Review Article

Prosocial Consequences of Interpersonal Synchrony

A Meta-Analysis

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Abstract: The capacity to establish interpersonal synchrony is fundamental to human beings because it constitutes the basis for social connection and understanding. Interpersonal synchrony refers to instances when the movements or sensations of two or more people overlap in time and form. Recently, the causal influence of interpersonal synchrony on prosociality has been established through experiments. The current meta-analysis is the first to synthesize these isolated and sometimes contradictory experiments. We meta-analyzed 60 published and unpublished experiments that compared an interpersonal synchrony condition with at least one control condition. The results reveal a medium effect of interpersonal synchrony on prosociality with regard to both attitudes and behaviors. Furthermore, experimenter effects and intentionality moderate these effects. We discuss the strengths and limitations of our analysis, as well as its practical implications, and we suggest avenues for future research.

Keywords: interpersonal synchrony, prosociality, meta-analysis, attitude, behavior

Ranging from soldiers marching in step, to rave dancers moving to the same beat, andto the synchronous bowing of praying men in a mosque, interpersonal synchrony is part of many rituals and also occurs in mundane events. The notion that synchrony fosters social bonding dates back as far as Durkheim (1912). However, only recently have experiments established a causal link between interpersonal synchrony and prosociality (e.g., Hove & Risen, 2009; Macrae, Duffy, Miles, & Lawrence, 2008; Wiltermuth & Heath, 2009). Interpersonal synchronization is a prerequisite for empathy (Valdesolo & DeSteno, 2011) and successful cooperation (Valdesolo, Ouyang, & DeSteno, 2010) – and it is therefore vital for almost every social interaction.

Interpersonal synchrony has been examined in a variety of forms (e.g., tapping, walking, bouncing) and outcomes (e.g., affiliation, helping behavior, other-related memory). However, no quantitative review to date has integrated the scattered experiments on the social consequences of interpersonal synchrony. Furthermore, little is known about the variables that moderate interpersonal synchrony's potential effects on social outcomes – a fact that appears to be particularly relevant in light of failed replications (Kirschner & Ilari, 2013; Schachner & Mehr, 2015).

The central goal of this study is to investigate, by meta-analyzing the available experiments, whether and to what extent interpersonal synchrony fosters prosociality. Moreover, we examined moderators that may explain variability regarding the effect of interpersonal synchrony on prosociality.

Definition of Interpersonal Synchrony

Interpersonal coordination is a prerequisite for smooth social interaction, and it can be divided into behavioral matching (i.e., mimicry) and interpersonal synchrony (Bernieri & Rosenthal, 1991). Whereas mimicry refers to the imitation of others' actions and thereby entails a time lag, interpersonal synchrony refers to instances when the movements of two or more people overlap in time (Bernieri, Reznick, & Rosenthal, 1988). In accordance with a narrow definition of synchrony, the time overlap is characterized by behaviors occurring in-phase, in contrast with anti-phase coordination (Reddish, 2012). Although in-phase and anti-phase are both stable modes of coordination, in-phase synchrony is the more stable mode (Kelso, 1995).

Interpersonal synchrony is not limited to behavioral synchrony but includes synchrony on neural, physiological, and affective levels (Mazzurega, Pavani, Paladino, & Schubert, 2011; Semin, 1996). For example, observing others' actions elicits neural synchronization in terms of time-locked resonance in the motor cortex (Fadiga, Craighero, & Olivier, 2005), ritual spectators show synchronized arousal with performers (Konvalinka et al., 2011), and protesters entrain their emotional reactions (Páez, Rimé, Basabe, Wlodarczyk, & Zumeta, 2015). The causal link between interpersonal synchrony and prosociality has been repeatedly established with regard to synchronous movement (e.g., Fessler & Holbrook, 2014; Wiltermuth & Heath, 2009), synchronous vocalization (e.g., Harmon-Jones, 2011), and synchronous sensory stimulation (e.g., Mazzurega et al., 2011), hinting at a common mechanism.

In the current meta-analysis, we included two types of interpersonal synchrony, namely synchronization of motor movements and synchronization of sensory stimulation. Synchronization of motor movements encompasses instances when two or more individuals synchronize the movements of their bodies, parts of their bodies, or their vocalizations. This category includes not only active movement but also passive movement (i.e., movements caused by a third person, such as when infants are held by the experimenter and are gently bounced up and down, Cirelli, Einarson, & Trainor, 2014). Synchronization of sensory stimulation refers to instances when two or more individuals experience a synchronous sensory experience (e.g., being touched by a paint brush on the cheek). We focused on these two types of interpersonal synchrony because they were investigated in a sufficient number of experiments, and their effects were argued to arise from a common mechanism (i.e., both synchronous motor movement and synchronous sensory stimulation result in synchronization of the individual's bodily sensations; Paladino, Mazzurega, Pavani, & Schubert, 2010). Therefore, in this meta-analysis, we use the term motor-sensory interpersonal synchrony (MSIS) to consider these two facets of interpersonal synchrony.

Regarding motor synchrony, synchronization can concern the same or different actions, whereas the most common operationalization of interpersonal synchrony in experiments is to use actions that are matched in form. To allow for sufficiently homogenous operationalization of interpersonal synchrony, we adopted this narrow definition of MSIS and included only experiments that applied movement/sensory stimulation matched in form, rather than synchronous but dissimilar movement/sensory stimulation.

Furthermore, motor synchrony can be established by three types of entrainment processes: reciprocal entrainment, which refers to instances when all interaction partners deliberately synchronize their movements; unilateral entrainment, which pertains to situations when a single actor entrains his or her actions to the interaction partner (s); and orchestral entrainment, which refers to situations where interpersonal synchrony is established as a by-product of entraining to an external pacemaker (Cacioppo et al., 2014).

Why Interpersonal Synchrony?

The notion that cultural groups deliberately use MSIS to establish social bonds and to facilitate cooperation has roots that date back to Durkheim (1912). Durkheim observed that during collective rituals, movements and verbal expressions synchronize to create what he called *collective effervescence*, understood as a community spirit that manifests itself in a feeling of excitement and loss of individuality. Similarly, the hypothesis that synchronized movement diminishes self-other boundaries and elicits bonding lies at the heart of contemporary theories of collective ritual (Collins, 2005; McNeill, 1997) and crowd behavior (Haidt, Seder, & Kesebir, 2008). Common to all of these theories is the notion that MSIS is a rewarding experience. Corroborating this observation, recent neurological research has found evidence that synchronous drumming activates the reward system (Kokal, Engel, Kirschner, & Keysers, 2011). Because pleasure is a powerful mechanism by which evolution has encouraged adaptive behavior (Wheatley, Kang, Parkinson, & Looser, 2012), it is not surprising that researchers have examined the adaptive benefits of MSIS.

Merker (Merker, 2000; Merker, Madison, & Eckerdal, 2009; Merker, Morley, & Zuidema, 2015) argued that the human ability to entrain to an isochronous beat (which is rare in the animal world) evolved as a means for males to attract mates. Specifically, synchronous singing or vocalizing intensifies the signal and, in turn, increases its geographic reach (the so-called *Beacon effect*). This leads to a mating advantage in attracting migrating females because (1) the signal attracts greater attention; (2) it communicates resource richness by indicating membership in a large group; and (3) it signals a high level of cooperativity among group members. The last point is compatible with Hagen and Bryant's (2003) *coalitional signaling hypothesis*, which argues that synchronous singing communicates the group's cooperative strength to other individuals, above all to competing groups.

Corroborating the idea of a selection pressure for human entrainment capacity, interpersonal synchronization can emerge automatically (e.g., synchronized clapping in large crowds, Neda, Ravasz, Brechet, Vicsek, & Barabasi, 2000) and can even be observed in newborns (Condon & Sander, 1974).

Wheatley and colleagues (2012) added that being in synchrony may be rewarded because it is an effective means of understanding one's interaction partner – a prerequisite for successful cooperation. Specifically, synchronization leads to an alignment of one's own and the interaction partner's neural representations, thus facilitating communication (Nummenmaa et al., 2012). Therefore, Wheatley and colleagues (2012) argued that being *in sync* is a rapid and effective way to "get on the same page" while reducing the processing load of the brain and thereby saving energy. A second reason for the adaptive benefits of interpersonal synchrony, they argued, is signaling social proximity or similarity. On the one hand, it is adaptive to recognize the similarity of others, such as one's family or group, from a survival point of view. On the other hand, drawing on Dawkins's *selfish gene theory* (Dawkins, 1989), it is beneficial to recognize and support those who share many genes to foster these genes' reproduction – these people may include family, as well as unrelated individuals. Corroborating this idea, research has found that we are more inclined to help and mate with individuals who are similar to ourselves (Fessler & Holbrook, 2014; Lumsden, Miles, & Macrae, 2014). Thereby, similarity cannot be reduced to physical or olfactory characteristics, but it includes similar behavior (Dawkins, 1989). Interestingly, there is a bidirectional association between interpersonal synchrony and similarity: we synchronize most with related individuals (Konvalinka et al., 2011); however, the opposite is also true in that synchronization fosters perceived similarity and closeness (e.g., Mazzurega et al., 2011). Taking this concept one step further, this means that human beings have the capacity to induce similarity and closeness artificially (and thereby a fitness advantage) by synchronizing with others. In support of this idea, it was found that people synchronize more when motivated to belong to a group (Miles, Lumsden, Richardson, & Macrae, 2011).

Potential Mediators of the Effect of MSIS on Prosociality

The reason why interpersonal synchrony promotes prosociality lies at the heart of various approaches, which we review in the following. Hatfield, Cacioppo, and Rapson (1993) promoted the idea that MSIS facilitates emotional contagion by enhancing the moment-by-moment tracking of the partner's feelings. Similarly, Macrae and others (2008) have argued that individuals are prone to perceptual and attentional biases toward synchronous counterparts: after synchronous movement, participants were better able to remember other-related information (Miles, Nind, Henderson, & Macrae, 2010), as well as to perform better at recognizing the interaction partner's face (Macrae et al., 2008). As a consequence of this heightened attentiveness, it has been argued that synchronous individuals are more inclined to communicate (Anshel & Kipper, 1988) and/or to feel empathy (Hatfield et al., 1993) with their counterparts, thus promoting cooperation and helping behaviors.

