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Does Money Change Political Views? – An Investigation of Money Priming and the Preference for Right-Wing Politics

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Abstract

In a multistudy approach across seven studies we explored whether, as suggested by previous research, money primes affect people's political orientation. Across the studies we used different dependent variables and samples, and we combined the results in a small-scale meta-analysis to test two competing hypotheses. Independent of the measures and experimental setting, our findings did not indicate that money primes lead to stronger right-wing orientations (main-effect hypothesis). However, we obtained a marginally significant interaction effect suggesting that the money priming effect is moderated by subjective socioeconomic status (moderation hypothesis). These findings suggest that, contrary to previous research, the money priming effect on political orientation is at best small and dependent on one's subjective socioeconomic status. Implications for money priming research and political psychology are discussed.

Keywords: money, priming, political orientation, subjective socioeconomic status, meta-analysis

Non-Technical Summary

Background

Previous research suggested that merely reminding people unobtrusively of the concept of money (for example through visual exposure to banknotes) affects certain political beliefs that are associated with political right-wing orientation (e.g. more acceptance of a free-market system). Hence, according to these findings, the mere visual presence of money could shift people's political views to the right. However, these findings were challenged by replication studies, which found no such effect and by other research that showed that the effect depended on people's subjective socioeconomic standing in society.

Why was this study done?

Given that money is a nearly omnipresent stimulus in our daily lives we wanted to investigate whether reminding people of money could alter their general political orientation (e.g. whether they see themselves as left- or right-wing) beyond possibly changing certain underlying political beliefs. Such an effect on political orientation could have wide-ranging implications and would render reminders of money a considerable subtle instrument for right-wing party campaigns.

What did the researchers do and find?

Across seven studies we explored whether reminders of money affect people's political orientation. We tested two competing hypotheses. One that tested for a direct effect on political orientation and a second one that tested for such an effect taking account of people's subjective socioeconomic standing in society. Across the studies we used different instruments and participants and combined the results in a synthesized analysis. Our findings did not indicate that reminders of money lead to stronger right-wing orientations. However, the findings leave room for the possibility that for people with a higher subjective standing in society, reminders of money provoke stronger right-wing orientations and for people with lower subjective standing in society they provoke stronger left-wing orientations.

What do these findings mean?

These findings suggest that, contrary to previous research, the money priming effect on political orientation is at best small and dependent on one's subjective socioeconomic status. Thus, there is most likely little menace to the political process in real life. Nonetheless, depending on specific factors of the context, it is possible that money cues produce significant effects in real-life environments as has been indicated with similar environmental cues.

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Money is a central cue in our everyday life. In the modern western world it is barely possible not to get in touch with money or money-related cues. In this regard it seems somewhat concerning that research has shown that subtle reminders of money can have tremendous effects on people's behavior and attitudes across different cultural backgrounds and age groups (for review see: [Vohs, 2015](#)). Regardless of whether participants are Asian, European, or American, when being primed with money, they tend to work more persistently on tasks ([Boucher & Kofos, 2012](#); [Vohs, 2015](#); [Vohs, Mead, & Goode, 2006](#)) and focus more on their own personal needs and goals ([Vohs et al., 2006](#); [Vohs, Mead, & Goode, 2008](#)). However, this emphasis on the personal self comes along with less concern about others. Subjects reminded of money have shown to be less inclined to help others (e.g., [Vohs et al., 2006](#)) and less susceptible to others' needs ([Gino & Mogilner, 2014](#); [Kouchaki, Smith-Crowe, Brief, & Sousa, 2013](#); [Reutner & Wänke, 2013](#)). While it seems that being subtly reminded of money is beneficial for the personal self, it is bad for the social self ([Vohs et al., 2008](#)). Thus, not surprisingly, money priming can also affect attitudes about the societal structure. [Caruso, Vohs, Baxter, and Waytz \(2013\)](#) found that U.S.-participants reminded of money were more likely to justify the existing system, believe in a just world, accept social inequality and dominance, and endorse free-market systems.

It should be noted that the endorsement of free-market systems, the justification of and belief in the existing system, and the acceptance of social inequality and social dominance are deemed underlying core beliefs of political right-wing orientation ([Jost, Blount, Pfeffer, & Hunyady, 2003](#); [Jost, Glaser, Kruglanski, & Sulloway, 2003](#); [Jost & Hunyady, 2005](#)). Also suggesting the notion of a parallel between a political right-wing orientation and the effects of being reminded of money, right-wing politics value personal effort and achievement ([Caprara, Schwartz, Cappanna, Vecchione, & Barbaranelli, 2006](#); [Feldman, 2013](#); [Goren, Federico, & Kittilson, 2009](#)), as do participants who were reminded of money ([Boucher & Kofos, 2012](#); [Vohs, 2015](#); [Vohs et al., 2006, 2008](#); [Zedelius, Veling, & Aarts, 2013](#)). Analogously the diminished concern for others' needs stands in contrast to left-wing politics, which typically defend social egalitarianism, justice and social support for other's needs ([Edlund & Pande, 2002](#); [Jæger, 2008](#); [Roemer, 1998](#)). Hence, given that money primes can change people's underlying political beliefs, would they also affect people's general political orientation (also termed *political ideology*; see [Jost, 2006](#))?

