A Report on "Spontaneous Giving and Calculated Greed" by Rand et al. (2012)

Reviewer 2

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v2

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> I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.
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> Plato, *The Apology of Socrates*, 21d

> To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.
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> Karl Popper, 'Knowledge and the Shaping of Reality'

Overview

Citation: Rand, D. G., Greene, J. D., & Nowak, M. A. (2012). Spontaneous Giving and Calculated Greed. *Nature*, Vol. 489, pp. 427–430.

URL: https://www.nature.com/articles/nature11467

Abstract Summary: The study uses a dual-process framework and economic games to explore the cognitive basis of cooperation, finding that faster decisions are more cooperative, suggesting that intuition supports cooperation and reflection can undermine these prosocial impulses.

Key Methodology: Ten studies using economic games (Public Goods Game, Prisoner's Dilemma) with decision time analysis, time pressure/delay manipulations, and conceptual priming on Amazon Mechanical Turk (AMT) and in a physical laboratory.

Research Question: Are humans intuitively selfish, requiring reflection to cooperate, or are they intuitively cooperative, with reflection leading to selfish behavior?

Editor's Note

Version 2 of this report has been written by an improved model of Reviewer 2.

Summary

Is It Credible?

Rand et al. present a provocative and influential argument that human cooperation is fundamentally intuitive, while selfishness is the result of reflective processing. Through a series of ten studies involving both correlational data and experimental manipulations, the authors advance the "Social Heuristics Hypothesis." They argue that because cooperation is typically advantageous in daily life due to repetition and reputation, people internalize cooperative strategies as automatic heuristics, which then "spill over" into one-shot laboratory experiments (p. 427). The authors support this with evidence showing that faster decisions are associated with higher contributions in Public Goods Games (p. 427) and that forcing subjects to decide quickly increases cooperation compared to forcing them to reflect (p. 428). While the theoretical framework is compelling and the convergence of evidence across multiple studies is impressive, the credibility of the causal claims is complicated by methodological limitations regarding selection bias and the interpretation of decision time.

The most critical threat to the article's causal claims lies in the experimental design of the time-constraint studies. In Study 6, the authors report excluding subjects who failed to comply with the time restrictions. This resulted in a significant asymmetry: 48% of subjects in the "time pressure" condition were excluded for being too slow, compared to only 19% in the "time delay" condition (p. S12). This severe attrition compromises the random assignment, as the "compliant" subjects in the pressure condition likely differ systematically from those who could not make a decision in under 10 seconds. They may be more impulsive, more eager to please, or cognitively distinct in other ways. When the authors include these non-compliant subjects in a robustness check, the coefficient for the time pressure condition drops from 10.91 to 5.54 in the direct comparison model (p. S13). However, it is crucial to note that

this model controls for non-compliance, which itself has a massive negative effect on cooperation. Furthermore, when comparing the time pressure condition against the baseline, the effect size remains remarkably stable whether non-compliant subjects are excluded (4.93) or included (4.47) (p. S14). Thus, while the selection bias is a valid concern, the data suggest the effect is more robust than a simple "halving" of the coefficient implies, though the reliance on a subset of "fast" responders remains a limitation.

Furthermore, the construct validity of decision time as a proxy for "intuition" versus "calculation" is debatable. The authors frame slower decisions as "calculated greed," implying that time is used to override moral impulses with rational self-interest. However, slowness can also reflect confusion, distraction, or a struggle between competing moral values. While the authors attempt to rule out comprehension issues by controlling for post-decision quizzes, this does not fully resolve the ambiguity of what cognitive processes are actually occurring during those extra seconds (p. S6). Additionally, the explicit instructions to decide "as quickly as possible" or to "carefully consider" introduce potential demand characteristics (pp. S40, S42). Subjects might interpret the instruction to be fast as a cue to follow a "gut instinct" (which they may perceive as moral), while interpreting the instruction to be careful as a cue that the "correct" answer requires rational self-interest.

Finally, the evidence supporting the mechanism—that these intuitions are learned from daily life—relies on relatively coarse measures. The authors use a single self-report question about trust in daily interactions to proxy for a subject's social environment (p. S27). While the moderator analysis shows that the time-pressure effect is significantly stronger for those who trust others, relying on a subjective survey question to capture the complex game-theoretic structure of a person's real-world interactions is a limitation (p. 429). Despite these issues, the article's demonstration that cognitive style correlates with cooperation remains a significant contribution, even if the strict causal arrow from reflection to selfishness is less definitive than the

title suggests.

The Bottom Line

The article provides a strong theoretical argument and converging correlational evidence that intuitive processing is associated with greater cooperation. However, the causal claim that reflection actively undermines cooperation is complicated by selection bias in the experimental manipulations, where nearly half of the participants in the "intuitive" condition were excluded. While the "Social Heuristics Hypothesis" remains a plausible and influential explanation for human altruism, the specific assertion that "calculated greed" drives slower decisions is an interpretation that outstrips the strength of the experimental data.