A second stream of research has found evidence that interpersonal synchrony facilitates the blurring of self-other boundaries (see Smith, 2008 for a review). According to the shared circuits model (SCM; Hove, 2008) human beings detect self-produced action by the temporal alignment of predicted and actual sensory consequences. For example, if I plan to clap my hands and instantaneously feel and see my hands clap, I conclude that the clapping hands are my hands and that I caused the hands to clap. However, in the case of synchronous clapping, the other's clapping overlaps with my own clapping prediction, thus rendering it difficult to distinguish self-produced and other-produced movements or self-other distinctions in general. In other words, the temporal closeness of prediction and sensation, which is usually used to determine self-other boundaries, becomes ineffective in cases of interpersonal synchrony. The rubber-hand illusion is a striking example of this effect. As a consequence, interpersonal synchrony causes a merging of the concept of the self and the other, in the sense of including the other in the self. This process entails projecting the positive views of the self onto the other (Smith, 2008). Taken one step further, this overlap can explain prosocial behavior because the tendency to favor the self in the distribution of resources now extends to the other (who is, in effect, part of the self) (Aron, Aron, Tudor, & Nelson, 1991). Corroborating this idea, the extent of self-other overlap was found to predict cooperative behavior (Reddish, Fischer, & Bulbulia, 2013), as well as compassion (Valdesolo & DeSteno, 2011).

Inspired by Hagen and Bryant's *coalitional signaling theory* (Hagen & Bryant, 2003), Reddish and others (Reddish, Fischer, & Bulbulia, 2013) found empirical support for their *reinforcement of cooperation* model, which posits that synchrony signals cooperative ability not only to adversaries but also to the group itself. This perceived cooperative ability in turn fosters a feeling of unity and trust, which heightens the perceived probability that co-participants behave cooperatively and thus increases the individual's propensity to cooperate. Interestingly, it was shown that synchrony actually improves the ability to cooperate (Knoblich, Butterfill, & Sebanz, 2011; Sebanz, Bekkering, & Knoblich, 2006; Valdesolo et al., 2010). That is, on the one hand, interpersonal synchrony fosters the synchronizers' *motivation* to engage in cooperative behavior – in part because they believe that their synchronous partners will cooperate as well – and on the other hand, interpersonal synchrony enhances the *ability* to cooperate. Valdesolo et al. (2010) found that a boost in perceptual sensitivity regarding the movements of the interaction partner mediated the effect of interpersonal synchrony on enhanced success in the cooperation task. In other words, the signal of cooperative ability that emanates from synchronous movement is paralleled by an actual increase in cooperative capacity among those who synchronize their actions.

Finally, neurological research has identified the activation of the reward system as a potential mediator of the effect of interpersonal synchrony on helping behavior (Kokal et al., 2011). Specifically, it was found that synchronous drumming activates the caudate region and that the extent of activation of this brain region in turn predicts the extent of helping. Kokal and others (2011) concluded that synchrony results in the release of reward signals and that this reward history is then linked to the synchronous counterpart. Later, this learned positive association fosters the participant's propensity to help his or her co-drummer.

Consequences of MSIS and Potential Moderators

Motor-sensory interpersonal synchrony (MSIS) was found to have various prosocial consequences ranging, from perceptual closeness (Mazzurega et al., 2011), affiliation (Hove & Risen, 2009), and increased other-related memory (Miles et al., 2010) to cooperation (Wiltermuth & Heath, 2009) and helping behavior (Cirelli, Einarson, et al., 2014; for a review, see Repp & Su, 2013). In the current meta-analysis we chose to differentiate between two categories of outcome variables: attitudes and behavior. This decision was based on both empirical research and theoretical models that highlight the gap between attitudes and behavior in general as well as prosocial attitudes and prosociocial behavior in particular. In their meta-analysis, Glasman and Albarracín (2006) reported a medium-sized correlation between attitudes and behavior (r = .51; 95% CI [.48, .54]), which indicates that attitudes go hand in hand but are not perfectly consistent with future behavior. Kruglanski et al. (2015) theorize that attitudes and behavior do not always overlap because several conditions have to be met for attitudes to predict behavior (e.g., the attitude toward the target must be transferred into a goal, the goal must be dominant, the specific behavior must be deemed an adequate strategy to reach this goal). In the domain of prosociality, Anker, Feeley, and Kim (2010) who studied the link between prosocial attitude and actual prosocial behavior using the example of donations, observed a considerable gap between the extent of pro-donation attitudes and actual donations. Based on this research and theorizing we considered it likely that MSIS will have different effects on attitudes and behavior and therefore conducted two separate meta-analyses on these two outcome categories.

In addition to a growing body of empirical evidence for this effect of MSIS on prosociality, few studies have systematically investigated the potential variables that moderate this effect (e.g., Reddish, Fischer, & Bulbulia, 2013; Wiltermuth & Heath, 2009). The need to investigate potential moderators has been emphasized by recent failed replications (Kirschner & Ilari, 2013; Schachner & Mehr, 2015). In this meta-analysis, we examined the following potential moderators.

Intentionality

Motor-sensory interpersonal synchrony (MSIS) can occur intentionally, as well as incidentally. Knoblich and others (2011) use the terms *planned coordination* and *emergent coordination* to denote instances when coordination occurs as a consequence of a common goal (i.e., intentionally) versus as a consequence of simple perception-action coupling (i.e., incidentally). An example of emergent interpersonal synchrony is participants entraining to the same beat without having the goal of performing synchronous movements.

Shared intentionality by itself is a putative mechanism to create prosociality because the imperative of representing and integrating another person's mental state to establish synchrony, as well as the existence of a common fate, facilitates interdependent self-construal (Kirschner & Tomasello, 2010; Reddish, 2012). Furthermore, Reddish (2012) argued that shared intentionality boosts the effect of interpersonal synchrony on prosociality by two mechanisms: (1) shared intentionality necessitates increased attention toward the other person, which in turn intensifies self-other blurring, and (2) if established intentionally, interpersonal synchrony is an ideal marker for the extent of cooperativity of a group. As discussed above, high cooperative ability encourages each individual's propensity to behave cooperatively. Corroborating these ideas, it was found that intentionality increases the effect of MSIS on prosociality (Reddish, Fischer, & Bulbulia, 2013), which is what we expect to find in this meta-analysis.

Large Versus Small Muscle Involvement

On the one hand, studies have varied regarding the body parts involved in synchrony (Delaherche et al., 2012). On the other hand, research into the question of the type of movement that promotes prosociality most has been scarce. Anthropologists have focused their investigations on gross-motor "muscular-bonding," such as marching and dancing (McNeill, 1997). An empirical study by Wiltermuth and Heath (2009), however, failed to find a difference between synchronous treatments involving singing only versus moving and singing in synchrony. Because most primary research examines only one movement, the type of synchronized movement that is most effective in establishing prosociality remains largely unknown. The current meta-analysis aims to fill this gap.

Relationship Between Interaction Partners

Research on the effects of existing social bonds on the occurrence of synchrony has suggested that individuals are more inclined to synchronize with related/in-group members (Grammer, Kruck, & Magnusson, 1998; Konvalinka et al., 2011) or with individuals with whom they wish to bond (Miles et al., 2011). To our knowledge, no study has yet investigated whether the effect of experimentally induced synchrony on prosociality differs for similar/related versus dissimilar/unrelated individuals. Therefore, in the current meta-analysis, we investigate whether the interaction partner's sex (same vs. different) and prior social bonds moderate the effect of MSIS.

Number of Interaction Partners

Most research on interpersonal synchrony has been conducted with dyads rather than groups (Reddish, Fischer, & Bulbulia, 2013), and it is likely that the effects of MSIS differ depending on whether participants synchronize with one agent, rather than more than one agent. In dyadic interactions, feedback concerning the level of synchrony may be more direct and less ambiguous than in group settings, in which the level of synchrony may differ from person to person. Furthermore, it is sensible to assume that the blurring of self-other boundaries is somewhat easier in two-person interactions because giving someone one's undivided attention presumably facilitates including the other in one's self-concept. Therefore, we investigate if the effect of MSIS is more pronounced in dyadic interactions than in groups.

Music

Music figures prominently in social gatherings, and it has been assigned a putative role in the evolution of group cohesion ("vocal grooming," Fitch, 2006). Hagen and Bryant (2003) presented evidence that the synchrony that is established through music is a specifically credible index of group coalition quality to the out-group because music requires practice to be performed correctly and therefore indicates the group's longevity and ability to perform complex actions. However, the hypothesis that, by the same token, interpersonal synchrony is more effective in eliciting prosociality in group members when accompanied by musical elements was not confirmed in prior studies (Harmon-Jones, 2011). In light of this controversy, in the current meta-analysis, we explore the effects of MSIS regarding the role of musical elements. Specifically, we contrasted experiments in which MSIS was accompanied by a predictable rhythmic sound (e.g., a metronome beat, sounds produced by participants' drumming, a song) with experiments in which no sound accompanied the synchronous movement/stimulation.

Experimenter Effects

It has been well established that the experimenter's expectations can influence participants' behavior – even if the contact between the experimenter and participant is scripted and minimal (Rosenthal & Rubin, 1978). Preliminary evidence has suggested that the effects of MSIS dissipate when controlling for this bias (Schachner & Mehr, 2015). Thus, in this meta-analysis, we assume that the effect of MSIS on prosociality is larger in the presence of an unblinded experimenter.

Other Methodological Characteristics

Finally, we investigated whether the design of the primary study (within vs. between), type of synchrony (active movement, passive movement, sensory stimulation), and implementation of a manipulation check (vs. lack thereof) moderate the effects of MSIS.

Objectives

Because research on MSIS has been largely conducted in the form of single studies, often on the basis of small and homogenous samples, the current meta-analysis aims to synthesize the isolated and sometimes contradictory findings. To date, there have been no quantitative reviews of the effect of MSIS on prosociality. Whereas synchronization to an external beat has been intensively studied (see Repp, 2006a, 2006b; Repp & Su, 2013 for a review), only one qualitative systematic review (Repp & Su, 2013) has examined the effects of interpersonal synchrony on social outcomes. Repp concluded that interpersonal synchrony yields positive effects in terms of heightened prosociality; however, the size of these effects as well as potential moderators remain unclear.

In the present meta-analysis, we quantitatively assessed the social consequences of MSIS and systematically investigated potential moderators of this relationship – including both moderators already explored in primary research and additional moderators that are difficult to manipulate in one-shot experiments.

Research Questions

The meta-analysis at hand seeks to answer the following questions:

RQ1a: Which social consequences does MSIS entail?

RQ1b: What is the size of the effects, if there are any effects?

RQ2: Which variables (if any) moderate the effects of MSIS on social outcomes?

RQ3: Does the effect of MSIS depend on the type of comparison group used?

Methods

Inclusion and Exclusion Criteria

To be included in the meta-analysis, studies needed to report at least one effect size or information to calculate an effect size of the effects of MSIS (as defined above) on social outcomes. We defined social outcomes as all reactions pertaining to other social entities involved in the synchronous or control intervention, as well as all variables measuring characteristics of social interactions among participants. Importantly, in this meta-analysis, social outcomes were limited to the individuals immediately involved in the MSIS. We did not include outcomes concerning social behavior/attitudes toward individuals or groups not involved in the MSIS (e.g., prosocial attitude in general). Furthermore, we included only studies that used an experimental design in which MSIS was compared with at least one control group. Regarding the type of sample, no age requirements were set; however, only samples of nonclinical participants were included.

