

My Command, My Act: Observation Inflation in Face-To-Face Interactions

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ABSTRACT

When observing another agent performing simple actions, these actions are systematically remembered as one's own after a brief period of time. Such *observation inflation* has been documented as a robust phenomenon in studies in which participants passively observed videotaped actions. Whether observation inflation also holds for direct, face-to-face interactions is an open question that we addressed in two experiments. In Experiment 1, participants commanded the experimenter to carry out certain actions, and they indeed reported false memories of self-performance in a later memory test. The effect size of this inflation effect was similar to passive observation as confirmed by Experiment 2. These findings suggest that observation inflation might affect action memory in a broad range of real-world interactions.

KEYWORDS

memory bias,
action observation,
observation inflation,
motor simulation

INTRODUCTION

Working interdependently on a task comes with a range of challenges, not only for immediate coordination, but also in terms of long-term processes that rely on the memory of each individual agent. For one, performance can be improved if individuals memorize who knows what about which subject, instead of trying to encode all information by themselves. And indeed, research suggests that human agents spontaneously memorize the fields of expertise of other group members and use this knowledge to improve performance (Brandon & Hollingshead, 2004; Wegner, 1986; Wegner, Giuliano, & Hertel, 1985). For another, successful task performance is only possible if each agent keeps track of who has performed which actions. This built-up and maintenance of “agent-action bindings” seems to be rather difficult and error-prone (Earles, Kersten, Curtayne, & Perle, 2008; Loftus, 1976). A peculiar memory error involving agent-action bindings can occur whenever someone merely observes another agent performing an action. In this situation, observers will show a consistent bias to remember the observed actions as their own, a phenomenon that is called *observation inflation* (Lindner, Echterhoff, Davidson, & Brand, 2010).

A typical experimental design to study observation inflation proceeds in three different phases: In the first phase, participants are

presented with short action statements (e.g., “Shake the bottle”). They are either asked to perform the described actions themselves or to read the corresponding action statements without actually performing the actions. Some of these actions are reused in the following second phase, in which participants typically watch videos showing an actor performing some actions that the participants performed and some that they did not perform in the first phase of the experiment. The third phase follows after a retention interval to probe the participants' memory. In this final phase, participants are to indicate for each action statement whether or not they had performed the action in the first phase. Observation inflation becomes evident in a higher proportion of affirmative responses (“Yes, I did perform this action.”) for those items that had been observed in the second phase as compared to those that had not been observed. Even though this pattern was also found for actions that had actually been performed, critically, it was found to apply even to actions that were merely read before—that is, partici-

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pants systematically claimed to have performed actions that they had only observed another person performing.

Observation inflation has been reported in a number of settings, suggesting that this memory bias is a robust phenomenon (e.g., Foley, Passalacqua, & Ratner, 1993; Lindner, Schain, Kopietz, & Echterhoff, 2012; Sommerville & Hammond, 2007). Yet, the experimental approach outlined above comes with two limitations, especially when studying adult samples (e.g., Lindner et al., 2010, 2012). First, previous studies restricted the participants to be passive observers rather than studying interactive settings. Second, observed actions were mostly displayed via standardized videos rather than live performance of another agent. These videos were focused entirely on the action—that is, they only showed a close-up view of the actor's hands and the relevant object and did not include any additional details. Even though observation inflation can be induced reliably in these settings, they do not allow gauging whether similar inflation effects would occur when observing another's action in a real-world interaction. In fact, observation inflation was found to be reduced considerably when the video stimuli were enriched to feature also the actor's face—a feature that is likely to be present in many real-world interactions (Schain, Lindner, Beck, & Echterhoff, 2012). Studies on children cast further doubt on the generalizability of observation inflation. These studies employed direct interactions of the participating children and the experimenter, and reported robust evidence for observation inflation in preschool children (Foley et al., 1993; Sommerville & Hammond, 2007) whereas there is only mixed evidence for older children (Foley et al., 1993; Foley, Ratner, & House, 2002).

Such developmental findings were taken to suggest that observation inflation derives from actively imagining oneself performing the observed actions (Foley et al., 2002; Ratner, Foley, & Gimpert, 2002; Sommerville & Hammond, 2007). A similar account in terms of egocentric motor simulation was also proposed to underlie observation inflation in adults (Lindner et al., 2010). In line with this proposal, observation inflation was found even for perceptually impoverished video stimuli that were mostly reduced to motion cues (Lindner, Schain, & Echterhoff, 2016). The same study showed that performing incongruent as compared to congruent movements, to counter motor simulation, decreased observation inflation, suggesting that motor simulation plays a key role for inflation effects to occur. Whether or not direct, face-to-face interactions give rise to sufficient motor simulation to induce observation inflation in human adults remains to be explored, however, and this was the goal of the present experiments.

