

RESEARCH ARTICLE

Partisan Homogeneity Does Not Increase Collaborative Corruption

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Abstract

This study examines the behavioral consequences of partisan group composition on cooperation in a setting where cooperation is mutually beneficial but unethical. Collaborative corruption highlights that corruption is not a solitary act but necessitates cooperation. Based on the premise that partisanship serves as a social identity, leading ordinary citizens to reward co-partisans and penalize out-partisans, we expect that collaborative corruption is higher in partisan-wise homogeneous groups. To test this expectation, we conducted a preregistered, large-scale experiment among U.S. voters playing an online version of the collaborative cheating game by Weisel and Shalvi. We find no evidence that partisan homogeneity affects collaborative cheating. These results suggest a critical scope condition: while partisan homogeneity improves cooperation in social dilemmas, it does not extend to contexts of unethical collaboration. They also refute common concerns that partisan homogeneity may facilitate cooperative corruption.

Keywords: Partisanship; affective polarization; dishonesty; behavioral ethics; lab-in-the-field experiment

Understanding the behavioral consequences of polarization is critical to the survival of liberal democracy. Political polarization, characterized by animosity between partisan groups over policies, and affective polarization, rooted in emotional attachments rather than policy differences (Iyengar et al. 2019, 129), have both been traced back to partisanship as a social identity. Affective polarization, in particular, contributes to prejudice and discrimination against political out-groups, decreased willingness to engage in constructive political dialog, and erosion of trust in democratic institutions (Hobolt, Lawall, and Tilley 2024; Druckman, Green, and Iyengar 2023; Broockman, Kalla, and Westwood 2023).

   This article has earned badges for transparent research practices: Open data, Open materials, and Preregistered. For details see the [Data Availability Statement](#).

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Affective polarization also affects citizens' behavior in other nonpolitical settings. Spillovers of partisan animosities have been documented, for example, in the evaluation of job applicants, in dating behavior, and in online labor markets (e.g., Iyengar *et al.* 2019; Easton and Holbein 2021). These findings are supported by a range of studies using behavioral experiments. Iyengar and Westwood (2015), Carlin and Love (2013), and Whitt *et al.* (2021) employed economic games, such as the dictator and trust game, to study the effect of partisan cues on cooperation. These games show that ordinary citizens reward co-partisans and penalize out-partisans. Yet it remains unclear whether this pattern, documented in above-board strategic tasks, extends to ethically questionable behaviors such as corruption, particularly in an era of heightened polarization.

Corruption's detrimental economic and political effects are well known (De Vries and Solaz 2017; Klanja, Lupu, and Tucker 2021). Collaborative corruption emphasizes that corruption is not a solitary act but requires cooperation. Examples of collaborative corruption include politicians and business leaders working together to secure government contracts at taxpayers' expense. This often neglected aspect of corruption is captured in the collaborative cheating game (Weisel and Shalvi 2015), in which two individuals must cheat together to gain an advantage. The collaborative cheating game has been used in multiple studies (Leib *et al.* 2021), showing, for example, that collaborative cheating is contagious (Gross *et al.* 2018; Kocher, Schudy, and Spantig 2018), that it increases with communication (Tonnesen *et al.* 2024) and with feelings of similarity among group members (Irlenbusch *et al.* 2020). Despite widespread recognition of partisan-wise group composition affecting cooperation, its effect on collaborative corruption has not been examined. However, to effectively combat corruption, it is imperative to understand how partisanship affects the internal dynamics of corrupt behavior.

To address this research challenge, we conduct a large-scale interactive online experiment among U.S. voters to identify the effect of partisan group composition on behavior in a collaborative cheating game (Weisel and Shalvi 2015). Unlike other experimental tasks commonly used to study the behavioral effects of partisan group composition (Iyengar and Westwood 2015; Carlin and Love 2013; Whitt *et al.* 2021), the collaborative cheating game provides unique incentives for cooperation. In social dilemma, games such as the prisoner's dilemma or the trust game, self-interested agents often fail to achieve cooperative outcomes because the individual and collective optima diverge. In contrast, in the collaborative cheating game, self-interested agents are expected to cooperate because their preferences are aligned. Thus, under the assumption of rational self-interest, collaborative cheating should be common, regardless of partisan group composition. Previous experimental evidence on collaborative cheating, however, does not support the narrow self-interest prediction. Instead, brazen cheaters are rare, and subjects do not lie to the maximum extent possible (Leib *et al.* 2021). This is because cheating takes an internal toll, increasing moral tension, and undermining the individual's positive self-image (Weisel and Shalvi 2022). To achieve a mutually beneficial cooperative outcome, both players must violate a fundamental ethical principle: honesty.

