (2016). Do (and say) as I say: Linguistic adaptation in human-computer dialogs. *Human-Computer Interaction, 31,* 59–95. http://dx.doi.org/10.1080/07370024.2014.934180

Kunde, W. (2001). Response-effect compatibility in manual choice reaction tasks. *Journal of Experimental Psychology: Human Perception and Performance, 27,* 387–394. http://dx.doi.org/10.1037/0096-1523.27.2.387

Kunde, W., Lozo, L., & Neumann, R. (2011). Effect-based control of facial expressions: Evidence from action-effect compatibility. *Psychonomic Bulletin & Review, 18,* 820–826. http://dx.doi.org/10.3758/s13423-011-0093-x

Kunde, W., Weller, L., & Pfister, R. (2018). Sociomotor action control. *Psychonomic Bulletin & Review, 25,* 917–931. http://dx.doi.org/10.3758/s13423-017-1316-6

Lelonkiewicz, J. R., & Gambi, C. (2017). Spontaneous adaptation explains why people act faster when being imitated. *Psychonomic Bulletin & Review, 24,* 842–848. http://dx.doi.org/10.3758/s13423-016-1141-3

Logan, G. D., & Crump, M. J. (2010). Cognitive illusions of authorship reveal hierarchical error detection in skilled typists. *Science, 330,* 683–686. http://dx.doi.org/10.1126/science.1190483

Madhavan, P., & Wiegmann, D. A. (2007). Similarities and differences between human-human and human-automation trust: An integrative review. *Theoretical Issues in Ergonomics Science, 8,* 277–301. http://dx.doi.org/10.1080/14639220500337708

Manzey, D., Reichenbach, J., & Onnasch, L. (2012). Human performance consequences of automated decision aids: The impact of degree of automation and system experience. *Journal of Cognitive Engineering and Decision Making, 6,* 57–87. http://dx.doi.org/10.1177/1555343411433844

Müller, R. (2016). Does the anticipation of compatible partner reactions facilitate action planning in joint tasks? *Psychological Research, 80,* 464–486. http://dx.doi.org/10.1007/s00426-015-0670-0

Müller, R., & Jung, M. L. (2018). Partner reactions and task set selection: Compatibility is more beneficial in the stronger task. *Acta Psychologica, 185,* 188–202. http://dx.doi.org/10.1016/j.actpsy.2018.02.012

Neumann, R., Lozo, L., & Kunde, W. (2014). Not all behaviors are controlled in the same way: Different mechanisms underlie manual and facial approach and avoidance responses. *Journal of Experimental Psychology: General, 143,* 1–8. http://dx.doi.org/10.1037/a0032259

Notebaert, W., Houtman, F., Opstal, F. V., Gevers, W., Fias, W., & Verguts, T. (2009). Post-error slowing: An orienting account. *Cognition, 111,* 275–279. http://dx.doi.org/10.1016/j.cognition.2009.02.002

Pfister, R. (2019). Effect-based action control with body-related effects: Implications for empirical approaches to ideomotor action control. *Psychological Review, 126,* 153–161. http://dx.doi.org/10.1037/rev0000140

Pfister, R., Dignath, D., Hommel, B., & Kunde, W. (2013). It takes two to imitate: Anticipation and imitation in social interaction. *Psychological Science, 24,* 2117–2121. http://dx.doi.org/10.1177/0956797613489139

Pfister, R., & Janczyk, M. (2013). Confidence intervals for two sample means: Calculation, interpretation, and a few simple rules. *Advances in Cognitive Psychology, 9,* 74–80. http://dx.doi.org/10.5709/acp-0133-x

Pfister, R., Janczyk, M., Gressmann, M., Fournier, L. R., & Kunde, W. (2014). Good vibrations? Vibrotactile self-stimulation reveals anticipation of body-related action effects in motor control. *Experimental Brain Research, 232,* 847–854. http://dx.doi.org/10.1007/s00221-013-3796-6

Pfister, R., Janczyk, M., Wirth, R., Dignath, D., & Kunde, W. (2014). Thinking with portals: Revisiting kinematic cues to intention. *Cognition, 133,* 464–473. http://dx.doi.org/10.1016/j.cognition.2014.07.012

Pfister, R., Kiesel, A., & Hoffmann, J. (2011). Learning at any rate: Action-effect learning for stimulus-based actions. *Psychological Research, 75,* 61–65. http://dx.doi.org/10.1007/s00426-010-0288-1

Pfister, R., Obhi, S. S., Rieger, M., & Wenke, D. (2014). Action and perception in social contexts: Intentional binding for social action effects. *Frontiers in Human Neuroscience, 8,* 667. http://dx.doi.org/10.3389/fnhum.2014.00667

Pickering, M. J., & Garrod, S. (2006). Alignment as the basis for successful communication. *Research on Language and Computation, 4,* 203–228. http://dx.doi.org/10.1007/s11168-006-9004-0

Recht, S., & Grynszpan, O. (2019). The sense of social agency in gaze leading. *Journal on Multimodal User Interfaces, 13,* 19–30. http://dx.doi.org/10.1007/s12193-018-0286-y

Richardson, M. J., Marsh, K. L., Isenhower, R. W., Goodman, J. R., & Schmidt, R. C. (2007). Rocking together: Dynamics of intentional and unintentional interpersonal coordination. *Human Movement Science, 26,* 867–891. http://dx.doi.org/10.1016/j.humov.2007.07.002

Sahaï, A., Pacherie, E., Grynszpan, O., & Berberian, B. (2017). Predictive mechanisms are not involved the same way during human–human vs. human–machine interactions: A review. *Frontiers in Neurorobotics, 11,* 52. http://dx.doi.org/10.3389/fnbot.2017.00052