A particularly relevant type of real-world interactions that involve action observation are tasks in which one agent commands another agent to perform a certain action. Following previous findings on observation inflation, the observer who commanded the action might remember the observed action actually as their own deed. To determine whether observation inflation indeed occurs for commanded actions in direct, face-to-face interactions, Experiment 1 adopted the general experimental design described above. However, participants did not observe action videos during the observation phase; instead, they actively commanded the experimenter to perform the actions.

EXPERIMENT 1: COMMANDED ACTIONS

In Experiment 1, we adopted the three-phase design of previous studies on observation inflation (Lindner et al., 2010, 2012; Schain et al., 2012) but studied observation inflation during direct, face-to-face interactions in which participants commanded the experimenter to perform the actions. More precisely, in Phase 1, participants were presented with action statements which they either acted out or read out loud. Half of these statements were reused in Phase 2 in which participants saw a choice display with two different action statements in each trial. They were instructed to choose one of these statements and command the experimenter to perform the action. Participants were asked to choose between two possible actions to ensure a sufficient degree of agency for their commands. Phase 3 was a memory test that took place after a retention interval of about two weeks. For each item, participants were asked whether or not they had performed the corresponding action during Phase 1. We analyzed the percentage of affirmative responses (“Yes, I did perform this action.”) as a function of actual performance in Phase 1 and as a function of whether or not the action had been commanded in Phase 2. Observation inflation, if present, should manifest as an impact of command on the percentage of affirmative responses.

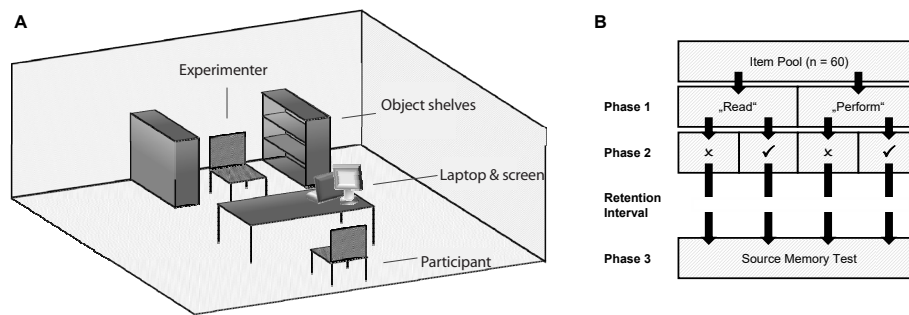
In addition to measuring observation inflation in terms of the described memory test, we administered a debriefing questionnaire and three standardized questionnaires after Phase 2. The debriefing questionnaire served as a manipulation check, whereas the remaining questionnaires were designed for exploratory analyses. They measured constructs that likely mediate a participant's likelihood of engaging in active motor simulation for commanded actions as derived from studies on empathy (Aron, Aron, & Smollan, 1992; Paulus, 2009) and interpersonal power (Gruenfeld, Inesi, Magee, & Galinsky, 2008; see the Methods section for details).

Methods

PARTICIPANTS, APPARATUS, AND STIMULI

We recruited a sample of 24 participants ($M_{\text{age}} = 22.6$ years, 18 female). This sample size ensures a high statistical power of $1-\beta = .99$ to detect observation inflation effects of previous studies (e.g., $d_z = 0.93$ as computed from the F -statistic reported in Experiment 1 of Lindner et al., 2010), assuming a two-tailed test and a significance level of $\alpha = .05$ (power calculations were done with the `power.t.test` function of R 3.3.0). Furthermore, should the effects of observation inflation for the present face-to-face interactions be weaker than in previous, video-based studies, the sample size would still allow for a power of $1-\beta \geq .80$ for medium effect sizes of $d_z \geq 0.60$.

Figure 1 shows a schematic of the experimental setup. Participants were tested individually by a female experimenter. Participant and experimenter sat facing each other at a table and the experimenter had a laptop computer placed to her left. A second monitor was connected to the laptop but turned towards the participant. Two shelves were placed to the experimenter's left and right; each contained 30 test items that

**FIGURE 1.**

Apparatus and procedure of the experiments. A = The experimenter and the participant sat face to face at a table; the experimenter further had access to all relevant objects in two shelves to her left and right. A laptop computer was turned towards the experimenter, whereas the participant had view of an external screen. B = The experiment used an item pool of 60 items describing simple object-oriented actions (e.g., “Flasche schütteln,” German for “Shake the bottle,” see the Appendix for a complete list). For each participant, the item pool was randomly split into 30 items that the participant simply read during Phase 1, and 30 items that were to be performed. Of each subset, 15 items were randomly selected for further use in Phase 2, in which the participant commanded the experimenter to perform the corresponding actions (Experiment 1) or observed the experimenter perform them (Experiment 2). The remaining 15 items of each subset did not appear in Phase 2. In Phase 3, we probed the participants’ memory by asking whether he or she had performed each action in the preceding session.

were arranged for easy access during testing. The experiment consisted of three phases, with Phases 1 and 2 immediately following one another, and Phase 3 after a retention interval of about 2 weeks (average: 13.8 days; $SD = 0.89$, range: 11–15 days).

