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Social Psychology

Self-interest Is Overestimated: Two Successful Pre-registered Replications and Extensions of Miller and Ratner (1998)

Cameron Brick[1], Adrien Fillon[2], Siu Kit Yeung[3], Meiying Wang[3], Hongye Lyu[3], Jing Yee Jane Ho[3], Sze Chai Wong[3] [a], Gilad Feldman[3] [b]

[1] Psychology, University of Amsterdam, Amsterdam, the Netherlands, [2] Social Psychology, Aix-Marseille University, Aix-en-Provence, France, [3] Psychology, University of Hong Kong, Hong Kong SAR

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Self-interest is a central driver of attitudes and behaviors, but people also act against their immediate self-interest through prosocial behaviors, voting incongruously with their finances, or punishing others at personal cost. How much people believe that self-interest causes attitudes and behaviors is important, because this belief may shape regulation, shared narratives, and institutional structures. An influential paper claimed that people overestimate the power of self-interest on others' attitudes and behavioral intentions (Miller & Ratner, 1998). We present two registered, close, and successful replications (U.S. MTurk, $N = 800$; U.K. Prolific, $N = 799$) that compared actual to estimated intentions, with open data and code. Consistent with the original article, participants overestimated the impact of payment on blood donation in Study 1, $ds = 0.59$ [0.51, 0.66], 0.57 [0.49, 0.64], and overestimated the importance of smoking status for smoking policy preferences in Study 4, $ds = 0.75$ [0.59, 0.90], 0.84 [0.73, 0.96]. These replications included two extensions: 1) communal orientation as a moderator of overestimation and 2) a more detailed measure of self-interest in Study 4 (ordinal smoking status). Communal orientation did not predict overestimation, and the ordinal smoking measure yielded similar results to the main study. Verifying the overestimation error informs behavioral theories across several fields and has practical implications for institutions that require trust and cooperation. All materials, data, and code are available at osf.io/57mdc/

How much do personal interests drive others' attitudes and behaviors? Self-interest is fundamental in behavioral theories across the sciences such as in rational choice theory, evolutionary psychology, behaviorism, criminology, and beyond (Agnew, 2014; Barkow et al., 1995; Force, 2003; Miller, 1999; Nelson, 2020). People are also compelled by the idea that self-interest is the primary driver of the attitudes and behaviors of others. Individuals tend to attribute prosocial actions to intentions for personal gain (Gardner & Ryan, 2020) and maintain such narratives of self-interest even in the face of disconfirming evidence (Critcher & Dunning, 2011). These beliefs are deeply consequential: they inform not just personal interactions but also public policies and institutions such as the criminal justice system. As a more specific example, people might believe in 2021 that most people would choose not to wear face masks, choosing self-interest over community disease prevention, but compliance is high.

Similarly, evidence is growing that people also think and act against their immediate self-interest by helping, sharing, funding, and cooperating (Batson & Powell, 2003; Besley & Ghatak, 2018). In addition, self-interest such as financial gain is a surprisingly weak predictor of voting and policy preferences (Caplan, 2011). Individuals are also concerned with ideology, equality, fairness, and collective outcomes when making decisions (Dawes et al., 1988; Güth & Kocher, 2014; Mahmoodi et al., 2015; Sears & Funk, 1990; Tyler, 1990). The extent to which scientists and the public assume that self-interest drives attitudes and behaviors has implications for behavior change theories, public policy, charitable giving, conservation, criminology, and organizational management (Agnew, 2014; Felin & Foss, 2009; Kals et al., 2001; Ostrom, 1990/2011; Ratner et al., 2011; Ratner & Miller, 2001).

Choice of Replication Target

By comparing estimates to actual choices, one landmark paper challenged the central concept that people make ra-

a Brick, Fillon, Yeung, Wang, Lyu, Ho, and Wong are equal-contribution first authors

b gfeldman@hku.hk / giladfel@gmail.com

tional decisions to maximize personal gain (Miller & Ratner, 1998). They provided evidence that individuals overestimate how much self-interest determines the intentions and preferences of other people. For example, how much do financial incentives affect prosocial behavior, and how much does a person's smoking status determine their preferences for smoking bans? Seeing self-interest as primary continues to influence theory and practice, particularly in political science, management, social psychology, and economics. In March 2021, Miller & Ratner (1998) had been cited 487 times on Google Scholar. As far as we know, there have been no published close replications. Importantly, self-reported willingness was used as a proxy for behavior in the original and our replication, and therefore any discrepancy between estimates and reported willingness could also be explained by differences between reported willingness and actual behavior. However, the overestimation interpretation is most plausible based on evidence from different designs (Vuolevi & Van Lange, 2009) and findings that people think of others as more selfish and less fair than themselves (Van Lange & Sedikides, 1998).

Miller & Ratner (1998) informed the perennial tension between self-interest and the common good. Overestimating self-interest was consistent with large-scale studies on political attitudes finding that vested personal interests hardly predict individuals' attitudes towards policies, even on issues that should be highly relevant to considerations of self-interest (Boninger et al., 1995; Jost et al., 2004). The overestimation effect shows that early conceptions of self-interest were too narrow for overlooking motivations such as being a good group member. Another influential study that cited Miller & Ratner (1998) was 'Party over policy' (Cohen, 2003) on the influence of group memberships on political attitudes (1179 citations on Google Scholar). While Cohen's participants were unaware of how self-interest drove their own beliefs, they readily assumed that self-interest drove the beliefs of their political adversaries. Miller & Ratner (1998) is also part of the foundation of arguments about self-interest in moral psychology and decision making, and has implications for altruism and prosocial behaviors. For example, follow-up studies continued to target self-interest in promoting charity donation rather than other frameworks (Simpson et al., 2006).