This study argues that a shared social identity based on partisanship can help individuals bear the moral costs of unethical behavior while maintaining a positive self-image (Tajfel and Turner 1979). When partisanship is strong enough to create a

salient group identity, it may be easier for individuals to preserve a positive social identity if those aware of their transgression are from their own party, particularly when the behavior is mutually beneficial. In this sense, co-partisans are “helping each other win,” even through cheating. By contrast, the threshold for signaling a willingness to cheat to out-partisans is presumed to be higher, especially if partisans believe themselves morally superior to their political opponents (e.g., Puryear et al. 2024). From this reasoning, we expect more collaborative cheating in partisan-homogeneous groups than in partisan-heterogeneous groups. Specifically, we hypothesize: **H1**. Collaborative cheating is higher in partisan-wise homogeneous groups compared to partisan-wise heterogeneous groups.

Affective polarization is presumed to reinforce the perception of profound partisan differences, even beyond policy disagreements (Hobolt, Lawall, and Tilley 2024; Druckman, Green, and Iyengar 2023; Broockman, Kalla, and Westwood 2023), thereby reducing the willingness to cooperate with out-partisans. Conversely, lowering misperceptions about out-partisans (Ahler and Sood 2018) may weaken partisanship as a social identity and foster cooperation in partisan-wise heterogeneous groups. Such a reduction in perceived intergroup distance should lower the inhibition threshold for engaging in mutually beneficial, albeit unethical, behavior. Thus, our auxiliary hypothesis reads: **H2**. Decreasing affective polarization in partisan-wise heterogeneous groups increases collaborative cheating.

Research design and sampling

Online collaborative cheating game

To test these hypotheses, we designed a preregistered online experiment programmed in (Chen, Schonger, and Wickens 2016) and used an established survey instrument to manipulate respondents’ affective polarization (Ahler and Sood 2018). The preregistration document, preanalysis plan (PAP), and the executed PAP are available at https://osf.io/dxga9/?view_only=4c6863a6c44c4ed88e53154d173b8be5. See Appendix D for information about the correspondence between the manuscript and the preregistration document.¹ To measure collaborative cheating, we had U.S. voters play an online version of the collaborative cheating task, where both players’ outcomes are aligned, with monetary rewards (Weisel and Shalvi 2015). The sample is restricted to respondents who identify as Democrats or Republicans. In the experiment, players’ party affiliation is known. Participants are put into groups of two and decide sequentially. Each group consists of two types of players: X and Y. The two roles are randomly assigned and do not change throughout the experiment (partner matching). The interaction between player X and Y proceeds in three steps: (1) Player X privately rolls a dice and reports the result on the computer.² (2) Player Y is informed about the number reported by player X. Player Y then privately rolls a dice and also reports the result on the computer. (3) The two players are informed about the reported numbers and their

¹The data and code, including the oTree code for the online version of the Collaborative Cheating Game, are available at Jankowski, Erlbruch, and Tepe 2025.

²As participants may not have actual dice, the instructions include links to online dice (Appendix A Figure 2). Also see Appendix B for Instructions and Screenshots.

payoff. The payoff for each player depends on both reported numbers. The payoff is 0 USD if the two reported numbers differ. If the two reported numbers are equal (“double”), the payoff for each player equals the number they both reported in USD, that is, higher doubles lead to a higher reward. This sequential interaction is repeated 15 times. After round 15, respondents answer a post-experimental survey, and at the end of the survey, one round is randomly selected, and respondents are paid in USD the payoff of this round in addition to a fixed show-up fee (2 USD).

Similar to Weisel and Shalvi (2015), the key dependent variable measuring cheating is the number of reported “doubles” in a group. In addition, we consider alternative operationalizations of unethical behavior. One perspective is that player X, who reports the result of the dice roll first, determines the amount of money that can be gained by unethical behavior. Hence, if player X chooses to behave unethically, she/he report higher values to increase the payoffs for both players. Accordingly, we use the sum of dice numbers reported by player X and the number of reported 6s as additional measures of the cheating intention of player X. Another perspective is to focus on the maximum amount of money that can be achieved through collaborative cheating. That is, player Y matches a six reported by player X. Therefore, the number of double 6s is also used as a dependent variable. These four different operationalizations should provide a nuanced set of measures to detect unethical behavior in the online collaborative cheating game and were preregistered as such.