Procedure

PHASE 1

Each trial of Phase 1 started with the experimenter placing an object on the table. The participant then saw either the instruction “Please read:” or the instruction “Please perform:,” accompanied by the item (e.g., “Shake the bottle”; for a complete list of the items, see Table A1 in the Appendix).

Reading instructions prompted the participant to read out loud the action phrase on the screen (e.g., saying “Shake the bottle.”) without using the object on the table. Performance instructions, by contrast, prompted the participant to carry out the action and place the object back on the table afterward. The experimenter then stored the object at its previous location and terminated the trial by pressing the space bar in case participants adhered to the instruction or by pressing the return key in case anything special happened in the course of the trial (e.g., if participants erroneously started to perform the action after having been instructed to read the phrase; < 0.1%). Phase 1 consisted of 60 trials—that is, each item was presented once with either a read instruction (30 items) or a perform instruction (30 items). The mapping of items to instructions was randomized across participants.

PHASE 2

Phase 2 followed after a short break and used a reduced item set of 30 items: Fifteen items that had been read in Phase 1 and 15 items that had been performed in Phase 1. Trials were now started by the

participant who was presented with two statements (e.g., “Shake the bottle,” “Ring the bell”). He or she chose one of the two statements and commanded the experimenter to perform the action. The experimenter fetched the corresponding objects from the shelves, performed the action, and terminated the trial afterward.

A randomization routine ensured that each of the 30 items was commanded 2–3 times in the course of Phase 2 if possible (we opted for repeatedly presenting the items following previous methods; cf. Lindner et al., 2010). More precisely, the algorithm selected the current pair of two items randomly from the pool of 30 pairs with the restriction that none of the two items was allowed to match the previously commanded action. Items were removed from the pool if they had been selected a total number of three times during Phase 2. The program terminated when either only one type of action was left or, alternatively, if a valid trial could not be formed within 100,000 iterations of the randomizer. This procedure ensured a mean choice frequency of 2.86 times for each item, with a total of 5.72 presentations as a potential choice option, resulting in 84–86 trials per participant in Phase 2 (85.8 trials on average). One participant did not choose a particular option throughout Phase 2 (“Tear off some toilet paper.”) and another participant did not choose two options (“Disassemble the pen,” “Insert the card into the envelope”). These items were removed from the analysis for the corresponding participants.

At the end of the session, participants were asked to fill out a debriefing questionnaire. This questionnaire featured four items that had to be answered on a rating scale ranging from 1 (*not at all true*) to 7 (*completely true*), with verbal labels attached to the poles of the scale. The four items were: (1) “I had the impression that the experimenter and I jointly performed the task,” (2) “In the second part, I had the impression that the experimenter acted to my command,” (3) “When observing the actions of the experimenter, I tried to put myself in her

position,” and (4) “I would have performed the actions differently than the experimenter.” Additionally, we administered the three questionnaires for exploratory analyses: the Inclusion of Other in the Self scale (IOS; Aron et al., 1992), targeting the perceived proximity of the experimenter to the participant, the Saarbrücker Persönlichkeitsfragebogen (Paulus 2009; German version of the Interpersonal Reactivity Index), targeting empathy, and the Objectification Scale (Gruenfeld et al., 2008), targeting person perception in situations that are relevant for interpersonal power—that is, situations in which one is able to command others.

PHASE 3

The participants were re-invited after a retention interval of about two weeks for Phase 3 (cf. Lindner et al., 2010). They were seated as in the preceding session and could observe the shelves containing the objects, though the testing only consisted of a brief computerized memory test. In this test, participants were presented with each item once (in random order) and had to judge whether they had performed the action in question in Phase 1 of the experiment. Participants answered by pressing a left response key for “Yes, I did perform this action in the last session” or a right key for “No, I did not perform this action in the last session.” Each item stayed on screen until the participant had responded, and the next item appeared after an intertrial interval of 500 ms.

Results

MEMORY BIAS

Our main dependent variable was the relative frequency of “Yes, I did” responses in Phase 3 which we analyzed as a function of whether or not the action had indeed been performed in Phase 1 and as a function of whether or not the action had been commanded in Phase 2 (see Figure 2). Accordingly, our main analysis was a 2×2 (Phase 1 Processing [read, performed] \times Phase 2 Processing [commanded, not commanded]) analysis of variance (ANOVA) on the mean frequencies of “Yes, I did” responses.

