The dual-process approach to human sociality: A quantitative review

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Abstract

Which social decisions are intuitive? Which are deliberative? The dual-process approach to human sociality has emerged in the last decades as a vibrant and exciting area of research. This review and meta-analysis synthesizes the existing literature on the cognitive basis of cooperation, altruism, truth-telling, positive and negative reciprocity, and act utilitarianism, and introduces a framework inspired to Rand and colleagues' Social Heuristics Hypothesis that organizes the experimental regularities. The results suggest that intuition favours a set of heuristics that are related to the instinct of self-preservation. People avoid being harmed, avoid harming others (especially when there is a risk of harm to themselves), and are averse to disadvantageous inequalities. Finally, this paper highlights some open questions that deserve the attention of future research.

Keywords: dual-process, prosociality, cooperation, altruism, truth-telling, reciprocity, moral judgments, heuristics, self-preservation.

The dual-process approach to human sociality: A quantitative review

We are unique in the animal kingdom for our capacity to live in large societies made of thousands, if not millions, of unrelated individuals. Bees, ants, and mole naked rats, for example, live in large societies, but individuals within the same society tend to share a substantial degree of biological relatedness. Other non-human primates live in groups of unrelated individuals, but these groups tend to be relatively small. We are the only animals that are able to organize themselves in large societies, to the point that several scholars have argued that this uniquely-human capacity has been crucial for our evolutionary success as an animal species (Kaplan, Hill, Lancaster & Hurtado, 2000; Fehr & Fischbacher, 2003; Boyd & Richerson, 2005; Tomasello, Carpenter, Call, Behne & Moll, 2005). An obvious consequence of living in large societies is that most of our decisions do not affect only ourselves, but they affect also other people or society as a whole. Since the well-being of our societies highly depend on these “social decisions”, understanding how people make this type of decisions is a topic that has attracted the attention of social scientists for decades. In the last twenty years, in particular, there has been a proliferation of studies viewing at human sociality through a dual-process lens. Dual-process theories contend that people’s choices result from the interplay between two cognitive systems, one that is fast and intuitive, and one that is slow and deliberative. By applying this dual process lens to human sociality, scholars have started investigating questions such as: Which social choices are intuitive? Which are deliberative? These questions turned out to be extremely fruitful and have shed light on a series of exciting empirical results. These experimental findings have been paralleled by the development of theoretical frameworks. At the same time, however, a number of important problems remain unsolved, making this field of research among the most thrilling and fascinating across all the behavioural sciences. I thus believe that the time is mature

to present a quantitative review of the state of the art. The goal of this survey is fourfold: (1) to introduce new researchers to a field that is growing exponentially and offers a number of open questions; (2) to conduct an exhaustive literature review and meta-analysis that can be helpful for scholars already in the field; (3) to present a novel theoretical framework inspired by the Social Heuristics Hypothesis proposed by Rand et al. (2014); and (4) to describe a number of open problems that are fundamental to further advance our understanding of the cognitive basis of human sociality.

Preliminaries

Social decisions

In this review, I will focus on social decisions. Broadly speaking, social decisions are defined as decisions that affect people other than the decision maker. However, without further specifications, it is unlikely to draw general conclusions regarding the cognitive foundations of all social decisions due to the vast differences among them. In fact, social decisions can vary along multiple dimensions, including their consequences and the ability of those affected to influence the decision maker. To narrow the focus, this review primarily examines cooperation, altruism, truth-telling, positive and negative reciprocity, and act utilitarianism, as these have been the primary focus of previous research. Exploring the cognitive basis of other types of decisions is certainly an important direction for future work and I will come back to this in the final section, where I will discuss the few studies that have investigated the cognitive basis of equity-efficiency, ingroup favouritism, and aggression. In what follows, I list only the types of decisions that are the primary focus of this review (I selected these decisions because, for each of them, there have been at least *ten* experiments exploring their cognitive basis). For each of these

decision types, I provide a brief definition, and more details about their measurement will be discussed in the corresponding sections.

Cooperation

Two or more decision-makers have to simultaneously decide whether to pay a cost to increase the payoff of their group.

Altruism

A decision-maker has to decide whether to pay a cost to increase the payoff of another person or group of people.

Truth-telling

A decision-maker has to decide whether to lie or to tell the truth. The decision-maker's and another person's payoffs depend on whether the decision-maker lies or tells the truth. The other person might be abstract (i.e., the experimenter) or concrete (i.e., another participant).

Reciprocity

After observing (or being the recipient of) an action, a decision-maker has to decide whether to pay a cost to punish or to reward the actor. Several types of reciprocity exist, depending on the context. "Direct reciprocity" usually refers to situations in which the decision-maker was the recipient of the action (Trivers, 1971); "indirect reciprocity" usually refers to situations in which the decision-maker observed the action, but was not directly affected by it (Nowak & Sigmund, 2005). Additionally, one can distinguish between "positive reciprocity" and "negative reciprocity", depending on whether the decision-maker has observed (or was the recipient of) a good or a bad action.

Deontology vs. act utilitarianism

A person has to decide whether to act so as to maximize the greater good, or to respect certain rights or fulfil certain duties, even if these lead to suboptimal outcomes.

The dual-process approach

Dual process theories refer to a set of frameworks sharing the core idea that people's choices result from the interplay between two cognitive systems, one that is fast and intuitive, and one that is slow and deliberative.

This idea has been formalized in several ways (Fodor, 1983, 2001; Scheider & Shiffrin, 1977; Epstein, 1994; Epstein & Pacini, 1999; Chaiken, 1980; Chen & Chaiken, 1999; Reber, 1993; Evans & Over, 1996; Evans, 1989, 2006; Sloman, 1996; Smith & DeCoster, 2000; Hammond, 1996; Stanovich, 1999, 2004; Nisbett et al, 2001; Wilson, 2002; Lieberman, 2003; Toates, 2006; Strack & Deutsch, 2004; Kahneman & Frederick, 2005; Evans, 2008; Glöckner & Betsch, 2008; Glöckner & Witteman, 2010; Kahneman, 2011; Stanovich, 2011; Evans & Stanovich, 2013; De Neys & Pennycook, 2019), leading to a class of models that differ from each other in many details, not only in the names given to the two cognitive systems, but also in the specific functions that these systems are assumed to perform, to the point that these theories do not exactly map onto each other (Evans, 2008; Evans & Stanovich, 2013).

In this review, I adopt the point of view of Evans and Stanovich (2013), who provided convincing evidence of the existence of a dual-processing distinction which, theoretically, can be explained by assuming the existence of “rapid, autonomous processes, Type 1, that are assumed to yield default responses unless intervened on by distinctive higher order reasoning processes (Type 2)”, which rely heavily on hypothetical thinking and working memory.

It is worth noting that there is an ongoing discussion about when typically Type 1 and typically Type 2 responses are produced. Classic dual process theories tend to assume that Type 1 responses are generated immediately after the stimulus, whereas Type 2 responses come at a later time. This assumption has been challenged by a recent line of research suggesting that people can process basic logical principles intuitively (Handley et al, 2011; De Neys, 2012; Banks & Hope, 2014; Pennycook, Fugelsang & Koehler, 2015; Ball, Thomson & Stuppel, 2017). This observation is leading towards a revision of dual process models. In “dual process 2.0” models (see De Neys and Pennycook, 2019, for a review) “heuristic”, intuitive, responses and “logical”, intuitive responses are both generated automatically. Then, if the two intuitive responses are in conflict (i.e., if they have similar strength), deliberation will favour the “logical” intuitive response over the “heuristic” intuitive response. On the other hand, if they are not in conflict, deliberation will favour the one with greater strength. It is crucial to emphasize, however, that according to these 2.0 models, it can never be the case that Type 2 overrides the “logical” intuitive response in favour of the “heuristic” intuitive response. Therefore, dual process 2.0 models differ from their 1.0 antecedents regarding the moment in which typically Type 2 responses are produced, but not regarding which choice is preferred by Type 2.

In this article, I will not take a personal stand on which specific dual-process theory is correct or on the specific functions of the two types of processes, but I will follow a pragmatic approach using the so-called “typical correlates”. These are measures that can be manipulated experimentally which *typically* correlate with the two types of processes. Importantly, these measures do *not* define the types. For example, to say that Type 1 processes tend to be fast, whereas Type 2 processes tend to be slow, does not mean that Type 1 processes will always be faster than Type 2 processes. It does not even mean that Type 1 responses will be generated

earlier than Type 2 responses. It simply means that, by experimentally manipulating people's response time, for example by including a time pressure that forces people to provide their answer within a certain window of time, the experimenter would be more likely to observe an increase in the frequency of Type 1 responses, compared to the control condition, with no time pressure; symmetrically, by including a time delay that forces people to provide their answer after a certain amount of time, the experimenter would be more likely to observe an increase in the frequency of Type 2 responses, compared to the control condition, with no time delay. This assumption does not require that Type 2 responses are generated later than Type 1 responses. In fact, this assumption is satisfied also in dual process 2.0 models, where "heuristic" and "logical" intuitive responses are assumed to be generated at the same time; but then, at a later time, Type 2 can override the heuristic intuition in favour of the logical intuition, but never the converse.

Coming to the "typical correlates", I will focus on those that have been used in most previous research applying the dual process approach to human sociality. In this line of research, Type 1 responses are assumed to be typically fast, autonomous, and effortless, while Type 2 responses are assumed to be typically slow, controlled, and effortful. All these correlates appear in the list of correlates reported by Evans and Stanovich (2013), Table 1. Besides these correlates, I will consider another correlate that has been used in many experiments because easy to manipulate in the laboratory: reliance on emotions vs reliance on reason.

A critical property of these correlates is that they can be easily manipulated in the laboratory. The most popular manipulation techniques are time constraints, cognitive load, conceptual primes, ego-depletion, and the two-response paradigm.²

Time constraints

Since Type 1 processes tend to be fast and Type 2 processes tend to be slow, a simple technique to promote the use of Type 1 over Type 2 is to ask participants to respond within a given window of time (“time pressure” condition). Conversely, to promote the use of Type 2 over Type 1, one can ask participants to stop and think for some time over the decision problem before making a decision (“time delay” condition).

Although useful, time constraints can be criticized along several dimensions. The first critical issue originates from the observation that time constraints must be set in the decision screen and not in the instruction screen, because otherwise they would interfere with task comprehension. However, this methodological necessity implies that will-be-under-time-pressure participants can actually stay as long as they want in the instruction screen, where they are free to start using Type 2. Therefore, it is unclear whether the decision that then they make under time pressure can be considered their Type 1 decision. The second limitation regards the length of the time pressure, which is typically relatively long (5-10 seconds). On the one hand, relatively long time windows are needed for practical reasons; i.e., it requires a few seconds to inform participants that they have to make a decision. But, on the other hand, this raises the issue of whether decisions observed after 10 seconds can be considered Type 1 decisions, especially in light of evidence that first decisions are made almost instantaneously, and sometimes even before people become aware of them (Soon, Brass, Heinze & Haynes, 2008). A potential response to this criticism is that it is not really important that all participants under time pressure make a Type 1 decision: it is enough that a significant proportion of them make a Type 1 decision, in order to detect, if it exists, a significant difference between Type 1 and Type 2 decisions. However, this creates another issue: in case one observes no difference between decisions under

time pressure and decisions under time delay, then it is not possible to disentangle whether this null result is driven by the fact that the time pressure does not affect the decision at hand, or by the incapacity of time pressure to promote Type 1 processes for a significant proportion of participants.

The third limitation regards the compliance rate in the time pressure condition. While it is possible to force participants to respond after a certain amount of time (for example, by making the “submit” button in the decision screen visible only after 10 seconds), it is not possible to force participants to respond within a given amount of time. This generates a methodological issue. If the experimenter does not allow participants to submit their choice after a certain amount of time, there will be a selection bias; if the experimenter does allow participants to submit at any time, then there will be a proportion of participants who will not respect the time constraint. This is particularly problematic because participants are typically heterogeneous in their response time, therefore participants who fail to respond within the time constraint may systematically differ from the remaining ones in some (potentially unobservable) variable. This raises the question of how these participants should be treated in the analysis. In the seminal work by Rand et al. (2012), these participants were eliminated from the analysis. More recently, experimenters have started including all participants in the analysis (Tinghög et al, 2013; Rand, Newman & Wurzbacher, 2015; Tinghög et al, 2016; Bouwmeester et al, 2017; Capraro, 2017). A third, more radical, solution is to have participants practice in a series of games, before the actual decision, so that they get used to respond quickly, which helps to minimize the proportion of participants who fail to comply with the time pressure (Everett et al., 2017). Another solution is to incentivize participants to reply quickly by making them lose money for each second they spend on the decision screen (Alós-Ferrer & Garagnani, 2020).

Cognitive load

Since Type 2 relies heavily on working memory (Evans & Stanovich, 2013) – the cognitive system with a limited capacity that is responsible for storing short-term memory (Miyake & Shah, 1999) – one can inhibit the use of Type 2 by making participants use their short-term memory to solve a parallel, unrelated task (Gilbert & Hixon, 1991), as, for example, memorizing a sequence of digits (Swann, Hixon, Stein-Seroussi & Gilbert, 1990), or hearing a series of letters while having to press a button whenever they hear a character that resounded as two letters before (Gevins & Cutillo, 1993). Since participants need to store a temporary information in their working memory, they will be less likely to use their working memory when making the primary, parallel, decision. For example, Gilbert & Hixon (1991) found that cognitively loaded participants are more likely to rely on stereotypes; Shiv & Fedorikhin (1999) found that cognitively loaded participants are less likely to exert less self-control when choosing between cake and fruit salad; Hinson, Jameson and Whitney (2003) found that cognitively loaded participants have greater discounting of future rewards.

Studies based on cognitive load have limitations too. First, it is possible that the task being used to impair participants' access to working memory interacts with the primary decision problem. Second, analogously to time constraint studies, a number of participants typically fail to correctly perform the task aimed at taxing their working memory; how should these participants be treated? Similar to time constraint studies, to avoid selection bias, experimenters typically include all participants in the analysis. Third, cognitive load manipulations can be used only to *undermine* Type 2, and never to *promote* it.

Conceptual primes

A technique that can be used to promote both Type 1 and Type 2 takes the name of conceptual priming. The idea is simple: before making a choice, participants are *primed* to rely either on Type 1 or on Type 2. The priming can be done in several different ways.

