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Cooperation, Fast and Slow: Meta-Analytic Evidence for a Theory of Social Heuristics and Self-Interested Deliberation

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Abstract
Does cooperating require the inhibition of selfish urges? Or does "rational" self-interest constrain cooperative impulses? I investigated the role of intuition and deliberation in cooperation by meta-analyzing 67 studies in which cognitive-processing manipulations were applied to economic cooperation games (total N = 17,647; no indication of publication bias using Egger's test, Begg's test, or p-curve). My meta-analysis was guided by the social heuristics hypothesis, which proposes that intuition favors behavior that typically maximizes payoffs, whereas deliberation favors behavior that maximizes one's payoff in the current situation. Therefore, this theory predicts that deliberation will undermine *pure* cooperation (i.e., cooperation in settings where there are few future consequences for one's actions, such that cooperating is not in one's self-interest) but not *strategic* cooperation (i.e., cooperation in settings where cooperating can maximize one's payoff). As predicted, the meta-analysis revealed 17.3% more pure cooperation when intuition was promoted over deliberation, but no significant difference in strategic cooperation between more intuitive and more deliberative conditions.

Keywords
prosociality, decision making, heuristics, social cognition, moral psychology

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Cooperation, in which people pay costs (e.g., time, effort, money) to benefit others, is a central feature of human social interaction. The collective benefits of cooperation are threatened, however, by personal incentives to act selfishly. A long tradition of research across the social and natural sciences has been aimed at understanding what motivates people to cooperate, and how cooperation can be promoted.

In recent years, there has been considerable interest in exploring cooperation using dual-process models of decision making (for a review, see Zaki & Mitchell, 2013). Dual-process models conceptualize decisions as resulting from the competition between (a) relatively intuitive, automatic, fast, effortless, and often emotional processes and (b) relatively deliberative, controlled, slow, effortful, and rational processes (Gilovich, Griffin, & Kahneman, 2002; Sloman, 1996). Applying this dual-process lens to cooperation, some researchers have argued that intuition favors cooperation and deliberation leads to selfishness (Rand, Greene, & Nowak, 2012; Rubinstein, 2007), whereas others have contended that deliberation is needed to overrule selfish impulses (Achtziger, Alós-Ferrer, & Wagner, 2011; Lohse, in press). Still others have maintained that there is no conflict between intuition and deliberation in the context of cooperation (Tinghög et al., 2013; Verkoeijen & Bouwmeester, 2014). Some empirical evidence supports each of these positions, so it remains unresolved whether cooperation is supported by intuition or deliberation.

In the study reported here, I aimed to shed new light on how intuition and deliberation are linked to cooperation by considering all of the relevant experimental evidence together through meta-analysis. Furthermore, rather than just asking the descriptive question of what effects intuition and deliberation have on cooperation, I tested the predictions of a specific formal theory that not

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only describes what these effects should be in various situations, but also provides an explanation for why such relationships would have come to exist in the first place.

Theoretical Framework and Predictions

The theory that I brought to bear is the social heuristics hypothesis (SHH; Rand et al., 2014; for a formal mathematical implementation, see Bear & Rand, 2016). The SHH uses an explicitly dual-process perspective to synthesize and extend various theories proposed by psychologists and evolutionary biologists who have argued that cooperative decisions are shaped by those behaviors that were successful in situations experienced in the past (Chudek & Henrich, 2011; Kiyonari, Tanida, & Yamagishi, 2000; Tomasello, Melis, Tennie, Wyman, & Herrmann, 2012; Tooby & Cosmides, 1990; Van Lange, De Bruin, Otten, & Joireman, 1997). The SHH contends that it is particularly our *intuitive* responses that are shaped by past experience: The behavior that is typically advantageous in daily-life social interaction (i.e., that maximizes payoffs in the long run) is automatized as a social heuristic, or a generalized default response. Deliberation, conversely, allows us to adjust to the *specific* social situation we are facing at any given time, overriding the intuitive response if that response is not actually payoff maximizing in the current setting.

This logic generates clear, falsifiable predictions regarding how intuition and deliberation are related to cooperation. Rather than predicting a universal relationship, the SHH predicts variation based on both context and individual differences. If intuitive responses are shaped primarily by prior experience, they can favor either cooperation or noncooperation, depending on one's daily-life social environment; however, for most subjects in typical experiments, intuition should favor cooperation, given the pervasiveness of mechanisms (e.g., repeated interactions, concerns about reputation, sanctions, the threat of ostracism) that make cooperation advantageous in daily life in the long run (for a review, see Rand & Nowak, 2013). And if the behavior favored by deliberation depends on the current situation, deliberation also can favor either cooperation or noncooperation, but which is favored depends on the extent to which there are incentives to cooperate in the current situation.

When future consequences are sufficiently unlikely, there is no self-interested motive to cooperate. Such situations pose social dilemmas, and any cooperation that occurs is what I refer to as *pure* (i.e., altruistic) cooperation. The SHH predicts that deliberation in social dilemmas will favor noncooperation.

When sufficient future consequences exist, however, the payoff structure is such that cooperating can maximize one's material payoff, as long as others are also cooperative. For example, if I know that you will cooperate with me next time we meet, but only if I cooperate with you now (e.g., that you are playing a tit-for-tat strategy), I can maximize my payoff by paying the cost of cooperation now in order to receive the benefits of your reciprocal cooperation in the future. Future consequences can therefore change a social dilemma into a coordination problem; purely on the basis of material payoffs, rather than any psychological transformation of changes in utility, it is optimal to cooperate if and only if the other person is also cooperative. Thus, when future consequences are sufficiently strong, self-interested motives to cooperate do exist, and cooperation is what I call *strategic*. The SHH predicts that in such situations, deliberation will favor either cooperation or noncooperation depending on the individual's explicit beliefs about which behavior will maximize his or her payoff (and these beliefs are influenced, e.g., by expectations about how others will act).

This reasoning generates two clear predictions that can be easily tested through meta-analysis. First, experimentally promoting intuition over deliberation should increase pure cooperation, on average, because in social dilemmas, intuition can favor either cooperation or noncooperation (depending on the individual), whereas deliberation always favors noncooperation. Second, experimentally promoting intuition over deliberation should have no overall effect on strategic cooperation, because both intuition and deliberation may favor either cooperation or noncooperation, depending, respectively, on the individual's past experiences and his or her explicit expectations about what strategy will be payoff maximizing in the current context.

The Current Work

I assessed these predictions by meta-analyzing studies that applied cognitive-processing manipulations to incentivized economic games. For several reasons, I focused on behavior in these games, in which subjects make decisions about how to allocate real money between themselves and others. First, cooperating in these games requires paying actual costs to help others; in contrast, when self-report or hypothetical measures are used, subjects can claim, without cost, to be cooperative in order to maintain their self-concept, present themselves positively to the experimenter, or satisfy other reputational concerns. Second, there is a standard set of economic games in which subjects can pay costs to benefit others, and research has shown that a given subject's play correlates strongly across these games, which suggests that they are all measures of a common "cooperative phenotype" (Peysakhovich, Nowak, & Rand, 2014; Yamagishi et al., 2013). Third, focusing on economic games made coding for the meta-analysis very straightforward, as they all share an identical outcome variable: the fraction of a monetary endowment spent on benefiting others. Fourth, it is clear to subjects playing these games whether a given decision will have future consequences, and this is critical for testing diverging theoretical predictions for pure versus strategic cooperation.

