Title. Effects of "We"-Framing and Partner Number on 2- and 3-Year-Olds' Sense of Commitment

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Declarations of Interest. None.

Data Availability. Materials, data, and programming script to reproduce all analyses, figures, and tables may be found at *XXX*.

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Highlights

- Linguistic "we"-framing increased 2-year-olds' sense of commitment towards partners.
- The number of partners did not influence 2-year-olds' sense of commitment.
- Three-year-olds responded at floor in both conditions.
- Two-year-olds may feel a not-fully-normative sense of responsibility towards partners.

Abstract

Committed partners feel normatively bound to one another. This normative pressure causes partners not to abandon one another for attractive alternatives. Research suggests that this sense of commitment emerges at around 3 years of age. This study investigated effects of partner number and linguistic "we"-framing on 2- and 3-year-olds' commitment (N = 48 per age group). One or three puppet partners framed a boring game as something either "we" or "you" are doing. As participants played with their partner(s), a fun, alternative game appeared. Two-year-olds remained longer with partner(s) before abandoning them following "we"-framing compared to "you"-framing, particularly when committed to a group of partners. There were no reliable effects on 3-year-olds, who readily abandoned their partner(s). This is the first report of a manipulation that reliably influences 2-year-olds' sense of commitment. These results may suggest a not-fully-normative, partner-based sense of responsibility in 2-year-olds, though additional research is warranted.

Keywords

development; collaboration; commitment; framing; partner number; responsibility

Introduction

Commitment is important for collaboration. Committed partners hold one another accountable for their behavior while collaborating. For instance, partners cannot suddenly abandon commitments. Barring agreement to cease collaborating, committed partners expect to continue collaborating (Michael & Pacherie, 2014). More than just expectation, commitment during collaboration is prototypically a kind of normative obligation (Gilbert, 1990). Commissive obligations entail that partners forego fleeting, individual desires and, instead, faithfully execute their roles. When does a sense of commitment develop? What factors affect its development?

Two developmental steps underlie commitment (alternatively, see Michael & Székely, 2018). In the first step, 2-year-olds collaborate without commitment (reviewed in Tomasello & Hamann, 2012). For example, 2-year-olds reengage partners after they cease collaborating (Warneken et al., 2006). However, 2-year-olds are similarly likely to reengage partners after an explicit commitment or parallel play (Gräfenhain et al., 2009). Moreover, 2-year-olds are similarly likely to help partners who did or did not help them (Hamann et al., 2012).

Commitment as obligation implies that commitment should develop once children can undertake and uphold obligations. The latter emerges at around 3 years of age as a "normative turn" in social relations (Tomasello, 2018). The normative turn depends on cognitive skills that enable 3-year-olds to conceptualize self-inclusive social groups (e.g., Vasil, Price, et al., 2023; reviewed in Tomasello, 2019). The ability to conceptualize self-inclusive groups underwrites one's ability to experience deontic pressure to undertake and uphold commitments.

The normative turn is the second step. Whereas 2-year-olds are indifferent to commitments, 3-year-olds reengage once-committed partners (Gräfenhain et al., 2009) or remain committed to partners, themselves (Hamann et al., 2012). Moreover, 3-year-olds understand when to hold

partners accountable for abandonment (Kachel et al., 2018, 2019), what constitutes a reasonable excuse for abandonment after-the-fact (Li et al., 2022; see Rawls, 1955), and when to make excuses for partners (Gräfenhain et al., 2013). Interestingly, 3-year-olds may have difficulty identifying appropriate reasons for releasing partners from prior commitments (Bonalumi et al., 2023; see Shpall, 2014). On balance, however, these results from recent research on children's commitment contradict those of early research on the topic (Astington, 1988a, 1988b, 1988c; Mant & Perner, 1988). Early research suggested that the sense of commitment does not emerge until middle childhood. In contrast, recent research suggests that the sense of commitment emerges at around 3 years of age. Arguably, this discrepancy follows from the use of action-based methods to investigate children's commitment in recent research. In contrast, early research used vignettes.

What factors affect commitment? Commissive intentions are often in partners' personal common ground (CG). These are shared mental states that contextualize collaboration (Clark, 1996). Partners can modify CG through communication (Tomasello, 2008), or rely on non-communicative cues to infer CG (e.g., interdependence, Kachel & Tomasello, 2019). For example, communication enables CG agreement on partners' goals and roles (Bratman, 1992). Inferences about CG rely on cues to CG (Vasil, 2023). Various communicative and non-communicative cues to CG may modify partners' CG beliefs about commitment. For example, children's commitment is enhanced by communicative cues to CG like explicit agreements (Gräfenhain et al., 2009; Kachel & Tomasello, 2019; Winter & Tomasello, 2024), promises (Kanngiesser et al., 2017, 2021, 2023; also, Heyman et al., 2015), and nonlinguistic "communicative looks" (Siposova et al., 2018).

Recently, studies have investigated effects of linguistic framing cues on children's commitment. There are various theoretical approaches to linguistic framing. These include cooperative game theory (Schelling, 1960), political science (Chong & Druckman, 2007) and

judgement and decision making (Tversky & Kahneman, 1981). From a game theoretic perspective, team reasoning and related models suggest that framing influences commitment (e.g., Sugden, 1995). Framing modifies the conceptual structure that mediates partners' reasoning about behavioral strategies. For example, use of "we" language may induce interdependent "we" frames (Bacharach, 2006). Partners who adopt "we" frames conceptualize coordination from the perspective of their dyad or group. This conceptualization induces a logic of interdependence (Roberts, 2005). Psychologically, a logic of interdependence provides partners with reasons to choose mutualistic but risky strategies, such as are involved in joint commitment (Pacherie, 2011).

Recent work suggests that interdependent framing increases children's sense of commitment (reviewed in Vasil, 2022). First, Butler and Walton (2013) framed a difficult, collaborative task to 4- and 5-year-olds using interdependent or individual framing. Interdependent framing coupled partners' fates, such that task success required both partners to complete it. Individual framing decoupled the partners' fates, such that one's partner's actions were irrelevant to one's success on the task. Participants worked for longer on the difficult task following interdependent framing compared to individual framing. Second, Koomen et al. (2020) investigated 5- and 6-year-olds' tendency to delay a desirable outcome when framed interdependently or independently. Participants were more likely not to access the desirable outcome, and waited longer to do so, following interdependent framing. Third, Vasil and Tomasello (2022) investigated 3- and 4-year-olds' commitment to a puppet partner who used either "we"-framing or "you"-framing. "We"-framing invited a "we" frame through the use of first-person plural pronouns and collaborative adjectives (e.g., "We will color our paper with our markers."). In contrast, second-person pronouns and no collaborative adjectives invited a "you" frame in the "you"-framing condition. Three-year-olds more often notified partners about their

intentions to abandon after "we"-framing. Altogether, these studies suggest that interdependent framing is a commitment cue. This type of cue stokes the sense of commitment.

The three studies, above, share two limitations. First, none compared the commitment of children younger and older than 3 years. This is important. Children should be sensitive to commitment cues only after the normative turn, at 3 years of age. In the present study, the commitment cue was "we"-framing. "We"-framing is appropriate for 2-year-olds insofar as they produce first-person plural pronouns (Vasil, Moore, et al., 2023) and appropriately interpret them (Vasil, Price, et al., 2023). Thus, 2-year-olds' predicted insensitivity to "we"-framing should reflect insensitivity to commitment cues and not a failure to understand the word *we*.

Second, none of the studies investigated relations between linguistic framing and non-communicative commitment cues. Partner number may be one such cue. Specifically, the earlier development of skills for dyadic compared to group interaction (reviewed above and in Tomasello, 2019) may presage an earlier emergence of normative pressure to individuals compared to groups. Psychologically, dyads are arguably simpler to engage with insofar as fewer perspectives have to be coordinated and acted on, whereas groups have more perspectives. For young children still learning how to successfully coordinate perspectives, it is possible that a single partner is a more tangible target for commitment than is a group of partners. The resulting hypothesis is that 3-year-olds should be more committed to individuals compared to groups. However, in contrast to this prediction, Winter and Tomasello (2024) found that 3- and 5-year-olds did not distinguish dyadic and group commitments. While their use of pre-recorded videos adds experimental control, it is a limitation insofar as it constrains experimenters' ability to respond flexibly and contingently to participants' behavior (e.g., such as in the context of collaborative activity). In the present study, participants engaged with puppets in real-time. Puppets are an appropriate replacement for peers

in studies of normative development (Stengelin et al., 2023). Regarding commitment, specifically, convergent results have been found with puppet (e.g., Kachel et al., 2019) and peer partners (e.g., Kachel et al., 2018). Moreover, potential conformist biases might be reduced with puppet groups compared to peer groups (see Winter & Tomasello, 2024).

A third limitation was of Vasil and Tomasello (2022),. Those authors manipulated, simultaneously, personal pronouns and collaborative adjectives (e.g., "together"). In contrast, the present study manipulated only personal pronouns to isolate their effects on commitment.