Based on similar studies, Ratner & Miller (2001) proposed that the assumption of self-interest could be self-fulfilling through a positive feedback loop between theory and social structures. Believing that humans are mostly self-interested led to the design of social institutions that facilitated this outcome. It is hard to overstate the potential importance of this feedback loop, and psychologists could partner with experts in institutions to better understand how assumptions of self-interest might have informed the design of educational systems, branches of government, economic models, and also smaller contexts such as workplace regulations. Additionally, strong social norms of self-interest may lead people to behave in self-interested ways just to avoid norm violations. "People treat self-interest as a natural law and because they believe they should not violate a natural law, they try to obey it" (Kagan, 1989, p. 283). Similarly, institutional design, norms, stories, literature, and management practices can lead to self-fulfilling processes based on popular ideas (Ferraro et al., 2005). Therefore, beliefs about self-interest appear to impact practical as well as theoretical outcomes (Ghoshal & Moran, 1996; Ratner & Miller, 2001; Vuolevi & Van Lange, 2009), and such beliefs may reduce trust and cooperation (Darke & Chaiken, 2005; Evans & Krueger, 2016).

Debates continue about whether people overestimate how much self-interest drives others' preferences. Inaccurate estimations of other groups are also demonstrated in political psychology: members of political groups overestimate how different their political opponents' policy positions are (Van Boven et al., 2018). Another impactful study suggested that people actually underestimate the influence of self-interest (Epley & Dunning, 2000). Their studies compared charitable behaviors predictions for self and others, compared to only attitudes in previous studies. Participants overestimated the likelihood that they would act in generous ways by incorrectly weighing moral sentiments over self-interest and overlooking base rates, but participants predicted others' behavior more accurately. These authors argued that when hedonic consequences are immediate, as in their study designs, self-interest influences behavior, and people's cynical beliefs about others are likely to be accurate. However, when hedonic concerns are remote, as in hypothetical choices like in Miller & Ratner (1998), self-interest is less influential and people are more likely to overestimate its impact. Separate from resolving these boundary effects and potential moderators, these are examples of how Miller & Ratner (1998) is woven into subsequent theory and therefore worth replicating. An important note is that these studies and the current ones were based on Western samples, which leaves a large gap about how most of the world's population would respond to such scenarios.

We did not know if the key effect would replicate even within a Western sample. An informal Twitter poll yielded the second-lowest estimation of successful replication among 12 effects (Feldman, 2020). We tested the classic overestimation effect on self-interest in two samples and two domains (blood donation and smoking-related policies). Our goal was to evaluate reproducibility and replicability in response to a growing recognition of their importance in psychological science (Open Science Collaboration, 2015; Zwaan et al., 2017). We present two pre-registered, well-powered, independent, very close replications of Miller & Ratner (1998), with open data, code, and RMarkdown output. We chose Studies 1 and 4 (originally *N*s = 56 and 81) because they were the most straightforward designs to replicate closely and feasibly online; for example, the original Study 2 was a poor candidate because it was about abortion and appeared deeply contextualized in a specific political era in the USA. There is no Study 2 nor Study 3 in this project; we conducted two studies and both are reported here using the study numbers from the original paper for ease of comparison.

Original Study Design

In Study 1, participants were randomized to report both a) their self-reported willingness to donate blood with and without a financial incentive and b) their estimate of others' willingness with or without a financial incentive. The mean

Table 1. Hypotheses of Miller and Ratner (1998)

Study	Hypothesis
1	1A: Participants overestimate how much payment changes others' willingness to donate blood.
	1B: The individual tendency to overestimate is unrelated to how much payment influenced that individual's willingness to donate blood.
4	4A: Smokers are more opposed than nonsmokers to policies restricting smoking.
	4B: Participants overestimate the impact of smoking status on others' attitudes towards these policies.

Note. Original effect sizes were not provided and could not be precisely estimated because variance was not reported either. Hypothesis 1B predicted a null effect and the result was reported as "$F(1, 54) < 1$, *ns*."

Table 2. Samples from the Original Study and Replications

	Miller and Ratner (1998)	MTurk	Prolific
N	$N_{S1} = 54, N_{S4} = 81$	799	799
Country	USA	USA	UK
Sample	Students	General population	
% Female	Unknown	49.1%	59.6%
Age, *M*	Unknown	41.6	40.8
Age range	Unknown	19-78	18-76
Setting	On paper in person	Online by computer	
Compensation	Study 1: course credit or $5 Study 4: unknown	$0.90 for 6 minutes	£0.70 for 6 minutes
Year	1998	2019	

reported willingness was then compared to the mean estimated willingness across payment conditions. In Study 4, separate participants were randomized to either report their smoking status (yes/no) and then indicate their support of eight policies that restricted smoking, or to estimate the willingness of other smokers and nonsmokers to endorse those policies. Similarly, the mean policy support for smokers vs. nonsmokers was compared to the estimated support of those groups.

The original paper confirmed all of these predictions. The ostensibly large effects (from visual inspection; no variance was reported) could either be signs of a robust phenomenon or signs of research practices that were normal at the time but are now recognized as inflating effect sizes and false positives (John et al., 2012). Given the era, the original article was normative in lacking open data, code, transparency about the timing of analytic decisions, information about a file drawer, or complete descriptives (e.g., *SD*s were missing). Further, the samples were small and the tests weakly powered (Table 2). At the same time, these findings might be expected to replicate given that over-estimation was shown in multiple contexts across quite distinct methods and topics in the original paper, and in related papers since.

Replication Pre-registrations

Both replications were pre-registered prior to data collection at the Open Science Framework (Nosek & Lakens, 2014) including pre-planned analyses and simulated data (reported in the pre-registration Supplements). The replications were conducted in parallel by different teams working independently. Anonymized data, code, and files from the current manuscript are here: https://osf.io/57mdc/. This link also includes the pre-registrations, original manuscripts, code, Qualtrics exports, and pre-registration supplements of both independent samples. Minor deviations from these pre-registrations are listed in the Supplement.

All studies, samples, measures, manipulations, and exclusions conducted for this investigation are reported, all inferential tests not explicitly marked "exploratory" were pre-registered with power analyses, and data collection was completed before hypothesis testing. All *t*-tests were two-tailed and $\alpha = .05$.

Power and Sensitivity Analyses

The power analyses in the pre-registration supplements were based on estimations of the unreported original variance, and therefore were speculative. The sample size in each replication was ten times larger than the original. Sensitivity analyses run in the R package pwr using $\alpha = .05$, $\beta = .80$, $N = 799$ showed the minimum effect detectable for each hypothesis in each separate sample: H1A, H4B one-sample *t*-tests, $d = .10$; H1B, H2A:F, H4A Pearson's correlations, $r = .10$. The combined sample analyses for $N = 1598$ were more sensitive, yielding $d = .07$ and $r = .07$, respectively.