To examine how intuition and deliberation relate to cooperation in these games, I meta-analyzed effects of cognitive-processing manipulations on cooperation, rather than correlations of cooperation with, for example, endogenous decision times or individual differences in cognitive style, as correlations do not demonstrate causality. For example, recent work has shown that decision times in cooperation games are driven more by decision conflict than by the relative extent of intuitive versus deliberative processing (Evans, Dillon, & Rand, 2015; Krajbich, Bartling, Hare, & Fehr, 2015).

In sum, I used meta-analysis to determine the overall effect of experimentally promoting intuition (relative to experimentally promoting deliberation) on pure cooperation and strategic cooperation in economic games.

Method

Cooperation measure

To measure cooperation, I considered standard economic games in which subjects could pay real monetary costs to provide real monetary benefits to other people—as opposed to, for example, games in which subjects could pay costs to impose costs on others (i.e., punishment, which is psychologically distinct from cooperation; Peysakhovich et al., 2014; Yamagishi et al., 2012) or games that were hypothetical or played against a computer rather than another person. In choosing which economic cooperation games to include in my analysis, I was guided by theory: A key element of the SHH's predictions is the mismatch between behavior that is payoff maximizing when future consequences exist (e.g., in the repeated interactions of daily life) and behavior that is payoff maximizing when interactions are one-shot and anonymous (such that no future consequences exist). Therefore, I examined play in the four canonical games in which this mismatch exists, such that cooperation can be self-interested if the game is repeated, but noncooperation is always self-interested when the game is one-shot (i.e., there are cooperative Nash equilibria in the repeated game, but the unique subgame perfect Nash equilibrium in the one-shot game is noncooperation): the prisoner's dilemma, the public-goods game, the trust game, and the ultimatum game. I did not include studies of the dictator game, in which subjects unilaterally decide how to split money with a recipient, because transferring money does not maximize payoffs in this game even if it is repeated, and therefore no mismatch exists between one-shot and repeated play (for a theoretical treatment and meta-analysis of the role of intuition and deliberation in the dictator game, see Rand, Brescoll, Everett, Capraro, & Barcelo, 2016).

In the prisoner's dilemma, two players simultaneously choose to cooperate or defect, and the payoff structure is such that the payoff is higher if both players cooperate than if both players defect, but no matter what the other person chooses, a player always earns more by defecting than by cooperating. Thus, the highest individual payoff in a one-shot game comes from defecting while the other player cooperates. If the game is repeated, however, it can be payoff maximizing to cooperate in the current round in order to induce the other person to cooperate in the next round.

In the public-goods game, multiple (typically more than two) players are each given a cash endowment and simultaneously choose how much to contribute to a "common project." All contributions are multiplied by some factor (smaller than the number of players) and divided equally among all players. Therefore, as in the prisoner's dilemma, everyone's payoff is higher if everyone contributes the maximum than if no one contributes at all, but a player always personally loses money by contributing (no matter what the other players do). Thus, the highest individual payoff in a one-shot public-goods game comes from contributing nothing while everyone else contributes the maximum. By the same logic as in the prisoner's dilemma, however, contributing can be payoff maximizing if the game is repeated (especially when directed punishment or reward is possible; Rand, Dreber, Ellingsen, Fudenberg, & Nowak, 2009).

In the trust game (also called the investment game), Player 1 is given a cash endowment and chooses how much to transfer to Player 2. Any amount transferred is multiplied by some factor, and then Player 2 decides how much to return to Player 1 (any money returned is not multiplied further). In a one-shot game, the payoff-maximizing behavior for Player 1 depends on Player 2's behavior. If Player 2 keeps most of the money, then Player 1 maximizes his or her payoff by transferring nothing, but if Player 2 is trustworthy and returns a sufficiently large fraction of the money, then Player 1 maximizes his or her payoff by transferring the maximum amount. Player 2, conversely, always maximizes his or her payoff by returning nothing. If the trust game is repeated, however, it can be payoff maximizing for Player 2 to return a substantial amount in order to induce Player 1 to transfer money in the next period.

In the ultimatum game, Player 1 makes an offer of how to split a cash endowment with Player 2, who can either accept or reject this offer. If the offer is accepted, both players are paid accordingly. If the offer is rejected, neither player earns anything. (Note, therefore, that Player 2's decision is a punishment decision, not a cooperation decision, as it involves paying a cost to impose a cost on Player 1, rather than giving Player 1 a benefit; therefore I did not include Player 2's behavior in my analyses.) In a one-shot game, the payoff-maximizing behavior for Player 1 depends on Player 2's behavior (as in the trust game). If Player 2 is willing to accept any offer, then Player 1 maximizes his or her payoff by offering nothing (or at most, the smallest possible positive offer); but if Player 2 will reject unfair offers, then Player 1 maximizes his or her payoff by transferring a substantial amount (typically, half the endowment). Player 2, conversely, maximizes his or her payoff by accepting any offer in a one-shot game. If the ultimatum game is repeated, on the other hand, it can be payoff maximizing for Player 2 to reject an unfair offer in order to induce Player 1 to make a fair offer in the next period.

Thus, subjects in these games make a choice about how much money to give up in order to provide a monetary benefit to one or more other subjects. Critically, however, these cooperation decisions come in two forms: pure-cooperation decisions, in which cooperating is not in one's self-interest, and strategic-cooperation decisions, in which cooperating can be in one's self-interest depending on the behavior of the other player or players.

Cognitive-processing manipulations

To investigate the effects of intuition and deliberation on cooperation in these games, I examined studies in which cognitive processing was experimentally manipulated using any of the four standard types of intervention: cognitive load, time constraints, ego depletion, and induction. Although each of these manipulations has limitations (as I discuss in this section), taken together they can give valuable insight into the roles of intuition and deliberation in decision making.

In cognitive-load studies, subjects are required to engage in an easy or difficult cognitive task (e.g., holding a three-digit vs. a seven-digit number in memory) at the same time that they are participating in the economic game. Compared with subjects engaging in the easy task, those engaging in the difficult task have fewer cognitive resources to devote to the economic game, and therefore are less able to engage in deliberation (and their decisions are correspondingly more heavily influenced by intuition; Gilbert, Pelham, & Krull, 1988). Although cognitive load is widely used as a tool for impairing deliberative processing, it is possible that cognitive load also alters how intuitive processing itself functions, thus evoking a response different from the one that is favored by intuition under typical (nonload) circumstances.