To remedy the above three limitations, the present study investigated the effects of "we"-framing and partner number on 2- and 3-year-olds' sense of commitment with live partners.

The two key predictions concerned differential effects of either condition as a function of age. First, 3-year-olds, but not 2-year-olds, were predicted to abandon their partner(s) less frequently, and to take longer to do so, following "we"-framing compared to "you"-framing. Second, 3-year-olds, but not 2-year-olds, were predicted to abandon their partner(s) less frequently, and to take longer to do so, in dyadic compared to group contexts. Additionally, it was thought possible that 3-year-olds who heard "we"-framing in dyadic contexts would abandon the least often and to take the longest to do so. Thus, this three-way interaction between age and both conditions was examined, below. However, this study was likely underpowered for this effect.

Methods

Participants. There were 101 participants. Five participants were excluded (n = 3 2-year-olds, n = 1 3-year-old, n = 1 unknown). Thus, there were 96 final sample participants. There were 48 2-year-olds ($N_{females}$ = 24, mean age = 2;6, range = 2;3-2;9) and 48 3-year-olds ($N_{females}$ = 16, mean age = 3;6, range = 3;3-3;9). The sample size was 24 participants per age per framing condition.

There were 48 participants per age per partner number condition. The framing sample size was determined due to the same sample size having shown prior evidence for effects of "we"-framing in 3-year-olds (Vasil & Tomasello, 2022). The partner number sample size was determined based on prior null findings with 20 participants per condition (Winter & Tomasello, 2024).

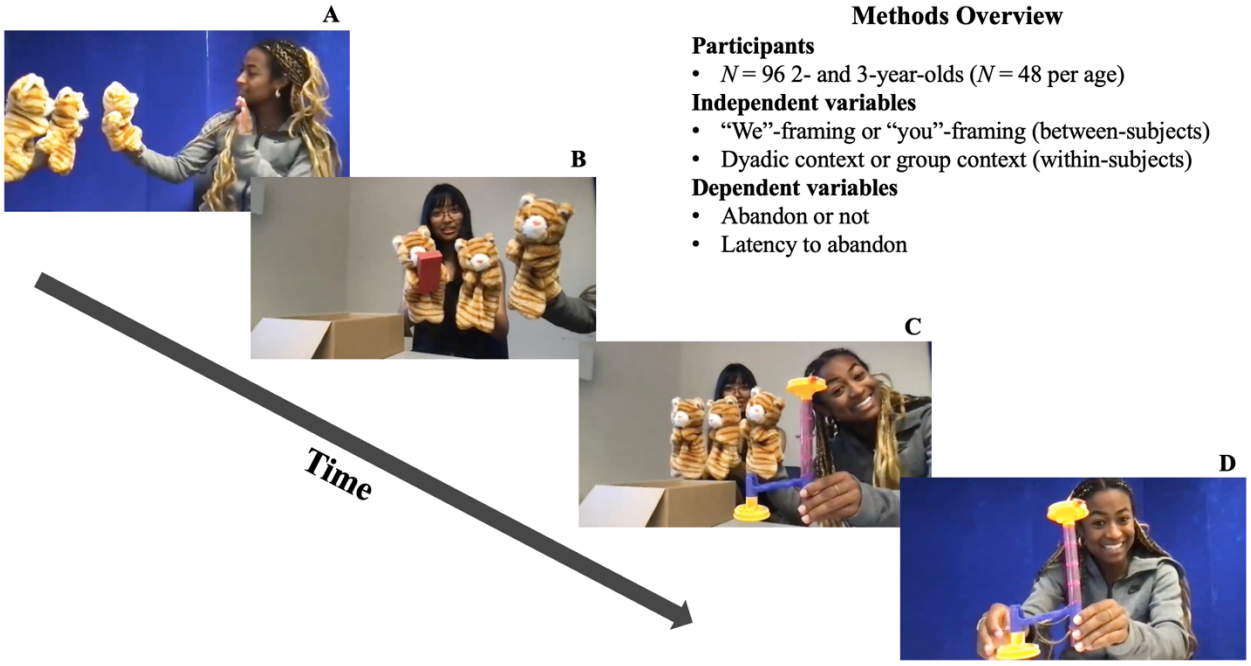
Participants were excluded if caregivers did not complete consent (n = 1); or if, on both trials, participants were inattentive or fussy (n = 1), E2 gave the incorrect framing (e.g., said "we" during "you"-framing, n = 1), or a caregiver or sibling interfered (e.g., said "we" during "you"-framing, n = 2). Caregivers identified as White (n = 76), Bi- or Multiracial (n = 11), Black (n = 4), Asian (n = 3), and Hispanic or Latine (n = 2). Caregivers claimed annual household incomes greater than $70,000 ($n$ = 69), between $30,000-70,000 ($n$ = 5), or no response (n = 22). Caregivers with children in the intended age range were emailed randomly from a university database of caregivers who had indicated interest in being contacted about child development studies. Study participation occurred after written, informed consent from caregivers. Participants were recruited from a moderate size city in the southeastern USA. The protocol was approved by the *XXX* Campus Institutional Review Board (protocol *XXX*).

Design. A 2 x 2 mixed design was used. Framing was a between-subjects factor ("we"-framing or "you"-framing). Partner number was a within-subjects factor (dyadic context or group context). The study was conducted remotely and "live," without prerecorded video or audio.

Materials. A cardboard box and wooden block; plastic bucket and ball; large, multi-panel cardboard sheet painted blue; three bunny puppets and three cat puppets; and two, handheld marble runs made of wood or plastic.

Procedure. ***Zoom Setup Phase***. Participants were tested via Zoom (Fig. 1). E1 greeted caregivers and participants. E1 asked caregivers to "Hide Self View" in Zoom, to ensure that Zoom was full screen, and to not talk during the procedure.

Fig. 1. *Procedure Timeseries*. Still A depicts partner introduction (group context), Still B a relatively boring game, Still C a peek by E1, and Still D a relatively fun game.



Warmup Phase. E1 asked participants friendly questions (e.g., their favorite animal) to ensure that participants were capable of and comfortable interacting via Zoom.

Test Phase. E1 asked participants whether "playing a game sounds fun" before saying that "some of my friends want to play, too!" E1 introduced participants to one puppet controlled by E2 (dyadic context) or three puppets, two of which were controlled by E2 and one by E1 (group context). Save for E1's introduction of a fun game and prompts (below), all subsequent talk came from the puppets (i.e., experimenters moved puppets' mouths, used a modified tone, etc.). Puppets used

"we"-framing or "you"-framing. In dyadic contexts, one partner spoke. In group contexts, all three partners spoke several times. However, one partner was the "speaker puppet," who spoke more.

Partner(s) greeted participants. "Hi there! It's so nice that **we** can meet, [*participant name*]! "**We** should find something fun to play with. Hm, what could **we** play with, [*participant name*]?" In group contexts, the speaker puppet said this. "You"-framing replaced the bolded, first-person plural pronouns with second-person singular pronouns. For example, the corresponding "you"-framing condition was "Hi there! It's so nice to meet **you**, [*participant name*]! "**You** should find something fun to play with. Hm, what could **you** play with, [*participant name*]?" Note the slight difference in the wording of the first sentence in the "you"-framing condition compared to the "we"-framing condition (i.e., "… that **we** can meet…" compared to "… to meet **you**"). This difference was intended to minimize the stiltedness of the interaction. Unless otherwise noted, below, all instances of "you"-framing simply replaced the corresponding personal pronouns.

The discourse continued with an introduction to a relatively boring, uninteresting game. Partner(s) said "Oh, there's a fun game next door that **we** can play! **We** would have so much fun playing. [*Participant name*], do **we** want to go see it? Do **we** want to go see the game?" After participants affirmed, the partner(s) said "Okay, great, **we** can go next door! The game is there waiting for **us**." In group contexts, each partner spoke two of these sentences. Subsequently, partner(s) said, "**Let's** go! **We're** going to have so much fun!" In group contexts, the first sentence was spoken by one partner and the second by another partner. In the "you"-framing condition, the first sentence was "**You** go ahead!"

Before participants left with their partner(s), E1 introduced them to a fun game. E1 said "Have fun! I'm going to stay here and play one of my favorite games, the [*fun game*] [*demonstrates*

game]. Come back whenever to play the [*fun game*] with me! I'll check in to see if playing the [*fun game*] together sounds fun. Have fun playing, [*participant's name*], I'll check in soon."

Participants "left" E1's "room" to go to another "room" with their partner(s) (Fig. 1). E1 surreptitiously panned the camera to another part of the testing space. Concurrently, the puppet(s) "walked" into the second room and thus remained in frame. In the first "room," with the fun game and E1, a cardboard sheet was painted blue and located behind E1. This formed a blue background. The second "room," with the boring game, was visually distinct, without a blue background. This was intended to simulate the experience of changing locations with partner(s) to play a game.

