Article

Does Subjective SES Moderate the Effect of Money Priming on Socioeconomic System Support? A Replication of Schuler and Wänke (2016)

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Abstract

Findings that money priming increases socioeconomic system support have proven difficult to replicate. Schuler and Wänke found that subjective socioeconomic status (SES) moderates money priming effects on system justification and belief in a just world. We conducted three preregistered replications of this research, with sample sizes 3 times those of the original studies. Replication 1 was a conceptual replication that combined elements from the original two studies, and Replications 2 and 3 were close replications of Studies 1 and 2, respectively. None of the four subjective SES × Money Prime interaction effects tested were statistically significant, and only one of the four survived a "small telescopes" analysis. We discuss reasons for our general failure to replicate the original findings and implications for money priming effects.

Keywords

money priming, priming effects, replication, system justification, BJW

Vohs, Mead, and Goode (2006) reported that subtle money cues impacted a range of behaviors including working independently without asking for assistance, contributing less to charity, and providing less assistance to experimenters and confederates. Other research found that money priming effects decreased empathy, compassion, and people's willingness to volunteer and donate time and money (Chatterjee, Rose, & Sinha, 2013; Molinsky, Grant, & Margolis, 2012; Pfeffer & DeVoe, 2009).

Further, Caruso, Vohs, Baxter, and Waytz (2013) reasoned that because money is a symbol of American free-market capitalism, subtle money reminders would make people more accepting of free market and other belief systems that justify existing structural inequality. They reported five studies suggesting that money priming increased support for such beliefs systems (i.e., system justification, belief in a just world [BJW], social dominance orientation, and fair market ideology).

These findings have been called into question by notable failed replication attempts. Rohrer, Pashler, and Harris (2015) reported four high-powered replication attempts of Studies 1–4 from Caruso et al. (2013), with sample sizes 2–4 times larger than the original studies. No effects were significantly different from zero. They also uncovered questionable research practices in Vohs et al.'s (2006) and Caruso et al.'s (2013) research, including failures to report studies or results on outcome variables inconsistent with expected money priming effects. These practices led to overestimated effect sizes in

those reports (Rohrer, Pashler, & Harris, 2015). Further, in the Many Labs Replication Project's (Klein et al., 2014) replication of Caruso et al. (2013), only one of the 36 labs observed a significant effect; the aggregate effect size across labs was in the opposite direction and not significantly different from zero.

More recently, Schuler and Wänke (2016) reasoned that money primes may only increase support for the socioeconomic system among people whom the system benefits—specifically, people high in subjective socioeconomic status (SES). Schuler and Wänke (2016) used Caruso et al.'s (2013; study 2) word descrambling task to manipulate money priming, measuring two of the socioeconomic system support outcomes included in Caruso et al. (2013): system justification and BJW. As in Rohrer et al. (2015) and Klein et al. (2014), there were no money priming main effects on system justification or BJW. Rather, money priming effects were moderated by subjective SES. This interaction effect was statistically significant in Schuler and Wänke's (2016) study 1 ($p = .02$) and marginally significant in study 2 ($p = .06$), but not significant in a third

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study reported in their supplemental materials ($p = .36$); however, a meta-analysis of these three studies supported the conclusion that subjective SES moderates money priming effects on socioeconomic system support.

The robustness of factors that influence money priming effects is of clear interest to the field (Klein et al., 2014; Rohrer et al., 2015; Schuler & Wänke, 2016); if Schuler and Wänke (2016) have identified a reliable moderator of money priming effects, it is valuable to observe independent replication. We therefore conducted three replications of Schuler and Wänke's (2016) studies. Replication 1 was a conceptual replication that included elements from both Studies 1 and 2 reported in Schuler and Wänke (2016). Replications 2 and 3 were close replications of Studies 1 and 2, respectively. We received original materials from Johannes Schuler. We preregistered our hypotheses and analysis plan on the Open Science Framework and followed the templates for the Replication Recipe (Brandt et al., 2014) for Replication 1 and AsPredicted (https://aspredicte d.org/) for Replications 2 and 3 (https://osf.io/t3xps/).