Some scholars explicitly asked participants to use their intuition or deliberation while making their choice (Liu & Hao, 2011), others asked participants to write a piece of text in which they have to describe a time in their life in which following their gut reactions (or thinking carefully) led them to good decision making (Rand, Greene & Nowak, 2012), yet others asked participants to make a decision by “relying on emotion” vs. “relying on reason” (Levine, Barasch, Rand, Berman & Small, 2018).³

The main limitation of these explicit primes is that they mention the words “intuition”, “deliberation”, or similar. This may create demand effects such that participants make a decision according to what they believe the intuitive or the deliberative choice should be (Rand, 2016). A potential solution would be to use subtler primes, as, for example, those used by Small, Loewenstein and Slovic (2007). However, to the best of my knowledge, this was the only work that used implicit primes to study social decisions of the type considered in the current review.

Another priming technique consists in making participants read the instructions and express their decision in a foreign language. Since native language is intuitive and automatic, while foreign languages tend to be processed more slowly, foreign language might affect the cognitive process being used to make a decision. According to this idea, foreign language has been shown to reduce heuristics and biases (Keysar et al., 2012; Costa et al., 2014a) and to reduce emotionally-driven responses in moral dilemmas (Costa et al., 2014b, Geipel, Hadjichristidis & Surian, 2015a; Cipolletti, McFarlane & Weissglass, 2016; Corey et al., 2017;

Hayakawa, Tannenbaum, Costa, Corey & Keysar, 2017). This line of research thus suggests that information processed in a second language reduces intuitive thinking.

However, also priming through a foreign language has two limitations. One is that several works have shown that information processed in a second language does not increase analytic thinking. For example, it does not improve performance on cognitive reflection tests (Costa et al., 2014a), conjunction fallacy or base rate neglect problems (Vives et al., 2018), nor does it improve detection of semantic illusions (Geipel et al., 2015b; but see Hadjichristidis, Geipel, & Surian, 2017). Therefore, if anything, foreign language can be useful to impair Type 1 responses, but not to promote Type 2 thinking. The second limitation is potentially more critical. Reasoning in a second language is typically cognitively tiring and this might contribute to depletion of cognitive resources that works in the opposite direction of the prime (Volk, Köhler & Pudelko, 2014).

Ego depletion

Ego depletion is an experimental manipulation based on the theoretical assumption that Type 2 requires the exertion of self-control to override immediate, autonomous, Type 1-driven reactions. Although there is an ongoing debate about whether self-control is a limited resource (Baumeister, Heatherton & Tice, 1994; Baumeister & Tierney, 2011) or not (Inzlicht & Schmeichel, 2012; Inzlicht, Schmeichel & Macrae, 2014), several studies have provided evidence that exerting self-control at Time 1 reduces the ability to exert self-control at Time 2. Following this line of work, social scientists have investigated how participants make social decisions at Time 2 after having completed, at Time 1, a task meant to undermine their self-control.

Self-control can be impaired in several ways, for example using the Stroop task (Stroop, 1935), or the e-hunting task (Moller, Deci & Ryan, 2006). Other empirical techniques that are sometimes used to decrease self-control are sleep deprivation,⁴ hunger,⁵ alcohol intoxication,⁶ and mortality salience.⁷

Studies using ego depletion have their shortcomings too. First, while ego-depletion tasks can be used to impair Type 2 processing, there is no symmetric equivalent to impair Type 1 processing. A potentially more critical limitation is related to the ongoing debate about whether the ego-depletion effect actually exists. On the one hand, some meta-analyses have shown that, if one corrects for publication bias, the ego-depletion effect may not be different from zero (Carter, Kofler, Forster & McCullough, 2015; Carter & McCullough, 2014; Hagger et al, 2016). On the other hand, also these meta-analyses have been criticized along several dimensions, one of which is that they include also studies without a manipulation check. Therefore, it is possible that the overall lack of an effect is ultimately driven by the inclusion of studies that were unable to manipulate self-control. Consequently, whether ego-depletion is a real or an illusory effect is still discussed (Frieze, Loschelder, Gieseler, Frankenbach & Inzlicht, 2019).

The two-response paradigm

The two-response paradigm, introduced by Thompson, Turner and Pennycook (2011), asks participants to make two consecutive decisions, one under time pressure or cognitive load, and one under no constraints (or under time delay). By analysing if and how participants switch between choices, one can determine which choice is deliberate and which is intuitive.

The main conceptual limitation of this method is that it introduces an experimental demand for consistency, such that participants may be reluctant to change their initial, intuitive choice. Classic work indeed shows that people have strong preferences for maintaining their previous

decision (Samuelson & Zeckhauser, 1988). One potential solution is to introduce a control treatment in which subjects make only the deliberative choice (Bago, Bonnefon & De Neys, 2019). Another limitation is that it promotes Type 1 decisions using either time pressure or cognitive load, therefore it suffers from the same limitations as these cognitive manipulations.

The cognitive basis of cooperation

One of the most important forms of social behaviour is cooperation. Many scholars have come to argue that our uniquely-human capacity to cooperate with unrelated others has been the secret of our enormous evolutionary success as an animal species (Darwin, 1871; Trivers, 1971; Axelrod & Hamilton, 1981; Ostrom, 2000; Fehr & Gächter, 2002; Milinski, Semman & Krambeck, 2002; Gintis, Bowles, Boyd & Fehr, 2003; Fehr & Fischbacher, 2004; Nowak, 2006; Bowles & Gintis, 2011; Rand & Nowak, 2013; Perc et al, 2017). Along these lines, research in psychology has suggested that the psychological basis of cooperation, *shared intentionality*, is what makes humans uniquely humans, as it is possessed by children, but not by great apes (Tomasello et al., 2005). Therefore it seems reasonable to start the central section of the article, by reviewing the literature on the cognitive basis of cooperation.

Measures of cooperation

Cooperation can be operationally defined in different ways. Here, I follow the literature on social dilemmas. A social dilemma is any situation in which there is a conflict between individual interest and group interest (Olson, 1965; Hardin, 1968; Dawes, 1980; Kollock, 1988; Fehr & Fischbacher, 2004; Nowak, 2006; Rand & Nowak, 2013; Capraro, 2013; Perc et al, 2017). The main social dilemmas are the prisoner's dilemma and the public goods game.

The prisoner's dilemma

In the prisoner's dilemma, two players can either cooperate or defect. If both players cooperate, they receive the *reward* for cooperation, R ; if both players defect, they receive the *punishment*, P . If one player cooperates and the other defects, the cooperator receives the *sucker's* payoff, S , and the defector receives the *temptation* payoff, T . Payoffs are assumed to satisfy the inequalities $T > R > P > S$. These inequalities guarantee that both players would be better off if they both cooperate than if they both defect. However, unilateral defection brings the highest individual payoff. This generates the conflict between individual payoff and group payoff that is at the core of the social dilemma.

The public goods game

In the public goods game, N players start with an initial endowment. Each player has to choose how much of it, if any, to contribute to a public pool. The total contribution gets multiplied by a constant (greater than 1 and smaller than N) and then equally distributed among the N players. Therefore, the final payoff of each player is the part of the initial endowment that they decided not to contribute plus the proportion of the public good they received. As in the prisoner's dilemma, players would be better off if they all cooperate, compared to when they all defect; however, each individual is better off when they defect, independently of the other players' strategies. Therefore, also in this case there is a conflict between individual payoff and group payoff.

Other social dilemmas

The prisoner's dilemma and the public goods game are not the only games characterized by a conflict between individual payoff and group payoff. Other examples are the traveller's dilemma and the Bertrand competition. However, to the best of my knowledge, no one has

explored the cognitive underpinnings of cooperation in these social dilemmas.¹¹ Therefore, in this review, I will not consider these games.

Other definitions of cooperation

There are other ways to define cooperative behaviour. Of particular interest for the current article is the distinction between *pure cooperation* and *strategic cooperation* proposed by Rand (2016).

Pure cooperation refers to any game in which: (i) a player pays a cost to give a benefit to one or more other players; (ii) the payoff maximizing strategy in the iterated variant of the game is different from the payoff maximizing strategy in its one-shot, anonymous, variant. One can easily see that one-shot prisoner's dilemmas and public goods games (as well as traveller's dilemmas and Bertrand competitions) have these two properties. But there are also other games which satisfy them. Consider the trust game. Player 1 is given a certain amount of money and has to decide how much of it, if any, to invest. The amount invested is multiplied by a factor greater than 1 and given to Player 2. Then Player 2 decides how much of the amount received to return to Player 1. Clearly, in one-shot anonymous interactions, Player 2 has no incentive to return any money, and therefore Player 2's payoff-maximizing strategy is to keep the whole amount. However, in repeated interactions it might be optimal for Player 2 to return part of the amount in order to avoid that Player 1 in the next round does not invest any money, leading to a long-term loss for Player 2. Therefore, the trust game (played in the role of Player 2) is an example of pure cooperation in the sense of Rand (2016).

Strategic cooperation refers to those games in which a player pays a cost to give a benefit to another player; however, whether this choice maximizes the player's payoff depends on the decision of the other player. The two prototypical cases considered by Rand (2016) are the one-

shot and anonymous trust game (player 1), and the one-shot and anonymous ultimatum game (player 1). Indeed, in the one-shot and anonymous trust game, whether investing money maximizes Player 1's payoff depends on the expected behaviour of Player 2: if Player 1 believes that Player 2 will return a sufficient amount of money, then it is optimal for Player 1 to invest their money; otherwise, it is not. In the ultimatum game, Player 1 has to decide how to split a certain sum of money and Player 2 has to decide whether to accept or reject the proposed split. In case Player 2 accepts the offer, then the two players are paid according to the agreed split; in case Player 2 rejects the offer, both players end up with nothing. Therefore, Player 1 can make a larger offer (i.e., paying a cost to give a benefit to Player 2), but this choice will be optimal only if Player 1 believes that Player 2 will accept the corresponding offer and will reject any smaller offer. Other cases of strategic cooperation are the repeated public goods game and the repeated prisoner's dilemma.

Review of the experimental evidence

Several dozen experimental papers have explored whether intuitive or deliberative processing are causally linked to cooperative behaviour. The full list is reported in Table 1.

Table 1. List of papers exploring the cognitive basis of cooperation. PGG stands for “public goods game”; PD stands for “prisoner's dilemma”; CPR stands for “common pool resource” (economically equivalent to the PGG, but in a take frame).

	Measure	Main result
Time constraints		
Rand et al. (2012)	PGG	Pressure <i>increases</i> cooperation, compared to Delay
Tinghög et al. (2013)	PD	<i>No effect</i> of Pressure vs Delay
Tinghög et al. (2013)	Binary PGG	<i>No effect</i> of Pressure vs Delay

	Measure	Main result
Cone & Rand (2014)	Competitive PGG	Pressure <i>increases</i> cooperation, compared to Delay
Rand et al. (2014)	PGG	Pressure <i>increases</i> cooperation, compared to Delay, but only among inexperienced participants
Rand & Kraft-Todd (2014)	PGG	Pressure <i>increases</i> cooperation, compared to Delay, but only among participants that are naïve and trusting
Verkoeijen & Bouwmeester (2014)	PGG	<i>No effect</i> of Pressure vs Delay
Capraro & Cococcioni (2015)	PD	<i>No effect</i> of Pressure vs Delay
Rand et al. (2015)	PGG with outgroups	Pressure <i>increases</i> cooperation, compared to Delay
Capraro & Cococcioni (2016)	PD	Pressure <i>decreases</i> cooperation, compared to Delay
Lohse (2016)	PGG	Pressure, compared to Baseline, <i>decreases</i> cooperation, but only among subjects with high CRT
Strømmland et al. (2016)	PGG	<i>No effect</i> of Pressure vs Delay
Bouwmeester et al. (2017)	21 preregistered PGGs	<i>No effect</i> of Pressure vs Delay on the whole sample. Pressure <i>increases</i> cooperation, among participants who respect the time constraint
Kessler et al. (2017)	PD with varying b/c	Pressure, compared to Delay, <i>increases</i> cooperation for small b/c and <i>decreases</i> cooperation for large b/c
Everett et al. (2017)	PD	Pressure <i>increases</i> cooperation, compared to Delay, also towards competitive out-groups

	Measure	Main result
Goeschl & Lohse (2018)	PGG	Pressure <i>decreases</i> cooperation, compared to Baseline
Isler et al. (2018)	PGG	Pressure, compared to Delay, <i>increases</i> cooperation
Bird et al. (2019)	PGG only males	Pressure, compared to Delay, <i>increases</i> cooperation among risk-averse males; Pressure <i>decreases</i> cooperation among risk-seeking males
Alós-Ferrer & Garagnani (2020)	PGG	Pressure, compared to Delay, <i>increases</i> cooperation among prosocial participants; Pressure has <i>no effect</i> on individualist participants; Pressure <i>decreases</i> cooperation among competitive participants
Gao et al. (2020)	PGG	Pressure <i>increases</i> cooperation, compared to Delay
Bilancini et al. (2021)	PGG	Pressure <i>decreases</i> cooperation with respect to “motivated delay”, where participants have to motivate their action
Isler et al. (2021a)	PGG/CPR	Pressure, compared to Delay, <i>increases</i> cooperation in the PGG; pressure <i>decreases</i> cooperation in the CPR
Isler et al. (2021b)	PD	Pressure, compared to Delay, has <i>no effect</i> on cooperation
Bogliacino & Gómez (in preparation)	PD	Pressure, compared to Motivated Delay, <i>decreases</i> cooperation
Nava et al. (2023)	PGG	Pressure, compared to Delay, <i>increases</i> cooperation among adults; pressure <i>decreases</i>

	Measure	Main result
		cooperation among adolescents
Cognitive load		
Gao et al. (2020)	PD	Load <i>increases</i> cooperation
Conceptual prime		
Rand et al. (2012)	PGG	Intuition <i>increases</i> cooperation, compared to Deliberation
Lotz (2015)	Asymmetric PD	Intuition <i>increases</i> cooperation, compared to Deliberation
Urbig et al. (2016)	PGG	Second language <i>decreases</i> cooperation
Gargalianou et al. (2017)	PG G	<i>No effect</i> of second language on cooperation
Levine et al. (2018)	PD	Emotion <i>increases</i> cooperation, compared to Reason
Camerer et al. (2018)	PGG	<i>No effect</i> of Intuition vs Deliberation
Gärtner et al. (2022)	PD, PGG, and DG	Emotion <i>promotes</i> pro-sociality compared to Reason
Ego depletion		
Capraro & Cococcioni (2016)	PD	Ego depletion <i>decreases</i> cooperation
Rantapuska et al. (2017)	PGG	<i>No effect</i> of hunger on cooperation
Häusser et al. (2019)	PGG	<i>No effect</i> of hunger on cooperation
McEvoy et al. (2021)	PD	<i>No effect</i> of sleep restriction on cooperation
2-response paradigm		
Bago et al. (2021)	PGG	Deliberation has <i>no effect</i> on cooperation

Given the number of studies, here I will focus on meta-analyses and some key references. The first meta-analysis was published by Rand (2016), who analyzed 67 experiments in which participants' cognitive processing was manipulated while making a pure cooperation decision; he included in the meta-analysis also studies using the trust game (only player 2), and sequential

prisoner's dilemmas (only second movers). He also included two field experiments in which subjects had to decide whether to return a lost glove under different time constraints (Artavia-Mora, Bedi & Rieger, 2017; Artavia-Mora, Bedi & Rieger, 2018) and one study in which cooperating with one player implied competing with a third party (De Dreu, Dussel & Ten Velden, 2015). In doing so, he reported an overall positive effect of intuition on cooperation. Moreover, this positive effect was not present in strategic cooperation. He used this result to support the Social Heuristics Hypothesis (SHH). The SHH (Rand et al, 2012; Rand et al, 2014) contends that people internalize strategies that are optimal in everyday interactions, which are typically repeated, and use them as default strategies in novel settings, when their capacity to deliberate is impaired. Then, after deliberation, people override these social heuristics and tailor their behaviour towards the one that is optimal in the given interaction. Therefore, the SHH predicts that intuition favours pure but not strategic cooperation, which is exactly what Rand found in his 2016 meta-analysis.

