# On the ball: Short-term consequences of movement fakes

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## Abstract

In competitive situations, humans sometimes use fake actions. Fake actions are carried out to pretend a certain action goal, which however is not actually pursued, such as pump fakes in basketball, or drop shots in tennis. Here, we studied the short-term consequences of producing or observing fakes on the planning and detection of subsequent fake actions. Two players participated in a game, an attacker and a defender. Attackers had to either throw a ball into a target basket of the defender, or to mimic such a throw without actually throwing. Defenders had to discriminate between real throws and faked throws. Participants changed the roles of attacker and defender, and switched between real and faked throws randomly, on a trial-by-trial basis. We found that the (self-)observation of a fake action facilitated the detection of subsequent fake actions of opponents, but did not facilitate the subsequent planning of own fake actions. We conjecture that previous encounters of fake actions help to focus on the movement aspects that are most diagnostic for such fake actions. As a potential practical consequence, we recommend to not generate multiple fake actions in sports within a short time, to prevent potential short-term perceptual adaptation effects of defenders.

**Keywords:** faking; dishonesty; lying; lie detection

## 1. Introduction

1.1. Deception has a bad reputation. Telling lies to a friend, sharing fake news on social media or being pranked by a fake handshake are not well-accepted forms of deception. However, deception through fake actions is accepted and actually common practice in many competitive sports (Jackson, Warren, & Abernethy, 2006). For example, in team handball, players fake throws during penalty throwing to induce premature movements of the goalkeeper (Cañal-Bruland & Schmidt, 2009; Cañal-Bruland, van der Kamp, & van Kesteren, 2010), basketball players fake shots or passes to induce an inappropriate defensive movement of the opponent (Sebanz & Shiffrar, 2009); and beach volleyball players sometimes fake a smash, but actually play a lob (Güldenpenning, Steinke, Koester, & Schack, 2013). The typical faking scenario thus involves two people, a faker and a responder. The faker pretends a certain intended action, which however, is not carried out in the pretended way. The responder aims to discriminate between real and fake actions.

Recent research has revealed various constraints regarding the production of fake movements and their impact on observers, with the majority of studies focusing on the impact of fakes on observers (cf. Güldenpenning, Kunde, & Weigelt, 2017 for a review). A key topic here relates to the role of expertise. A robust finding is that extensive observation of fake movements facilitates the detection of fakes of others (Cañal-Bruland & Schmidt, 2009; Jackson et al., 2006; Sebanz & Shiffrar, 2009). Motor expertise, that is extensive production of corresponding movements, including observation of self-produced movements (i.e., self-observation), might have similar effects. This assumption finds support in the idea that decoding observed movements of others and generating own actions rely on the same processes and possibly neural structures (Calvo-Merino, Grèzes, Glaser, Passingham, & Haggard, 2006; Schütz-Bosbach & Prinz, 2007), though the specific contributions of perceptual and motor expertise are difficult to disentangle experimentally (Cañal-Bruland et al., 2010). In any case, previous research that examined the impact of perceptual and motor expertise on the detection of fake actions has been confined to *long-term* effects, that is experience that has been accumulated over a long time (typically years; Jackson et al., 2006), and/or over a large number of encounters (typically several thousands, e.g. Güldenpenning, Schütz, Weigelt, & Kunde, in press).

The present study focuses on *short-term* effects of perceiving or producing fake actions. Specifically, we studied short-term influences on (i) the detection of fake actions of others, and (ii) on the mental specification of own fake actions. By 'short–term', we refer to the immediately preceding perception (or production) of a fake action prior to perceiving (or producing) a corresponding action of oneself or another person. In the present paradigm, these preceding encounters of corresponding movements date back no longer than a few seconds.

Previous research suggests that short-term influences on perception could be a consequence of cognitive conflict, triggered by the perception, and possibly production, of a fake action. Cognitive conflicts generally occur when mental codes of more than one stimulus or response alternative are concurrently active (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Botvinick, 2007). Interference tasks, such as the Stroop-task or the Eriksen-Flanker task examine such conflict. For example, in the Stroop-task there is conflict between a nominally irrelevant stimulus feature (word meaning) and a relevant stimulus feature (word color). We conjecture that observing a fake action creates a conflict as well, namely conflict between an initially suggested, though eventually irrelevant, and an actually executed, and thus relevant action. The experience of cognitive conflict has consequences on perceptual processing, namely a stronger focus on task-relevant information (Egner & Hirsch, 2005). For example, when participants encountered a target-incongruent flanker in an Eriksen Flanker task, they focus more on the task-relevant target in the next trial (Wendt, Luna-Rodriguez, & Jacobsen, 2012).

There is indirect evidence for similar perceptual consequences of processing head fakes in basketball. When participants respond to the pass direction of a virtual opponent, response times typically increase when the opponent does not gaze in the direction of passing (thus a head fake), than when head and gaze are aligned (no head fake, Kunde, Skirde, & Weigelt, 2011). However, if expert basketball players have encountered a head fake in the previous trial, this increase of response times is reduced in the next trial (Weigelt, Güldenpenning, Steggemann-Weinrich, Alaboud, & Kunde, 2017). This ‘fake adaptation effect’ mirrors congruency sequence effects in other interference tasks, in which interference effects are often reduced after incongruent trials (Gratton, Coles & Donchin, 1992; Kunde & Wühr, 2006). It is unclear, though, whether improved perceptual discrimination of fake and non-fake actions (i.e. basketball passes), or other processes, such as a reduced tendency to respond to the irrelevant head orientation, drives this adaption effect. Thus, direct evidence for improved *perceptual discrimination* of non-fake and fake actions after encounters of fake actions is missing.