Method

Participants

Schuler and Wänke (2016) recruited 150 U.S. residents for each study. Simonsohn (2015) recommends that replications recruit samples at least 2.5 times the size of the original study. We therefore recruited more than 375 U.S. residents for each replication (Replication 1: $N = 462$; Replication 2: $N = 460$; and Replication 3: $N = 448$). Replication 1 was collected in June 2016. Replications 2 and 3 were collected simultaneously in May 2017; participants were randomly assigned to one or the other replication. Schuler and Wänke (2016) recruited participants through Crowdflower; we used Mechanical Turk, a similar online recruitment platform. Participants were compensated between US$0.25 and US$0.30 in the original studies, but US$0.50 in the replications.

In their primary analyses, Schuler and Wänke (2016, p. 367) excluded participants who "did not follow the instructions to create four-word phrases" in the descrambling task. Personal communication with Johannes Schuler (July 10, 2016) indicated that the original authors excluded participants who committed three or more errors (e.g., blank space, nongrammatical phrase, and three- or five-word phrase). We followed this exclusion criterion.[1] After exclusions, 436, 418, and 425 participants remained in Replications 1–3, respectively, which exceeds the target sample size of 375 (see Online Supplemental Material for gender, age, income, and education demographics for each sample).

Materials and Procedure

Replication 1. Participants learned that the purpose of the study was to "examine effects of playful learning methods" and "assess interpersonal skills." They first encountered the descrambling task manipulation. Participants were asked to form a four-word grammatical sentence from a five-word string

(e.g., "you held pencil building the" becomes "you held the pencil"). Participants in the money condition descrambled 15 strings with money-related words (e.g., "won green the I lottery") and 15 strings with words unrelated to money (e.g., "on printer grass she walked"). Participants in the control condition descrambled 30 strings with words unrelated to money. String order was randomized within each condition. In Schuler and Wänke's (2016) study 1, a mix of personal and third-person pronouns were included in the stimuli, whereas pronoun type (personal or third person) experimentally varied in Schuler and Wänke's (2016) study 2. Because Schuler and Wänke's (2016) study 2 revealed no effect of pronoun usage, we did not manipulate pronoun usage in Replication 1 and used only strings from the personal pronoun condition of Schuler and Wänke's (2016) study 2.

After the descrambling task, participants completed the manipulation check, which consisted of 15 words stems, of which seven could be completed with either money-related or non-money-related words (e.g., "sp" as either "spend" or "spider"). Words stem order was randomized.

In Schuler and Wänke (2016), participants completed the 8-item System Justification Scale (e.g., "American society has to be radically restructured;" $1 = strongly disagree$; $7 = strongly agree$) and the 20-item BJW Scale (e.g., "It is rare for an innocent to be wrongly sent to jail"; $1 = strongly disagree$; $7 = strongly agree$) in Studies 1 and 2, respectively (Caruso, Vohs, Baxter, & Waytz, 2013). In Replication 1, participants completed both scales in random order. Item order on each scale was randomized.

Schuler and Wänke (2016) measured subjective SES with the MacArthur Scale of subjective SES (Piff, Kraus, Côté, Cheng, & Keltner, 2010), which asks participants to imagine a 10-rung ladder representing the social hierarchy in the United States, where the top and bottom rungs represent the highest and lowest standings in the social hierarchy, respectively. Participants placed themselves on this ladder ($1 = worst off$; $10 = best off$). In study 1, Schuler and Wänke (2016) varied whether subjective SES was measured at the beginning or end of the survey and found no order effect, so therefore assessed subjective SES only at the end of study 2. In Replication 1, we also assessed subjective SES only at the end of the survey. Lastly, participants provided demographic information including gender, age, ethnicity, English-language proficiency, marital status, political orientation, and political party identification.[2]

Replication 2. Replication 2 was a close replication of Schuler and Wänke's study 1. The subjective SES measure was presented either directly before the manipulation or directly after the dependent variable (system justification). Participants were randomly assigned to the control condition or money condition (which included a mix of personal and third-person pronouns as in Schuler and Wänke's study 1), which was followed by the manipulation check, the System Justification Scale, and demographic information. Following Schuler and Wänke, System Justification Scale item order was not randomized. No other

Table 1. Correlations Among and Descriptive Statistics for Subjective SES, System Justification, and BJW.