However, this overall conclusion was subsequently challenged by Kvarven et al. (2020), who conducted an updated meta-analysis (82 experiments). In their meta-analysis, Kvarven and colleagues included only social dilemmas, meaning that they did not include the trust game, the sequential prisoner's dilemma, and the two natural field experiments by Artavia-Mora and colleagues (2017, 2018). In doing so, they found an overall effect of intuition on cooperation, but, critically, they found that this effect was entirely driven by conceptual primes explicitly asking participants to rely on their emotion. The authors argued that these primes might have induced a demand effect. Moreover, the same result was obtained when adding, in the analysis, the trust game and the sequential prisoner's dilemmas, but not Artavia-Mora and colleagues' field experiments, because they interpreted these works as studying altruistic behaviour and not

cooperative behaviour (the person who lost the glove could not reciprocate the action). In a response paper, Rand (2019) argued that there is no theoretical reason to exclude the trust game, the sequential prisoner's dilemma and the two field experiments, because they all meet the criteria for being classified as "pure cooperation". Adding these papers, Rand (2019) found a positive effect of intuition on pure cooperation, which remains significant also when restricting the analysis to studies which do not use the explicit primes. Since 2020, there have been eleven more experimental studies testing the effect of manipulating cognitive processes on cooperation using social dilemmas: five of them reported a positive effect of promoting intuition on cooperation, four of them reported a null effect, and two of them reported a negative effect. However, the negative effect was obtained by comparing a "time pressure" manipulation with a "motivated delay" treatment, where participants were asked to motivate their action, before implementing it (Bilancini et al. 2022; Bogliacino & Gómez, in preparation). This intervention may activate other processes than pure deliberation. In conclusion, the available experimental evidence points towards a positive (probably small and very heterogeneous) effect of promoting intuition on cooperation.

Some works have sought to explain part of the heterogeneity across studies by investigating the role of individual-level moderators. Rand et al. (2014) found that the effect was driven by participants with no previous experience in laboratory experiments. Rand and Kraft-Todd (2014) found that time pressure increases cooperation, but only among participants who are both inexperienced and have high trust on people with whom they interact in their everyday life. These results fit well with the SHH, because this framework does predict that inexperienced participants who live in a trustworthy environment are more likely to bring cooperative heuristics to the lab than participants who are either experienced or who live in an untrustworthy

environment; the formers may have learned that defecting is optimal in the given one-shot interaction, while the latter may have internalized defective heuristics.

Another work investigated the cognitive foundations of cooperation as a function of participants' social value orientation (SVO). Specifically, Alós-Ferrer and Garagnani (2020) implemented incentive compatible time constraints as follows: participants under time delay were paid €1 if they responded after 10 seconds; participants under time pressure lost 30 cents for every second spent on the decision screen. In doing so, they found no overall effect of time pressure on cooperation in a one-shot public goods game; however, they found that time constraints interacted with SVO, in such a way that time pressure made prosocials more prosocials, and competitors more competitive; individualists were unaffected by the time constraints. They replicated this result also in a repeated public goods game with random re-matching and no feedback. They concluded that intuition enhances intrinsic predispositions.

Outlook and open problems

In this section, I shortly summarize the previous findings and I list a set of open problems that I think deserve attention in future research.

As described above, the evidence regarding a potential causal link between cognitive process and cooperation is somewhat mixed. Rand's (2016) meta-analysis shows that, overall, Type 1 processing favours cooperative behaviour, whereas Type 2 processing promotes non-cooperative behaviour. However, this conclusion was challenged by a larger meta-analysis, with different inclusion criteria (Kvarven et al., 2020), which, in turn, was challenged by an even larger meta-analysis (Rand, 2019). All these meta-analyses however agree on one point: explicit conceptual primes of emotion increase cooperative behaviour. They disagree on the effect of other cognitive manipulations.

The overall conclusion that one can draw from this debate is that there is a need for more and better designed studies. The vast majority of experiments used time constraints or explicit primes of intuition. However, both these methods have been criticized. The methodological issues of time constraints have been resolved in later studies (Everett et al. 2017; Isler et al. 2018; Alós-Ferrer & Garagnani, 2020); therefore, future work using time constraints can prioritize these methods. Regarding conceptual primes, there is a need of studies using more implicit conceptual primes. Note, indeed, that there is only one study using conceptual primes of intuitions that are not explicit (i.e., one study using second-language). To increase generalizability, it would be helpful also to conduct studies using different cognitive manipulations, as there is only one study using cognitive load and four using ego-depletion.

Another important question regards the role of potential moderators. Alós-Ferrer and Garagnani (2020) found that the effect of time pressure on cooperation depends on the social value orientation, such that prosocials tend to become more cooperative when deciding under pressure, whereas individualists tend to be unaffected by the manipulation and competitive subjects even tend to become more selfish.¹¹ To explain these results, they proposed that time pressure makes people more likely to behave according to their social value orientation, while time delay makes people readapt this initial response towards what they believe to be the norm in the given situation. I believe that testing this hypothesis is an important direction for future research. Note that, if confirmed, this hypothesis would be at odds with the SHH in terms of the predicted effect of deliberation on cooperation, but not in terms of the predicted effect of intuition. This because the SHH makes predictions about the moderating role of the environment in which participants live. More precisely, the SHH contends that Type 1 processing promotes cooperation among participants who live in a cooperative setting, but not among those who live

in a non-cooperative setting, who are predicted to remain unaffected by the cognitive manipulation. I am not aware of any study correlating a participant's SVO with the cooperativeness of the environment in which this participant lives, but it is reasonable to expect that these two measures are correlated. Therefore, the predictions of the SHH are very similar to those of Alós-Ferrer and Garagnani's hypothesis, in terms of the effect of intuition. By contrast, while the SHH predicts no positive effect of deliberation on cooperation on any subset of subjects, Alós-Ferrer and Garagnani's hypothesis predicts a positive effect of deliberation on cooperation among pro-self individuals.

Another relevant, but poorly explored, line of research regards the existence of a potential inverted-U effect of cognitive effort on cooperation. Study 3 in Moore and Tenbrunsel (2014) found that cognitive complexity has an inverted-U effect on cooperation in a non-incentivized, social dilemma based on the Shark Harvesting and Resource Conservation exercise. Capraro & Cococcioni (2016) used this result to explain why extreme cognitive depletions (extreme time pressure and ego-depletion) seem to give rise to lower rates of cooperation: it is possible that the effect of cognitive effort on cooperative behaviour is non-linear. The theoretical rationale for this assumption is that cooperation might be partly driven by an abstract desire to do the morally right thing (Capraro & Rand, 2018); and it has been long argued that the application of abstract moral rules is not immediate, but appears only at later stages of moral reasoning (Kohlberg, 1921, 1963). Therefore, exploring a potential inverted-U effect of cognitive reflection on cooperation is a fascinating and non-trivial question, with obvious theoretical and practical implications. One remark is that this question is inherently difficult because implementing extreme cognitive manipulations (e.g., extreme time pressure, or very hard cognitive load or ego

depletion tasks) may interfere with participants understanding of the game or increase drop-out rates, leading to potential selection biases.

Another open problem regards whether the effect of promoting intuition vs. deliberation on cooperation depends on the benefit-to-cost ratio. Kessler et al. (2017) found that time pressure increases cooperation for relatively small b/c , but the effect is reversed for bigger values. If confirmed, this effect would have important theoretical implications, because it would not be consistent with any of the frameworks previously mentioned, and therefore would require a new theorization. As a potential explanation, Kessler et al. (2017) proposed that Type 2 processing puts more weight on social efficiency.

Finally, an observation regarding asymmetric social dilemmas, that is, social dilemmas where the cost and the benefit of cooperation are player-dependent. These dilemmas are important in light of practical applications, because, in reality, there is often power imbalance among people, which can be modelled in terms of asymmetries between costs and benefits of cooperation. In spite of this practical importance, however, only one study explored the cognitive underpinnings of cooperation in asymmetric dilemmas: Lotz (2015) used conceptual primes and found that intuition promotes cooperation. Clearly, more studies are needed also on this topic.

The cognitive basis of altruism

We often pay costs to help others, even in situations where the recipient has no way to reciprocate our action. For example, when we buy food for a homeless person sitting at the entrance of a food shop. The notion of altruism formalizes the core of this behavioural domain. Formally, a decision-maker is said to perform an altruistic behaviour if they pay a cost to help someone. The recipient of the altruistic action need not be a single individual; it could also be a group of individuals, such as a humanitarian organization.

Measures of altruism

The prototypical measure of altruism is the dictator game, in which the *dictator* is given a certain amount of money and has to decide how much of it, if any, to donate to the *recipient*, who is initially given nothing. The recipient has no choice but to accept whatever the dictator decides to give. In the standard version of the dictator game, the recipient is a person. However, one can consider variants in which the recipient is not a single person. In particular, a variant that has received considerable attention is when the recipient is a charitable organization. Since there is no theoretical reason to distinguish between these two variants, I will consider both in this article.

Another variant can be obtained by changing the ratio between the monetary cost and the monetary benefit of the altruistic action. For example, when the monetary cost of helping is lower than the monetary benefit created, the altruistic action not only helps the other person but also increases social efficiency. However, this introduces a confounding factor since equity and efficiency might be cognitively different. To avoid this, I focus on studies that use a dictator game where the monetary cost of the altruistic action is equal to its monetary benefit. In the final section of this article, I will review the literature on the cognitive basis of altruism in dictator games with varying cost-to-benefit ratios.

Review of the experimental evidence

Table 2 reports the list of papers that explored the cognitive basis of altruism, along with a brief description of the main result for each paper.

Table 2. List of papers exploring the cognitive basis of altruism. DG stands for the standard dictator game; DG charity stands for the DG when the recipient is a charitable organization.

	Measure	Main result
Time constraint		
Carlson et al. (2016)	DG charity	<i>No effect</i> of Pressure compared to Delay
Jarke-Neuert & Lohse (2016)	DG	<i>No effect</i> of Pressure compared to Delay
Tinghög et al. (2016)	DG charity	<i>No effect</i> of Pressure compared to Delay
Artavia-Mora et al. (2017)	Field experiment	Pressure <i>increases</i> helping compared to Delay
Kessler et al. (2017)	DG	Pressure has <i>no effect</i> compared to Baseline
Mrkva (2017)	DG charity	Delay <i>increases</i> giving, compared to Baseline
Artavia-Mora et al. (2018)	Field experiment	Pressure <i>increases</i> helping, compared to Delay
Andersen et al. (2018)	DG	<i>No effect</i> of Delay compared to Baseline
Chuan et al. (2018)	DG charity	Longer and longer delay (there is no baseline) <i>decreases</i> giving
Gärtner (2018)	DG	Pressure <i>decreases</i> giving, compared to Baseline, but only when the dictator game is presented with no status-quo distribution
Grolleau et al. (2018)	DG	Longer and longer delay (no baseline) <i>decreases</i> giving
Strømmland & Torsvik (2019)	DG	Pressure has <i>no effect</i> on giving, compared to Delay
Bilancini et al. (2021)	DG	Motivated Delay <i>increases</i> giving compared to Baseline and Delay
Cognitive load		
Roch et al. (2000)	Taking from common pool	Load <i>increases</i> taking
Cornelissen et al. (2011)	DG	Load <i>increases</i> giving among prosocials; no effect among individualists
Benjamin et al. (2013)	DG	Load has <i>no effect</i> on giving
Schulz et al. (2014)	DG	Load <i>increases</i> giving
Kessler & Meier (2014)	DG charity	Load has <i>no effect</i> on giving
Hauge et al. (2016)	DG	Load has <i>no effect</i> on giving
Tinghög et al. (2016)	DG charity	Load has <i>no effect</i> on giving
Grossman & Van der Weele (2017)	DG charity	Load has <i>no effect</i> on giving
Cognitive prime		

	Measure	Main result
Small et al. (2007)	DG charity	Intuition <i>increases</i> giving compared to Deliberation, but only when the target is identifiable and not when is statistical
Zhong (2011)	DG charity	“Feel” <i>increases</i> giving, compared to “Decide”
Kinnunen & Windmann (2013)	DG charity	Intuition has <i>no effect</i> on giving, compared to Deliberation
Shi et al. (2020)	Helping situations	Deliberation, compared to Intuition, <i>decreases</i> helping, especially for subjects with low Honesty-Humility (Study 1) or proself subjects (Study 2)
Nockur & Pfattheicher (2021)	DG	Intuition, compared to Baseline, <i>decreases</i> giving among people low in Honesty-Humility, and has <i>no effect</i> among people high in Honesty-Humility
Ego depletion		
Briers et al. (2006)	DG	Hunger <i>decreases</i> giving
Anderson & Dickinson (2010)	DG	Sleep deprivation has <i>no effect</i> on giving
Aarøe & Petersen (2013)	DG	Drinking Sprite (containing carbohydrates) has <i>no effect</i> on giving, compared to drinking Sprite zero (an artificial sweetener)
Halali et al. (2013)	DG	Depletion <i>decreases</i> frequency of equal split
Xu et al. (2014)	DG	Depletion <i>decreases</i> giving
Achtziger et al. (2015)	DG	Depletion <i>decreases</i> giving
Ferrara et al. (2015)	DG	Sleep restriction <i>decreases</i> giving, but only for females
Harel & Kogut (2015)	DG charity	Hunger <i>decreases</i> giving to a charity that help people to purchase food
Banker et al. (2017)	DG	Depletion has <i>no effect</i> on giving, it only makes participants more likely to choose the status quo

	Measure	Main result
Dickinson & McElroy (2017)	DG	Sleep restriction <i>decreases</i> giving
Rantapuska et al. (2017)	DG charity	Hunger has <i>no effect</i> on giving
Itzhakov et al. (2018)	DG charity	Depletion <i>decreases</i> giving, but only when giving is accompanied by a persuasion message
Häusser et al. (2019)	DG and a volunteering task	Hunger has <i>no effect</i> on giving and volunteering
Holbein et al. (2019)	DG charity	Sleep restriction <i>decreases</i> giving
Wang et al. (2019)	Helping in high-crisis scenarios	Depletion <i>increases</i> helping in high-crisis scenarios
2-response paradigm		
Bago et al. (2021)	DG	Deliberation has <i>no effect</i> on altruism

Given the number of studies, also in this behavioural domain I will primarily focus on two meta-analyses and some key references. Rand, Brescoll, Everett, Capraro and Barcelo (2016) meta-analyzed 22 dictator game experiments (total N = 4,366) and found that cognitive processing does not effect altruistic behaviour. The same result was found by Fromell, Nosenzo and Owens (2020), who analysed 60 experiments (total N = 12,574). Since 2020, there have been four more studies, one positive effect of deliberation on giving, two null results, and one negative effect. Therefore, the available research suggests that, overall, cognitive processing plays no major role on altruistic behaviour.