In the second room, participants and partners saw a cardboard box atop a table. The speaker puppet said "Look! This game is called the box game. Do **we** want to play the box game, [*participant name*]?" After participant affirmation, the partner said "Okay, great! **We** are going to have a lot of fun playing the box game. To play the box game, **we** have to put this block somewhere [*grabs block from box*]. **We** can put the block in the box, or not in the box. [*Participant name*], where do **we** want to put the block? Do **we** want to put it in the box, or not in the box? Where do **we** want to put the block?" Then, the partner did as participants instructed, before saying "Good job! That's how **we** play the box game. This is so much fun! **We** should play again!" Non-speaker puppets sometimes interjected with condition-specific praise, e.g., "**We** did it!" In group contexts, the speaker puppet performed the action requested by participants. The boring game was intended to be sufficiently boring, relative to the fun game, to induce variation in abandonment behavior.

Participants played the boring game with partner(s) up to eight times. On every second play, E1 peeked into the second room to invite participants to come back to the first room to play the fun game (Fig. 1). Thus, E1 gave four peeks per trial. While peeking, E1 displayed the fun game and delivered a verbal prompt. After the second play of the boring game, E1 peeked into the

second room. While showing the fun game to participants, E1 said "Hey [*to puppet(s)*]. Hey [*participant name*]. Don't forget about this!" If participants did not respond after three seconds, E1 said "Interested?" If participants verbally affirmed, or if they affirmed nonverbally and this was followed by another nonverbal or verbal affirmation (in response to a clarification question by E1), then partner(s) said "Okay, **let's** go!" Next, E1 surreptitiously panned the camera back to the first "room." Concurrently, the puppet(s) appeared to "walk" back into the first room. In contrast, if participants responded negatively or failed to respond to E1's prompt, then E1 left the screen and partner(s) said "**We** can play the box game again!" After the fourth play, E1's second prompt was the same as the first, except "Don't forget about this!" became "Don't forget about this really fun game I have!" After the sixth play, it became "Don't forget, the [*fun game*] is here to play whenever!" After the eighth play, it became "Want to play the [*fun game*] with me?"

Back in the first room, E1 asked participants if they had fun playing with their partner(s). Then, the partner(s) left the screen. Participants were alone with E1. E1 played the fun game several times for participants. Then, trial two began. The procedure above was repeated. The trial one partner number condition, boring game, and puppet were counterbalanced. Fun game order was fixed.

Coding. There were 192 trials. Of these, 14 participants had one trial excluded (six 2-year-olds, 11 "we"-framing, four group context). Trials were excluded if participants were fussy or if there was experimenter error or caregiver interference. This left 178 trials from 96 participants. Trials were coded for abandonment and abandonment prompt (i.e., the prompt on which participants abandoned their partner(s)). Participants "abandoned" (1) if they gave an affirmative verbal or nonverbal response to one of E1's four prompts (otherwise, 0). Participants almost always abandoned on both trials – only four 2-year-olds on five trials did not abandon. Thus, abandonment

was not analyzed further, and those five trials on which participants did not abandon were excluded from the analyses reported, below. Thus, 173 trials were analyzed for abandonment prompt. These 173 trials came from rom 94 participants, with 15 contributing one trial and 79 contributing two trials. Participants received 1 if they left on the first, 2 the second, 3 the third, and 4 the fourth E1 prompt. Abandonment prompt was dichotomized to {1, 2+} because participants abandoned on prompt one 131 times and only 43 times on later prompts (second prompt 23 times, third prompt 9 times, fourth prompt 10 times). This dichotomous variable was analyzed and reported, below. A naïve coder coded all trials. A knowledgeable coder coded a random 25% of all 192 trials, abandonment prompt kappa = .837, abandonment prompt agreement = 92%. Disagreement was resolved by using the naïve coder's code. Participant videos were coded via QuickTime.

Analysis. The data was analyzed in R (R Core Team, 2023) with Bayesian generalized linear mixed models (Gelman et al., 2013) via Stan (Stan Development Team, 2022) in brms (Bürkner, 2017). Bayesian inference quantifies researchers' confidence in the truth of propositions to aid in evaluations of theoretical claims (Savage, 1954; see discussion in Gigerenzer & Marewski, 2015). The estimated out-of-sample predictive utility of each of several candidate models was compared. Unless otherwise noted, models assumed a Bernoulli distributed outcome (logit link). Comparison was performed via stacking (Wolpert, 1992). Stacking finds a linear combination of weights that minimizes the estimated out-of-sample prediction error of an ensemble model. Greater model weight (max 1.00) implies greater estimated model predictive utility, relative to competing models. Stacking outperforms alternative comparison methods, like model averaging (Hinne et al., 2020), when no candidate model describes the latent process (Clarke, 2003; Yao et al., 2018). Weights were based on their approximate leave-one-out information content (all Pareto-k less than 0.7;

Vehtari et al., 2017). The DV "prompt" was nominal with values {1, 2+}. Predictors were discrete and sum coded, unless otherwise noted.

Posterior full model parameters were characterized in two ways. First, the 95% highest density interval (HDI) lower bound (LB), mode, and 95% HDI upper bound (UB) are reported as the triplet [95% LB, mode, 95% UB]. Second, following Meehl (1967), the posterior evidence for directional hypotheses about *x* are reported as $\Pr(\beta > x|\boldsymbol{D})$. We say that there is "strong evidence" that $\beta > x|\boldsymbol{D}$ if $1.000 \geq \Pr(\beta > x|\boldsymbol{D}) \geq .950$ or that $\beta < x|\boldsymbol{D}$ if $.000 \leq \Pr(\beta > x|\boldsymbol{D}) \leq .050$; "moderate evidence" if $.950 > \Pr(\beta > x|\boldsymbol{D}) \geq .900$ or if $.050 < \Pr(\beta > x|\boldsymbol{D}) \leq .100$; and "weak evidence" if. $900 > \Pr(\beta > x|\boldsymbol{D}) > .500$ or if $.100 < \Pr(\beta > x|\boldsymbol{D}) < .500$. The full model structure depended on the particular analysis under consideration and is reproduced where relevant.

The Appendix discusses the interpretation of $\Pr(\beta > x|\boldsymbol{D})$ as posterior odds, reports and justifies priors, discusses posterior model checks, and states all predictor levels. Appendix Table 1 reproduces the formulas for all models whose formula is not reproduced in the Results Section. Appendix Table 2 provides posterior parameter estimates for most models reported in the Results Section. Appendix Tables 3, 4, and 5 provide model posterior probabilities and Bayes factors for all models that were compared via stacking in the Results Section.

Transparency and Openness. Materials, data, and programming script to reproduce all analyses, figures, and tables are available at *XXX*. No aspect(s) of this study were preregistered.

Results

The "main analysis" precedes "additional analyses." The main analysis investigated the posterior parameters of a control variables model of abandonment prompt, compared seven models

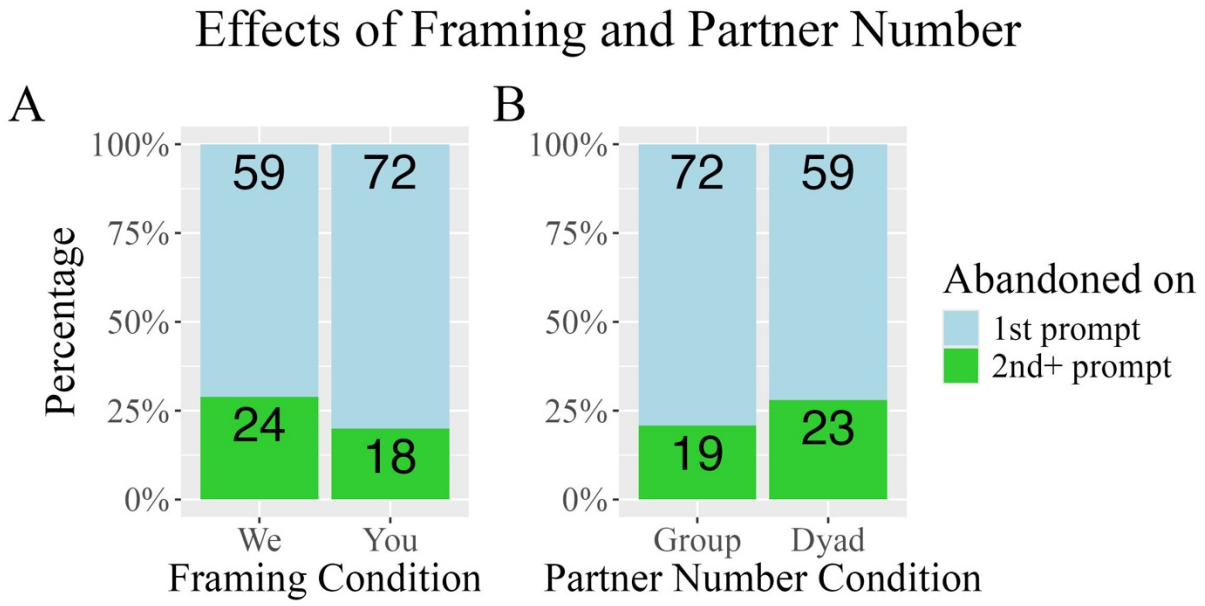
of abandonment prompt, and investigated the posterior parameters of a full model of abandonment prompt. The additional analyses investigated condition effects within levels of the other condition; compared and investigated models of abandonment prompt in 2-year-olds and 3-year-olds, respectively; compared and investigated models of the ratio of participants' abandonment prompt scores; and investigated models fitted to all 96 participants' abandonment prompt data (i.e., including the five excluded observations, noted above).

