My Turn or Yours? Me-You-Distinction in Feature-Based Action Planning

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All experiments were preregistered (https://osf.io/zpf9q, https://osf.io/64xay, https://osf.io/rh4kf). For better comprehensibility, the reporting order of Experiments 1 and 2 is reversed. All raw data, analyses scripts and experimental material are openly available at the open science framework (Mocke et al., 2023, November 27, https://osf.io/uzyqg/).

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Abstract

Binding accounts propose that action planning involves temporarily binding codes of the action's unique features, such as its location and duration. Such binding becomes evident when another action (B) is initiated while maintaining the action plan A. Action B is usually impaired if it partially overlaps with the planned action A (as opposed to full or no feature overlap). In Experiment 1, in which participants bimanually operated two keys, we replicated these partial overlap costs. In a second experiment, two participants sat side by side, each handling one key. We tested whether an action B would be affected by duration overlap with the planned action A of another person similarly as by duration overlap with a planned action A of the participant's other hand. Here, we found no partial overlap costs. However, in a third experiment, proposing a common reward yielded partial overlap costs. This suggests that in joint action planning, another person's action plan can impact own actions through feature binding, but only with sufficient incentives to co-represent the other's actions (i.e., when goal achievement depends on both participants' performance). This furthers the understanding of how we represent other people's yet-to-be-executed action plans alongside our own.

Keywords: Episodic binding, Episodic retrieval, Action Plan, Action Co-representation

Public Significance Statement

To navigate social life, we must not only consider our own plans but also the plans of others. In these studies, we demonstrate that codes referring to the person planning the action are integrated in action representations. Crucially, this only occurs for people whose performance is highly relevant for mutual goal attainment.

Each voluntary action, be it a simple movement or a highly complex type of behavior, is, at least to a certain degree, planned before its initiation (Rosenbaum, 1980). Ideo-motor accounts of action control assume that anticipating the perceivable changes that come with a certain efferent activity is the key to preparing that efferent activity (Shin et al., 2010). The effects one aims to produce with their actions therefore seem to be at the heart of the cognitive representations of planned actions. Recent action control research has put the specifics of the architecture of these representations more into focus (Frings et al., 2020).

Cognitive Representations of Own Action Plans

According to the Theory of Event Coding, the action planning process involves the formation of so-called *event files* (Hommel et al., 2001). Event files can represent perceived events (like an object) as well as self-produced events (like an action). They consist of codes of features that describe the respective event, which become bound. In perception, such event file creation is supported by sensory stimulation. In action planning, event files have to be created by internally activating and temporarily binding essential feature codes that describe the action's perceptual (e.g., tactile, visual, or auditory) effects and distinguish it from other relevant action options. While there is no structural difference between files that code perceptual versus produced events, the latter are sometimes labelled 'action files' (Hommel, 2004).

The assumption of feature-based action representation is not limited to the event file approach, but rather common in research on motor control. For example, in motor programming models, features are construed as motor parameters that fill slots in a (generalized) motor program (Schmidt, 1975; Rosenbaum, 1980). Such parameters, like movement extent or duration, can be filled into the program in different order (Leuthold & Jentzsch, 2009). Once all slots are filled, the corresponding action can be executed. However, the unique assumption of the event file perspective is that such features are not only specified or activated prior to action execution but bound to each other.

Stoet and Hommel (1999) investigated this feature binding during planning processes. In their ABBA paradigm, participants were asked to prepare an action A. Yet, before carrying out that action, another action B was requested. Initiating action B, which is requested first, is typically hampered if it shares some features with the maintained action plan A, compared to when it shares neither or all of its features (e.g., Stoet & Hommel, 1999; Wiediger & Fournier, 2008, Mocke et al., 2020). These so-called *partial overlap costs* have been discussed to result from feature codes being less accessible for action B when they are already bound into, and thus occupied, by another plan, that is, the plan for action A (e.g., Stoet & Hommel, 1999). Alternatively, they may result from unwanted retrieval of the whole action A plan upon repetition of one of its features when planning action B (e.g., Frings et al., 2020). This account can also explain an observation which seems puzzling from the perspective of motor programming. Specifically, 're-programming' an initially planned action is sometimes faster if all parameters of the planned response have to be changed (no overlap) as compared to only some parameters (partial overlap, Larish & Frekany, 1985; Rosenbaum & Kornblum, 1982). To conclude, partial repetition costs typically serve as a marker of feature binding within action plans and indicate that such bindings are maintained until the action plan has been executed.

Cognitive Representations of Others' Action Plans

While it is crucial to understand the particular mechanisms of the planning process in the actor, we seldom act in isolation but rather perceive social partners as planning and acting as well, sometimes towards a common goal. This is known as joint action (see Sebanz & Knoblich, 2021 for a recent review). Not only do people monitor errors of others (Pfister et al., 2020; van Schie et al., 2004), but it seems that when we work with others, features of their actions are mentally co-represented as well. For example, in a traditional Simon task, two stimuli (e.g., a blue or red color dot) are assigned to left and right responses of a single agent. Crucially, the stimuli can also appear on the left/right side of the display. Although stimulus position is nominally irrelevant for the task, it affects performance, reflected in better (vs. impaired) performance if the stimulus location is compatible (vs. incompatible) with the response location (Simon effect; Craft & Simon, 1970). In a Joint Simon task, the stimuli are assigned to the single responses of two people, who sit side by side (Sebanz et al., 2003). Here, the other person's task is not relevant to the own task, as one operates only a single key, which makes coding of the own response as 'left' or 'right' redundant. Strikingly, performance is still superior when the stimulus is presented on the respective key's side (as in the traditional Simon task). This suggests that the 'task' of the partner is co-represented, or at the least, that the partner provides a spatial reference point that fosters the coding of the own response as being 'left' or 'right', relative to that partner (Dolk et al., 2013). Neurophysiological evidence also suggests that another person's motor activities are represented in joint action scenarios. For example, the presence of another person modulates one's own motor activity when observing that person (Bolt & Loehr, 2021). Moreover, in tasks requiring highly synchronous manual actions of two partners, like clinking glasses, indicators of the electroencephalogram suggest that actors spatially distribute attention between their own and their partner's hand and represent the temporal aspects of their partner's action similar to performing the action bimanually with the own left and right hand (Kourtis et al., 2014). All in all, it seems as if people default to representing others' actions even if not required for their task (Sebanz & Knoblich, 2021).

In the present paper, we aim to test whether coding of other peoples' planned actions can affect own actions in a similar fashion as coding another action of the own behavioral repertoire. We do so in a situation that requires asynchronous activities of partners, which could be carried out even without taking the partner's activities into account. Moreover, by relying on a tried-and-tested paradigm and a behavioral indicator of feature binding, we demonstrate that such influences of another person's action plan can be described in a feature-binding framework. Here, we test whether the same mechanisms that have been assumed to be crucial for the representation of own actions, that is, feature activation and binding, are also at work when representing the actions of other people.