The Present Research

In contrast to a person's political beliefs or attitudes, the general political orientation represents a person's placement on a political dimension (*left-right*; Piurko, Schwartz, & Davidov, 2011) and reflects a broader range of motivational and attitudinal aspects than the previously assessed political core beliefs (Caruso et al., 2013; Klein et al., 2014). In other words, while underlying political beliefs usually reflect content-specific attitudes, the general political orientation is affected by a wide range of different factors such as social identity, personality, values, beliefs, and situational influences (e.g., Brewer, 2001; Crawford, Brady, Pilanski, & Erny, 2013; Jost, 2006; Kandler, Bleidorn, & Riemann, 2012; Keefer, Goode, & Van Berkel, 2015). However, even though the political orientation of a person depends on many different factors, it is not invariant. Previous studies have shown empirical evidence that subtle contextual primes can activate certain norms and concepts and have powerful effects on political judgments (Berger, Meredith, & Wheeler, 2008; Burger & Bless, 2016; Druckman, 2004; Weinberger & Westen, 2008) and on people's general political orientation (Brown-Iannuzzi, Lundberg, Kay, & Payne, 2015; Carter, Ferguson, & Hassin, 2011). For example, Helzer and Pizarro (2011) have shown that the mere presence of a hand-sanitizer dispenser in a hallway is sufficient to provoke changes in participants' general political orientations reported on a field-survey.

Therefore, in the present research we aimed to thoroughly test whether money primes – beyond changing certain underlying right-wing beliefs – would also affect participants' general political orientation. Such an effect on political orientation could have wide-ranging implications and would render money primes a considerable subtle instrument for right-wing party campaigns. Nowadays, governments put an increasing effort in eliminating undue influence on voting (cf. Berger et al., 2008). However, increasing evidence shows that subtle primes can be successfully applied in voting campaigns (Druckman, 2004; Weinberger & Westen, 2008) and can alter people's political judgments in quite a long-lasting way (Carter et al., 2011). But political campaigns are not the only possible source where money cues might substantially alter people's political orientation. Due to the ubiquity of money cues in people's lives that show no signs of waning (Vohs et al., 2008), money could – even if only for a short instance – unobtrusively affect, whether one signs a political petition, reads certain information from a political party, or simply to which political opinion a person pays attention on Facebook.

In sum, we sought to test in an experimental setting whether money primes would lead participants to change their self-reported political orientation and their preferences for political parties, which could – in its most extreme consequence – even alter people's voting behavior in real life. While so far our argument suggested a possible main effect, there is also evidence suggesting a more complex effect. Both hypotheses are outlined in the following.

Main-Effect Hypothesis

Previous research across Western countries suggested that some of the core beliefs of political right-wing orientation such as the acceptance of social inequality, social dominance orientation, the justification of free market systems, and the conviction that only personal effort and achievement should be rewarded, are susceptible to subtle cues of money (Boucher & Kofos, 2012; Caruso et al., 2013; Vohs, 2015; Vohs et al., 2006, 2008; Zedelius et al., 2013). Thus, one may hypothesize that reminders of money subtly shift participants' political orientation to the right, because money primes change the beliefs and values on which it is built on. During the course of this research, this assumption had been challenged. Firstly, the results by Caruso et al. (2013) were disputed by a replication attempt that found no significant effects of money primes on political core beliefs (Rohrer, Pashler, & Harris, 2015).

Moderation Hypothesis

Secondly and pertinent to the current research, [Schuler and Wänke \(2016\)](#) reasoned that if reminders of money stimulate people to focus more on their self and their personal benefits (e.g., [Gino & Pierce, 2009](#); [Reutner & Wänke, 2013](#); [Vohs, 2015](#); [Vohs et al., 2006](#)), they should also favor a system which serves these personal needs as compared to a system which serves the necessities of others. Accordingly, the authors showed that when being primed with money, U.S. participants who felt socioeconomically privileged showed more support of the U.S.-American societal system that favors members from the upper levels of society, compared to participants who felt socioeconomically disadvantaged. In line with this reasoning, one would hypothesize that when being primed with money, only those who believe they would actually benefit from its policies and the general ideology that comes along with it should endorse right-wing politics. In other words, when considering oneself at the top of the social hierarchy (i.e. higher subjective socioeconomic status [subjective SES]), right-wing policies, which – by tendency – aim to preserve the social order and criticize social egalitarianism, redistribution, and equal chances ([Giddens, 1998](#); [Jost, Glaser, et al., 2003](#); [Roemer, 1998](#)), are more in line with one's personal interests (see [Brown-Iannuzzi et al., 2015](#)). For people who see themselves more at the bottom of society (lower subjective SES) this benefit is less clear. On the one hand, they should favor left-wing politics when inclined to pursue their personal interests through money primes. Left-wing policies seek to minimize social hierarchies, support the interests of the disadvantaged, and fight for inclusion, redistribution, and equal rights ([Edlund & Pande, 2002](#); [Jæger, 2008](#); [Shen & LaBouff, 2016](#)) and are therefore supported by people with a lower subjective SES ([Brown-Iannuzzi et al., 2015](#)). On the other hand, people with a lower subjective SES do not necessarily believe that left-wing politics would be favorable for them (e.g., [Jost, Pelham, Sheldon, & Ni Sullivan, 2003](#)). Still, overall priming effects should be more pronounced for those subjectively higher in the social hierarchy and therefore moderated by subjective SES.

Methodological Approach

To perform a rigorous test of the hypothesis that money primes affect political orientation, we adopted a multistudy approach using different dependent variables, and slightly varied the settings throughout the studies (i.e. we performed conceptual replications). Such an approach is more informative than the test of a single paradigm ([Tuk, Zhang, & Sweldens, 2015](#)) and allows examining whether a possible underlying effect is restricted to a certain measure or generalizes across several measures. Because the moderation hypothesis seemed a plausible alternative to the main-effect hypothesis, we included subjective SES as a possible moderator for the second half of our studies and conducted further studies online to increase the samples' variance on subjective SES.