Not only responding to fake actions, but also the production of such fake actions comes with a cost, in terms of increased initiation times. For example, in basketball, it takes not only longer to respond to a head fake of another person, but also to generate such a fake pass in the first place as compared with a pass without head fake (Güldenpenning, Weigelt, & Kunde, 2018). The reasons for such 'fake production costs' might be multiple, and they might be specific for the particular fake action. First, the motor programming demands of fake actions are probably higher. For the production of a head fake, head and arms move into different directions, and movements of effectors into different directions in general produce response-response incongruency costs (Hazeltine, 2005; Heuer, 1995; Peterson, 1965). Second, there are probably other sources of difficulty beyond peripheral motor processes. Faking violates the widely shared rule and prevalent tendency to be honest to other people, and rule violations come with a performance cost as compared to rule compliances, as do dishonest compared to honest responses (e.g., Debey, De Houwer, & Verschuere, 2014; Foerster, Wirth, Herbort, Kunde, & Pfister, 2017; Pfister, Wirth, Schwarz, Steinhauser, & Kunde, 2016; Wirth, Pfister, Foerster, Huestegge, & Kunde, 2016). Moreover, deceiving another person seems to require close monitoring of the consequences of these actions, which creates additional costs (Foerster, Wirth, Berghoefer, Kunde, & Pfister, in press; Walczyk, Roper, Seeman, & Humphrey, 2003; Wirth, Janczyk, & Kunde, 2018). Whatever the reasons for fake production costs beyond motor programming are, they should only occur, when fake actions are generated to intentionally deceive another person. In contrast, costs due to motor programming should occur irrespective of whether the related action aims to deceive another person, or not.

There is also evidence that producing a fake reduces the costs of subsequent faking (Debey, et al., 2014; Foerster, Wirth, Kunde, & Pfister, 2017, Foerster, et al., in press). Yet, these studies relied on tasks with a negligible motor component. Basically, participants were asked to either fake or not, and doing so was accomplished by pressing one of two response buttons. Whether fake production costs are reduced following more complex and natural faking actions, as is typically the case in sports, is currently unknown. Consequently, another question addressed in the present study is whether the previous production (or observation) of a fake movement can reduce the costs of planning a specific fake action, as compared to the previous production (or observation) of a non-fake action.

To study short-term influences of fakes on the detection of fake actions of others and on the production of own fakes, we engaged participants in an experimental game, which we called "fake ball". Basically, each trial type in the game consisted of the combination of two factors: A participant was either the "producer of", or "responder to" an action. This action was either a regular "non-fake" action or a "fake" action. Two participants faced each other at a table. Each of them had a small ball and a basket in front of them, while their hands rested on a home button (cf. Figure 1). In each trial, a visual stimulus signaled who of the two participants would be the producer and who would be the responder in that trial. The task of the producer was to throw, or to fake to throw, as quickly as possible, his/her ball into the basket of the opponent, according to a color stimulus. The task of the responder was to shield his/her own basket against the approaching ball with the hand that had rested on the home button, but to refrain from responding if the action of the opponent was a fake. Producers earned a credit, if they hit the opponents' basket or if the opponent responded to an own fake by leaving the home button. Responders earned a credit, if they defended a throw or did not respond to the producer's fake. Thus, the producer was rewarded for producing fake actions as authentic as possible, while the responder was rewarded for detecting such fakes as good as possible. Because this task loosely mimics the situation of an attacking basketball player, who aims to throw a ball into the basket, while facing a defensive player trying to prevent him/her from scoring, we refer to these two roles as "attacker" and "defender"[1].

We hypothesized that the previous observation of a fake action of an opponent, and possibly also the corresponding *self-observation* of an own previous fake, would facilitate the responders' subsequent discrimination of regular (non-fake) and fake actions. Moreover, the previous production of a fake action, and possibly also the corresponding *simulation* of a previously observed fake action of another person which typically accompanies such observation (cf. Gallese & Goldman, 1998; Schütz-Bosbach & Prinz, 2007), might reduce the subsequent costs of producing such a fake action, as compared to the production of a regular throw.

On top of answering these central questions, we aimed to better understand the causes of the fake production costs that we expected. As explained above, we conjectured that these costs do not only reflect more complicated motor programming of fake throws, as compared to non-fake throws, but might reflect additional costs of deceiving another person. Obviously, such costs come into play only when another person is involved, who is potentially deceived. Therefore, we also had a control condition (so called individual blocks) in which participants were asked to throw a ball, or to stop the throw before the ball left the hand, with the opponent turning his or her back towards the attacker, thereby removing any interaction between participants. We conjectured that increased fake production costs with than

[1] A video illustrating the task and data can be downloaded at https://osf.io/fjk25/

without another person being involved would be indicative of the contribution of cognitive costs of deception, in addition to (potential) motor programming costs.