Table 3. Replication Closeness

Design facet	Replication	Deviation from the original study
Effect/hypothesis	Same	*n/a*
IV construct	Same	*n/a*
DV construct	Same	*n/a*
IV operationalization	Same	Minor wording differences (Tables S1 and S2)
DV operationalization	Same	Minor wording changes (see Supplement)
Population (e.g., age)	Similar	The original studies used U.S. introductory students at the State University of N.Y. in Study 1 and Princeton University in Study 4. We sampled MTurk (U.S. residents) and Prolific (U.K. residents), which are more diverse and older populations.
IV stimuli	Similar	The vignette in Study 1 was changed from the Mandela Room of the student union to the neighborhood of the participant.
DV stimuli	Same	*n/a*
Procedural details	Different	Each participant completed two studies instead of only one. Order effects were minimized through random assignment.
Physical settings	Different	The original studies used a questionnaire packet in person and the replications used online participation using a computer.
Contextual variables	Different	The original studies were reported in 1998 or earlier and the replications were conducted in 2019.
Replication classification	Very close replications	

Participants and Overview

All participants completed Studies 1 and 4 in randomized order to minimize order effects (in the 1998 paper, participants only completed one of the studies). After every scenario, the participants responded to comprehension questions to make sure they understood the content. Also, the results are overall very similar between the two samples, which suggests there were not major quality issues. Finally, we used planned exclusions for lack of English proficiency or self-reported lack of understanding the materials. The replication sample characteristics are compared to the original article in Table 2.

MTurk

A total of 800 United States residents recruited through Amazon Mechanical Turk (MTurk) completed the study through Cloudresearch/TurkPrime (Litman et al., 2017) (age M = 41.6 years, SD = 12.8; 401 men, 393 women, and six other/rather not disclose). One person reporting an age of 11 was excluded; this was not pre-registered, and see the Supplement for a complete list of deviations. For Study 4, the MTurk sample was randomized to either the direct replication (n = 414) or the extension with a more granular measure of smoking status (n = 386).

Prolific

A total of 799 United Kingdom residents recruited through Prolific completed the study (age M = 40.8 years, SD = 13.7; 319 men, 476 women, and four other/prefer not to say); for demographics of the overall Prolific population, see Palan & Schitter (2018). For example, the modal Prolific panel member in the larger population was employed full-time and had a bachelor's degree.

Sample quality

Online panel samples offer improved external validity compared to student samples due to more representative population sampling (Mason & Suri, 2011; Palan & Schitter, 2018). For example, because we sought to generalize the findings to all adults, these samples include a wide range of ages (18-78). Additionally, classic psychological effects replicate in these online populations (Berinsky et al., 2012; Hauser & Schwarz, 2016).

Replication Closeness and Evaluation Criteria

We compared the replication effects using established criteria (LeBel et al., 2019) and classed both samples as very close replications (Table 3).

Study 1

This study investigated the impact of financial incentives on the reported willingness to donate blood. Participants estimated the likelihood to donate blood for themselves and others, both when there was payment and when there was none.

Method

Participants completed a two-condition (payment or not) within-subjects design nearly identical to the original (see Table 3, and Table S1 for the vignette text). Participants were told that the blood supply in the United States had reached record lows in the past month and that the American Red Cross would be coming to the neighborhood for a blood drive in several weeks. The Red Cross was asking to get a sense of how many people would be willing to donate blood and what factors might make volunteering more

Table 4. Blood Donation Rates in Study 1

	Self			Others (Estimated)		
	Original	*MTurk*	*Prolific*	*Original*	*MTurk*	*Prolific*
n	54	799	799	54	799	799
		%			*M* (*SD*)	
Paid	73.2	72.7	76.8	62.5 (?)	55.8 *(27.0)*	59.9 *(26.4)*
Unpaid	62.5	59.2	71.2	32.6 (?)	37.9 *(26.0)*	42.4 *(27.8)*
Difference	*10.7*	*13.5*	*5.7*	*29.9*	*17.9*	*17.5*

*Note. SD*s were not reported in the original paper.

attractive. Both the US and UK samples saw the same text about the American Red Cross.

Manipulation: Payment

In the paid condition, participants were told that the Red Cross was considering paying $15 to each individual who donates blood. In the unpaid condition, participants were told that the Red Cross was considering collecting donations without financial compensation. Participants saw both payment conditions in random order.

Extension: Communalism

Thinking about social interactions in terms of self-interest could lead a person to act competitively and selfishly due to limited perceived resources (Kool, 2008). This focus may also lead individuals to suspect others are driven primarily by self-interest (Kool, 2008). In contrast, when individuals are more communally oriented, self-interest may be less important both in their decision making and for their perceptions of others' behaviors.

This extension tested communalism as a potential moderator of overestimation. Communal motivation means believing that others' needs and feelings are vital in relationships and that people should help and care for each other (Clark et al., 1987). Individuals with higher communal motivation care more for the welfare and needs of others, and expect others to be responsive and concerned about their welfare. A meta-analysis recently showed that communal motivation is positively associated with personal well-being and relationship partner satisfaction (Le et al., 2018). Compared to less communal individuals, we hypothesized:

> *Hypothesis 2. More communal individuals will report being more willing to donate blood (H2A) and support restrictive smoking policies (H2B); More communal individuals will more often estimate that others are willing to donate blood (H2C) and support restrictive smoking policies (H2D);*

and the key test:

> *More communal individuals will overestimate self-interest less in estimates for donation (H2E) and policy support (H2F)* (i.e., estimate more accurately).

The results of Hypothesis 2 are at the end of the Results section.

Measures and Procedures

Communalism was measured with the 14-item Communal Orientation Scale (Clark et al., 1987) (see Supplement for all items). An example item was "When making a decision, I take other people's needs and feelings into account", rated from 1 (*extremely uncharacteristic of me*) to 7 (*extremely characteristic of me*); Cronbach's α = .84. Participants then indicated whether they would donate blood (*yes* or *no*) for both payment conditions. Participants then estimated the percentage of their peers who would donate blood by giving a number from 0 - 100 (%) for both payment conditions.