To overcome power problems in single studies ([Cumming, 2014](#); [Tuk et al., 2015](#)), we conducted a small-scale meta-analysis ([Braver, Thoemmes, & Rosenthal, 2014](#); [Goh, Hall, & Rosenthal, 2016](#)) across our studies. The analysis is based on more data, has smaller confidence intervals for the estimated effect size and is thus more reliable in detecting whether an effect exists or not ([Cumming, 2012](#)). Because we performed conceptual replications, it is possible that the true underlying effect sizes differ. Thus, we used a random-effects model in our meta-analysis as the generally more appropriate method of analysis ([Borenstein, Hedges, Higgins, & Rothstein, 2010](#); [Cumming, 2012](#)).

Moreover, by applying a random-effects model, the internal meta-analysis can indicate whether the obtained effect sizes represent the same or different underlying effects and can – depending and limited by the number of included studies – help to detect possible moderators across studies ([Borenstein, Hedges, Higgins, & Rothstein, 2009](#)).

Because tests for heterogeneity and moderators tend to be underpowered with small amounts of studies ($k < 20$; Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006), we set the alpha level for both tests to .10 (Tuk et al., 2015). Furthermore and as an additional criterion for heterogeneity that is independent of the included number of studies, we applied $I^2 > 50\%$ as a criterion for heterogeneity (Braver et al., 2014). The test statistic I^2 refers to the percentage of variance that can be attributed to systematic study heterogeneity as opposed to random variation of the true effect size between studies (Higgins, Thompson, Deeks, & Altman, 2003).

The present article includes data from seven studies. We first provide a general description of our samples and dropout criteria and then proceed with descriptions of the manipulation, the dependent variables and subjective SES as a possible moderator. After providing an overview of our studies, we test our two hypotheses and conduct some exploratory moderation analyses.

Method

The studies were either conducted in the laboratory or online. All studies had in common that participants worked on two ostensibly unrelated experiments. Across all studies the first experiment was a money priming manipulation that was framed as a task on playful language learning methods and comprised a descrambling sentence task (see Hansen, Kutzner, & Wänke, 2013). The second experiment was framed as a study on decision-making and contained one of the dependent variables. Materials in all studies, including pretests, were in German. Minor variations between studies, such as whether demographic information was assessed at the beginning or at the end of the study, are described in Appendix A.

Participants and Dropouts

In all studies we exclusively recruited German participants to retain the same manipulation and to be able to relate studies where we used preferences for German political parties as the dependent measure with studies where we used self-reports of political orientation. Because the suggested psychological mechanisms of money priming seem to be universal and have been found across many different countries (see Vohs, 2015), we did not expect particular cultural differences.

For a total of seven studies ($N = 666$; Table 1) we applied one common dropout criterion to make sure only those participants who were adequately primed with money were included in the studies. Hence, we excluded participants who participated twice (33), participants who did not follow the instructions to create four-word phrases in the priming task in at least one third of the cases (18), non-German native speakers (due to the language-based prime; 27) and participants who interrupted the study (i.e., upper quartile of study duration + 1.5 interquartile range; 16)¹ The remaining sample consisted of 572 participants (237 males, 2 other; $M_{\text{age}} = 28.5$, $SD = 11.5$).

Independent and Moderator Variables

Descrambling Task

Participants were provided with 22 word-sets, each consisting of five words, and had to descramble each set into a correct sentence of four words. In the experimental condition, 15 out of 22 word-sets contained words associated with money (e.g. money, salary, coin, cashbox). In the control group these words were substituted with money-unrelated words (for a more detailed description see Appendix B).

Subjective Socioeconomic Status (subjective SES)

In order to assess subjective SES, participants completed a digital version of the MacArthur Scale (Piff, Kraus, Côté, Cheng, & Keltner, 2010). An image of a 10-rung ladder was depicted as a representation of where people stand in society. While the top rung was labeled as representing those with the highest standing in the social hierarchy, the lowest rung was labeled as representing those with the lowest standing in the social hierarchy. To measure subjective SES, participants were instructed to indicate on a 10-point scale where they would place themselves on this ladder relative to others in society ($M = 5.94$, $SD = 1.75$; only Studies 4-7).

Dependent Variables

Self-Report (A)

As a measure for political orientation that we used in our first study, we applied a three-item measure that had been shown to be sensitive to environmental influences (Helzer & Pizarro, 2011). Participants were asked to which extent they identify themselves as fiscally, morally and socially conservative (1 = *very conservative*) or liberal (7 = *very liberal*). The inter-item correlation of the fiscal item was low ($r_{\text{moral}} = .05$; $r_{\text{social}} = -.02$), which was possibly due to the unclear meaning of the term fiscal conservatism among German participants.ⁱⁱ Hence, the reliability of the three-item scale was low, with a Cronbach's alpha of .48 (Cronbach's $\alpha = .65$ in Helzer & Pizarro, 2011). The removal of the fiscal item raised the internal consistency to a Cronbach's alpha of .76. In consequence, we conducted the analysis with the two-item version of the scale and used different items for self-reported political orientation for subsequent studies.

Self-Report (B)

For the succeeding studies we used an item from a German Political Panel Study (Breyer, 2015) that reflects participants' self-placement on an inter-culturally robust left-right dimension (Puurko et al., 2011). To assess general political orientation ("Where would you place yourself if 1 was *left* and 11 was *right*?"), participants reported their political orientation on an eleven-point scale.ⁱⁱⁱ

Political Party Preferences

As a complement to the self-report measures, we asked participants to indicate their preference for the six most popular political parties in Germany at the time (Die Linke, Die Grünen, SPD, AfD, CDU/CSU, FDP). To compute our dependent variable, each participant evaluated the six parties in a randomized order (1, "very negative" – 11, "very positive"). We also obtained ratings for the perceived general political orientation of each party (1, "very left-wing" – 4, "very right-wing") in an independent pretest (see Appendix C for details). To compute an index for political orientation, we then calculated (Fisher's z-transformed) correlations between the parties' political orientation scores (assessed in the pretest) and participants' evaluations of each party (assessed in each study). Scores above 0 indicate a more favorable rating for parties considered on the political right compared to parties considered left; scores below 0 indicate the opposite. Our measure for party preferences consistently showed strong correlations with participants' self-reported political orientation (assessed before the manipulation) in the control groups of each experiment ($r_{\text{range}} = .54 - .65$; see Table 1 for details). We thus used the obtained indices, which are typically strongly related to left-right self-placement measures (Van Deth & Geurts, 1989), as a reliable choice measure for political orientation.