In time-constraint studies, subjects are required either to make their decision quickly, before a specified amount of time elapses (i.e., time pressure), or to stop and think for a specified amount of time prior to deciding (i.e., time delay). (The maximum time allowed in time-pressure studies of economic cooperation is typically 10 s or less, following the protocol introduced in Rand et al., 2012.) Reducing the amount of time subjects have to decide shortens the window of opportunity for deliberation to rein in intuition, whereas forcing them to wait has the opposite effect. Thus, decisions made under time pressure are more heavily influenced by intuition, whereas decisions made under time delay are more heavily influenced by deliberation (Wright, 1974). Note that studies with time constraints typically also have explicit instructions telling subjects whether to use intuition or deliberation, so the pure effect of constraining decision time is often confounded with the effect of inducing an intuitive or deliberative mind-set.

There is also a danger that time pressure confounds the effect of increasing intuition with the effect of decreasing comprehension: If time pressure is applied to the reading of instruction or during presentation of information about the payoffs of the game, then subjects under time pressure may act differently simply because they are not able to fully assess the details of the decision they are faced with. Therefore, I included in the meta-analysis only those time-constraint studies in which pressure or delay was applied solely during the decision. (This comprehension confound is not as much of an issue for the other cognitive-processing manipulations, because even if an intuition-increasing manipulation makes it more difficult for subjects to understand the instructions, they can take as long as they need in order to achieve understanding.)

In ego-depletion studies, as in cognitive-load studies, subjects are required to complete a more or less cognitively demanding task in addition to the economic game. The difference, however, is that in ego-depletion studies, subjects complete the cognitive task prior to the economic game. Compared with subjects who complete the less cognitively demanding task, those who complete the more cognitively demanding task are more mentally fatigued (depleted) during the subsequent economic game, and so are less able (or less willing) to inhibit their automatic intuitive responses (Baumeister, Bratslavsky, Muraven, & Tice, 1998). An important caveat regarding ego depletion, however, is that it has recently been argued that depletion, unlike cognitive load or time pressure, does not actually reduce the cognitive resources available for decision making, but instead reduces subjects' motivation to expend resources (Inzlicht & Schmeichel, 2012). Even more radically, other researchers have suggested that ego depletion may not be a robust phenomenon at all (Hagger et al., in press). Thus, ego depletion may not function in the same way as other cognitive-processing manipulations (or may not function at all).

In induction studies, subjects are encouraged to engage in either intuitive or deliberative decision making. In some cases, this has been done simply by asking subjects to use intuition or deliberation (e.g., Liu & Hao, 2011). In other cases, subjects have been asked to write an account of a time in their lives when they used intuition or deliberation and this led to either a positive or a negative outcome. Recalling a time when intuition worked out well or deliberation worked out poorly induces subjects to rely more on intuition; conversely, recalling a time when intuition worked out poorly or deliberation worked out well induces subjects to rely more heavily on deliberation (Shenhav, Rand, & Greene, 2012). Although this latter technique is less well established than the other cognitive-processing manipulations, there is evidence to support its efficacy: For example, the recall induction has been found to have effects similar to those of other cognitive-processing manipulations on belief in God (Shenhav et al., 2012), cooperation (Rand et al., 2012), and lying (Cappelen, Sorensen, & Tungodden, 2012), and subjects made decisions more quickly when this recall technique was used to increase reliance on intuition than when it was used to increase reliance on deliberation (Ma, Liu, Rand, Heatherton, & Han, 2015). Also, an examination of subjects' free-response descriptions of their decision-making process revealed that those induced to rely on intuition used more intuition-related words, whereas those induced to be deliberative use more deliberation-related words (Roberts et al., 2014). It is important to note, however, that intuition inductions have the potential downside of being transparent in their purpose, and therefore potentially leading to demand effects (e.g., subjects might respond according to what they explicitly believe the function of intuition or deliberation is or what they think the experimenter expects them to do).

Studies using these cognitive-processing manipulations often include checks to ensure that subjects actually obeyed the manipulation instructions: For example, did subjects remember the easy or hard numbers (cognitive-load studies), decide within the allotted time (time-constraint studies), complete the prior depleting task (ego-depletion studies), or write a sufficient amount (induction studies involving recall writing)? It is common practice to exclude subjects who fail such checks, because they have not been successfully treated by the manipulation. Such exclusions are potentially problematic, however, as they may undermine causal inference by introducing systematic differences between the subjects who are included in one condition and those who are included in the other (as pointed out by Tinghög et al., 2013). Therefore, as a robustness check, I also determined the effect of the cognitive-processing manipulations without excluding such subjects. (Note that, as indicated in Tables S1 and S2 in the Supplemental Material available online, this was not possible for four studies in which data were not available for subjects who did not comply with the cognitive-processing manipulations; however, the fraction of excluded subjects in these few studies was low—between 0.6% and 6%—and therefore I included these studies, despite their exclusions, in my no-exclusion analyses.)

Data sets

For this meta-analysis, I collected data sets satisfying two inclusion criteria: (a) Subjects played one or more (nonhypothetical) prisoner's dilemmas, public-goods games, trust games, or ultimatum games with human (rather than computer) partners, and (b) cognitive processing was manipulated between subjects using cognitive load, time constraints, ego depletion, or induction. I excluded studies in which time constraints were applied to the instructions or information about the payoffs, because this introduces a comprehension (attention) confound.

To find qualifying studies, I began by conducting a 5 × 6 keyword search across PsychINFO, EconLit, Sociological Abstracts, International Bibliography of the Social Sciences, PubMed, SSRN, IDEAS REPEC, and Google Scholar, using each possible combination of "prisoner's dilemma," "public goods game," "trust game," "investment game," or "ultimatum game" with "cognitive load," "time pressure," "ego depletion," "intuition priming," "intuition recall," or "intuition conceptual priming." I also conducted controlled-vocabulary searches combining the terms "cooperation," "social dilemma," and "trust" with "intuition" and "dual-process models" when possible (the relevant controlled vocabulary for this meta-analysis was available only in PsychINFO and Sociological Abstracts). Furthermore, I solicited published and unpublished data sets from the field at large using e-mail lists associated with social psychology, judgment and decision making, and experimental economics. Finally, once I had collected an initial set of eligible data sets, I also considered all articles or manuscripts listed in Google Scholar as citing those data sets. Figure 1 is a flow diagram of this process.