## 2. Methods

### 2.1. Participants

We included 24 right-handed participants (two male, mean age 22 years) in the statistical analyses (one participant left the laboratory for a break; data of this dyad was replaced by a new pair of participants). All participants signed informed consent at the beginning of the experiment and received monetary compensation or course credit for their participation.

### 2.2. Stimuli and Apparatus

The experimental paradigm featured a ball-throwing task at a table. Two participants sat on opposite sides of the table, with the front side of their torsos heading towards the back of their chairs. The experimenter sat on one side of the table and a computer and its monitor stood at the opposite of the experimenter. A little basket served as a goal. Two buttons, of which one had an upside-down crown cap on top and two light-emitting diodes (LEDs; yellow and red), were fixed on each long side of the table, respectively (see Figure 1). Participants placed the chair at a position where their fingertips could reach the backside of the closer basket. The right index fingers of each participant lay on the response close to the edge of the table. Two soft balls (i.e., hacky sacks) lay on top of upside-down crown caps for better stability on the buttons close to the baskets. Participants had to throw the ball into the distal basket. Participants movements were filmed by a camera positioned behind the experimenter[2].

Participants received a validated German version, the *Saarbrückener Persönlichkeitsfragebogen v6.1* (SPF; Paulus, 2009), of the interpersonal reactivity index (IRI) (Davis, 1980) to assess trait empathy, the *Reading the Mind in the Eyes* (RME) test to assess theory of mind, and the *Inclusion of Others in the Self* (IOS) test to measure interconnectedness of paired participants before and after the game. Finally, participants were asked to indicate which strategies they used to make the distinction of their throws and fakes more difficult for their opponents and to distinguish between throws and fakes of their opponent.

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[2] Due to technical problems, only video data of nine dyads were recorded completely. Therefore, video data was not further analyzed.

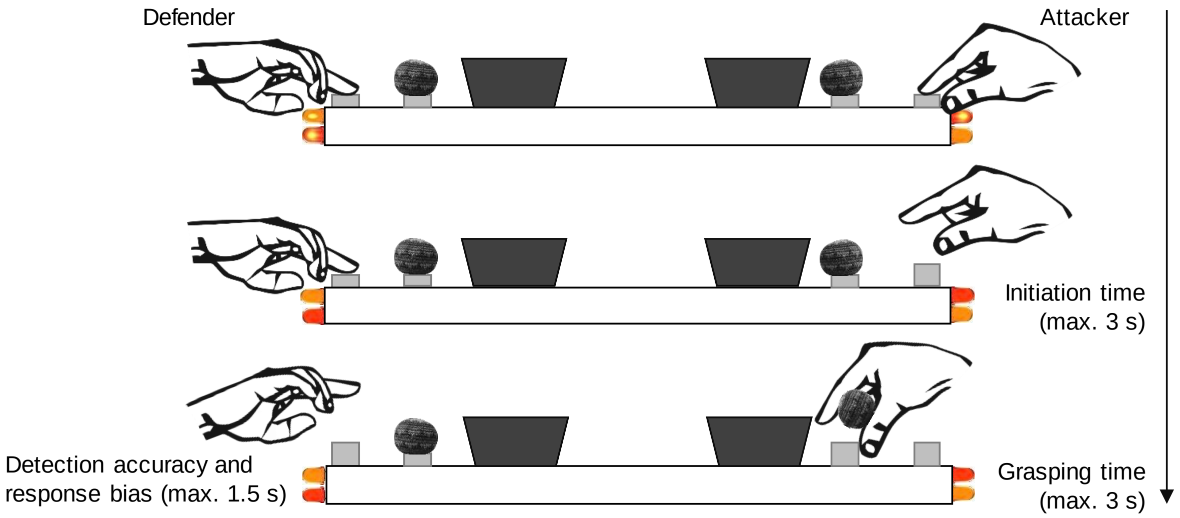


**Figure 1.** Exemplary trial procedure with a defending player on the left and an attacking player on the right. When both participants pushed their home buttons, one of the two lamps turned on to indicate a throw or fake for the current attacking player (upper row, right side), whereas both lamps turned on for the current defending player (upper row, left side). Participants could only see the two lamps in front of them, respectively. Attacking players had to release their button within 3 seconds (s) and grasp the ball (and release the respective ball button) within another 3 s (middle and lower row, right side). The time that passed between the action signal and the release of the finger denoted the initiation time and the time interval between finger and ball release was the grasping time. The ball release triggered a 1.5 s window to capture the reaction of defending players (lower row, left side), i.e., whether they kept pushing or released the button, and to determine detection accuracy and response bias.

### 2.3. Procedure

Upon arrival, participants first received the questionnaires. Participants were instructed that they would play a ball-throwing game and would alternate between two modes: playing against each other in competition blocks in which points could be earned, or playing individually in individual blocks without points. Participants were told to only use their right hand to attack and defend.