	1	2	3
1. Subjective SES			
2. System justification	.22**, .24***, NA		
3. BJW	.13**, NA, .12*	.62***, NA, NA	
M	5.09, 5.00, 5.04	3.70, 3.67, NA	3.93, NA, 4.06
SD	1.71, 1.72, 1.75	1.00, 1.17, NA	0.59, NA, 0.62
α	NA	.80, .87, NA	.73, NA, .76

Note. Within each row, statistics are reported in order from Replications 1 and 3. NA = not applicable; BJW = belief in a just world; SES = socioeconomic status.
*p < .05. **p < .01. ***p < .001.

measures were included, and no alterations were made to Schuler and Wänke's study 1 procedures.

Replication 3. Replication 3 was a close replication of Schuler and Wänke's study 2. Participants were first randomly assigned to the control, my-money condition (all personal pronouns), or their-money condition (all third-person pronouns). The only procedural alteration made in Replication 3 is that Schuler and Wänke's (2016) study 2 did not include the manipulation check; however, we did include it directly between the manipulation and the dependent variable (BJW) because it is necessary to determine the prime's effectiveness (Johannes Schuler, personal communication, May 13, 2016). Following Schuler and Wänke, BJW Scale item order was not randomized. We then measured subjective SES, followed by demographic information. No other measures were included.

Results

Correlations among variables and descriptive statistics are reported in Table 1. In each replication, subjective SES was positively correlated with both system justification and BJW, and its relationship to system justification was stronger than it was with BJW. System justification and BJW were strongly positively correlated in Replication 1.

The central questions of interest are whether (a) the "money" concept was successfully primed by the manipulation and (b) subjective SES moderated effects of money priming on the outcome measure(s). We report tests of those questions below.

Was the Money Concept Successfully Primed?

As per Schuler (personal communication, May 13, 2016), it is necessary to test whether there were significant differences between conditions in the word descrambling task in order to draw conclusions regarding money priming effects on downstream processes (e.g., system justification, BJW).

Replication 1. Participants completed the word stems in the manipulation check with more money-related words in the money condition ($M = 1.01$, $SD = 1.09$) than in the control condition ($M = 0.53$, $SD = 0.75$), $t(434) = 5.26$, 95% confidence intervals (CIs) [.30, .66], $p < .001$, $d = .51$ (95% CIs [.31, .70]).

Replication 2. Participants completed the word stems in the manipulation check with more money-related words in the money condition ($M = 0.71$, $SD = 0.83$) than in the control condition ($M = 0.47$, $SD = 0.72$), $t(416) = 3.13$, 95% CIs [.09, .39], $p = .002$, $d = .31$ (95% CIs [.11, .50]).

Replication 3. Replication 3 is a replication of Schuler and Wänke's (2016) study 2, which did not include the manipulation check. Because there is no guidance on the most appropriate analytic choice for the manipulation check, we present two alternative analyses (neither of which were preregistered).

A one-way analysis of variance indicated significant differences between the three groups in completing the word stems with money-related words, $F(2, 422) = 4.61$, $p = .010$, $\eta_p^2 = .02$. Although means appeared higher in the their-money ($M = 0.83$, $SD = 1.00$) and my-money ($M = 0.71$, $SD = 0.96$) conditions relative to the control condition ($M = 0.50$, $SD = 0.73$), Tukey post hoc comparisons indicated that only the "their-money" and control conditions were significantly different from each other (95% CIs [.07, .58], $p = .008$); the "my-money" versus control (95% CIs [−.05, .46], $p = .130$) and my-money versus their-money (95% CIs [−.14, .37], $p = .553$) comparisons were not significant.