Search Strategies

Electronic literature searches were performed in the following outlets: PsycINFO, Psyndex, Medline, ERIC, Web of Science, wiso Wirtschaftswissenschaften, Business Source Premier, Dissertation & Theses, A & I, and Sociological Abstracts. To come up with suitable keywords, the research question was decomposed into its components (interpersonal, motor, sensory, synchrony, social consequences). For each component, we identified synonyms (if available, controlled vocabularies, such as Thesaurus of Psychological Index Terms, were used) and entered a combination of these search terms into each database. In addition, we scanned the Web using the Google Scholar search engine, we used the ancestry approach by scanning the reference lists of the relevant articles, and we applied the descendancy approach by searching for articles that had cited relevant articles using indexing sources. Finally, active researchers in the field were contacted and asked for further unpublished studies, and relevant conference programs and proceedings were examined. The literature search was completed in May 2015.

Coding Procedure

If available, we collected and coded each experiment in terms of the moderators suggested by theory or empirical evidence (see Introduction). Regarding experimenter effects, we coded experiments as blinded, if the authors stated explicitly that the experimenter was not aware of the hypotheses or condition or if the experimenter was not present during the manipulation and measurement of outcome variables. We coded experiments as not blinded, if the experimenter was present during the manipulation phase or during the measurement of outcome variables and the authors did not state that the experimenter was unaware of the hypotheses or condition. Lastly, we coded experiments as information not available (n.a.) if it was not clear from the description whether the experimenter was present during the manipulation or during the measurement of outcome variables and authors did not report whether or not the experimenter was aware of the hypotheses or condition. For descriptive purposes, we recorded the (1) year, (2) source (i.e., search strategy that produced the report) of each study, and (3) sample composition. All experiments were coded by the first author. In addition, a random sample of 27 experiments (45%) was coded by a research assistant with a bachelor's degree in psychology to obtain an estimate of interrater agreement for moderator variables and study characteristics. The average interrater agreement was $\kappa = 0.91$. Additionally, the first author coded the effect sizes extracted from each article twice with 33% of the articles to calculate intrarater agreement (Table 1). All of the diverging assessments were discussed until a consensus was reached.

Statistical Methods

Analyses concerning RQ1 and 2 were performed using the Comprehensive Meta-Analysis software (Borenstein, Hedges, Higgins, & Rothstein, 2005). Because the aim of this meta-analysis was to compare the social consequences of MSIS with a control group, and the outcome measures were mainly continuous, we calculated Hedges' *g*. Hedges' *g* is a variation of Cohen's *d* that corrects for small sample sizes (Hedges, 1980). Like Cohen's *d*, Hedges' *g* expresses the distance between the two group means in units of standard deviation. If available, the effect size (ES) was calculated by entering the group means, standard deviations, and number of participants. Otherwise, ES was calculated from the test statistic or converted from other reported ES measures. When information to calculate an ES was not included in the article, we contacted the authors. For paired-samples the correlation between the two conditions is needed to calculate an ES. When the correlation was not available we assumed that the scores in the two conditions are correlated at the level of $r = 0.5$. To pool individual effect sizes, we applied a random-effects model (DerSimonian & Laird, 1986). Whereas the fixed-effects model assumes that all studies that go into the meta-analysis come from the same population, the random-effects model assumes that studies are drawn from different populations that may have different true effect sizes (e.g., study populations that differ

Table 1. Interrater and intrarater reliability for coded variables

Variable	Measure	Interrater	Intrarater
Intentionality	κ	0.70	
Muscles involved	κ	0.85	
Familiarity with interaction partner	κ	1.00	
Gender of interaction partner	κ	0.57	
Number of interaction partners	κ	0.92	
Music	κ	0.76	
Experimenter blindedness	κ	1.00	
Manipulation check	κ	1.00	
Design	κ	1.00	
Type of MSIS	κ	1.00	
Comparison group	κ		1.00
Outcome	κ		0.96
g	ICC		0.999
se	ICC		1.00

Notes. κ = Cohen's κ; ICC = the intraclass correlation coefficient; *g* = Hedges' *g*; *se* = standard error of *g*.

in characteristics that can have an impact on effect size, such as intensity of treatment, age of participants, etc.). Consequently, under a fixed-effects model all variation in effect sizes across studies is assumed to be due to sampling error, whereas the random-effects model allows the study-level variance to be an additional source of variation. As we expected heterogeneity in effect sizes, the random-effects model was more appropriate (Hedges & Vevea, 1998). For the general analysis (RQ1), we used only one data point per experiment. For moderator analyses (RQ2), we conducted two separate meta-analyses for each class of outcome variables (attitudes vs. behavior) and again included only one data point per experiment in each of these analyses to ensure independence among data points.

Decisions concerning the selection of data points were based on the following rules. If experiments included comparisons of the experimental group with two or more control groups, we chose the group that differed from the experimental group in as few other characteristics (except synchrony) as possible to prevent biases due to confounds (Table 2). In cases in which experiments included two or more synchronous groups (e.g., synchrony established intentionally vs. incidentally), we chose the synchronous group that was expected to yield the greatest effect on prosociality. Expectations concerning the effectiveness of a manipulation were derived from prior research (e.g., intentional synchrony was preferred over incidental synchrony). Similarly, if studies included more than one control group of the same category, we chose the control group that was expected to have the greatest effect on prosociality. Again we made these predictions a priori and based on prior research. If studies reported more than one social outcome, we calculated a combined effect size by averaging across outcomes because it is the more conservative approach (Scammacca, Roberts, & Stuebing, 2014). In our network analysis, all comparisons reported in a given experiment were included; however, if experiments reported more than one comparison group of the same category, only one of these groups was selected, based on the same procedure as described above.

If studies included only the overall sample size and did not detail the assignment of participants to the experimental and control group(s), we assumed that the sample sizes were equal across groups. If the total sample size was odd, we placed the remainder in the experimental group.

To estimate the between-study variance (τ^2), the method of moments (DerSimonian & Laird, 1986) was used. A *Z*-test was performed to test the overall effect. The homogeneity of effects was assessed using the *Q* statistic and I^2. The *Q* statistic reflects the total amount of variance in the meta-analysis. A significant *Q* statistic indicates that the observed variation is different from that expected by sampling error alone. The I^2 value indexes the proportion of variance that is attributable to between-study differences. Values of I^2 range from 0% to 100% and it has been recommended to interpret 25%, 50%, and 75% as low, moderate, and high heterogeneity, respectively (Higgins, Thompson, Deeks, & Altman, 2003). Moderator analyses were conducted using a mixed effects analysis. In mixed effects analysis, a random-effects model is used to combine studies within each subgroup. A fixed-effect model is used to combine subgroups, and it yields the overall effect. The study-to-study variance (τ^2) was pooled across subgroups, because we had no reason to assume that the study-to-study variation was different for subgroups and the estimate of τ^2 is more precise when using a pooled estimate based on more studies (Borenstein, Hedges, Higgins, & Rothstein, 2009). To investigate the unique contribution of each moderator and to control for confounds, we ran a multivariate meta-regression model including all moderator variables that were shown to have a significant association with effect size using the package Metafor in R (Viechtbauer, 2010). Model fit was assessed using the proportion of the between-study variance explained by the moderator(s) (R^2_{analog}), along with a significance test of the hypothesis that the residual between-study variance equals zero. The between-study variance explained by the moderator(s) was calculated by subtracting the residual between-study variance in the model including the moderators from its value in a model without moderators. R^2_{analog}, the relative reduction in the between-study variance, was calculated by dividing the explained variance by the total variance.

Table 2. Preference strategy for selection of control group

		Comparison group is equivalent to the synchronous group in the following characteristics					
	Type of comparison	Synchrony	Coordination	Same m/s	Inter-action	Group setting	Treatment
1	Same m/s, coordinated (anti-phase)	No	Yes	Yes	Yes	Yes	Yes
2	Same m/s, not coordinated	No	No	Yes	Yes	Yes	Yes
3	Different m/s, interacting	No	No	No	Yes	Yes	Yes
4	Different m/s, not interacting	No	No	No	No	Yes	Yes
5	No group setting	No	No	No	No	No	Yes
6	No treatment	No	No	No	No	No	No

Note. m/s = movement/sensory stimulation.

To determine whether the effect of MSIS depends on the type of the comparison group (RQ3), we used the package netmeta in R software (Rücker, Schwarzer, Krahn, & König, 2015). Network meta-analysis is a generalization of pairwise meta-analysis that compares all pairs of treatments within a number of treatments for the same condition. Network analysis requires that the findings for each intervention group be sufficiently homogenous (homogeneity assumption) and that effect estimates derived from direct and indirect evidence be consistent (consistency assumption). To test whether these assumptions are met, we used the net heat plot (Krahn, Binder, & König, 2013).

Finally, we assessed the likelihood of inclusion bias using Begg and Mazumdar's rank correlation test (Begg & Mazumdar, 1994), Egger's regression test (Egger, Smith, Schneider, & Minder, 1997), Rosenthal's fail-safe *N* (Rosenthal, 1979), and Orwin's fail-safe *N* (Orwin, 1983), as well as Duval and Tweedie's trim and fill analysis (Duval & Tweedie, 2000a, 2000b).

Results

Description of the Studies

The literature search identified 42 published or unpublished articles, including 60 experiments that met our inclusion criteria (see Figure 1 for a flow diagram depicting the selection procedure, Table 3 for an overview of included studies, and Table 4 for coded moderators). The studies were either published, or studies with unpublished data were run between 1988 and 2015. The sample sizes ranged from 15 to 336, with a median of 48. The average proportion of male participants was 32% (range: 0%–100%). Most of the experiments (k = 41) used a between-subjects design, whereas 19 used a within-subjects design. The majority of experiments used a student sample (k = 21), 6 experiments recruited a mixed sample of students and nonstudents, 4 studies included only children in their samples, and for 29 experiments, this information was not available.

Social Outcomes (RQ1a)

Among the 60 independent experiments 48 assessed prosocial attitudes and 35 assessed prosocial behavior. Operationalizations of prosocial attitudes included perceived self-other merging, entitativity, unity, closeness, similarity, liking, and trust. Operationalizations of prosocial behavior were cooperation, conformity, helping behavior, and other-related attention (e.g., memory for other-related information, face recognition). Thus, corroborating the conclusion of Repp and Su (2013), the studies summarized in this meta-analysis examined positive outcomes. The only exception pertains to conformity, which, while often benefitting the in-group, can have negative consequences for individuals outside of the synchronized group or dyad.