Binding of Person Features in Action Plans

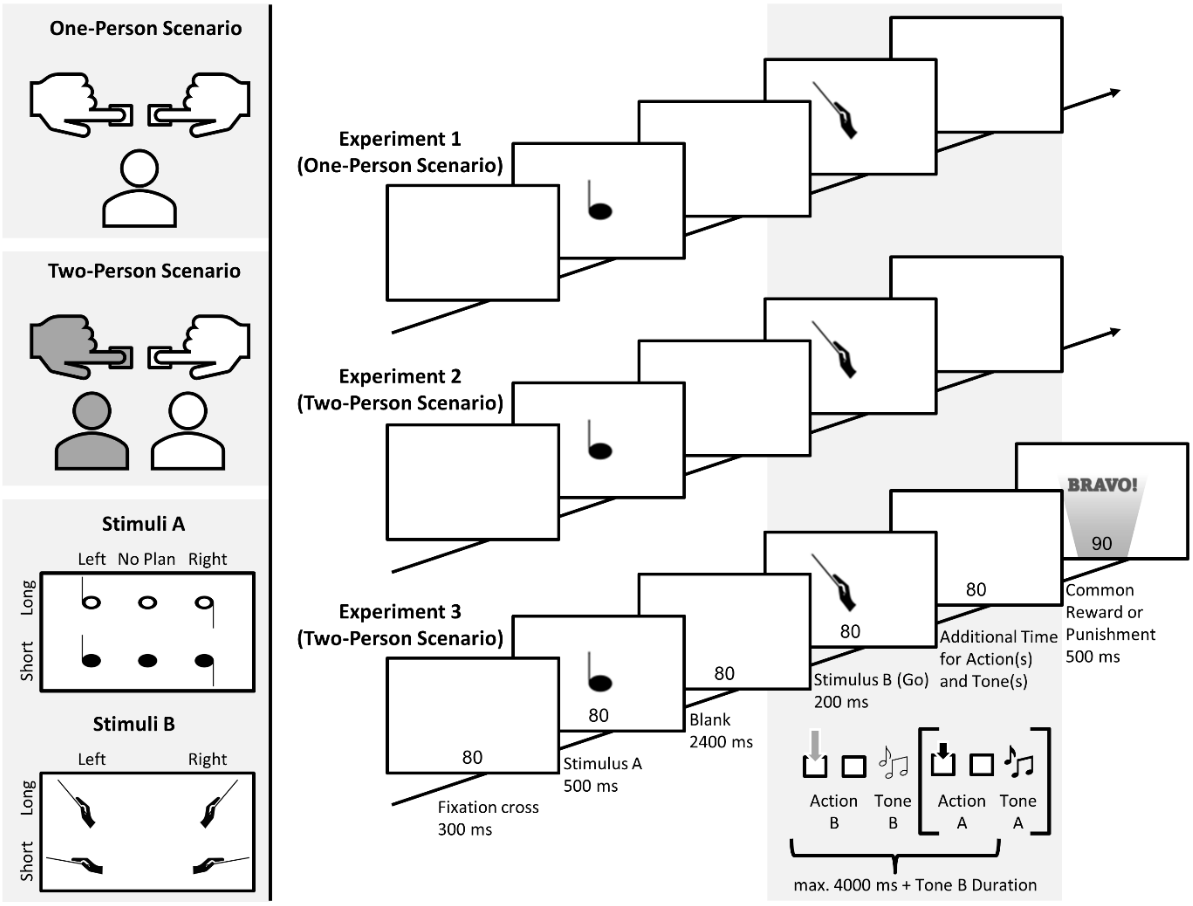
To approach that question, a logical first step is to examine whether a code of the person whose turn it is to act is bound within the event file in situations with more than one actor, that is, when either oneself or someone else plans an action. Hence, it needs to be determined whether 'person' codes, which may relate to the other person's identity or location, are bound in the same way as other action features are. Put differently, when only oneself serves as a possible actor, there should be no need to use an action feature dimension describing the person in charge or their location, when representing the action plan (as the person feature would always be *Me*, it represents a constant and would therefore not be distinctive for the planned action). In other words, in individual task settings, one would not need the minimal distinction for joint action, that is a distinction between ‘me’ and ‘not me’ (Vesper et al., 2010). If, however, there is sufficient uncertainty as to whose turn it is to do something, it might be necessary to bind the respective person feature (i.e., *Me* vs. *You*) alongside other features describing the action (see Mocke et al., 2022, Schmalbrock et al., 2023, for a discussion of the role of feature predictability).

To test this, we adapted a version of the ABBA paradigm, in which participants can plan four possible actions, that is, left short, left long, right short, or right long keypresses (see Figure 1 for example trials). Each action alternative was therefore determined by its value regarding location (left vs. right) and duration (short vs. long, for evidence of binding duration features in event files see Bogon et al., 2007; Köllnberger et al., 2023; Mocke et al., 2022). To illustrate this traditional one-person scenario, consider the left panel of Figure 2. The connected grey circles illustrate the supposed binding of features of a planned action A. The connected black circles illustrate the required binding of features of another, first initiated, action B. Performance should be worse when a to-be-initiated action B requires features that are already bound into the plan for another action A (bottom left and top right cases) as compared to when there is no overlap (bottom right) or full use of the already existing action plan (top left). Thus, feature binding (i.e., the binding of location and duration features) is indicated by the interaction of location repetition/switch and duration repetition/switch in performance measures. Previous work has exactly shown this performance pattern (Mocke et al., 2020). In Experiment 1, we replicate this typical finding.

Experiments 2 and 3 addressed the question how features of other persons’ action plans interact with representations of our own actions. We had two participants operate one key each in a two-person scenario instead of one participant operating both keys in a one-person scenario (right panels in Figure 2). This way, each participant had to determine whether they or their partner would have to plan a short or long movement, thus turning the 'left-right'-features into 'me-you'-features (see x-axes in Figure 2). The question was whether performance in action B would be similarly impacted by the assignment of a (non-)overlapping duration feature to the action plan A of another person (two-person scenario) as by the assignment of a (non-)overlapping duration feature to the own action plan A but to-be-conducted with the other hand (one-person scenario in Experiment 1). This might happen if participants construed the other person in the two-person scenario in a similar way as their other hand in the one-person scenario (middle panel). Note that for this scenario, the unique prediction of the event file account goes beyond motor programming accounts (Schmidt, 1975; Rosenbaum, 1980): When the other person plans an action, performing a completely different action (no overlap) should be easier than an action that resembles the other's plan (partial overlap). Alternatively, participants may focus on their own actions only while ignoring the other person's action plans A. In that case, one's own action B would be conceivably facilitated (impaired) by having prepared the same (other) duration for one's own action A. The duration of the other person's action plan A would go unnoticed and could thus not impact own performance B (right panel). In sum, while an interacting effect between person/location relation (repetition vs. switch) and duration relation (repetition vs. switch) might occur in any case, a comparison of the size and direction of this interaction between different scenarios allows at least a qualitative comparison of the strength of feature binding involved.

Figure 1

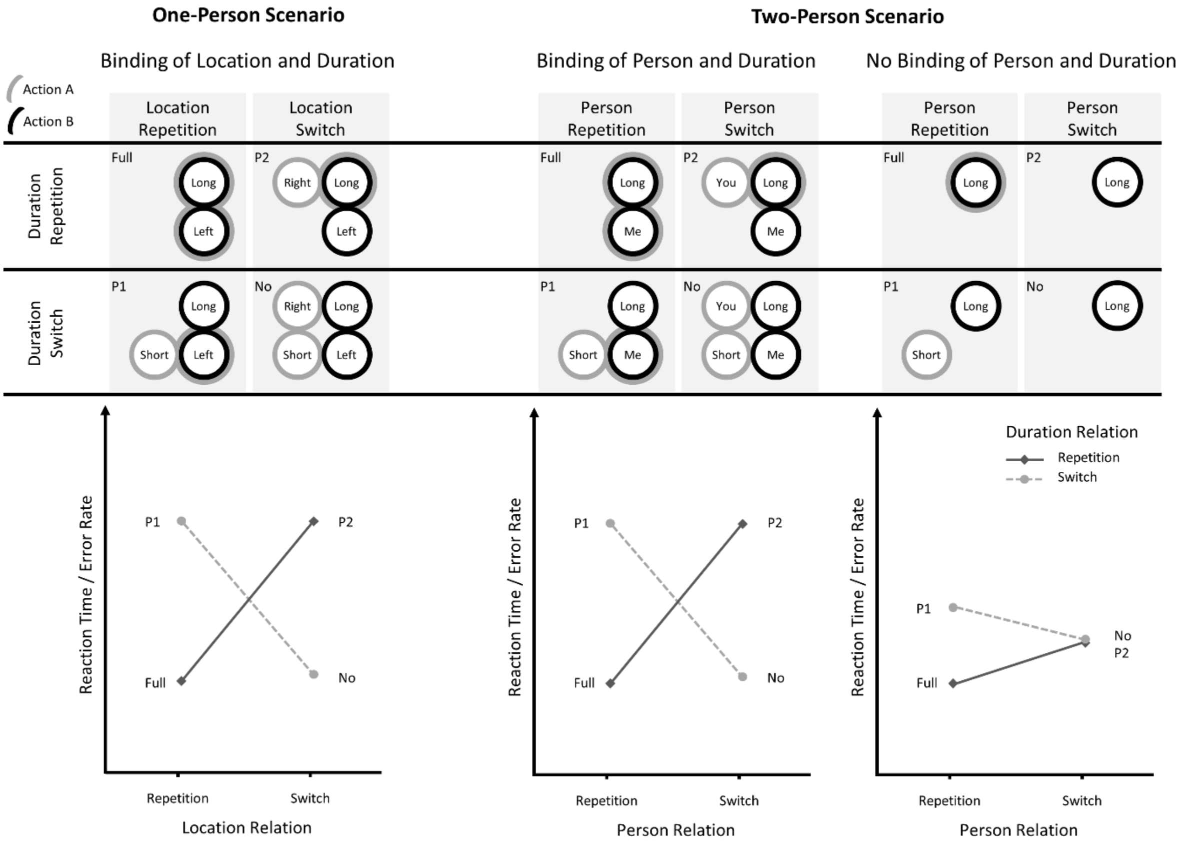
Experimental Setup, Stimuli and Example Trials for all Three Experiments.



