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Making an impression: The effects of sharing conspiracy theories☆

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ABSTRACT

Conspiracy theories are widely viewed as stigmatized beliefs, and it is often assumed that sharing them will therefore have negative reputational consequences for individuals. In six experiments (two pre-registered), we examined how sharing conspiracy theories can have important consequences for both impression-management and impression-formation. Experiment 1 (N = 354) highlighted people's awareness of an impression-management strategy in sharing conspiracy theories. Participants perceived that others would share conspiracy theories when aiming to create unfavorable impressions, and would avoid sharing them to create favorable impressions. Experiments 2 and 3 (Ns = 137 and 150) examined participants' own impression-management motives for sharing conspiracy theories and demonstrated that these motives depended on their own conspiracy beliefs. Specifically, participants with weaker conspiracy beliefs perceived that they would share conspiracy theories mainly to portray themselves negatively, and as radical, unstable, and unique people, whereas those with stronger conspiracy beliefs perceived that they would share conspiracy theories mainly to appear stable and honest. Experiments 4a, 4b and 5 (Ns = 248, 250 and 417) focused on impression-formation. Participants evaluated fictitious politicians who shared (vs. refuted) conspiracy theories as less predictable and competent, but also as a "rogue" political outsider who is likely to effect change. Moderation analyses indicated that these differences were less pronounced or even reversed among participants with right-wing attitudes (Experiments 4a, Experiment 5) and those with strong conspiracy beliefs (Experiment 5). We discuss the importance of examining conspiracy theories from this communicative perspective.

Conspiracy theories propose that malicious plots are carried out in secret by self-interested groups (e.g., Abalakina-Paap, Stephan, Craig, & Gregory, 1999; Goertzel, 1994). For example, well-known conspiracy theories propose that the 9/11 attacks in New York were an inside job that was carefully executed to justify the war on terror. More recently, conspiracy theories about COVID-19 have proposed that the virus was released deliberately by the Chinese government to wage war on the West, or that vaccines contain microchips as a means of government surveillance and control. A significant and rapidly growing literature has identified some of the reasons why people are attracted to conspiracy theories, and has also revealed that conspiracy theories have important consequences for individuals, groups, and societies (for reviews, see Douglas, Sutton, & Cichocka, 2017; Douglas et al., 2019 and Douglas & Sutton, 2018). However, very little is known about when and why people choose to share conspiracy theories, and even less is known about the social consequences people might experience when they do. We addressed these questions in six experiments. We first examined the extent to which people perceive that others will share—and that they, themselves will share—conspiracy theories when aiming to create specific impressions. We then focused on the impressions people form of individuals who do share conspiracy theories.

The psychology of conspiracy theories is a flourishing topic of research, and has grown rapidly in the last 15 years. Research suggests that people adopt conspiracy narratives when fundamental psychological motives are frustrated, including the motives to achieve certainty and accuracy, security and control, and to maintain both individual and group self-esteem (Douglas et al., 2017). People endorse conspiracy theories in an attempt to restore these psychological goods and to cope with life's problems, even though this attempt may not be successful (Marchlewska, Green, Cichocka, Molenda, & Douglas, 2021). Some of the negative consequences of conspiracy theories have also been identified in recent years, such as political disengagement (Jolley & Douglas, 2014a), illegal political action (Imhoff, Dieterle, & Lamberty, 2021), violence (Jolley & Paterson, 2020; Uscinski & Parent, 2014), vaccine

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hesitancy (Hornsey, Finlayson, Chatwood, & Begeny, 2020; Jolley & Douglas, 2014b), reluctance to engage in efforts to reduce the spread of COVID-19 (Biddlestone, Green, & Douglas, 2020), science denial (Rutjens, Heine, Sutton, & van Harreveld, 2018), and denial of climate change (Lewandowsky, Oberauer, & Gignac, 2013). Psychologists have therefore learned, and continue to learn, important details about why people believe in conspiracy theories, and what the consequences are of these beliefs.

Despite this progress, much less is known about people's perceptions of conspiracy theories and the people who share them, either interpersonally or on a larger scale (Douglas et al., 2019). Shedding light on these issues has important theoretical and practical implications related to how conspiracy theories that are shared widely affect political and moral attitudes, as well as political behavior (e.g., electoral support). At the interpersonal level, understanding how people perceive others who share conspiracy theories has important implications for the quality of personal relationships.

Scholars have argued that evaluations of individuals who share conspiracy theories are generally negative. For example, it has been argued that believing in conspiracy theories points to gullibility and a lack of credibility (Cassam, 2016; Klein, van der Linden, Pantazi, & Kissine, 2015). Further, the term "conspiracy theory" itself seems to carry significant baggage (Harambam & Aupers, 2017; Husting & Orr, 2007). Research suggests that the term is largely rejected by people when they describe statements they endorse (Wood & Douglas, 2013), and instead it is used to describe statements they do not endorse (Douglas, Prooijen, & Sutton, 2021). There is also evidence that people perceive conspiracy theories to be stigmatizing beliefs. In two studies, Lantian et al. (2018) asked French Internet respondents to either defend or support conspiracy theories about the 2015 Charlie Hebdo attack in Paris. Participants who had been asked to defend the conspiracy theories were more fearful of social exclusion compared to those who were asked to criticize them. In both experiments, this effect was mediated by the fear of being evaluated negatively. Participants in this research also rated other individuals who believed in the Charlie Hebdo conspiracy theories as more likely to be targets of negative evaluation. People therefore seem to perceive that there are social consequences for individuals who believe in conspiracy theories.