To have a better understanding of heterogeneity across studies, scholars have looked at potential moderators. One variable that has attracted considerable attention, for theoretical reasons, is gender. Previous work suggests that men and women tend to behave according to stereotypes regarding their social roles, with women being more communal and unselfish, and men being more agentic and independent (Eagly, 1987). In agreement with this view, the

experimental research has repeatedly found that women tend to be more altruistic (Engel, 2011; Brañas-Garza, Capraro & Rascón-Ramírez, 2018) and are expected to be more altruistic than men in dictator games (Aguiar, Brañas-Garza, Cobo-Reyes, Jimenez & Miller, 2009; Brañas-Garza, Capraro & Rascón-Ramírez, 2018), and that, when women behave in a way that is perceived to be insufficiently altruistic, they are liked less, less likely to be helped, hired, promoted, paid fairly, and given status (Heilman & Okimoto, 2007). Therefore, to the extent to which Type 1 reactions reflect internalized strategies that people quickly access when they have no opportunity to reason about the specific situation at hand, one might expect that intuition promotes altruism for women but not for men. Following this idea, in their meta-analysis Rand et al. (2016) studied the moderating role of gender and found that, in line with their prediction, Type 1 promotes altruism for women but not for men. Moreover, in line with the aforementioned interpretation, in a second study published in the same article, the authors reported that, the more women described themselves using masculine attributes relative to feminine attributes, the more Type 2 reduced their altruistic behaviour. This suggests that the specific negative effect of deliberation on altruism among women was partly explained by identification with the other gender. This result however, was not fully replicated by Fromell et al. (2020), who did find an interaction between gender and cognitive mode, but in a different direction than Rand et al. (2016). Specifically, Fromell and colleagues found that promoting Type 1 has no effect on women, whereas it has a marginally significant negative effect on men. In sum, two meta-analyses of the cognitive basis of altruistic behaviour agree on the existence of a differential effect on women and men, but they disagree on the main effects: Rand et al. (2016) argue that Type 1 favours altruism for women but not for men; Fromell et al. (2020) argue that Type 1 disfavors altruism for men but not for women.

Scholars have also investigated whether the effect of cognitive mode on altruistic behaviour is moderated by individual measures of prosociality. Cornelissen et al. (2011) found that cognitive load favours dictator game giving, but only among prosocial people; by contrast, non-cognitively loaded prosocial people gave as much as non-cognitively loaded individualists. By contrast, Shi et al. (2020) reported that inducing gut reactions reduces altruistic behaviour of both prosocial and prosocial individuals, although more so for prosocial individuals. The evidence is mixed also when using, as an individual measure of prosocial predispositions, Honesty-Humility (HH; a personality factor from the HEXACO model of personality, that has been associated with various prosocial behaviours, including altruism; Thielmann et al., 2020). Specifically, Shi et al. (2020) found that people high in HH are not affected by a conceptual prime of intuition vs deliberation, whereas people low in HH tend to decrease altruistic behaviour under deliberation. However, also these results were not fully replicated in subsequent research. Nockur and Pfattheicher (2021) confirmed the null effect among people high in HH, but found that inducing intuitive decisions, compared to control, decreases giving among people low in HH.

Outlook and open problems

To summarize, two meta-analyses agree that intuition and deliberation have no overall effect on altruistic behaviour. However, research points towards a potential moderating role of gender as well as of individual measures of prosociality, but the details of these moderations are still poorly understood.

Another question that deserves further attention regards the role of default options. In general, promoting intuition may increase status-quo bias, because the default option may work as an anchor that is quickly accessible without reflection (Tversky & Kahneman, 1974; Samuelson & Zeckhauser, 1988). In the standard dictator game, giving nothing is the natural

default option. This raises the issue whether promoting intuition in the dictator game may in some cases increase self-interest not because it increases self-interest per se, but because self-interest happens to be the default option. Banker et al. (2017) asked this question and found that ego-depletion does not affect altruistic behaviour, but simply makes people more likely to choose the status-quo, whichever that is. Gärtner (2018) addressed the same question using time constraints and found that prosocial and proself status-quo affect altruistic behaviour, but without interacting with time pressure. However, the results trend in the same direction as those of Banker and colleagues, suggesting that promoting intuition may indeed favour the default option in the dictator game. Future work could shed further light on this point.

The cognitive basis of truth-telling

The conflict between lying and truth-telling is at the core of all social and economic interactions that include communication with asymmetric information. In these cases, people may be tempted to misreport their private information for their benefit. Both empirical research and anecdotal experience suggest that a significant proportion of people do indeed take advantage of their private information and use it for their own benefit. As a consequence, lying has a large negative impact on people, businesses, and society as a whole. For example, tax evasion costs about 100 billion a year only to the US government (Gravelle, 2009). Understanding whether intuition favour or disfavour truth-telling is therefore a topic of primary importance.

Measures of lying

Behavioural scientists have introduced several ways to measure people's tendency to misreport the truth through laboratory experiments. These measures can be classified in two

types: measures in which lying harms an “abstract” person (e.g., the experimenter), and measures in which lying harms a “concrete” person (e.g., another participant in the experiment).¹³

Lying tasks (lying harms “abstract others”)

A simple way to measure people's (dis-)honesty in situations in which lying harms an abstract person is to let participants complete a task and then pay them according to the self-reported performance in the task. In this case, the abstract person harmed by the lie is the experimenter.

A popular task that has been used for this purpose is the so-called “matrix task” (Mazar, Amir & Ariely, 2008). In the original version, participants are presented with a series of twenty 4x3 matrices, whose entries are three-digit numbers between 0 and 10 (e.g. 1.34). Participants are given four minutes to find two numbers per matrix that add up to 10. At the end of the experiment, participants are paid according to the self-reported performance in the task. There are other tasks that serve the same purpose. For example, Muraven, Pogarsky and Shmueli (2006) used logic puzzles that were modified in such a way to be unsolvable; Zhong (2011) used maths problems; Pitesa, Thau and Pillutla (2013) used self-reported performance in a Raven test.

Another popular lying task is the dice-under-cup task (or dice-rolling task). Here, participants roll a dice in private (under a cup), and then are paid according to the reported outcome (Fischbacher & Föllmi-Heusi, 2013).

The sender-receiver game (lying harms “concrete others”)

The case in which lying harms concrete others is typically considered through the sender-receiver game (Gneezy, 2005). In this game, the *sender* and the *receiver* are informed that there are two possible allocations of money; however, only the sender is informed about the exact monetary payoffs corresponding to these options. Then, the sender sends a message to the

receiver, chosen among two possible messages: Message A: “Option A gives you a better payment than Option B”; Message B: “Option B gives you a better payment than Option A”. After receiving the message, the receiver decides which of the two options to implement as a payment.

The sender-receiver game generates a potential confound, known as “sophisticated deception” (Sutter, 2009): there might be senders who tell the truth not because they want to be honest, but because they want to deceive the receiver, but, at the same time, they believe that the receiver will not believe them. In order to eliminate this confound, scholars have developed variants of the sender-receiver game in which either the receiver does not make any decision (Biziou van Pol, Haenen, Novaro, Occhipinti-Liberman & Capraro, 2015) or their decision has no effect on the sender’s payoff (Cappelen, Sørensen and Tungodden, 2013; Gneezy, Rockenbach & Serra-Garcia, 2013).

Review of the empirical literature

I refer to Table 3 for a short summary of each paper investigating the cognitive basis of truth-telling.

Table 3. List of experimental studies exploring the cognitive basis of truth-telling. SR stands for sender-receiver game; DR stands for die-rolling task; SSC stands for self-serving cheating task.

	Measure	Main result
Time constraint		
Gunia et al. (2012)	SR	Delay <i>increases</i> truth-telling, compared to Baseline
Shalvi et al. (2012)	DR	Pressure <i>decreases</i> truth-telling, compared to Baseline
Foerster et al. (2013)	DR	Pressure <i>increases</i> truth-telling, compared to Baseline

	Measure	Main result
Capraro (2017)	SR	Pressure <i>increases</i> truth-telling, compared to Delay
Andersen et al. (2018)	SSC	Delay has <i>no effect</i> on truth-telling, compared to Baseline
Lohse et al. (2018)	SSC	Pressure, compared to Delay, <i>increases</i> truth-telling, by making participants less aware of the opportunity to lie
Capraro et al. (2019a)	SR	Pressure <i>increases</i> truth-telling, compared to Delay
Van der Cruyssen et al. (2020)	DR	Pressure has <i>no effect</i> on truth-telling, compared to Baseline
Cognitive load		
van't Veer et al. (2014)	DR	Load <i>increases</i> truth-telling
Mitkidis et al. (2022)	SSC	Load has <i>no effect</i> on truth-telling
Reis et al. (2022)	DR	Load <i>increases</i> truth-telling in the gain dimension; load <i>decreases</i> truth-telling in the loss dimension
Conceptual primes		
Zhong (2011)	SR	Emotion <i>increases</i> truth-telling, compared to Deliberation
Cappelen et al. (2013)	SR Pareto lies	Intuition <i>increases</i> truth-telling, compared to Deliberation, but only for males
Bereby-Meyer et al. (2020)	DR	Second language <i>decreases</i> truth-telling
Ego depletion		
Muraven et al. (2006)	SSC	Ego depletion <i>decreases</i> truth-telling
Mead et al. (2009)	SSC	Ego depletion <i>decreases</i> truth-telling
Gotlib & Converse (2010)	SSC	Ego depletion <i>decreases</i> truth-telling
Gino et al. (2011)	SSC	Ego depletion <i>decreases</i> truth-telling
Pitesa et al. (2013)	SSC	Ego depletion <i>decreases</i> truth-telling, but only when

	Measure	Main result
Welsh et al. (2014)	SR	consequences on others are not salient Ego depletion has <i>no effect</i> on truth-telling
Yam et al. (2014)	SSC	Ego depletion <i>decreases</i> truth-telling, but only when there is low social agreement that the dishonest action is immoral
Chiou et al. (2017)	SSC only males	Ego depletion <i>decreases</i> truth-telling
Yam (2018)	SSC	Ego depletion <i>decreases</i> honesty
Dickinson & Masclet (2021)	DR and SSC	Sleep restriction <i>decreases</i> truth-telling

Köbis, Verschuere, Bereby-Meyer, Rand and Shalvi (2019) meta-analysed all the studies manipulating cognitive process during a task involving lying and looked at variability in the proportion of liars (73 studies, N = 12,711) and in the magnitude of lying (50 studies, N = 6,473). In doing so, they found that promoting Type 1 processing increases the proportion of liars and the magnitude of lying in tasks in which lying harms abstract others, but not in tasks in which lying harms concrete others. In the latter case, promoting Type 1 over Type 2 has no effect on dishonest behaviour. Köbis and colleagues interpreted these results in light of the Social Heuristics Hypothesis: when lying harms abstract others, the negative consequences of lying are not salient and participants tend to act in a self-interested way;¹⁴ in contrast, when lying harms concrete others and the negative consequences of lying are salient, self-interest conflicts with the intuitive social heuristics “don’t harm”, that pushes in the opposite direction, and nullifies the effect of promoting intuition on dishonesty. Two more studies were published after the meta-analysis, both of them confirmed its result that inducing intuition increases lying in cases when lying harms abstract others (Bereby-Meyer et al. 2020; Dickinson & Masclet, 2021).

Outlook and open problems

In sum, the available evidence suggests that promoting intuition increases dishonesty when lying harms abstract others, but not when lying harms concrete others, in which case intuition does not have any effect on dishonesty.

However, it is important to note that, in most experiments, participants could calculate (or easily deduce) the payoff maximizing strategy before the start of the cognitive manipulation. For example, in the sender-receiver game, the sender is informed about the payoffs associated with the available options before the cognitive manipulation. This has two consequences. First, participants can in principle make a decision before the cognitive manipulation. Second, the logic of the Social Heuristics Hypothesis cannot be applied to this case. According to the SHH, people bring heuristics learned in their everyday life to the laboratory. They use these heuristics when they are in a situation where they are depleted of the cognitive resources needed to compute the payoff maximizing strategy. Therefore, if the payoff maximizing strategy is already known, there is no room for the SHH. Motivated by this observation, two studies implemented a variant of the sender-receiver game in which participants were not initially informed about the payoff-maximizing option, but they had to infer it during the cognitive manipulation (Capraro, 2017; Capraro et al. 2019a). Both these studies found evidence that time pressure *increased* truth-telling. Lohse et al. (2018) obtained a conceptually similar result: they found that time pressure increased truth-telling when people were not initially aware of the possibility of lying. These results provide a first piece of evidence that, when participants do not have access to the payoff maximizing option, they tend to act honestly. Applying the logic of the social heuristics hypothesis, this may happen because truth-telling is payoff maximizing in the long-term, and therefore people internalize it as a useful heuristic. However, more work should be done to validate this theory by using appropriate methods, such as implementing lying tasks where the

cognitive process manipulation is crossed with a manipulation of the moment in which participants are informed about the payoff maximizing option.