Notes. In a one-person scenario (Experiment 1), one participant operated both keys. In a two-person scenario (Experiments 2 and 3), two participants operated one key each. In the depicted partial overlap trials, the maintained action plan A and the intermediate action B overlap with respect to the location feature (P1). Here, action A consisted of a short left keypress and action B of a long left keypress. With the release of each correct keypress, a corresponding effect tone was played. Trials in Experiment 3 additionally showed a shared score and a final screen informing participants about the performance-dependent loss or gain of points.

Figure 2

Possible Event Files and Corresponding Data Patterns in One- and Two-Person Scenarios.



Notes. Connections between grey circles represent possible feature bindings for Action A, whereas connections between black circles represent possible feature bindings for Action B. Condition labels mean full overlap (Full), no overlap (No), partial overlap with respect to the location feature (P1) and with respect to the duration feature (P2). Action A is planned and maintained during the initiation of action B. Partial overlap costs in the form of a crossover interaction emerge for location and duration features in a one-person scenario (left panel), and in a two-person scenario if person features are bound with duration features (middle panel). A less pronounced interaction is predicted if the person dimension is not represented in the action plans (right panel).

General Method

Transparency and Openness

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the studies. All raw data described here including analyses scripts and experimental material are openly available at the open science framework (Mocke et al., 2023, https://osf.io/uzyqg/). Data were analyzed using R, version 4.2.1 (R Core Team, 2022), and the afex package (Singmann et al., 2023). According to the ethical guidelines of the German Society for Psychology (DGPs) and regulations of the local ethics committee, ethics approval is not required for research that provides signed informed consent from study participants, collects data anonymously, and has no foreseeable negative impact on participants. The reported studies did not involve any form of deception and participants gave written informed consent prior to data collection. All data was collected in 2022 and 2023.

Power Considerations

Please note that while we switched the order of reporting for better readability, Experiment 2 was in fact conducted first, followed by Experiment 1 and Experiment 3. The following power considerations were preregistered (Exp.1: https://osf.io/zpf9q, Exp.2: https://osf.io/64xay, Exp.3: https://osf.io/rh4kf). In a pilot study (n = 20, Md_{Age} = 25, range = 21 – 65, 6 men, 14 women, 19 right-handed, 1 left-handed) with a similar procedure as Experiment 1 (one-person scenario), we found partial overlap costs (i.e., the difference between the averaged partial overlap conditions and the average between full and no overlap conditions) sized d_z = 1.10.

For Experiment 2 (two-person scenario without reward), to replicate this effect with a power of 1–β = 80% and an alpha level of α = 5% using a two-tailed paired-samples *t*-test[1], we would have needed n = 9 participants (G*Power, Faul et al., 2007). However, since we were also interested in substantially smaller effects, and lab resources allowed us to do so, we aimed

[1] We want to clarify that the paired samples *t*-test on the absolute effect of duration repetition (vs. switch) comparing location relation conditions is mathematically equivalent to the test of the interaction in a 2 (duration relation: repetition vs. switch) × 2 (location relation: repetition vs. switch) repeated-measures analysis of variance (ANOVA) with df=1 and $t^2=F$.

at and preregistered a sample size of n = 50. With this sample size, effect sizes of d_z = 0.40 or larger can be detected with a power of 1–β = 80%.

For Experiment 1 (one-person scenario), we were also interested in effects smaller than d_z = 1.10. To keep the number of trials constant to Experiment 2, we preregistered a sample size of n = 25 participants. With this sample size, effect sizes of d_z = 0.58 or larger can be detected with a power of 1–β = 80% (note however that each participant delivered twice as many observations as one participant in the other experiments).

Finally, in Experiment 3, detecting partial overlap costs as found in Experiment 1 (d_z = 0.44) required n = 43 participants. To be able to detect also slightly smaller partial overlap costs (d_z = 0.40) and to keep sample size consistent to Experiment 2, we preregistered at a sample size of n = 50 again.

Apparatus and Stimuli

In each of the three experiments described below, two keys were available, a left ('D' key) and a right one ('J' key). Keypresses were either short (0-200ms) or long (201-500ms). To make the experiments more engaging, they were framed in terms of a classical concert. Specifically, each keypress was linked to a tone sung by one of two opera singers, introduced to participants in the beginning. The left key was linked to a male (low pitch) and the right key to a female (high pitch) singer, and the keypress duration was linked to the singer holding a tone for a short or long period of time.

Additionally, participants had to interpret musical notes, that is, stimuli A (for preparing action A) and follow a "conductor", that is, stimuli B (for action B). Specifically, stimulus A was a quarter (short) or half (long) note with a stem facing upwards (low pitch/left key) or downwards (high pitch/right key), or no stem (control condition: no plan). Stimulus B was either the sketch of a conductor's hand raising the baton high (long) or low (short) and to the left (low pitch/left key) or to the right (high pitch/right key).

Procedure

Each experiment consisted of 336 trials in total (48 trials in each of the 7 experimental blocks), to reach an overall experimental duration of 45 minutes. The exact course of one trial was as follows (see Figure 1): After a fixation cross (300ms), participants saw stimulus A (500ms), followed by the planning interval (2000ms). After that, stimulus B appeared (200ms), and responses B (and A, except for no plan trials) were required within another 4000ms (+ duration of tone B, i.e., 151 ms vs. 377 ms) after stimulus B onset. Each key release after a correctly executed action produced an auditory effect of the respective duration (i.e., short: 151ms vs. long: 377ms) in high or low pitch, depending on the key. A trial was terminated as soon as a) an error was made, followed by an error message (2000ms), b) action B was executed correctly followed by its tone and then action A was executed correctly followed by its tone, or c) in no plan trials, action B was executed correctly followed by its tone and the response limit elapsed. The next trial started after an intertrial interval (ITI) of 100 ms.

Design

The essential features of all actions in all experiments were the location feature, that is, the key to be pressed (left vs. right), and the duration feature (short vs. long). In each experiment, we manipulated trial-wise whether the planned duration of action A was identical to the duration required for the intermediate action B or not. Additionally, we manipulated trial-wise whether stimulus A signaled the location that would also follow for action B, the other location, or no location at all. More precisely, a third of trials served as a control, in which stimulus A contained information only about the duration but not about the location feature (no note stem), meaning that no action A had to be planned.

This results in a 2 (duration relation: repetition vs. switch) × 3 (location relation: repetition vs. switch vs. no plan) within-subjects design for each experiment. Dependent variables were reaction times (RT) and error rates (ER) in the first initiated action B.

Experiment 1

Although the paradigm was highly similar to previous experiments of that sort (Mocke et al., 2022), it still differed in some respects (e.g., effect tones and different stimuli). Therefore, to check the basic functionality of the paradigm, we conducted Experiment 1, in which participants took the experiment alone and put their left index finger on the left and their right index finger on the right key (one-person scenario). This experiment was preregistered (https://osf.io/zpf9q).

Participants

We collected data from a total of n = 26 participants, from whom we excluded one participant with more than 30% errors in action B (in line with Mocke et al., 2022), resulting in a final sample of n = 25 (Md_{Age} = 23 years, range = 20 – 66 years, 21 female, 3 male, 1 NA, 21 right-handed, 3 left-handed, 1 NA). The study took Md = 44 minutes, and participants were financially rewarded with €12. They were sampled via the Sona platform and from a population of BSc Psychology students, who took part for course credit, did not have any vision or hearing impairments and were fluent in German.