General Effect (RQ1b)

We tested for outliers using Grubbs' test (Grubbs, 1950). Because there were no outliers, all primary effect sizes were retained for further analyses. The weighted average effect using a random-effects model was Hedges' g = 0.48, with a 95% confidence interval (95% CI) ranging from 0.39 to 0.56 (z = 11.41, p < .0001). Applying a fixed-effects model showed similar results with the 95% CI falling into the interval of the random-effects analysis. Therefore, the hypothesis that the effect of interpersonal synchrony on prosociality is null was rejected. The Q-test indicated that the 60 effect sizes display significantly greater variability than expected by chance, with I^2 indicating low to moderate heterogeneity between studies (Q = 101.11, df = 59, p = .001, I^2 = 41.65). Therefore, in the next step, we performed analyses for two types of outcome measures separately and examined potential moderators.

Analyses by Outcome (RQ1b + RQ2)

We ran two separate meta-analyses for attitudinal prosociality and behavioral prosociality. As there were no significant outliers for either class of outcomes, all of the effect sizes were retained.

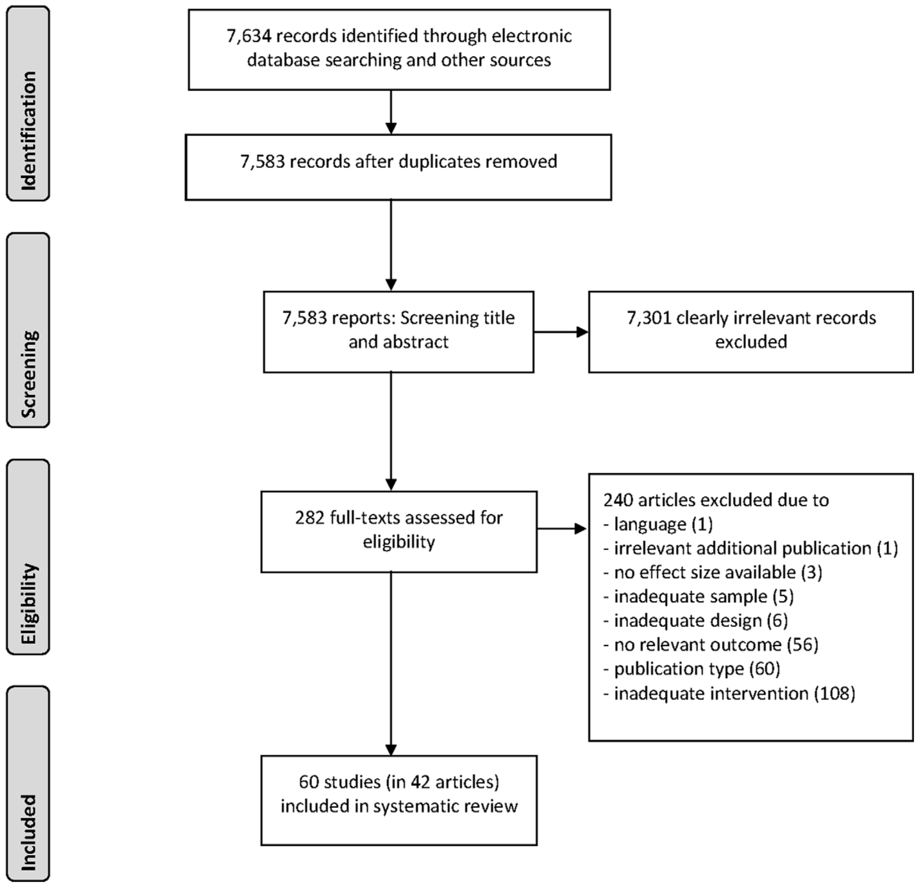

Figure 1. Study selection process.

Prosocial Attitudes

The effect of MSIS on prosocial attitudes, as investigated in 48 experiments, was highly significant (g = 0.49, 95% CI [0.40; 0.57], z = 11.37, $p < .0001$; Figure 2). The Q-test was significant (Q = 75.01, df = 47, p = .01, I^2 = 37.34), suggesting that differences in effect sizes across studies cannot be explained by sampling error alone. The I^2 value indicates low to moderate heterogeneity among studies. Moderator analyses showed that blinding of experimenter affected the effect of MSIS on prosocial attitudes. None of the other potential moderators was related to effect size (Table 5). Meta-regression revealed the effect of MSIS on prosocial attitudes to be larger by g = 0.29, 95% CI [0.10; 0.50], when experimenters were aware of the hypotheses as compared to blinded experimenters, z = 2.90, p = .004, and larger by g = 0.30, 95% CI [0.13; 0.48] when compared to studies for which no information concerning experimenter blindedness was available, z = 3.40, p = .001. The overall effect sizes of studies for which no information about experimenter blindedness was available did not differ from the overall effect size of blinded studies, z = −0.11, p = .91. Despite the presence of the moderator effect, the effect of MSIS on prosocial attitudes differed from zero for all subgroups, all $p < .001$. The proportion of between-study variance explained by including the moderator in the model was R^2_{analog} = 61.39%. The test of the hypothesis that the residual variance after including the moderator into model equals zero, was not significant, Q = 54.92, p = .15, which indicates that the variance in true effects among studies with the same predicted value (i.e., studies in the same subgroup) is due to sampling error.

Prosocial Behavior

There was a highly significant effect of MSIS on prosocial behavior as investigated in 35 independent studies (g = 0.45, 95% CI [0.30; 0.60], z = 5.79, $p < .0001$; Figure 3). The Q-test was significant (Q = 83.19, df = 34, $p < .0001$, I^2 = 59.13), which points at additional sources of variation beyond sampling error. As indicated by I^2 the heterogeneity in effect sizes among studies was moderate. In agreement with our expectations, effect sizes were affected by whether or not MSIS was established intentionally and by whether or not the experimenter was blinded (Table 5). None of the other potential moderators was associated with effect size. We ran a meta-regression that included both moderators in the model to investigate the unique contribution of each moderator when the other

Table 3. Characteristics of selected experiments examining the effects of MSIS on prosociality

	Study	*N*	Sample type	Type of synchrony	Comparison group(s)	Design	Outcomes	Hedges' *g* with 95% CI
1	Anshel & Kipper, 1988 (music sample)	48	n.a.	AM	4	b	a, coop	0.74 [0.16, 1.32]
	Anshel & Kipper, 1988 (no music sample)	48	n.a.	AM	4	b	a, coop	0.71 [0.12, 1.30]
2	Bufalari, Lenggenhager, Porciello, Serra Holmes, & Aglioti, 2014	27	n.a.	SS	2	w	a, f	0.83 [0.39, 1.27]
3	Cacioppo et al., 2014	47	M	AM	2	w	a	0.30 [0.01, 0.60]
4	Cardini, Tajadura-Jiménez, Serino, & Tsakiris, 2013	25	n.a.	SS	2	w	f	0.51 [0.10, 0.92]
5	Cirelli, Einarson, & Trainor, 2014	68	C	PM	2, 1	b	hb	0.09 [−0.49, 0.68]
6	Cirelli, Wan, & Trainor, 2014	28	C	PM	2	b	hb	0.57 [−0.20, 1.34]
7	Dong, Dai, & Wyer, 2015	85	S	AM	2	b	f, oa	0.89 [0.35, 1.43]
8	Fessler & Holbrook, 2014	96	S	AM	2	b	a, f	0.69 [0.28, 1.10]
9	Fini, Cardini, Tajadura-Jiménez, Serino, & Tsakiris, 2013	30	n.a.	SS	2	w	a,f	0.46 [0.08, 0.84]
10	Harmon-Jones, 2011 (singing sample)	52	S	AM	4	b	a	−0.16 [−0.70, 0.38]
	Harmon-Jones, 2011 (speaking sample)	53	S	AM	4	b	a, coop	0.32 [−0.21, 0.86]
11	Hove & Risen, 2009	69	S	AM	2, 5	b	a	0.73 [0.13, 1.32]
12	Kirschner & Tomasello, 2010	96	C	AM	3	b	coop, hb	0.59 [0.17, 1.02]
13	Kokal et al., 2011	18	n.a.	AM	2	b	hb	1.26 [0.29, 2.24]
14	Kurzban, 2001 (male participants)	139	S	AM	3, 6	b	coop	−0.27 [−0.80, 0.26]
	Kurzban, 2001 (female participants)	149	S	AM	3, 6	b	coop	0.17 [−0.35, 0.68]
15	Launay, Dean, & Bailes, 2013	38	M	AM	2	w	coop	1.08 [0.34, 1.82]
16	Launay, Dean, & Bailes, 2014 (sample 1)	33	S	AM	2	b	a	0.72 [0.02, 1.42]
	Launay, Dean, & Bailes, 2014 (sample 2)	41	S	AM	2	b	a	0.44 [−0.19, 1.07]
17	Lumsden, Miles, & Macrae, 2014	64	n.a.	AM	2	b	a, f	0.36 [−0.13, 0.85)
18	Macrae, Duffy, Miles, & Lawrence, 2008	30	S	AM	1, 5	b	oa	1.14 [0.47, 2.36]
19	Maister, Cardini, Zamariola, Serino, & Tsakiris, 2015	16	n.a.	SS	2	w	a, f	0.02 [−0.46, 0.51]
20	Maister, Tsiakkas, & Tsakiris, 2013	15	n.a.	SS	2	w	f, oa	0.65 [0.12, 1.19]
21	Mazzurega, 2010 (sample 1)	24	S	SS	2	w	a, f	0.70 [0.26, 1.14]
	Mazzurega, 2010 (sample 2)	25	S	SS	2	w	a, f	0.70 [0.27, 1.13]
22	Mazzurega et al., 2011	18	S	SS	2	w	a, f, con	0.28 [−0.19, 0.75]
23	Miles, Nind, Henderson, & Macrae, 2010	36	S	AM	1	b	oa	0.96 [0.28, 1.63]
24	Paladino et al., 2010	16	S	SS	2	w	a, f, con	0.60 [0.09, 1.11]
25	Porciello et al., 2014	38	n.a.	SS	2	w	f	1.00 [0.43, 1.58]
26	Rabinowitch & Knafo-Noam, 2015	148	C	AM	2, 6	b	f	0.53 [0.06, 0.99]
27	Reddish, 2012	91	S	AM	2, 4	b	f, coop	−0.01 [−0.63, 0.43]
28	Reddish, Bulbulia, & Fischer, 2013 (sample 1)	81	S	AM	2, 4	b	f, hb	0.22 [−0.31, 0.74]
	Reddish, Bulbulia, & Fischer, 2013 (sample 2)	65	n.a.	AM	3	b	a, coop, f, hb	0.08 [−0.40, 0.56]
29	Reddish, Fischer, & Bulbulia, 2013 (sample 1)	119	n.a.	AM	2, 4	b	a, coop, f	0.46 [−0.05, 0.96]
	Reddish, Fischer, & Bulbulia, 2013 (sample 2)	27	n.a.	AM	1	b	a, coop, f	0.41 [−0.36, 1.17]