Table 1

Studies Overview With Different Dependent Variables

| Study | Total <i>N</i> | Included <i>N</i> | Study Type | Dependent Variable | Age | | Gender | | Subjective SES | | Party preference | |
|---------|----------------|-------------------|------------|--------------------|----------|-----------|----------|--------------------|----------------|-----------|------------------|----------|
| | | | | | <i>M</i> | <i>SD</i> | Male (%) | Female (%) | <i>M</i> | <i>SD</i> | <i>r</i> | <i>n</i> |
| Study 1 | 91 | 76 | Lab | Self-report (A) | 22.95 | 4.18 | 27.60 | 72.40 | - | - | - | - |
| Study 2 | 120 | 104 | Lab | Party preferences | 21.14 | 2.56 | 28.00 | 72.00 | - | - | .65*** | 48 |
| Study 3 | 86 | 63 | Lab | Party preferences | 21.49 | 4.00 | 23.80 | 76.20 | - | - | .63*** | 30 |
| Study 4 | 73 | 70 | Lab | Self-report (B) | 22.77 | 4.42 | 24.30 | 75.70 | 6.37 | 1.40 | - | - |
| Study 5 | 117 | 101 | Online | Self-report (B) | 40.07 | 13.15 | 74.30 | 25.70 | 5.43 | 1.87 | - | - |
| Study 6 | 77 | 65 | Online | Self-report (B) | 26.71 | 7.25 | 28.10 | 70.30 ^a | 6.48 | 1.45 | - | - |
| Study 7 | 102 | 93 | Online | Party preferences | 38.91 | 11.85 | 68.80 | 30.10 ^a | 5.56 | 1.76 | .54*** | 50 |

Note. Total *N* = number of participants; Included *N* = participants included in the analysis; *Self-report* = self-report measures (A) and (B); *Party preferences* = preference measure for political parties; Subjective SES = subjective Socioeconomic Status (1-10); Party preferences = Pearson correlation in control groups between party preference measure and self-reported political orientation (assessed before manipulation). All descriptive statistics are based on the reduced number of included participants. Dashes indicate that no data was obtained for the respective cell.

^aMissing percentage chose "other".

p* < .05. *p* < .01. ****p* < .001.

Results

We meta-analyzed our studies by converting main effects and interaction effects into Pearson's correlation for comparability and ease of interpretation (see Table 2 for complete overview).

Table 2

Studies Overview on Obtained Effect Sizes

| Study | Included (<i>N</i>) | CG (<i>n</i>) | EG (<i>n</i>) | DV CG | | DV EG | | Main effect <i>r</i> | Interaction effect <i>r</i> |
|---------|-----------------------|-----------------|-----------------|----------|-----------|----------|-----------|----------------------|-----------------------------|
| | | | | <i>M</i> | <i>SD</i> | <i>M</i> | <i>SD</i> | | |
| Study 1 | 76 | 39 | 37 | 4.44 | 1.33 | 4.61 | 1.27 | -.07 | - |
| Study 2 | 104 | 51 | 53 | -.31 | 0.76 | -.31 | 0.65 | .00 | - |
| Study 3 | 63 | 31 | 32 | -.28 | 0.51 | -.20 | 0.55 | .08 | - |
| Study 4 | 70 | 37 | 33 | 5.30 | 1.79 | 5.27 | 1.79 | -.01 | .05 |
| Study 5 | 101 | 53 | 48 | 5.75 | 1.84 | 4.96 | 2.22 | -.19 | .07 |
| Study 6 | 65 | 33 | 32 | 4.53 | 1.80 | 4.56 | 1.63 | .01 | .38** |
| Study 7 | 93 | 50 | 43 | -.17 | 0.62 | -.52 | 0.78 | -.25* | .07 |

Note. Included *N* = participants included in the analysis; CG = control group; EG = experimental group; DV = value of dependent variable in respective study. Higher (positive) values indicate higher right-wing preference; Main effect & Interaction effect = Pearson correlation effect sizes for main effect and interaction prime x subjective socioeconomic status. Dashes indicate that no data was obtained for the respective cell.

p* < .05. *p* < .01. ****p* < .001.

All correlations were then transformed into Fisher's z scores for analysis. For our internal meta-analyses we applied a random-effects model (fitted with a restricted maximum-likelihood estimator for heterogeneity; see Viechtbauer, 2005), using the package METAFOR in R (Viechtbauer, 2010). The meta-analytic estimates and respective confidence intervals were then converted back to Pearson's correlation coefficient for presentation throughout the text (cf. Goh et al., 2016).

The Main-Effect Hypothesis

Overall, the small-scale meta-analysis on participants' right-wing orientation revealed a non-significant main effect. On a descriptive level it points in the opposite direction. As shown in Figure 1, money-primed participants did not report significantly higher right-wing orientations than participants not reminded of money, $N = 564$, $r = -.07$, 95% CI = [-0.17, 0.02], $Z = -1.57$, $p = .117$, $Q(6) = 7.00$, $p = .32$, $I^2 = 17.83\%$.^{iv} Cochran's Q and I^2 are both test statistics used to evaluate the consistency of a meta-analytic effect (Higgins et al., 2003). A non-significant Q and low I^2 indicate that there is low heterogeneity in the included studies and they all reflect the same effect.