Design

The first independent variable was the duration relation between actions A and B (repetition vs. switch). The second independent variable location relation was equivalent to the hand (or key location) relation between actions A and B (repetition vs. switch vs. no plan).

In the no plan condition, stimulus A signaled a duration but no hand, meaning that no action A was required at the end of the trial. With this control, we could therefore test the effect of duration repetition (vs. switch) produced by a stimulus that does not prompt the formation of any action plan A, and compare this to the effect of duration repetition (vs. switch) resulting from an action plan A made for the other hand.

Hypotheses

We investigated the effect of duration relation (repetition vs. switch) between actions A and B on performance in action B, depending on location relation (repetition vs. switch vs. no plan). If there was a binding of location and duration features, we should find partial overlap costs in the form of a crossover interaction (location relation: repetition vs. switch × duration relation: repetition vs. switch). Specifically, duration repetition should facilitate performance in location repetition trials (RT and ER: location and duration overlap < location overlap only) but hamper performance in location switch trials (RT and ER: no overlap < duration overlap only). Note that partial overlap costs as a combination of facilitative and cost effects are computed here as the difference between the averaged partial overlap conditions and the average between full and no overlap conditions ([P1+P2]/2-[Full+No]/2, in Figure 2, see for example Moeller et al., 2016, for this approach). In the no plan condition, the duration feature can be processed but it cannot be bound to any location feature. Still, there might be duration feature activation, facilitating a subsequent action B with the same duration (Stoet & Hommel, 1999, Exp. 3), which would show in better performance in duration repetition than duration switch trials.

Data Analysis

For analysis, we focused on action B, the first required action. All data, including performance in action A and a table with individual error counts are available at the OSF repository (Mocke, et al., 2023, https://osf.io/uzyqg/). For the RT analyses, we excluded all trials with erroneous action B and action A. For the ER analyses, we excluded only trials with responses prior to stimulus B onset (i.e., premature responses), and trials in which both keys were pressed simultaneously.

For both RT and ER analyses, to ensure as much as possible that action A was correctly planned, we included only those correct action B trials in which action A was then executed correctly as well in the ABBA sequence (Mocke et al., 2022). Further, correct trials with RTs smaller than 150 ms (likely reflecting premature responding) and those deviating more than 2.5 standard deviations (SDs) from the respective cell's mean were excluded from the RT and the ER analyses. After excluding error trials and outliers, 82.5% of all trials were included in the RT analysis.

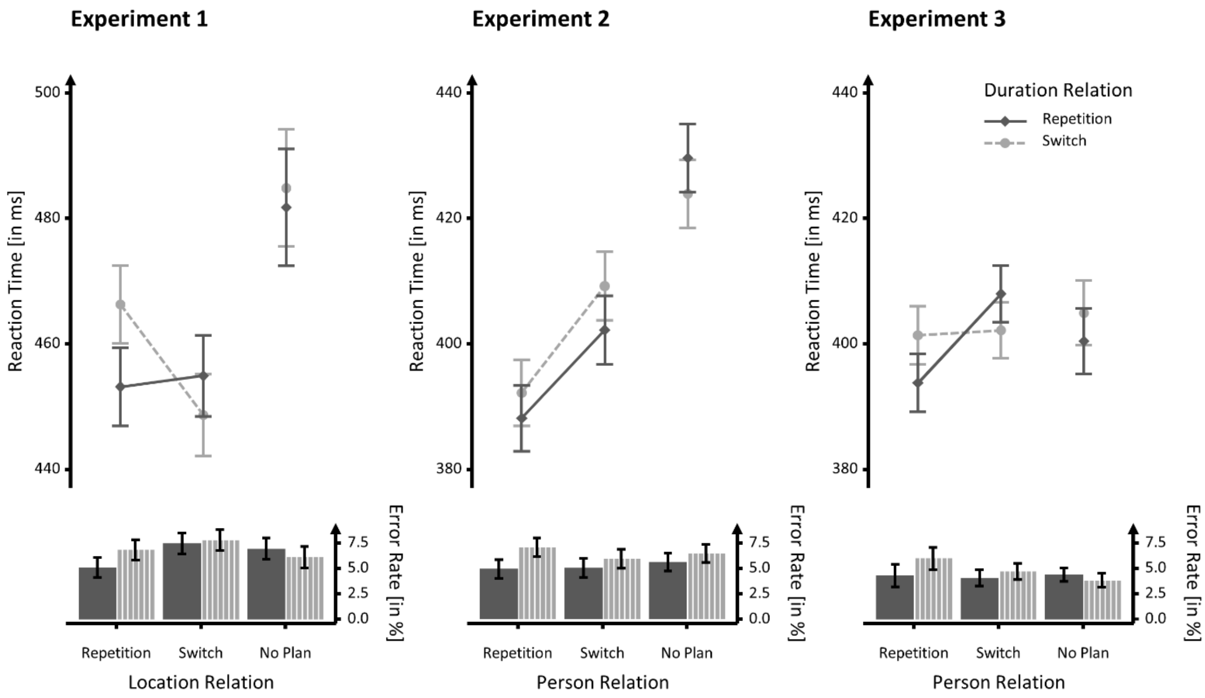
As an overall test, we conducted 3 (location relation) × 2 (duration relation) rmANOVAs for RT and ER. Most importantly, we tested an a priori interaction contrast for the 2 (location repetition vs. switch) × 2 (duration repetition vs. switch) interaction (contrast 1), which effectively represents binding of duration and location features. This interaction effect corresponds to a *t*-test against zero of the partial overlap costs (see also Footnote 1). For exploratory reasons, we also tested the interaction contrast regarding the orthogonal location relation contrast, which corresponds to the 2 (plan vs. no plan) × 2 (duration repetition vs.

switch) interaction (contrast 2). The plan condition corresponds to the collapsed location repetition and location switch conditions. To disentangle individual effects of duration relation for each location condition, we conducted paired-samples *t*-tests (displayed as error bars in Figure 3).

Results

Figure 3

Mean Reaction Times and Error Rates per Experimental Cell and Experiment.



Notes. Error bars depict standard errors of the paired differences SE_{PD} (Pfister & Janczyk, 2013).

Reaction Times

Table 1 presents the global results of the full 3 (location relation) × 2 (duration relation) rmANOVA for RTs in action B. Only the main effect of location relation was significant, with generally longest RT for action B when no action A followed (location switch vs. no plan: $t(24) = 2.28$, $p = .031$, $d_z = 0.46$; location repetition vs. switch: $t(24) = 1.13$, $p = .271$, $d_z = 0.23$). Most importantly, the planned 2 (location repetition vs. switch) × 2 (duration repetition vs. switch) interaction contrast (contrast 1) turned out significant, $t(24) = 2.18$, $p = .039$, $d_z = 0.44$, reflecting the typical pattern of partial overlap costs (see Figure 3). The 2 (plan vs. no plan) × 2 (duration repetition vs. switch) interaction contrast (contrast 2) was not significant, $t(24) = 0.03$, $p = .973$, $d_z < 0.01$, meaning that the duration relation effect did not depend on whether action A was generally planned or not.

Error Rates

The 3 (location relation) × 2 (duration relation) rmANOVA for ER is presented in Table 1 as well, but no effect reached significance. Contrast 1 for location repetition vs. switch × duration repetition vs. switch interaction was not significant, $t(24) = 0.96$, $p = .349$, $d_z = 0.19$, meaning that no partial overlap costs emerged for ER. Contrast 2 comparing duration relation effects between plan and no plan conditions was not significant, $t(24) = 1.74$, $p = .094$, $d_z = 0.35$.

Table 1

Results of 3 × 2 rmANOVAs for Reaction Times and Error Rates B of all Three Experiments.