In competition blocks, both participants faced the table with their dominant, right hand index finger on the home button and their left hand on their back. A trial started when both balls lay on their buttons and participants hands rested on the home button (see Figure 1). The onset of the LED lamps served as a go-signal, which specified relevant events for the upcoming trial. The number of LEDs indicated whether a player would have to attack or defend in the next trial. For the defending player, both lamps turned on, while only one of the two lamps turned on for the attacking player. The color of the LEDs indicated whether the attacking player would have to throw the ball or to fake a throw in the next trial (the assignment of colors to actions was counterbalanced across participants). Participants were informed about errors by a corresponding feedback presented on the screen (2 s). More specifically, a trial was counted as erroneous, if attacking players did not release their home button within 3 s after the go-signal or if she/he did not grab the ball within 3 s after response initiation. Likewise, if defending players released their finger before attacking players or if the ball of either player slipped from its button at any time, a corresponding error message appeared. At the end of a trial, the experimenter indicated whether the attacking player selected the correct action (throwing vs. faking). In case of a false action, an appropriate error message appeared. In case of a successful throw, the experimenter also indicated whether the ball had hit its goal.

The score of each participant appeared on the display until the experimenter initiated the next trial. For throwing actions, attacking players received a point if the ball hit the goal. In contrast, defending players had to clear the ball. More specifically, only if they released their home button within a critical time window (1.5 s from ball grasping of attacking players) and if the ball of the attacking player did not hit the goal, they received a point. For fake actions, attacking players had to pick up the ball and pretend to throw it. Attacking players received a point if defending players released their home button (within 1.5 s from ball grasping of attacking players). However, if defending players kept their home buttons pushed, they received a point for themselves.

In individual blocks, only one participant performed throwing and stopping actions, while the other person turned her back to the table. Each individual block started with one participant, followed by the other participant in the second half of the block. The instruction explicitly distinguished between fakes during competition blocks and stops during individual blocks. Everything else was identical to trial sequences in competitions blocks.

The time interval between the onset of the go-signal and the release of the home button of the attacking player denoted the initiation time. The time interval between the release of the home button and the lifting of the ball from its button denoted the grasping time.

We counterbalanced whether participants started with an individual block or a competition block and which participant (left or right) was first in individual blocks. Participants first went through two practice blocks à 16 trials (one competition and one individual block). Ten individual blocks and ten competition blocks alternated with self-paced breaks in between. In competition blocks, each participant had to perform ten throws and ten fakes, resulting in 40 (2 players x 20 actions) randomly arranged trials in each block. In individual blocks, one participants started with a random sequence of ten throws and ten stops, followed by the other participant. At the beginning of each block, the computer screen informed participants and the experimenter whether the next block would be an individual or competition block. At the end of the experiment, participants received a second IOS and had to write down any strategies.

### 2.4. Data treatment

We excluded all practice trials and the first trial after each break. The remaining trials were correct in 99.7% of the individual blocks and 95.6% of the competition blocks. Erroneous trials comprised of response anticipations of defending players in competition blocks (1.9%), slips of the ball (individual: < 0.1%, competition: 1.6%), wrong action choices of the attacking player (i.e., throw when fake was instructed or vice versa; individual: 0.2%, competition: 0.7%) and omission errors (individual: < 0.1%, competition: 0.1%). Since error rates were low, we did not analyze them further and excluded all error and post-error trials from the analyses.

## 3. Results

### 3.1. Detection accuracy and response bias of defending players

We aggregated responses of defending players to fakes and throws in competition blocks separately for fakes and throws in the preceding trial (preceding action) and for the role of the defending player in the preceding trial (defending player vs. attacking player). As such, we looked at detection accuracy of defending players as a function of whether they observed (preceding role: defending player) or executed (preceding role: attacking player) either a regular throw (i.e. non-fake action) or a fake throw (i.e. fake action) in the preceding trial. Throw trials (signals) in which defending players released their home buttons in the critical time window were counted as hits. In fake trials (noise), responses of defending players were counted as false alarms. Hit rates were computed as number of hits / number of signals and false alarm rates as number of false alarms / number of noise trials, separately for each experimental cell and participant. We corrected values of 0 or 1 in both, hit and false alarm rate to 0.05 and 0.95, respectively, before computing z-values (cf. Desender, van Opstal, & van den Bussche, 2014). This correction was necessary for four participants. We computed the discriminability index d' (i.e., detection accuracy of throws and fakes by releasing or pressing the home button) as z(hit rate) – z(false alarm rate) and the response bias index c (i.e., bias to release or press the response button) as -0.5 * [z(hit rate) + z(false alarm rate)] for each experimental cell and participant, respectively. Table 1 in the Appendix shows descriptive statistics of hit and false alarm rates, d' and c for each design cell.