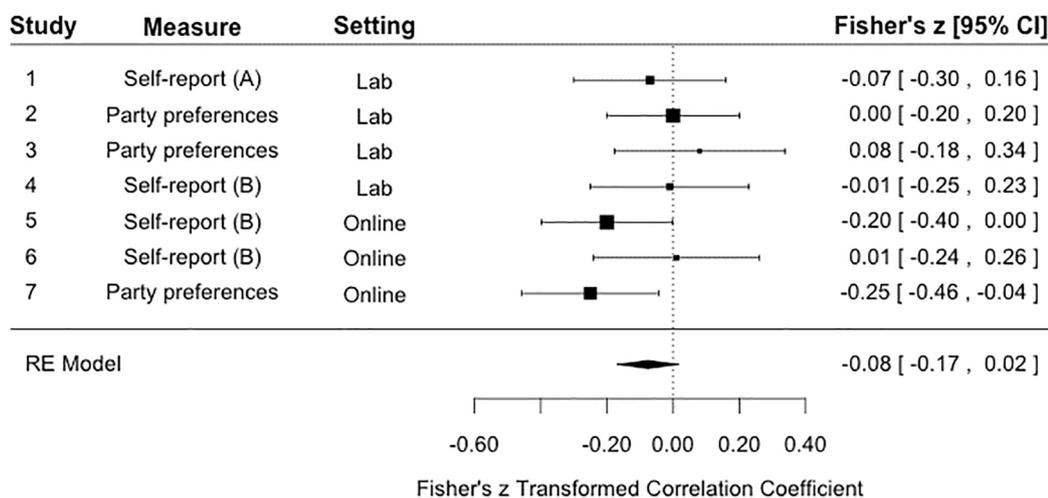


Figure 1. Forest plot of the effect of money priming on political orientation. Positive effects reflect stronger right-wing orientation after being reminded of money. For each included study, the measure type, the setting, Fisher's z value, and the corresponding 95% confidence interval (black lines) are reported. The effect sizes are illustrated with squares whose sizes are representing the relative weight of each study in the random-effects meta-analysis. The diamond depicts the effect size of the meta-analytic estimate and its 95% confidence interval.

The cumulative evidence of our seven studies did not support the hypothesis that being reminded of money leads to stronger right-wing preferences. Considering the small confidence intervals of the effect size estimate (for a detailed interpretation of confidence intervals see Cumming, 2012), one can conclude that the effect is most likely either essentially zero or, if anything, small and in the opposite direction as predicted.

The Interaction-Effect Hypothesis

To test the hypothesis that subjective SES moderates the effect of money priming on political orientation, we once again performed a small-scale meta-analysis. Effect sizes for the interaction effect of money priming x subjective socioeconomic status (SES) on political orientation are obtained for each individual study, while controlling for the main effect of money priming on the dependent variable. The meta-analysis (Figure 2) now revealed a

marginally significant interaction effect in the expected direction, $N = 327$, $r = 0.14$, 95% CI = [-0.01, 0.28], $Z = 1.83$, $p = .068$, $Q(3) = 5.42$, $p = .14$, $I^2 = 42.97\%$.^v Heterogeneity analysis indicated moderate but not disproportionate levels of heterogeneity.

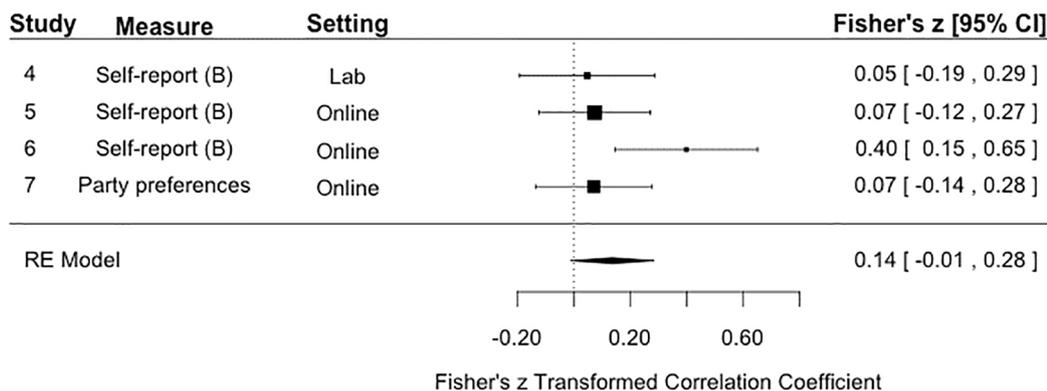


Figure 2. Forest plot of the interaction effect of money priming x subjective socioeconomic status (SES) on political orientation. Positive effects reflect stronger (weaker) right-wing orientation for participants with higher (lower) subjective SES after being reminded of money. For each included study, the measure type, the setting, Fisher's z value, and the corresponding 95% confidence interval (black lines) are reported. The effect sizes are illustrated with squares whose sizes are representing the relative weight of each study in the random-effects meta-analysis. The diamond depicts the effect size of the meta-analytic estimate and its 95% confidence interval.

Evidence across the four studies revealed a tendency that when being reminded of money participants' right-wing orientation depended on their subjective SES: Participants with higher subjective SES indicated stronger right-wing orientations compared to participants with lower subjective SES (Figure 3).

