

Social Psychology

# Eliciting Short-Term Closeness in Couple Relationships With Ecological Momentary Interventions

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Relationship closeness is considered an important psychological variable for studying couple relationships, and is often postulated as cause for important relationship outcomes. The current study evaluates four micro-interventions for their suitability to experimentally elicit feelings of closeness towards one's partner. Using participants' smartphones, and a combination of experience sampling, event sampling, and ecological momentary interventions, individuals reported for a week on their experiences of closeness before and after completing daily either a neutral task or a task meant to enhance relationship closeness. The closeness tasks included showing physical affection, sharing a childhood memory, looking each other in the eyes for five minutes, and discussing shared life achievements. Results of intention-to-treat analyses on a within-person level showed that closeness increased from pre- to post-measurement on average more strongly on days of any of the four examined closeness conditions than on days of the neutral control conditions. Interindividual variability of this effect was observed, emphasizing the relevance of using within-person designs to evaluate such interventions. Exploratory analyses showed that effect sizes declined across time within the day. This study provides instruments for research on causal effects of closeness in everyday relationship life, and an evidence basis for smartphone-delivered interventions in practitioner settings.

It is a long-established fact that relationship closeness and intimacy are important aspects of high-quality relationships (Berscheid et al., 1989; Clark & Reis, 1988; Hassebrauck & Fehr, 2002; Mashek & Aron, 2004; Reis & Shaver, 1988).<sup>1</sup> The experience of closeness to one's partner is for example predictive of relationship commitment (Frost & Forrester, 2013), relationship or marital satisfaction (Frost & Forrester, 2013; Greeff & Malherbe, 2001; Hassebrauck & Fehr, 2002; Laurenceau et al., 2005; Pascoal et al., 2015; Tzapelas et al., 2009) and relationship stability (Frost & Forrester, 2013; Le et al., 2010). It is further associated with individuals' self-esteem (Cramer & Donachie, 1999), sexual desire and frequency (Muise et al., 2019; Witherow et al., 2016), as well as sexual and general well-being (Ditzen et al., 2008; Pietras & Briken, 2020). As long as it does not exceed one's desired level (Frost & Forrester, 2013; Frost &

LeBlanc, 2021), closeness seems at first glance to be worthy to pursue. This presumes though that the associations with desirable outcomes are based on a causal effect of closeness, rather than closeness being a consequence of them (see e.g., Kok et al., 2013; Waugh & Fredrickson, 2006 for takes on positive emotions leading to closeness). Research on the consequences of closeness requires interventions that are suitable to alter the experience of closeness. The development of such interventions for implementation in natural settings is the main goal of our study.

Many previous experimental studies looked at means to intervene in relationships and dyadic interactions to alter relationship processes and experiences in general (see Kanter & Schramm, 2018 for a review of brief micro-interventions), for example by promoting relationship excitement (e.g., Coulter & Malouff, 2013), humor (e.g., Treger

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<sup>1</sup> Although the term intimacy is sometimes considered as more narrow than closeness (e.g., Parks & Floyd, 1996), we use the terms synonymously and review literature for both concepts as there is considerable overlap between definitions of intimacy and definitions of closeness (Laurenceau et al., 1998; Mashek & Aron, 2004; Moss & Schwebel, 1993; Parks & Floyd, 1996).

et al., 2013), gratitude (e.g., Algoe & Zhaoyang, 2016), self-disclosure (see Collins & Miller, 1994 for a review; see e.g., Sprecher & Treger, 2015; Troy & Lewis-Smith, 2006 for more recent research), physical touch (e.g., Jakubiak & Feeney, 2016), sexual activities (e.g., Loewenstein et al., 2015; Rosa et al., 2019) and shared activities more generally (e.g., Aron et al., 2000; Flora & Segrin, 1998; Lyons et al., 2016; Reissman et al., 1993), but also by teaching psychological skills such as mindfulness or reappraisal strategies (e.g., Carson et al., 2004; Finkel et al., 2013; Houssais et al., 2013; Marigold et al., 2007, 2010; Marigold & Anderson, 2016; Rogge et al., 2013). These studies were often coupled with the question of whether these interventions improve relationship quality in one way or another. Many studies already tackled the question of which interventions are apt to increase closeness as a preliminary step before examining whether these interventions may then affect relationship quality, mostly applying different types of self-disclosure, memory sharing or humor (Alea & Bluck, 2007; Aron et al., 1997; Beike et al., 2016; Brandon et al., 2017; Fraley & Aron, 2004; Guan, 2018; Kok & Singer, 2017; Lee et al., 2019; Lin & Utz, 2017; Pinel et al., 2006; Sedikides et al., 1999; Sprecher, 2014, 2021; Sprecher, Treger, & Wondra, 2013; Sprecher, Treger, Wondra, et al., 2013; Treger et al., 2013; van Bel et al., 2009), but also through other means like physical or sexual affection, meditation, synchronous stimulation, empathy/perspective taking, gazing behavior, or gift giving (Aknin & Human, 2015; Batson et al., 1997; Carson et al., 2004; Davis et al., 1996; Durbin et al., 2021; Galinsky & Moskowitz, 2000; Paladino et al., 2010; Prause et al., 2021; Tsai et al., 2020; Zhou et al., 2018).

Yet, out of these studies, only one study was conducted completely in the field (Durbin et al., 2021), that is by intervening directly in individuals' lives, compared to interventions conducted (mostly) in the laboratory, in artificial settings, or those that prompt the recollection of experiences (some studies at least assigned exercises to be implemented at home after/along with training sessions: Carson et al., 2004; Kok & Singer, 2017; Tsai et al., 2020). Further, only a few studies (also) used a within-person design, in which the same individuals are exposed to different conditions (Guan, 2018; Kok & Singer, 2017; Prause et al., 2021; Zhou et al., 2018) compared to a between-person or -couple design, in which different groups of individuals or couples are exposed to each condition. These studies, however, either lacked an intraindividual control condition (i.e., a condition in which the same individuals who complete the closeness intervention also complete another task that does not involve a potential elicitation of closeness), and/or did not control for baseline levels of closeness. This imposes two limitations: First, studies conducted in the laboratory may limit ecological validity and thus generalizability to individuals' real lives, as well as transferability to tasks that can be assigned to clients in therapeutic settings. Second, studies conducted in between-person designs do not mirror exactly what happens at the individual level, that is, when a single individual changes their behavior compared to their usual behavior. They can only inform about average individual effects, but not about the

variability of these individual causal effects (Rohrer & Murayama, 2021). Moreover, within-person designs are sometimes better suited to examine the causal effect and its unfolding over time of sometimes rather short-lived everyday behaviors (Rohrer & Murayama, 2021; Schmiedek & Neubauer, 2020): On the one hand, time can be more adequately represented as an influencing variable on the effect in (especially repeated) within-person assessments. On the other hand, the flexible alternation between intervention and control condition within individuals may counteract habituation, maturation and other period effects that may arise when individuals are constantly in either the intervention or the control group.

The current study aims to overcome these limitations and combines the advantages of investigating interventions that are carried out in the field by applying so-called Ecological Momentary Interventions via smartphones (Heron & Smyth, 2010) with the advantages of using a within-person design with pre- and post-measures of closeness, including an intraindividual control condition. Using these methodological approaches, the overarching goal of the study is to investigate the effectiveness of encouraging interventions aimed at increasing intraindividually experienced closeness in the field. Apart from potential applications in practitioner settings, such interventions can additionally be used to scrutinize theories that postulate mechanisms concerning closeness and intimacy as causal factors on the process level (e.g., the intimacy process model by Laurenceau et al., 1998; the Dynamics of Motive Satisfaction model by Zygar et al., 2018).

### Closeness as Inclusion of Other in the Self

There are a multitude of coexisting conceptions about the construct of closeness (see Ben-Ari & Lavee, 2007 for a review). It has for example been defined as a 'high interdependence between two people's activities' (Berscheid et al., 1989, p. 792), as cognitive and affective interdependence through interconnected selves (Aron et al., 1992), or more generally as a state of positive or negative relatedness that can be given and received (Birtchnell et al., 2016). As such, closeness can be differentiated along several dimensions, for instance whether it is experienced cognitively, behaviorally, physically or emotionally, or whether it pertains to a state or a trait (Ben-Ari & Lavee, 2007; see also Dibble et al., 2012).

For our study we were interested in a measure of closeness that covers the construct rather broadly, while being suitable for repeated measurements across the day by having a short answering time and reflecting the assessment of a state. The Inclusion of Other in the Self Scale (IOS, Aron et al., 1992) fitted these requirements perfectly: This measure was originally derived from the self-expansion model by Aron and Aron (1986), which postulates that 'in a close relationship each person includes in the self, to some extent, the other's resources, perspectives and identities' (Aron et al., 2004, p. 27). It is often predominantly discussed as a cognitive measure of closeness (e.g., Agnew et al., 2004), whereby a cognitive reorganization is the assumed process that leads to a perceived self-other overlap

and feelings of interconnectedness (Aron et al., 1992, 2004). However, it also shows to cover feelings of emotional closeness, supposedly through perceived mutual affective dependence (in fact, it is often noted that the IOS measure was developed to overcome shortcomings of other measures in the affective dimension of closeness, Aron et al., 1992). The evidence that the IOS assessment taps also into the level of behavioral closeness in terms of interdependence in activities is mixed (Aron et al., 1992; Aron & Fraley, 1999). In sum, the IOS scale can be viewed as a broad measure of subjectively experienced closeness in a relationship (see also Gächter et al., 2015 for making this point).