Because the hierarchical regression model used to test the subjective SES × Prime Effect on BJW utilizes a contrast that treats the two money conditions the same, we also conducted an independent samples t test comparing money-related word completion in the control condition ($n = 137$, $M = 0.50$, $SD = 0.73$) to that of the two money conditions combined ($n = 288$, $M = 0.77$, $SD = 0.98$). This difference was significant, $t(423) = 2.84$, $p = .005$, $d = .30$, 95% CIs [.09, .50]). None of the effects in Replications 1–3 were moderated by subjective SES, all ps > .123.

Did Subjective SES Moderate Money Priming Effects on System Justification and BJW?

Replication 1. As in Schuler and Wänke (2016), we used Hayes' (2013) PROCESS macro to perform hierarchical regression analyses. In each model, the outcome was regressed on prime (−1 = control, 1 = money; mean centered), subjective SES (mean centered), and the Prime × Subjective SES interaction.

Schuler and Wänke observed main effects of subjective SES on system justification and BJW, which we also observed (system justification: $b = .12$, $SE = .03$, 95% CI [.07, .18], $t = 4.30$, $p < .001$; BJW: $b = .05$, $SE = .02$, 95% CI [.01, .08], $t = 2.59$, $p = .010$). As in Schuler and Wänke, we observed no prime main effects (system justification: $b = −.001$, $SE = .05$, 95% CI [−.10, .09], $t = −0.03$, $p = .979$; BJW: $b = −.01$, $SE =$

Table 2. Results of Small Telescopes Analyses of Replications 1–3.

Replications	Original N	Replication N	Point Estimate (β)	Power, Point Estimate (%)	β, Upper 90% CI	Power of β, Upper 90% CI (%)
Replication 1 (system justification)	124	432	.088	13.4	.182	39.6
Replication 1 (BJW)	124	413	.008	4.6	.064	15.2
Replication 2 (system justification)	124	418	Negative β, p > .05	<33	Negative β, p > .05	<33
Replication 3 (BJW)	118	413	.018	6.8	.061	22.8

Note. Discrepancies between *N*s reported in this table and in the text reflect missing data. Note that in Replication 2, effects were in opposite direction, and so small telescopes analyses were not conducted. BJW = belief in a just world; CI = confidence interval.

.03, 95% CI [−.06, .05], $t = -0.18$, $p = .858$). However, the critical Subjective SES × Prime interactions were not observed for system justification ($b = .04$, $SE = .03$, 95% CI [−.01, .10], $t = 1.53$, $p = .127$) or BJW ($b = .004$, $SE = .02$, 95% CI [−.03, .04], $t = 0.23$, $p = .820$).

We considered the possibility that the priming effects were temporary, and therefore performed exploratory subjective SES × Prime × Order interactions on system justification and BJW to test whether the interaction effect was stronger when the key outcome variable was presented first (e.g., when system justification was measured before BJW). There was a marginally significant three-way interaction on system justification ($p = .063$). However, the pattern of findings was *inconsistent* with the possibility that money priming effects are stronger on system justification when it is presented first, because if anything, they were stronger when BJW was presented first. There was no significant three-way interaction on BJW ($p = .651$).

Replication 2. Following Schuler and Wänke's (2016) study 1, we regressed system justification on prime (−1 = control, 1 = money; mean centered), subjective SES (mean centered), SES order (−1 = before manipulation, 1 = after dependent variable; mean centered), and all interaction terms. As in Schuler and Wänke's (2016) study 1, there was a main effect of subjective SES, $b = .16$, $SE = .03$, 95% CI [.10, .22], $t = 5.24$, $p < .001$, and no prime main effect, $b = -.001$, $SE = .06$, 95% CI [−.06, .05], $t = -0.02$, $p = .983$. Unlike Schuler and Wänke, there was an SES order main effect, $b = .11$, $SE = .06$, 95% CI [.004, .22], $t = 2.03$, $p = .043$, such that system justification was higher when subjective SES was measured after the dependent variable compare to before the manipulation. The critical subjective SES × Prime interaction effect was not significant and in the *opposite direction* of the original effect, $b = -.04$, $SE = .03$, 95% CI [−.10, .02], $t = -1.21$, $p = .228$. No other higher order effects were significant (ps > .250).