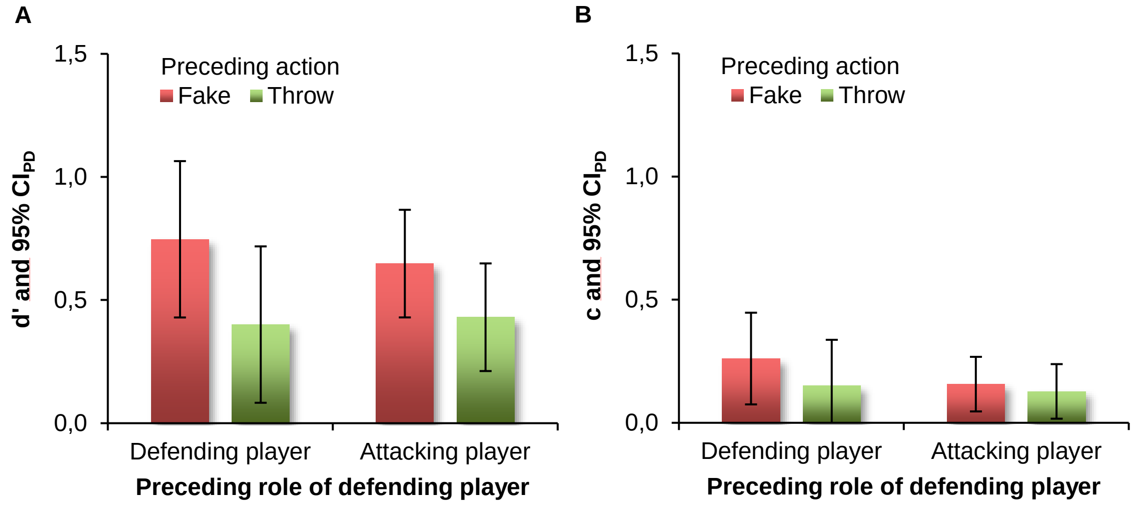


**Figure 2.** Mean discrimination index d' **(A)** and response bias index c **(B)** of defending players in competition blocks for fakes and throws in the preceding trial as a function of the role of defending players in the preceding trial (defending player vs. attacking player). Discrimination of fakes and throws was better than chance and improved similarly after observing (preceding role: defending player) and committing fakes (preceding role: attacking player). Error bars represent the 95% confidence interval of paired differences ($CI_{PD}$; Pfister & Janczyk, 2013), computed separately for each preceding role of defending player.

The signal detection indices d' and c were analyzed in separate 2x2 repeated-measures analyses of variance (ANOVAs) with the factors preceding action (fake vs. throw) and preceding role of defending player (defending player vs. attacking player). We also report the results of the constant term of these analyses to evaluate whether d' or c deviated significantly from 0. Two-tailed paired-samples *t*-tests were used to further scrutinize significant interactions.

Participants discriminated fakes and throws better than chance, indicated by a significant constant term (see Figure 2A), $F(1, 23) = 15.14$, $p = .001$, $\eta_p^2 = .40$. Most interestingly, fakes compared to throws in the preceding trial improved detection accuracy, $F(1, 23) = 13.36$, $p = .001$, $\eta_p^2 = .37$. None of the other effects was significant, *F*s < 1.

There was only a non-significant trend toward a conservative response bias to rather press than release the home button (see Figure 2B), $F(1, 23) = 4.18$, $p = .053$, $\eta_p^2 = .15$. None of the other effects were significant, $Fs(1, 23) \leq 1.50$, $p \geq .234$, $\eta_p^2 s \leq .06$.

## 3.2. Performance of attacking players in competition blocks

We aggregated initiation time (IT) and grasping time (GT) of attacking players separately for the role of the attacking player in the preceding trial (defending player vs. attacking player), current action (fake vs. throw) and preceding action (fake vs. throw). As such, we looked at performance measures of throws and fakes of attacking players as a function of whether they observed (preceding role: defending player) or executed (preceding role: attacking player) a fake or a throw in the preceding trial. We excluded trials with initiation or grasping times that deviated more than 2.5 standard deviations from their respective cell mean as outliers (4.2%). Table 2 in the Appendix shows descriptive statistics of initiation and grasping time for each experimental cell. We analyzed initiation and grasping time in separate 2x2x2 repeated-measures ANOVAs with the factors preceding role (defending player vs. attacking player), current action (fake vs. throw) and preceding action (fake vs. throw). We scrutinized significant three-way interactions in separate 2x2 ANOVAs for role repetitions and role switches and significant two-way interactions in two-tailed paired-samples *t*-tests.

Initiation times were longer for fake actions than for regular throws, $F(1, 23) = 78.16$, $p < .001$, $\eta_p^2 = .77$, reflecting a fake production cost. Moreover, ITs were longer after throws than after fakes, $F(1, 23) = 20.80$, $p < .001$, $\eta_p^2 = .48$. The two-way interaction of current and preceding action was not significant, $F < 1$, which suggests that neither the production nor observation of a fake helped to reduce fake production costs. None of the other effects were significant, $F(1, 23) \leq 3.14$, $p \geq .090$, $\eta_p^2$s $\leq .12$.

Grasping times were longer when attacking players also attacked than when they defended in the preceding trial, $F(1, 23) = 5.74$, $p = .025$, $\eta_p^2 = .20$, and prolonged grasping times emerged after fakes than after throws, $F(1, 23) = 11.10$, $p = .003$, $\eta_p^2 = .33$. Again, the two-way interaction of current and preceding action was not significant, $F < 1$. None of the remaining effects was significant, $F$s$(1, 23) \leq 2.00$, $p \geq .171$, $\eta_p^2$s $\leq .08$.

### 3.3. Performance of attacking players: Competition vs. individual condition

To have a fair comparison of performance in individual and competition blocks, we selected trials in which attacking players also attacked in the preceding trial, to account for the fact that players did not switch from trial to trial in individual blocks. We then aggregated initiation time and grasping time of attacking players separately for play modes (individual blocks vs. competition blocks), current action and preceding action (stop/fake vs. throw). Trials with initiation or grasping times that deviated more than 2.5 standard deviations from their respective cell mean were excluded as outliers (4.1%). Table 3 in the Appendix shows descriptive statistics of initiation and grasping time for each design cell. We analyzed initiation and grasping time in separate 2x2x2 repeated-measures ANOVAs with the factors play mode (competition blocks vs. individual blocks), current and preceding action (stop/fake vs. throw).