Further, the IOS scale is a single-item measure: Participants are presented with a series of seven pictorials depicting gradually overlapping circles, for which they should indicate which best represents their relationship with another person. As such, it is not only highly efficient in terms of answering time, but also suited as a state-measure when asking participants to indicate what *currently* represents their relationship with someone else best. Although there seems to be some stability in the IOS assessment of a relationship across two weeks (Aron et al., 1992) - which is to be expected - the IOS scale has been successfully implemented and adapted as a state measure in various research (see e.g., Pietras & Briken, 2020 for a review of studies about couples' sexuality using the IOS as a state measure; or Pusch et al., 2022 for an application in research on couple's motivations). For our research, it was especially relevant that individuals also attend to the emotional closeness experienced towards their partner when answering the IOS measure, and not confuse it for example with an assessment of physical closeness, hence we altered the instructions of the original IOS scale to particularly highlight this aspect (see Materials section for details).

### Development of Suitable Ecological Momentary Interventions (EMIs)

The development of interventions that can be delivered via smartphones and carried out in individuals' everyday lives poses unique challenges (Steinhart et al., 2019): One key element is that they have to be sufficiently simple to understand and implement for the target population, so that no additional guidance by the study investigators is needed beyond what is presented on the smartphone, and high compliance to the procedure can be expected. Hence, the interventions must be quite compact, and should not require special resources or relationship circumstances to be executed.<sup>2</sup> As our study targeted individuals in committed relationships (but not their partners), it was further necessary that the instructions sent to only one partner of the couple was sufficient for a successful implementation of the intervention. Further, in terms of time and effort, the interventions had to be reasonably easy to implement while

individuals go about their daily lives. A last requirement concerned the theoretically supposed time-span of the intervention effect: As we expect relationship closeness as a state to naturally vary in individuals' everyday lives, our (micro-)interventions had to aim at a short-term impact on the level of experienced closeness rather than a prolonged intervention effect that unfolds its impact through slow and more persistent changes in closeness towards the partner.

We approached the search for interventions suitable for these main criteria in two ways: On the one hand, we screened the experimental closeness manipulations reported in the literature in laboratory settings for their suitability for usage in our field study. On the other hand, we conducted an interview with an expert working in couple counseling, exploring the methods that are being used in practitioner settings to enhance closeness between partners.<sup>3</sup> This resulted in a total of four interventions (two derived from the literature, and two derived from the interview) that were tested for their usefulness to induce short-term closeness on an intraindividual level. The reasoning for the selection of these interventions will be shortly described in the following (for a more thorough explanation, see Cristoforo & Zygar-Hoffmann, 2021; Wolf & Zygar-Hoffmann, 2021). Their concrete instruction is described later in the Methods.

First, we decided on a '*physical affection intervention*' based on the successful implementation of such an intervention in an EMI between-person design by Durbin et al. (2021). Physical affection can be easily instructed, is feasible to implement, and can be tailored to individuals' unique relationship behavior. Plus, showing pronounced affection can be expected to have a short-term impact on closeness, as it is something that participants mention and naturally implement themselves when they are asked about ways to increase closeness to their partner (Girme et al., 2014).

Second, we chose a '*memory sharing intervention*' in which individuals bidirectionally share personal childhood memories with their partner. This was based on the considerable amount of research on the positive effect of self-disclosure on experienced closeness in various relationship types (see Sprecher, 2021 for an overview), with memory sharing being a specific type of self-disclosure suitable for close relationships (Beike et al., 2016; Guan, 2018). In an (unpublished) pilot study we conducted before the current study, this intervention also proved to be applicable in an EMI design and in exploratory analyses effective to increase closeness on an intraindividual level (see Cristoforo, 2021 for details).

Third, we implemented an '*eye contact intervention*' that instructed participants to look at each other for five minutes without speaking. This was suggested by the interviewed practitioner because in their experience it leads to feeling acknowledged and literally being 'seen' by the part-

2 Since our study was conducted during the COVID-19 pandemic, it further had to be considered that typical relationship circumstances might have been obstructed, for instance that certain activities might not have been as possible as under normal circumstances.

3 The interview was conducted in German, for a (partly summarized) transcription see Wolf and Zygar-Hoffmann (2021).

ner, without much explanation needed about the purpose and procedure. This fits to research showing gazing behavior as a feature of close relationships and intimacy (see Cui et al., 2019 for recent research; Andersen et al., 2006; Kleinke, 1986 for reviews), and direct gaze presentations as a way to generate closeness with a photographed stranger (Zhou et al., 2018). Further, exactly such an eye contact intervention was also part of a larger intervention session that successfully showed to enhance feelings of closeness a week later (Tsai et al., 2020). The interviewed practitioner also mentioned that the exercise works well for couples in diverse circumstances, and is therefore assumed to be applicable quite generally. Together, this fitted well with our criteria for a viable realization in the study design, and with our perspective on closeness reflecting a feeling of interconnectedness.

Fourth, we designed a slightly more complex ‘*life achievement intervention*’ that prompted the couple to discuss achievements they successfully mastered as a team. Although this intervention takes more time than the other selected ones, the interviewed practitioner argued it would be a promising approach to induce closeness, and guidance of the conversation by some questions could help with a successful and feasible implementation. In the counseling context this exercise is part of a more thorough examination of each other’s strengths and the mutual resources the couple can rely on, and is rooted in systemic family and couple therapy (Nemetschek, 2006; see Snyder & Balderama-Durbin, 2012 for a brief introduction to the concept of resource activation). We had some concerns about the applicability of this intervention for various relationship types in terms of the need to have already mastered something together to be able to discuss it. Nevertheless, we decided to give this intervention a try, as we envisioned our study to provide some evidence basis also for applied settings, and for testing out the limits of the applied smartphone-based intervention strategy.

As control conditions, we developed three parallel ‘*neutral interventions*’ that instructed participants to share their preferences for varying media (such as a song they like) with an acquaintance. As such, these interventions changed two aspects compared to the closeness interventions (the task itself and target of the task) and are intended as active control conditions, aimed at eliciting self-disclosure to another person. However, since the target of the interventions is not the relationship partner but someone else, they can be considered as a rather weak version of an active control condition that might (only) catch if the interventions elicit a rather generalized but not target-specific enhanced closeness experience. We changed both the target and the task in the neutral interventions to avoid a) inadvertently eliciting closeness to the relationship partner by involving them, thereby making them less neutral, b) altering the potential effect of doing a task for the first time compared to already being used to it, and c) reducing compliance due to repetitiveness of the tasks. A similar variant of these neutral interventions produced no change in closeness to the relationship partner in our aforementioned pilot study (Cristoforo, 2021).

## Current Study

In the current study, we investigated whether encouraging partnered individuals one day in their everyday lives to implement a certain intervention intended to enhance relationship closeness would indeed elicit a change in closeness from before to after the intervention, compared to neutral interventions not expected to increase closeness. Hence, we are interested in the ‘intention-to-treat’ effect, which represents the causal effect of the encouragement to implement an intervention irrespective of the actual compliance (McCoy, 2017). It must be differentiated from the treatment effect, which estimates the causal effect of the intervention itself. For the repeated assessment of closeness and other psychological and situational variables we used the Experience Sampling Method (ESM, Csikszentmihalyi & Larson, 1987) with time- and event-contingent sampling. The hypotheses were preregistered at <https://osf.io/ge4sd/> (for the *physical affection intervention* and the *memory sharing intervention*) and <https://osf.io/5czf7> (for the *eye contact intervention* and the *life achievement intervention*; see label “I3” in both preregistrations). Generally, the hypotheses are constructed in the same fashion, in short form:

‘The post-pre difference (‘change score’) in closeness is more positive on the day of a closeness condition than on days of a neutral condition.’

Formulated separately for each intervention, and with some detail on the calculation of the differences in closeness, this translates to:

The difference in closeness from morning measurement to the next measurement (after the intervention) is more positive on the day of the encouragement to ...

H1 (*physical affection intervention*): ... show physical affection to one’s partner that goes beyond the physical affection shown to the partner in everyday life ...

H2 (*memory sharing intervention*): ... share one of one’s best childhood memories with one’s partner and subsequently ask the partner to share one of their best childhood memories ...

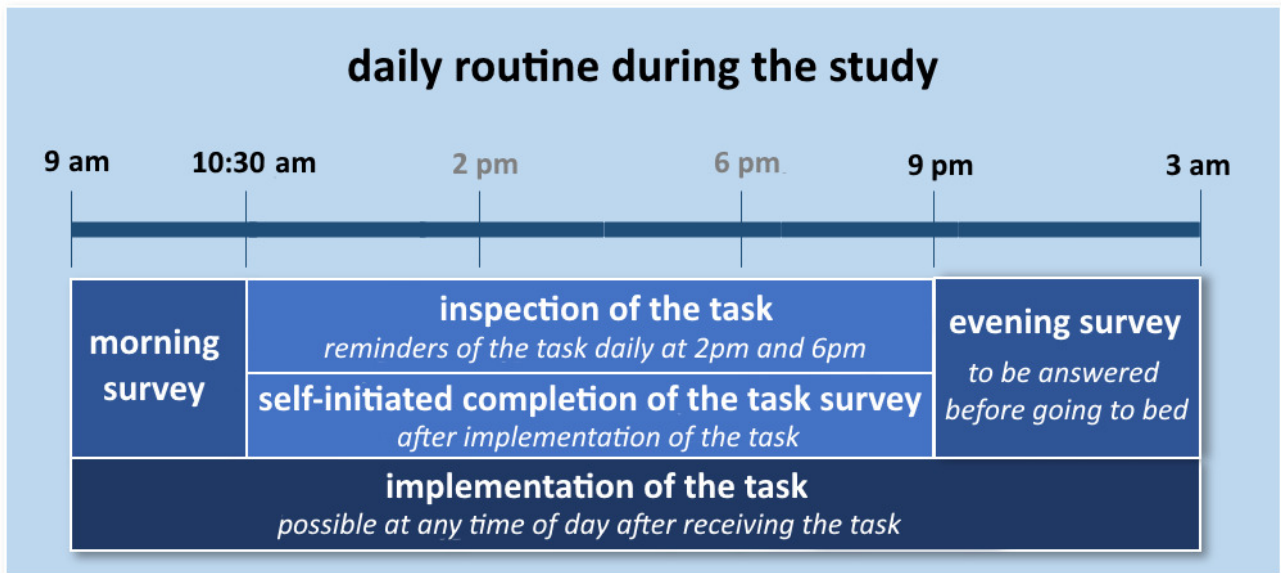
H3 (*eye contact intervention*): ... look into the partner’s eyes for five minutes ...