Main Analysis. A model comprised of control variables was fitted to participants' abandonment prompts. The model formula was prompt ~ sex + e1 + e2 + trial number + order game + puppet + game + (condition partner | participant id). There was strong evidence that participants abandoned their partner(s) earlier on trial two than on trial one, $[0.28, 0.64, 1.08], \Pr(\beta > 0|\boldsymbol{D}) = 1.000$. There was moderate evidence that participants abandoned cat partner(s) earlier than bunny partner(s), $[-0.11, 0.25, 0.64], \Pr(\beta > 0|\boldsymbol{D}) = .913$. There was moderate evidence that participants abandoned earlier with one E2 than others, $[-0.15, 0.54, 1.21], \Pr(\beta > 0|\boldsymbol{D}) = .936$. There was weak evidence for the direction of all other effects (see Appendix Table 2). Trial number was included as a control variable where appropriate, below.

Seven models were fitted to the data. All models included age and trial number predictors, with a random intercept of participant ID and a random slope of partner number condition. Six models included linear or interactive combinations of age with partner number condition or framing condition (see formulas in Appendix Table 1). Model comparison via stacking was equivocal between a trial + age model and a trial + age + framing model, both weights = .500. The other models received zero weight. This suggest some utility of framing condition for predicting participants' abandonment prompts. However, model comparison via Bayes factor favored the trial + age model over the trial + age + framing model by 2 to 1 (see Appendix Table 3).

To investigate the direction of age, framing, and partner number effects, the posterior parameters of the full, three-way interaction model were investigated (i.e., age * framing * partner number). In the posterior model, there was moderate evidence that participants who heard "we"-framing remained longer with their partner(s) than participants who heard "you"-framing, $[-0.13, 0.26, 0.67], \Pr(\beta > 0|\boldsymbol{D}) = .908$. Figure 2A displays the data. Moreover, there was strong evidence that 2-year-olds remained longer with their partner(s) than did 3-year-olds, $[0.13, 0.52, 0.93], \Pr(\beta > 0|\boldsymbol{D}) = .996$. Additionally, there was strong evidence that participants abandoned sooner on trial two than on trial one, $[0.20, 0.59, 0.99], \Pr(\beta > 0|\boldsymbol{D}) = .999$.

Fig. 2. Effect of framing condition (Panel A) and partner number condition (Panel B) on abandonment prompt. Inset values display counts. There was moderate evidence for the direction of the framing effect, but only weak evidence for that of the partner number effect. Importantly, note that participants could contribute up to two observations in the "we"-framing bar or the "you"-framing bar (Panel A), depending on which framing condition they received.

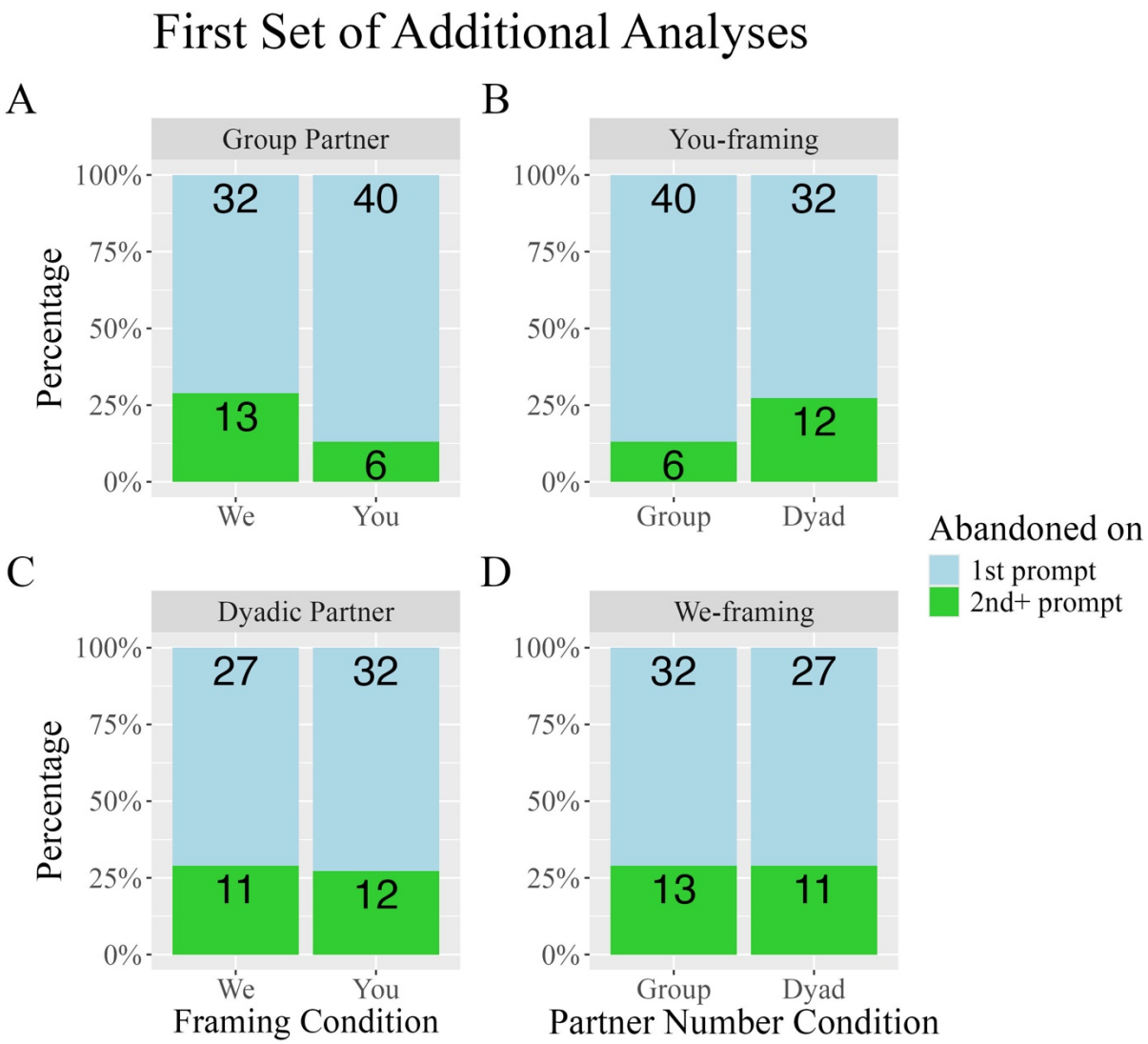


Taken together, these results lend some support to the notion that "we"-framing increased participants' sense of commitment to their partner(s). Against predictions, there was little evidence for the existence (per model comparison) or direction (per posterior parameters) of partner number effects on participants' abandonment prompt. Moreover, there was no clear evidence for an interaction of age with either predictor. However, 2-year-olds remained longer with their partners than did 3-year-olds.

Additional Analyses. There were four sets of additional analyses. The first set of additional analyses investigated the direction of framing effects within partner number condition; and the direction of partner number effects within framing condition. This involved fitting four models to different subsets of the data investigated in the main analysis, above. The first model investigated effects of "we"-framing in the group partner condition (formula: trial number + age * framing, *n* = 91 observations from as many participants, Fig. 3A). This model indicated moderate evidence that participants remained longer with their partner(s) after "we"-framing compared to "you"-framing, $[-0.11, 0.43, 0.95], \Pr(\beta > 0|\boldsymbol{D}) = .939$. The second model investigated effects of "we"-framing in the dyadic partner condition (same formula, *n* = 82 observations from as many participants, Fig. 3C). This model indicated weak evidence that participants remained longer with their partner(s) after "we"-framing compared to "you"-framing, $[-0.43, 0.11, 0.63], \Pr(\beta > 0|\boldsymbol{D}) = .655$. The third model investigated effects of partner number in the "you"-framing condition [formula: trial number + age * partner number + (partner number | participant ID), *n* = 90 observations from 47 participants, Fig. 3B]. This model indicated weak evidence that participants remained longer with dyadic than with group partners, $[-0.88, -0.29, 0.28], \Pr(\beta > 0|\boldsymbol{D}) = .165$. The fourth model investigated effects of partner

number in the "we"-framing condition (same formula, n = 83 observations from 47 participants, Fig. 3D). This model indicated weak evidence that participants remained longer with group than with dyadic partners, $[-0.48, 0.05, 0.55], \Pr(\beta > 0|\boldsymbol{D}) = .542$. Additionally, all four models indicated moderate to strong evidence for the same-direction trial number and age effects noted in the previous paragraph (see Appendix Table 2). In sum, there was some evidence to suggest that participants remained longer following "we"-framing compared to "you"-framing, given that they had group partners. There was little evidence for the direction of framing effects with dyadic partners and for partner number within either framing condition.