Original Effect Sizes

Miller & Ratner (1998) did not report effect sizes, and they could not be calculated precisely because standard deviations or other variance measures were not reported. However, the replication effects were compared with the original (LeBel et al., 2019). There are normally three components to the interpretation. Signal indicates a significant effect, Consistency is whether the effect size is comparable, e.g., whether 95% CIs cover the original effect size, and Direction clarifies the direction of any inconsistencies. To be cautious, we do not precisely estimate the original effect sizes and therefore only provide Signal and Direction from this replication framework. The original effects look very large (see "difference" row in Table 4) and the direction was always consonant with the original.

Related studies can also inform a likely replication effect size. Few experiments report comparisons of actual to estimated self-interest with similar paradigms, but one such study used a dice-rolling method (Vuolevi & Van Lange, 2009) and showed that participants overestimated how much self-interest drove behavior in a financially incentivized task. We converted the *t*-test results in Vuolevi & Van Lange (2009) Study 2 to an overestimation effect of d = 0.96, which appears visually comparable to Miller & Ratner (1998).

Results

The Study 1 descriptives are shown in Table 4 for the original (1998) and both replication samples across self-rating and estimates for others. Reported willingness to donate was computed by a mean of *no* (0) or *yes* (1), then multiplied by 100 to yield a percentage; estimated willingness

Table 5. Overestimation of Self-Interest in Study 1

Sample	*n*	*M*	*SD*	*t*	*d*	*95% CI*	Interpretation
MTurk	799	12.2%	*20.8*	16.6	0.59	[0.51, 0.66]	Signal Same Direction
Prolific	799	11.8%	*20.8*	16.1	0.57	[0.49, 0.64]	Signal Same Direction
Combined	*1598*	*12.0%*	*20.8*	*23.1*	*0.58*	*[0.53, 0.63]*	*Signal Same Direction*

Note. Overestimation *M* was calculated by taking the mean difference of estimates between paid and unpaid conditions and subtracting the mean difference between paid and unpaid conditions of self-reported willingness. The one-sample *t*-tests were against $M = 0$ (equivalence). All *p*s < .0001. The interpretation is based on LeBel et al. (2019), and the size of the effects could not be precisely compared to the original due to missing information.

was a simple mean within payment conditions within each study.

Replication

The key overestimation effect is shown below in Hypothesis 1A. See Table S3 for the effect of payment on donation willingness and on estimates, and Figure 1 for the estimated donation willingness by payment. All effect sizes below are shown followed by 95% CIs.

Being paid increased the self-reported willingness to donate blood, based on paired-sample *t*-tests, $t(798) = 4.17$, $p < .001$, $d = 0.13$ [0.07, 0.19] for the Prolific sample and $t(798) = 8.5$, $p < .001$, $d = 0.29$ [0.22, 0.35] for the MTurk sample. As expected, participants estimated that others would be more willing to donate blood when paid, $t(798) = 23.7$, $p < .001$, $d = 0.64$ [0.58, 0.7] for the Prolific sample and $t(798) = 24.3$, $p < .001$, $d = 0.67$ [0.61, 0.73] for the MTurk sample.

Participants underestimated donation rates in both conditions based on one-sample t-tests: willingness to donate was higher than estimated in the paid condition: $t(798) = 11.84$, $p < .001$, $d = 0.48$ [0.38, 0.58] for Prolific, $t(798) = 10.88$, $p < .001$, $d = 0.46$ [0.36, 0.56] for MTurk; and the unpaid condition: $t(798) = 18.29$, $p < .001$, $d = 0.77$ [0.67, 0.87] for Prolific, $t(798) = 12.49$, $p < .001$, $d = 0.54$ [0.44, 0.64] for MTurk.

Hypothesis 1A (combined samples). The key test is whether individuals overestimated the effect of payment on self-reported willingness to donate (see Table 4 for raw means). We compared Paid vs. Unpaid between the Self and Others conditions. In a one-sample *t*-test we found support for a discrepancy, $t(1598) = 33.85$, $p < .001$, $d = 0.85$ [0.79, 0.90] (see Table 5 and Figure 2 for results by sample). Overall, participants overestimated the effect of payment by $M = 12\%$, and the one-sample *t*-test against $\mu = 0$ for overestimation was $t(1597) = 23.1$, $p < .0001$, $d = 0.58$ [0.53, 0.63]. Testing the distribution of overestimation directly against the distribution of willingness was not possible because these data are from different participants (and willingness was binary for each payment condition). Also consistent with the original article, the effect of payment was smaller for the self than in estimates for others, Self: $t(1597) = 9.14$, $p < .001$, $d = 0.23$ [0.18, 0.28], Others: $t(1597) = 33.89$, $p < .001$, $d = 0.85$ [0.79, 0.91].

Hypothesis 1B (combined samples). Whether the paid vs. unpaid difference in willingness to donate was related to the estimation of others was examined with a *t*-test. Participants who were willing to donate in the paid but not unpaid condition were labeled price-sensitive, and participants whose willingness did not change on payment (or changed in the other direction) were labeled price-insensitive. Price sensitivity predicted higher estimates of self-interest in others, $t(1596) = -7.67$, $p < .0001$, $d = 0.19$ [0.14, 0.24]. See Table S3 for the tests by sample.

Order effects. Study 1 overestimation was not different based on the order of first completing Study 1 vs. Study 4, $t(189) = -0.56$, $p = .58$, 95% CI of the overestimation difference -8.89, 4.95.

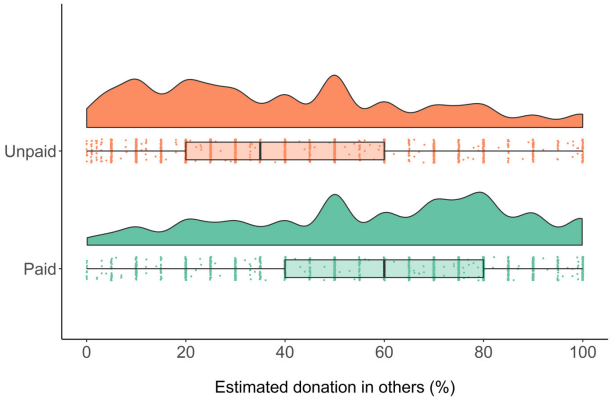

Figure 1. Estimated Donation Willingness (%) by Payment (Study 1; *N* = 1598)

Note. Shown as raincloud plots (Allen et al., 2019). The boxes indicate the interquartile range of that row (25–75%), the whiskers the values within 1.5 times that range, and the vertical black lines the medians.