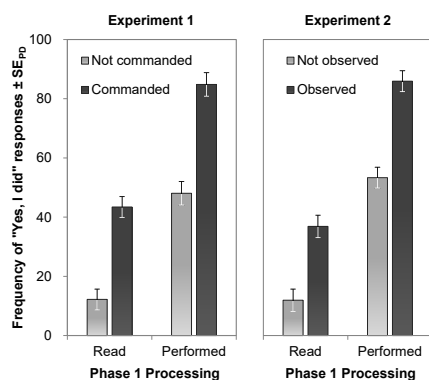


FIGURE 2.

Main results of Experiments 1 and 2. Error bars indicate standard errors of paired differences (SE_{PD} ; Pfister & Janczyk, 2013) that were computed separately for read and performed items.

This analysis yielded two robust main effects: Participants more frequently indicated to have performed an action if this actually had been the case in Phase 1, $F(1, 23) = 175.06, p < .001, \eta_p^2 = .88$, and, crucially, also if they had commanded this action than if they had not commanded it in Phase 2, $F(1, 23) = 108.98, p < .001, \eta_p^2 = .83$. Descriptively, the memory bias induced by the commands was slightly larger for items that had actually been performed in Phase 1 ($\Delta = 36.8\%$, $t(23) = 9.25, p < .001, d_z = 1.89$, as compared to those that had been read ($\Delta = 31.2\%$, $t(23) = 8.75, p < .001, d_z = 1.81$, but the interaction did not reach significance, $F(1, 23) = 2.21, p = .150, \eta_p^2 = .09$).

POST-EXPERIMENTAL QUESTIONNAIRES.

As a preliminary manipulation check, we computed the 95% CI for the debriefing question of whether the participants had the impression that the experimenter had acted according to their command in Phase 2. The corresponding CI clearly spanned the upper end of the scale, $M = 6.29, 95\% CI_{\text{Command}} [5.85, 6.50]$, suggesting that the participants did perceive the situation as intended (for a more thorough validation, see the Results section of Experiment 2).

As a further, exploratory analysis, we correlated the questionnaire data with the observation inflation effect across participants (see Table 1). To account for ceiling effects in the participants’ “Yes, I did” responses, we submitted the individual percentages to an arcsine-transformation for these analyses. For the objectification scale, we computed a summary score (according to Gruenfeld et al., 2008), with higher values indicating a higher tendency to objectify others. For the Saarbrücker Persönlichkeitsfragebogen, we focused on the subscale Perspective Taking and the compound scale Empathy (Paulus, 2009) because these scales are most closely related to simulation mechanisms that have been proposed to underlie observation inflation (Lindner et al., 2016). Because the IOS only consists of one item (Aron et al., 1992), we did not further preprocess these data; higher values also indicate higher inclusion ratings.

A first result of this correlation analysis was a substantial intercorrelation of $r = .51$ between the observation inflation effects for read and for commanded items. Furthermore, small to medium correlations also emerged between the participants’ objectification scores and their observation inflation effects, not only when taking the overall effect into consideration, $r = .44$, but also separately for read items, $r = .45$, and performed items, $r = .28$ (see Table 1 for inferential statistics; a similar picture emerged when using nontransformed percentages, though with slightly smaller effect sizes: $r = .32$ for the overall effect, $r = .26$ for read items, and $r = .30$ for performed items).

Discussion

Experiment 1 investigated whether observation inflation occurs for direct, face-to-face interactions, especially if one agent commands the other to perform a certain action. The data provide strong evidence in favor of such an effect: Participants showed a clear tendency to remember commanded actions as having been performed by themselves, even if this had not been the case throughout the experiment. This first demonstration indicates that observation inflation might indeed

TABLE 1.

Bivariate Correlations Between the Observation Inflation Effect and the Post-Experimental Questionnaires in Experiment 1

	Δ_{Overall}	Δ_{Read}	$\Delta_{\text{Performed}}$	O-S	PT	Empathy	IOS	Q1 _{Jointly}	Q2 _{Command}	Q3 _{Perspective}	Q4 _{Different}
Δ_{Overall}		.79	.93	.44	.06	.01	-.26	-.10	-.01	-.20	-.14
Δ_{Read}	<.001		.51	.45	.05	-.04	-.30	-.19	-.27	-.10	-.08
$\Delta_{\text{Performed}}$	<.001	.011		.28	.06	.04	-.19	-.03	.15	-.22	-.15
O-S	.032	.028	.189		-.31	-.36	-.06	.11	-.07	-.17	-.07
PT	.770	.816	.786	.144		.45	.61	.22	-.08	.18	.22
Empathy	.960	.848	.857	.086	.029		.38	.39	-.02	.44	-.15
IOS	.215	.153	.375	.794	.002	.071		.32	-.05	.53	.16
Q1 _{Joint}	.647	.378	.901	.603	.303	.067	.133		-.02	.32	-.12
Q2 _{Command}	.976	.197	.481	.733	.710	.922	.820	.936		-.15	-.01
Q3 _{Perspective}	.357	.642	.312	.419	.408	.036	.008	.128	.484		.30
Q4 _{Different}	.505	.709	.478	.752	.299	.491	.450	.576	.957	.154	