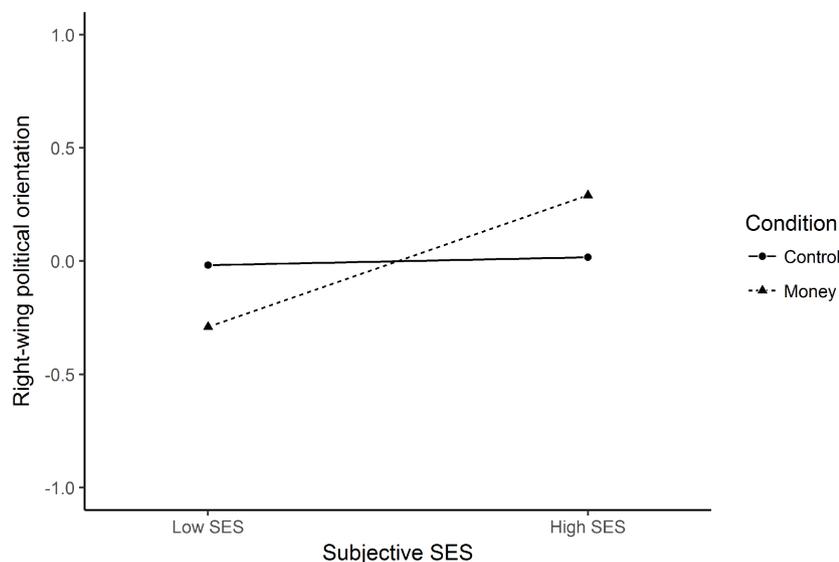


Figure 3. Synthesized simple slopes of control ($\beta = 0.017$) and money condition ($\beta = 0.291$) for Studies 4 – 7. To allow for accumulation, beta regression coefficients were derived from z-standardized variables and synthesized with a weighted least squares approach (Becker & Wu, 2007). Values of right-wing political orientation above zero indicate stronger right-wing orientation and values below zero indicate stronger left-wing orientation for lower (1 SD below the mean) and higher (1 SD above the mean) subjective socioeconomic status (SES).

The cumulative analysis on the direct measures seems to provide some tentative support for the interaction hypothesis. In contrast to the main effect hypothesis and in line with previous research (Schuler & Wänke, 2016), the cumulative evidence revealed a marginally significant effect for the moderation hypothesis. However, the effect did not reach conventional levels of significance ($p < .05$) and should be interpreted with caution. In conjunction with previous research, the moderation effect is most likely of small to medium size and further studies would be needed to provide solid evidence for (or against) the effect.

Moderation Analyses

Interestingly, the interaction effect money \times subjective SES on the direct measures revealed moderate heterogeneity. Such heterogeneity is generally not surprising, because variability in priming effects across different samples and contexts should actually be expected (Cesario, 2014). Past priming research has revealed a great variety of moderating factors (e.g., DeMarree, Wheeler, & Petty, 2005; Ma & Correll, 2011; Yi, 1993). Thus, many critical factors within the sample and the experimental design that might seem irrelevant can, if varied, eliminate or even reverse priming effects. Yet, even though such moderators clearly decrease the generalizability of the priming effect, they are of significant value for researchers. Priming research is still in its beginning with regard to the exploration of its processes and its process-related variables. Thus, the discovery of moderating variables can provide meaningful information about the effect and the theory (Cesario, 2014).

To test for the variation in the interaction effect money \times subjective SES between the studies, we applied a mixed-effects model (Borenstein et al., 2009) and explored two potentially relevant characteristics that could have caused some of the variance in our priming effects. First, different measures might have varied in their sensitivity to assess political orientation and thus possibly caused different effect sizes. However, whether we used self-reports (A & B) or the choice measure (party preferences) to assess political orientation did not significantly moderate the overall effect, $QM(df = 1) = 0.10, p = .755$.^{vi} Second, our studies in the laboratory differed in many aspects from our online studies. Factors such as the elevated subjective SES among our laboratory samples (6.68 versus online: 5.82), being surrounded by in-group members in the laboratory (Fessler & Holbrook, 2013) or a difference in the intensity of the prime in the laboratory compared to the online studies could have potentially moderated the results. However, the study location did not show statistically significant differences in the effect sizes across studies, $QM(df = 1) = 0.47, p = .493$.

Even though our analyses did not reveal significant moderators, we cannot rule out the possibility that study location or the application of different measures moderate the effect. With only a small number of studies, the power to detect relevant moderators decreases and therefore the moderation analyses should be interpreted with caution (Borenstein et al., 2009).

Discussion

We considered the hypothesis that money primes change people's political orientation, which was suggested by several publications (e.g., Caruso et al., 2013; Vohs, 2015; Zedelius et al., 2013) and subsequently questioned by other researchers (Klein et al., 2014; Rohrer et al., 2015; Schuler & Wänke, 2016; Vadillo, Hardwicke, & Shanks, 2016). We adopted a multistudy approach and across seven studies, we found little evidence for a main effect of money priming on political orientation among German participants. Furthermore, for the alternative hypothesis

that subjective SES moderates the priming effect on political orientation, we found tentative, marginally significant evidence for a small to medium size effect.

From a basic research perspective, it was interesting to note that the predictions and empirical findings of money priming allow for two competing hypotheses. On the one hand, money primes evoke a focus on personal achievement and make people less sensitive to the needs of others (Vohs et al., 2008). Thus, because money priming triggers central convictions of right-wing politics, money primes should also make people endorse right-wing politics in general. On the other hand, reminders of money lead to a focus on personal concerns and one's personal advantages (see Gino & Pierce, 2009; Kouchaki et al., 2013; Reutner & Wänke, 2013; Vohs, 2015; Vohs et al., 2006). Thus, money priming should make people endorse a political orientation that increases their personal benefits. More specifically, when primed with money, people should favor right-wing politics when feeling socioeconomically privileged and less so when feeling socioeconomically disadvantaged. These two opposing predictions did not only emphasize the relevance to investigate this research question but suggest as well that a more specified theory of money priming might be warranted.