The means of this analysis are shown in Figure 3A. We scrutinized significant three-way interactions in separate 2x2 ANOVAs for individual and competition blocks and significant two-way interactions in two-tailed paired-samples *t*-tests.

Initiation times were longer in competition blocks than in individual blocks (see Figure 3A), $F(1, 23) = 107.41$, $p < .001$, $\eta_p^2 = .82$, and for fakes/stops compared to throws, $F(1, 23) = 68.08$, $p < .001$, $\eta_p^2 = .75$. Both factors interacted, $F(1, 23) = 23.60$, $p < .001$, $\eta_p^2 = .51$, indicating that the difference between fakes and throws in competition blocks, $t(23) = 8.10$, $p < .001$, $d_z = 1.65$, was indeed larger than the difference between stops and throws in individual blocks, $t(23) = 5.84$, $p < .001$, $d_z = 1.19$. Throws compared to stops/fakes in the preceding trial prolonged initiation times, $F(1, 23) = 47.31$, $p < .001$, $\eta_p^2 = .67$, and this main effect of preceding action was also qualified by play mode, $F(1, 23) = 26.21$, $p < .001$, $\eta_p^2 = .53$. The difference in initiation times after throws compared to fakes in competition blocks, $t(23) = 4.04$, $p = .001$, $d_z = 0.83$, was smaller than the difference after throws compared to stops in individual blocks, $t(23) = 8.06$, $p < .001$, $d_z = 1.64$. Although we selected only trials of competition blocks that were preceded by players own actions, it seems possible that memories of own previous fakes and throws were slightly less distinct in competition blocks, where they occurred randomly mixed with actions of the other person, as compared to individual blocks where own stops and throws were the only possible trial types. Neither, the two-way interaction of current and preceding action, nor the three-way interaction, were significant, *F*s < 1.

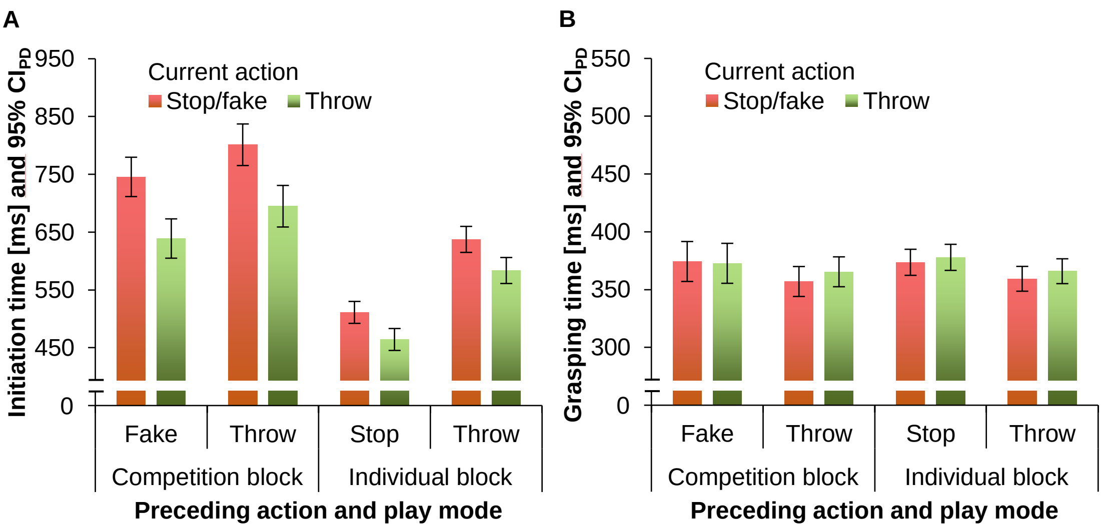


**Figure 3.** Mean initiation time **(A)** and grasping time **(B)** of attacking players (who also attacked in the preceding trial) for fakes and throws in the current and preceding trial of competition and individual blocks. Error bars represent the 95% confidence interval of paired differences, computed separately for each preceding action and play mode.

Grasping times were longer after stops/fakes than after throws (see Figure 3B), $F(1, 23) = 23.01$, $p < .001$, $\eta_p^2 = .50$. None of the remaining effects was significant, $Fs(1, 23) \leq 2.13$, $p \geq .158$, $\eta_p^2 s \leq .09$.

### 3.4. Exploratory analyses

We averaged the signal detection parameters d’ and c across conditions. We also averaged initiation times from our competition vs. individual condition analyses across preceding action. We then computed faking costs (fake – throw) and stopping costs (stop – throw) in initiation times and subtracted the latter from the former to approximate the faking effect (ΔFaking). Descriptive data of these measures as well as of the questionnaire data, namely SPF empathy[3], ΔIOS and RME scores are in Table 4 in the Appendix. None of these measures correlated significantly, *Pearson* $rs \leq .40$, $ps \geq .053$. Participants reported various, unsystematic strategies to conceal and detect fakes.