This search resulted in a total of 67 studies that matched my criteria (17 of these studies were unpublished or published only as working papers at the time of their inclusion in the meta-analysis). The studies involved a total of 17,647 independent subjects (median sample size per study = 163 before and 150 after excluding subjects who did not comply with the cognitive-processing manipulation). Of the 67

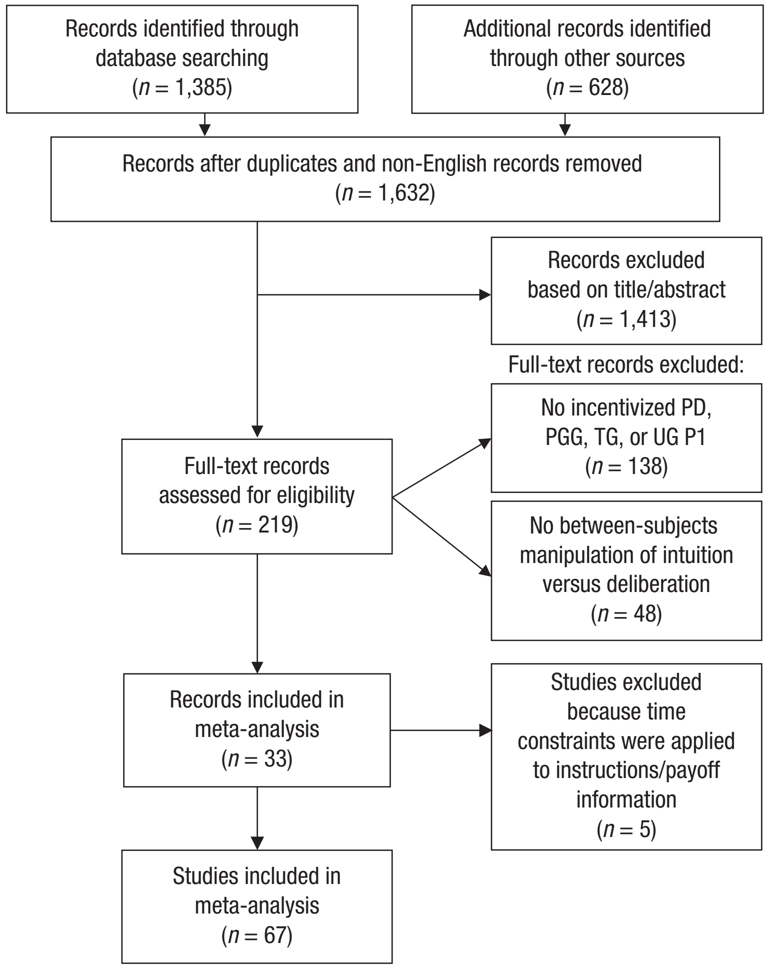

Fig. 1. Flow diagram of the literature search and the application of the inclusion and exclusion criteria. Note that many articles and unpublished data sets contained more than one study. P1 = Player 1; PD = prisoner's dilemma; PGG = public-goods game; TG = trust game; UG = ultimatum game.

studies, 51 (15,850 subjects) examined pure cooperation, and 16 (2,220 subjects) examined strategic cooperation (423 subjects made both pure and strategic decisions, and each study in which this was the case was counted as 2 separate studies for the purpose of the meta-analysis; thus, the total N is smaller than the sum of the ns for pure and strategic cooperation). Table 1 summarizes the distribution of the studies by type of game and manipulation, and Tables S1 and S2 in the Supplemental Material provide details on each study.

Coding

Whether a given study manipulated cognitive processing using cognitive load, time constraints, ego depletion, or an induction was often clearly defined (i.e., these precise terms were used in description of the methods). Nonetheless, I had two separate coders code each study for the type of cognitive-processing manipulation. There was 100% agreement between the two coders.

The coding of the type of cooperation (pure vs. strategic) was based on game-theoretic calculations and thus entirely nonsubjective. All decisions in which noncooperation was payoff maximizing regardless of the actions of the other player or players were coded as decisions about pure cooperation: decisions in one-shot prisoner's dilemmas, decisions in one-shot public-goods games, Player 2 decisions in one-shot trust games, and decisions in the final period of any fixed-length repeated game. All decisions in which cooperation could be payoff maximizing for some set of choices by the other player or players were coded as decisions about strategic cooperation: Player 1 decisions in one-shot trust games and ultimatum games, decisions in repeated prisoner's dilemmas and public-goods games, Player 1 decisions in repeated trust games and ultimatum games, and Player 2 decisions in

Table 1. Distribution of the Studies Included in the Meta-Analysis

Distribution by type of game		Distribution by type of cognitive-processing manipulation	
Game	n	Manipulation	n
Public-goods game	35	Time constraint	31
Prisoner's dilemma	16	Ego depletion	17
Trust game, Player 1	7	Induction	13
Ultimatum game, Player 1	6	Cognitive load	8
Trust game, Player 2	5		

Note: Because some studies involved multiple games or multiple manipulations, the *n*s sum to more than the total number of studies.

repeated trust games (with the exception of the final period in any fixed-length repeated game).

Coding of the outcome variable in economic-game studies is similarly straightforward and nonsubjective: The fraction of the endowment subjects choose to give up to benefit others is a clear measure of cooperation, and is clearly defined in all such studies. Thus, the outcome variable for each study in the meta-analysis was calculated as the average fraction of the endowment given in the more intuitive condition minus the average fraction of the endowment given in the more deliberative condition (i.e., the size of the effect of promoting intuition on cooperation). For studies in which these values were not reported, I contacted the authors to obtain the required information or raw data.

For studies in which subjects made multiple decisions, I used the fraction of the endowment given averaged over all decisions. The only exception was for fixed-length games (in which the number of rounds was known to subjects at the outset): For these games, I used average cooperation over all rounds except the final round as a measure of strategic cooperation (because those decisions had future consequences) and cooperation in the final round as a measure of pure cooperation (because subjects knew that those decisions would have no future consequences). (Averaging over all moves ignored the interdependence of observations from multiple subjects within the same repeated-play group, but group information was not available for all studies, and so it was not possible to use repeated measures procedures consistently across the studies; therefore, I opted for this averaging procedure, which could be applied identically. Note, however, that clustering standard errors at the group level when possible did not qualitatively change the results.)

When players could condition their cooperation on the behavior of their partner or partners (e.g., Player 2 in trust games, subjects in asynchronous prisoner's dilemmas), my approach was guided by the theory: The future consequences that typify daily life transform social dilemmas into coordination games, and as a result, it is typically advantageous to cooperate only when the other person also cooperates. Therefore, the SHH predicts intuitive cooperation only in response to the cooperation of others (and intuitive defection or punishment in response to the defection of other players). Thus, I focused on subjects' responses when the other player or players were maximally cooperative (defined as being maximally trusting in the trust game, cooperating in the prisoner's dilemma, or contributing the maximum amount in the public-goods game). However, in the Results section, I also show that my results are robust to averaging across all responses of the other players.

Finally, for studies that crossed a secondary manipulation (e.g., framing, payoff structure) with the cognitive-processing manipulation, I collapsed over the secondary manipulation and examined the main effect of promoting intuition (in order to avoid introducing researcher degrees of freedom regarding which secondary manipulations to include).

Analysis

I performed random-effects meta-analysis using the *metan* function in Stata/IC 13.1 to determine the overall effect size (and associated standard error) for the effect of cognitive-processing manipulations on cooperation, measured as cooperation in the more intuitive condition minus cooperation in the more deliberative condition (in percentage points). I also determined the absolute level of cooperation in the more deliberative condition, which allowed me to calculate the percentage change associated with increased intuitive processing: I divided the effect size for intuitive processing by the average cooperation in the more deliberative condition to arrive at the percentage change for each study, and then calculated the overall percentage change as the weighted average across studies, using the study weights assigned by the meta-analysis. For moderation analyses, I conducted random-effects metaregression using the *metareg* function in Stata/IC 13.1.