Another important line of future research regards the consequences of lying. Virtually all studies explored the cognitive underpinnings of black lies (lies that benefit the sender at a cost to the receiver). However, in reality, lies may have different consequences. For example, spiteful lies harm both the sender and the receiver, altruistic white lies help the receiver at a cost to the sender, Pareto white lies help both the sender and the receiver. Yet, there is very little research on the cognitive basis of these types of lies. To the best of my knowledge, only one study explored the cognitive basis of Pareto white lies (Cappelen et al, 2013), while another one investigated altruistic white lies (Cantarero & Van Tilburg, 2014). Exploring the cognitive basis of lies other than black lies is important for understanding the full range of cognitive processes involved in deception. Pareto white lies in particular are of great theoretical interest, because they represent the setting in which one can measure *intrinsic* lying aversion, depurated from any other confound (e.g. in the case of black lies, telling the truth coincides with being altruistic). It is noteworthy that the only study that manipulated cognitive process in Pareto white lie situations found that promoting intuition increases lying aversion, but only among men (Cappelen et al. 2013).

The cognitive basis of reciprocity

In the previous sections, I reviewed the cognitive basis of human sociality in situations where there is only one decision-maker (dictator game, lying tasks) or multiple decision-makers who make decisions simultaneously (social dilemmas) or without knowledge of each other's payoff-maximizing strategy (sender-receiver games). However, in reality, many interactions involve an element of reciprocity, where the first mover chooses between a self-regarding and an

other-regarding option, knowing that the second mover or a third-party may punish or reward their choice. This type of interaction will be the focus of this section.

Measures of reciprocity

Measures of negative reciprocity

Negative reciprocity refers to situations in which a person has the choice to pay a cost to decrease the payoff of a decision-maker in response to a choice made by the decision-maker themselves. There are two ways to measure negative reciprocity. One is through the second player in the ultimatum game (see below), the other consists of social dilemmas followed by a punishment phase. In this case, the punisher might or might not be the target of the initial action. Situations in which the punisher is the target of the initial action take the name of second-party punishment; situations in which the punisher is not the target of the initial action take the names of third-party punishment. Below, I describe these two kinds of interactions in more details.

Second-party punishment

In the ultimatum game, Player 1, the *proposer*, has to propose how to divide a sum of money between themselves and Player 2, the *responder*. The responder can either accept or reject the offer. In case the responder accepts the offer, then the sum of money is split between the two players as agreed; in case the responder rejects the offer, neither the proposer nor the responder receives any money. Therefore, rejecting the proposer's offer corresponds to incurring a cost (equal to the offer received) to decrease the proposer's payoff.

Another way of measuring second-party punishment is by means of a social dilemma followed by a punishment phase, in which participants can pay a cost to decrease the payoff of other players. Various implementations are possible. For instance, as a social dilemma, instead of the public goods game, one can use the prisoner's dilemma; or, in the punishment phase,

punishers can choose to punish all players indiscriminately, rather than targeting a specific player.

Third-party punishment

Third-party punishment is similar to second-party punishment, with the difference that the punisher is not the target of the first action but only observes the behaviour of other participants, and then decides whether to punish those participants or not, based on their behaviour.

Measures of positive reciprocity

Positive reciprocity occurs when a person incurs a cost to increase the payoff of a decision-maker in response to their choice. There are two types of positive reciprocity: one in which the rewarder is the target of the decision-maker's choice and one where they are not.

Second-party rewarding

In the trust game, there are two players: Player 1 (the investor) and Player 2 (the investee). Player 1 has to decide whether to transfer a sum of money to Player 2. Any amount invested is multiplied by a factor greater than 1 (typically equal to 3) and given to Player 2. Player 2, in turn, has to decide how much, if any, to return to Player 1. This means that returning money incurs a cost (equal to the amount returned) to increase the payoff of Player 1.

Another way of measuring second-party rewarding is through a social dilemma followed by a rewarding stage. In this scenario, players can pay a cost to increase each other's payoff in response to the social dilemma interaction.

Third-party rewarding

Third-party rewarding differs from second-party rewarding in that the rewarder is not the recipient of the action to be rewarded, but an observer.

Measures of expectation of reciprocity

The complementary notion of reciprocity is the expectation of reciprocity, where one person makes a decision with the knowledge that someone else will respond to it, either by punishing or rewarding it. This concept can be divided into two parts: expectations of negative reciprocity and expectations of positive reciprocity. Expectations of reciprocity can be measured through the ultimatum game (player 1) and the trust game (player 1), as well as social dilemmas followed by a punishment or rewarding stage.

Review of the empirical evidence on negative reciprocity

Table 4 reports the list of papers exploring the cognitive basis of negative reciprocity.

Table 4. List of experimental studies exploring the cognitive basis of negative reciprocity. UG stands for ultimatum game; IG for impunity game; IPG for iterated prisoner's dilemma; SG for steal game.

	Measure	Main result
2P-punishment		
Time constraint		
Cappelletti et al. (2011)	UG responder	Pressure <i>increases</i> rejection of low offers, compared to Delay
Grimm & Mengel (2011)	UG responder	Delay <i>decreases</i> rejection of low offers, compared to Baseline
Wang et al. (2011)	UG responder	Delay <i>decreases</i> rejection of low offers, compared to Baseline
Neo et al. (2013)	UG responder	Delay <i>decreases</i> rejection of low offers, compared to Baseline
Hochman et al. (2015)	UG responder	Pressure <i>decreases</i> rejection rates, compared to Baseline
Oechssler et al. (2015)	UG responder	Delay, compared to baseline, <i>decreases</i> rejections rates when offers

	Measure	Main result
		are made using lottery tickets and not when using cash
Balafoutas & Tarek (2018)	IG responder	Pressure has <i>no effect</i> rejection rates
Cognitive load		
Cappelletti et al. (2011)	UG responder	Load has <i>no effect</i> on rejection of low offers
Olschewski et al. (2018)	UG responder	Load has <i>no effect</i> on rejection of low offers
Ego depletion		
Anderson & Dickinson (2010)	UG responder	Sleep restriction <i>increases</i> minimum acceptable offers
Crockett et al. (2008)	UG responder	Serotonin depletion <i>increases</i> rejection rates of unfair offers
Crockett et al. (2010a)	UG responder	Serotonin depletion <i>increases</i> rejection rates of unfair offers
Crockett et al. (2010b)	UG responder	Enhancing serotonin <i>decreases</i> rejection rates of unfair offers
Halali et al. (2014)	UG responder	Ego depletion <i>increases</i> rejection of low offers
Liu et al. (2015)	UG responder	Ego depletion <i>increases</i> rejection of low offers
Achtziger et al. (2016)	UG responder	Ego depletion <i>decreases</i> rejection of low offers
Achtziger et al. (2018)	UG responder	Ego depletion has <i>no effect</i> on rejection of low offers
2-response paradigm		
Bago et al. (2021)	UG responder	Deliberation has <i>no effect</i> on rejection rates: most rejection choices under deliberation are already rejection choices under intuition
3P-punishment		
Time constraint studies		
Wang et al. (2011)	TG with punishment	Pressure <i>increases</i> punishment compared to Delay
Artavia-Mora et al. (2017)	Field experiment	Pressure has <i>no effect</i> on punishment, compared to Delay

	Measure	Main result
Cognitive load		
Yudkin et al. (2016)	SG with punishment	Load makes people punish outgroup members more severely than ingroup members
Ego depletion		
Liu et al. (2015)	UG with punishment	Ego depletion <i>increases</i> punishment of unfair offers

To the best of my knowledge, no meta-analysis has been conducted on the studies investigating the effect of promoting Type 1 processing on negative reciprocity (see Hallsson, Siebner & Hulme (2018) for a review). To address this gap, I computed the standardized mean differences and 95% confidence intervals for each study, and then I conducted a random-effects meta-analysis. The results indicate a significant overall effect, with an increase in negative reciprocity observed when promoting Type 1 processing ($b = 0.107$, $z = 3.559$, $p < 0.001$). The forest plot of the meta-analysis is presented in Figure 1. Although I was not able to obtain standardized mean differences for three papers (Achtizer et al., 2006; 2008; Crockett et al., 2010a), all of these experiments reported similar results, demonstrating that promoting Type 1 processing increases negative reciprocity. Therefore, the inclusion of these studies does not affect the qualitative result.

To test for small-study effects, which refer to the possibility of publication bias, I use Egger's regression test. This test regresses standardized coefficients over standard errors, and the results provide evidence of a small-study effect ($b = 1.88$, $z = 2.30$, $p = 0.021$). This indicates that better-powered studies tend to have smaller coefficients, suggesting an overreporting of small studies with significant results. The funnel plot, presented in Figure 2, provides visual evidence of this effect. To account for publication bias, I use the trim-and-fill method (Duval & Tweedie, 2000). This method estimates the number of missing studies, imputes them, and then

computes a new meta-analytic effect with the imputed studies. However, no missing studies were found by the algorithm, providing evidence that, if publication bias is present, it has little influence on the overall effect size.

Finally, it is worth noting that there is significant heterogeneity across studies; the proportion of variance due to between-study heterogeneity is estimated to be 75.3% ($Q = 105.44$, $df = 26$, $p < 0.001$). This suggests the presence of one or more moderators. One could be the cognitive manipulation. Grouping studies by cognitive manipulation, I indeed find significant heterogeneity between groups ($Q = 28.90$, $df = 3$, $p < 0.001$). The overall effect is driven by studies using time constraints ($b = 0.275$, $z = 5.671$, $p < 0.001$) and ego depletion ($b = 0.346$, $z = 2.878$, $p = 0.004$), while it is not statistically significant for studies using cognitive load ($p = 0.953$) and the two-response paradigm ($p = 0.288$). As far as I can say, previous work did not focus much on other moderators, with one notable exception. Yudkin et al. (2016) found that promoting Type 1 increases punishment towards outgroup members, but has the opposite effect on punishment of ingroup members. This suggests that punishing ingroup members for breaking norms requires more deliberate thought than punishing outgroup members, who are viewed more automatically as outsiders. This may be linked to the fact that people may have an inherent preference for their own group, which is supported by emotions (Joseph & Haidt, 2004).

Figure 1. Random-effects meta-analysis of the studies testing the effect of cognitive manipulations on negative reciprocity.

Figure 2. Funnel plot of the studies testing the effect of cognitive manipulations on negative reciprocity.

Review of the empirical evidence on positive reciprocity

Table 5 reports the list of papers, including a short description for each of them.

Table 5. List of experimental papers exploring the cognitive basis of positive reciprocity. TG stands for trust game; PGG for public goods game.

	Measure	Main result
Time constraint		
Neo et al. (2013)	TG investee	Delay has <i>no effect</i> on return rates, compared to Baseline
Cabrales et al. (2022)	TG investee	Delay <i>has no effect</i> on return rates, compared to Pressure
Conceptual primes		
Urbig et al. (2016)	Sequential PGG	Second language <i>decreases</i> reciprocation of high contributions
Cognitive load		
Bogliacino & Gómez (in prep)	TG investee	Load has <i>no effect</i> on return rates
Ego depletion		
Halali et al. (2014)	TG investee	Ego depletion <i>increases</i> return rates
Dickinson & McElroy (2017)	TG investee	Sleep restriction <i>decreases</i> return rates
Rantapuska et al. (2017)	TG investee	Hunger <i>increases</i> return rates

As far as I am aware, currently there is no meta-analysis of studies testing the effect of cognitive manipulations in the domain of positive reciprocity (see Hallsson et al. (2018) for a review). To address this gap, I conducted a random-effects meta-analysis of standardized mean differences and 95% confidence intervals. The results reveal that, overall, the effect of promoting Type 1 processing on positive reciprocity is not statistically different from zero ($b = -0.013$, $z = -0.274$, $p = 0.784$). See Figure 3 for the forest plot. Furthermore, the Egger's test finds no evidence of publication bias ($b = -0.54$, $z = -0.33$, $p = 0.745$); see Figure 4 for the funnel plot. However, there is significant heterogeneity across studies ($Q = 27.99$, $df = 8$, $p = 0.001$), suggesting the existence of one or more moderators. One potential moderator is cognitive manipulation, which indeed accounts for a statistically significant heterogeneity ($Q = 14.41$, $df = 3$, $p = 0.002$). Worth of interest is the observation that studies using ego-depletion produced, meta-analytically, a positive effect of System 1 processing on positive reciprocity ($b = 0.277$, $z = 3.063$, $p = 0.002$), whereas studies using time constraints produced, meta-analytically, the opposite effect, although marginally significantly ($b = -0.108$, $z = -1.688$, $p = 0.091$). Given the small number of studies per cognitive manipulation, however, these results should be taken, at most, as a starting point for future investigations.

Figure 3. Forest plot of the random-effects meta-analysis of the studies testing the effect of cognitive manipulations on positive reciprocity.

Figure 4. Funnel plot of the studies testing the effect of cognitive manipulations on positive reciprocity.

Review of the empirical evidence on expectations of negative reciprocity

Table 6 reports the list of papers investigating the cognitive underpinnings of expectations of negative reciprocity, including a short description for each of them.

Table 6. List of experimental studies exploring the cognitive basis of expectations of negative reciprocity. UG stands for ultimatum game; PGG for public goods game; DG for dictator game.