Exp.		Effect	df		*F*	η_p^2	*p*
1	RT	Duration Relation	1,	24	0.64	.026	.430
		Location Relation	1.42,	34.03	4.05	.144	.039
		Duration Relation × Location Relation	1.80,	43.23	1.61	.063	.213
		Duration Relation x Location Relation in plan trials (Contrast 1)	1	24	4.77	.166	.039
	ER	Duration Relation	1,	24	0.37	.015	.550
		Location Relation	2.00,	47.89	1.78	.069	.179
		Duration Relation × Location Relation	1.80,	43.20	1.79	.069	.182
		Duration Relation x Location Relation in plan trials (Contrast 1)	1	24	0.91	.037	.349
2	RT	Duration Relation	1,	49	0.30	.006	.585
		Person Relation	1.98,	97.06	10.66	.179	<.001
		Duration Relation × Person Relation	1.96,	96.18	1.63	.032	.202
		Duration Relation x Person Relation in plan trials (Contrast 1)	1	49	0.16	.003	.690
	ER	Duration Relation	1	49	4.40	.082	.041
		Person Relation	1.94,	94.84	0.35	.007	.695
		Duration Relation × Person Relation	1.89,	92.57	0.74	.015	.473
		Duration Relation x Person Relation in plan trials (Contrast 1)	1	49	1.36	.027	.250
3	RT	Duration Relation	1	49	0.66	.013	.420
		Person Relation	1.83,	89.45	0.85	.017	.420
		Duration Relation × Person Relation	1.88,	92.27	2.05	.040	.138
		Duration Relation x Person Relation in plan trials (Contrast 1)	1	49	4.50	.084	.039
	ER	Duration Relation	1	49	1.15	.023	.290
		Person Relation	1.73,	84.88	1.14	.023	.320
		Duration Relation × Person Relation	1.63,	80.06	1.71	.034	.193
		Duration Relation x Person Relation in plan trials (Contrast 1)	1	49	0.65	.013	.423

Note. Exp = Experiment, RT = Reaction Time, ER = Error Rate, rmANOVA = repeated measures analysis of variance. The first three rows per variable present Greenhouse-Geisser overall rmANOVA results. The last row presents a planned comparison computed based on the fitted rmANOVA results (contrast 1) that tests the Duration Relation [-1, 1] × Location Relation [-1, 1, 0] interaction (i.e. partial overlap costs).

Discussion

We found significant partial overlap costs in RT for action B, as shown by the interaction of location and duration repetition versus switch, which indicates binding of location (left vs. right) and duration (short vs. long) features in action plans A (Figure 2, left panel). Thus, Experiment 1 replicated findings from previous research (Mocke et al., 2022) and showed that the version of the ABBA paradigm applied here is capable of yielding robust binding effects. Therefore, we conclude that the current method proved suitable for testing binding between person and duration features in Experiment 2.

Interestingly, response times increased when no action A was planned at all. We conjecture that this occurred because there was a constant time limit to complete the entire trial sequence which allowed participants to take more time for action B if no action A was required later. Moreover, in the no plan condition there was descriptively, but not significantly, faster responding with overlap to the previously announced duration feature. Such a lack of a reliable duration overlap effect suggests that the duration feature was more or less ignored if it could not be assigned to a response location.

Experiment 2

In the second experiment, two participants sat side by side, one controlling the left and one controlling the right key. The participant on the right put their right index finger, and the participant on the left their left index finger on their respective key. Thus, compared with Experiment 1, participants now simply had to prepare either a short or a long keypress, while the location dimension (referred to as the person here) was split between them and their partner. This experiment was preregistered as well (https://osf.io/64xay).

Participants

We collected data from a total of n = 52 participants. We checked for participants with more than 30% erroneous actions B overall (Mocke et al., 2022), and excluded one affected team. The final sample thus consisted of *n* = 50 participants who took part in 25 teams (Md_{Age} = 20 years, range = 18 – 28 years; 38 female, 8 male, 4 NA; 43 right-handed, 3 left-handed, 4 NA). The study took place in the lab, lasted *Md* = 46 minutes and participants were financially rewarded with €12.

Design

The first independent variable was again the duration relation between actions A and B (repetition vs. switch). In this experiment, the second independent variable, person relation, was equivalent to the person to whom the planned action A was assigned (repetition: the same person, i.e., actor action A = actor action B vs. switch: the other person, i.e., actor action A ≠ actor action B vs. no plan: no one). As in Experiment 1, a corresponding sound effect was played after each correctly executed keypress. Especially in person switch trials, the offset of the sound which followed as soon as action B was executed by one person should have facilitated the timing of the onset of action A for the other person.

In the no plan condition, stimulus A (i.e., a note without a stem, see Figure 1) signaled a duration but no person, meaning no participant had to plan action A as none was required at the end of the trial. Therefore, stimulus A should have neither led to a representation of an own action nor to a representation of an action of the other person. With this control condition, we had the opportunity to test the representation of other people's action plans A not only against the representations of their own action plans A but also against the representation of a stimulus that does not require any action plan.

Hypotheses

We investigated the effect of duration relation between actions A and B on performance in action B, depending on person relation (repetition vs. switch vs. no plan). Now that participants could not only represent their own action plans A but also those of their partner, the main question was how participants represent their partner's action plans A.

If, in this two-person scenario, action plan representations contained a feature dimension making it possible to distinguish between one's own plans and the partner's plans, and if such features were bound to the features 'short' and 'long', we should obtain a structurally similar data pattern as in Experiment 1. Specifically, if there was binding of person features and duration features (Figure 2, middle panel), we should find partial overlap costs in the form of a crossover interaction (person relation: repetition vs. switch × duration relation: repetition vs. switch). Specifically, duration repetition should facilitate performance in person repetition trials (RT and ER: person and duration overlap < person overlap only) but hamper performance in person switch trials (RT and ER: no overlap < duration overlap only). Again, partial overlap costs were an index of facilitative and cost effects, computed as the difference between the averaged partial overlap conditions and the average between full and no overlap conditions ([P1+P2]/2-[Full+No]/2, in Figure 2)

Alternatively, there might be no binding between the duration dimension and a dimension distinguishing the two people or locations, meaning that we would not observe a crossover interaction. This would be the case if participants processed the eventually required duration of the final action A only if they themselves must carry out that action at the end of the trial. If so, we should see superior performance when a prepared keypress duration A corresponds to the first requested duration B, but only if it was the participant, rather than their partner, who planned action A (see Figure 2, right panel). In that case, others' action plans might in fact be represented like no action plans at all and the effect of duration relation should not significantly differ from the effect in the control (no plan) condition.

Data Analysis

Each participant's dataset consists of those trials in which this participant was required to execute action B (i.e., 168 trials), and therefore half as many trials as in Experiment 1. For the RT analyses, we excluded all trials with erroneous action B. For the ER analyses, we excluded only erroneous trials with responses prior to stimulus B onset, and trials in which the other person responded to stimulus B. For both RT and ER analyses, we applied the following additional exclusion criteria depending on the person relation condition, to ensure as much as possible that action A has been correctly planned. Generally, a trial was terminated as soon as an error was made. For person repetition trials in which action A was reached by correctly executing action B, we included only trials in which action A was then executed correctly as well (i.e., correct ABBA sequences). For person switch and no plan trials in which action A was reached, we included all trials in which the participant in question did not erroneously press any key for action A, as this would have been a sign of incorrect planning (while the other person could have made any error for action A). With these criteria, 85.7% of all trials were included in the RT analysis. In all other aspects, the analysis strategy was the same as in Experiment 1.

In line with our alternative hypothesis that other people's action plans might in fact be represented like no action plans, we had planned not only to compare the size of duration relation effects between location repetition and switch conditions (to test partial overlap costs), but also between location switch and no plan conditions. Specifically, if a duration relation benefit had emerged in person repetition, but not in person switch or no plan conditions, it would have been crucial to test the idea that representations of others' plans and of no plans resemble each other. However, to foreshadow, this pattern did not robustly emerge, which is why we report these comparisons (including Bayes Factors to test for equivalence) for all three experiments in the supplemental material (Mocke, et al., 2023, https://osf.io/uzyqg/).

Results

Reaction Times

Table 1 presents the global results of the full 3 (person relation) × 2 (duration relation) rmANOVA for RTs. As in Experiment 1, only the main effect of person relation was significant, with generally longest RT for executing action B when no action A would follow at all, faster RT when the other person would execute action A afterwards (no plan vs. switch: $t(49) = 2.53$, $p = .015$, $d_z = 0.36$) and descriptively fastest performance when the person themselves had to execute action A (switch vs. repetition: $t(49) = 2.00$, $p = .051$, $d_z = 0.28$).