Note. Observation inflation effects Δ were computed as the difference in affirmative responses for commanded versus not commanded actions, with Δ_{Overall} being pooled across all items, and Δ_{Read} and $\Delta_{\text{Performed}}$ computed separately for items that had been read or performed in Phase 1, respectively. Numbers above the diagonal represent correlation coefficients whereas numbers below the diagonal represent p values when testing the corresponding correlations against zero. O-S = Objectification Scale, PT = Perspective Taking (subscales of the Saarbrücker Persönlichkeitsfragebogen), IOS = Inclusion of Other in the Self. Q1-Q4 indicate responses to the debriefing questions.

apply to direct, face-to-face interactions. To compare the effect size of the present observation inflation effects to less interactive face-to-face settings, we replicated the setup of Experiment 1 but used passive observation instead of active commands.

EXPERIMENT 2: PASSIVELY OBSERVED ACTIONS

Experiment 2 was identical to Experiment 1 except for the procedure during Phase 2: Instead of commanding the experimenter to carry out one of two suggested actions, participants passively watched the experimenter and had to identify which of two suggested actions she had just performed. Experiment 2 thus provided a replication of the previous findings on observation inflation (Lindner et al., 2010) in a face-to-face setting, and added a baseline condition to assess the effects of commands as observed in Experiment 1.

Method

We recruited a new sample of 24 participants ($M_{\text{age}} = 25.1$ years, 20 female); the mean retention interval amounted to 14.0 days ($SD = 0.74$, range = 13-17 days). Apparatus, design, and procedure were as in Experiment 1, except for the following modifications concerning Phase 2.

In Phase 2, participants now took the role of mere observers and watched the experimenter perform the actions. Each participant was yoked to a participant of Experiment 1, and the experimenter saw on her monitor the sequence of action choices that the yoked counterpart of the participant had made. This was done to control for random variation that comes as a necessary by-product of giving the participants in Experiment 1 the choice between different possible commands. The

participants also saw a display containing two action statements (the choice display used in Experiment 1) and had to read out loud the statement that corresponded to the experimenter's action.

Results

MEMORY BIAS

As in Experiment 1, participants more frequently indicated to have performed an action if this actually had been the case in Phase 1 than if they had not performed this action, $F(1, 23) = 173.52, p < .001, \eta_p^2 = .88$ (see Figure 2). Crucially, this was also the case if they had observed this action than if they had not observed it in Phase 2, $F(1, 23) = 108.61, p < .001, \eta_p^2 = .83$. Again, the interaction did not reach significance, $F(1, 23) = 2.53, p = .125, \eta_p^2 = .10$, even though the memory bias induced by observation was descriptively smaller for items that had been read in Phase 1 ($\Delta = 24.9\%$), $t(23) = 6.57, p < .001, d_z = 1.34$, as compared to those that had actually been performed ($\Delta = 32.6\%$), $t(23) = 9.25, p < .001, d_z = 1.89$ (with an identical effect size as in Experiment 1 for the latter comparison).

A subsequent between-experiments ANOVA, with the within-subject factors Phase 1 Processing (2) and Phase 2 Processing (2) and the between-subjects factor Experiment (2) did not yield a main effect of experiment nor any significant two-way interactions involving the factor experiment, $ps > .149$. The interaction of Phase 1 processing and Phase 2 processing showed a small but significant effect, $F(1, 46) = 4.71, p = .035, \eta_p^2 = .09$, which, however, was not further modulated by experiment, $F(1, 46) = 0.12, p = .732, \eta_p^2 = .00$.

POST-EXPERIMENTAL QUESTIONNAIRES

As an additional manipulation check for Experiments 1 and 2, we compared the participants' responses for the four debriefing questions

between both experiments. A marked difference between the experiments resulted for the critical question of whether the experimenter had acted according to the participants' command in Phase 2, with higher values for Experiment 1 than for Experiment 2 (6.29 vs. 1.96), $t(46) = 13.87, p < .001, d_z = 4.00$. Participants in Experiment 1 also indicated more strongly that they would have performed the actions differently than the experimenter (2.63 vs. 1.96), $t(46) = 2.01, p = .050, d_z = 0.58$. Responses to the remaining questions did not differ systematically between experiments, $ps > .302$ (impression of working jointly on the task: 5.08 vs. 4.79, perspective taking: 4.29 vs. 3.79).

Follow-up correlational analyses were as in Experiment 1. However, the observation inflation effects for read items and for performed items were no longer correlated, $r = .09, p = .67$ (see Table 2; $r = .14, p = .52$, when using untransformed data; see also Lindner & Davidson, 2014). Also, the objectification score did not predict the observation inflation effect ($-.15 < r < .24$).