Treatments

In a between-subjects design, the collaborative cheating game was played under three different conditions. In the first condition – which serves as a control condition for the other two treatments – participants were randomly paired with a participant with a different party preference (*Heterogeneous Grouping*); either a Democrat plays the role of X, and a Republican plays the role of Y, or vice versa. In the second condition, the partisan affiliation of the two players is identical (*Homogeneous Grouping*).

In the third condition, participants were paired with someone of the opposing political affiliation, similar to the heterogeneous condition. However, before the instructions of the collaborative cheating game, respondents answered the item battery developed by Ahler and Sood (2018), which is designed to decrease affective polarization by correcting misperceptions about the out-party (*Heterogeneous Grouping with Depolarization*).³ Ahler and Sood (2018) demonstrate that partisans have strong misperceptions of out-partisans. They find that correcting these misperceptions decreases negative views about members of out-party. Other studies that used this battery found that it is effective in reducing affective polarization (e.g., Santoro and Broockman 2022; Broockman, Kalla, and Westwood 2023).

³Democrats were asked: “What percentage of Republicans do you think are . . . ? Age 65+ (in %) Earn over 250k\$ (in %) Evangelical (in %) Southern (in %).” Republicans were asked: “What percentage of Democrats do you think are . . . ? Black (in %) Atheist or agnostic (in %) Union members (in %) Gay, lesbian or bisexuals (in %).” After providing their responses, the respondents were informed about the true values and how much their estimates deviated from the truth using the following statement: “The percentage of [PARTY] who are [ATTRIBUTE] is [smaller/larger] than you think. Only [TRUTH]% are [ATTRIBUTE]. (You [overestimated/underestimated] by [NUM]%).”

Table 1. Number of groups by conditions

Condition		DD	RR	DR	RD	Total
Heterogeneous grouping (control)	N			83	75	158
	% row			52.5	47.5	100.0
Homogeneous grouping	N	212	69			281
	% row	75.4	24.6			100.0
Heterogeneous grouping with depolarization	N			75	83	158
	% row			47.5	52.5	100.0
Total	N	212	69	158	158	597
	% row	35.5	11.6	26.5	26.5	100.0

Note: R = Republicans, D = Democrats, 1194 respondents

Participants were assigned to treatment conditions sequentially after completing the consent form and pretreatment questionnaire. This assignment was based on their declared partisanship (Democrat/Republican), implementing blockwise complete random assignment. The grouping was determined by treatment condition, partisanship, and arrival order. Within each group, simple random assignment determined the roles (X/Y). This procedure ensured theoretically balanced matching, preventing individuals from being assigned treatments without potential partners⁴. The unequal proportion of homogeneous Democrat and Republican groups reflects the underrepresentation of Republicans in the sample. Table 1 provides insights into the realization of specific pairings during the matching phase; constant assignment probabilities at the individual level allow us to interpret specific pairs in the context of sample imbalance.

To test H1, we compare the collaborative cheating behavior of respondents in the first (Hetero.) and second (Homo.) condition. To test H2, we compare the behavior of respondents in the first (Hetero.) and third (Hetero. Depol) condition.

Sample

Participants were recruited through the online crowd-sourcing platform Prolific, which builds and maintains its participant pool primarily through word of mouth, including social media (Peer 2024). A key advantage of the platform is the ability to prescreen participants by demographic characteristics. For this study, we employed partisan affiliation as the inclusion criterion, explicitly targeting individuals who identified as Democrats or Republicans. Apart from this prescreening, we used the platform's default sampling option, whereby the study is published to all registered participants and filled on a first-come, first-served basis. Prolific's US sample includes 14,246 participants, of whom 11,329 identified themselves as Democrats and 2,917 as Republicans (June 2023). To address this imbalance, we used

⁴We examined potential treatment heterogeneity in the homo treatment and accounted for it in the analysis, finding no substantial differences in results. Consequently, detailed reporting is omitted.