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Table 3. (Continued)

	Study	*N*	Sample type	Type of synchrony	Comparison group(s)	Design	Outcomes	Hedges' *g* with 95% CI
	Reddish, Fischer, & Bulbulia, 2013 (sample 3: group goal)	42	n.a.	AM	1, 2	b	a, coop, f, oa	0.13 [−0.54, 0.81]
	Reddish, Fischer, & Bulbulia, 2013 (sample 3: individual goal)	40	n.a.	AM	1, 2	b	a, coop, f, oa	0.06 [−0.66, 0.78]
30	Rennung & Göritz, 2016	23	M	AM	2	b	a	1.35 [0.47, 2.24]
31	Schachner & Mehr, 2015 (sample 1)	27	S	AM	2	b	a, coop, f	−0.04 [−0.77, 0.70]
	Schachner & Mehr, 2015 (sample 2)	55	n.a.	AM	2	b	a, con, f	−0.11 [−0.64, 0.41]
	Schachner & Mehr, 2015 (sample 3)	45	n.a.	AM	1, 5	b	a, f	0.29 [−0.42, 1.00]
	Schachner & Mehr, 2015 (sample 4)	120	M	AM	2, 1	b	hb	−0.03 [−0.39, 0.32]
32	Sforza, Bufalari, Haggard, & Aglioti, 2010	24	n.a.	SS	2, 6	w	f	1.12 [0.62, 1.62]
33	Shaw, Czekóová, Chromec, Mareček, & Brázdil, 2013	72	S	AM	2, 1, 5	b	a, coop	0.33 [−0.32, 0.97]
34	Son, 2013	336	S	AM	2	b	f	0.23 [0.01; 0.44]
35	Tajadura-Jiménez et al., 2012 (sample 1)	256	M	SS	2	w	f	0.54 [0.41, 0.68]
	Tajadura-Jiménez et al., 2012 (sample 2)	56	M	SS	2	w	a, f	0.68 [0.39, 0.97]
36	Tajadura-Jiménez, Grehl, & Tsakiris, 2012 (sample 1)	39	n.a.	SS	2	w	f	0.39 [0.07, 0.71]
	Tajadura-Jiménez, Grehl, & Tsakiris, 2012 (sample 2)	20	n.a.	SS	2	w	f	0.72 [−0.26, 1.70]
	Tajadura-Jiménez, Grehl, & Tsakiris, 2012 (sample 3)	30	n.a.	SS	2	w	f	0.27 [−0.08, 0.63]
37	Tajadura-Jiménez, Lorusso, & Tsakiris, 2013	30	S	SS	2	w	a, f	0.32 [−0.04, 0.68]
38	Valdesolo & DeSteno, 2011	69	n.a.	AM	2	b	a, f, hb	0.73 [0.25, 1.23]
39	Valdesolo, Ouyang, & DeSteno, 2010	76	n.a.	AM	2	b	coop, f	0.73 [0.20, 1.26]
40	Wiltermuth, 2012a (sample 1)	70	S	AM	2, 1	b	f	0.52 [−0.06, 1.09]
	Wiltermuth, 2012a (sample 2)	89	n.a.	AM	1, 2, 5	b	con	0.62 [0.01, 1.24]
41	Wiltermuth, 2012b	156	S	AM	2, 4	b	con, f	0.66 [0.14, 1.18]
42	Wiltermuth & Heath, 2009 (sample 1)	30	n.a.	AM	2	b	a, coop, f	1.14 [0.39, 1.89]
	Wiltermuth & Heath, 2009 (sample 2)	96	n.a.	AM	2, 4	b	coop, f	0.83 [0.33, 1.34]
	Wiltermuth & Heath, 2009 (sample 3)	105	n.a.	AM	2, 4	b	a, coop, f	0.53 [0.06, 1.00]

Notes. c = Children; s = students; M = Mixed (not only students); n.a. = information not available; AM = active movement; PM = passive movement; SS = sensory stimulation; 1 = same m/s; coordinated (anti-phase); 2 = same m/s, not coordinated; 3 = different m/s, interacting; 4 = different m/s; not interacting; 5 = no group setting; 6 = no treatment; b = between-subjects design; w = within-subjects design; a = affiliation; con = conformity; coop = cooperation; f = fusion; hb = helping behavior; oa = other-related attention.

Table 4. Coded moderators

	Study	Intentional	Muscle involvement	Familiarity interaction partner	Sex of interaction partner	Number of interaction partners	Music	Experimenter	Manipulation check
1	Anshel & Kipper, 1988 (music sample)	Yes	Small	Unknown	Same	> 1	Yes	Not blind	No
	Anshel & Kipper, 1988 (no music sample)	Yes	Small	Unknown	Same	> 1	Yes	Not blind	No
2	Bufalari et al., 2014	No	Small	n.a.	Same	1	No	Not blind	No
3	Cacioppo, 2014	No	Small	n.a.	Not same	1	No	Blind	Yes
4	Cardini et al., 2013	No	Small	Unknown	Same	1	No	Not blind	No
5	Cirelli, Einarson, & Trainor, 2014	No	Large	n.a.	Not same	1	Yes	Not blind	Yes
6	Cirelli, Wan, & Trainor, 2014	No	Large	Unknown	Not same	1	Yes	Not blind	Yes
7	Dong, Dai, & Wyer, 2015	Yes	Large	n.a.	Not same	> 1	No	Not blind	Yes
8	Fessler & Holbrook, 2014	Yes	Large	n.a.	Same	1	No	Not blind	No
9	Fini et al., 2013	No	Small	n.a.	Same	1	No	n.a.	No
10	Harmon-Jones, 2011 (singing sample)	No	Small	n.a.	Same	> 1	Yes	Blind	No
	Harmon-Jones, 2011 (speaking sample)	No	Small	n.a.	Same	> 1	Yes	Blind	No
11	Hove & Risen, 2009	No	Small	n.a.	Not same	1	Yes	Blind	Yes
12	Kirschner & Tomasello, 2010	Yes	Large	Known	Same	1	Yes	Not blind	No
13	Kokal et al., 2011	No	Large	n.a.	Same	1	Yes	Blind	Yes
14	Kurzban, 2001 (male participants)	Yes	Small	n.a.	Same	> 1	Yes	Blind	No
	Kurzban, 2001 (female participants)	Yes	Small	n.a.	Same	> 1	Yes	Blind	no
15	Launay, Dean, & Bailes, 2013	Yes	Small	Unknown	n.a.	1	Yes	n.a.	Yes
16	Launay, Dean, & Bailes, 2014 (sample 1)	Yes	Small	unknown	n.a.	1	Yes	Blind	Yes
	Launay, Dean, & Bailes, 2014 (sample 2)	No	Small	Unknown	n.a.	1	Yes	Blind	Yes
17	Lumsden, Miles, & Macrae, 2014	Yes	Large	n.a.	Same	1	No	n.a.	Yes
18	Macrae et al., 2008	No	Small	n.a.	Not same	1	Yes	Not blind	No
19	Maister et al., 2015	No	Small	Unknown	Same	1	No	Not blind	No
20	Maister, Tsiakkas, & Tsakiris, 2013	No	Small	n.a.	Same	1	No	Not blind	No
21	Mazzurega, 2010 (sample 1)	No	Small	Unknown	Same	1	No	Not blind	No
	Mazzurega, 2010 (sample 2)	No	Small	Unknown	Same	1	No	Not blind	No
22	Mazzurega et al., 2011	No	Small	Unknown	n.a.	1	No	Not blind	Yes
23	Miles et al., 2010	No	Large	n.a.	Same	1	Yes	Not blind	Yes
24	Paladino et al., 2010	No	Small	Unknown	Same	1	No	Not blind	No
25	Porciello et al., 2014	No	Small	Known	Same	1	No	Not blind	No
26	Rabinowitch & Knafo-Noam, 2015	No	Small	Unknown	Same	1	No	Not blind	Yes
27	Reddish, 2012	No	Small	Mixed	Not same	> 1	Yes	n.a.	No
28	Reddish, Bulbulia, & Fischer, 2013 (sample 1)	No	Large	Mixed	Not same	> 1	Yes	n.a.	No
	Reddish, Bulbulia, & Fischer, 2013 (sample 2)	Yes	Large	Mixed	Not same	> 1	No	n.a.	No
29	Reddish, Fischer, & Bulbulia, 2013 (sample 1)	Yes	Large	Mixed	Not same	> 1	Yes	n.a.	No
	Reddish, Fischer, & Bulbulia, 2013 (sample 2)	Yes	Small	Mixed	Not same	> 1	Yes	n.a.	Yes
	Reddish, Fischer, & Bulbulia, 2013 (sample 3: group goal)	Yes	Large	Mixed	Not same	> 1	No	n.a.	Yes
	Reddish, Fischer, & Bulbulia, 2013 (sample 3: individual goal)	No	Large	Mixed	Not same	> 1	No	n.a.	Yes
30	Rennung & Göritz, 2016	No	Large	Known	Not same	> 1	Yes	Not blind	No
31	Schachner & Mehr, 2015 (sample 1)	No	Small	n.a.	Not same	> 1	Yes	Blind	No
	Schachner & Mehr, 2015 (sample 2)	No	Small	n.a.	Not same	> 1	Yes	Blind	No
	Schachner & Mehr, 2015 (sample 3)	Yes	Large	n.a.	Not same	1	yes	Blind	No
	Schachner & Mehr, 2015 (sample 4)	No	Small	Unknown	Not same	1	yes	Blind	No
32	Sforza et al., 2010	No	Small	Known	Same	1	No	Not blind	No

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Table 4. (Continued)

	Study	Intentional	Muscle involvement	Familiarity interaction partner	Sex of interaction partner	Number of interaction partners	Music	Experimenter	Manipulation check
33	Shaw et al., 2013	No	Small	Unknown	Not same	1	Yes	Blind	Yes
34	Son, 2013	Yes	Large	Known	Same	1	Yes	n.a.	No
35	Tajadura-Jiménez, Longo, Coleman, & Tsakiris, 2012 (sample 1)	No	Small	Unknown	Same	1	No	Not blind	No
	Tajadura-Jiménez, Longo, Coleman, & Tsakiris, 2012 (sample 2)	No	Small	Unknown	Same	1	No	Not blind	No
36	Tajadura-Jiménez, Grehl, & Tsakiris, 2012 (sample 1)	No	Small	Unknown	Same	1	No	Not blind	No
	Tajadura-Jiménez, Grehl, & Tsakiris, 2012 (sample 2)	No	Small	Unknown	Same	1	No	Not blind	No
	Tajadura-Jiménez, Grehl, & Tsakiris, 2012 (sample 3)	No	Small	Unknown	Same	1	No	Not blind	No
37	Tajadura-Jiménez, Lorusso, & Tsakiris, 2013	Yes	Small	Unknown	Same	1	no	Not blind	No
38	Valdesolo & DeSteno, 2011	No	Small	n.a.	n.a.	1	Yes	Not blind	No
39	Valdesolo et al., 2010	Yes	Large	Mixed	n.a.	1	No	n.a.	No
40	Wiltermuth, 2012a (sample 1)	Yes	Large	n.a.	Not same	1	No	Blind	Yes
	Wiltermuth, 2012a (sample 2)	Yes	Large	n.a.	Not same	1	No	Not blind	Yes
41	Wiltermuth, 2012b	No	Large	n.a.	n.a.	> 1	Yes	Not blind	Yes
42	Wiltermuth & Heath, 2009 (sample 1)	Yes	Large	n.a.	Not same	> 1	No	Not blind	No
	Wiltermuth & Heath, 2009 (sample 2)	No	Small	n.a.	Not same	> 1	Yes	Not blind	No
	Wiltermuth & Heath, 2009 (sample 3)	No	Small	n.a.	Not same	> 1	Yes	Not blind	No

Notes. n.a. = Information not available; mixed = sample included both participants that knew each other before the experiment and participants that did not know each other before the experiment.