	Measure	Main result
Time constraint studies		
Cappelletti et al. (2011)	UG proposer	High pressure (15s) <i>increases</i> offers, compared to Low pressure (180s)
Grimm & Mengel (2011)	UG proposer	Delay, compared to Baseline, has <i>no effect</i> on offers
Neo et al. (2013)	UG proposer	Delay, compared to Baseline, has <i>no effect</i> on offers
Ego depletion studies		
Halali et al. (2013)	UG proposer	Depletion <i>increases</i> proportion of fair offers
Achtziger et al. (2016)	UG proposer	Depletion <i>increases</i> offers
Achtziger et al. (2018)	UG proposer	Depletion <i>decreases</i> offers

To shed light on the overall effect, I conducted a random-effects meta-analysis of standardized mean differences and 95% confidence intervals. The results unveil a statistically significant effect, such that promoting Type 1 processing increases expectations of negative reciprocity ($b = 0.117$, $z = 2.019$, $p = 0.044$), which reflects the positive effect of promoting Type 1 processing on negative reciprocity reported before. See Figure 5 for the forest plot. The Egger's test find no evidence of publication bias ($b = 0.78$, $z = 0.60$, $p = 0.549$). See Figure 6 for the funnel plot. Furthermore, there is evidence of significant heterogeneity across studies ($Q = 28.94$, $df = 10$, $p = 0.001$), suggesting the existence of one or more moderators. However, this

time the heterogeneity due to cognitive manipulations is not statistically significant ($p = 0.297$).

Exploring other potential moderators is therefore an interesting direction for future research.

Figure 5. Random-effects meta-analysis of the studies testing the effect of cognitive manipulations on expectations of negative reciprocity

Figure 6. Funnel plot of the studies testing the effect of cognitive manipulations on expectations of negative reciprocity

Review of the empirical evidence on the expectations of positive reciprocity

Table 7 includes a comprehensive list of papers that feature an experiment manipulating Type 1 or Type 2 cognitive processes in participants who are required to make decisions with the knowledge that their choices may be rewarded from another participant.

Table 7. List of experimental studies exploring the cognitive basis of expectations of positive reciprocity. TG stands for trust game; PGG stands for public goods game.

	Measure	Main result
Time constraint studies		
Neo et al. (2013)	TG investor	Delay, compared to Baseline, has <i>no effect</i> on trust
Jaeger et al. (2019)	TG investor	Pressure <i>increases</i> trust compared to Delay
Ego depletion studies		
Anderson & Dickinson (2010)	TG investor	Sleep deprivation <i>decreases</i> trust
Evans et al. (2011)	TG investor	Ego depletion <i>has no effect</i> on trust, but it makes people more likely to follow the default
Ainsworth et al. (2014)	TG investor	Ego depletion <i>decreases</i> trust
Dickinson & McElroy (2017)	TG investor	Sleep deprivation <i>decreases</i> trust
Rantapuska et al. (2017)	TG investor	Hunger has <i>no effect</i> on trust
Cognitive load studies		
Bogliacino & Gómez (in preparation)	TG investor	Load has <i>no effect</i> on trust

To estimate the overall effect, I conducted a random-effects meta-analysis of the standardized mean differences and 95% confidence intervals computed at the treatment-level. It appears that, overall, Type 1 processing promotes expectations of positive reciprocity (overall $\text{coeff} = 0.145$, $z = 3.417$, $p = 0.001$). However, not only the heterogeneity across studies is significant ($Q = 78.75$, $df = 11$, $p < 0.001$), but the overall effect appears to be entirely driven by

a single treatment, with a relatively larger power, compared to the other ones (Jaeger et al. 2019; Study 4, high temptation condition). Removing this study, the meta-analytic effect loses statistical significance and even trends in the opposite direction (overall coeff = -0.109, $z = -1.5$, $p = 0.134$). See Figure 7. Moreover, the Egger's test of all the studies reveals a significant small-study effect ($b = -2.81$, $z = -2.55$, $p = 0.011$). The trim-and-fill method estimates three missing studies. Calibrating for these studies, the meta-analytic effect loses statistical significance ($b = 0.041$, 95% CI = [-0.245, 0.316]). See Figure 8 for the funnel plot before and after the inclusion of imputed studies. In sum, the available research produces no reliable evidence that cognitive processing influences expectations of positive reciprocity. However, it is possible that an effect exists in specific circumstances, as those highlighted by Jaeger and colleagues (when the investee in a trust game has a high temptation to return no money). Future work could shed light on this and others potential moderators.

Figure 7. Random-effects meta-analysis of the studies on the cognitive basis of expectations of positive reciprocity.

Figure 8. Funnel plot of the studies on the cognitive basis of expectations of positive reciprocity, including also the missing studies estimated by the trim-and-fill method (reported in orange).

Outlook and open problems

To summarize, the evidence regarding the effect of cognitive processes manipulations on negative reciprocity is clear as most studies report a positive effect of Type 1 processing on both second- and third-party punishment. This effect is also reflected on the behaviour of first-players, as Type 1 processing increases also expectations of negative reciprocity. At the same time, however, little is known about the role of potential moderators. Both in the case of negative reciprocity and expectations of negative reciprocity, the meta-analyses revealed significant heterogeneity across studies. Although part of this heterogeneity may be explained by the specific experimental manipulation used to affect cognitive processing (especially in the case of negative reciprocity), this is hardly the end of the story. For example, Yudkin et al. (2016) suggested that whether the person to be punished is an ingroup member or an outgroup member may affect the intuitiveness of negative reciprocity. Further work could focus on these and other moderators.

Regarding positive reciprocity, the meta-analyses suggested that, overall, cognitive processing has little effect on positive reciprocity and expectations of positive reciprocity. However, a closer look at the results showed that, underneath these null or weak effects, there is some complexity that need to be understood. The effect of Type 1 processing on positive reciprocity appeared to be highly dependent on the cognitive manipulation, to the point that ego depletion studies increased positive reciprocity, while time pressure studies marginally decreased positive reciprocity. More experiments may help clarify this point.

The literature reviewed in the sections on negative reciprocity pertains to prosocial punishment, which refers to the punishment of antisocial behaviours. However, research has also

shown that people sometimes punish prosocial behaviours (Herrmann, Thoni, & Gächter, 2008). As far as I know, only one study has explored the cognitive basis of antisocial punishment. Pfattheicher, Keller, and Knezevic (2017) found that when compared to a baseline condition, conceptual priming of intuition increases antisocial punishment in a series of one-shot public goods games followed by a punishment stage. In a subsequent study published in the same paper, Pfattheicher and colleagues also found that inhibiting intuition decreases antisocial punishment, but activating or inhibiting deliberation did not have the same effect. These results provide initial evidence suggesting that Type 1 processing drives antisocial punishment.

The cognitive basis of act utilitarianism

Many choices involve a moral component. Philosophers and psychologists have been interested in how people make decisions in such situations for centuries. This has led to two main traditions: consequentialism and deontology. Consequentialist moral theories hold that an action's rightness or wrongness depends solely on its consequences. One popular consequentialist theory is act utilitarianism, which states that "actions are right in proportion as they tend to promote happiness, wrong as they tend to produce the reverse of happiness" (Mill, 1863; Bentham 1789/1983). Consequentialism can be logically objected in many ways. The primary non-consequentialist theory discussed in contemporary research is deontology, which holds that an action's morality depends on whether it respects certain rights or fulfils certain duties, independently of its consequences (Kant, 1797/2002). Specifically, research on the dual-process approach to moral judgments has focused on the "don't harm" proscription. This because situations where people must decide whether it is acceptable to inflict harm on an innocent person to save a greater number of people are thought to generate a conflict between Type 2 cost-

benefit reasoning (“save more lives”) and Type 1 emotional responses (“don’t harm!”) (Greene et al. 2001).

Measures of the conflict between deontology and consequentialism

Personal moral dilemmas

The conflict between the deontological proscription of “don’t harm” and the consequentialist maxim of “save more lives” has been traditionally assessed through personal moral dilemmas. These are hypothetical scenarios where participants must decide whether it is morally acceptable to cause direct harm to one person to save a greater number of people. One of these dilemmas, considered among others in Greene et al.’s (2001) pioneering work, is the “footbridge dilemma”. In this scenario, a runaway trolley is heading towards five people, and the only way to save them is by pushing a large stranger off a footbridge, causing his death but stopping the trolley. When faced with such dilemmas, almost everyone experiences a conflict between cost-benefit reasoning (“better to save more lives”) and a negative emotional response to harming an innocent person (“don’t push!”). Thus, these dilemmas offer a *within-subject* measure of the tension between deontology and consequentialism. Even if everyone made the same choice, each person would still experience internal tension. Therefore, personal moral dilemmas are particularly useful for dissociating cognitive processes within individuals.

The process-dissociation approach

In recent years, moral psychologists have adapted personal moral dilemmas, which were originally designed to dissociate cognitive processes within individuals, to measure individual differences between people. Indeed, personal moral dilemmas have limitations as a between-subject measure, as they cannot distinguish between individuals who may appear utilitarian due

to a lack of emotional response, such as psychopaths, from those who genuinely seek to maximize total welfare (Patil et al. 2021).

To overcome this limitation, Conway and Gawronski (2013) proposed an approach based on Jacoby's (1991) process dissociation procedure. The starting observation of Conway and Gawronski was that classical personal moral dilemmas treat deontology and act utilitarianism as inversely related. For example, in the footbridge dilemma mentioned earlier, choosing the deontological option also means rejecting the utilitarian option, and vice versa.

To address this, Conway and Gawronski compared responses to *incongruent dilemmas*, in which the deontological and utilitarian responses are misaligned, to responses to *congruent dilemmas*, in which they are aligned. An example of a congruent dilemma is torturing a person to prevent a paint bomb from exploding, while an example of an incongruent dilemma is torturing a person to prevent a pipe bomb from exploding. By comparing responses to these two types of dilemmas, the relative influence of deontological and utilitarian inclinations can be determined. This method allows one to estimate separately, within a single person, their deontological and utilitarian inclinations. Importantly, these inclinations are distinct and correlated with different factors. Deontological inclinations are associated with empathic concern, perspective-taking, and religiosity, while utilitarian inclinations are associated with a need for cognition (Conway & Gawronski, 2013). In summary, this process dissociation procedure turns personal moral dilemmas into useful measures of individual differences.

A two-dimensional model of act utilitarianism

Kahane et al. (2018) have proposed another approach for measuring individual differences in moral decision-making. Their goal is to sort people according to strictly philosophical criteria, by conceptually defining utilitarianism and then measuring the extent to

which people behave consistently with this definition. Utilitarianism, according to Kahane and colleagues, is defined through two dimensions. The first is inspired by the work of utilitarian philosopher Peter Singer (1979) and requires people to value the well-being of sentient beings impartially, or “impartial beneficence”. The second dimension is inspired by personal moral dilemmas and requires individuals to sacrifice lesser goods for greater goods, or “instrumental harm”. Kahane and his team designed a scale, called the Oxford Utilitarianism Scale (OUS), to measure these two factors and showed that they are distinct in the general population. It is important to note that these factors do not exactly map onto the utilitarian and deontological inclinations as defined by Conway and Gawronski (2013), as the correlates are different.¹⁵

Review of the empirical evidence on the cognitive basis of deontology and utilitarianism

Table 8 lists the experimental papers containing at least one study applying a cognitive manipulation to one of the measures introduced in the previous section, along with a short description of the main result of each paper.

Table 8. List of experimental studies exploring the cognitive basis of deontology vs. act utilitarianism. PMS stands for personal moral dilemmas; OUS for Oxford Utilitarianism Scale.

	Measure	Main result
Time constraint studies		
Ham & van den Bos (2010)	PMD	Delay, compared to Pressure, has <i>no effect</i> on deontological judgments
Houston (2010)	PMD	Pressure has <i>no effect</i> on deontological decisions, compared to Baseline
Suter & Hertwig (2011)	PMD	Pressure <i>increases</i> deontological choices, compared to Baseline
Cummins & Cummins (2012)	PMD	Pressure <i>increases</i> deontological choices, compared to Baseline

	Measure	Main result
Körner & Volk (2014)	PMD	Pressure, compared to Delay, <i>increases</i> deontological decisions, but only when induced to process information concretely
Trémolière & Bonnefon (2014)	PMD varying save/kill	Pressure, compared to Baseline, <i>increases</i> deontological choices for save/kill = 5 but not save/kill = 500
Tinghög et al. (2016)	PMD	Pressure, compared to Delay, has <i>no effect</i> on deontological choices
Rosas & Aguilar-Pardo (2020)	PMD	Pressure, compared to Delay, <i>decreases</i> deontological choices
Kroneisen & Steghaus (2021)	PMD	Pressure, compared to Baseline, has <i>no effect</i> on deontological choices
Cognitive load studies		
Greene et al. (2008)	PMD	Load has <i>no effect</i> on deontological choices
Trémolière & Bonnefon (2014)	PMD varying save/kill	Load has <i>no effect</i> on deontological choices for save/kill = 5 but not save/kill = 500
Conway and Gawronski (2013)	Process dissociation	Load <i>reduces</i> utilitarian judgments, while leaving deontological judgments unaffected
Tinghög et al. (2016)	PMD	Load has <i>no effect</i> on deontological choices
Białek & De Neys (2017)	PMD	Load <i>reduces</i> utilitarian judgments
Li et al. (2018)	Process dissociation	Load <i>reduces</i> utilitarian judgments, but has <i>no effect</i> on deontological judgments
Rosas et al. (2019)	PMD	Load has <i>no effect</i> on deontological judgments
Liu & Liao (2021)	PMD	Load decreases deontological judgments
Conceptual primes studies		

	Measure	Main result
Valdesolo & DeSteno (2006)	PMD	Affect induction <i>increases</i> deontological judgments, compared to Baseline
Suter & Hertwig (2011)	PMD	Fast induction <i>increases</i> deontological judgments, compared to Slow induction
Cummins & Cummins (2012)	PMD	Emotion induction <i>increases</i> deontological judgments compared to Baseline; Cognition induction has <i>no effect</i> compared to Baseline
Paxton et al. (2012)	PMD	Deliberation induction <i>increases</i> utilitarian judgments, compared to Baseline
Conway and Gawronski (2013)	Process dissociation	Emotion induction, compared to Baseline, <i>increases</i> deontological judgments, while leaving utilitarian judgments unaffected
Kvaran et al. (2013)	PMD	Emotional prime <i>increases</i> deontological judgments; analytical prime <i>increases</i> utilitarian judgments
Costa et al. (2014b)	PMD	Second language <i>increases</i> utilitarian judgments
Paxton et al. (2014)	PMD	Inducing Reflection <i>increases</i> utilitarian judgments, compared to Baseline
Geipel et al. (2015a)	PMD	Second language <i>increases</i> utilitarian judgments
Schwitzgebel & Cushman (2015)	PMD	Inducing Reflection along with a 15s time delay <i>increases</i> deontological judgments, compared to Baseline
Cipolletti et al. (2016)	PMD	Second language <i>increases</i> utilitarian judgments
Corey et al. (2017)	PMD	Second language <i>increases</i> utilitarian judgments
Gürcay & Baron (2017)	PMD	Inducing Deliberation, compared to Intuition, has