H4 (*life achievement intervention*): ... discuss shared life achievements ...

... than the average difference in closeness from morning measurement to the next measurement on days of a neutral condition.

## Methods

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study (Simmons et al., 2012). Anonymized data, a codebook, reproducible analysis scripts, as well as further Supplementary Materials (e.g., additional analyses, the exact sequence of interventions defined in the stratification variants, or the timing of reminders) can be found at <https://osf.io/agchm/> (Zygar-Hoffmann et al., 2022). We used R (version 4.1.1, R Core Team, 2021) with the packages *dplyr* and *tidyr* for data handling (Wickham, 2021; Wickham et al., 2021), the



**Figure 1. Illustration of the study schedule (for those who chose 9 am for the morning survey).**

Note: For morning surveys starting at 8 am or 10 am, only the first two times shown in the Figure differed. Figure available at <https://osf.io/agchm/>, under a CC-BY4.0 license.

package *papaja* (Aust & Barth, 2020) for manuscript writing and the packages *ggplot2* (Wickham, 2016) and *ggstatsplot* (Patil, 2021) for figure creation. The study was approved by the local ethics committee.

### Procedure

Individuals were recruited to participate in a smartphone study on closeness in couple relationships via social media, a university newsletter, and by asking couple therapists and consulting centers in Germany to promote the study among their clients. To take part in the study individuals had to be 18 years or older, be in a monogamous relationship (however, only one partner of a couple was eligible for participation), be fluent in German, possess an iPhone or Android smartphone, and physically meet their partner at least three times per week. They had to give informed consent when registering with their email address on the study's website (which gave an overview of the study's procedure). They were then sent an email with a short overview of the study (Figure 1) and instructions on how to download, install and log into the experience sampling application *Tellmi* (developed at LMU Munich) with a personalized code. From then on, the study was conducted via the application.

After login participants received further details about the study procedure and were instructed to complete a pre-ESM questionnaire on their demographic and relationship characteristics, their global relationship satisfaction and (unrelated to the current analyses) their explicit affiliation

motive. Also, to assess baseline levels of relevant variables before any intervention, at 9 pm that day a first evening survey started, which participants should complete right before going to bed. On the day after login, the seven-day experience sampling part of the study started. On four of the experience sampling days participants were asked to implement an intervention involving their partner (closeness conditions), whereas on the other three days participants were given a neutral task to implement with an acquaintance (neutral conditions).<sup>4</sup> On the last study day subjects were asked to answer a short post-ESM questionnaire for feedback on the study.

Every day consisted of a time-contingent experience sampling morning survey (during registration participants could indicate whether this survey should start at 8 am, 9 am or 10 am; this time was the same for all study days), a task that participants were asked to implement at any time during the day (the intervention), an event-contingent event sampling survey called *task report* that participants were asked to complete immediately after task implementation, and a time-contingent experience sampling evening survey at 9 pm (see Figure 1).

In all three surveys relationship closeness was measured. In the morning and evening survey, state relationship satisfaction and mood were additionally assessed. The evening survey further consisted of a few additional questions about the time spent with the partner on that day and about the participant's evaluation of the intervention. The task report included questions on the concrete implementation of the

<sup>4</sup> Participants were randomly assigned to one of four semi-randomly generated stratification variants, that followed a certain structure: The sequence of interventions differed across variants to avoid positioning effects, with closeness and neutral interventions alternating. Every stratification variant ensured that one neutral intervention and one closeness intervention was scheduled on the weekend. Across all variants, each intervention occurred at least once on the weekend.



task and on the responsiveness of the interaction partner during the task.

The morning survey could be completed within 1.5 hours before it timed out. Participants received the task instructions for the intervention of the respective day at the end of the morning survey and were asked to implement the task throughout the day. The task instructions could be reviewed again during the day to avoid incorrect task implementation due to subjects forgetting the concrete instructions. Participants were asked to start the task report themselves directly after task implementation. It could be initiated right after the completion of the morning survey and until 9 pm. At 9 pm the evening survey started and could be completed throughout the rest of the evening (until 3 am of the next day). However, participants were instructed to fill out the evening survey right before going to bed (and after task implementation, if they had not completed the task by 9 pm). Participants could therefore also implement the task after 9 pm, although the self-started task report was then omitted. The questions in the evening survey differed depending on whether the participants indicated that they had already completed a task report. Participants were notified by their smartphones at regular intervals of an active morning or evening survey, and got two reminders of the implementation of the task throughout the day (see again [Figure 1](#)).

To be eligible for study compensation, participants had to complete the pre-ESM and post-ESM questionnaires and reach a minimum completion rate of 50% of the ESM surveys. They could then choose between obtaining course credit or taking part in a raffle of 25 vouchers valued 20€ each. From a completion rate of at least 75%, participants got higher course credit, or their chance of winning a voucher was doubled.

## Sample

The sample size was determined by time constraints as the data collection was part of two final theses with a pre-specified time table. Data collection started on May 14<sup>th</sup> 2021 and we preregistered to include all data by participants who would log into the app until the first of June 2021. Additionally, we calculated a power analysis for a

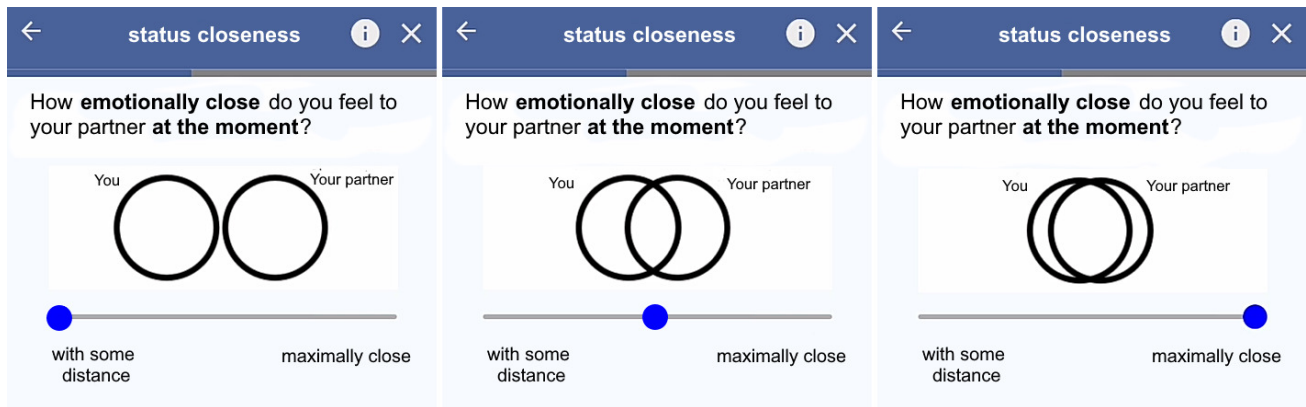
one-sided *t*-test for paired samples with the *pwr* package (Champely, 2020) to tailor our recruitment effort to what an ideal sample size would look like for our study. To estimate an assumed effect size, we drew on the results from our aforementioned pilot study (Cristoforo, 2021), where we observed in exploratory analyses within-person effect sizes of  $d_z = 0.21$  and  $0.33$  for two interventions meant to enhance closeness.<sup>5</sup> Using  $d_z = 0.27$  as the average of both effect sizes as input for a power analysis with  $\alpha = .05$  and  $1 - \beta = .80$  results in a necessary sample size of 87 participants, which was the minimum we hoped to achieve. Taking more conservatively the smaller observed sample size of  $d_z = 0.21$  instead results in a required sample size of 142 participants for final inclusion in the analyses.

While the pre-ESM questionnaire was completed by 169 participants, some individuals had insufficient experience sampling data for the necessary outcome variable to be computed, hence the final analyses included total subsample sizes of 106 (for the *eye contact intervention* and *life achievement intervention*), 108 (for the *memory sharing intervention*) and 109 (for the *physical affection intervention*).<sup>6</sup> Even though eight individuals from the final subsamples self-reported in response to a data quality question that their data should not be used for analyses, as preregistered, these individuals were not excluded, as they did not specify in a followup question that they deliberately answered study items incorrectly or that their answers misrepresent their experiences (instead most of them indicated no reason, or that they have missed too many reports). Hence, for the minimal achieved subsample size of 106, the resulting statistical power was 87 % for an expected  $d_z = 0.27$ , but only 69 % for an expected  $d_z = 0.21$ . The post-ESM questionnaire was completed by 125 of the subsample participants.