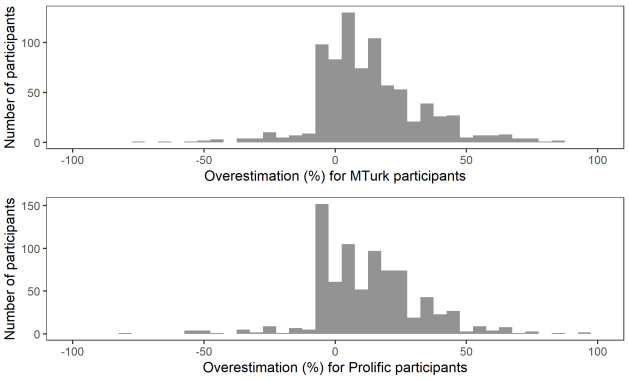

Figure 2. Histogram of Overestimation of Self-Interest in Study 1 (N = 1598)

Summary

Study 1 provided evidence for an overestimation of financial self-interest in willingness to donate blood. Consistent with the original study, payment increased both actual and estimated willingness to donate, and sensitivity to payment predicted higher estimates of self-interest in others. Hypotheses 1A and 1B were both supported with moderate effect sizes. However, the design of Study 1 has an additional confound in that participants were asked to estimate others' donation, not estimate others' self-reported willingness to donate; asking for estimations of the latter could have yielded more accurate estimates. Below, we examined self-interest in an unrelated paradigm about smokers to address this and two other potential issues in Study 1.

Study 4

In Study 4, smokers and nonsmokers indicated their preferences for eight cigarette taxes and smoking restrictions, and estimated the preferences of smokers and nonsmokers addressing three potential limitations in Study 1. First, some individuals may not find financial incentives relevant for prosocial behaviors like blood donation. When the true effect of self-interest could be null or inconsistent, overestimation could be less informative. In contrast, Study 4 uses a scenario with a vested interest that impacts people's attitudes: smoking status. This may provide a more stringent test of overestimation because there is less room to overestimate a positive compared to null effect of self-interest. Second, this design aligns the self-report and the estimates such that others are estimating the same behavior (self-reported policy endorsement). Third, the 1998 study only measured smoking as yes or no, similar to Study 1 only having two conditions (paid and unpaid). In an extension in the MTurk sample, we introduced a five-item ordinal measure of smoking frequency to test for a more granular relationship between self-interest and policy preferences.

Method

Participants were told that the study was to investigate smokers' and nonsmokers' attitudes toward smoking-related policies and were then randomized to the conditions below (see Table 3 to compare the method with the original, and Table S2 for the exact text).

Manipulation 1: Self or Others' Attitudes

Participants were randomized to one of two main conditions. In the Self group, participants indicated their own smoking status and their own attitudes towards policies. In the Others group, participants only estimated what percentage of others would support the policy based on others' smoking status.

Manipulation 2: Smoking Status (MTurk Sample Only)

This extension is about a more precise measurement of smoking status. In the Prolific sample and the original study in the own-attitudes condition, participants reported their smoking status as "yes" or "no". In the MTurk sample only, participants were randomized either to that binary choice or to a five-item ordinal scale: 1 (nonsmokers: never smoked for more than 6 months), 2 (former smokers: not smoking currently, but having smoked for more than 6 months), 3 (light smokers: <10 cigarettes per day), 4 (moderate smokers: 10-20 cigarettes per day), or 5 (heavy smokers: >20 cigarettes per day). Similarly, in the Prolific sample and the original in the others-attitudes condition, participants estimated the policy support of others based on others' smoking status as "smoker" or "nonsmoker", but the MTurk sample only, participants were randomized either to that same design or to estimate others' policy preferences for each of the five categories above.

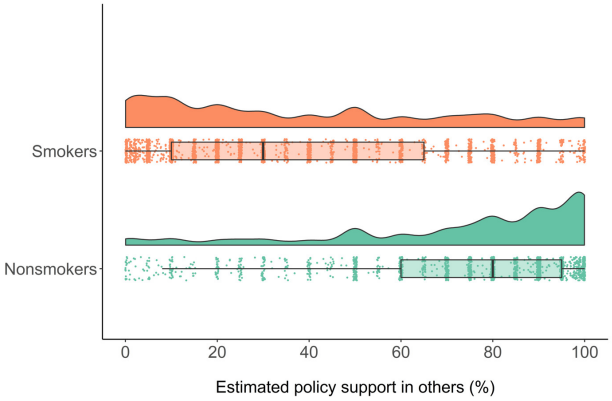

Figure 3. Estimated Policy Support by Others' Smoking Status (*N* = 965)

Note. Shown as raincloud plots (Allen et al., 2019). The boxes indicate the interquartile range of that row (25–75%), the whiskers the values within 1.5 times that range, and the vertical black lines the medians.

Measures

Participants rated attitudes towards eight smoking-related policies: (1) increased tax on cigarettes; (2) a complete ban on cigarette advertisement; (3) a complete ban on smoking in public spaces, and restrictions on smoking in (4) restaurants, (5) workplaces, (6) buses and trains, (7) airplanes, and (8) hotels and motels. Participants in the Self condition rated the items *support*, *oppose*, or *no opinion*. For means and tests, the answers were coded 0 (*oppose*; 12.3%), 0.5 (*no opinion*; 5.5%), 1 (*support*; 82.1%). See below for a robustness check excluding "no opinion" values; the main results are consistent.

Results

Participants in the Others condition estimated support for each smoking policy from 0 - 100 (%) for both smokers and nonsmokers (Table 6 and Figure 3). The mean estimates across policies are shown in Figure 3.