Another interesting question is whether defenders used attackers’ initiation times to distinguish between attackers’ real and fake actions. To test that we computed logistic regressions with the binary dependent variable of classification (fake or throw) and the independent variable of initiation time of the attacker. We extracted individual regression slopes for each participant in the role of defender and tested these against 0 in a two-tailed one-sample *t*-test (cf., Lorch & Meyers, 1990; Pfister, Schwarz, Carson, & Janczyk; 2013). Longer initiation times of attackers indeed significantly increased the probability that defenders classified these actions as fakes (mean slope = 0.0009, standard deviation = 0.0002, $t(23) = 3.52$, $p = 0.002$, $d_z = 0.72$). Thus, prolonging initiation times by 1 ms increased the chance to classify attacks as fake rather than throw by 0.1% (Odds ratio [OR] = $e^{0.0009*(1-0)} = 1.001$), and prolonging initiation times by 100 ms increased this chance by 10.5% ($OR^{100} = 1.105$).

## 4. Discussion

The present study explored two main questions: First, how does the previous observation of an opponent’s fake action or the production of an own fake action impact the subsequent discrimination between fake and non-fake actions? Second, how does the previous observation or production of a fake movement impact the own production of fake actions as compared to non-fake actions?

[3] The SPF empathy score included only 23 participants as one participants missed an item of the SPF. Accordingly, correlations with the measure empathy include 23 participants, while all other correlations considered the whole sample.

Regarding the first question, we found that a previous encounter of a fake action improved discrimination between fake and non-fake actions of opponents as compared to previous encounters of non-fake actions. Importantly, fake actions do not simply induce a response bias towards judging subsequent actions as fakes, which would be indicated by a change of response bias parameter c, but they improved the discrimination between fake and non-fake actions, as indicated by an increase of the d' parameter. Thus, encountering a fake action induces a short-term perceptual benefit for telling apart fake from non-fake actions. This is different from the impact of long-term motor expertise on action observation, which induces a response bias towards judging an action as deceptive (Cañal-Bruland & Schmidt, 2009).

Interestingly this effect was independent of whether the previous fake action was observed or performed. This finding converges with the idea that the observation of other people's actions prompts an internal simulation of these movements in the observer (Schütz-Bosbach & Prinz, 2007). Thus, the production and observation of actions rely on the same processes and representations (e.g. Pfister, Dignath, Hommel, & Kunde, 2013; Dignath & Eder, 2013). We conjecture, that the observation of an own fake action or that of another person helps to focus on those action components that distinguish them from regular throws (i.e. non-fake actions) (Cavallo, Koul, Ansuini, Capozzi, & Becchio, 2016). One cue that biases observers to judge an attacker's action as fake is the increased time it takes the attacker to initiate a fake action rather than a regular action (see logistic regression of defenders' judgments as a function of initiation times of the attacker). Another diagnostic cue might be a perhaps stronger deceleration of the hand by the end of the movement with a fake rather than a regular throw. However, such a potential difference in movement execution could not show up in the temporal measures we employed here, which limits possible conclusions. To reveal such cues in movement execution would require more fine grained motion tracking, which is certainly a worthwhile topic of future research.

Regarding the second question, we observed that fake movements took longer to initiate as compared to regular throws. This may not come as a big surprise, since the required motor programs for fake throws are likely more complex than those for a regular throw. However, other processes relating to the differences on top of 'motoric' differences likely contribute to this cost. This is indicated by the comparison of performance in fake throws as compared to regular throws when another person was potentially deceived (competition blocks) than when no other person was involved (individual blocks).

Initiation times were generally higher, and the difference between fake and non-fake actions was larger, when another person, who could be potentially deceived, was present rather than absent. We suggest that actors mentally prepare own actions by anticipating the intended perceptual consequences of these actions, and that this anticipation takes time. The idea that movements are planned by codes of their perceptual consequences, including the responses of other people, is supported by much evidence (e.g. Pfister et al., 2013; Pfister, Weller, Dignath, & Kunde, 2017; Weller, Pfister, & Kunde, in press). With an averted partner (i.e. without the interaction of two people), no partner response can be intended, which spares time compared to situations with a facing and thus potentially responsive partner. Moreover, with a facing partner a fake action intends to produce a response of the partner (a premature defense response of the partner), whereas a regular throw aims at exactly the opposite, namely no response of the partner (no defense action). Assuming that anticipating, or imaging, a response of the partner takes longer than anticipating or imaging no response, this would explain the larger difference between fake and regular throws with another, potentially deceived, person involved. This speculation is derived from a framework on sociomotor action control we have suggested recently (Kunde, Weller, & Pfister, 2018), but more work is necessary to empirically support this speculation, which is beyond the scope of the present study.