The participants who belonged to at least one of the four final subsamples were predominantly female (83%) in a different-sex relationship (98%), currently enrolled as students (70%), and living in a shared a household with their partner (63%). The mean age was 27.43 years ( $SD = 8.34$ , Range = 18-54) and the average relationship duration was 5.65 years ( $SD = 7.05$ , Range = 2 months - 37 years). Only 19% of the participants were married, and only 15% had children. Global relationship satisfaction was average ( $M =$

<sup>5</sup> The effect size  $d_z$  was calculated with the *powerAnalysis* package (Fan, 2017), see Lakens (2013) for details of this effect size measure. We disregarded the effect sizes of  $d_z = 0.08$  and  $-0.03$  for two other interventions examined in the pilot study as being not large enough to be interesting. They translate to a difference of less than 0.10 points on the raw IOS scale, whereas the effects considered in the power analysis translate to a difference of at least 0.37 points. A priori, we did not systematically decide on a smallest effect size of interest (Baguley, 2009), but post hoc we would argue that it might even be smaller than a difference of 0.37 IOS points (see discussion). However, given that we based our effect size expectations on the other results of the pilot study, our study would be heavily underpowered to reliably detect such smaller effects (see achieved sample size in the next section). Further, we want to note that the two effect sizes considered for our power analysis are about one-quarter as large as the effect sizes commonly reported in the literature for the effect of various interventions on relationship satisfaction (see <https://osf.io/9pdxs/> for a small literature search of the pilot study, where an average  $d$  of about 0.5 was identified, which corresponds to a  $d_z$  of about 1.1 given a correlation between pre- and post-measurement of  $r = 0.9$ ). This difference in effect size strength could be explained by the more controlled nature of commonly used interventions compared to our only encouraged micro interventions, the different outcome variable, and by publication bias. Still, our approach to estimate the effect size from a pilot study is also not optimal (see Albers & Lakens, 2018).

<sup>6</sup> Neither age nor relationship duration or relationship satisfaction differed significantly between those included in at least one of the final subsamples and those who provided too few ESM surveys ( $ps \geq .269$ ).



**Figure 2.** Illustration of the measurement of closeness on participants' smartphones using an adapted version of the Inclusion of Other in the Self Scale (Aron et al., 1992).

Note: The two endpoints and the midpoint of the scale are presented. Figure available at <https://osf.io/agchm/>, under a CC-BY4.0 license.

63.47,  $SD = 13.75$ , Range = 16-81, 18% could be classified as unhappy according to the cutoff proposed by Funk & Rogge, 2007).

## Materials

All measures were administered in German. For details on the measures not used in the analyses for the current paper, please see the codebook.

### Global relationship satisfaction assessed in the pre-ESM questionnaire

Participants' overall, global evaluation of their relationship was assessed by summing 16 items of the Couples Satisfaction Index (CSI, Funk & Rogge, 2007). The CSI includes questions such as 'In general, how often do you think that things between you and your partner are going well?' and adjectives describing a relationship that are to be rated on 6- and 7-point Likert scales (McDonald's  $\omega = 0.97$ ).

### Experience Sampling and Event Sampling

**State Relationship Closeness.** Closeness to the partner was measured using an adapted version of the Inclusion of Other in the Self (IOS) Scale (Aron et al., 1992). The IOS scale is a single-item pictorial measure that displays seven pictures with two increasingly overlapping circles (we kept the circles of a constant size, like the adaptation by e.g., Mashek et al., 2007). Instead of the original item instruction of the IOS, which asks about which pair of circles best describes the participant's relationship, the question 'How emotionally close do you feel to your partner at the moment?' was used. Answers were given on a picture slider ranging from *with some distance* (=1) to *maximally close* (=7), see Figure 2. Even though we additionally included another operationalization of closeness in the study (which was based on the Subjective Closeness Index trait measure by Berscheid et al., 1989, see codebook), the IOS scale was our preregistered state closeness measure for all analyses.

**State Relationship Satisfaction.** For exploratory analyses, state relationship satisfaction was measured using the

three following items introduced in Schönbrodt et al. (2021), all with answers on a continuous slider: 'How do you feel about your relationship at the moment?' (0=*bad* to 10=*exceptionally good*), 'How are you feeling at the moment in your relationship?' (0=*totally frustrated* to 10=*totally satisfied*), and 'How annoyed are you about your partner at the moment?' (reverse-coded, 0=*not at all* to 10=*strongly*). The within-person, between-moment reliability of this scale was better than in previous research ( $R_{WPM} = 0.71$ ), probably because no variance on the dyadic level could be considered (Schönbrodt et al., 2021).

**State Responsiveness.** Based on a scale used by Boker and Laurenceau (2006), responsiveness of the other person during the intervention (e.g., of the partner in the closeness interventions, and of the acquaintance in the neutral interventions) was assessed with four separate questions about how 1) understood, 2) validated, 3) accepted and 4) cared for participants felt by this person during task implementation (0=*very little* to 10=*a great deal*). Answers on the four items were averaged, and between-moment reliability was good ( $R_{WPM} = 0.85$ ).

### Interventions

All but the physical affection intervention contained the possibility to implement the task either in person or in some technically mediated way (for example via video chat). In these cases, if participants indicated having completed the task by the end of the day, they were asked in the evening survey in which way they implemented it.

Further, for all interventions, participants were asked to report in the evening survey about the intrusiveness to everyday life ('How easy or difficult did you find it to incorporate the task into your daily life today?') and the difficulty of implementation ('How easy or difficult was it for you to implement the task?') on Likert scales ranging from 1=*very easy* to 5=*very difficult*, as well as on the idiosyncratically perceived oddity of the task ('How unusual for your natural behavior and your own personality was the task for you?') on a Likert scale ranging from 1=*not unusual* to 4=*extremely unusual*.

**Physical Affection Intervention.** The instruction for the physical affection intervention read: *'Please show physical affection to your partner today in any way you like. Think of something special that goes beyond the casual physical affection in your everyday life.'* In a tooltip (an information box where participants could get additional information on the respective instruction) participants were given examples of physical affection, so that they could decide themselves which type of physical affection they feel comfortable with, which we hoped would make the task feel as natural as possible. At the same time, to represent an actual intervention to usual everyday life, they were told that the chosen affection should be more intense or longer than usual, or that they should otherwise choose a type of affection they do not normally show.

On the day of this intervention participants completed additional questions in the evening survey about the type of physical affection shown, given that they previously indicated having implemented the task. Based on categories identified by Gullede et al. (2003), they could choose between the following options (multiple options could be selected): *giving a massage, caressing, cuddling, holding hands, hugging, kisses on the lips, kisses on the face/ body, as well as sex or other* (which included a follow-up query for specification). They were further asked to indicate on a continuous slider with answers ranging from *not at all* (=0) to *very strongly* (=10), how much the physical affection shown went beyond usual levels. Lastly, participants rated how their partner reacted to the affection on a continuous slider with answers ranging from *my partner refused it* (=0) over *neutral* (=5) to *my partner returned it* (=10).

**Memory Sharing Intervention.** The instruction for the memory sharing intervention read: *'Tell your partner today in a personal conversation, on the phone or via video chat about one of your best childhood memories that your partner probably does not know about yet. Afterwards, ask them about theirs.'* As such, participants were encouraged to share new self-related experiences and information, but had the chance to select between several good memories. Childhood memories were chosen instead of individuals' memories in general to avoid that participants talk about shared memories with their partner, which might induce a confounding effect by possibly not only manipulating closeness, but also the motivation to experience the shared memory again. A tooltip clarified that participants could alternatively use text messaging, in case they cannot see or talk to their partner on that day.

**Eye Contact Intervention.** The instruction for the eye contact intervention read: *'Take some time together with your partner today in a quiet moment. Set an alarm for 5 minutes and look your partner in the eye for 5 minutes. Alternatively, you can video chat. While doing this, focus completely on your partner. Ask your partner to try this with you. Five minutes may seem a bit long, but try to both challenge yourself and focus fully on the other person for those 5 minutes. Any reaction that occurs during the task is fine and belongs. Blinking and breathing are allowed and encouraged ;-)'* The specifics of the instruction, such as the validation of various

reactions, were derived from the interview with the couple counselor.

**Life Achievement Intervention.** The instruction for the life achievement intervention read: *'Couple psychologist practitioners have observed that every couple faces various challenges in the course of their relationship. The things that went wrong are often particularly remembered. Today's task, therefore, is to focus on the positive things. So today, at a quiet time of the day, sit down with your partner for about 20 minutes. Alternatively, you can talk on the phone or video chat. Then answer the following questions: What first thing that comes to mind when you think about what you have already mastered together with your partner? The following questions relate to this challenge. Now answer each question in turn. That way, each person can say what they feel. - How did you accomplish this together? - What characteristic/ gesture/ support of your partner has been especially helpful for you? - Why did you master this together?'* Again, the specifics of the instruction, such as the exact type of questions to be discussed, were derived from the interview with the couple counselor.

**Neutral Interventions.** The instruction for the neutral interventions read: *'Get in touch with a good friend today (e.g. by phone, text message or also in person). Share with them two [media] that you particularly like. Tell them why you particularly like those.'* The [media] placeholder was concretized with 'movies/series', 'songs/podcasts/radio show' or 'books/(online) magazines/blog entries/comics' to achieve three parallel versions. The tooltip reminded participants that the intervention should not include their partner. They were further informed that they could also get in touch with a person from their family (other than their partner) and that they were free to decide whether to tell the involved person that the task is part of a study.

## Data Analysis

### Additions to the preregistration

Although this was not envisioned in the preregistration, we excluded data from 12 answered task reports in a first step, which had been completed right before the start of the morning survey. Since task instructions were only given at the end of the morning survey, they could not be considered as valid task reports and it was not reasonable to take them as post-measures of closeness. Similarly, we excluded closeness answers from six task reports, in which individuals indicated that they have not yet implemented the intervention, as task reports were instructed to be completed after task implementation.