From an application perspective, the question whether subtle money primes could alter people's political orientation is of fundamental relevance. In western societies, money cues become rather abundant and could thus – as a repetitive prime – potentially provoke lasting changes in people's political orientation and ultimately voting behavior (cf. Carter et al., 2011). As a prerequisite, money primes would need to work in the field – not only in experimental settings – and would need to have long lasting effects. Recent research demonstrates the efficacy of money priming in noisy real-life contexts such as coffee bars (Mogilner, 2010), on the street (Guéguen & Jacob, 2013), or in work environments (Mok & De Cremer, 2016) on daily behaviors. More importantly, Beus and Whitman (2017) suggest that money can have durable effects in work contexts, when made chronically salient. An effect of money cues, whether in close proximity to the voting booth or as a repetitive visual cue in political campaigns and daily contexts, would be rather troubling and calls for careful examination.

In evaluating the implications of our results, it is useful to consider possible shortcomings of this research. First, while we varied the dependent variable between studies, the money priming manipulation remained identical for all studies. Since the descrambling task has been used in previous research to successfully induce money priming (e.g. Hansen et al., 2013) we do not think that this necessarily weakens our results. Still, this point is worth mentioning as a direction for future research. Second, Caruso and colleagues (2013) found money priming effects on certain political beliefs only among U.S.-American participants, “because the key components of the experimental design were focused on America” (p. 304). Although our measures were not restricted to U.S. participants, it is possible that money-priming effects on political orientation would reveal different results with U.S. participants due to cultural differences. However, we find this to be unlikely. Empirical support for the theoretical claim that money priming makes people more focused on themselves and their personal benefits has been shown across different countries and cultural backgrounds (Vohs, 2015). Furthermore, Schuler and Wänke (2016) tested U.S. participants and found an interaction that concurs with our findings. Thus, although it remains an empirical question, we are not aware of a moderator in the field that would make different predictions for U.S. and German participants regarding the suggested effect. Third, it is unclear how and whether these effects would generalize in noisy real-life environments, in which a myriad of different factors drives people's actions (Mogilner, 2010). Even though previous research suggests that the effects of money priming can have significant effects in the field (e.g., Guéguen & Jacob, 2013; Mok & De Cremer, 2016), it remains an open question to which degree money primes could influence actual political orientation or voting behavior during an election.

Conclusions

Although more research is needed to conclude whether an interaction effect exists or not, our data provide some interesting conclusions. On the one hand, we found tentative evidence for the theoretically predicted interaction effect with subjective SES. After a decade of promising money priming research (Vohs, 2015) more and more researchers raise questions about the robustness, replicability, and credibility of the discovered effects and their theoretical framework (Klein et al., 2014; Pashler, Rohrer, Abramson, Wolfson, & Harris, 2016; Rohrer et al., 2015; Vaddillo et al., 2016). Perhaps it is time to thoroughly search for moderators and limitations of money priming, in order to further advance the field and theory. Interestingly, it appears that a person's standing in the social hierarchy, which is increasingly considered in social psychology in general (Brown-Iannuzzi et al., 2015; Gruenewald, Kemeny, & Aziz, 2006; Keefer et al., 2015; Kraus, Côté, & Keltner, 2010), also plays a role for money priming (see also Schuler & Wänke, 2016) and might partly explain why money priming effects are generally not so robust. Hence, it seems worthwhile to keep this meaningful variable in mind when conducting future research with more representative or diversified samples in social and political psychology.

On the other hand, our goal was to examine whether the effects of money priming would have practical relevance for society. At first glance, our findings suggest that there is most likely little menace to the political processes in real life. Under relatively controlled settings, we found some tentative evidence for a small to medium money prime interaction effect, which might not hold in more unstable settings. It may well be that such priming effects are smaller and much more limited than oftentimes assumed (Helzer & Pizarro, 2011; Weinberger & Westen, 2008). Nonetheless, depending on the context of the prime and further moderators, it is of course possible that the prime produces significant effects in real-life environments as has been indicated with similar environmental cues (Carter et al., 2011).

Accordingly, this research opened up some interesting lines for future research. A closer investigation of certain facets of political orientation such as its economic and social dimension (Ashton et al., 2005; Everett, 2013) could reveal that cues of money affect these concepts differently. Former research (Caruso et al., 2013) indicated that beliefs (e.g., social dominance orientation) associated with the economic dimension of political orientation (Jost, Federico, & Napier, 2009) were influenced by money priming. These findings fit with the general assumption that money primes would rather affect a dimension labeled compassion vs. competition (economic) than a dimension labeled moral regulation vs. individual freedom (social; cf. Ashton et al., 2005). Following a compatibility logic (Ajzen & Fishbein, 1977), this would imply that our dependent variables measured a broader concept (political orientation) and that measuring a sub-dimension would lead to stronger effects. Furthermore, considering the potential implications that unobtrusive money priming effects would have for elections, field experiments would be a useful complement to our research to examine whether an effect on political decisions and ideologies holds in this setting. While we do not claim that a single priming episode will change people's attitudes for good it may suffice at a given moment to sign a petition, or stop to take a leaflet or listen to a candidate's arguments. While these individual actions might not seem too influential on their own, taken together and over time they could tilt opinions in favor of certain political judgments.