Fig. 3. Data for the first set of additional analyses. Inset values display counts. Data for framing effects given a group partner (Panel A) or dyadic partner (Panel C) and for partner number effects given "you"-framing (Panel B) or "we"-framing (Panel D). There was moderate evidence for the direction of framing effects in the group partner condition (Panel A). There was weak evidence for direction in all other comparisons (Panels B-D).

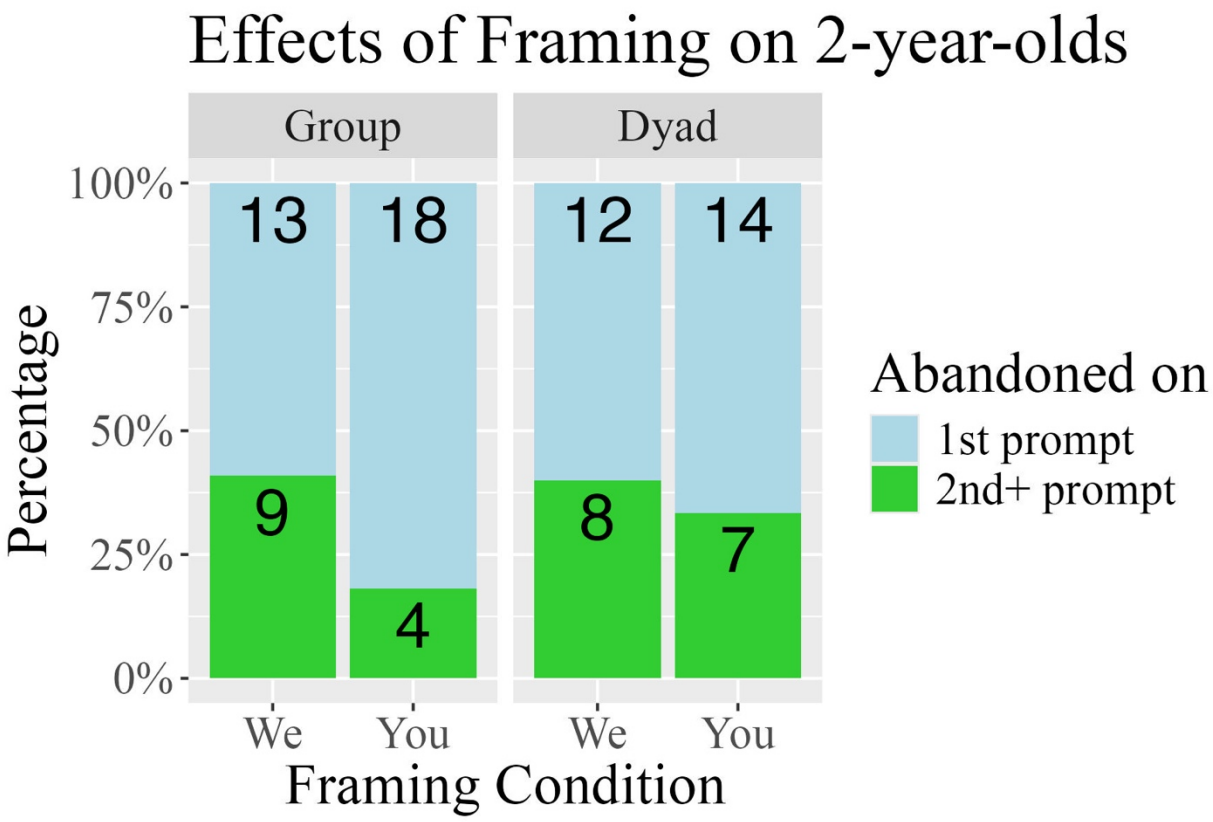


The second set of additional analyses investigated condition effects within 2-year-olds and 3-year-olds, respectively. Five models were fitted to the data within age group, ranging from a model that included trial number only to the full model, composed of trial number plus the interaction of framing and partner number conditions (see Appendix Table 1). Among 2-year-olds, model comparison via stacking slightly supported the trial + framing model, weight = .580, over the trial only model, weight = .420. All other models received zero weight. (However, model comparison via Bayes factor favored the trial only model over the trial + framing model by 4 to 1;

see Appendix Table 4.) In the full, trial number + framing * partner number model, there was moderate evidence that 2-year-olds took longer to abandon their partner(s) after "we"-framing compared to "you"-framing, $[-0.10, 0.38, 0.85], \Pr(\beta > 0|\boldsymbol{D}) = .937$ (Fig. 4; see Appendix Table 2 for parameter estimates). In contrast, there was weak evidence that 2-year-olds remained longer with dyadic than with group partners, $[-0.63, -0.15, 0.31], \Pr(\beta > 0|\boldsymbol{D}) = .260$. Moreover, there was moderate evidence that 2-year-olds in the group partner condition remained longer with their partners following "we"-framing compared to "you"-framing, $[-0.12, 0.50, 1.21], \Pr(\beta > 0|\boldsymbol{D}) = .944$, but only weak evidence for the same effect with dyads, $[-0.47, 0.19, 0.81], \Pr(\beta > 0|\boldsymbol{D}) = .702$ (Fig. 4). This suggests that "we"-framing framing was more effective among 2-year-olds when they had a group compared to one partner.

In contrast, among 3-year-olds, the same model comparison supported the trial number only model, weight = 1.000. All other models received zero weight. In the trial number + framing * partner number model, there was weak evidence that 3-year-olds took longer to abandon their partner(s) after "we"-framing compared to "you"-framing, $[-0.49, 0.11, 0.74], \Pr(\beta > 0|\boldsymbol{D}) = .653$. There was weak evidence that 3-year-olds remained longer with dyadic than with group partners, $[-0.70, -0.08, 0.54], \Pr(\beta > 0|\boldsymbol{D}) = .397$. In sum, these results suggest that, compared to "you"-framing, 2-year-olds remained longer with partner(s) after "we"-framing than "you"-framing (given group partnership). Against predictions, no effects were found among 3-year-olds.

Fig. 4. Effects of framing in 2-year-olds with group or dyadic partnership. Inset values display counts. There was moderate evidence that 2-year-olds remained longer after "we"-framing compared to "you"-framing.

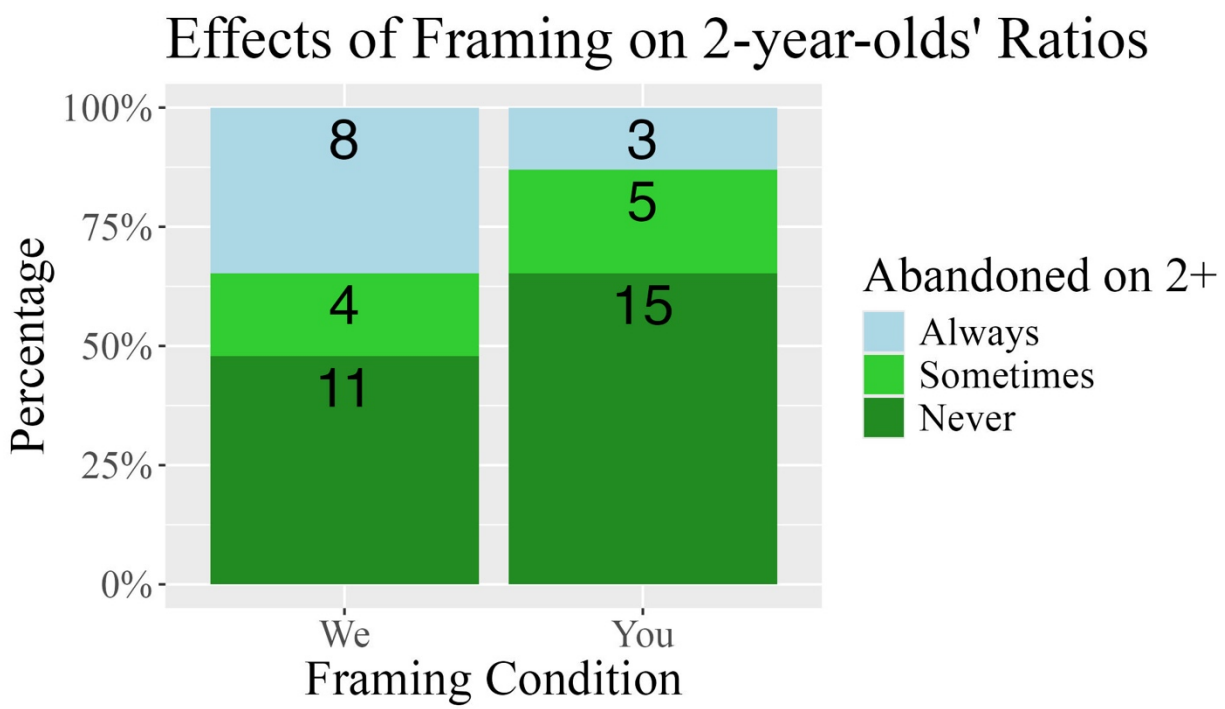


The third set of additional analyses investigated abandonment prompt by participant. Specifically, the dependent measure was the ratio of trials in which participants abandoned their partner(s) on the second or later prompts. Participants contributed either one or two trials to this analysis. Only participants who contributed two trials could receive a ratio score of .50. Specifically, data would be coded .50 only if a participant abandoned their partner(s) on the second or later prompt on one out of two trials. Altogether, the response variable included levels .00 ("never"), .50 ("sometimes"), and 1.00 ("always"). The data was modeled as a latent categorical process (multivariate logit link, "never" reference category). Because this outcome collapsed across repeated measurements, and only framing was manipulated between subjects, this analysis only investigated framing and not partner number effects. Three models were compared, including an age only model, an age + framing model, and an age * framing model (see Appendix Table 1).