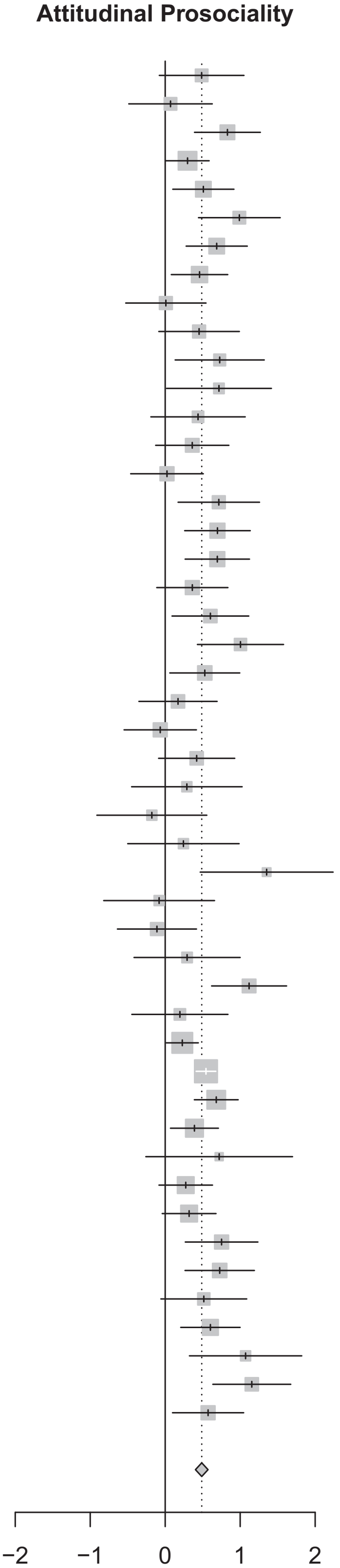
moderator was held constant (i.e., partialled out). For both moderators, the test of between-group differences was significant when holding the other moderator constant, $Q = 5.21$, $p = .02$ (intentionality) and $Q = 12.16$, $p = .002$ (blinding of experimenter). This indicates, that the two moderators explain a unique proportion of the variance between groups. Results indicated that effect sizes with regard to prosocial behavior on average were higher by $g = 0.31$, 95% CI [0.04; 0.58], when MSIS was established intentionally as compared to incidentally, $z = 2.28$, $p = .02$. Furthermore, if the experimenter was not blind to hypotheses/condition, g was larger by 0.52, 95% CI [0.22; 0.83], $z = 3.41$, $p = .001$, compared to studies with blinded experimenters, whereas there was no difference as compared to studies for which information on experimenter blindedness was not available, $t = 1.74$, $p = .08$. Studies with blinded experimenters and studies for which no information concerning experimenter blindedness was available did not differ significantly, $z = 1.29$, $p = .20$. For experiments with a non-blinded experimenter as well as experiments for which no information was available, the effect of MSIS on prosocial behavior was significantly different from zero, both $p < .002$. For the subset of nine experiments with a blinded experimenter, the hypothesis that g equals null could not be rejected, $z = 0.56$, $p = .58$, providing preliminary evidence that a methodological artifact may account for the reported behavioral effects of MSIS.

The proportion of between-study variance explained by including the two moderators in the model was $R^2_{analog} = 54.70\%$. The test of the hypothesis that the residual variance after including the moderators into model equals zero, was significant, $Q = 51.03$, $p = .01$, indicated that these two moderators did not explain all of the variance, but that there was variance in true effects among studies with the same predicted value that was unlikely due to sampling error alone.

Lastly, we added the two moderators' interaction term to the model to explore whether the effect of intentionality differs as a function of experimenter blindedness and vice versa. Results suggest that this was not the case, $Q = 3.84$, $p = .15$.

Type of Comparison

For general prosociality (combining attitudes and behavior), 59 of the 60 experiments provided data for network meta-analysis. Regarding the type of comparison, six control groups were compared with the synchronous experimental group: same m/s coordinated (anti-phase), same m/s not coordinated, different m/s interacting (e.g., watching a movie, listening to music), different m/s not interacting (e.g., solving a puzzle together), no group setting, and no treatment. The 59 experiments included a total of 98



Study	Hedges' g	95%−CI	W(random)
Anshel & Kipper 1988 (music sample)	0.49	[−0.08; 1.05]	1.7%
Anshel & Kipper 1988 (no music sample)	0.07	[−0.49; 0.63]	1.7%
Bufalari et al. 2014	0.83	[0.39; 1.27]	2.3%
Cacioppo et al. 2014	0.30	[0.01; 0.59]	3.7%
Cardini et al. 2013	0.51	[0.10; 0.92]	2.6%
Dong, Dai, & Wyer 2015	0.99	[0.44; 1.53]	1.7%
Fessler & Holbrook 2014	0.69	[0.28; 1.09]	2.6%
Fini et al. 2013	0.46	[0.08; 0.83]	2.8%
Harmon−Jones 2011 (singing sample)	0.01	[−0.53; 0.54]	1.8%
Harmon−Jones 2011 (speaking sample)	0.45	[−0.09; 0.99]	1.8%
Hove & Risen 2009	0.73	[0.13; 1.32]	1.5%
Launay, Dean & Bailes 2014 (sample 1)	0.72	[0.02; 1.42]	1.2%
Launay, Dean & Bailes 2014 (sample 2)	0.44	[−0.19; 1.07]	1.4%
Lumsdon, Miles, & Macrae 2014	0.36	[−0.13; 0.85]	2.0%
Maister et al. 2015	0.02	[−0.46; 0.51]	2.1%
Maister, Tsiakkas, & Tsakiris 2013	0.72	[0.17; 1.26]	1.8%
Mazzurega 2010 (sample 1)	0.70	[0.26; 1.13]	2.4%
Mazzurega 2010 (sample 2)	0.70	[0.27; 1.12]	2.4%
Mazzurega et al. 2011	0.36	[−0.11; 0.84]	2.1%
Paladino et al. 2010	0.60	[0.09; 1.11]	1.9%
Porciello et al. 2014	1.00	[0.43; 1.58]	1.6%
Rabinowitch & Knafo−Noam 2015	0.53	[0.06; 0.99]	2.2%
Reddish, Bulbulia & Fischer 2013 (sample 1)	0.17	[−0.35; 0.69]	1.9%
Reddish, Bulbulia & Fischer 2013 (sample 2)	−0.07	[−0.55; 0.42]	2.1%
Reddish, Fischer, & Bulbulia 2013 (sample 1)	0.42	[−0.09; 0.93]	1.9%
Reddish, Fischer, & Bulbulia 2013 (sample 2)	0.29	[−0.45; 1.03]	1.1%
Reddish, Fischer, & Bulbulia 2013 (sample 3: group goal)	−0.18	[−0.91; 0.56]	1.1%
Reddish, Fischer, & Bulbulia 2013 (sample 3: individual goal)	0.24	[−0.50; 0.99]	1.1%
Rennung & Göritz 2016	1.35	[0.47; 2.24]	0.8%
Schachner & Mehr 2015 (sample 1)	−0.08	[−0.82; 0.66]	1.1%
Schachner & Mehr 2015 (sample 2)	−0.11	[−0.64; 0.42]	1.8%
Schachner & Mehr 2015 (sample 3)	0.29	[−0.42; 1.00]	1.2%
Sforza et al. 2010	1.12	[0.62; 1.62]	2.0%
Shaw et al. 2013	0.20	[−0.44; 0.84]	1.4%
Son 2013	0.23	[0.01; 0.44]	4.6%
Tajadura−Jiménez, Longo, Coleman, & Tsakiris 2012 (sample 1)	0.54	[0.41; 0.68]	5.6%
Tajadura−Jiménez, Longo, Coleman, & Tsakiris 2012 (sample 2)	0.68	[0.39; 0.97]	3.7%
Tajadura−Jiménez, Grehl, & Tsakiris 2012 (sample 1)	0.39	[0.07; 0.71]	3.4%
Tajadura−Jiménez, Grehl, & Tsakiris 2012 (sample 2)	0.72	[−0.26; 1.70]	0.7%
Tajadura−Jiménez, Grehl, & Tsakiris 2012 (sample 3)	0.27	[−0.08; 0.63]	3.0%
Tajadura−Jiménez, Lorusso, & Tsakiris 2013	0.32	[−0.04; 0.68]	3.0%
Valdesolo & DeSteno 2011	0.75	[0.27; 1.24]	2.1%
Valdesolo, Ouyang, & DeSteno 2010	0.73	[0.26; 1.19]	2.2%
Wiltermuth 2012a (sample 1)	0.52	[−0.06; 1.09]	1.6%
Wiltermuth 2012b	0.60	[0.21; 1.00]	2.7%
Wiltermuth & Heath 2009 (sample 1)	1.07	[0.32; 1.82]	1.1%
Wiltermuth & Heath 2009 (sample 2)	1.16	[0.64; 1.67]	1.9%
Wiltermuth & Heath 2009 (sample 3)	0.57	[0.10; 1.05]	2.1%
Random effects model	**0.49**	**[0.40; 0.57]**	**100%**

Test for overall effect: p<0.0001

Figure 2. Individual and overall effect sizes of the impact of MSIS on attitudinal prosociality.