Most importantly, as in Experiment 1, we tested two interaction contrasts. The 2 (person repetition vs. switch) × 2 (duration repetition vs. switch) interaction contrast (contrast 1) was not significant, $t(49) = 0.40$, $p = .690$, $d_z = 0.06$, indicating that no partial overlap costs emerged. The 2 (plan vs. no plan) × 2 (duration repetition vs. switch) interaction contrast (contrast 2) also failed to reach significance, $t(24) = 1.74$, $p = .087$, $d_z = 0.25$, meaning that the duration relation effect did not significantly depend on whether action A was planned (irrespective of the person planning it) or not.

Error Rates

The full 3 (person relation) × 2 (duration relation) rmANOVA for ER is presented in Table 1. It reflects only a general duration repetition benefit in the form of a significant main effect. Mimicking the pattern for RT, in ER, contrast 1 for the 2 (person repetition vs. switch) × 2 (duration repetition vs. switch) interaction was not significant, $t(49) = 1.17$, $p = .250$, $d_z = 0.16$, indicating that no partial overlap costs emerged. Also, contrast 2 comparing duration relation effects between plan and no plan conditions, was not significant, $t(24) = 0.58$, $p = .566$, $d_z = 0.08$.

Exploratory analyses

As post-hoc tests, we further explored keypress durations in this experiment (Pfister et al., 2023). The aim of these additional analyses was to find indicators for an effect of instructed durations A on actual durations B, and of actual durations B on actual durations A (e.g., contrasting or assimilation of the two response durations). Such indicators would support the notion that participants plan actions A and represent their partner's actions as well.

Effect of Plan A on Duration B. At least for long keypresses, similar experiments (Mocke et al., 2022) have shown robust contrast effects for durations B with respect to the planned durations A. Specifically, long keypresses B turned out longer when a short keypress A was planned than when a long keypress A was planned. This effect, which can only show if stimulus A is not ignored, can be taken as an indicator for a representation of action A during action B initiation. In the present study, the contrast effect was present in person repetition trials, $M_{Diff} = 11$ ms, $t(49) = 3.32$, $p = .002$, $d_z = 0.47$, but not in person switch trials, $M_{Diff} = 3$ ms, $t(49) = 0.83$, $p = .409$, $d_z = 0.12$. In turn, a significant assimilation effect emerged in no plan trials, $M_{Diff} = -7$ ms, $t(49) = 2.26$, $p = .028$, $d_z = 0.32$, which might be taken as an indicator that stimuli A, even if not self-relevant, are processed at least to a certain extent.

Effect of Duration B on Duration A. To explore whether participants processed their partner's actions at all, we further checked each participant's correlation between actual durations B and A for trials with two correct keypresses of the same duration category. Interestingly, this correlation was not only significantly larger than 0 in person repetition trials, r_{short} = .39, $t(49) = 7.59$, $p < .001$, $d = 1.07$, r_{long} = .48, $t(49) = 13.97$, $p < .001$, $d = 1.98$, but also in person switch trials, r_{short} = .08, $t(49) = 2.37$, $p = .021$, $d = 0.34$, r_{long} = .13, $t(49) = 2.64$, $p = .011$, $d = 0.37$. Although these tests should be interpreted with caution, they at least suggest that participants adjusted the duration of action A to the previously executed duration of Action B (or at least to the duration of the auditory feedback presented after it), and that they did so, although to a lesser extent, even if the other person executed action B.

Discussion

In Experiment 2, against expectations, we found no partial overlap costs for action B, as the interaction between person relation (repetition vs. switch) and duration relation (repetition vs. switch) was absent for RT and ER alike. This speaks against binding of person (me vs. you) and duration (short vs. long) features in action plans A.

However, some other findings are noteworthy: First, action B was initiated substantially slower when no action A would follow (i.e., in the no plan control condition compared to the person repetition and switch conditions). The same pattern was already apparent in Experiment 1. Put differently, the overall RT in action B depended on whether someone had been asked to plan an action A for later (see also General Discussion). Altogether, this suggests that already at the point in time of initiating action B, participants represented whether there would be an action A later, even if it was the other person's turn to execute it.

Second, the data further yielded at least indications for participants coding the duration of the finally required action A, even if it was not their turn to execute that action. Albeit significant only in ER, performance was generally better when the duration of action B matched the duration of the future action A. This could not have happened if the duration of the future action of the other person had been ignored completely. There were also indications for an influence of the subsequent action A duration on the actual action B duration (and vice versa, see exploratory analyses), but these indications were more pronounced when the agent for action B also carried out action A rather than their partner. These observations suggest that the duration of the final action A was represented, but more strongly or more frequently when it was the agent themself to produce the initially announced action A later.

To sum up, while there were indications that both the duration of the later required action A as well as the person responsible for that action were coded in the two-person scenario applied here, these features were not bound in a way that resembles the binding of duration and location features in a one-person scenario. Otherwise, we should have seen partial overlap costs similar to those in Experiment 1. The lack of such partial overlap costs is incompatible with the idea that features differentiating the two actors are bound in the action plan representations. Altogether, the partner's action planning was apparently not relevant enough to interact with the preparation of own intermediate actions (see Mocke et al., 2020 for a discussion of the role of task-relevance in feature binding). Consequently, we tried to emphasize the relevance of the other person's actions in Experiment 3.

Experiment 3

Experiment 2 represents a socially shared variant of the ABBA paradigm. Thus, we can relate it to other tasks that study the influence of joint action on action co-representation. Importantly, many of the effects investigated in these tasks are moderated by social relevance. Specifically, the more two people consider themselves being similar, or working towards a common or shared goal, the more they are said to operate in a so-called 'we' mode (Gallotti & Frith, 2013; van der Wel, 2015). For instance, interference effects in the Joint Simon task (Sebanz et al., 2003) become stronger as a function of interdependence among co-actors or partners in the task (Hommel et al., 2009; Iani et al., 2011, Ruys & Aarts, 2010). The same holds true for incidental bindings between stimulus features and observed actions, as these effects hinge on some form of situational or chronic interdependency between both co-actors (Giesen et al., 2014; 2018). For instance, in the study by Giesen and colleagues (2014), observationally acquired stimulus-response bindings were strongest in a condition of positive interdependency, that is, when both partners had to perform well to achieve a common reward. If only one of them performed well and the other did not, none would get the extra reward. It seems that people generally monitor their co-actors' actions regarding errors (van Schie et al., 2004, Schuch & Tipper, 2007). With the achievement of the reward critically depending on the correct responding of the partner, the other person's actions, and particularly their errors, should become even more relevant. Therefore, Experiment 3 is a modified version of Experiment 2 where we added a manipulation of positive interdependency, while keeping all other aspects of the experimental structure the same. As the previous ones, this experiment was preregistered (https://osf.io/rh4kf).

Positive Interdependency

In each trial, either both participants or none of them received a reward. The team gained 10 points if the whole action sequence was executed correctly. In turn, if one of the actions was executed incorrectly (because one or the other partner made an error), the team lost 10 points. Importantly, the team was rewarded together, meaning that the final reward depended on good performance of both partners. If teams had reached a score of 3000 points or higher by the end of the experiment (a criterion known to participants in advance), both participants received a financial bonus. For teams with lower scores, none of the partners received a reward.

To make this shared goal attainment more salient, we permanently displayed the current score at the bottom of the screen (see Figure 1). Finally, after tone A had finished or with the onset of an error message, we presented a visual effect (either "BUUUH!" written in red or "BRAVO!" written in green, for 500 ms, depicting feedback by the imaginary audience). We instructed participants that they could see an overall happiness score of the audience at the bottom of the screen, and that they could increase happiness by 10 points by playing the correct tone sequence but annoy the audience by playing incorrectly, which would lead to a subtraction of 10 points.

Participants

We collected data from $n = 58$ participants to end up with a final sample of $n = 50$ after excluding 4 teams with ER exceeding 30% ($Md_{Age} = 25$ years, range = 19 – 62 years; 34 female, 14 male, 1 diverse, 1 NA; 41 right-handed, 7 left-handed, 1 both, 1 NA). The study took place in the lab and lasted $Md = 50$ minutes. Participants were rewarded with at least 12€

(plus €3 bonus depending on their performance, n = 14, reward score Md_{Score} = 2870, range = 1650 – 3220).