Model comparison via stacking supported the age only model, weight = .810, over the age + framing model, weight = .190. Moreover, model comparison via Bayes factor favored the age only model over the age + framing model by 4 to 1 (see Appendix Table 5). In the full, age * framing model, there was weak evidence for the direction of all condition effects and their interaction, all $\Pr(\beta > 0|\boldsymbol{D})$ between .429 and .824 (see Appendix Table 2). There was strong evidence that 2-year-olds were more likely than 3-year-olds to always, as opposed to never, abandon their partner(s) on the second or later prompt, $[0.19, 0.82, 1.64], \Pr(\beta > 0|\boldsymbol{D}) = .995$.

Two further models explored condition effects within age. A model including only a framing predictor was fitted to 2-year-olds' ratio data. The posterior model indicated strong evidence that 2-year-olds were more likely to always, instead of never, abandon their partners(s) on the second or later (as opposed to on the first) prompt after "we"-framing compared to "you"-framing, $[-0.11, 0.57, 1.32], \Pr(\beta > 0|\boldsymbol{D}) = .951$ (Fig. 5). Among 3-year-olds, there was weak evidence for framing effects, $\Pr(\beta_{sometimes} > 0|\boldsymbol{D}) = .370$, $\Pr(\beta_{always} > 0|\boldsymbol{D}) = .483$. In sum, the main posterior model comparison and posterior parameters suggested little effect of framing on participants' abandonment prompt ratios. However, in a separate model of 2-year-olds' ratios, there was strong evidence that 2-year-olds remained longer with their partner(s) following "we"-framing compared to "you"-framing. There was no such effect among 3-year-olds.

Fig. 5. Effects of framing on 2-years-olds' abandonment ratios. Inset values display counts. There was strong evidence that 2-year-olds remained always, as opposed to never, after "we"-framing compared to "you"-framing.



The fourth set of additional analyses investigated two models fitted to the full dataset. The full dataset included the five observations of participants who never abandoned their partner(s) (see Coding, above). Thus, both models were fitted to 178 observations from 96 participants. The first model was a Bernoulli model. This model assumed that never abandoning partner(s) was the same outcome as abandoning after two or more trials. The second model was a categorical model ("remained" reference category). This model assumed that never abandoning partner(s) was not the same as abandoning after one trial or two or more trials. This is arguably a more appropriate way to model the data than is the first method (i.e., of stipulating a ceiling value for participants who did not abandon). This point may be practically immaterial, however, as both models indicated weak evidence for the direction of condition effects (see Appendix Table 2).

To recap, the additional analyses supported the notion that "we"-framing increased participants' sense of commitment compared to "you"-framing. This was identified most clearly

in analyses that investigated the direction of effects of framing in the group partner condition and in 2-year-olds, respectively. Moreover, an analysis of the ratio of trials in which participants abandoned on the second or later prompt clearly support the notion that 2-year-olds were more committed to their partners after "we"-framing compared to "you"-framing. These results with 2-year-olds are particularly surprising. So far as we are aware, this study presents the first evidence of a manipulation that reliably influences 2-year-olds' abandonment behavior.

Discussion

This study investigated effects of "we"-framing and partner number on 2- and 3-year-olds' commitment. Participants played a boring game with puppet partner(s). While playing, participants were tempted to abandon their partner(s) to play a fun game. Before and while playing the boring game, either one partner or three partners used "we"-framing or "you"-framing to discuss the game. Three-year-olds were predicted to take longer to abandon their partner(s) after "we"-framing than "you"-framing and in dyadic contexts than group contexts (especially after "we"-framing in dyadic contexts). In contrast, 2-year-olds were not predicted to be impacted by either manipulation. These predictions were based on research that suggests that 3-year-olds understand commitment but 2-year-olds do not (Tomasello, 2019). Arguably, this is because 3-year-olds have undergone a normative turn and so understand obligation.

In the main analysis, there was some evidence to suggest that participants remained longer with their partner(s) after "we"-framing compared to "you"-framing. Surprisingly, there was little evidence that the effect of framing depended on age. Moreover, there was little evidence for effects of partner number on participants' behavior; and, while a model comparison clearly disfavored several competing models, the results were ambivalent between a null (trial + age) and reduced

framing model (trial + age + framing) of participants' behavior. These results partially accorded with predictions. While there was some evidence to suggest that participants were more committed after "we'-framing than "you"-framing, this did not depend on age. Moreover, there was no clear effect of partner number. Four sets of additional analyses investigated the data in various ways.

The first set of additional analyses isolated effects of "we"-framing to group, but not dyadic, partnership. That is, only when participants were partnered with groups did they remain longer with their partners after "we"-framing compared to "you"-framing (see Fig. 3A). One interpretation of these results is that, given group partnership, participants were more committed to group partners following "we"-framing compared to "you"-framing.

The second set of additional analyses investigated condition effects within age. Surprisingly, model comparison and posterior parameter analyses indicated stronger evidence for the direction of "we"-framing effects among 2- than 3-year-olds. Inspection of the data suggests that a good explanation for this age pattern is not that 2-year-olds are, in fact, more strongly influenced by "we"-framing than are 3-year-olds. To be clear, this explanation may have merit. However, the present data are neutral on this point. Rather, the present data support the explanation that 3-year-olds simply abandoned their partner(s) overwhelmingly often on the first prompt. Indeed, whereas the odds of 2-year-olds abandoning on the first prompt rather than second or later prompts was 2 to 1 (57 to 28 trials), the same number for 3-year-olds was 5 to 1 (74 to 14 trials). This suggests a kind of floor effect among 3-year-olds that was not present for 2-year-olds. Indeed, this floor effect was marked in the second trial, in which the same odds were 22 to 1 among 3-year-olds (44 to 2 trials) but 3 to 1 among 2-year-olds (33 to 11 trials). Regardless of the interpretation of 3-year-olds' behavior, the interpretation stands that – at least behaviorally – 2-year-olds were more committed to their partner(s) after "we"-framing compared to "you"-framing.

The third set of additional analyses differed from the others in its outcome variable. In these analyses, the outcome was the ratio of trials in which participants left on the first prompt as opposed to the second or later prompts (out of one or two trials, depending on the number of trials contributed by participants). Again, stronger evidence for the direction of framing effects occurred among 2-year-olds compared to 3-year-olds. Specifically, there was strong evidence to suggest that 2-year-olds were more committed to their partner(s) following "we"-framing compared to "you"-framing (Fig. 5).

Taken together, we suggest that this study has produced the first reliable statistical evidence, to the present authors' knowledge, of a manipulation that influences 2-year-olds' commitment towards partner(s), namely, "we"-framing. However, it is reiterated that 2-year-olds' behavior in this study – though resembling that of 3-year-olds in other studies of commitment (reviewed in the Introduction) – may be supported by psychological skills and motives distinct from the group-mindedness characteristic of 3-year-olds.

Before proceeding to further interpretation of 2-year-olds' behavior in this study, we note three limitations of the present evidence. First, all three sets of model comparisons via stacking were not unequivocal in their support for the favored nonnull model (if a nonnull model was favored). Second and relatedly, model comparison Bayes factor (see Appendix Tables 3-5) indicated support (typically "weak", following Jeffreys, 1961) for the null model over any alternative. Third, the fourth set of additional analyses indicated that the strength of evidence of framing effects relied on excluding from analysis those participants who did not abandon.