Potential Issues

Asymmetric non-compliance in time-constraint experiments: The article's causal claims rest heavily on experiments that manipulate decision time, yet these studies suffer from high and asymmetric rates of non-compliance that may introduce selection bias. In the main online experiment (Study 6), 48% of subjects in the "time pressure" condition failed to meet the 10-second deadline and were excluded from the primary analysis, compared to only 19% non-compliance in the "time delay" condition (p. S12). This pattern was directionally similar in the lab study (Study 7), where non-compliance was roughly 46% for time pressure versus 10% for time delay (p. S15). Such attrition breaks the integrity of random assignment, as the subjects who were able to comply with the rapid deadline may be systematically different from the large portion who could not. The authors acknowledge this issue by conducting a robustness check that includes non-compliant subjects. While the coefficient for the time pressure condition drops from 10.91 to 5.54 in the direct comparison model, this model controls for the act of disobeying, which is strongly negatively correlated with cooperation (p. S13). Crucially, when comparing against the baseline condition, the effect of time pressure is stable (4.93 vs. 4.47) regardless of exclusions (p. S14). This suggests the result is not entirely an artifact of selection, but the reliance on such a heavily filtered sample for the primary analysis remains a methodological weakness.

Decision time as a proxy for cognitive processes: The article's correlational evidence is founded on the premise that decision time is a valid proxy for distinguishing intuitive from reflective cognitive processes. This operationalization is a central and debatable assumption, as decision time is a noisy signal that could be influenced by numerous factors other than the proposed intuition-reflection dichotomy. For instance, faster decisions may reflect confusion or a lack of comprehension of the game's selfish incentives, while slower decisions may reflect the time taken to under-

stand the payoff structure. Slower decisions could also result from internal conflict between competing impulses (e.g., fairness vs. selfishness) rather than a switch to "calculated" reasoning. The authors attempt to address some of these confounds, for example by showing that their results are robust to controlling for post-decision comprehension checks (p. S6) and, in one study, for time spent reading instructions (p. S34). However, whether these controls are sufficient to isolate the specific cognitive mechanism of intuition remains a matter of theoretical debate, and the interpretation of decision speed as a clean proxy for intuition is a significant simplification.

Post-decision comprehension checks and potential confounds: In the main online experiments, comprehension questions were administered after subjects made their contribution decision to avoid inducing a reflective mindset (p. S4). The authors validate this concern in a supplemental study showing that pre-decision comprehension questions significantly reduce cooperation (p. S38). While this justifies the design choice, it creates a methodological trade-off. It is impossible to be certain that subjects understood the game's incentives at the moment of decision, leaving open the possibility that the core finding is confounded by confusion among faster responders. The authors address this by statistically controlling for comprehension in their analyses. However, this introduces a different statistical concern, as controlling for a variable measured after the decision process (a post-treatment variable) can potentially introduce collider bias. The authors' analyses show that their results are robust to this control, suggesting comprehension does not explain the effect in their data, which mitigates the practical concern even if the theoretical risk exists (p. S30).

Potential for demand characteristics in time-constraint manipulations: The experimental manipulations used to establish causality may be subject to demand characteristics. The explicit instruction to "Please make your decision as quickly as possible" in the time pressure condition, or to "Please carefully consider you decision" in the time delay condition, could signal the experimenter's hypothesis to participants (pp. S40, S42). Subjects might interpret the "quick" instruction as a cue to be

cooperative and not overthink, and the "careful" instruction as a cue that a more rational, self-interested response is expected. The article attempts to address this in a lab study by measuring subjects' expectations of others' behavior. They find no significant difference in predicted contributions of others between the time pressure and time delay conditions (p. S15). This is a strong defense; if subjects were simply conforming to a "be nice" demand effect, they would likely project that expectation onto others as well. Additionally, the authors analyze the difference between a subject's contribution and their expectation of others, finding that subjects under time pressure contribute significantly more than they expect others to (p. S16). This evidence helps rule out simple conformity, though it does not fully eliminate the possibility that subjects were responding to perceived expectations about their own specific role.

Interpretation of slow decisions as "calculated greed": The article's narrative, including its title, frames the findings as a contest between "Spontaneous Giving and Calculated Greed," implying that slower decisions reflect an effortful, rational calculation aimed at maximizing selfish gain. While the data show a negative correlation between decision time and cooperation, the evidence does not directly reveal the cognitive content of slower decisions. Alternative explanations, such as a prolonged struggle with moral conflict, confusion, or simple distraction, are not ruled out. The authors provide evidence from a supplemental study showing that forcing subjects to perform a payoff calculation does indeed slow them down and reduce cooperation, which is consistent with their interpretation (p. S38). However, this shows that calculation causes slowness, not that all slowness is caused by calculation. The framing of slower decisions as "calculated" remains an interpretive leap not fully substantiated by the data.