### Preregistered approach

Separately for each hypothesis, a one-sided paired t-test was calculated to compare days with the respective closeness intervention of that hypothesis (closeness condition) to days of neutral interventions (neutral condition) regarding the difference in closeness before and after the encouragement to implement the intervention (outcome variable). To calculate the outcome variable, for each day of each par-



ticipant, the IOS measure of the morning survey (pre-measure of closeness) was subtracted from the closest IOS measure collected after the encouragement to implement the daily task (post-measure of closeness), which was either the IOS measure from a task report or from the evening survey. This difference score (or change score) could range from -6 to 6. If the difference could be calculated on more than one neutral day, the mean difference across all neutral days was used as outcome for the neutral condition. Consequently, only those participants could be included for the analysis of the respective hypothesis, for whom a post-pre difference in closeness could be computed for the day on which the intervention of interest was instructed, and for whom such a difference could be computed for at least one day with a neutral intervention.<sup>7</sup>

### Additional analyses

For our hypotheses and some exploratory analyses, we additionally calculated Bayes Factors (computed with the *BayesFactor* package, Morey & Rouder, 2021), given default Cauchy priors with a scale parameter of 0.27 (for analyses regarding the effect of the intervention on different outcomes, which implies that the median effect size under H1 is a Cohen's  $d_z$  of 0.27, consistent with our sensitivity power analysis) or a scale parameter of 0.4 (for analyses concerning the timing of the post assessment, which corresponds to the average published effect size in psychology, Bosco et al., 2015; Richard et al., 2003). In case of our hypotheses, these were directional Bayes Factors, which compare the probability of the difference between conditions being greater than zero with the null hypotheses that the difference is zero (point null). In case of the exploratory analyses, these were two-sided Bayes Factors that compare the alternative that there is a difference between conditions or groups (greater or lower than zero) with a point null.

Other exploratory analyses included Pearson correlations of the differences in change scores between conditions and responsiveness of the interaction partner or global relationship satisfaction.

## Results

### Descriptive Statistics

For each day with a pre-measurement of closeness, it was checked in a first step whether any post-measurement of closeness (from a task report or an evening survey) was available, which was the case in 90.2% to 97.4% of all days. In a second step, for the subset of these days that was included in the analyses (i.e., days from individuals who provided a pre- and post-report on at least one closeness and one neutral intervention day), it was analyzed how many

of the post-measurements could be taken from the task report, which was the preferred measurement (since being closest to the implementation) when both the task report and the evening survey were answered. Such sufficiently answered task reports were available in 51% of the cases for the *physical affection intervention*. This rate increases to 69% of cases for the *memory intervention*, with the rates for the other interventions falling in between (60% for the *life achievement intervention*, on average 61% for the *neutral interventions*, and 67% for the *eye intervention*).

For those who were included in the analyses, self-reported compliance regarding complete implementation of the task ranged between 81% as average for the *neutral interventions* and 90% for the *memory intervention*, with the compliance of the other interventions falling in between (85% for the *life achievement intervention* and the *physical affection intervention*, and 87% for the *eye intervention*). Another 6% (*memory intervention*) to 11% (*life achievement intervention*) indicated having at least partially fulfilled the intervention (e.g., tried it out, but a reaction from or participation by the partner was lacking). The remaining 4% to 8% indicated not having implemented the task.

[Table 1](#) illustrates how participants rated the closeness interventions in terms of their intrusiveness, implementation difficulty and oddity (see [Table 1](#) in the Supplemental Materials for ratings of the neutral conditions). The ratings were overall low across these dimensions, with the *eye intervention* scoring highest on all characteristics. For the interventions that could be completed either in person or in a technically mediated way, [Table 1](#) further shows the chosen implementation modes: Most engaged in the tasks in person.

Across interventions, partner responsiveness was high ( $M = 8.51$ ,  $SD = 1.76$ ). For the *physical affection intervention* participants indicated to have most often included cuddling (57%) and kissing on the lips (56%). Types of physical affection other than those offered as options in the question were chosen least often (8%), followed by sex as second-least often chosen option (26%). On average, the shown physical affection only moderately exceeded usual levels ( $M = 4.44$ ,  $SD = 2.67$ ). Most partners returned the affection ( $M = 9.09$ ,  $SD = 1.47$ ).

[Table 2](#) shows the descriptive statistics of the closeness and state relationship satisfaction change scores in all conditions. For all interventions, the average closeness and relationship satisfaction post-pre difference was descriptively more positive in the respective closeness condition than in the neutral conditions (see [Figure 1](#) in the Supplemental Materials for an illustration of the change scores). Inspecting the post-pre difference in the neutral conditions reveals that on average - when no intervention involving the partner was encouraged - feelings of closeness seem to descrip-

<sup>7</sup> Tables 5-8 in the Supplemental Materials present the results of an alternative approach to analyze the hypotheses with multilevel models, thereby handling missing values and omitting the necessity to a priori average over the closeness measurements of the neutral interventions, and additionally allowing to control for a linear trend of time on closeness over the study period. While the estimated effect sizes for the *memory intervention* and the *eye intervention* are smaller in these analyses, all effects remain significant when treated as directional tests.

**Table 1. Reported characteristics and implementation modes of the interventions**

Intervention	M (SD) of Intervention Characteristics			Implementation Modes			
	Intrusiveness	Difficulty	Oddity	In person	On the phone	Using video chat	Using text messages
Affection	1.80 (0.87)	1.64 (0.77)	1.26 (0.49)	100%	-	-	-
Memory	1.94 (1.00)	2.11 (1.06)	1.56 (0.65)	86%	5%	4%	4%
Eye	2.33 (1.14)	2.29 (1.04)	2.38 (0.87)	87%	-	13%	-
Life Achievement	2.28 (1.02)	2.28 (0.96)	1.82 (0.85)	87%	13%	0%	-

Note. Sample sizes for these data range from 90 to 96, because the corresponding questions were only asked in the evening survey, and not in the task report. For entries marked with a "-" symbol, the respective implementation mode was not an option.

tively increase across the day, whereas state relationship satisfaction seems to (slightly) decrease.

### Confirmatory analyses

All hypotheses could be confirmed, as shown by the test statistics in [Table 3](#), illustrated in [Figure 3](#): For each intervention, the difference in self-reported closeness from pre- to post-measurement was significantly more positive on days of the closeness conditions than on days of the neutral condition. However, there was also considerable variance in the effects, as indicated by the standard deviation of this change score difference. Specifically, the share of individuals who had a zero or negative change score difference ranged between 41% (in the *eye intervention*) to 50% (in the *physical affection* and *life achievement* intervention).

An inspection of the corresponding directional Bayes Factors supported the alternative hypotheses that the differences between conditions are greater than zero compared to being zero. However, the evidence for the alternative hypothesis varied in strength, from a rather low Bayes Factor for the *life achievement intervention* that indicates that the alternative is only about two to three times more likely than the point null to a high Bayes Factor for the *eye intervention* indicating more than 35 times more evidence of a positive difference (see again [Table 3](#)).

### Exploratory analyses

No significant effects could be found when replacing closeness with state relationship satisfaction as outcome in exploratory analyses, as shown in [Table 4](#).<sup>8</sup> The evidence in favor of the null hypothesis was supported by two-sided Bayes Factors. However, the evidence was only moderate, corresponding to the null hypothesis being about three to four times more likely than the alternative hypotheses for all interventions.

Various correlations involving the difference between conditions in change scores of closeness and state relationship satisfaction are presented in [Table 5](#): First, considerable positive correlations between the closeness and relationship satisfaction differences in change scores highlight that individuals who show an increase in one outcome variable after the intervention (compared to the neutral condition) tend to also show an increase in the other outcome variable. This association is especially pronounced for the *memory intervention* and the *eye intervention*, but also present for the other interventions. Second, associations between these differences and responsiveness of the interaction partner were small to moderate, being highest for the *eye intervention*, indicating a slightly stronger increase in the outcome variables when the partner was more responsive during the closeness condition. Responsiveness ratings between the closeness and neutral conditions were moderately to considerably positively correlated, which could be

<sup>8</sup> Similarly, no significant effects were found for valence and arousal mood measures, or the other (more trait-like) operationalization of closeness as outcomes, see Tables 2-4 in the Supplemental Materials.

indicative of selection effects (i.e., individuals choosing interaction partners for the neutral condition who are similarly responsive as their relationship partners) or individual response biases (i.e., individuals perceiving responsiveness rather similar across different interaction partners). Third, and not surprisingly, global relationship satisfaction with one's romantic relationship was considerably positively associated with how responsive individuals perceived their partner during the closeness intervention. Last, correlations of global relationship satisfaction with changes score differences were small to zero. Across interventions, the differences between conditions in change scores of closeness correlated between  $r = .20$  and  $r = .60$  for the participants (not shown in the Table), reflecting some trait-like susceptibility to the effectiveness of the interventions.

Finally, by computing independent  $t$ -tests for each intervention, we investigated how the observed effects of our interventions differ depending on whether closeness was assessed in the task report or in the evening survey on days of the closeness intervention: The results of these exploratory analyses show that the timing of assessment matters significantly for all but the *life achievement intervention*, with higher mean differences between conditions when the evaluation of closeness directly follows the implementation, as represented by the task report, compared with a possibly time-delayed evaluation, as represented by the evening

survey (mean difference of the effect between individuals whose closeness assessment in a task report could be used and those whose evening assessment had to be used:  $M_{d_{Affection}}[95\%CI] = -0.54 [-1.06, -0.02]$ ,  $t(106.99) = -2.07$ ,  $p = .041$ ,  $BF_{10} = 1.7$ ;  $M_{d_{Memory}}[95\%CI] = -0.98 [-1.65, -0.31]$ ,  $t(50.06) = -2.92$ ,  $p = .005$ ,  $BF_{10} = 18.0$ ;  $M_{d_{Evening}}[95\%CI] = -0.86 [-1.43, -0.29]$ ,  $t(81.29) = -3.03$ ,  $p = .003$ ,  $BF_{10} = 7.3$ ). Although not significant, the *life achievement intervention* descriptively also shows a sharp reduction in the size of the change score difference between task and evening assessment ( $M_d[95\%CI] = -0.44 [-1.05, 0.18]$ ,  $t(88.97) = -1.40$ ,  $p = .166$ ,  $BF_{10} = 0.7$ ). Separate descriptive statistics for both groups, including separate significance tests, are provided in Table 4 in the Supplemental Materials.<sup>9 10</sup>

## Discussion

This study evaluated four micro-interventions aimed at enhancing feelings of relationship closeness at an individual level. Using repeated sampling, ecological momentary interventions, within-person control conditions, and intention-to-treat analyses, the average causal effect of encouraging individuals to implement certain relationship behaviors on changes in self-reported experience of closeness was confirmed for all examined interventions. The interventions were developed based on the literature and an ex-

9 When considering only evening reports of individuals who indicated having implemented the task, the difference in the *affection intervention* is no longer significant, the other results do not change in terms of significance. The descriptively visible drop in effect size remains for all analyses.