Replication 3. Following Schuler and Wänke's (2016) study 2, we created one contrast for the "my" and "their" money conditions versus the control condition (.5, .5, −1, respectively; mean centered), and one contrast between the my and their money conditions (−1, 1, control = 0; mean centered), and regressed BJW on these two contrasts, subjective SES (mean

centered), and the interactions between subjective SES and each contrast. There was a main effect of subjective SES, $b = .04$, $SE = .02$, 95% CI [.004, .07], $t = 2.17$, $p = .031$, but no main effects for either prime contrast (money vs. control contrast: $b = .02$, $SE = .04$, 95% CI [−.07, .11], $t = 0.49$, $p = .624$; my vs. their contrast: $b = -.07$, $SE = .04$, 95% CI [−.14, .01], $t = -1.80$, $p = .073$). Inconsistent with Schuler and Wänke, the critical interaction between subjective SES and the first contrast was not significant, $b = .02$, $SE = .03$, 95% CI [−.03, .07], $t = 0.71$, $p = .477$. The interaction between subjective SES and the other contrast was also not significant, $b = -.04$, $SE = .02$, 95% CI [−.08, .003], $t = -1.85$, $p = .066$.[3]

Would the Replication Effects Have Been Detectable in the Original Studies?

We adapted Simonsohn's (2015) "small telescopes" approach to evaluate the success of our three replications. This approach examines whether the effect sizes observed in our replications would have been detectable in the original study. In other words, were our observed effects so small as to be undetectable using Schuler and Wänke's (2016) sample sizes? To do so, we estimated the power of each subjective SES × Prime interaction effect given the original studies' sample sizes and calculated the 90% CIs around this estimate, as well as the power associated with the upper bound of that CI. If the estimated power of the point estimate is lower than 33%, and especially if estimated power at the upper bound of the 90% CI is less than 33%, then the original study would not have had adequate power to capture the effect observed in the replication. All of these analyses were performed using a simulation method to construct CIs and estimate statistical power empirically (Ruscio, 2017).

Table 2 reports the original and replication sample sizes, effect size estimates (β) for each interaction effect and their corresponding power estimates, and effect size estimates at the upper bound of the 90% CI and their corresponding power estimates. In no case did the power for the point estimate of the effect size exceed 33%, and the power estimate at the upper bound of the 90% CI exceeded 33% in only one case (39.6%; Replication 1, system justification). These findings suggest very weak statistical power to detect the effect sizes we observed in our replications given the sample sizes in the original studies.[4]

Discussion

We conducted three preregistered replications of Schuler and Wänke's (2016) studies, each with sample sizes 3 times larger than the originals. We replicated Schuler and Wänke's (2016) observed lack of money priming main effects and their observed main effects of subjective SES on system justification (note that we observed main effects on BJW, which they did not observe). However, inconsistent with Schuler and Wänke (2016), subjective SES did not moderate money priming effects on either system justification (Replications 1 and 2) or BJW (Replications 1 and 3), as none of the subjective SES × Prime interactions reached statistical significance (all ps > .127).

We supplemented these traditional decisions about replicability (i.e., Is p < .05 in the predicted direction?) with a small telescopes approach (Simonsohn, 2015) examining whether the original studies had sufficient power to observe the interaction effects obtained in the replication. Statistical power never exceeded 33% for a point estimate of effect size, and the power estimate for the effect size at the upper bound of the 90% CI exceeded 33% (specially, 39.6%) in only one of the four cases (Replication 1, system justification), meaning we could not rule out failure to replicate in this one case.

Why Did the Original Studies Largely Fail to Replicate?

There are several possible reasons for why Schuler and Wänke's original effects largely failed to replicate in these three replication studies.