Results

Pure cooperation

As predicted, the meta-analysis revealed a highly significant positive overall effect of increased intuitive processing on pure cooperation (cooperation in settings in which noncooperation was always payoff maximizing, regardless of the choices of other players), effect size = 6.1 percentage points, 95% confidence interval (CI) = [3.4, 8.9], $Z = 4.35$, $p < .0001$ (Fig. 2). On average, there was 17.3% more cooperation in the more intuitive condition relative to the more deliberative condition. Furthermore, 74.7% of

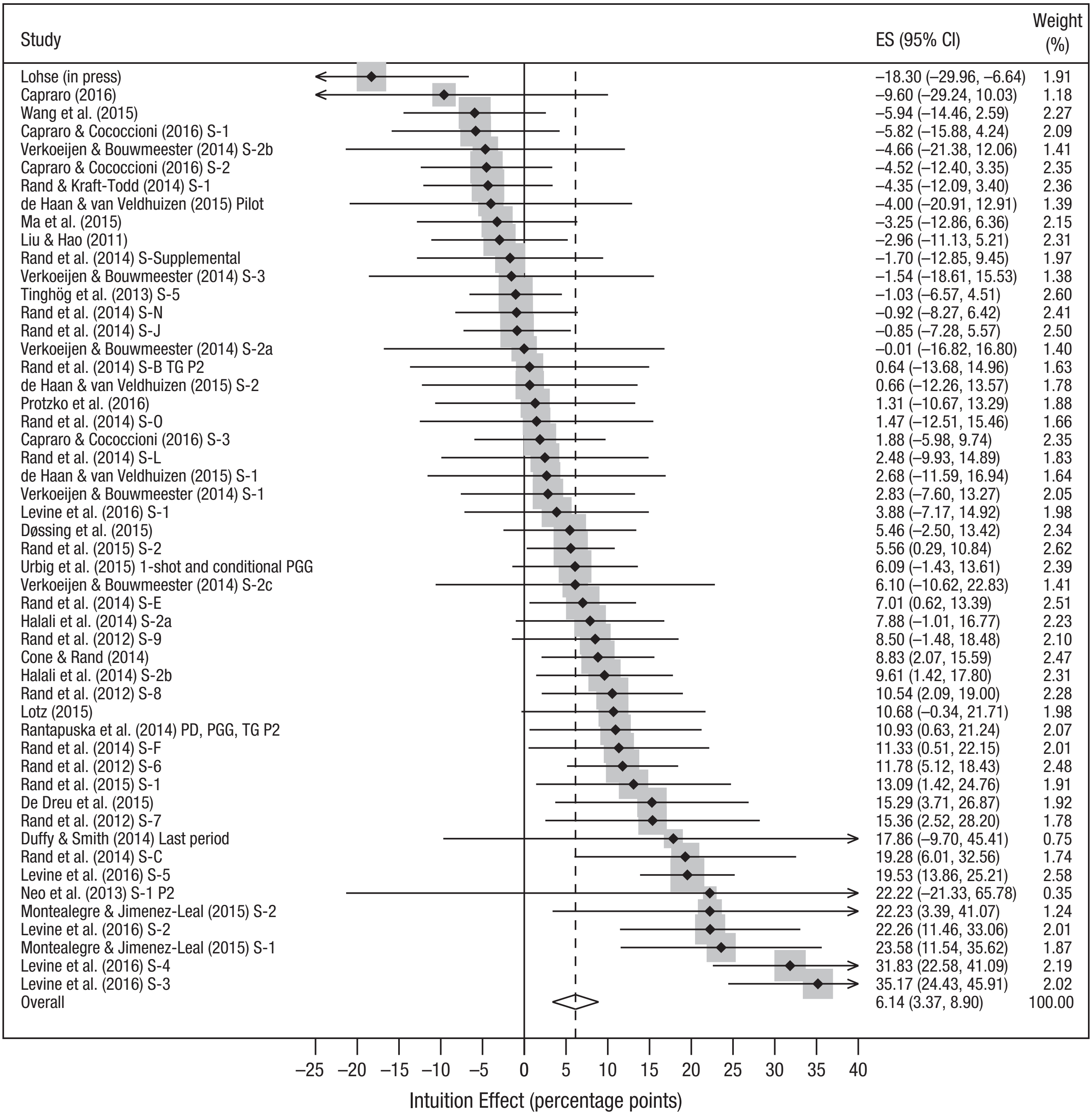

Fig. 2. Results of the random-effects meta-analysis of the difference in pure cooperation between conditions in which intuition was promoted and conditions in which deliberation was promoted. Positive values imply more cooperation in the more intuitive condition. Effect sizes (ESs) were measured as the percentage-point difference between conditions in the fraction of the endowment spent on helping other subjects. Error bars indicate 95% confidence intervals; an arrow at the end of an error bar indicates that the confidence interval extends beyond the range of the *x*-axis. The relative sizes of the gray boxes indicate the weighting assigned to the studies by the meta-analysis. For articles or unpublished data sets with more than one study, the specific study in a given row is indicated by "S-." P2 = Player 2; PD = prisoner's dilemma; PGG = public-goods game; TG = trust game.

the variation across studies was found to be due to true underlying heterogeneity in effect size, which indicated the existence of one or more important moderators (as predicted by the theory).

There was no evidence of significant publication or reporting bias, or of *p*-hacking. Both Egger's test, $t = 0.23$, $p = .82$, and Begg's test, $z = 0.40$, $p = .69$, revealed no indication of small-study effects (i.e., there was no association between standard error and average effect size; see Fig. S1 in the Supplemental Material for funnel plots). Furthermore, *p*-curve analysis (Simonsohn, Nelson, & Simmons, 2014), using *p*-curve app 4.03 (accessed at www.p-curve.com/app4/pcurve4.php), indicated the presence of strong evidential value, $p < .0001$ for both full and half *p*-curves, and did not find indications of inadequate evidential value, $p > .99$ for both full and half *p*-curves. (Note that only studies yielding $p < .05$ when analyzed individually are included in *p*-curve analysis; there were 19 such studies among the pure-cooperation studies in this meta-analysis. See Figs. S2 and S3 in the Supplemental Material for *p*-curve plots.)