How might sense be made of 2-year-olds' behavior following "we"-framing? One explanation is that the present study may have stoked a more "minimal" sense of commitment than in previous studies (Michael et al., 2015). This more minimal sense of commitment may not rely

on an understanding of obligation towards one's self-inclusive moral community but, rather, something like responsibility towards partners (Tomasello, 2016). Responsibility is distinct from full-blown, normative obligation in its partner-based organization, in which one recognizes another's psychological co-equality with oneself (Darwall, 2004). This suggests that responsibility – in the sense in which we are using the word here – should not rely on the same agent-neutral, group-minded conceptual structure that emerges at around 3 years of age (Tomasello, 2019) and that supports, e.g., normative protest following defection from commitments (Kachel et al., 2018).

Specifically, this means that 2-year-olds may display commitment *qua* responsibility insofar as they remain longer with partners following "we"-framing compared to "you"-framing but do not also, e.g., engage in normative protest. In contrast, 3-year-olds display commitment *qua* obligation insofar as they are committed to, and normatively protest, defectors. This might be seen as a kind of behavioral definition of commitment *qua* responsibility, as distinct from commitment *qua* obligation. The word "responsibility" is suggested, but the phenomenon could be called anything so long as the definition in terms of testable behavioral predictions is clear. Indeed, this distinction is testable, e.g., in a study that investigates commitment and norm enforcement in 2- and 3-year-olds. Were this distinction useful for predicting behavior, it would have implications for our understanding of the ontogeny of commitment – first as arising from the recognition of partner co-equality and only later as a normatively laden, group-minded phenomenon. The plausibility of this argument is buttressed by the fact that peer collaboration begins at 2 years of age (reviewed in Brownell, 2011). Thus, our suggestion that 2-year-olds develop commitment *qua* responsibility may be seen as a case of coordinated timing in the development of functionally related traits (West-Eberhard, 2003). In this light, commitment *qua* responsibility resembles the account of Michael and Pacherie (2014) in that commitment is a tool for uncertainty reduction

with a novel set of partners whose commitment is uncertain (i.e., peers), in contrast to the relatively certain commitment of children's typical partners up to that point in ontogeny (i.e., adults).

An alternative explanation of 2-year-olds' behavior following "we"-framing is offered by Michael and Székely (2018). Those authors might class "we"-framing as a relatively implicit linguistic commitment cue that stokes a correspondingly implicit sense of commitment. This sense of commitment is minimal insofar as it does not rely on the common ground obligations entailed by explicit commitments. This account may go some way towards explaining why "we"-framing was more effective when 2-year-olds were in groups than in dyads (see the second set of additional analyses). If "we"-framing were a relatively implicit commitment cue (cf. say, a promise), then 2-year-olds may be more sensitive to "we"-framing from multiple partners compared to just one.

There were several limitations to this study. First, future work should carefully calibrate the boringness of the boring game relative to the fun game, and to the prompts' explicitness, for 3-year-olds. For instance, less explicit prompts that lack the "…Interested?" may increase variation in 3-year-olds' abandonment prompt. A second limitation was remote testing. On the one hand, this study was the first remote investigation of children's commitment. On the other hand, these results should be replicated in a laboratory setting. This is important for increased compatibility and comparability with prior research. Moreover, children may reason about costs and benefits of abandonment differently in remote compared to laboratory situations. A third limitation was that this study was limited in the measures used to gauge children's sense of commitment. This study focused solely on children's tendency and time to abandon partners. These measures are probably informative with respect to children's sense of commitment. Nonetheless, future research might include measures that provide complementary windows into children's sense of commitment, e.g., partner release (Bonalumi et al., 2023) or leave taking (Vasil & Tomasello, 2022).

A final limitation concerns power (see Gelman & Carlin, 2014). This study was likely underpowered with respect to the possible three-way interaction noted in the Introduction (i.e., that 3-year-olds in dyadic "we"-framing contexts would take the longest to abandon). However, power was less of a concern with respect to the two, key age-condition interactions predicted in the Introduction. Indeed, this was especially true for effects of partner number (n = 48 per age per condition). As for framing (n = 24 per age per condition), a prior study that used that sample size reported reliable evidence for "we"-framing effects in 3-year-olds (Vasil & Tomasello, 2022). Regardless, future research might recruit larger, more diverse, preregistered samples to increase power and generalizability. Caution is urged against overzealously interpreting or generalizing these results. It was not found that an effect of "we"-framing "exists" in 2-year-olds, with certitude. Rather, merely "strong" evidence was found to believe that the effect probably takes a specified direction, and this evidence came from one, online study of one small, WEIRD sample.

In conclusion, this study investigated effects of "we"-framing and partner number on young children's sense of commitment. Three-year-olds quickly abandoned their partner(s) in all conditions. Surprisingly, 2-year-olds who heard "we"-framing took longer to abandon their partner(s) than those who heard "you"-framing. This is the first report of a manipulation that influences 2-year-olds' commitment. One explanation is that a not-fully-normative, partner-based sense of responsibility at 2 years of age precedes a normative sense of obligation at 3 years of age.

Appendix

Appendix Table 1. Outcome variables and model formulas for all models investigated via model comparison in the main and additional analyses.

Analysis	Outcome	Formula
Main	prompt	trial num + age + (partner | participant) trial num + age + partner + (partner | participant) trial num + age * partner + (partner | participant) trial num + age + framing + (partner | participant) trial num + age * framing + (partner | participant) trial num + age + partner + framing + (partner | participant) trial num + age * framing * partner + (partner | participant)
2nd Set	prompt	trial num + (partner | participant) trial num + partner + (partner | participant) trial num + framing + (partner | participant) trial num + framing + partner + (partner | participant) trial num + framing * partner + (partner | participant)
3rd Set	ratio	age age + framing age * framing

Appendix Table 2. "R1" and "R2" in the Analysis column denote "data recode 1" and "data recode 2," respectively, as described in the Results Section. Column "Dis. Par." displays distributional parameter; "secondplus" denotes the distributional parameter for participants who abandoned on the second or later prompt. Column "Estimate" displays maximum a posteriori value. Column "Post. Prob." displays $\Pr(\beta > 0.00|\boldsymbol{D})$. "Evidence" is labeled as stated in Coding, above (see below for more description).

Analysis	Dis. Par.	Parameter	Estimate	95% HDI	Post. Prob	Evidence
Control		intercept	-1.19	[-1.77, -0.70]	.000	strong
		sex	0.23	[-0.15, 0.61]	.889	weak
		e11	0.20	[-0.37, 0.75]	.754	weak

	e21	0.02	[-0.57, 0.62]	.538	weak
	e22	0.54	[-0.15, 1.21]	.936	moderate
	e23	-0.05	[-0.86, 0.82]	.492	weak
	trial num	0.64	[0.28, 1.08]	1.000	strong
	order game	0.18	[-0.26, 0.62]	.784	weak
	pup	0.25	[-0.11, 0.64]	.913	moderate
	game	-0.16	[-0.57, 0.24]	.212	weak
Main	intercept	-1.32	[-1.77, -0.95]	.000	strong
	trial num	0.59	[0.20, 0.99]	.999	strong
	age	0.52	[0.13, 0.93]	.996	strong
	framing	0.26	[-0.13, 0.67]	.908	moderate
	partner	-0.15	[-0.56, 0.25]	.227	weak
	age * framing	0.14	[-0.27, 0.53]	.746	weak
	age * partner	-0.03	[-0.40, 0.40]	.498	weak
	framing * partner	0.19	[-0.21, 0.59]	.818	weak
	age * framing * partner	0.01	[-0.39, 0.41]	.533	weak
Set 1 (Group)	intercept	-1.44	[-2.05, -0.90]	.000	strong
	trial num	0.43	[-0.11, 0.95]	.939	moderate
	age	0.50	[-0.06, 1.06]	.960	strong
	framing	0.43	[-0.11, 1.02]	.943	moderate
	age * framing	0.11	[-0.43, 0.69]	.683	weak
Set 1 (Dyad)	intercept	-1.22	[-1.83, -0.67]	.000	strong
	trial num	0.68	[0.17, 1.31]	.996	strong
	age	0.49	[0.00, 1.05]	.974	strong
	framing	0.11	[-0.43, 0.63]	.655	weak
	age * framing	0.12	[-0.42, 0.63]	.651	weak
Set 1 (You)	intercept	-1.60	[-2.28, -1.05]	.000	strong
	trial num	0.69	[0.12, 1.33]	.992	strong
	age	0.36	[-0.18, 0.94]	.901	moderate
	partner	-0.29	[-0.88, 0.28]	.165	weak
	age * partner	-0.07	[-0.61, 0.54]	.441	weak
Set 1 (We)	intercept	-1.02	[-1.59, -0.52]	.000	strong
	trial num	0.44	[-0.03, 0.97]	.967	strong
	age	0.58	[0.11, 1.14]	.992	strong
	partner	0.05	[-0.48, 0.55]	.542	weak
	age * partner	0.02	[-0.51, 0.53]	.525	weak
Set 2 (2-yo)	intercept	-0.77	[-1.28, -0.30]	.001	strong
	trial num	0.37	[-0.09, 0.85]	.941	moderate
	framing	0.38	[-0.10, 0.85]	.937	moderate