Hypothesis 4A. For each policy, *t*-tests were conducted between the support rates for smokers and nonsmokers (Table S4). Nonsmokers were more supportive than smokers towards the policies, consistent with the original article. Of the 16 *t*-tests (eight policies per study), 13 were significant in the hypothesized direction. This suggests that the poli-

Table 6. Policy Attitudes by Smoking Status in Study 4

		Self		Others (Estimated)	
		Smokers	Nonsmokers	Smokers	Nonsmokers
n MTurk		58	149	206	206
n Prolific		83	317	399	399
Policy	Sample	%		M (*SD*)	
Increase cigarette taxation	MTurk	25.9	74.2	12.3 (*20.6*)	77.5 (*29.5*)
	Prolific	33.1	83.6	13.5 (*16.6*)	79.2 (*22.6*)
Ban cigarette ads	MTurk	66.4	76.2	40.2 (*31.1*)	77.2 (*27.2*)
	Prolific	71.1	90.7	46.5 (*30.2*)	81.1 (*21.3*)
Ban smoking in public places	MTurk	33.6	79.5	18.8 (23.5)	76.6 (*25.9*)
	Prolific	44.0	80.3	19.1 (*21.9*)	77.3 (*23.6*)
Restrict smoking in restaurants	MTurk	72.4	91.6	44.3 (*31.6*)	88.0 (*20.4*)
	Prolific	86.1	97.8	48.4 (*30.2*)	92.2 (*15.1*)
Restrict smoking in workplaces	MTurk	72.4	89.9	43.1 (*31.6*)	85.6 (*22.3*)
	Prolific	80.7	96.1	40.8 (*29.4*)	88.6 (*18.3*)
Restrict smoking on buses and trains	MTurk	88.8	95.3	51.4 (*32.4*)	89.6 (*19.6*)
	Prolific	88.6	97.6	50.2 (*31.2*)	92.6 (*13.9*)
Restrict smoking on airplanes	MTurk	91.4	95.6	63.3 (*34.1*)	91.5 (*20.1*)
	Prolific	90.4	98.7	60.3 (*34.1*)	94.9 (*14.2*)
Restrict smoking in hotels & motels	MTurk	56.9	86.9	32.0 (*27.2*)	84.6 (*22.6*)
	Prolific	69.3	95.1	38.9 (*28.0*)	88.0 (*17.1*)
Total (*M*)	MTurk	63.5	86.2	38.2 (*33.0*)	83.9 (*23.6*)
	Prolific	55.3	92.5	39.7 (*31.8*)	86.7 (*19.6*)

Note: Total rows were calculated with the means of all eight policies within participants and then the mean across participants.

cies were interpreted as being negative for smokers. Note that there is no Hypothesis 3. Hypothesis 4 corresponds to Study 4, and this maintains continuity with the pre-registrations.

Hypothesis 4B: Overestimation. This is the key result in Study 4. First, we conducted one-sample *t*-tests on the replications to compare the actual vs. estimated differences between smokers and nonsmokers. We replicated the discrepancies between self and others in all policies in both samples: estimated self-interest was higher than actual self-interest for all policies (all *p*s < .001) (Table S5).

Overestimation of self-interest was measured by subtracting the estimate for smokers from nonsmokers within each policy, and then taking the mean of all policies within participants to yield an estimate of self-interest. From this value, we subtracted the actual discrepancy between smokers and nonsmokers from the other condition of the study, which was *M* = 27.1%. Overall, participants overestimated the self-interest of smokers by *M* = 19.5% (see Table 7 and Figure 4 for the results by sample). The one-sample *t*-test against $\mu = 0$ for overestimation in both samples combined was $t(604) = 19.9$, $p < .0001$, $d = 0.81$ [0.72, 0.90]. As a separate robustness check, the "no opinion" ratings (5.5% of responses) were excluded and these calculations re-run. The result also showed overestimation across both samples (*M* = 12%) and the interpretation in the Discussion remains consistent with either effect size. Note that policy endorsement

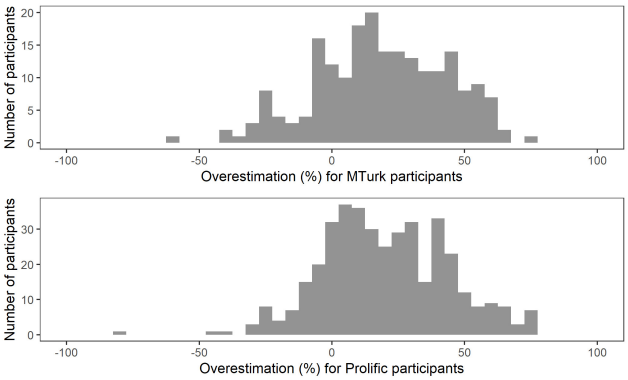

Figure 4. Histogram of Overestimation of Self-Interest in Study 4 (*N* = 605)

was generally high, which could represent a possible ceiling effect.

Order effects. Study 4 overestimation was not different based on the order of first completing Study 1 vs. Study 4, $t(788) = 1.68$, $p = .09$, 95% CI of the overestimation difference -0.40, 5.37.

Table 7. Overestimation of Self-Interest in Study 4

Sample	*M*	*SD*	*df*	*t*	*d*	*95% CI*	Interpretation
MTurk	18.5	*24.9*	205	10.7	0.75	[0.59, 0.90]	Signal Same Direction
Prolific	19.9	*23.7*	398	16.8	0.84	[0.73, 0.96]	Signal Same Direction
Combined	*19.5*	*24.1*	*604*	*19.9*	*0.81*	*[0.72, 0.90]*	*Signal Same Direction*

Note. Overestimation was calculated by subtracting the estimate for smokers from nonsmokers within each policy, and then taking the mean of all policies within participants to yield estimated self-interest. From this value, we subtracted the actual discrepancy between smokers and nonsmokers from the self-reported condition. The one-sample *t*-tests were against $M = 0$ (equivalence); both $ps < .0001$. The lower *df* for MTurk is because of randomization to the extension. The interpretation is based on LeBel et al. (2019), and the size of the effects could not be precisely compared to the original due to missing information.

Extension: Smoking Status Measure (MTurk Sample)

An ordinal smoking status measure was introduced for a random half of the MTurk sample to help test how self-interest impacts actual and estimated attitudes towards smoking policies. Actual and estimated policy support by ordinal smoking status is shown in Table 8.

The rarity of light ($n = 15$), moderate ($n = 18$), and heavy ($n = 6$) smokers meant there was not enough power for inferential tests on actual vs. estimated policy support in the extension. However, visual analysis suggests that overestimation may be most pronounced when individuals consider others with stronger vested interests (here: more frequent smokers). For example, the actual policy support of moderate smokers was $M = 72\%$ and heavy smokers $M = 71\%$, but others sharply underestimated those values (moderate smoker support estimated at $M = 36\%$; heavy smokers $M = 27\%$). Any overestimation effect in these groups should be treated with special caution due to the small samples and the analytic choice to use one-sample t-tests against a certain value, since this does not include variance underlying that value's estimate.