	Measure	Main result
		<i>no effect</i> on deontological decisions
Hayakawa et al. (2017)	Process dissociation	Second language <i>decreases</i> both deontological and utilitarian judgments
Lyrintzis (2017)	PMD	Inducing Reflection <i>increases</i> utilitarian judgments, compared to Baseline
Muda et al. (2018)	Process dissociation	Second language <i>decreases</i> both deontological and utilitarian judgments
Spears et al. (2018)	PMD	Attention prime has <i>no effect</i> on utilitarian judgments, compared to Baseline
Capraro et al. (2019b)	OUS	Intuition, compared to Deliberation, <i>increases</i> non-utilitarian judgments in the instrumental harm dimension; intuition has <i>no effect</i> on beneficence dimension
Liu & Liao (2021)	PMD	Inducing Deliberation <i>increases</i> utilitarian judgments, compared to Baseline
Ego depletion studies		
Killgore et al. (2007)	PMD	Sleep restriction <i>decreases</i> deontological judgments among people with average emotional intelligence, but has <i>no effect</i> among people with high emotional intelligence
Starcke et al. (2012)	PMD	Inducing stress <i>increases</i> deontological decisions
Trémolière et al. (2012)	PMD	Ego depletion <i>increases</i> deontological judgments
Cellini et al. (2017)	PMD	Lunch nap <i>increases</i> utilitarian judgments
Weippert et al. (2018)	PMD	Physical fatigue has <i>no effect</i> on deontological judgments

	Measure	Main result
Timmons & Byrne (2019)	PMD	Ego depletion <i>increases</i> deontological judgments
Li et al. (2021)	PMD	Inducing stress <i>increases</i> deontological judgments
2-response paradigm studies		
Cummins & Cummins (2012)	PMD	Second response <i>increases</i> utilitarian judgments
Bago & De Neys (2019)	PMD	Most utilitarian deliberative judgments were already utilitarian under intuition

As far as I know, there has been no meta-analysis examining the overall effect of cognitive manipulations on moral judgments in personal dilemmas (Rehren's (2022) meta-analysis does not differentiate between personal and impersonal moral dilemmas). To address this gap, I calculated the standardized mean differences and their 95% confidence intervals at the treatment level and then conducted a random-effects meta-analysis. For studies using the process dissociation approach, I followed the "traditional analysis" that is usually reported in papers using the process dissociation approach (e.g., Conway & Gawronski, 2013), and used only the incongruent dilemmas. For the only study using the OUS (Capraro et al. 2019b), I included only the instrumental harm dimension; I will discuss in the next section the results in the impartial beneficence dimension. The findings indicate that promoting Type 1 processing reduces utilitarian choices overall ($b = -0.135$, $z = -9.170$, $p < 0.001$). The Egger's test shows no indication of publication bias ($p = 0.196$). The trim-and-fill method does find 12 out of 101 missing studies, but even imputing these studies the effect remain abundantly significant ($b = -0.082$, 95% CI = $[-0.110, -0.054]$). See Figure 9 for the forest plot and Figure 10 for the funnel plot with and without imputed studies. However, there is significant heterogeneity across studies ($Q = 538.58$, $df = 87$, $p < 0.001$), which appears to be partly due to the cognitive manipulations. Grouping by cognitive manipulation reveals significant heterogeneity across cognitive

manipulations ($p < 0.001$). The meta-analytic effect is significant and negative for studies using conceptual primes ($b = -0.262$, $z = -12.02$, $p < 0.001$), ego depletion ($b = -0.296$, $z = -4.790$, $p < 0.001$), and cognitive load ($b = -0.069$, $z = -1.998$, $p = 0.046$), while it is significant and positive for studies using time constraints ($b = 0.057$, $z = 2.083$, $p = 0.037$).

In summary, the available research shows that Type 1 processing is associated with deontological judgments in personal moral dilemmas, although there is likely some variability across cognitive manipulations.

Figure 9. Random-effects meta-analysis of the studies on the cognitive basis of utilitarianism using sacrificial dilemmas.

Figure 10. Funnel plot of the studies on the cognitive basis of utilitarianism using sacrificial dilemmas; in orange the missing studies estimated by the trim-and-fill method.

Outlook and open problems

To summarize, the overall evidence suggests that Type 1 processing is associated with deontological judgments in personal moral dilemmas, although there is some variability across cognitive manipulations. Of particular interest is the fact that studies using time constraints appear to show the opposite effect. However, within studies using time constraints, there is high variability, with some reporting a positive effect of promoting Type 1 processing on deontological choices (e.g., Suter & Hertwig, 2011), while others report a negative effect (e.g., Rosas & Pardo, 2020). Therefore, it may not be time constraints per se that have a different effect, but rather some other specifics of the experiment. Future work could help elucidate this point.

Several other moderators are worth exploring as well. For example, Körner & Volk (2014) found that time pressure increases deontological decisions only when participants are induced to process information concretely, rather than abstractly. This may be because the emotional response that drives intuitive deontology is favoured by a concrete representation of the dilemma at hand. Trémolière & Bonnefon (2014) reported that increasing the saved-to-killed ratio not only makes people more utilitarian but also makes them more intuitively utilitarian. This may be because a large saved-to-killed ratio makes the utilitarian option more readily accessible. Understanding the effect of these moderators may be important from a practical perspective because real-life moral dilemmas may appear with very different saved-to-killed ratios or at different levels of concreteness.

Theoretical framework

In this section, I aim to organize the results into a coherent theoretical framework. One possible approach is to generalize the Social Heuristics Hypothesis (SHH), which was originally

introduced by Rand and colleagues (2014) in the domain of cooperative behaviour, to other social decisions. Using the authors' words, the SHH contends that "cooperation is typically advantageous in everyday life, leading to the formation of generalized cooperative intuitions. Deliberation, by contrast, adjusts behaviour towards the optimum for a given situation. Thus, in one-shot anonymous interactions where selfishness is optimal, intuitive responses tend to be more cooperative than deliberative responses".

However, a straightforward extension of the SHH does not explain all the experimental regularities discussed in the preceding sections. This is because any such extension would predict that Type 2 processing increases self-regarding behaviour. But we have seen that Type 2 processing increases honesty (and therefore decreases self-interest) when dishonesty harms abstract others.

Nonetheless, the SHH may not be entirely incorrect. Although this counterexample violates the SHH's predictions regarding the role of Type 2, it does not violate those regarding the role of Type 1. Therefore, the next question is whether a straightforward generalization of the other "half" of the SHH makes predictions that align with the experimental evidence. I propose a generalization of the first "half" of the SHH, which I will denote as GSHH1:

Type 1 processing promotes heuristics that have been internalized in everyday social interactions that resemble the decision under consideration.

In the following subsection, I would like to derive some social heuristics that many people are likely to internalize.

The instinct for self-preservation and its consequences

The starting point is the (obvious) observation that self-preservation is one of the most fundamental animal instincts. All animals, and humans in particular, have developed and

internalized a set of automatic behaviours that preserve them from danger. I would like to argue that this generates, among humans, the evolution of two heuristics.

First, people not only internalize heuristics that protect them from being harmed, but they also internalize heuristics that avoid them to *directly* harm others. This happens because, in real everyday interactions, directly harming others usually come with a cost that in some cases might be large or even huge. The argument is straightforward: in everyday interactions, if we physically harm someone, it is very likely that this person, moved by their own instinct for self-preservation, will fight back, putting ourselves in danger of being harmed. Since our own instinct for self-preservation preserves us from being harmed, then it may make us avoid directly harming others at the first place.

Second, heuristics for self-preservation tend to make people averse to disadvantageous inequalities, and they do so for at least two reasons. At a very basic level, getting less resources than others may threaten the possibility of surviving or reproducing. This might explain why many human and even non-human interactions seem to be guided by expectations about how resources should be divided, to the point that inequity aversion is probably universal among humans (although with some variability; Henrich et al. 2001) and has been observed also among non-human primates (Brosnan & De Wall, 2003). At a slightly less basic level, there is another, probably uniquely-human, reason why inequity aversion may descend from the instinct for self-preservation. When we receive less than others without a good reason, we feel that we have been left behind, that we do not have value, and this threatens our self-esteem, one of the most fundamental psychological mechanisms for self-preservation, so fundamental that some psychologists believe that it descends directly from the uniquely-human awareness of their own mortality (Becker, 1973; Greenberg, Pyszczynski & Solomon, 1986).

In sum, there are at least three heuristics that we may expect many people have internalized: (i) don't get harmed; (ii) don't harm others, especially when they can harm you back; (iii) don't get fewer resources than others. Now I would like to combine these heuristics with the GSHH1 and see which hypotheses can be derived about the effect of Type 1 processing on social decisions, and then check whether these hypotheses are reflected in the experimental evidence.

Hypotheses 1a-1b: The Ultimatum Game

Among UG responders, we may expect that Type 1 processing increases aversion to disadvantageous inequalities and therefore increases rejections of low offers. Among proposer, we may expect that Type 1 processing increases concerns about harming people that have the potential to harm them back, and therefore increases offers. In sum, the GSHH1 predicts that Type 1 processing is positively related with (H1a) rejection rates and with (H1b) offers. The experimental evidence reviewed before provides support for both these predictions.

Hypothesis 2: Comparing the Dictator Game to the Ultimatum Game

The main difference between the DG and the UG, in the context of this analysis, is that the DG recipient has no means to retaliate against the dictator, which means that the dictator can make a small donation without fear of harm. As a result, we might expect the positive effect of Type 1 processing on UG offers to be less pronounced in the DG. In other words, the GSHH1 predicts that (H2) the positive effect of Type 1 processing on UG offers is significantly stronger than its effect on DG giving. The experimental results align with this prediction, with meta-analytic findings indicating that Type 1 processing favours UG offers, whereas there is no statistical evidence for an overall effect of Type 1 processing on DG donations.

Note that the GSHH1 does not make any prediction about the effect of Type 1 processing on the DG. It might be zero, or it may not be zero, particularly for participants who are punished in their everyday lives for acting altruistically or egoistically. The GSHH1 does not contradict the existence of specific participant groups for which Type 1 processing is either favorable or unfavorable towards DG giving, as shown in studies exploring gender (Rand et al. 2016; Fromell et al. 2020) or personality factors (Shi et al. 2020; Nockur & Pfattheicher, 2021). It only predicts that, in general, the effect is likely to be weak and certainly weaker than in the UG.

Hypothesis 3: Truth-telling

Since Type 1 processing makes people less likely to directly harm others, the GSHH1 applied to the context of truth-telling predicts that (H3) the relationship between Type 1 processing and honesty when dishonesty harms concrete others is greater than the relationship between Type 1 processing and honesty when dishonesty harms abstract others. This prediction aligns with the meta-analytic findings reported by Köbis and colleagues (2019).

Note that the GSHH1 does not make any specific prediction about the overall effect of promoting Type 1 processing on honesty. Promoting Type 1 processing may increase or decrease honesty for different individuals or in different situations (Speer et al. 2020; Gai & Puntoni, 2021; Speer et al. 2022). The only specific prediction of the GSHH1 regards the comparison of the effect of Type 1 processing when dishonesty harms concrete vs abstract others.

Hypothesis 4: Direct harm in personal moral dilemmas

Directly harming someone to save a greater number of people in personal moral dilemmas such as the footbridge dilemma comes with no actual cost for the decision maker: the payoff of the decision maker is exactly the same whether they push the large man off the bridge or not. However, in reality directly harming another person usually does come with a cost – the

person being harmed might resist or fight back. Therefore, since the GSHH1 is based on the idea that Type 1 processing does not promote choices that are optimal in the given situation, but choices that are optimal in everyday interactions that resemble the given situation, it makes the prediction that (H4) Type 1 processing increases rejection of direct harm in personal moral dilemmas. This hypothesis aligns with the meta-analytic results presented before.

Hypothesis 5: Harm as a side-effect in trolley problems

Consider the classic trolley problem: a person must decide whether to divert a trolley onto another track to save five people, knowing that one person will be killed as a side-effect. In this dilemma, the harm is a foreseeable side-effect rather than direct. Therefore, the GSHH1 predicts that the association between Type 1 processing and deontological choice is stronger in sacrificial dilemmas involving direct harm than in those where harm is a side-effect (H5).

I can test this prediction through a meta-analysis of available studies, using standardized mean differences and 95% confidence intervals. The meta-analytic effect is not statistically significant ($p = 0.824$), and is significantly smaller than the effect for sacrificial dilemmas. For personal moral dilemmas, the effect was -0.135 (95% CI = $[-0.164, -0.106]$), while for side-effect dilemmas the effect is -0.005 (95% CI = $[-0.051, 0.040]$). Further evidence supporting H5 comes from the analysis of potential publication bias. Egger's test shows a significant small-study effect for side-effect dilemmas ($b = -2.66$, $z = -4.20$, $p < 0.001$). The trim-and-fill method estimates that there are fourteen missing studies, and imputing these studies results in a meta-analytic effect that is even further from the effect for personal dilemmas ($b = 0.056$, 95% CI = $[-0.022, 0.134]$). See Figure 11 for the forest plot and Figure 12 for the funnel plot with and without imputed studies.

Figure 11. Forest plot of the studies exploring the cognitive basis of utilitarianism in side-effect dilemmas.

Figure 12. Funnel plot of the studies testing the cognitive basis of utilitarianism using side-effect dilemmas.

Cooperation and positive reciprocity

The GSHH1 does not make any general predictions about the association between Type 1 processing and cooperation or positive reciprocity. This is because these behaviours do not involve harming others in a context where they can harm you back. However, this does not mean that the GSHH1 predicts that Type 1 processing never promotes cooperation or positive reciprocity. It may be the case that certain groups of people have internalized these behaviours as a default strategy for specific reasons, but this is not a general expectation. The results of the earlier meta-analyses support this conclusion. The debate about the effect of Type 1 processing

on cooperation has suggested that it promotes cooperation weakly and only for particular groups of people, such as inexperienced subjects living in cooperative societies (Rand et al., 2014), which is consistent with the GSHH1. For instance, people living in cooperative societies may have internalized cooperation to a greater extent than people in non-cooperative societies because non-cooperating in a cooperative society comes with a greater reputational cost. Regarding positive reciprocity, the available studies do not provide evidence of an effect of cognitive processing, and I am not aware of any study looking at potential moderators of cognitive processing's effect on positive reciprocity.