Weakness of measures for the "daily life spillover" hypothesis: The article proposes that cooperative intuitions are learned in daily life and "spill over" into the lab, a mechanism tested with two moderator variables. The evidence supporting this

mechanism is limited by the coarseness of these measures. The cooperativeness of a subject's daily life was measured with a single, self-report question about perceived trust in others (p. S27). This captures a subjective belief, which could reflect dispositional optimism as much as the objective strategic nature of one's environment. Similarly, prior experience with experiments was measured with a variable classifying subjects based on their answer to the question, "To what extent have you participated in studies like this one before?" (p. S22). While the results of the moderator analyses are consistent with the article's theory—showing a significant interaction where trust predicts the effect of intuition—the conclusions about the proposed mechanism are necessarily tentative given the limitations of these single-item measures (p. 429).

Strength of claims based on null correlations with dispositional traits: The article argues that its findings are driven by within-subject variation in cognitive style rather than stable, between-subject dispositions. This claim is supported by the experimental manipulations, which randomly assign subjects to cognitive styles, and is supplemented by null findings from a correlational study. In that study, standard individual difference measures like the Cognitive Reflection Test showed no significant correlation with cooperation in a sample of 341 subjects (p. S32). While these null results are consistent with the authors' argument, and the sample size is reasonably large ($N = 341$), making a positive claim about the absence of an effect based on null correlations is always challenging. The authors' overall argument is strengthened by triangulating these null correlations with their successful manipulation experiments, but the claim about the lack of a role for dispositional traits rests in part on an absence of evidence (p. S36).

Dismissal of the anchoring and adjustment heuristic: A plausible alternative explanation for the results in the Public Goods Game is the anchoring and adjustment heuristic, where subjects anchor on an equitable contribution and then adjust downwards with more reflection. The article dismisses this by arguing that for this heuristic to explain the observed results, one must assume that the natural anchor is a high

contribution, which is not self-evident compared to an anchor of zero (p. S36). More substantively, it points to the re-analysis of prior experiments using a binary Prisoner's Dilemma, where a numerical anchor is not possible, yet the same negative relationship between decision time and cooperation is observed (p. S36). While this provides a reasonable empirical counterargument, it does not directly rule out the possibility that anchoring plays a role specifically within the Public Goods Game paradigm, which is the primary focus of the new experiments presented.

Generalizability and sample selection: The article's claims of robustness are based on a diverse set of studies, but some limitations on generalizability should be noted. The re-analysis of prior lab studies (Studies 2–5) is based on a convenience sample of the authors' own work, selected based on the availability of decision time data (p. 428). The authors are transparent about this criterion, and the fact that the data had not been previously analyzed for this purpose mitigates concerns of cherry-picking. Additionally, while the online samples are diverse, cultural heterogeneity is addressed with a simple binary control for US residency. The authors report that 37% of subjects lived outside the US, with over half of those living in India, but the formal analysis uses only the binary US/non-US variable, finding no significant differences (pp. S2, S6). A more granular analysis of cultural background could provide a richer test of the "daily life spillover" hypothesis.

Minor presentation and transparency issues: There are minor transparency issues in the supplementary materials. For instance, in the analysis of the priming experiment (Study 8), the authors state that subjects who wrote fewer than eight sentences were excluded, but they do not report how many subjects were excluded under this criterion or whether exclusion rates differed by condition (p. S19). They do, however, control for paragraph length in the subsequent analysis, addressing the potential for bias from differential effort. Additionally, the sample size for Study 10 is reported as $N = 341$ in one analysis (p. S28) but $N = 338$ in another (p. S30), likely due to item-level missing data for a specific covariate rather than a clerical er-

ror, though this is not explicitly stated. These minor issues do not affect the main conclusions but represent small gaps in documentation.

Future Research

Intent-to-treat designs for time constraints: Future work must address the severe attrition observed in time-pressure paradigms. Researchers should employ "intent-to-treat" analyses that include all randomized participants regardless of compliance, or utilize designs that do not require excluding half the sample (e.g., milder time incentives rather than strict cutoffs). This is essential to ensure that observed differences in cooperation are due to the manipulation of cognitive processing speed rather than the selection of specific personality types who are capable of rapid compliance.

Process tracing and neural imaging: To validate the claim that slower decisions represent "calculation" rather than confusion or moral conflict, future research should move beyond simple response times. Techniques such as eye-tracking, mouse-tracking, or fMRI could be used to observe the accumulation of evidence during the decision process. This would help distinguish whether "slow" subjects are actively computing payoffs (calculated greed) or oscillating between options (moral conflict), thereby clarifying the cognitive mechanism behind the correlation.

Ecological validation of social heuristics: The hypothesis that cooperative intuitions are learned from daily life requires more rigorous testing than single-item survey questions allow. Future studies could use ecological momentary assessment (EMA) or data from actual social networks to map the strategic structure of participants' real-world interactions. Correlating these objective measures of a participant's "daily life" environment with their heuristic behavior in the lab would provide much stronger evidence for the spillover mechanism proposed by the authors.

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