		partner	-0.15	[-0.63, 0.31]	.260	weak
		framing * partner	0.19	[-0.28, 0.68]	.791	weak
Set 2 (3-yo)		intercept	-1.94	[-2.74, -1.33]	.000	strong
		trial num	0.91	[0.28, 1.67]	.998	strong
		framing	0.11	[-0.49, 0.74]	.653	weak
		partner	-0.08	[-0.70, 0.54]	.397	weak
		framing * partner	0.07	[-0.52, 0.73]	.615	weak
Set 3	Always	intercept	-1.85	[-2.68, -1.20]	.000	strong
	Sometimes	intercept	-1.11	[-1.66, -0.62]	.000	strong
	Always	age	0.82	[0.19, 1.64]	.995	strong
		framing	0.31	[-0.41, 1.03]	.804	weak
		age * framing	0.32	[-0.38, 1.07]	.824	weak
	Sometimes	age	0.04	[-0.47, 0.54]	.556	weak
		framing	-0.05	[-0.56, 0.45]	.429	weak
		age * framing	0.06	[-0.43, 0.57]	.609	weak
Ser 3 (2-yo)	Always	intercept	-0.88	[-1.69, -0.20]	.005	strong
	Sometimes	intercept	-1.02	[-1.83, -0.31]	.002	strong
	Always	framing	0.57	[-0.11, 1.32]	.951	strong
	Sometimes	framing	0.04	[-0.71, 0.74]	.522	weak
Set 3 (3-yo)	Always	intercept	-2.50	[-3.87, -1.55]	.000	strong
	Sometimes	intercept	-1.07	[-1.82, -0.47]	.000	strong
	Always	framing	-0.05	[-1.09, 1.05]	.483	weak
	Sometimes	framing	-0.11	[-0.77, 0.54]	.370	weak
Set 4 (R1)		intercept	-1.24	[-1.64, -0.86]	.000	strong
		trial num	0.52	[0.16, 0.91]	.998	strong
		age	0.60	[0.24, 1.02]	.999	strong
		framing	0.25	[-0.14, 0.63]	.891	weak
		partner	-0.23	[-0.60, 0.18]	.149	weak
		age * framing	0.13	[-0.29, 0.50]	.701	weak
		age * partner	-0.03	[-0.43, 0.35]	.415	weak
		framing * partner	0.17	[-0.22, 0.56]	.793	weak
		age * framing * partner	0.00	[-0.41, 0.36]	.462	weak
Set 4 (R2)	first	intercept	3.57	[2.76, 4.58]	1.000	strong
	secondplus	intercept	2.16	[1.27, 3.19]	1.000	strong
		trial num	-0.08	[-0.79, 0.62]	.387	weak
		age	-0.79	[-1.69, -0.02]	.022	strong
	first	framing	0.10	[-0.77, 0.88]	.544	weak
		partner	0.44	[-0.43, 1.20]	.818	weak
		age * framing	0.08	[-0.72, 0.92]	.591	weak

	secondplus	age * partner	0.36	[-0.49, 1.16]	.787	weak
		framing * partner	0.09	[-0.74, 0.92]	.585	weak
		age * framing * partner	0.14	[-0.67, 0.98]	.640	weak
		trial num	0.55	[-0.19, 1.29]	.924	moderate
		age	-0.26	[-1.13, 0.60]	.285	weak
		framing	0.37	[-0.50, 1.21]	.790	weak
		partner	0.18	[-0.66, 1.06]	.671	weak
		age * framing	0.17	[-0.62, 1.10]	.706	weak
		age * partner	0.35	[-0.53, 1.20]	.773	weak
		framing * partner	0.28	[-0.57, 1.17]	.748	weak
		age * framing * partner	0.14	[-0.71, 1.02]	.636	weak

Appendix Table 3. Posterior probabilities and Bayes factors of each in the main analysis. Bayes factors (BF) computed by dividing the posterior probability of the alternative model by the posterior probability of the null model (uniform prior on models).

Model	Post. Prob.	BF
trial + age	.491	
trial + age + partner	.114	0.232
trial + age * partner	.021	0.043
trial + age + framing	.249	0.508
trial + age * framing	.064	0.130
trial + age + partner + framing	.061	0.124
trial + age * framing * partner	.000	0.000

Appendix Table 4. Posterior probabilities and Bayes factors of each model in the second set of additional analyses (i.e., within age). Bayes factors (BF) computed by dividing the posterior probability of the alternative model by the posterior probability of the null model (uniform prior on models).

Model	Post. Prob. (2yr)	BF (2yr)	Post. Prob. (3yr)	BF (3yr)
trial	.694		.857	
trial + partner	.066	0.095	.058	0.067
trial + framing	.175	0.252	.060	0.071
trial + framing + partner	.049	0.071	.018	0.021

trial + framing * partner	.016	0.023	.006	0.007

Appendix Table 5. Posterior probabilities and Bayes factors of each model in the third set of additional analyses (i.e., by participant). Bayes factors (BF) computed by dividing the posterior probability of the alternative model by the posterior probability of the null model (uniform prior on models).

Model	Post. Prob.	BF
age	.791	
age + framing	.183	0.232
age * framing	.026	0.033

Labels of Evidentiary Strength. Following Savage (1962), $\Pr(\beta > 0|\boldsymbol{D})$ has the interpretation as the posterior odds of directional hypotheses about x, $\frac{\Pr(\beta>x|\boldsymbol{D})}{1-\Pr(\beta>x|\boldsymbol{D})} = \frac{\Pr(\beta>x|\boldsymbol{D})}{\Pr(\beta<x|\boldsymbol{D})}$. The labels of evidentiary strength "moderate" and "strong" attached to statements of $\Pr(\beta > 0|\boldsymbol{D})$ imply that the probability of a directional hypothesis is at least 9 and 19 times greater than the alternative, respectively. Note that priors symmetric about $\beta = 0$ were used (see below). Consequently, $\Pr(\beta > 0|\boldsymbol{D})$ has the interpretation of the numerator of a Bayes factor in favor of one hypothesis, $\beta > 0$, relative to the alternative, denominator hypothesis, $\beta < 0$ (Jeffreys, 1961). That is, $\frac{\Pr(\beta>0|\boldsymbol{D})}{\Pr(\beta<0|\boldsymbol{D})} = \frac{\Pr(\boldsymbol{D}|\beta>0)}{\Pr(\boldsymbol{D}|\beta<0)} \times \frac{\Pr(\beta>0)}{\Pr(\beta<0)} = \frac{\Pr(\boldsymbol{D}|\beta>0)}{\Pr(\boldsymbol{D}|\beta<0)} \times \frac{.50}{.50} = \frac{\Pr(\boldsymbol{D}|\beta>0)}{\Pr(\boldsymbol{D}|\beta<0)}$. Several key references suggested this analytical method. Bakan (1966) and Meehl (1967) discuss the triviality of the nil-is-null hypothesis. Gelman and Tuerlinckx (2000) demonstrate the relevance of Type S error for inference about directional hypotheses. Rouder (2014) demonstrates the irrelevance of Type I error for evidentiary strength assessments as measured by Bayes factors (though, for models, not directional hypotheses). Makowski et al. (2019) compare Bayesian directional hypotheses to frequentist *p*-values (see also Marsman & Wagenmakers, 2017).

Priors. The following priors were used: $Intercept \sim N(0, 1.50)$, $\beta \sim N(0, 1.00)$, $SD \sim t(10, 0, 0.10)$. These priors were chosen with the ideal of being weakly informative, save for the random intercept prior which required more prior information for model convergence. This means that the priors assigned nonnegligible credibility to a broad but plausible set of values of the effect sizes of framing and of partner number.

Posterior Checks. Trace plots, graphical posterior predictive checks, effective sample sizes, and R-hats were adequate (Gelman et al., 2013). One exception was that 3-year-olds' model of trial two abandonment prompt had some difficulty replicating the observed data. However, this appeared to be due more to extreme response profiles than to model deficiencies. Models were sampled with 7 chains (2500 warmup, 7500 test iterations; zero divergences). Marginal posteriors and model weights were stable across independent sets of posterior samples with weaker and more informative priors than those used in the analyses reported, above.

Predictor Levels. Partner condition had levels {dyadic context, group context}, framing condition {"we"-framing, "you"-framing}, age {2-year-olds, 3-year-olds}, sex {male, female}, partner order {dyadic context first, group context first}, game order {Bucket Game first, Box Game first}, trial number {1, 2}, puppet {cat, bunny}, and game {Bucket Game, Box Game}. E1 had 2 and E2 had 6 levels. E2 was collapsed to a four-level predictor in the analyses reported, above, so that E2 A tested 38 participants, E2 B (the collapsed E2 level) 22 participants, E2 C 16 participants, and E2 D 18 participants. Individuals who were E1 were never E2 and vice versa. One E1 tested 78 participants (35 "we"-framing, 43 "you"-framing) and the other 18 participants (13 "we"-framing, 5 "you"-framing). E2s tested between two to 38 participants.

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