Discrepancies between the original and replication studies. Although Replications 2 and 3 were close replications of Schuler and Wänke's (2016) Studies 1 and 2, respectively, Replication 1 was a conceptual replication, borrowing elements from both. It is unlikely that its status as a conceptual replication explains failure to replicate, however. First, whereas the interaction effect on BJW failed to replicate in Replication 1, it also failed to replicate in the close replication (Replication 3). Second, Replication 1 is the only study that actually produced results consistent with Schuler and Wänke's original finding (i.e., the small telescopes analysis on the nonsignificant interaction effect on system justification).

The original findings are false positives. Schuler and Wänke's (2016) original findings may have been false positives. Samples sizes of the original studies were 3 times smaller than ours. Regarding BJW, none of our interaction effects were significant, and the small telescopes analyses suggested that the original studies did not have the power to detect these effects. Coupled with the fact that the original studies produced marginally significant (study 2) and nonsignificant (Supplemental Online Material study) interaction effects on BJW, one might conclude that the original effects on BJW are false positives. The answer is less clear for system justification. The interaction effect was significant in the original study (p = .02). In Replication 1, the coefficient was nonsignificant (p = .127) but in

the expected direction, and although the effect size point estimate was below 33%, the small telescopes indicated that we could not rule out the possibility that the original study had sufficient power to observe this effect. However, the coefficient in Replication 2 was nonsignificant (p = .228) and in the opposite of the hypothesized direction.

One might conclude that whereas BJW effects are not replicable, system justification effects *might be* replicable. We cannot rule out this conclusion. If this is the case, however, it is difficult to conclude that subjective SES moderates money priming effects on socioeconomic system support when such effects do not replicate on one of the two outcome variables Schuler and Wänke used to measure socioeconomic system support (especially when those variables are highly correlated; r = .62 in Replication 1). It further suggests that it may not have been appropriate for Schuler and Wänke to meta-analyze their findings across system justification and BJW measures.

On a related note, one could argue that Schuler and Wänke' (2016) never actually reported evidence for their own hypothesis. They write that "people who would view themselves more as the beneficiaries of the socioeconomic system should endorse the system after being reminded of money" (p. 367), suggesting that money priming effects should emerge among people high but not low in subjective SES. However, rather than reporting the critical comparisons between the control and money conditions among people high in subjective SES, they only reported and interpreted the simple slopes from the interactions.

Money thoughts were not effectively primed. Schuler and Wänke (2016) included a manipulation check in study 1 and in their Supplemental Online Material study, but not in study 2. They reported a significant effect in study 1 (p < .001, d = .62) and a marginally significant effect in their Supplemental Online Material study (p = .08, d = .32). We observed significant differences between the control and money conditions in Replications 1 (p < .001, d = .51) and 2 (p = .002, d = .31). Schuler and Wänke did not include a manipulation check in study 2, so the preferable test of the manipulation check in Replication 3 is unclear. Given that Schuler and Wänke's test of their interaction effect in study 2 involves a contrast that treats the two money conditions similarly, a comparison between the control condition and the two money conditions combined is arguably the most appropriate approach to the manipulation check. This revealed a money priming effect (p = .005, d = .31). Given statistically significant group differences in all three replications (ps < .006) with effect sizes whose CIs did not cross zero and were commensurate with typical social psychology effects (Richard, Bond, & Stokes-Zoota, 2003) and with typical priming effects (Pashler, Coburn, & Harris, 2012), money priming appears to have increased money-related thoughts in these studies.

One may question whether the money primes were strong enough to produce downstream interaction effects on the outcome variables. Schuler and Wänke (2016) observed a

significant interaction effect on system justification ($p = .02$) with a medium manipulation check effect ($d = .62$) in study 1, and a nonsignificant interaction effect on BJW ($p = .36$) with a small manipulation check effect ($d = .32$) in their Supplemental Online Material study. In our replications, the only replication that produced evidence potentially consistent with Schuler and Wänke's interaction effect (Replication 1, system justification) yielded a relatively larger manipulation check effect size ($d = .51$) than did Replications 2 and 3 ($ds = .31$). One might therefore conclude that the magnitude of the manipulation check effect corresponds to support for the hypothesized interaction effect on the outcome variable. We cannot rule out this possibility. However, two facts give us pause in reaching this conclusion. First, it is difficult to discern such a pattern from so few studies. Second, any relationship between the manipulation check and interaction effect strength is confounded by outcome variable use—Schuler and Wänke observed a weaker manipulation check in their study using BJW than in their study using system justification, and as mentioned above, our results suggest that the effect on BJW is less replicable than on system justification.