Furthermore, the results were robust to including subjects who did not comply with the cognitive-processing manipulations. With these subjects included, the effect size was 4.2 percentage points, 95% CI = [1.8, 6.6], $z = 3.44$, $p = .001$, and there was, on average, 13.5% more cooperation in the more intuitive condition than in the more deliberative condition (and there continued to be no evidence of publication bias or *p*-hacking). The results were also robust to including responses to all levels of others' cooperativeness rather than focusing on responses to maximal cooperativeness (for studies in which conditional cooperation decisions occurred), both when noncompliant subjects were excluded, effect size = 5.5 percentage points, 95% CI = [3.0, 8.1], $z = 4.21$, $p < .0001$, and when they were included, effect size = 3.8 percentage points, 95% CI = [1.5, 6.0], $z = 3.30$, $p = .001$. In addition, the results were robust to including five studies (from Capraro & Cococcioni, 2015, and Tinghög et al., 2013) that were excluded because time pressure was applied during presentation of information about the payoff structure, both with noncompliant subjects excluded, effect size = 5.2 percentage points, 95% CI = [2.6, 7.9], $z = 3.94$, $p < .0001$, and with noncompliant subjects included, effect size = 3.6 percentage points, 95% CI = [1.3, 5.8], $z = 3.12$, $p = .002$. Finally, the results were robust to treating multiple studies conducted by the same group as nonindependent (by first meta-analyzing each group's studies to produce a single effect-size estimate per group and then meta-analyzing those resulting effect sizes), both with noncompliant subjects excluded, effect size = 5.0 percentage points, 95% CI = [1.2, 8.7], $z = 2.61$, $p = .009$, and with noncompliant subjects included, effect size = 3.4 percentage points, 95% CI = [0.6, 6.3], $z = 2.37$, $p = .018$.

Considering sources of variance in effect size across studies, I found that the effect of increased intuitive processing was substantially smaller in studies that manipulated cognitive processing using time constraints (effect size = 4.3 percentage points, 11.7% increase relative to the more deliberative condition) compared with studies that used the other kinds of manipulations (effect size = 8.0 percentage points, 22.7% increase relative to the more deliberative condition). This difference was not significant in a metaregression, $t(49) = 1.24$, $p = .22$, but when subjects who failed manipulation checks were included, the difference became more pronounced and reached statistical significance (time constraints: effect size = 1.3 percentage points, 4.7% increase; other manipulations: effect size = 7.2 percentage points, 21.1% increase), $t(49) = 2.29$, $p = .026$. There was still no evidence of publication or reporting bias when analysis was restricted to studies that did not use time constraints: $p > .4$ for Egger's and Begg's tests, half and full *p*-curve *p*s < .0001 for presence of evidential value and > .99 for inadequate evidential value, whether or not subjects who failed manipulation checks were included. I was not able to assess differences among the other three cognitive-processing manipulations, as there were not enough studies using each manipulation.

I also considered other potential sources of study-level variance and found that there was little difference in effect size between studies conducted in physical laboratories and those conducted online; with subjects who failed manipulation checks excluded, the effect sizes were 5.7 and 6.3 percentage points, respectively, $t(49) = 0.17$, $p = .87$; with subjects who failed manipulation checks included, the effect sizes were 3.6 and 4.4 percentage points, respectively, $t(49) = 0.24$, $p = .81$. There was also no significant difference in effect size between studies conducted by my research group (myself or my students) and studies run by other research groups; with subjects who failed manipulation checks excluded, the effect sizes were 5.3 and 6.5 percentage points, respectively, $t(49) = 0.36$, $p = .72$, and with subjects who failed manipulation checks included, the effect sizes were 2.1 and 5.4 percentage points, respectively, $t(49) = 1.21$, $p = .23$. Moreover, the positive effect of promoting intuition on pure cooperation was robust to excluding studies run by myself or my students—with noncompliant subjects excluded: effect size = 6.5 percentage points, 95% CI = [2.4, 10.7], $z = 3.09$, $p = .002$; with noncompliant subjects included: effect size = 5.4 percentage points, 95% CI = [1.6, 9.2], $z = 2.81$, $p = .005$. (See Table S3 in the Supplemental Material for full metaregression details.)

Next, I tested two alternative interpretations of the observed increase in cooperation with intuitive processing that do not have to do with social heuristics. The first alternative stems from recent analyses showing that correlations between cooperation and (endogenous rather than manipulated) reaction time are driven by decision conflict (strength of preference) rather than by the relative use of intuition versus deliberation (Evans et al., 2015; Krajbich et al., 2015). Might decision conflict also explain the effects of the manipulations considered in the present work? In the context of studies correlating cooperation with reaction time, people who are more conflicted take longer to decide. Across studies, this leads to

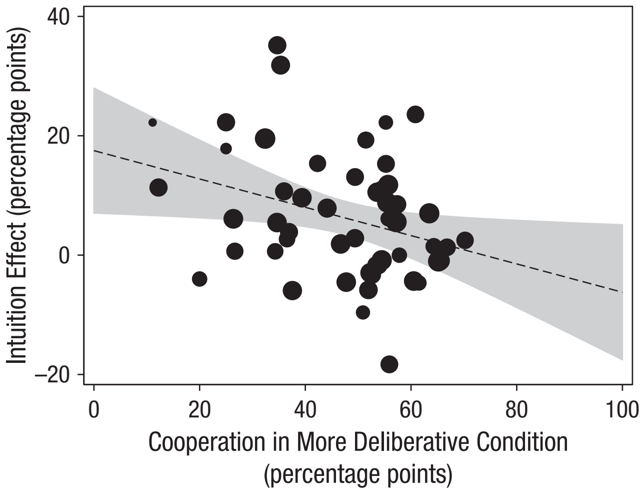

Fig. 3. Size of the effect of intuitive processing on pure cooperation in individual studies as a function of cooperation level in the more deliberative condition. The size of each dot is proportional to the study's assigned weight in the random-effects meta-analysis. Also shown is the best-fitting regression line (from the random-effects metaregression), with the 95% confidence interval indicated by the gray band.

a negative relationship between the absolute level of cooperation and the difference in cooperativeness between faster and slower deciders: The more attractive cooperation is, on average, to subjects in a given study (and thus the higher the absolute level of cooperation), the easier, or less conflicted (and thus faster), the decision will tend to be for subjects who choose cooperation and the harder, or more conflicted (and thus slower), the decision will tend to be for subjects who choose defection (Krajbich et al., 2015).

Thus, if the effect of the cognitive-processing manipulations in the present meta-analysis reflects decision conflict, rather than the relative use of intuition versus deliberation, one would expect a positive correlation between the level of cooperation in the baseline (more deliberative) condition, and the difference in cooperation between manipulation conditions (the intuition effect). That is, there should be a more positive effect of "promoting intuition" in studies in which cooperation is more attractive, and thus more common, in the baseline (more deliberative) condition. On the contrary, however, I observed a negative relationship between absolute level of average cooperation in the more deliberative condition and the effect of more intuitive processing on cooperation (Fig. 3)—with noncompliant subjects excluded: $b = -0.237$, $t(49) = -2.27$, $p = .03$; with noncompliant subjects included: $b = -0.325$, $t(49) = -3.80$, $p < .001$.

There was also no positive relationship between the baseline level of cooperation and the effect of intuitive processing in a regression including only time-constraint studies and only subjects who obeyed the time constraints, $b = -0.199$, $t(25) = -1.37$, $p = .18$. This lack of a positive relationship helps to address concerns about excluding noncompliant subjects. Because slow deciders were excluded from the time-pressure condition and fast deciders were excluded from the time-delay condition, a negative correlation between decision time and cooperation could have led to the higher level of cooperation I observed under time pressure (as suggested by Tinghög et al., 2013). If this were the case, however, I would have seen a positive relationship between absolute cooperation level (attractiveness of cooperation) and the effect of time pressure relative to time delay, as is observed in studies correlating cooperation with decision time (e.g., Krajbich et al., 2015). The fact that no such pattern was observed suggests that correlations between cooperation and decision time did not in fact drive the effect of time constraints when noncompliant subjects were excluded, and supports use of this exclusion.