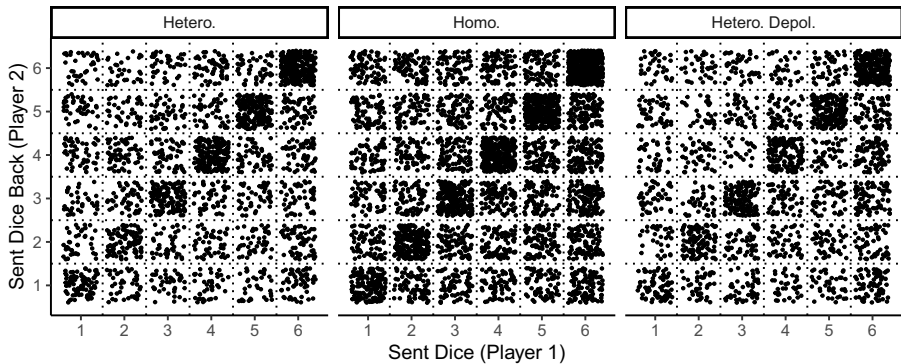


Figure 1. Observed dice distribution across conditions.

block-wise treatment assignment and oversampled Republicans. Participants who could not be matched with a partner in their specific condition were paid the show-up fee and excluded from the final sample. The target sample size was 1,002 individuals in 501 groups, with the groups evenly distributed across the treatment conditions. Power analyses indicate that this design is sufficiently powered for an average treatment effect size of 0.3 and above (Blair *et al.* 2019). A priori power analysis was conducted, considering varying expected effect sizes, with a significance criterion of $\alpha = 0.05$ and a power of $\beta = 0.80$. The recruitment of Republican participants proved difficult, so the realized sample of 1,194 individuals in 597 dyads, summarized in Table 1, fell short of the planned number of homogeneous Republican groups. A posteriori power analysis was conducted to account for the imbalance (Appendix A, Figure 6). Based on the realized sample, the least-powered comparison (Heterogeneous Grouping vs. Heterogeneous Grouping with Depolarization) yields a minimum detectable effect of approximately 0.32 at $\beta = 0.80$ and $\alpha = 0.05$. The total amount of bonus payments was 1,993 USD, corresponding to an average of 1.67 USD per participant (Min. 0, Max. 6 USD). The final sample is balanced concerning key socio-demographic and political indicators (Appendix A Table 1).

Results

Figure 1 allows for an initial visual inspection of the behavior in the cheating game under the three different conditions. Each point represents the two numbers reported in one of the rounds. It is evident that the observations cluster on the diagonal, representing doubles, with the highest density in the field representing double 6s. This confirms that participants actively cheated. Figure 2 reports the observed and expected probabilities for specific doubles per round and group across the three experimental conditions. The solid line represents the expected share of doubles if respondents had reported their dice throws honestly. We observe a probability of 0.41 for a double per round across groups in all treatments (Panel A) and no substantial differences in the types of doubles across treatment conditions (Panel B).⁵ Both figures, Figures 1 and 2, confirm that there is a substantial amount

⁵For the “aligned outcome” treatment, Weisel and Shalvi (2015) report a probability of 0.82 for a double.

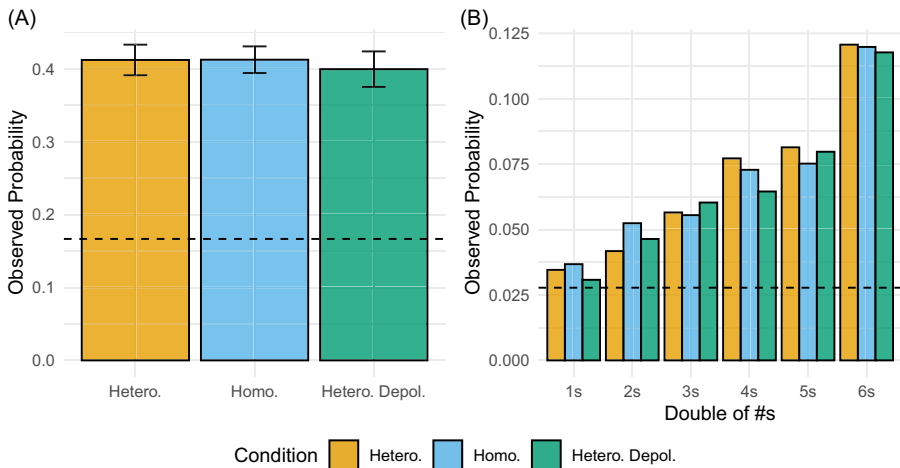


Figure 2. Collaborative cheating by condition. Mean observed probabilities for (specific) doubles at the group level across conditions with expected probability lines. The dotted line represents the expected probability assuming honesty. (A) Mean observed probability of reported doubles with error bars indicating ± 1 SE. (B) Specific reported doubles as mean observed probability.

of collaborative cheating; however, the amount of collaborative cheating seems not to differ between treatment conditions.