Extension: Communalism (Prolific Sample)

Hypothesis 2 informs whether the individual difference of communalism is associated with prosocial behavior, estimates for others, and the degree of overestimation (Prolific sample only).

Prosocial behavior. In Study 1, correlations suggested that more communal individuals were more likely to donate in both the paid, $r(797) = .16$, $p < .001$, and unpaid conditions, $r(797) = .25$, $p < .001$ (*H2A;* point-biserial). In Study 4, communality appeared unrelated to support for the smoking restrictions for smokers, $r(158) = .03$, $p = .72$, and nonsmokers, $r(455) = .02$, $p = .73$ (*H2B*; Pearson's *r*).

Estimates for others. Correlations suggested that more communal individuals gave higher estimations of others donating blood in the paid, $r(797) = .15$, $p < .001$, and unpaid conditions, $r(797) = .16$, $p < .001$ (*H2C;* Pearson's *r*). In Study 4, communality seemed unrelated to policy estimations for smokers, $r(397) = .04$, $p = .48$, and estimations for nonsmokers, $r(397) = .08$, $p = .09$ (*H2D*; Pearson's *r*).

Overestimation. The key test in this extension was whether more communal individuals would overestimate less; that is, whether their estimates would be more accurate than less communal individuals. Communalism was unrelated to overestimation using Pearson's *r* correlation in Study 1, $r(797) = -.02$, $p = .53$ (*H2E*), or in Study 4, $r(397) = .02$, $p = .72$ (*H2F*). In exploratory correlations between overestimation in both studies with age, social class, gender, skill in English, and participating carefully, most effects were null or small except for younger people overestimating more in Study 1, $r(1596) = -.23$, $p < .0001$. This is consistent with younger people being more sensitive to payment in Study 1 (younger people showed more self-interest; correlation between age and Study 1 self-interest $r(1596) = -.17$, $p < .0001$).

General Discussion

The results in both samples and both studies strongly supported the original findings. Individuals overestimated the impact of self-interest on intentions to donate blood, and also how much smoking status determined support of smoking regulations ($ds > 0.58$). The overestimation effects may have been smaller than the original paper, but original effect sizes could not be precisely calculated because the variances were not reported. Any discrepancies in effect size from the original could be attributed to noise from their small sample size, an estimation error due to the lack of their reported statistics, or differences in the context or manipulation strength. For example, because of currency inflation, $15 was less incentive in 2019 than in 1998, which could lead to smaller perceived incentive in the replication.

In Study 4, the original study did not find significant effects of self-interest for four out of eight policies in self-ratings, perhaps due to lack of statistical power. We found support for self-interest effects for 13 out of 16 tests (smokers endorsed the policies less; eight policies in two samples), with particularly large effects in the MTurk sample (Table S4). Replications often focus on replicating the significant original effects, but finding support for non-significant effects in the original article is also informative (Chandrashekar et al., 2020; LeBel et al., 2019). Here, these additional findings suggest strong generalizability of the overestimation effect across different types of smoking policies (e.g., restriction and taxation).

To evaluate a more granular measure of self-interest, a random half of participants in a Study 4 extension gave responses for five categories of smoking frequency rather than just two. The ordinal smoking status scale did not yield enough smokers within each category for inferential tests. However, it appears from visual analysis that overestimation may be most pronounced when individuals consider

Table 8. Extension: Actual and Estimated Policy Support (%) by Ordinal Smoking Status (MTurk Only, *N* = 377)

	Non-smokers		Former		Light		Moderate		Heavy	
n	105	176	62	175	15	174	18	173	6	174
%	Self	Others	Self	Others	Self	Others	Self	Others	Self	Others
Policy	*M*	*M* (*SD*)	*M*	*M* (*SD*)	*M*	*M* (*SD*)	*M*	*M* (*SD*)	*M*	*M* (*SD*)
Tax	76.7	77.3 (29.3)	70.2	60.9 (33.7)	36.7	22.2 (27.6)	30.6	14.8 (24.0)	33.3	11.2 (24.4)
Ads	76.7	76.9 (31.1)	79.8	67.6 (33.2)	50.0	40.0 (32.0)	88.9	34.6 (31.4)	66.7	29.5 (32.0)
Public	85.7	79.4 (29.7)	64.5	63.9 (33.0)	36.7	29.6 (31.4)	33.3	21.9 (27.7)	66.7	15.8 (26.4)
Resta.	97.1	87.6 (25.8)	91.9	76.7 (30.4)	66.7	50.9 (34.2)	77.8	43.0 (33.0)	100	31.2 (31.8)
Work	93.8	86.3 (25.7)	90.3	72.7 (32.6)	73.3	46.5 (33.9)	88.9	37.0 (31.8)	50.0	25.8 (30.3)
Bus	96.7	86.0 (28.0)	96.8	75.0 (32.3)	73.3	55.0 (35.1)	88.9	45.8 (34.2)	100	34.2 (33.5)
Plane	96.7	89.5 (26.0)	99.2	80.3 (32.1)	73.3	63.6 (35.0)	94.4	56.9 (36.1)	83.3	47.0 (37.1)
Hotel	93.3	84.1 (28.8)	84.7	71.0 (33.1)	76.7	41.7 (32.7)	77.8	32.3 (30.0)	66.7	21.7 (27.2)
Total (*M*)	89.6	83.40 (28.5)	84.7	71.0 (33.1)	60.8	43.7 (35.1)	72.6	35.8 (33.5)	70.8	27.1 (32.3)

others with stronger vested interests. In the extension, that pattern could be partially due to an expectancy effect. Participants may have assumed that being asked about multiple categories of smoker implied that each category would be different in policy support.