Discussion

Experiment 2 yielded an overall similar pattern of results as Experiment 1: Participants tended to recall observed actions as their own ones, even if they had not performed these actions at any time throughout the experiment. Furthermore, the effect sizes of both experiments were in the same range and of considerable magnitude ($ds \geq 1.34$).

GENERAL DISCUSSION

The present study investigated observation inflation in direct, face-to-face interactions. Experiment 1 focused on a setting in which the participant commanded the experimenter to carry out different object-oriented actions. Commanding these actions indeed had a lasting impact on how participants recalled the actions in a later test phase: Commanded actions were more likely to be recalled as having

been performed by the participant him- or herself than actions that had not been commanded. These data represent a first demonstration for observation inflation during face-to-face interactions in adult participants. They further demonstrate that observation inflation is not limited to passive observation; rather, agents who prompt others to perform an action may systematically remember this action as if it were their own deed.

Mechanisms of Observation Inflation

Robust effects of observation inflation further emerged for items that had been performed earlier on and also for items that had only been read and therefore had never been performed by the participants themselves. That is, observation does not only inflate action memories that are already present, but it can also lead to false recall of action performance on its own (see also Lindner et al., 2010). For both types of items (read and performed ones), active commanding led to an increase of about 30% "Yes, I did" responses. This effect appears rather sizeable, though it likely represents an upper boundary for effects of observation inflation in everyday situations, especially considering that the observed actions were repeated several times, which likely increases their impact (Mitchell & Zaragoza, 1996; Zaragoza & Mitchell, 1996). The presence of a female experimenter might also have boosted the effects because most of our participants were also female and similarity between actor and observer has been shown to moderate observation inflation effects (Lindner et al., 2012; with gender likely being more salient in the present setup than in previous video-based studies). How strongly an agent's memory recall is affected by observation inflation arguably depends on various additional aspects, such as attention toward the observed actions, the length of the retrieval delay, and the distinctness of the action in question. Uttering a simple statement, for example, might be more easily remembered as one's own after a short

TABLE 2.

Bivariate Correlations Between the Observation Inflation Effect and the Post-Experimental Questionnaires in Experiment 2

	Δ_{Overall}	Δ_{Read}	$\Delta_{\text{Performed}}$	O-S	PT	Empathy	IOS	Q1 _{Jointly}	Q2 _{Command}	Q3 _{Perspective}	Q4 _{Different}
Δ_{Overall}		.66	.81	.09	.13	-.13	.03	-.06	.04	.22	.08
Δ_{Read}	<.001		.09	.24	.17	-.03	.45	.37	.22	.19	.11
$\Delta_{\text{Performed}}$	<.001	.671		-.15	.05	-.15	-.32	-.37	-.11	.13	.01
O-S	.665	.262	.494		-.16	-.45	-.48	-.40	-.45	-.28	.06
PT	.534	.436	.830	.466		.48	.31	.00	.05	.29	.31
Empathy	.561	.907	.496	.026	.018		.38	.08	.36	.35	-.17
IOS	.891	.026	.133	.018	.143	.065		.61	.22	.21	-.20
Q1 _{Joint}	.770	.078	.074	.052	.998	.695	.002		.09	.19	-.37
Q2 _{Command}	.840	.302	.594	.028	.808	.084	.307	.662		.44	-.04
Q3 _{Perspective}	.310	.363	.531	.184	.169	.091	.331	.384	.030		-.01
Q4 _{Different}	.722	.600	.950	.764	.145	.420	.361	.071	.837	.978	

Note. Numbers above the diagonal represent correlation coefficients whereas numbers below the diagonal represent p values when testing the corresponding correlations against zero. Q1-Q4 indicate responses to the debriefing questions. O-S = Objectification Scale, PT = Perspective Taking (subscale of the Saarbrücker Persönlichkeitsfragebogen), IOS = Inclusion of Other in the Self.

period of time (Brown, Cadera, Fields, & Marsh, 2015; Hollins, Lange, Berry, & Dennis, 2016), whereas other actions might be more resilient to observation inflation.