To test H1 and H2, we ran a set of regression models at the group level. The results are summarized in Appendix A, Tables 2 (H1) and 3 (H2). Each table represents four model specifications. In Model 1, the dependent variable is the number of doubles. Model 2 and Model 3 use, respectively, the dice numbers and the number of sixes reported by player X. Model 4 employs the number of double 6s as the dependent variable. All models are estimated using OLS regression with robust standard errors. The main independent variable for testing H1 is a dummy measuring whether the partisan group composition is homogeneous or heterogeneous. The main independent variable for testing H2 is a dummy for the depolarization task. Estimation results provide no support for H1 and H2. In none of the four models does either the homogeneous grouping or depolarization dummy yield a significant effect on cheating behavior. In addition to these group-aggregate models, we ran round-specific analyses (Appendix A Figures 3 and 4). Again, we find no support for H1 or H2 in these analyses.⁶ We also ran a series of models in which we replaced the dummy variable measuring group composition (homogeneous vs. heterogeneous) with a measure of the exact pairing (i.e., Republican-Republican, Democrat-Democrat, Republican-Democrat, Democrat-Republican). In this analysis, we also find no empirical evidence to support H1 (Appendix A, Table 5).

Finally, the effect of partisan-wise heterogeneous groups on collaborative cheating could be conditioned by respondents' self-reported strength of party affiliation. The effect hypothesized in H1 could be stronger when player Y's

⁶Considering round effects using a multilevel regression framework yields substantially similar results.

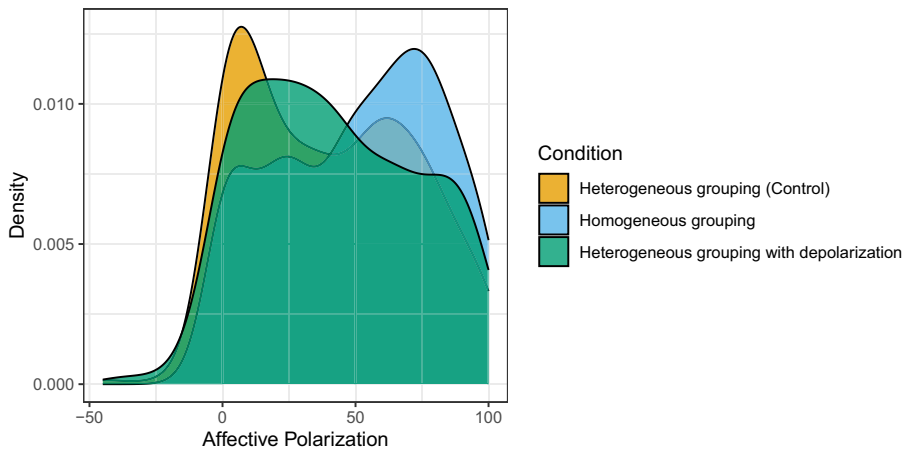


Figure 3. Affective polarization distribution across conditions.

self-reported party affiliation is stronger, and the effect hypothesized in H2 could be weaker when player Y's self-reported party affiliation is stronger. To test these expectations, we included an interaction term between the respective independent variable and the self-reported strength of party affiliation, which we had asked for in the preexperimental survey. This analysis also finds no empirical evidence for H1 or H2 (Appendix A, Figure 1, and Table 6).

As part of the robustness analysis, we checked the strength of the depolarization instrument (Ahler and Sood 2018). We expect respondents in the depolarization task to show lower levels of affective polarization, measured by the difference between in-party and out-party feelings. These differences should be significantly lower for respondents in the depolarization condition. Figure 3 shows the density of the affective polarization measure in the three conditions, indicating that the depolarization treatment was ineffective in reducing respondents' affective polarization. Because of this, we estimated the local average treatment effect (LATE) using random assignment as an instrument for affective polarization (Angrist, Imbens, and Rubin 1996; Blair, Coppock, and Humphreys 2023). However, the first-stage effect was extremely weak ($F = 1.07$, $p = 0.301$, Appendix A Table 7), so the effect cannot be credibly identified. Instead, Figure 3 suggests that playing the online collaborative cheating game in partisan-wise homogeneous groups increased affective polarization, which is an unexpected finding. To test the robustness of this observation (not preregistered), we estimated a series of regressions at the individual level (Appendix A Table 4), using affective polarization as the dependent variable and the treatment conditions as the independent variable. These analyses confirm the observation in Figure 3. The homogeneous group composition treatment caused an increase in affective polarization. This observation is also robust when controlling for respondents' characteristics such as age, gender, partisanship, and strength of party identification (Appendix A Table 4). Hence, the shared experience of collaborative cheating actually strengthens political group identity.