The second alternative explanation is that rather than activating a social heuristic, promoting intuition might only increase random play (e.g., confusion, "noisy" behavior, or randomly distributed "mistakes"; see Recalde, Riedl, & Vesterlund, 2014, for one articulation of this view). If that were the case, then the level of cooperation would have shifted closer to 50% (i.e., random chance) in the more intuitive conditions compared with the more deliberative conditions. That is, the only-randomness explanation predicts that intuition should (a) increase cooperation when the level of deliberative cooperation is below 50%, but (b) decrease cooperation (to exactly the same extent) when the level of deliberative cooperation is above 50%. In contrast, however, I found no such reversal: There was still a positive effect of promoting intuition even when the analysis was restricted to studies in which deliberative cooperation was above 50% (effect size = 3.5 percentage points with noncompliant subjects excluded and 1.1 percentage points with noncompliant subjects included). Furthermore, a metaregression predicting the size of the intuition effect as a function of cooperation level in the more deliberative condition (Fig. 3) showed a significant estimated positive effect of intuition at the 50% level of cooperation in the deliberative condition—with noncompliant subjects excluded: effect size = 5.6 percentage points, $F(1, 49) = 16.09$, $p = .0002$; with noncompliant subjects included: effect size = 3.7 percentage points, $F(1, 49) = 10.71$, $p = .002$. Thus, I found clear evidence that the observed positive effect of intuition on cooperation in social dilemmas is not solely the result of increased randomness in play (although random play may contribute to the effect).

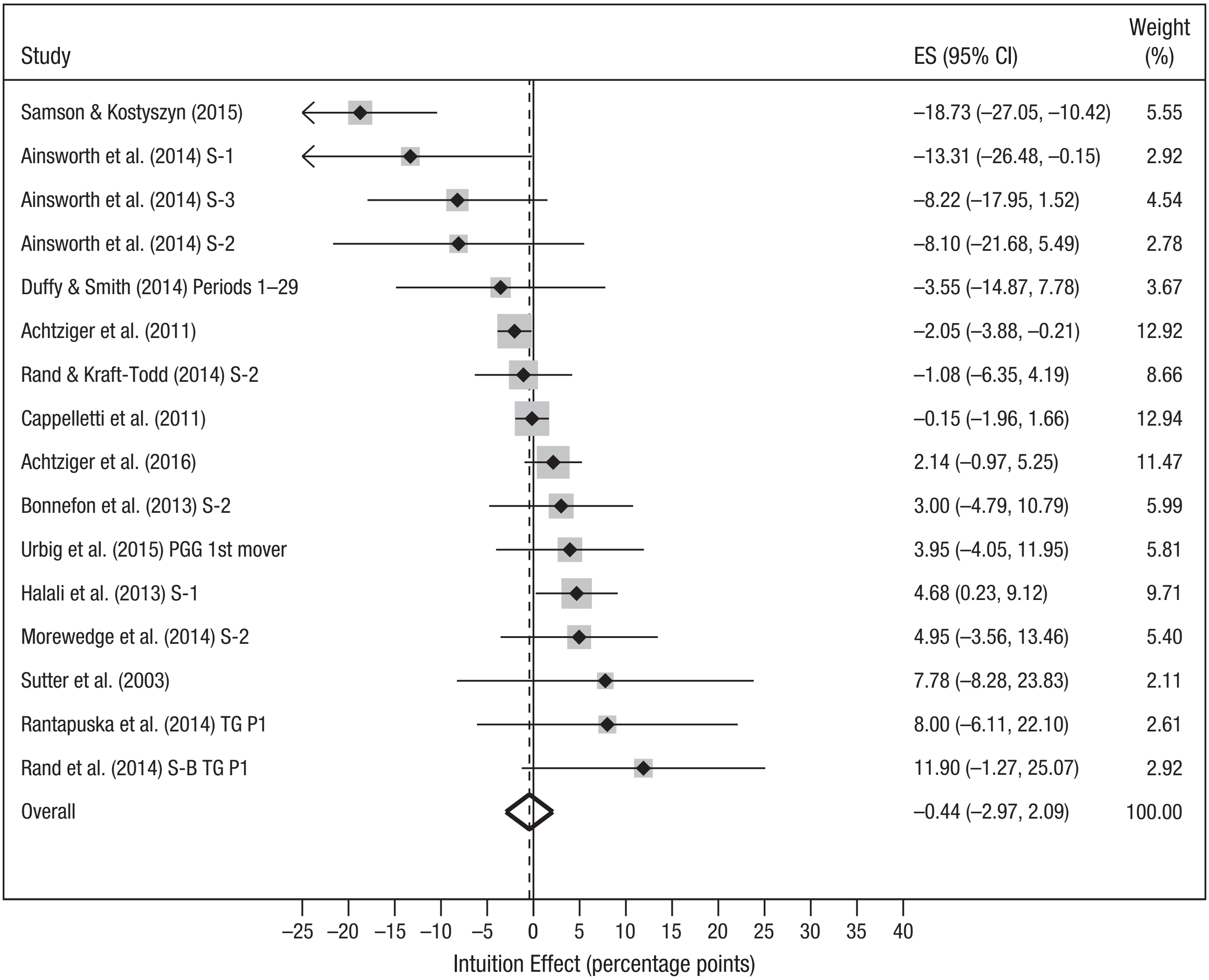

Fig. 4. Results of the random-effects meta-analysis of the difference in strategic cooperation between conditions in which intuition was promoted and conditions in which deliberation was promoted. Positive values imply more cooperation in the more intuitive condition. Effect sizes (ESs) were measured as the percentage-point difference between conditions in the fraction of the endowment spent on helping other subjects. Error bars indicate 95% confidence intervals; an arrow at the end of an error bar indicates that the confidence interval extends beyond the range of the *x*-axis. The relative sizes of the gray boxes indicate the weighting assigned to the studies by the meta-analysis. For articles or unpublished data sets with more than one study, the specific study in a given row is indicated by "S-." P1 = Player 1; PGG = public-goods game; TG = trust game.

Strategic cooperation

In contrast to the meta-analysis for pure cooperation, and in line with the SHH's predictions, the meta-analysis for strategic cooperation revealed no significant effect of increased intuitive processing, effect size = −0.4 percentage points, 95% CI = [−3.0, 2.1], z = 0.34, p = .74 (Fig. 4). As a result, the effect of promoting intuition was significantly larger for pure cooperation than for strategic cooperation, b = 6.8, t(65) = 2.50, p = .015. Furthermore, for strategic cooperation, as for pure cooperation, there was substantial heterogeneity across studies in the size of the intuition effect (67.6% of the variation across studies was found to be due to true underlying heterogeneity in effect size).