The other extension investigated individual differences that predict overestimation. The social norm in Western individualistic cultures that self-interest powerfully determines behavior may be relevant to overestimation (Ratner & Miller, 2001). Beliefs about self-interest may become self-fulfilling by influencing social institutions and individual decision-making processes, which in turn could reinforce the original idea of self-interested human nature. Therefore, communalism was tested in predicting donation, policy support, estimates of each, and overestimation of self-interest. As expected, communality was positively associated with more prosocial behavior and endorsement of smoking restrictions, and was also positively associated with higher estimates of others' prosociality in both studies. However, we found no support for a relationship between overestimation and communality in either study. Exploratory correlations with other demographics revealed mostly null effects, but being younger was associated with more overestimation in Study 1, perhaps because younger individuals have less money. It remains valuable to identify other individual differences associated with overestimation.

Limitations and Future Directions

Alternative Explanations

Self-reported willingness to donate blood or endorse smoking policies is not equivalent to objective behaviors like blood donation or voting. The main narrative in this paper is that people over-estimate others' self-interest, but the results are also consistent with the pattern that such estimates are accurate and that self-reported willingness is inaccurate; that in actual behavior people would manifest more self-interest than they expect or are willing to report. Further studies with observed behavior would be valuable for testing this account.

The experimental paradigms were copied from the original manuscript and not validated before testing the hypotheses. The vignettes and manipulations might have confounds or unknown effects orthogonal to the theory and predictions used here. Additionally, the participants were only given very sparse information about the targets, e.g., that they were smokers or nonsmokers. This could have created an expectancy effect or at the least an ecologically unusual focus on a single attribute when predicting how individuals would evaluate policies. By failing to provide rich, complex targets with varied mental experiences, the paradigms here may have encouraged individuals to focus on external behaviors like smoking, which could alter attributions and perceived self-interest (Vuolevi & Van Lange, 2009). Future studies could consider richer, more life-like vignettes, or paying participants for their accuracy.

Attitudes versus Behaviors

The original article and the current replications hinge on outcomes that may be better characterized as intentions rather than behaviors. This is important because self-interest may predict behavior better than attitudes (Ratner & Miller, 2001). For instance, one study found that people who owned property or had school-age children did not op-

pose school busing policy more than those without material stake in the policy, but they were much more likely to join anti-busing organizations (Green & Cowden, 1992). Another key paper found that people overestimated their likelihood of acting generously but accurately predicted other's behaviors (Epley & Dunning, 2000). Perceived self-interest may be higher when people face immediate, concrete outcomes (Boninger et al., 1995), and people's sensitivity to their self-interest increases after self-interest is made salient (Ratner & Miller, 2001). Thus, future research on the overestimation of self-interest could focus on consequential behaviors rather than hypotheticals. This could help resolve conflicting findings (Epley & Dunning, 2000; Vuolevi & Van Lange, 2009) and provide better generalizability to real-world contexts.

Constraints on Generality

The current findings and their interpretation are based on sampling and measurement choices that limit their generalizability as with any study (Simons et al., 2017).

Sample. The participants were recruited from MTurk (USA) and Prolific (UK). Both samples were more representative of their countries than university student samples, but the results may have limited generalizability to populations that are not Western, Educated, Industrialized, Rich, and Democratic (Cheon et al., 2020; Henrich et al., 2010). In particular, overestimation of others' self-interest may be inflated by social norms of self-interest in individualistic societies. There is a strong need for studies on overestimation of others' self-interests in non-Western samples. Cross-cultural, multi-lab studies such as through consortia like the Psychological Science Accelerator (Moshontz et al., 2018) could replicate and extend this phenomenon particularly in collectivistic cultures with weaker norms of self-interest.

Method, Measures, and Contexts. We closely replicated the original studies across two medical topics—blood donation and smoking—measuring attitudes and intentions but not objective behavior. Our results appeared to contradict Epley & Dunning (2000), but were consistent with Vuolevi & Van Lange (2009), which both measured behavior. These discrepancies could be due to differences in measures or topics. Future replication studies could focus on consequential behaviors and consider other decision contexts such as financial or environmental decisions.

Overestimating self-interest may also be higher when participants lack information about the other people making decisions. When study vignettes refer to unspecified others and only provide limited information, e.g., the decision maker is a smoker or not, participants may base their estimates on generalized perceptions of norms of self-interest (Vuolevi & Van Lange, 2009). Therefore, future studies could investigate contexts in which participants have more specific information or richer interactions with the estimation targets.

Additionally, there was a possible ceiling effect in self-reported policy endorsement in Study 4. This could have led to an artificially smaller difference between estimates and self-reported preferences due to the specific policies. That is, for a different set of policies, one might observe even more overestimation without this restriction in range.

Conclusion

We presented two well-powered, pre-registered studies across two samples that both successfully replicated the original Studies 1 and 4 by Miller & Ratner (1998) using online surveys of U.S. and U.K. residents. Individuals strongly overestimated the power of self-interest on others' blood donation willingness and smoking policy preferences. Self-interest may act as a self-fulfilling social norm (Ferraro et al., 2005), and therefore overestimation has broad implications for cooperation within and between social groups and institutions. Across a society, perceived self-interest is important because it could affect support for laws against individual interests such as on environmental issues. Our results that people overestimate self-interest could potentially help reduce demagogy appealing to individual interests. We encourage future studies to further investigate the generalizability and boundary conditions of the overestimation effect (Simons et al., 2017).

Data accessibility statement

Open data, code, materials, and the 2019 pre-registrations are available at https://osf.io/57mdc/.

Competing interests

The authors declared no potential competing interests with respect to the authorship and/or publication of this article.

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Authorship declaration

Please see the table below. The pre-registration authors were primarily responsible for the empirical methods, results, and first drafts, and therefore were essential. All authors approved the final manuscript for submission.

Contributor roles taxonomy

Role	HY	WC	LH	WM	GF	AF	WY	CB
Conceptualization					•			
Pre-registration	•	•	•	•				
Data curation								
Formal analysis	•	•	•	•				•
Funding acquisition					•			
Investigation	•	•	•	•				
Pre-registration peer review / verification					•	•	•	•
Data analysis peer review / verification					•	•	•	•
Methodology	•	•	•	•				
Project administration					•			•
Resources					•			
Software	•	•		•		•		•
Supervision					•			•
Validation	•	•				•		•
Visualization	•	•		•		•		•
Writing-original draft	•	•	•	•				•
Writing-review and editing					•	•	•	•

Note. See https://www.casrai.org/credit.html for the details and definitions of each role.

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