Discussion and conclusions

When partisanship becomes a salient social identity, it has been shown to reduce cooperation between co-partisans and out-partisans in social dilemma games (Iyengar and Westwood 2015; Carlin and Love 2013; Whitt et al. 2021). Extending this line of research, this study examined whether partisan group composition spills over into collaborative corruption, a form of corruption that is inherently cooperative, such as collusion between politicians and business leaders to secure government contracts at the public's expense. To investigate this, we conducted a large-scale online version of the collaborative cheating game (Weisel and Shalvi 2015) with 1,194 U.S. participants who identified as either Republicans or Democrats.

Consistent with prior findings on collaborative cheating (Leib et al. 2021), the narrow self-interest perspective predicting widespread cheating does not account for observed behavior. The rate of cheating (reporting doubles) remained relatively constant at around 40 % across all conditions. Contrary to *H1*, partisan homogeneity did not increase collaborative cheating. Similarly, a depolarization intervention (Ahler and Sood 2018) did not affect cheating in heterogeneous groups, contradicting *H2*. A manipulation check indicated that the depolarization intervention was ineffective at reducing polarization in our sample, despite evidence of its efficacy elsewhere (Santoro and Broockman 2022; Broockman, Kalla, and Westwood 2023). This observation may indicate that interventions at the level of thoughts (e.g., correcting misconceptions) are not, or no longer, sufficient to affect partisan animosity, and that other interventions at the relational or institutional level should be given greater consideration in future research (Hartman et al. 2022). Furthermore, previous research on partisanship and cooperative behavior has primarily examined the actions of the first player (e.g., proposer, trustor), while little attention has been given to the second player (e.g., responder, trustee). The null effects reported here suggest that player Y relied on direct information about player X's willingness to cheat rather than on partisan cues.

The mechanisms linking partisan group composition and collaborative cheating may also be more complex than expected. *H1* is derived from social identity theory (Tajfel and Turner 1979). Social comparison theory (Festinger 1954), a precursor and key component of social identity theory, could be used to derive an opposite expectation: co-partisans may be more hesitant to act poorly in front of members of their own party, while cheating more with out-partisans, as the latter's judgment is less relevant for their self-evaluation. Future research could try to disentangle the explanatory power of both theories for collaborative corruption by, for example, varying whether a third party observes participants' cheating behavior.

A noteworthy finding that has not been preregistered is that affective polarization is higher in ideologically homogeneous groups than in heterogeneous groups. Since the rate of cheating is virtually the same in these two groups, this suggests that the shared experience of collaborative cheating strengthens political group identity in homogeneous groups but not in heterogeneous groups. Thus, playing the collaborative cheating game appears to foster group identity among co-partisans. This is consistent with laboratory findings, where shared group activities (e.g., tournaments) have been effective in strengthening social groups (Eckel and Grossman 2005).

Previous research has emphasized the adverse effects of political and affective polarization, showing that partisan heterogeneity undermines cooperation and leads to socially suboptimal outcomes (Iyengar and Westwood 2015; Carlin and Love 2013; Whitt *et al.* 2021). However, these studies have not addressed whether partisan group composition influences cooperation when the cooperative behavior is mutually beneficial but unethical. Substantively, our findings suggest that partisan affiliation plays little role in enabling or deterring collaborative corruption. Theoretically, this points to a vital scope condition in the behavioral effects of polarization: while partisanship can hinder cooperation in prosocial contexts, it does not appear to inhibit or promote cooperation when the outcome is unethical. From a policy perspective, this is a reassuring insight. It suggests that partisan homogeneity, at least among the ordinary citizens studied here, does not in itself increase the risk of corrupt behavior.

Supplementary material. The supplementary material for this article can be found at <https://doi.org/10.1017/XPS.2025.10024>.

Data availability statement. The data, code, and any additional materials (including the oTree code for the online version of the Collaborative Cheating Game) required to replicate all analyzes in this article are available at the Journal of Experimental Political Science Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/OYWATT>.

Competing interests. The authors declare that there are no conflicts of interest.

Ethics statement. See online Appendix C.

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