Saunders, B., & Jentzsch, I. (2012). False external feedback modulates posterror slowing and the f-P300: Implications for theories of posterror adjustment. *Psychonomic Bulletin & Review, 19,* 1210–1216. http://dx.doi.org/10.3758/s13423-012-0314-y

Steinhauser, M., & Kiesel, A. (2011). Performance monitoring and the causal attribution of errors. *Cognitive, Affective & Behavioral Neuroscience, 11,* 309–320. http://dx.doi.org/10.3758/s13415-011-0033-2

Steinhauser, R., Wirth, R., Kunde, W., Janczyk, M., & Steinhauser, M. (2018). Common mechanisms in error monitoring and action effect monitoring. *Cognitive, Affective & Behavioral Neuroscience, 18,* 1159–1171. http://dx.doi.org/10.3758/s13415-018-0628-y

Stephenson, L. J., Edwards, S. G., Howard, E. E., & Bayliss, A. P. (2018). Eyes that bind us: Gaze leading induces an implicit sense of agency. *Cognition, 172,* 124–133. http://dx.doi.org/10.1016/j.cognition.2017.12.011

Striano, T., Henning, A., & Stahl, D. (2005). Sensitivity to social contingencies between 1 and 3 months of age. *Developmental Science, 8,* 509–518. http://dx.doi.org/10.1111/j.1467-7687.2005.00442.x

Thébault, G., Michalland, A. H., Derozier, V., Chabrier, S., & Brouillet, D. (2018). When the vibrations allow for anticipating the force to be produced: An extend to Pfister et al. *Experimental Brain Research, 236,* 1219–1223, 2014. http://dx.doi.org/10.1007/s00221-018-5190-x

Thomaschke, R., & Dreisbach, G. (2015). The time-event correlation effect is due to temporal expectancy, not to partial transition costs. *Journal of Experimental Psychology: Human Perception and Performance, 41,* 196–218. http://dx.doi.org/10.1037/a0038328

Thomaschke, R., Hoffmann, J., Haering, C., & Kiesel, A. (2016). Time-based expectancy for task relevant stimulus features. *Timing & Time Perception (Leiden, Netherlands), 4,* 248–270. http://dx.doi.org/10.1163/22134468-00002069

Thomaschke, R., Kunchulia, M., & Dreisbach, G. (2015). Time-based event expectations employ relative, not absolute, representations of time. *Psychonomic Bulletin & Review, 22,* 890–895. http://dx.doi.org/10.3758/s13423-014-0710-6

Thompson, J. C., Clarke, M., Stewart, T., & Puce, A. (2005). Configural processing of biological motion in human superior temporal sulcus. *The Journal of Neuroscience, 25,* 9059–9066. http://dx.doi.org/10.1523/JNEUROSCI.2129-05.2005

Valdesolo, P., Ouyang, J., & DeSteno, D. (2010). The rhythm of joint action: Synchrony promotes cooperative ability. *Journal of Experimental Social Psychology, 46,* 693–695. http://dx.doi.org/10.1016/j.jesp.2010.03.004

Van der Borght, L., Schevernels, H., Burle, B., & Notebaert, W. (2016). Errors disrupt subsequent early attentional processes. *PLoS ONE, 11*(4), e0151843. http://dx.doi.org/10.1371/journal.pone.0151843

Vesper, C., van der Wel, R. P., Knoblich, G., & Sebanz, N. (2011). Making oneself predictable: Reduced temporal variability facilitates joint action coordination. *Experimental Brain Research, 211,* 517–530. http://dx.doi.org/10.1007/s00221-011-2706-z

Weller, L., Schwarz, K. A., Kunde, W., & Pfister, R. (2018). My mistake? Enhanced error processing for commanded compared to passively observed actions. *Psychophysiology, 55*(6), e13057. http://dx.doi.org/10.1111/psyp.13057

Wirth, R., Janczyk, M., & Kunde, W. (2018). Effect monitoring in dual-task performance. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 44,* 553–571. http://dx.doi.org/10.1037/xlm0000474

Wirth, R., Pfister, R., Brandes, J., & Kunde, W. (2016). Stroking me softly: Body-related effects in effect-based action control. *Attention, Perception, & Psychophysics, 78,* 1755–1770. http://dx.doi.org/10.3758/s13414-016-1151-2

Wirth, R., Steinhauser, R., Janczyk, M., Steinhauser, M., & Kunde, W. (2018). Long-term and short-term action-effect links and their impact on effect monitoring. *Journal of Experimental Psychology: Human Perception and Performance, 44,* 1186–1198. http://dx.doi.org/10.1037/xhp0000524

Wolfensteller, U., & Ruge, H. (2011). On the timescale of stimulus-based action-effect learning. *The Quarterly Journal of Experimental Psychology, 64,* 1273–1289. http://dx.doi.org/10.1080/17470218.2010.546417

Wolpert, D. M., Doya, K., & Kawato, M. (2003). A unifying computational framework for motor control and social interaction. *Philosophical Transactions of the Royal Society of London Series B, Biological Sciences, 358,* 593–602. http://dx.doi.org/10.1098/rstb.2002.1238

Zoltan-Ford, E. (1991). How to get people to say and type what computers can understand. *International Journal of Man-Machine Studies, 34,* 527–547. http://dx.doi.org/10.1016/0020-7373(91)90034-5

Received September 4, 2019
Revision received December 21, 2019
Accepted January 21, 2020 ■