A final noteworthy observation is that the strength of observation inflation did not differ between active commanding and passive observation, as suggested by a direct comparison of Experiments 1 and 2. This result should be interpreted with caution, however, because the present between-groups comparison only ensured sufficient power for strong effects (i.e., $1-\beta \geq .8$ for $d_z \geq 0.83$), whereas the descriptive results suggested a small effect, if anything ($d_z = 0.35$ for the between-groups difference in the effect of command/observation).¹ Even if the effects for active commands and passive observation were similar in size, however, they still could derive from different sources, and the results of our exploratory analyses seem to support this speculation. Firstly, for the commanded actions used in Experiment 1, the observation inflation effects depended on the participants' tendency to objectify others—that is, to use others as “tools” to reach one's own goals (Gruenfeld et al., 2008), whereas this was not the case in Experiment 2. Representing the commanded action as a direct consequence of one's own action is also in line with recent results on basic action control processes which showed predictable partner actions to be represented like other controllable aspects of the environment (Müller, 2016; Pfister, Dignath, Hommel, & Kunde, 2013; Pfister, Obhi, Rieger, & Wenke, 2014). Assuming that controlled actions of others become integrated in own action representations might be suggestive of a combined contribution of action simulation and source monitoring to inflation effects for commanded actions as compared to pure action simulation (for a related theoretical proposal, see Smith & Mackie, 2015). Secondly, the observation inflation effects for read items and performed items were strongly correlated in Experiment 1 (suggesting a shared mechanism), whereas they were clearly independent in Experiment 2, as well as in previous studies (Lindner & Davidson, 2014). Observation inflation for commanded actions might therefore be only superficially similar to observation inflation for passively observed actions, and different mechanisms might underlie both effects. A prime candidate seems to be a differential role of source monitoring and source confusion, which might be particularly involved for commanded actions. Alternatively, commanded and observed actions might induce different types of response bias. It appears plausible that being able to command comes with a more action-oriented mindset (Galinsky, Gruenfeld, & Magee, 2003) which could, in turn, promote a more liberal response criterion to the question of whether an action was performed. Observation inflation effects for commanded and observed actions could thus stem from different contributions of actual false memories and response biases (for a similar discussion relating to imagination inflation effects, see Goff & Roediger, 1998).

An elaborate approach to questions concerning the precise mechanism behind observation inflation could be achieved by using richer measures of memory performance. Such measures would ideally comprise measures of recall and recognition memory, an assessment of subjective confidence across different conditions, as well as a more complete source monitoring test (e.g., Lindsay & Johnson, 1989; Zaragoza

& Koshminder, 1989). A more complete source monitoring test would not only allow for gauging the contribution of response biases that cannot be assessed with the dichotomous “yes/no” responses of the present experimental design, but it would also allow investigating whether participants actually remember the action in question or, instead, retrieve the necessary information only in presence of the exact memory item (i.e., the exact wording of the command or observe statement; see also Hayes-Roth & Thorndyke, 1979). As a final benefit, a source monitoring test would also allow investigating the participant's memory for whether or not the other person had performed the observed actions. That is, even if the results of the present experimental design were a direct measure of memory accuracy (rather than response bias), the results could indicate that participants generated an additional memory entry of them performing the observed action (while still remembering the experimenter to have performed the same action) or, alternatively, they could assimilate the memory entry by truly remembering themselves as the actor instead of the experimenter. Clarifying these issues will not only inform the present conditions of commanded and observed action, but it will likely result in a more thorough theoretical understanding of the mechanisms underlying observation inflation in general.

Observation Inflation Outside the Laboratory

Showing that observation inflation has a strong and lasting effect also for real-life interactions has important implications for a range of fields. For one, such inflation effects are likely to affect interactions at the workplace because such workplace-related interactions can bear direct resemblance to the setting of both experiments. The present results therefore complement previous reports of memory bias in cooperative settings in which two participants shared a task (Eskenazi, Doerrfeld, Logan, Knoblich, & Sebanz, 2013; Hyman, Roundhill, Werner, & Rabirot, 2014; Sommerville & Hammond, 2007). In these studies, participants tended to remember their coactor's share of the task as if they had performed it themselves (for converging findings, see Foster & Garry, 2012). The present results extend these findings by showing that a direct involvement in the task is not necessary to create false memories of self-performance. The results also provide a novel perspective on egocentric biases when judging the responsibility for a product of joint work performance, such as a joint decision (Ross & Sicoly, 1979). Previous accounts suggested that people overestimate their own contribution because they remember their contributions more vividly and accurately. And while the present results do not contradict this notion, they suggest that overestimating own contributions might also partly be caused by falsely remembering the contribution of others as one's own.

Workplace-related interactions may further come with a difference in power between two individuals, and such power gradients may moderate the strength of observation inflation effects. Having the power to command another person, for instance, tends to reduce attention to one's subordinates (Fiske, 1993) and, as a downstream consequence, power tends to decrease perspective taking for actions

of these subordinates (Galinsky, Magee, Inesi, & Gruenfeld, 2006). Because action simulation has been suggested to be a key mechanism behind observation inflation (Lindner et al., 2016), having the power to command another agent should reduce one's tendency to take another's perspective and therefore work against this memory bias in situations involving a power gradient between individuals (though it may at the same time increase response biases as discussed above).