Notes

- i) Because we were performing a meta-analysis with a comparably large and diverse sample size, we were more concerned about making sure that all included participants were correctly primed rather than further increase statistical power. We thus applied a more liberal dropout criterion.
- ii) With regard to fiscal policies the terms *conservative* and *liberal* are not well defined in Germany. For example, the liberal party in Germany (FDP) is advocating fiscally conservative policies despite their claim to be liberal. Thus, the item might have produced inconsistent responses.
- iii) Additionally, we assessed two content related political attitudes on immigration and taxation that turned out to be unsuitable to assess general political orientation. Due to prominent political events at that time (Euro crisis & refugee crisis), the items indicated specific attitudes and revealed a questionable internal consistency when included to the general political orientation item (Cronbach's α range = .59 - .63; George & Mallery, 2003).
- iv) When the dropout-criterion was not applied and the analysis was performed across all participants without excluding any cases, money-primed participants did not report significantly higher right-wing orientations than participants not reminded of money, $N = 653$, $r = -0.03$, 95% CI = [-0.13, 0.06], $Z = -0.74$, $p = .46$, $Q(6) = 8.28$, $p = .219$, $I^2 = 28.45\%$.
- v) When the dropout-criterion was not applied and hence the analysis was performed across all participants without excluding any cases, the interaction of money and subjective SES did not significantly predict participants' right-wing orientation, $N = 365$, $r = 0.16$, 95% CI = [-0.03, 0.34], $Z = 1.64$, $p = .102$, $Q(3) = 9.65$, $p = .022$, $I^2 = 70.13\%$.
- vi) QM is a Q-test for moderators in meta-analyses. As with Cochran's Q a non-significant QM indicates that the variable in question does not moderate the overall effect (Viechtbauer, 2010).

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Competing Interests

The authors have declared that no competing interests exist.

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Appendices

Appendix A

Detailed description of study procedures.

Study 1. After initial instructions that told participants they would work on two different experiments, they completed the descrambling task, which either contained money related words or not. After completion of our ostensible experiment 1, we asked participants about the purpose of the study. In experiment 2 they received further instructions (signed by a different researcher) and completed the self-report A as a dependent measure. Afterwards participants worked on an exploratory perception task, which was not relevant for our hypothesis. In the task, we presented ten faces of unknown male politicians and asked participants to indicate their voting preference for each of them. Thereby we wanted to investigate whether money primes would also affect the preference for warm or competent faces. After the perception task, we assessed demographic information and asked participants about the purpose of our ostensible experiment 2.

Study 2. First, participants provided demographic information and indicated their general political orientation on self-report B. Afterwards they completed the descrambling task. In the ostensible second experiment, participants were first asked to complete the perception task of Study 1 and then asked to complete the party preference measure as our dependent measure. Lastly, we asked participants about the purpose of the experiment.

Study 3. This study was a replication of Study 2 with the only exception that we did not assess the visual perception task between the manipulation and the dependent measure. As in Study 2 general political orientation (self-report B) was assessed before the manipulation.

Study 4. This Study was a replication of Study 1, with an identical procedure but a different dependent variable (self-report B instead of self-report A). Before providing demographic information, participants indicated their mood on a brief 3-item scale (the difference between the control group and the experimental group was not significant, $p = .997$) to control for mood effects. Additionally, subjective SES was assessed as part of the demographic information at the end of the study.

Study 5. This Study was a replication of Study 4 in which we recruited participants through the online platform CrowdFlower. In contrast to Study 4, mood was no longer assessed and subjective SES was assessed before the other demographic variables.

Study 6. This Study was again a replication of Study 4 in which we recruited participants online from various online study pools. In contrast to Study 4, mood was no longer assessed.

Study 7. This Study was a replication of Study 2 in which we recruited participants through the online platform CrowdFlower. In this study we did not assess the visual perception task between the manipulation and the dependent measure. As in Study 2 general political orientation (self-report B) was assessed before the manipulation. Additionally, we assessed subjective SES at the end of the study.

Appendix B

Detailed description of the independent variable (descrambling task).

The descrambling task was introduced as part of a larger international study about playful language learning methods for which our department ostensibly collected data in German-speaking countries. Accordingly, the descrambling task was conducted in German.

Participants were asked to work through 22 word-sets, each consisting of five words. Their task was to leave out one of the words and build a grammatically correct sentence with the remaining four words. An example was provided ("Example: saw, train, the, vehicle, they; Possible sentence: They saw the train").

In the experimental condition 15 out of 22 word-sets contained words related to money (e.g. “are, salaries, Switzerland, English, high”). In the control condition all money-related words were replaced with money-unrelated words (e.g. “are, towers, Switzerland, English, high”).

Every participant worked on the same word-sets, but they were arranged in a random order.

Appendix C

Pretest for dependent variable: Political party preferences.

Description of the sample. A total of 96 participants from Germany (34 males; $M_{\text{age}} = 25.61$, $SD = 6.41$) completed the questionnaire after they were recruited online through various message-boards on facebook.com. None were excluded before analysis. General political orientation was assessed with the same item used as the dependent variable *Self-reported political orientation (B)* (“Where would you place yourself if 1 was *left* and 11 was *right*?”). The sample was left-leaning ($M = 4.68$, $SD = 1.64$, Range 1-8).

Table C.1

Political Parties and Their Left-/Right-Wing Orientation, as Rated by Participants

| <i>Political Party</i> | <i>N</i> | <i>M</i> | <i>SD</i> | <i>range</i> |
|------------------------|----------|----------|-----------|--------------|
| Die Linke | 95 | 1.09 | 0.36 | 1-3 |
| Die Grünen | 96 | 2.00 | 0.46 | 1-3 |
| SPD | 96 | 2.11 | 0.43 | 1-4 |
| CDU | 96 | 2.94 | 0.48 | 1-4 |
| FDP | 94 | 2.81 | 0.61 | 1-4 |
| AFD | 96 | 3.70 | 0.58 | 1-4 |

Note. All scales from 1 (left) – 4 (right). *N* = sample size, *M* (*SD*) = mean and standard deviation of respective ratings. *N* varies between parties, because some participants failed to provide complete ratings.