One might also look to compare our observed effect sizes on the manipulation check to larger ones found in the literature (e.g., Vohs, Mead, & Goode, 2006: $d = .72$; Boucher & Kofos, 2012: $d = .74$). This would be problematic, though, because this literature is characterized by overestimated effect sizes (Rohrer et al., 2015). In a recent preregistered study, Caruso, Shapira, and Landy (2017) found effect sizes on the manipulation check that ranged from .24 to .40, commensurate with our own observed effect sizes (but as with our findings, no consistent money priming effects on socioeconomic system support).

What Are the Implications for Money Priming Research?

We were able to rule out replicability of Schuler and Wänke's (2016) original findings in three of four tests. What do our findings mean for money priming effects? If we conclude that Schuler and Wänke's effects do not replicate on BJW, but might on system justification, then future research should explore whether these effects are observed on measures of socioeconomic system support included in Caruso et al.'s (2013) original work but not in Schuler and Wänke's (i.e., SDO, fair market ideology). It may be useful to note that there was no interaction effect on economic conservatism (a proxy measure of fair market ideology) in Replication 1 (see Supplemental Online Material).

If we conclude that Schuler and Wänke's effects do not replicate on either BJW or system justification, especially given the centrality of system justification to the construct of socioeconomic system support, then it may mean at least one of two things. First, despite the fact that Schuler and Wänke selected a very reasonable potential moderator (subjective SES), future work might consider other more reliable potential moderators. That said, Caruso et al. (2017) found sparse and inconsistent moderation of money priming effects across a number of demographic factors (e.g., gender, ideology),

including the conceptually related objective SES (i.e., education and income; see Supplemental Online Material for similar null interaction effects in our data). Second, Schuler and Wänke's work is premised on the idea that Caruso et al.'s (2013) may be difficult to replicate (e.g., Caruso, Shapira, & Landy, 2017; Klein et al., 2014; Rohrer et al., 2015; their data and our own) because there is a hidden moderator. However, it may simply be the case that any evidence for money priming effects on socioeconomic system support were false positives (cf. Rohrer et al., 2015), and there is no moderator to be uncovered.

Supplemental Material

The supplemental material is available in the online version of the article.

Notes

1. Prior to confirming this exclusion criterion with the original authors, we used a criterion in Replication 1 that excluded participants who made at least one error. Because we preregistered both this erroneous exclusion criterion and Schuler and Wänke's original exclusion criterion for Replications 2 and 3, we report results under this erroneous exclusion criterion in Supplemental Online Material.
2. In Replication 1, we also included measures of economic and social conservatism, which always followed the system justification and Belief in a Just World (BJW) Scales and preceded the subjective socioeconomic status (SES) measure. After the subjective SES measure, we also included an alternative subjective SES measure and an objective SES measure. We preregistered our hypotheses for all of these measures except the alternative subjective SES measure. These analyses are reported in Supplemental Online Material.
3. Schuler and Wänke footnoted a more conservative set of exclusion criteria, excluding nonnative English speakers and those who interrupted the study (the upper quartile + 1.5 interquartile range in time to survey completion). Employing these additional exclusion criteria in all three replications did not change the conclusions. All manipulation checks were significant ($ds = .57, .28$, and $.32$ in Replications 1–3, respectively), and none of the interaction effects were significant (Replication 1, system justification: $b = .09, p = .13$; Replication 1, BJW: $b = -.003, p = .944$; Replication 2: $b = -.04, p = .245$; and Replication 3: $b = .02, p = .478$). We did not preregister these alternative criteria analyses.
4. These conclusions do not change under the alternative exclusion criteria described in Note 3.

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