The present demonstration of observation inflation in direct, face-to-face interactions might also explain memory biases in situations unrelated to joint task performance. A particularly relevant field seems to be the accuracy of eyewitness testimonies, where the study of memory biases has a long-standing tradition (e.g., Loftus, 2003; Wells & Loftus, 2003). Cowitnesses were shown to have a strong impact on the accuracy of eyewitness reports and previous studies mainly focused on memory distortions due to their reports (for a review, see Davis & Loftus, 2007). Observation inflation, however, might also cause memory errors due to the mere presence of cowitnesses because people might falsely remember to have performed an action that, in fact, had been performed by someone else. This could help to resolve potential inconsistencies across the reports of different witnesses, a possibility that has not yet been tested to our knowledge.

FOOTNOTES

¹ Exploratory post-hoc analyses of the data of Experiment 1 further suggested that merely commanding an action yields a similarly high frequency of "Yes, I did" responses as actually performing an item as indicated by a direct comparison of items that were read in Phase 1 and commanded in Phase 2 to items that were performed in Phase 1 and read in Phase 2, $t(23) = 0.89$, $p = .383$, $d_z = 0.02$. By contrast, the data of Experiment 2 suggested that merely observing an action led to lower frequencies of "Yes, I did" responses than actually performing an action, $t(23) = 3.01$, $p = .006$, $d_z = 0.61$. These results might give additional hints for future studies, though a direct between-experiments comparison of both differences did not return significant results for the present data, $t(46) = 1.53$, $p = .132$, $d_z = 0.45$.

² We are grateful to Ira Hyman for prompting this discussion.

AUTHOR NOTE

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APPENDIX A

TABLE A1.
Item Pool Used in the Experiments

German original	English translation
Spielzeugauto anschubsen	Move the toy car
Ei in Eierbecher setzen	Put the egg in the egg cup
Taschentuchpackung öffnen	Open the package of pocket tissue
Flasche schütteln	Shake the bottle
Band aufwickeln	Recoil the ribbon
Bleistift spitzen	Sharpen the pencil
Wecker verstellen	Reset the alarm clock
Tesafile abreißen	Tear off some adhesive tape
Löffel polieren	Polish the spoon
Seife in Dose legen	Put the soap in the tin
Papier lochen	Punch holes into the paper
Garn abschneiden	Cut off some twine
Mit Taschenlampe leuchten	Turn on the flashlight
Küchentuch falten	Fold the kitchen towel
Schachtel öffnen	Open the box
Tintenpatrone entnehmen	Take the ink cartridge
Perlen schütteln	Shake the beads
Klingel betätigen	Ring the bell
Gummiband dehnen	Stretch the rubber band
Schwamm wringen	Mangle the sponge
Toilettenpapier abreißen	Tear off some toilet paper
Teebeutel aus Tasse nehmen	Take the tea bag from the cup
Würfel werfen	Roll the dice
Sonnenbrille zusammenklappen	Fold the sunglasses
Teelicht in Glas stellen	Put the candle in the glass
Nudelpackung hochheben	Lift the package of pasta
Zahnbürste in Becher stellen	Place the toothbrush in the holder
Mäppchen öffnen	Open the pencil case
Papier stempeln	Stamp the paper
Buch aufschlagen	Open the book
Deo aufschrauben	Unscrew the lid of the deodorant
Kappe vom Textmarker nehmen	Take the lid off the highlighter
Stecker und Dose zusammenfügen	Connect plug and socket
Papier zerreißen	Tear apart the paper
Büroklammer verbiegen	Bend the paper clip
Fernbedienung betätigen	Press a button of the remote control
Karte in Umschlag stecken	Put the card into the envelope
Schloss schließen	Close the lock
Becher vom Stapel nehmen	Take a plastic cup from the stack
Pfeffermühle drehen	Use the pepper mill

Bonbon nehmen	Take a candy
Gabel auf Teller legen	Place the fork on the plate
Fingerhut aufsetzen	Put on the thimble
Zettel abreißen	Tear off a sheet of paper
Heft zusammenrollen	Roll a notebook
Taschenrechner anschalten	Turn on the calculator
Zollstock auseinander klappen	Unfold the folding rule
Knoten in Kordel machen	Knot the cord
Gürtelschnalle öffnen	Open the belt buckle
Nadel in Nadelkissen stechen	Stick needle in the pincushion
Nummer auf Handy wählen	Dial a number on the mobile phone
Kugelschreiber auseinander schrauben	Disassemble the pen
Sicherheitsnadel öffnen	Open the safety pin
Klebenotiz abziehen	Tear off a sticky note
Socke umkrempeln	Turn the sock inside out
CD aus Hülle nehmen	Take the CD out of its case
Streichholz aus Schachtel nehmen	Take a match from the matchbox
Karten mischen	Shuffle the cards
Handschuh anziehen	Put on the glove
Radiergummi verwenden	Use the eraser

Note. All items were created to carry both, a descriptive connotation as well as being suitable as commands. German "Flasche schütteln," for instance, can be understood as "to shake a bottle" (descriptive) or as "shake the bottle" (command).