10 The groups that are being compared in these independent  $t$ -tests have rather small sample sizes (e.g., only  $n = 33$  individuals with a closeness report from the evening were available for the analysis of the *memory intervention*), hence the model suffers from imprecise estimates. For that reason, our Supplemental Materials (see Tables 10-17) contain additional multilevel model analyses that contrast the differences in closeness reports between conditions at each assessment time point (pre, task, and evening measurement; within-person comparisons) and is thereby able to handle missing values in the data (whereas the analyses presented in the manuscript rely on complete sets of pre- and post-measurements). Still, we decided to report the results of the  $t$ -test in the main text, as they follow more closely the procedure and sample used for our preregistered main analyses, and thus helps to understand whether there are significant differences between these specific subsamples contained in our analyses (a between-person comparison).

Difference Variable	Intervention	n	Condition	M	SD	Min	Max	Range	Histogram	Boxplot
Post-pre closeness	Physical affection	109	closeness	0.61	1.15	-2.00	3.00	5.00		
			neutral	0.19	0.83	-2.33	2.50	4.83		
	Memory	108	closeness	0.57	1.25	-2.00	4.00	6.00		
			neutral	0.18	0.83	-3.00	2.50	5.50		
	Eye	106	closeness	0.61	1.42	-4.00	4.00	8.00		
			neutral	0.14	0.81	-3.00	2.00	5.00		
	Life achievement	106	closeness	0.53	1.43	-3.00	5.00	8.00		
			neutral	0.23	0.79	-3.00	2.50	5.50		
Post-pre RS	Physical affection	94	closeness	0.05	1.40	-3.65	5.74	9.39		
			neutral	-0.04	1.03	-3.72	2.69	6.41		
	Memory	95	closeness	-0.11	1.54	-4.68	3.56	8.24		
			neutral	-0.17	1.08	-3.72	2.29	6.01		
	Eye	90	closeness	-0.02	1.41	-4.04	5.10	9.14		
			neutral	-0.07	1.02	-3.72	2.09	5.81		
	Life achievement	92	closeness	-0.12	1.59	-5.97	3.04	9.01		
			neutral	-0.13	0.97	-3.72	2.09	5.81		

**Table 2. Descriptive statistics of the mean post-pre differences (change scores) in closeness and state relationship satisfaction by condition**

Note. RS = state relationship satisfaction, n = subsample size, Range = empirical range

pert interview, and involved pronounced physical affection, self-disclosure by memory sharing, looking each other in the eyes for a prolonged time, and discussing shared life achievements.

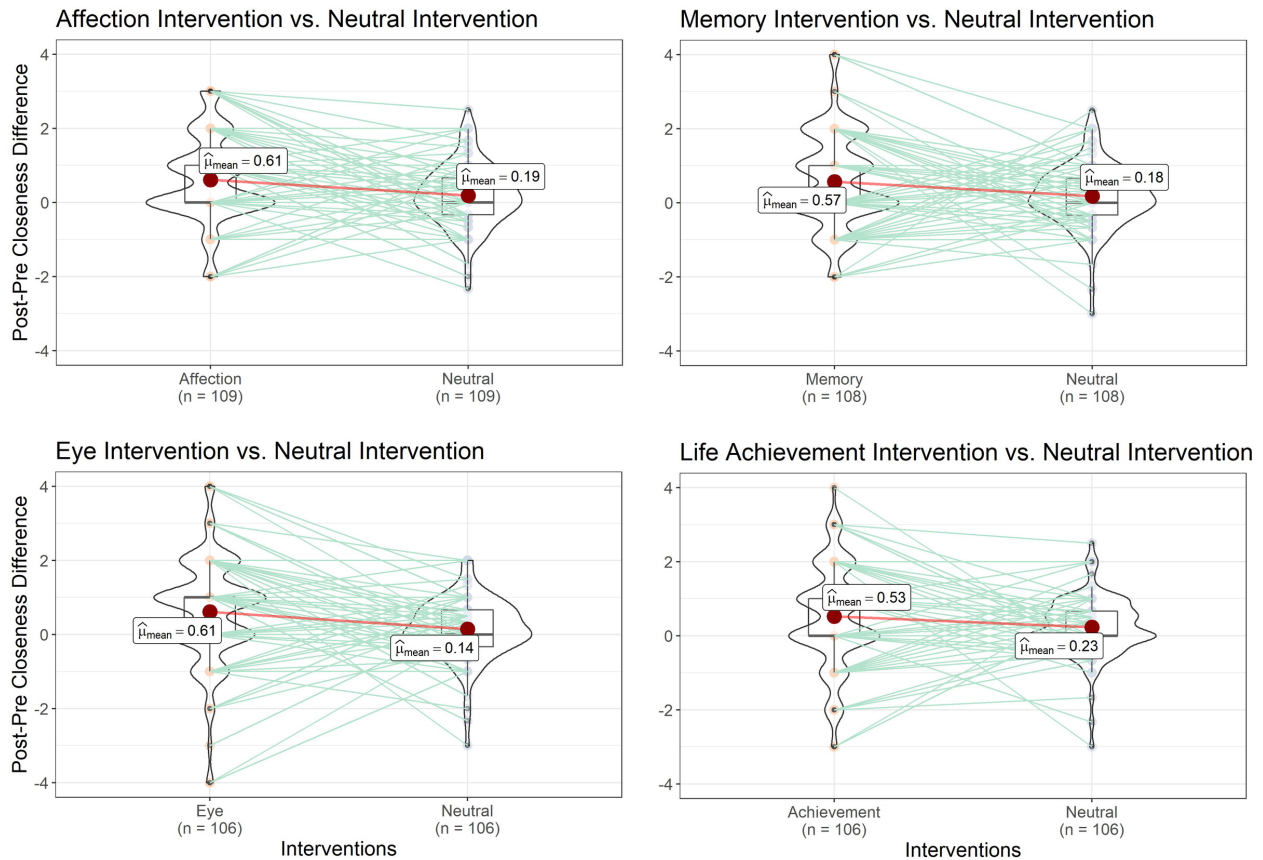
Applying a within-person experimental approach, our results support previous claims that central mechanisms to the experience of closeness and intimacy are affectionate physical interactions (Jakubiak & Feeney, 2017), self-disclosure (Beike et al., 2016), gazing (Andersen et al., 2006; Kleinke, 1986), as well as resource activation and building on strengths (Kauffman & Silberman, 2009; Snyder & Balderrama-Durbin, 2012; Zrenchik, 2015). We thereby (a) confirm and replicate these causal effects on an intraindividual level compared to the more commonly used interindividual approaches (e.g., Durbin et al., 2021), and (b) expand our knowledge on how to successfully apply these insights to interventions suitable to easy implementation in individuals' everyday life.

### Strength of Intraindividual Analyses: Examination of the Timing and Variability of Effects

There are two strengths associated with examining the causal encouragement effect of the interventions on closeness in a within-person experimental approach (Rohrer & Murayama, 2021; Schmiedek & Neubauer, 2020): First, our design allows us to not only evaluate the average causal effect of encouraging closeness to one's partner compared to neutral tasks with an acquaintance (as it is also possible in between-person experimental designs), but also to assess how variable this causal effect is across individuals, as

there exists a difference score for each single individual. Second, the timing of effects can be more closely scrutinized because within the intervention condition individuals differ in when they report on relevant outcomes, and this information can be used to make inferences about when the causal effect unfolds the strongest (again because of the presence of a difference score for each individual).

For the current research, these features of the study design nicely demonstrate that there indeed was variability in the observed effects that needs to be considered: While it could be concluded from the average effect that the interventions are generally capable of inducing closeness, the variability highlights that there was a great share of up to half of the sample for whom descriptively the interventions did not change or even deteriorate feelings of closeness. This variability of the average within-person effect in turn is interesting in itself, and invites to explore whether there are specific differences between individuals and situations that can explain for whom or when the interventions consistently work better or worse. Since we measured the effect of each intervention only once, our study is not suited to answer the question of consistency (Senn, 2018); however, in our correlational analyses, we took a first step towards identifying potential characteristics of individuals and situations with differential responses by examining two promising candidates on this matter: Partner responsiveness during the intervention and global relationship satisfaction with one's partner. There is a large body of research showing the crucial role of partner responsiveness for experiences of intimacy (Reis et al., 2004), and in the context of couples therapy, baseline levels of individual's relationship satisfaction predict the gains that can be achieved in



**Figure 3. Illustration of the difference between conditions in the post-pre closeness difference (change score), separately for each intervention**

Note: Figure available at <https://osf.io/agchm/>, under a CC-BY4.0 license.