Hypotheses

Hypotheses were the same as for Experiment 2. Additionally, we predicted that the introduction of positive interdependency between participants would make the person dimension more salient leading to an increase in partial overlap costs compared to Experiment 2.

Data Analysis

The analysis strategy was the same as for Experiment 2. Of all trials, 89.9% were included in the RT analyses. We will first present the results of the preregistered analyses, followed by the results of the same exploratory analyses as in Experiment 2. Additionally, we will report a between-experiment analysis of RTs. For this between-experiment analysis, we compared partial overlap costs, that is, the size of the 2 (location repetition vs. switch) × 2 (duration repetition vs. switch) interaction, among the three experiments.

To do so, we had originally planned and preregistered to a) combine the data from Experiment 2 and 3 (the current one) for a high-power test of partial overlap effects in a joint task, and b) compare Experiment 1 with Experiments 2 and 3 combined to find effects of splitting the task among two participants, and c) compare Experiments 2 and 3 to find effects of a joint reward in a two-person scenario. As the result patterns of Experiments 1 and 3 turned out to resemble each other more than the result patterns of Experiments 2 and 3, we refrain from reporting these planned analyses. Instead, we now focus on the two comparisons of most interest, that is, the comparison of Experiments 1 and 3 and of Experiments 2 and 3 by means of Welch two sample t-tests while providing also 95%-confidence intervals for the Cohen's d estimates using the esci package (Calin-Jageman, 2023). Note that a sensitivity analysis revealed that given our samples sizes, the two between-subjects comparisons can only detect effect sizes starting from d = 0.70 (Experiment 1 vs. 2) and d = 0.57 (Experiment 2 vs. 3), respectively (β = 80%, α = 5%, two-tailed independent-samples t-test, G*Power, Faul et al., 2007). The full preregistered between-experiment analyses for these comparisons and the same comparisons for the size of the 2 (location switch vs. no plan) × 2 (duration repetition vs. switch) interaction, can be found in the online supplemental material (Mocke, et al., 2023, https://osf.io/uzyqg/).

Results

Reaction Times

Table 1 presents the global results of the 3 (person relation) × 2 (duration relation) rmANOVA for RT. There were neither effects of person relation, nor of duration relation, nor a 3 × 2 interaction. Most importantly, as in Experiment 1, but contrary to Experiment 2, the 2 (person repetition vs. switch) × 2 (duration repetition vs. switch) interaction contrast (contrast 1) was significant, $t(49) = 2.12$, $p = .039$, $d_z = 0.30$, reflecting partial overlap costs (Figure 3). The 2 (plan vs. no plan) × 2 (duration repetition vs. switch) interaction contrast (contrast 2) failed to reach significance, $t(24) = 0.56$, $p = .582$, $d_z = 0.08$, meaning that the duration relation effect did not significantly depend on whether action A was planned (irrespective of the person planning it) or not.

Error Rates

The 3 (person relation) × 2 (duration relation) rmANOVA for ER is presented in Table 1 as well. None of the effects reached significance. In ER, contrast 1 for the 2 (person repetition vs. switch) × 2 (duration repetition vs. switch) interaction was not significant, $t(49) = 0.81$, $p = .423$, $d_z = 0.11$, indicating that no partial overlap costs emerged for ER, and neither was contrast 2 comparing duration relation effects between plan and no plan conditions, $t(49) = 1.81$, $p = .077$, $d_z = 0.26$.

Exploratory analyses

Effect of Plan A on Duration B. Long keypresses B turned out longer when a short keypress A was planned than when a long keypress A was planned in person repetition trials, $M_{Diff} = 12$ ms, $t(49) = 3.34$, $p = .002$, $d_z = 0.47$, but not in person switch trials, $M_{Diff} = 1$ ms, $t(49) = 0.40$, $p = .690$, $d_z = 0.06$, or in no plan trials, $M_{Diff} = 3$ ms, $t(49) = 1.34$, $p = .188$, $d_z = 0.19$. Therefore, Action B durations showed signs of concurrently representing the own Action A plans, but not the partner's Action A plans.

Effect of Duration B on Duration A. The correlation between actual durations B and A for trials with two correct keypresses of the same duration category was significant in person repetition trials, $\underline{r}_{short}$ = .40, $t(49)$ = 8.59, p < .001, d = 1.21, $\underline{r}_{long}$ = .46, $t(49)$ = 13.18, p < .001, d = 1.86, but, at least for long keypresses, also in person switch trials, $\underline{r}_{short}$ = .06, $t(49)$ = 1.33, p = .189, d = 0.19, $\underline{r}_{long}$ = .22, $t(49)$ = 4.91, p < .001, d = 0.69. As in Experiment 2, participants adjusted the Action A duration to the previously executed Action B (or Tone B) duration, even if the partner executed Action B.

Between Experiment Analyses

We compared partial overlap costs as reflected in the interaction of duration repetition versus switch and location / person repetition versus switch between experiments. There was no difference of these costs between the one-person scenario in Experiment 1 (M = 19ms, Figure 3, left panel) and the two-person scenario with positive interdependency in Experiment 3 (M_{Diff} = 13ms, Figure 3, right panel), $t(48.50)$ = 0.54, p = .588, d = 0.13 (95%CI: [-0.35,0.61]). Note that the location / person repetition and switch conditions also seem to differ in the slopes of both the duration repetition line and the duration switch line. Both slopes are, although only descriptively, $F(1,73)$ = 2.28, p = .136, η_p^2 = .03, larger for Experiment 3 than 1. This can be attributed to strategical influences in Experiment 3, in the sense that the time pressure reduced participants' response times for action B less when the other person (vs. the participant themself) was to follow up with action A.

We also compared partial overlap costs between the two two-person scenarios. These costs had been significant in Experiment 3 with positive interdependency (M_{Diff} = 13ms) but non-significant in Experiment 2 without interdependency (M_{Diff} = -3ms, Figure 3, middle panel). We had preregistered our hypothesis that these costs would be larger in Experiment 3 than in Experiment 2. This preregistered one-sided test was significant, $t(96.04)$ = 1.69, p = .047, d = 0.34 (two-sided 95%CI: [-0.06,0.73]).

Discussion

In this experiment, we found significant partial overlap costs showing binding between person (me vs. you) and duration features (short vs. long). Therefore, although the method resembled the one in Experiment 2, the outcomes between these two experiments differed. In our view, this can be attributed to the positive interdependency induction (i.e., collaboration towards a common reward) between the two team members, which was not present in Experiment 2. Similar to other studies, our findings indicate that binding effects in joint action setups are strongly influenced by positive interdependency (Giesen et al., 2014; 2018). Interestingly, the increased interdependency between participants still did not evoke a contrast effect in keypress durations B with respect to the duration A planned by the other person. Nonetheless, we could show that in a two-person scenario in which both people serve as potential actors, features coding the actor, whose turn it is to execute an action, are in fact bound into action plan representations (Figure 2, middle panel), at least when the other person's action plans are sufficiently relevant for the self.

General Discussion

Feature Binding in One-Person Scenarios

Experiment 1, together with previous experiments (Mocke et al., 2022), revealed robust partial overlap costs in a one-person scenario, in which the actor needs to distinguish their own action alternatives based on a duration and a location dimension. When planning an action A by binding one feature of each dimension in the event file, this binding can be measured as costs of repeating one of the plan's features as part of an intermediate action B. When, for instance, the duration of action B overlaps with the duration planned for action A, other action plan A features (e.g., left) are presumably retrieved, which leads to interference and accrues costs in case a different feature is required for action B (e.g., right).

Feature Binding in Two-Person Scenarios

Experiment 2, in which the location dimension was replaced with a person dimension, yielded a different pattern of results. The repetition of the duration A in action B apparently did not retrieve a 'You' feature from action plan A in person switch trials, which should have conflicted with the required 'Me' feature in action B (see Figure 2, middle panel). Similarly, a duration repetition did not retrieve a 'Me' feature from action plan A in person repetition trials, which should have facilitated action B initiation. In short, we did not observe partial overlap costs in Experiment 2, despite using the same procedure as in Experiment 1 but with two people handling one key each instead of only one person handling both keys. This speaks against binding (or at least against retrieval) of person features in action plans in such a two-person scenario.