Table 5. Moderator analyses

	Prosocial attitudes	Prosocial behavior
Intentional vs. incidental	g = 0.41 [0.26; 0.56], k = 16 vs. g = 0.52 [0.42; 0.62], k = 32; Q = 1.35, p = .25	g = 0.64 [0.41; 0.87], k = 14 vs. g = 0.31 [0.13; 0.50], k = 21; Q = 4.72, p = .03
Large vs. small muscle involvement	g = 0.45 [0.30; 0.61], k = 15 vs. g = 0.50 [0.40; 0.60], k = 33; Q = 0.24, p = .62	g = 0.58 [0.35; 0.81], k = 15 vs. g = 0.35 [0.16; 0.54], k = 20; Q = 2.24, p = .13
Known vs. unknown interaction partner	g = 0.66, [0.38; 0.93], k = 4 vs. g = 0.46 [0.34; 0.59], k = 18; Q = 1.54, p = .22	n.a.
Different-sex vs. same-sex interaction partner	g = 0.42 [0.26; 0.582], k = 18 vs. g = 0.50 [0.39; 0.62], k = 24; Q = 0.71, p = .40	g = 0.39 [0.19; 0.59], k = 19 vs. g = 0.50 [0.24; 0.76], k = 11; Q = 0.42, p = .52
Group vs. dyad	g = 0.40 [0.25; 0.56], k = 18 vs. g = 0.52 [0.42; 0.62], k = 30; Q = 1.63, p = .20	g = 0.38 [0.18; 0.58], k = 20 vs. g = 0.54 [0.30; 0.78], k = 15; Q = 1.01, p = .31
No music vs. music	g = 0.53 [0.42; 0.63], k = 28 vs. g = 0.42 [0.28; 0.56], k = 20; Q = 1.58, p = .21	g = 0.46 [0.17; 0.74], k = 10 vs. g = 0.45 [0.26; 0.63], k = 25; Q = 0.004, p = .95
Experimenter blinded vs. not blinded vs. unknown	g = 0.31 [0.13; 0.48], k = 11 vs. g = 0.60 [0.51; 0.69], k = 27 vs. g = 0.29 [0.13; 0.45], k = 10; Q = 15.60, p < .001	g = 0.07 [−0.19; 0.33], k = 9 vs. g = 0.63 [0.44; 0.82], k = 17 vs. g = 0.44 [0.17; 0.70], k = 9; Q = 11.62, p = .003
No manipulation check vs. manipulation check	g = 0.50 [0.40; 0.60], k = 34 vs. g = 0.45 [0.28; 0.62], k = 14; Q = 0.26, p = .61	g = 0.42 [0.23; 0.61], k = 22 vs. g = 0.50 [0.24; 0.77], k = 13; Q = 0.26, p = .61
Between vs. within-subjects design	g = 0.44 [0.33; 0.56], k = 30 vs. g = 0.54 [0.41; 0.66], k = 18; Q = 1.13, p = .29	g = 0.45 [0.29; 0.62], k = 31 vs. g = 0.41 [−0.03; 0.85], k = 4; Q = 0.03, p = .86
Movement vs. sensory stimulation*	g = 0.43 [0.32; 0.55], k = 31 vs. g = 0.56 [0.43; 0.68], k = 17; Q = 2.01, p = .16	g = 0.47 [0.31; 0.63], k = 32 vs. g = 0.25 [−0.24; 0.74], k = 3; Q = 0.69, p = .41

Notes. We included moderators in the analyses that divided experiments into subgroups of at least three experiments. *We collapsed active and passive movement into "movement" due to the small number of experiments in the passive movement subgroup (k = 2). n.a. = The number of studies in one subgroup was below the cutoff of k = 3.

comparisons (see Table 5 and Figure 4). We did not find any evidence of substantial between-design heterogeneity (Q = 13.83, df = 13, p = .39). The net heat plot showed only slight spots of inconsistency, and these spots were not attributable to a single design. Therefore, we concluded that the requirements to perform a network analysis were met. The forest plot shows that synchrony increases prosociality compared with all types of control groups, as indicated by the CIs excluding zero, except for different m/s interacting (Figure 5).

Inclusion Bias

Regarding attitudinal prosociality, the rank correlation test was not significant (Kendall's τ = 0.02, $p_{\text{1-tailed}}$ = .43), nor was the linear regression test (b = 0.13, t = 0.29, $p_{\text{1-tailed}}$ = .39). Regarding behavioral prosociality, the rank correlation test was significant (Kendall's τ = 0.30, $p_{\text{1-tailed}}$ = .01), as was the more powerful linear regression test (b = 3.73, t = 3.30, $p_{\text{1-tailed}}$ = .001). The fail-safe N was 697, exceeding Rosenthal's suggestion of $5k + 10$. Orwin's fail-safe N analysis indicated that it would require 104 additional studies yielding a g of zero to reduce the reported effect to less than the point of practical importance (g < 0.1). Duval and Tweedie's trim and fill analysis revealed that the adjusted point estimate suggests a somewhat smaller effect of MSIS on behavioral prosociality (g = 0.31, 95% CI [0.15; 0.47] instead of g = 0.45, 95% CI [0.30; 0.60]).

Discussion

This meta-analysis was the first to investigate the effect of MSIS on prosociality. MSIS significantly increases both attitudinal and behavioral prosociality, yielding a medium effect with either outcome. Regarding prosocial attitudes, effect sizes were impacted by whether or not the experimenter was blinded. In a similar vein, the effect of MSIS on prosocial behavior differed as a function of experimenter

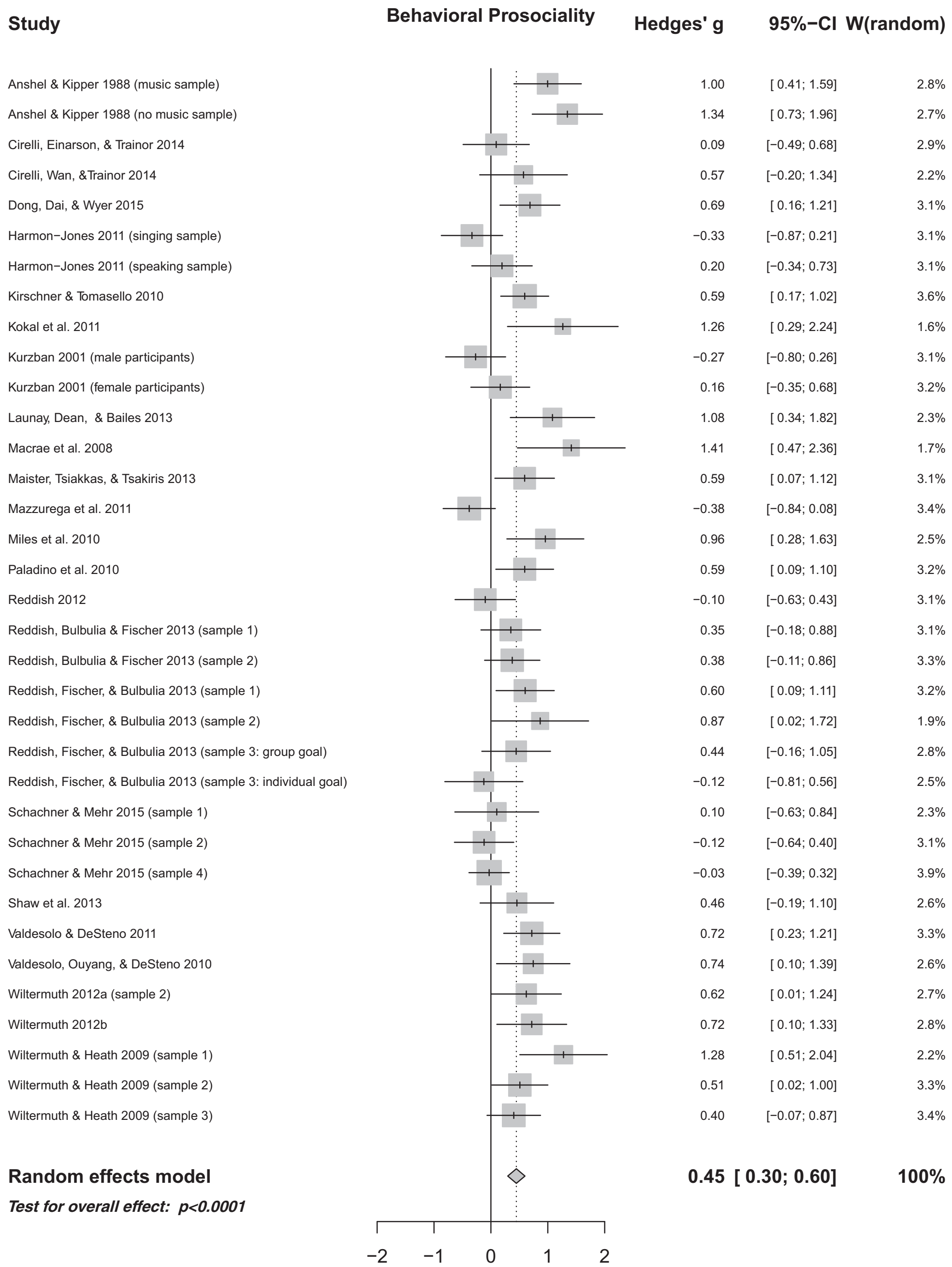


Figure 3. Individual and overall effect sizes of the impact of MSIS on behavioral prosociality.

blindedness. In addition, intentionality was identified as a moderator for effects of MSIS on prosocial behavior. For neither prosocial attitudes nor behavior did we find evidence that any of the other potential moderators influenced the effect of MSIS.

We first discuss the results regarding attitudinal prosociality: The effect of MSIS was smaller if the experimenter was blinded. Stated differently, the experimenter's knowledge about the hypotheses and/or conditions seemed to have implicitly reinforced participants' inclinations to report or actually experience attitudinal prosociality following synchronous manipulation and/or to reduce participants' reported or actual attitudinal prosociality after the control treatment. This result is potentially worrisome because it suggests that the effect of MSIS may in part be caused by a methodological artifact. However, while the awareness of the experimenter concerning the hypotheses may have increased the effect, there was a significant effect

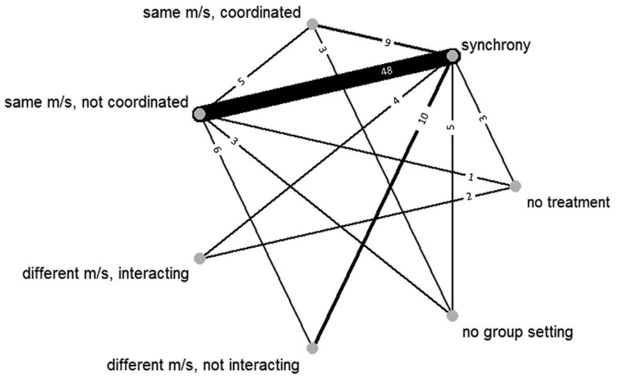


Figure 4. Network of available comparisons. The thickness of lines and numbers illustrate the number of experiments investigating the comparison.