Intervention	$M_d$	$SD_d$	$SE_d$	95% $CI_d$	$t$	$df$	$p$	$d_z$	Histogram	$BF_{0+}$
Physical affection	0.42	1.39	0.13	[0.20, $\infty$ ]	3.18	108	.001	0.30		34.3
Memory	0.39	1.52	0.15	[0.15, $\infty$ ]	2.67	107	.004	0.26		10.1
Eye	0.47	1.52	0.15	[0.23, $\infty$ ]	3.19	105	.001	0.31		35.2
Life achievement	0.30	1.58	0.15	[0.04, $\infty$ ]	1.94	105	.027	0.19		2.4





**Table 3. Paired one-sided t-test results of intention-to-treat analyses for the difference in change scores in closeness depending on condition (closeness vs. neutral), separately for each intervention**

Note.  $M_d$  = mean difference between conditions in post-pre closeness difference (change score),  $SD_d$  = standard deviation of this difference,  $SE_d$  = standard error of this difference,  $CI_d$  = one-sided confidence interval of this difference.  $BF_{0+}$  = directional Bayes Factor against the point null, given a Cauchy prior with a scale parameter of 0.27.

relationship quality (Roddy et al., 2020). Regarding the experience of closeness, the direction of correlations in our study are generally consistent with these findings, but of mostly negligible size (however, the rather small observed range of generally high responsiveness may be one reason why the corresponding effects are not more pronounced): The strongest effect could be observed for the correlation of partner responsiveness during the *eye intervention* and the observed change score difference, with a correlation coefficient of  $r = .24$ . This suggests that for this intervention it

might be particularly crucial that the partner is on board, and that proper guidance is especially needed. The correlations of global relationship satisfaction and the change score differences are generally smaller in magnitude than those with responsiveness and suggest that the interventions work quite similarly for individuals with different levels of global relationship satisfaction (if anything, showing the aforementioned trend that lower satisfaction leads to higher gains). However, as noted above, it must be acknowledged that our design does not allow to distinguish



Intervention	$M_d$	$SD_d$	$SE_d$	95% $CI_d$	$t$	$df$	$p$	$d_z$	Histogram	$BF_{10}$
Physical affection	0.09	1.61	0.17	[-0.24, 0.42]	0.53	93	.595	0.05		0.31
Memory	0.06	1.77	0.18	[-0.30, 0.42]	0.31	94	.761	0.03		0.28
Eye	0.05	1.90	0.20	[-0.35, 0.45]	0.24	89	.811	0.03		0.28
Life achievement	0.01	1.89	0.20	[-0.38, 0.40]	0.03	91	.973	0.00		0.27

**Table 4. Paired two-sided t-test results of intention-to-treat analyses for the difference in change score in state relationship satisfaction depending on condition (closeness vs. neutral), separately for each intervention**

Note.  $M_d$  = mean difference between conditions in post-pre state relationship satisfaction difference (change score),  $SD_d$  = standard deviation of this difference,  $SE_d$  = standard error of this difference,  $CI_d$  = two-sided confidence interval of this difference.  $BF_{10}$  = Bayes Factor against the point null, given a Cauchy prior with a scale parameter of 0.27.

true variability in the effectiveness of the intervention from error variance because we encouraged the different interventions each only once. Consequently, reliability of the measured effects on the intervention days is unknown (and probably lower than the reliability of the effects on the neutral days, which was the average across three possible measurements). Future research should therefore be devoted on the one hand to repeated assessments of the same closeness interventions within individuals, and on the other hand to a more extensive investigation whether there are indeed situational, individual and relationship variables that could *consistently* explain the variability in their effectiveness.

To disentangle the role of time, let us first have a look at the effect size of the average observed difference in changes of closeness between conditions: In terms of the raw metric, the effect ranged from one-third to nearly one-half point on the 7-point IOS scale, which can be considered quite large given a single micro-intervention and the assumption that intimacy is a process that ‘accrues across repeated interactions over time’ (Laurenceau et al., 1998, p. 1239). For comparison, Tsai et al. (2020) obtained similar effect sizes, but with a much more effortful online intervention program that included six separate interventions. We would argue that, depending on the intended use of the interventions, even smaller effects – such as a difference of one-fifth of a point of the IOS scale – would be of interest if they would show repeatedly, as such small increases could for example help create lasting intimacy in a relationship. The corresponding standardized effect sizes  $d_z$  are in the range of what we expected based on our own pilot study but are considerably smaller than the effects commonly reported in the literature of interventions on relationship quality (see Footnote 5). Given the meta-analytical effect of behavioral marital therapy of  $d = 0.585$  as upper realistically achievable effect of a prolonged psychological intervention (Shadish & Baldwin, 2005,  $d_z \approx 1.38$  given a correlation between pre- and post-measurement of  $r = 0.9$ ), our micro-interventions showing up to about one-fifth of this effect could be considered quite impactful. With regard to temporal patterns, our exploratory analyses showed that the estimated effect sizes were even considerably higher

when closeness was assessed in the task report immediately after the intervention compared to in the evening survey. This could indicate that the effects of the micro-interventions are indeed rather short-lived, and the associated closeness benefits are most pronounced directly after the intervention. Even when considering that the task reports include exclusively closeness reports of when the intervention was actually implemented, while the evening reports also include (the rather small share of) closeness reports on days in which the interventions were not implemented, this could not sufficiently explain the reduction in the observed effect. Still, our study design could be further improved to make definite statements about the exact timing of the effect and its persistence, as currently individuals were free to implement the interventions during any time of the day, and thus the evening report does not necessarily represent a (much) longer time span since implementation of the intervention (although it is on average so, because the task reports show that not all interventions were completed directly before going to sleep, when the evening assessment should be completed). Future studies should therefore either assess the time passed since the intervention was implemented, or instruct in a more controlled manner the time when it should be implemented.

In sum, our study demonstrates not only an average encouragement effect of the interventions on experienced closeness, but also variability in the effect that might be partly attributable to the responsiveness by the partner during the interventions, particularly in the *eye intervention*. It further shows that the effect on closeness seems to fade over time within a day. This questions our approach to merge task reports and evening reports as substitute assessments of post-levels of closeness, and generally calls for more detailed research on the unfolding and persistence of intervention effects.

### Specificity of the Intervention and the Role of Associated Psychological Processes

Another important aspect to consider when designing interventions aimed at manipulating psychological constructs is the precision with which the target variable – and ideally no other variable – is being changed. Introduc-

**Table 5. Correlations of differences in change scores and other variables, separately for each intervention**

	Affection				Memory				Eye				Life Achievement			
	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
1) Closeness: Difference between conditions																
2) RS: Difference between conditions	.38***				.47***				.49***				.37***			
3) Responsiveness in closeness condition	.11	.09			.19	.14			.24*	.22			.17	.04		
4) Responsiveness in neutral conditions	.11	.16	.38***		.00	-.01	.44***		.00	-.06	.18		-.10	-.14	.34**	
5) Global relationship satisfaction	-.11	.15	.56***	.19*	-.17	.02	.49***	.19*	-.04	.10	.49***	.19*	-.09	.04	.51***	.19*

*Note.* Difference between conditions = difference between neutral and closeness condition in post-pre difference (change scores), separately for the difference in closeness change scores and the difference in state relationship satisfaction (RS) change scores. Responsiveness is assessed with respect to how the interaction partner behaved. Global relationship satisfaction is assessed in respect to the relationship partner. Significance levels are reported to highlight the strength of certain correlations, with \*\*\*  $p < .001$ , \*\*  $p < .01$ , \*  $p < .05$ .

ing the concept of ‘fat-handed interventions’ for manipulations that affect multiple variables simultaneously, Eronen (2020) describes in great clarity that it is rather challenging (if not impossible) to develop psychological interventions that must not be classified as being fat-handed (given the indirectness with which interventions have to operate and the general measurement challenges and uncertainties associated with the assessment of psychological variables). Even if this limitation cannot be overcome completely and interpretations of the causal effect supposedly attributed to a certain psychological outcome (here: closeness) have to take this ambiguity into account, it still seems to be worthwhile to explore plausible alternative outcomes that might have inadvertently been affected by an intervention.

In our study, we focused on state relationship satisfaction as such an alternative outcome, finding no evidence for any of our intervention encouragements having on average a short-term influence on changes in experienced momentary relationship satisfaction. A major limitation of this analysis is that state relationship satisfaction was only assessed in the evening and not during the task report, hence - as discussed in the previous section - at a time where also for closeness as outcome the intervention effect was reduced or even vanished. Still, our finding is in line with the finding by Durbin et al. (2021) (see their Supplemental Materials) who reported no effect of a physical affection intervention on relationship satisfaction assessed after the study (but on intimacy directly after the intervention). While these are good news in terms of evaluating the fat-handedness of our interventions, this poses questions about the psychological process that is expected to unfold as a consequence of the experience of closeness, namely its positive influence on experiences of relationship quality (as laid out in the introduction).

In this regard, our correlational analyses inform about some influences on whether experiences of closeness following the intervention result in associated changes in relationship satisfaction as well: First, the more pronounced the increase in closeness was, the more pronounced increases in relationship satisfaction could be observed. Second, interaction partner responsiveness seemed to be a small relevant factor for both change score differences. These findings might indicate that the interventions exhibit a direct influence on closeness in a first step, which might then mediate (together with other influences uncontrolled by the intervention, such as partner responsiveness) whether this closeness experience also affects state relationship satisfaction. The Dynamics of Motive Satisfaction model (Zygar et al., 2018) for example would further assume an individual’s motivational states as important influence on whether closeness relationship experiences translate to an enhanced relationship satisfaction. Stable interindividual differences such as attachment styles could

further play an important role (see e.g., Poucher et al., 2022). Further research should be devoted to explicitly model the assumed mediation between the encouragement, the target effect and further outcomes, while also considering moderating influences such as partner responsiveness and motivation. Again, a close look at the temporal unfolding of these effects with the help of intensive longitudinal data can help uncover the sequence in which effects take place, and repeated implementation of the closeness interventions may help entangle systematic variability from error variance. Recently, Schmiedek and Neubauer (2020) introduced an analytical framework based on structural question modeling that would fit perfectly for these types of research questions, which would additionally allow to estimate the treatment effect itself next to the intention-to-treat effect.