As for pure cooperation, there was no evidence of significant publication or reporting bias according to Egger's test, t = −0.03, p = .97 or Begg's test, z = 0.00, p = 1.00. (Because only three studies individually achieved p values below .05, I do not report a p-curve analysis.)

The null result for strategic cooperation was robust to including subjects who did not comply with the cognitive-processing manipulations. With these subjects included, the effect size was −0.5 percentage points, 95%

CI = [−3.0, 2.0], z = 0.40, p = .69 (and the difference between pure and strategic cooperation remained significant when noncompliant subjects were included for both cooperation types, b = 5.12, t(65) = 2.12, p = .037). The null result for strategic cooperation was also robust to including only the first decision made in a given session—with noncompliant subjects excluded: effect size = −0.7 percentage points, 95% CI = [−3.8, 2.5], z = 0.41, p = .68; with noncompliant subjects included: effect size = −0.8 percentage points, 95% CI = [−3.8, 2.2], z = 0.51, p = .60. These results address the concern that the null effect might have been driven by repetition undermining the effect of the cognitive-processing manipulations.

Next, I considered two alternative interpretations of the lack of an effect of more intuitive processing on strategic cooperation. First, this null effect was not the result of a ceiling effect: Subjects spent 47.5% of their endowments on cooperation in the more deliberative conditions (weighted average using weights from the random-effects meta-analysis reported in Fig. 4), and that percentage is far from the 100% maximum.

Second, it is possible that the future consequences that exist for strategic cooperation (but not pure cooperation) reduce the variance in behavior. By this account, most players would make the same choice, and thus there would be little room for other factors to influence play (e.g., as has been found for the influence of social-value orientation on cooperation; Van Dijk, de Kwaadsteniet, & De Cremer, 2009). If this effect of reduced variance indeed drove the null result I observed, cognitive-processing manipulations would consistently have had little effect across strategic-cooperation studies. As a result, there would have been substantially less variation across studies in the size of the cognitive-processing effect for strategic cooperation compared with pure cooperation. Contrary to this prediction, however, there was considerable variation across studies in the effect size for strategic cooperation (see Fig. 4), and there was no significant difference in the variance in effect size across studies between strategic cooperation (*SD* of the effect size = 8.2 percentage points) and pure cooperation (*SD* of the effect size = 10.6 percentage points); Levene's robust test statistic for the equality of variances was not significant, $W_0(1, 65) = 1.30$, p = .26 (an F test generated equivalent results, as did including noncompliant subjects).

Discussion

I have presented a theoretically informed meta-analysis of the effect of manipulating intuition and deliberation on cooperation in economic games. The results provide clear support for a theory of social heuristics according to which intuition favors typically advantageous behavior and deliberation favors behavior that is payoff maximizing in the current situation. As predicted by this theory, the data showed that promoting the use of intuition over deliberation increased pure cooperation (i.e., cooperation in settings where noncooperation was always payoff maximizing, regardless of the other person's decision), but had no overall effect on strategic cooperation (i.e., cooperation in settings where cooperation could be payoff maximizing depending on the choices of the other players).

These findings highlight the importance of social heuristics for human cooperation. Critically, the key finding is not simply that promoting intuition over deliberation increases cooperation. Rather, the key finding is that this occurs when the SHH predicts it will (pure cooperation) and does not occur when the SHH predicts it will not (strategic cooperation). Not only does the SHH predict these empirical findings, but it also provides an explanation for why intuition and deliberation should have come to function as they do: A simple process of maximizing long-run payoffs (be it via evolution, social learning, or strategic reasoning) can explain the observed pattern of behavior as the result of an on-average optimal set of responses (Bear & Rand, 2016).

In addition to the predictions evaluated here, the SHH makes various predictions regarding individual difference moderators, such as the extent to which the daily social environment supports cooperation (Capraro & Cococcioni, 2015; Rand et al., 2012; Rand & Kraft-Todd, 2014) and prior experience with experiments (DeVoe & House, in press; Rand & Kraft-Todd, 2014; Rand et al., 2014). Although these predictions have been evaluated in individual studies, more data should be collected and meta-analysis performed.

Furthermore, the studies meta-analyzed here were almost exclusively conducted in WEIRD societies (i.e., Western, educated, industrialized, rich, and democratic societies; Henrich, Heine, & Norenzayan, 2010). It is critical for future work to explore intuition, deliberation, and cooperation using non-WEIRD populations. The SHH's predictions regarding when intuition will favor cooperation and when it will favor defection (based on which behavior is typically rewarding in a given society) can be used to guide cross-cultural investigations.

The estimate of the size of the intuition effect presented here is likely a lower bound on the true effect size, for several reasons arising from my erring on the side of inclusivity during study selection to avoid researcher degrees of freedom. First, I included studies in my meta-analysis regardless of how effective their cognitive-processing implementations were. Second, I included all experimental conditions for each study, even when secondary manipulations were expressly designed to reduce any positive effects of intuition (e.g., Protzko, Ouimette, & Schooler, 2016). Third, I included all subjects from each study, even when individual differences were shown to reduce the intuition effect (e.g., Rand & Kraft-Todd, 2014). Fourth, I included studies run on Amazon Mechanical Turk in the years after that platform had become extremely popular for running academic studies, so that subjects were largely nonnaive to economic-game paradigms—and nonnaiveté has been found to reduce effect sizes for various manipulations (Chandler, Paolacci, Peer, Mueller, & Ratliff, 2015), including time-pressure manipulations applied to cooperation (Rand et al., 2014; Rand & Kraft-Todd, 2014). Finally, many of these studies included subjects who failed comprehension questions about the payoff structure and did not understand that the game was a social dilemma, which would undermine any effect of the cognitive-processing manipulation (Strømland, Tjøtta, & Torsvik, 2016).

More generally, none of the manipulations used in these studies were process-pure; undoubtedly, many subjects in the "more intuitive" conditions were still able to engage in substantial deliberation. This seems particularly likely to be true for the time-constraint manipulations. Because time pressure was (by necessity) applied only during the decision, and not during presentation of instructions, subjects could deliberate prior to the application of time pressure. And accordingly, the meta-analytic effect size obtained was smaller for the time-constraint studies than for the studies using other kinds of manipulations. This result suggests that future work may do well to focus on cognitive-processing manipulations other than time constraints.

The relationship of intuition and deliberation to cooperation has received a great deal of attention in recent years. Here I have shown that when considered together, the accumulated experimental evidence supports a theory of social heuristics and deliberative self-interest, a theory that connects proximate psychology and cognition with ultimate causation to illuminate human cooperation.

Action Editor

Leaf Van Boven served as action editor for this article.

Author Contributions

D. G. Rand is the sole author of this article and is responsible for its content.

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The author declared that he had no conflicts of interest with respect to his authorship or the publication of this article.

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Supplemental Material

Additional supporting information can be found at http://pss.sagepub.com/content/by/supplemental-data

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Asterisks indicate studies included in the meta-analysis.

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