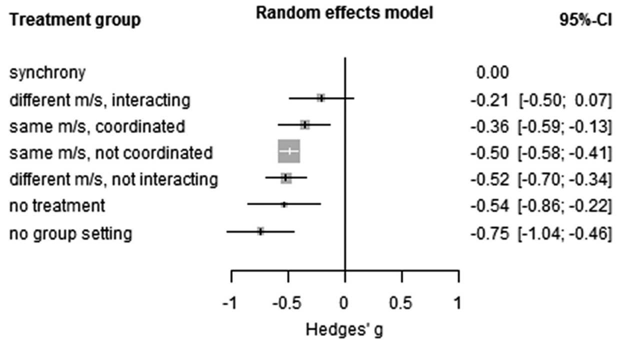


Figure 5. Estimates from network meta-analysis for different comparison groups compared to the synchronous group.

of MSIS for all subgroups and this effect was still in the moderate range (g = 0.30) when the experimenter was blinded. This suggests the existence of a genuine effect of MSIS on attitudinal prosociality. Interestingly, we did not find any difference in effect sizes between studies coded as blinded and studies coded as n.a., while n.a. studies differed significantly from non-blinded studies. This indicates that the subset of studies for which no information was available was more similar to blinded studies than to non-blinded studies. Remember that studies were coded as n.a. if the authors did not report whether or not the experimenter was aware of hypotheses and if it was not clear from the description of procedures whether or not the experimenter was present during the manipulation or during the measurement. As we consider it unlikely that authors fail to report that they applied blinding, this finding suggests that there was little interaction between experimenter and participants in experiments coded as n.a., comparably to experiments coded as blinded. However, we were limited by the detail of information provided in the studies. Most reports did not include information concerning the exact amount of interaction that took place between the experimenter and the participants, rendering it difficult to gauge the extent to which the experimenter's knowledge of the hypothesis could have biased participants' reactions. Therefore, we call on future researchers to investigate directly the influence of experimenter effects to increase our understanding of this potential source of bias.

In contrast to our expectation, the effect of MSIS on attitudinal prosociality was not weaker when MSIS was established incidentally as opposed to intentionally. Conceivably, intentionality is not prime for attitudinal prosociality to evolve, because attitudinal prosociality is mainly affected by the extent of self-other blurring and not so much by perceptions concerning the group's or dyad's cooperative ability (which was hypothesized to cause the beneficial effect of intentionality). Alternatively, the absence of this moderating effect may be explained by intentionality eliciting two competing processes: On the one hand, prior research found that intentionality heightens attention toward the other, which in turn increases a sense of unity (Reddish, Fischer, & Bulbulia, 2013). On the other hand, synchrony between participants is probably achieved more easily and therefore more precise, resulting in heightened blurring of self-other boundaries, when participants align their movements with an external beat or experience synchronous sensory stimulation rather than intentionally synchronize themselves with an interaction partner. Hence, these two competing processes may have balanced each other out with the result that the expected superiority of intentional synchrony over incidental synchrony did not materialize.

Now, we discuss MSIS' effects on prosocial behavior: analyses identified two moderators, experimenter blindedness and intentionality. The effect of MSIS decreased, even to insignificance, if the experimenter was blinded to the hypothesis. Because behavioral prosociality frequently included an interaction with the experimenter (e.g., the dropping pencils task), it is conceivable that subtleties in the experimenter's behavior influenced the outcome. Effect sizes for experiments coded as n.a. fell between those of blinded and non-blinded experiments, suggesting that this category included both experiments that were actually blinded and experiments that were not blinded. Although our results provide preliminary evidence that a methodological artifact may account for the reported behavioral effects of MSIS, we caution that more research is needed to consolidate this finding, because our conclusion is based on only nine studies that were run by blinded experimenters. Furthermore, as we investigated several moderators and compared several subgroups the likelihood of a Type I error increased. Therefore, we encourage future researchers to further investigate this source of bias.

In agreement with our expectation, the effect was stronger if MSIS was established intentionally rather than incidentally. This finding corroborates Reddish and colleagues' *reinforcement of cooperation model* (Reddish, Fischer, & Bulbulia, 2013), which purports that intentionality fosters the link between synchrony and cooperation because intentionality increases the informative content of interpersonal synchrony regarding the extent of cooperativity of a group.

For both attitudinal and behavioral prosociality we did not find evidence for a moderating effect of music. This is counter to the *muscular bonding hypothesis* (McNeill, 1997), however, it dovetails with prior experimental research (Wiltermuth & Heath, 2009). Similarly, in contrast to the *vocal grooming hypothesis*, the effect of MSIS on behavioral prosociality was not affected by whether or not music had accompanied the synchrony, paralleling the results of prior experimental research (Harmon-Jones, 2011). Presumably, synchrony that entailed entrainment to music or that had participants produce music themselves distracted some of participants' attention away from their interaction partners, or, the inclusion of music may have made it more difficult for participants to synchronize, thus offsetting a potentially stronger effect if music accompanies the synchrony.

Furthermore, for both attitudes and behaviors, the effect of MSIS on prosociality was not affected by the number of interaction partners, sex of interaction partner, or the participants' familiarity with their interaction partner(s). Likewise, we did not find any differences between MSIS treatments that entailed active movement compared to passive movement and compared to sensory stimulation. This finding suggests that the effect of MSIS is comparable in different social settings and for different kinds of treatments. This speaks to the robustness of the effect of MSIS and corroborates our decision to include these diverse operationalizations of MSIS in our meta-analysis.

Regarding the question of whether the effect of MSIS depends on the type of comparison group, network analysis suggests that MSIS is superior to all types of comparison groups, except for different m/s interacting. Different m/s interacting pertains to all control groups that entailed a group task involving interaction among participants, such as solving a puzzle together or communicating. In practice, this means that MSIS does increase prosociality, but it is not generally superior to interventions that include some type of interaction among participants. However, there were only four head-to-head comparisons of MSIS with different m/s interacting available, and the types of manipulations used in the primary studies were diverse. Therefore, a more detailed analysis is needed to derive recommendations concerning the comparison of MSIS with other types of interaction. For example, instead of performing experiments that compare MSIS to an established reference group, such as same m/s not coordinated, future research may compare MSIS with different types of control groups, including interaction.

Limitations and Further Research

Limitations pertain to, in this meta-analysis, almost all of the located experiments being conducted in laboratories (except Rennung & Göritz, 2016) and most of the experiments relying on student samples. Therefore, based on the current data, we cannot generalize the results to field settings and nonstudent samples. It would be desirable to see more studies conducted in a natural(istic) environment, as well as studies of nonstudent adults, as well as children. In a similar vein, the current meta-analysis has examined only two types of interpersonal synchrony: motor movement and sensory stimulation. Evidence has suggested that low-level processes, such as affective synchrony (Páez et al., 2015) and, relatedly, shared attention (Rennung & Göritz, 2015; Wolf, Launay, & Dunbar, 2015) facilitate prosociality. There is good reason to believe that shared attention underlies the effects of MSIS (Wolf et al., 2015), and we hope that future research will increase our understanding of this mechanism. A similar limitation pertains to the outcome of MSIS, which in this meta-analysis was confined to prosociality targeted at the synchronous interaction partner(s). Preliminary evidence has suggested that prosociality extends to individuals and groups beyond the synchronized group (Reddish, Bulbulia, & Fischer, 2013); however, this finding was not replicated in an infant sample (Cirelli, Wan, & Trainor, 2014). Therefore, more research is needed to answer the question of whether the effect of MSIS on prosociality is limited to co-performers. Moreover, MSIS not only affects prosociality but also entails positive effects for the individual, such as elevated pain tolerance (Cohen, Ejsmond-Frey, Knight, & Dunbar, 2010; Sullivan & Rickers, 2013; Sullivan, Rickers, & Gammage, 2014), increased self-esteem (Lumsden et al., 2014), and decreased work-related stress (Rennung & Göritz, 2016). Because these outcomes are beyond the scope of this meta-analysis, we hope that future meta-analyses will address the effects of MSIS in these domains.

Furthermore, only a few experiments recruited participants who knew each other well before the experiment (k = 4 and k = 1 for attitudinal and behavioral prosociality, respectively) and therefore this meta-analysis cannot answer the question of whether familiarity moderates the effect of MSIS. Similarly, to date no primary study has directly contrasted the effect of MSIS in participants who knew each other well before the study with participants who synchronized with strangers. Consequently, more research is needed to answer the questions of whether and to what extent prior social bonds impact the effect of synchrony on prosociality.

The meta-analysis at hand allows for sufficiently firm conclusions about the superiority of MSIS over same m/s not coordinated with regard to prosociality. However, due to the small number of experiments, the available evidence can provide only a rough estimate of how MSIS compares with types of control groups other than same m/s not coordinated. Therefore, our findings concerning comparisons of MSIS with other types of control groups should be interpreted with caution, unless confirmed by further research.

Regarding inclusion bias, as to the effect of MSIS on attitudinal prosociality, there is no hint that inclusion bias has occurred. With regard to behavioral prosociality, there is mild evidence of inclusion bias, and the true effect of MSIS is likely somewhat less than estimated in this meta-analysis.

Practical Implications

This meta-analysis shows that MSIS increases general prosociality at $g = 0.48$. In practice, this finding indicates that the average participant in the MSIS group scores higher in prosociality than approximately 68% of the participants in the control group (Cooper, 2010). Consequently, the results of this meta-analysis yield important insights for various domains. The powerful bonding mechanism of MSIS could be used in the educational system to foster the development of prosocial behavior. Corporations may employ MSIS interventions, such as joint calisthenics, in their team building programs. In psychotherapy, MSIS may be a promising supplement in the treatment of disorders that involve impaired social functioning. When introducing MSIS as an intervention to boost prosocial behavior rather than mere attitude, it seems advisable to have participants establish it intentionally.

However, based on the results of the current meta-analysis, we caution against rashly implementing MSIS interventions. Further research is needed to ensure that the behavioral effect of MSIS is genuine and cannot be reduced to an experimenter effect. Owing to this summary of primary studies, this potential bias was detected, which is difficult to detect with single studies. Because this meta-analysis found that the effect of MSIS is, at least in part, explained by experimenter effects, it is advisable to investigate MSIS further before initiating cost-intensive interventions. Whereas the effect of MSIS on attitudinal prosociality was weaker but significant in experiments that were run by blinded experimenters, the effect on behavioral prosociality was no longer statistically significant in experiments that were run by unblinded experimenters. In summary, MSIS seems to be a promising tool to foster prosociality in various domains, but at this early stage, reliable recommendations concerning the implementation of MSIS in practice that are targeted toward behavioral prosociality are premature.

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