### Implications of Investigating the Intention-To-Treat Effect

While it certainly is interesting for future research to have a look at the treatment effect, which represents the causal effect when the intervention is actually implemented rather than only encouraged, it is important to note that the intention-to-treat effect as estimated in our study provides important information for use cases of our interventions: On the one hand, when applied in psychological research with similar designs than ours, implementation of the intervention cannot be ensured, thus the potentially non-random non-compliance would not be considered when using the treatment effect as estimate of how much the intervention may induce changes in closeness. On the other hand, application in practitioner settings may also include assigning these interventions as ‘homework tasks’ which again contain the possibility of non-compliance. Finally, the intention-to-treat effect equals the treatment effect when complete compliance is ensured. Given the high compliance rates of up to 90% in our study, the difference between the intention-to-treat and the treatment effect might be negligible anyway.<sup>11</sup>

### Limitations: Possible Demand and Carry-Over Effects, Control Conditions without the Partner, Risk of Bias, and Constraints on Generalizability

The results must be interpreted given some limitations inherent to our study design and sample: First, participants were overtly instructed to report on their experiences after the interventions, which could have led to demand effects. Specifically, participants might have felt compelled to report higher levels of positive relationship outcomes after the closeness interventions, because they wanted to respond in accordance with what they think the hypotheses being tested are (Orne, 1962). It was probably clear which

11 Other studies report comparably lower compliance (e.g., 63% in Durbin et al., 2021). As compliance was self-reported it cannot be ruled out that participants responded in a social desirable fashion and/or thought their study compensation depended on this question (which was not the case, and communicated accordingly, but there were inquiries by participants on this matter).

interventions were meant to enhance closeness to the partner and which interventions were not, since the study was advertised as a study on closeness and since the control condition did not involve one's partner. However, as we found differences between task and evening reports, the discussed demand effect would have to be very distinctly related to a biased reporting of closeness during the task report. Still, future studies could be more ambiguous in informing participants about the nature of the construct being studied.

Second, although our semi-randomly generated stratification ensured a controlled number and specific alteration of neutral and closeness interventions for each participant (see Footnote 4), it might also have introduced specific carry-over effects from one type of intervention to the next. Future studies employing a more thorough randomization could rule out such effects, for example by randomizing the intervention that is instructed on each day of each participant.

Third, descriptively, even in the neutral conditions, feelings of closeness and state relationship satisfaction changed on average as the day unfolded. This demonstrates the importance of involving a control condition in the analyses, rather than just comparing feelings of closeness or relationship satisfaction from early in the day to after some intervention, neglecting naturally occurring changes in the outcomes across time. Still, albeit our neutral interventions can be classified as active rather than passive control conditions, they assigned tasks with an acquaintance and did not involve the partner. Hence, the role of the specific kind of interactions we instructed compared to other interactions *with the partner* remains unclear: All effects could be solely due to sharing more time with the partner than one would have done without the respective encouragements. Further, the neutral interventions involved self-disclosure about one's media preferences and thus some form of verbal communication with other people, thus being more comparable to the *memory intervention* and the *life achievement intervention* than to the *physical affection intervention* and the *eye intervention* - both which could in principle be implemented without any verbal communication. Future work that pinpoints the exact closeness-enhancing aspects of our interventions by developing neutral conditions that involve the partner would be informative about the relevant mechanisms. For example, communication with the partner that does not involve self-disclosure or shared resources would be interesting to compare with the *memory intervention* and the *life achievement intervention*. A challenging aspect would be to find communication topics that do not invite for naturally occurring disclosure of self- or relationship-related views, memories or strengths. For the *physical affection intervention* it would be worth taking a look at interventions that involve some kind of physical contact that is not considered affectionate, such as measuring each other's arm length or height. Finally, the *eye intervention* could be matched with a control condition involving both partners to focus on a point in the room, but not looking at each other.

Fourth, there is a small risk of bias associated with our post-measurement of closeness. For each intervention, there were days with a pre-measurement but not a post-measurement of closeness, neither in the task report nor in the evening survey, and thus no change score could be calculated (less than 10% of days for each intervention, between  $n = 3$  and  $n = 12$ ). These missing post reports might be subject to a systematic sampling and selection bias, for example if individuals skipped the evening assessment when they did not complete the intervention (even though they were instructed to do so nonetheless). Worse, such a bias might also occur if participants did not complete a survey because they had a bad day with their partner following the intervention, or a bad time while completing the task. This would lead to an overestimation of the effect size and an underestimation of the variability of the effect. Similarly, the observed drop in effect strength from task measure to evening assessment might also be due to a bias of increased reporting after the task when the intervention was a success. Future studies employing designs such as ours should therefore especially incentivize providing complete sets of pre- and post-assessments (rather than compensating the completion rate across all possible assessments), and place special emphasis on this during instruction of the participants.

Last, the study sample consists predominantly of female students who were in a different-sex relationship - with correspondingly low rates of marriage and children in these relatively young relationships. This imposes constraints on generalizability of our findings at two central levels: On the one hand, the sample represents only very specific relationship constellations. For example, couples with children have different challenges staying close to their partner than childless couples (Delicate et al., 2018; Kluwer, 2010; Lawrence et al., 2010). On top of that, relationship duration as well as marital and occupation status may play a crucial role for couples' interaction characteristics and time devoted to the partner (Flood & Genadek, 2016; Kalmijn & Bernasco, 2001; Reese-Weber, 2015). Although our sample did not consist solely of individuals who were highly satisfied with their relationship, a much higher rate of distressed participants would be expected among those seeking couples therapy. In addition, the study demanded quite some effort and specific time availability from participants, which may have led to a self-selection of participants, such as mainly attracting the student population we observed. On the other hand, our sample consists of individuals rather than of both partners of the couple, hence the effect of our interventions on the participants' partners - who was mostly the male counterpart - remains also unclear. While partner effects can be expected based on research showing for example that physical touch has actor as well as partner effects (Debrot et al., 2013), this should be verified for our concrete interventions by involving the partner in the encouragement and reporting parts of the study. In this regard it would be interesting to investigate if the effects are even more pronounced when both partners are encouraged and may thus be more committed to the implementation of the interventions.

## Use Cases in Practitioner and Research Settings

Several use cases for the evaluated interventions can be imagined. As intimacy is considered a focal outcome of relationship intervention programs (Roddy et al., 2020) the current research offers the evidence basis for tasks that may be considered as especially useful for application in practitioner settings. However, given the limitations discussed (especially about the representativeness of the sample) and also given that the study design focused on couple's natural everyday life rather than the more special setting of couples searching for help, research evaluating the interventions in direct application in counseling and therapy is required (see Sexton & Lafollette, 2016 for an overview of steps needed for translating relationship research into clinical practice). Considering the fact that a third person such as a counselor or therapist may help to guide the implementation of the interventions more strongly in directions that may improve their effectiveness (e.g., by discussing and practicing responsiveness, or tailoring certain characteristics of the intervention to individual's and their partner's needs), we would expect the achievable effect sizes to be even more pronounced. The successful instruction of the intervention via smartphones and also mostly irrespective of individuals' baseline global relationship satisfaction is further promising for initiatives that aim to provide low-threshold assistance with relationship difficulties (or relationship growth more generally) through tasks, tips and inspiration guided by smartphone applications (e.g., Lucier-Greer et al., 2018; see also Kanter & Schramm, 2018).

In research settings, the micro-interventions provided and evaluated in this study are an important starting point for experimental research about the effects of closeness on other individual and relationship outcomes, including the causality of processes often postulated in theories about relationship functioning. The variety of interventions tested in this study provides a useful palette of different associated intervention characteristics, that may play a role depending on the research question: For example, the *physical affection intervention* and the *eye intervention* are characterized by the feature that they can be instructed and implemented repeatedly to some extent, whereas the *memory intervention* and the *life achievement intervention* may be more suited for a one-time application. Parallel variants of the interventions may be considered in future research, for example by retaining the idea of sharing memories with the partner, but by changing the topic in repeated instances of the intervention. The interventions also differ in their requirements for implementations, for instance in terms of the physical presence of the partner (e.g., only relevant for the *physical affection intervention*) or the time demand and complexity involved (e.g., intrusiveness and implemen-

tation difficulty being highest for the *life achievement intervention* and *eye intervention*<sup>12</sup>), and partner involvement (e.g., not even required for a one-directional self-disclosure in the *memory sharing intervention*). Further, the higher level of reported oddity of the *eye intervention* may be considered a strength in terms of introducing novelty to relationship behaviors. Our study shows cases that even more complex and effortful tasks like the *life achievement intervention* are suitable as ecological momentary interventions. In sum, researchers may choose among our investigated interventions the one that best suits their envisioned study design and research question.

## Conclusion

By developing four micro-interventions with which experiences of closeness in couple relationships can be enhanced, this study made a contribution to current research on relationship functioning. The interventions, which rely on physical contact (*physical affection intervention*), self-disclosure (*memory intervention*), gazing behavior (*eye intervention*) and resource activation (*life achievement intervention*) showed to be successful in experimentally manipulating closeness. They may be used in future studies investigating the causal role of closeness in couple relationships. Moreover, they may be considered in psychological practice to help increase closeness between partners, for example complementary to couples counseling.

## Contributions

Contributed to conception and design: CZ-H, LC, LW, FS  
 Contributed to acquisition of data: LC, LW  
 Contributed to analysis and interpretation of data: CZ-H, LC, LW, FS  
 Drafted and/or revised the article: CZ-H  
 Approved the submitted version for publication: CZ-H, LC, LW, FS

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<sup>12</sup> The complexity of the eye intervention could be further reduced by limiting it to an implementation in person, as some participants reported that the implementation via video chat produced some difficulty as to whether to look into the camera or on the partner's eyes on the screen, making video chat not very well suited for this type of intervention.



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### Competing interests

The authors have no competing interests to declare.

### Data accessibility statement

Anonymized data, a codebook, reproducible scripts, as well as further Supplementary Materials can be found at <https://osf.io/agchm/> (Zygar-Hoffmann et al., 2022). The

preregistration for the *physical affection intervention* and the *memory sharing intervention* can be found at <https://osf.io/ge4sd/> and for the *eye contact intervention* and the *life achievement intervention* at <https://osf.io/5czf7/>.

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