General open problems

In the previous sections, I mentioned several open problems whose solution would further advance our understanding of the cognitive basis of a specific social decision. I would like to conclude this article by mentioning some open problems that do not regard one social decision in particular, but they rather regard this field of research in general.

The effect of Type 2 processing

A significant unresolved issue pertains to the impact of Type 2 processing on social decision-making. According to the original Social Heuristics Hypothesis, thoughtful consideration of options should lead people to select the optimal choice in a given situation. However, the available evidence indicates that the picture may be more complex. For instance, in lying tasks where dishonesty harms abstract others, deliberation appears to increase honest behaviour, which decreases self-interest. On the other hand, in situations like the ultimatum game, deliberation may increase self-interest. Thus, it is challenging to categorize the effects of deliberation in simple terms that depend solely on the outcomes of available actions. It is plausible that deliberation makes people more emotionally detached from the situation,

encouraging them to reason about it in abstract terms. This may lead to different consequences depending on the social decision. In the ultimatum game, deliberation may help people overcome the emotional reaction that prompts them to reject low offers. On the other hand, in lying tasks where dishonesty harms abstract others, deliberation may make people more likely to acknowledge the harm caused by dishonesty, leading to an increase in honest behaviour. In personal moral dilemmas, deliberation may lead to represent the dilemma in abstract terms and therefore to endorse an utilitarian perspective, as suggested by Körner and Volk (2014). However, this remains speculative, and future research can shed more light on the effect of Type 2 processing on social decisions.

The need of uniformizing measures and manipulations

This field of research operates on the underlying assumption that all cognitive process manipulations have similar effects on each social decision. Several meta-analyses conducted so far implicitly include this assumption by analyzing all cognitive manipulations together (Rand, 2016; Rand et al. 2016; Köbis et al. 2019; Fromell et al. 2020). However, an important question arises: is this assumption valid? Is it true that all cognitive manipulations have similar effects? While there is evidence that time pressure, ego depletion, conceptual primes of intuition, and cognitive load have comparable effects on cooperation (Rand, 2019; Kvarven et al. 2020), these meta-analyses indicate that explicit conceptual primes of emotion have a more significant impact. Moreover, the meta-analyses are limited to cooperative behaviour, leaving the question of whether all cognitive manipulations have similar effects on all social decisions largely unanswered.

The meta-analyses discussed in this article consistently show significant heterogeneity across cognitive manipulations. However, the heterogeneity primarily pertains to the size of the

effect, rather than its direction, indicating that cognitive manipulations may differ in their “quantitative strength” rather than their “qualitative effect.” Isler and Yilmaz (2022) support this view, as they found that several cognitive manipulations, including time pressure, time delay, cognitive load, and conceptual primes of emotion, have the anticipated effects on intuitive or deliberative decision-making, albeit to varying degrees.

To add complexity to the topic, Newton, Feeney and Pennycook (2023) presented experimental evidence that challenges the assumption that people vary along a single dimension of thinking style. Their results suggest that there are four distinct thinking style: actively open-minded thinking, close-minded thinking, preference for intuitive thinking, and preference for effortful thinking. This classification opens up new avenues for research, such as investigating the specific effects of cognitive manipulations on each of these styles and examining how they relate to social decision-making.

In my view, there is a more general problem *within* cognitive manipulations. For instance, when it comes to time pressure studies, some scholars use a time pressure of five seconds, while others use ten, fifteen, or even twenty seconds. Even if we assume that time pressure promotes Type 1 processing, it is difficult to compare studies that use different time constraints. This methodological issue can be observed in other cognitive manipulation techniques as well. Subjects are loaded with different parallel tasks, depleted using different tasks, and so on. For instance, it is challenging to compare the effect of the *e*-hunting task with the effect of the Stroop task. It is as if different researchers were measuring the length of the same stick, but one is using meters, another is using miles, another is using yards, and another is using... kilograms! Uniformizing the units of measurement is the first step to building a truly scientific theory. Until

we uniformize the units of measurement, the entire field of research will remain in a state of conceptual chaos. I believe that this a fundamental direction for future research.

Extending the dual-process approach to other social decisions

While the decisions discussed in this article cover a broad range of social decisions, they are not exhaustive. In fact, there are other types of social decisions that warrant further investigation.

One such example is the equity-efficiency trade-off, which refers to the conflict between creating equity in the distribution of resources and maintaining efficiency in the process. This trade-off is often considered the “fundamental problem in distributive justice” (Hsu et al. 2008) or the “big-trade-off” (Okun, 2015). Those responsible for resource distribution frequently face this conflict, as exemplified by Okun’s “leaky bucket” argument, where taxation increases equity but also imposes costs on the institution implementing it. Understanding the cognitive basis of this trade-off is therefore of primary importance, yet little is known about it. Capraro et al. (2017) found that time pressure promotes equity while time delay favors efficiency. Similarly, Kessler et al. (2019) found that time delay promotes altruism when the cost of the altruistic action is smaller than its benefit. These works suggest that Type 1 processing promotes equity, while Type 2 processing favours efficiency. However, Krawczyk and Sylwestrzak (2018) argued that time pressure does not promote equity in general, but only envy, which is the desire to decrease the other’s payoff when it is greater than one’s own. Other studies using dictator games with varying cost-to-benefit ratios failed to detect significant effects of cognitive process manipulations (Friehe and Schildberg-Hörisch, 2017; Balafoutas, Kerschbamer & Oexl, 2018; Merkel & Lohse, 2019), or found that time pressure simply increased prosocial behaviour in prosocial individuals and self-regarding behaviour in individualists (Chen & Krajbich, 2018).

Another behavioural domain worth exploring is ingroup favouritism. Ingroup favouritism is considered one of the most pervasive biases among humans. Even when groups are formed randomly, people tend to favour those in their own group (Tajfel, 1970). Ingroup favouritism has been observed in numerous economic games, including public goods, dictator, ultimatum, and prisoner's dilemma games (see Everett et al., 2015, for a review). Despite the significance of this behaviour, only three studies have investigated its cognitive underpinnings. Rand, Newman, and Wurzbacher (2015) found that time pressure increases cooperation with both in-group and out-group members. Similarly, Everett et al. (2017) found that time delay decreases cooperation with both in-group and out-group members. De Dreu, Dussel, and Ten Velden (2015) examined situations in which cooperating with in-group members automatically entails competing with out-group members. They found that cognitive load increases ingroup favoritism.

One more example is aggression. Aggression can manifest in different forms, depending on the underlying motivations and triggers. Reactive aggression, for instance, is often impulsive and defensive, while goal-oriented aggression tends to be more deliberative (Nelson & Trainor, 2007). Everett, Ingbreetsen, Cushman, and Cikara (2018) developed a novel economic game called the pre-emptive strike game to study whether defensive aggression is intuitive or deliberative. They found that time pressure increases aggression, especially against outgroup members.

In reality, there are situations in which cooperation is not costly, but risky, meaning that mutual cooperation is an equilibrium, but there are also "safer", less risky equilibria. The risk comes from the fact that the cooperator receives the benefit of cooperation only if the other partner cooperates (Skyrms, 2004). What are the cognitive basis of risky cooperation? To date, only one studied addressed this question. Belloc, Bilancini, Boncinelli and D'Alessandro (2019)

found that a 10-second time pressure increases cooperation in a one-shot stag-hunt game, suggesting that time constraints may promote cooperative behaviour in situations where cooperation is risky.

In recent years, behavioural scientists have begun studying the factors that lead people to share fake news on social media, taking a dual-process approach. In a study conducted by Bago, Pennycook and Rand (2020), participants were asked to rate the accuracy of news headlines using a two-response paradigm. The authors found that when given the opportunity to reconsider their initial choice, participants were more likely to correct intuitive mistakes and thus better able to distinguish real news from fake news. Other studies have also shown that emphasizing the importance of accuracy can reduce the sharing of fake news (Pennycook & Rand, 2022), likely because it directs people's limited attention towards accuracy (Pennycook et al., 2021). These findings suggest that people may share fake news on social media because they tend to rely on their intuition and need to consciously deliberate in order to overcome this tendency.

Conclusion

I reviewed the existing literature on the cognitive basis of cooperation, altruism, honesty, positive and negative reciprocity and their expectations, and moral judgments (deontology vs act utilitarianism). In cases where meta-analyses were lacking, I conducted a meta-analysis in order to be able to draw quantitative conclusions. For each behavioural domain, I identified several open problems that I believe are essential to further our understanding of the cognitive basis of human sociality. I concluded by presenting a theoretical framework inspired by Rand and colleagues' Social Heuristics Hypothesis, which contends that Type 1 processes promote the use of heuristics that have been internalized in everyday situations that resemble the situation at hand. I argued that three heuristics that many people are likely to have internalized are: don't get harmed; don't

harm others, when they can harm you back; don't receive fewer resources than others. The framework is promising, as it makes predictions that are generally in line with the empirical evidence. However, a number of questions remain unanswered. Future work should aim to shed light on the areas in which empirical evidence is inconclusive. My hope is that this quantitative review can contribute to bring further attention to an area of research that has emerged in the last decades as one of the most vibrant and exciting in social science.

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Footnotes

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² In this review, I will focus only on experimental manipulations, which allows to draw a causal link between the independent and the dependent variables, as opposed to correlational studies that can be driven by unobserved variables. For example, since Type 1 is typically fast and Type 2 is typically slow, one might think that looking at response times in the control condition, rather than experimentally manipulating response time, would be enough to draw conclusions regarding which choice is typically driven by Type 1 and which is typically driven by Type 2. However, it has been noticed that response time predicts decision conflict and choice discriminability, rather than deliberation, at least in some contexts (Evans, Dillon & Rand, 2015; Krajbich, Bartling, Hare & Fehr, 2015; Evans & Rand, 2018). Therefore, I will not include in this article studies that have looked at the correlation between response times (or other typical correlates of the cognitive processes) and social behaviour

³ There are also studies exploring how inducing a particular emotion affects a subsequent decision (Strohming, Lewis & Meyer, 2011; Ugazio, Lamm & Singer, 2012; Motro, Ordóñez, Pittarello & Welsh, 2018). These works have shown that different emotions affect social decisions differently. For example, Strohming et al. (2011) and Ugazio et al. (2012) found that anger tends to increase consequentialist judgments while disgust tends to increase deontological judgments. Motro et al. (2018) let participants write about a time in their life in which they felt angry vs guilty; they found that inducing anger made people more likely to lie, while inducing guilt made them more honest. This emotion-dependent effect of priming suggests that inducing a particular emotion does not simply promote Type 1 processing. For this reason, in this review I will include only studies inducing people to “rely on emotion”, *in general*, and not those inducing a particular emotion.

⁴ Some authors argued that sleep deprivation and ego depletion are different (Vohs, Glass, Maddox & Markman, 2011). However, a review found a positive effect of sleep deprivation on aggression, a behaviour typically driven by lack of self-control, suggesting that sleep deprivation successfully impairs self-control (Kamphuis, Meerlo, Koolhaas & Lancel, 2012).

⁵ For example, Danziger, Levav and Avnaim-Pesso (2011) found that judges' favourable rulings drop from nearly 65% to nearly 0% from the morning to lunch time, and then again from after lunch time to dinner time.

⁶ Alcohol intoxication is known to diminish the executive cognitive functions necessary for effortful and controlled processing (Moskowitz, Burns & Williams, 1985; Fillmore, Carscadden & Vogel-Sprott, 1998; Curtin & Fairchild, 2003).

⁷ Mortality salience is a cognitive manipulation based on Terror Management Theory (Becker, 1973; Greenberg, Pyszczynski & Solomon, 1986). According to this theory, people rely on their cognitive resources to avoid the anxiety provoked by death-related thoughts. Therefore, reminding people about their mortality has the effect of depleting their cognitive resources. Several studies supported this view (Greenberg, Pyszczynski & Solomon, 1986). However, mortality salience also has been criticized, as a pre-registered replication of the original study failed to find an effect (Sætrevik & Sjøstad, 2022).

⁸ The Need for Cognition is a scale introduced by Cacioppo and Petty (1982) to measure the extent to which a person is inclined towards effortful cognitive activities.

⁹ The Faith in Intuition is a scale introduced by Epstein, Pacini, Denes-Raj and Heier (1996) to measure the extent to which a person is inclined towards effortless, intuitive, activities.

¹⁰ There is some work looking at the correlation between response time and cooperation in the traveller's dilemma (Rubinstein, 2007; Santa, Exadaktylos, & Soto-Faraco, 2018), but, to the best of my knowledge, no studies exploring the causal link.

¹¹ There are also some correlational studies that are broadly in line with this view. Mischkowski and Glöckner (2016) found that response time is positively correlated with cooperation among prosocials, but not among pro-selves, for whom there is no correlation. Yamagishi et al. (2017) found the same result for prosocials; however, among pro-selves, they found response time to be negatively correlated with cooperation. However, a meta-analysis found that the relationship between social value orientation and response time is moderated by extremity of choice, suggesting that it might be driven by decision conflict (Andrighetto, Capraro, Guido & Szekely, 2020).

¹² Fromell et al. (2018) included in their meta-analysis also studies using the dictator game with varying cost to benefit ratio. However, in a private communication, one of the authors (Daniele Nosenzo) confirmed that their results remain qualitatively the same when restricting the analysis to the standard dictator games.

¹³ Some authors have also considered situations in which participants are not incentivized to lie. Several works have provided evidence that, in this case, honesty tends to be intuitive (Spence et al. 2001; Walczyk, Mahoney, Doverspike & Griffith-Ross, 2009; Debey, Verschuere & Crombez, 2012).

¹⁴ Note that this is not inconsistent with the result mentioned in Footnote 13, that honesty is intuitive when participants are not incentivized to lie. As observed by Verschuere and Shalvi (2014), “truth telling may be the natural response absent clear motivations to lie [...] and that lying may prevail as the automatic reaction when it brings about important self-profit”.

¹⁵ The purpose of this article is not to take a stance on the best way to define utilitarianism in philosophical terms or how to measure it. However, it is important to note that there is an ongoing debate on this topic. For instance, Conway, Goldstein-Greenwood, Polacek & Greene (2018) used a process dissociation approach to argue that sacrificial utilitarian judgments do reflect genuine moral concern. According to their work, “antisociality predicts reduced deontological inclinations, not increased utilitarian inclinations”.