The previous production or observation of a fake throw did not reduce the costs of producing fakes as compared to regular throws, indicated by an effect of previous type of action (regular vs. fake throw) on initiation times. Previous fakes speeded up the initiation of subsequent actions, but they did so equally for regular and fake throws (cf. Figure 3A). This outcome was somewhat unexpected, given that previous research had shown that faking can reduce the costs of subsequent faking as compared to non-faking (Debey et al., 2014; Foerster et al., 2017, in press). Moreover, participants should be highly motivated to make use of every means to alleviate differences in initiation times, as these provided defenders with a cue to distinguish between real and fake throws that they actually used. We conjecture that the time interval between trials might be crucial to explain this discrepancy (Egner, Ely, & Grinband, 2010). Whereas previous studies used constant intertrial intervals (ITIs) of typically less than one second, the ITIs in the present study were somewhat longer and more variable, depending on the landing position of the ball, and sometimes necessary repositioning of the balls to their start positions. Testing this assumption would require a more constrained setup with fixed ITIs, which likely reduces the resemblance to real faking scenarios, e.g. in sports, for which we were aiming at in the present setup. Moreover, there might be subtle spatial and velocity-based differences by the end of regular and fake actions that the present setup, which primarily relied on initiation times, did not capture. Therefore, more sensitive designs might reveal after effects of previous fake observation or own fake production on such movement-based measures.

Altogether, there is thus an interesting dissociation of previous fake experience on the detection and production of fake actions: Previous encounters of fake actions, self- or other-produced, facilitate the perceptual discrimination of fake and regular actions, whereas such encounters did not facilitate the subsequent production of fakes compared to regular actions. In other words, fake actions have a short-term influence on the perceptual detection of subsequent fake actions, but not on the mental process of planning such actions, as captured by initiation times.

We focused here on fake experience that dated back just one trial before, since this is the most recent experience the actor could possibly have. Of course it might be interesting to look at influences of previous fake encounters that date back longer such as two, three or more trials (Aben, Verguts, & Van den Bussche, 2017).

Assuming that our observations generalize to real sports situations, which of course has to be carefully tested in future research, they have implications for sports practice. The (self-)observation of a fake action briefly helps defenders to discriminate fake from non-fake actions. Consequently, the recommendation from this research is to not fake twice in a row. Doing so will help defenders to detect your next fake action, whereas it will not help yourself to produce your next fake action.

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## 6. Appendix

**Table 1.** Mean hit rates and false alarm rates of defending players in percent and the appropriate mean discrimination indices d' and response bias indices c (with standard deviations, SD), as a function of preceding action (fake vs. throw) and preceding role of defending player (defending player vs. attacking player).

| Preceding role of defending player | Preceding action | Mean hit rate (SD) | Mean false alarm rate (SD) | Mean d' (SD) | Mean c (SD) |
|---|---|---|---|---|---|
| Defending player | Fake | 55 (19.89) | 30 (23.51) | 0.75 (0.9508) | 0.26 (0.5075) |
| | Throw | 53 (20.08) | 38 (18.99) | 0.40 (0.6882) | 0.15 (0.4845) |
| Attacking player | Fake | 56 (14.84) | 34 (21.08) | 0.65 (0.6380) | 0.16 (0.4358) |
| | Throw | 54 (16.75) | 38 (25.27) | 0.43 (0.8601) | 0.13 (0.4770) |

**Table 2.** Mean initiation and grasping times of attacking players in milliseconds (with standard deviations, SD) in competition blocks as a function of preceding role of attacking player (defending player vs. attacking player), current and preceding action (fake vs. throw).

| Preceding role of attacking player | Preceding action | Current action | Initiation time (SD) | Grasping time (SD) |
|---|---|---|---|---|
| Defending player | Fake | Fake | 771 (149.45) | 356 (98.51) |
| | | Throw | 665 (159.57) | 364 (94.09) |
| | Throw | Fake | 821 (163.02) | 349 (94.81) |
| | | Throw | 699 (178.09) | 354 (85.28) |
| Attacking player | Fake | Fake | 746 (184.81) | 374 (118.56) |
| | | Throw | 639 (169.77) | 373 (98.96) |
| | Throw | Fake | 801 (169.78) | 357 (104.17) |
| | | Throw | 695 (203.38) | 365 (98.30) |

**Table 3.** Mean initiation and grasping times of attacking players in milliseconds (with standard deviations, SD) as a function of play mode (competition blocks vs. individual blocks), current and preceding action (stop/fake vs throw) for trials with the same attacking player in the current and preceding trial.

| Play mode | Preceding action | Current action | Initiation time (SD) | Grasping time (SD) |
|---|---|---|---|---|
| Competition blocks | Fake | Fake | 746 (184.81) | 374 (118.56) |
| | | Throw | 639 (169.77) | 373 (98.96) |
| | Throw | Fake | 801 (169.78) | 357 (104.17) |
| | | Throw | 695 (203.38) | 365 (98.30) |
| Individual blocks | Fake | Fake | 511 (130.49) | 374 (89.83) |
| | | Throw | 464 (130.08) | 378 (90.05) |
| | Throw | Fake | 638 (144.01) | 359 (94.29) |
| | | Throw | 584 (147.86) | 366 (83.95) |

**Table 4.** Mean discrimination accuracy index d', response bias index c, adjusted faking costs (ΔFaking), empathy score of the *Saarbrückener Persönlichkeitsfragebogen* (SPF), difference score of the *Integration of Others in the Self* tests (ΔIOS) and the *Reading the Mind in the Eye* (RME) score (with standard deviations, SD).

| d' | c | ΔFaking | SPF empathy | ΔIOS | RME |
|---|---|---|---|---|---|
| 0.56 (0.7007) | 0.17 (0.4172) | 56 (56.58) | 41 (4.25) | -0.71 (1.3300) | 25 (3.29) |