A crucial difference between the two- and the one-person scenarios employed here is the fact that the person dimension was much less relevant than the location dimension. This lack of relevance conceivably stems from the fact that each participant could optimize their task performance by simply focusing on their own but not the partner's actions. Put differently, if event files are formed for own actions, and no event files are formed that represent other people's actions, this would make the 'You' feature, and therefore the whole person feature dimension, dispensable for representing action plans (Figure 2, right panel). This emphasis on own action plans also manifested in the lack of a contrast effect for keypress durations B with respect to planned durations A in person switch (but not person repetition) trials. Importantly, the lack of social relevancy and/or the strong focus on one's own performance could have prevented the emergence of binding and/or retrieval effects in Experiment 2. Giesen et al. (2014, 2018) also reported null-findings for observationally acquired stimulus-response bindings when participants worked independently of each other (see also Ruys & Aarts, 2010). These findings are in contrast to joint action literature generally suggesting that the default is to represent others' actions even if they are not required for the own task (Sebanz & Knoblich, 2021). Still, Joint Simon effects appear to be stronger in interdependent scenarios (e.g., Iani et al., 2011; Ruys & Aarts, 2010).

Notably, while we did not find signs of person feature binding in Experiment 2, some aspects of the data still support the idea that participants at least attended to their partners' actions. First, we found resemblance of the keypress A durations to the previous keypress B duration, even when the other person had executed action B. Second, while the duration relation effects did not differ between person switch and no plan trials, these conditions still differed in other regards. Specifically, actions B were initiated substantially faster in Experiments 1 and 2 when knowing that no action A would follow. The reason for this delay in the no plan condition might be strategic considerations that result from the shared response time limit for the two actions (i.e., “I can take my time, as there will be no second keypress afterward anyway” vs. “I need to hurry with the first keypress so that I / my partner have enough time for the other keypress”). Alternatively, processing stimulus A that carries duration information but tells both participants to refrain from responding to it might lead to general motor inhibition, which would then show in delayed action B initiation as well (see Giesen & Rothermund, 2014). In short, while we have indicators that participants generally processed both the person and the duration features, these did not bind to each other within the action plan representations.

Finally, while not of theoretical relevance for the assessment of feature binding, and not directly tested, one might wonder why responses B tended to be slower with location repetition than with location switch in Experiment 1, whereas they tended to be faster with person repetition as compared to person switch in Experiments 2 and 3. Please note that in all cases, no prior response (neither an own nor another person’s) had actually occurred when RT B was measured. Repetition or switch is therefore relative to an action still in mind (i.e., action A). Knowing about having to do something twice in a row might have generally alerted participants in Experiments 2 and 3. Such alerting cannot become apparent in Experiment 1, in which participants always had to respond twice in a row (first action B, then action A).

Manipulating Relevance of Others’ Action Plans

In Experiment 3, we encouraged participants to represent their partner’s actions more by introducing a reward when the participant and their partner showed sufficiently good performance. The induction of collaboration between the two team-members had two observable effects.

First, when comparing the results of Experiment 3 to Experiments 1 and 2 there was a considerable performance improvement in the no plan condition relative to the two planning conditions. As discussed above, the increase of RTs in action B in Experiments 1 and 2 might have been due to strategic reasons or to motor inhibition resulting from the “no go” action A stimulus. Given the results from Experiment 3, the former explanation seems more likely. That is, because the emphasis on performance in the latter experiment seemed to have increased not only participants' accuracy but also performance in terms of speed and therefore prevented the tendency to take more time in trials in which only one action was required. Future studies can counteract these kinds of strategic effects by introducing separate response limits for action B and A instead of a shared one.

Second, and theoretically more importantly, we speculated that the lack of binding of person and duration features in Experiment 2 was due to the lack of event files being generated and/or retrieved for other people's actions if the performance of a partner is not relevant for attaining one's goal. Apparently, the increased relevance of the other person's actions by the positive interdependency manipulation via introducing a shared goal in Experiment 3 fostered the creation and usage (i.e., retrieval) of event files for actions the other person was planning (Figure 2, middle panel). That the same feature dimension (here: person) can be considered for the creation of event files or not, depending on its relevance, has also been shown for feature dimensions describing environmental action effects (Mocke et al., 2020). Moreover, the finding that cooperation or positive interdependency works as specific means to increase relevance of person features is very much in line with findings on other joint action effects (e.g., Giesen et al., 2014, 2018; Hommel et al., 2009; Iani et al., 2011; Ruys & Aarts, 2010). We cannot tell for sure whether this positive interdependency prompted a shift towards a 'we' mode of joint action. To reveal such a mode, it would be necessary to demonstrate that the relationship of two people's planned actions (e.g., whether they are the same or different), without knowing the identity of the individual actions, impacts performance (Kourtis et al., 2019). Such conditions were not employed in the present study.

Limitations and Directions for Future Research

Regarding the generalizability of our findings, please note that our samples primarily comprised German speaking, mostly young, female, Caucasian, and neurotypical participants. We conducted the study in a laboratory setting, introducing a somewhat artificial context for joint action planning. The decision to plan or execute actions was provided by instructions rather than arising from participants. Replicating these findings in more natural settings could offer additional insights into action plan co-representations.

As other joint action studies, the present one raises certain questions (for a critical review see Dolk et al., 2013). One concerns the type of feature dimension used to distinguish the self from a partner. Note that what we have called person dimension so far could indeed consist of rather abstract feature codes describing the actor's identity (Me vs. You). Nonetheless, also other distinctions are theoretically possible.

For instance, it has been suggested that another person creates a salient reference point that allows a spatial coding of own actions which would otherwise not be coded spatially (Dolk et al., 2013; Pfister et al., 2014). This applies to the present study as well. While in the one-person scenario in Experiment 1, it seems natural that the actions of the left and right hand were distinguished by features describing positions in the horizontal dimension, this appears less likely when each participant has to operate only one key in one location, as in the two-person scenarios in Experiments 2 and 3. Thus, it is plausible that participants use abstract codes like 'Me' and 'You' in these situations. Perhaps, though, seating two participants side-by-side induces a spatial coding that is not person-based. More precisely, the partner's actions might not be coded as the actions of the 'other person', but as the actions that happen to the 'right' ('left') of the participants' actions, while the participants' actions are the ones happening on the relative 'left' ('right'). Future research is needed to disentangle these possibilities, for instance by contrasting both accounts. If participants can no longer localize their partner (e.g., in an online setting) spatial coding can be experimentally dissociated from person-centered coding in a situation that still yields robust binding and retrieval effects (e.g., Giesen & Rothermund, 2022).

Moreover, participants might have even treated their partner's actions in a way as if they were their own actions. Consider for instance a case in which the partner's plan was represented as if it were an action of the participant's other hand (such as when participants simulate the partner's to-be-executed action with their own hand that is in proximity to the partner and not needed to press their own key). In that case, the duration features would be bound to features that are in fact not too different from the location features (left vs. right) from Experiment 1, which would result in event files at least highly similar to those formed for the participants' own actions. In a setting requiring highly synchronous actions of two people Kourtis et al. (2014) favored this idea of simulating another person's unimanual action, as if it were an action of an own, currently passive, hand. They inferred this from the observation that the amplitude of the contingent negative variation (CNV) was of similar size when clinking glasses with a partner as compared to clinking glasses bimanually. With the present paradigm, this might be an avenue for future research. Moreover, if simulation was the underlying force behind partial repetition costs for observed action planning, one should obtain comparable findings in a non-social situation when action plans are only internally simulated (but not overtly carried out). Again, more research is needed to explore this endeavor in more detail. No matter the exact nature of the features used to distinguish these actions, the differences in results between Experiments 2 and 3 show that the use of such a distinction depends on sufficient relevance of the other person's actions.

Conclusion

Humans, as distinctly social creatures, rely on others' actions. The present work has revealed that a mechanism which has been shown to form the basis for planning one's own actions also plays an important role in keeping track of other people's planned actions. Just as feature codes describing a planned action can be bound in event files, so too can feature codes distinguishing between the people who will potentially perform a future action that is relevant for us. This enables the formation of representations of both one's own and others' action plans while simultaneously distinguishing them from each other. However, this distinction between the self and others in action plan representations seems to be made only when other potential actors are sufficiently relevant to the self.

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