In the current research, we explored these social consequences in more detail, focusing specifically on the perceived social consequences of sharing conspiracy theories with others. The communication of conspiracy theories, and the implications of sharing them, have thus far been largely overlooked by psychologists (Douglas et al., 2019). Given that conspiracy theories are shared so energetically and are often shared more than the scientific news from which they originate (Bessi et al., 2015), it stands to reason that there should be both costs and benefits to sharing conspiracy theories depending on the context. Research to date does not allow us to understand the impression-formation consequences of sharing conspiracy theories, but this is an important open question.

People might perceive that conspiracy believers are non-mainstream dissenters within a problematic political system. For example, conspiracy theories are frequently used to challenge mainstream political sentiments (Sapountzis & Condor, 2013) and expressing dissenting views like this can sometimes be constructive in intra- and intergroup contexts by calling into question problems and triggering reform (Hornsey, 2016). There is also evidence that people may believe in conspiracy theories when they want to appear unique compared to others (Lantian, Muller, Nurra, & Douglas, 2017). Therefore, seeing someone share a conspiracy theory may mark them out to perceivers as 'not just one of the crowd', and someone who might hold unique and potentially important and useful information. These possible impressions that people might form of conspiracy believers have thus far not been explored.

The potential for conspiracy theories to shape impression-management and impression-formation is especially important in the domain of politics. The link between right-wing populism and conspiracy theories has been a growing concern (Bergmann, 2018). In recent years, political leaders, such as former US president Donald Trump, have frequently used conspiracy narratives to discredit political opposition and express objections to the 'system' (e.g., mainstream media) more generally. For example, former president Trump argued that the 2020 presidential election had been stolen from him by the fraudulent activities of the Democrats and their supporters (Southern, 2022). Furthermore, he regularly re-tweeted material from QAnon Twitter accounts arguing that the Democrats were involved in a human trafficking and child sex ring (Nguyen, 2020). He also frequently referred to COVID-19 as the "Chinese virus", perpetuating conspiracy theories about the origins of the virus and fueling intergroup tensions between the USA and China (Reja, 2021). On more than one occasion, former president Trump also criticized the mainstream media, calling them the "enemy of the people" (Smith, 2019) who maliciously mislead the American public.

Politicians may stand to gain and lose from spreading conspiracy theories like this. By sharing alternative or non-mainstream narratives, they may look unreliable, unstable, and a non-credible source of information (Cassam, 2016; Klein et al., 2015). On the other hand, they can portray themselves as political outsiders (Sapountzis & Condor, 2013). In other words, by establishing themselves as an alternative to an allegedly corrupt political elite, they may be able to garner political support from people who feel disenfranchised with the status quo, or who are undecided voters (Castanho Silva, Vegetti, & Littvay, 2017). Spreading conspiracy theories in particular contexts may therefore be a political tactic, conscious or unconscious, to appear to be 'one of the people' and attract votes.

In the present research, we endeavored to shed more light on people's perceptions of conspiracy theories and the people who share them. Specifically, across six experiments, we examined intended and actual consequences of sharing these allegedly stigmatized beliefs. In the first experiment, participants rated the extent to which they thought a person would share a conspiracy theory when they want to make a positive or negative impression of themselves (Experiment 1). In the next two experiments, participants rated the extent to which they personally would share conspiracy theories in order to create a range of different impressions both generally (Experiment 2) and within a political context (Experiment 3). Then, in three further experiments, we studied the impressions that people form of a fictitious political candidate who endorses conspiracy theories in a speech vs. refutes them (Experiments 4a, 4b, and 5). In other words, we examined the extent to which conspiracy theories are likely to increase or decrease support for a politician who shares them.

1. Experiment 1

In the first experiment, we examined the extent to which people think that others will share conspiracy theories when they are motivated to create a favorable vs. less favorable impression of themselves. We asked participants to complete a single-item measure of conspiracy beliefs, followed by different instructions. Specifically, following a similar design to Sutton, Robinson, and Farrall (2011), we asked one group of participants to answer in the way they thought others would if they wanted to appear stable—that is, if they wanted to fake appearing good. Another group were asked to answer in the way they thought others would if they wanted to create the opposite impression, namely, to appear unstable—that is, if they wanted to fake appearing bad. We compared these groups to each other, and a baseline group where participants responded to the single-item conspiracy scale without further instructions.

We hypothesized that, in comparison to the baseline group, participants would report higher conspiracy beliefs in the case of aiming to appear unstable (i.e., fake-bad), and lower conspiracy beliefs in the case of aiming to appear stable (i.e., fake-good).

1.1. Method

1.1.1. Participants and design

Participants were recruited in June 2019 using Prolific. The experiment was part of a larger study including measures that are not relevant to the current investigation. Eligibility for participation was restricted to Prolific workers from the USA who were at least 18 years of age and had an approval rate of at least 95% across prior assignments. The sample consisted of 354 participants (47.18% male, 51.41% female, 1.41% other; $M_{age} = 34.23$, $SD_{age} = 12.78$, range = 18–79 years). Sample size was determined based on resource constraints. Data were only analyzed once the experiment was complete. To inform the expected pairwise comparisons between experimental conditions, sensitivity analyses revealed that this sample size was sufficient to detect effect sizes as small as $d = 0.42$ in an independent sample *t*-test with 90% statistical power and $\alpha = 0.05$ (two-tailed). Participants were paid a small fee for their time. The experiment followed a three-group between-subjects design, with the experimental manipulation of the presentation instructions (baseline, fake-good, fake-bad).

1.1.2. Materials and procedure

After providing informed consent, participants completed a short self-presentation task, with instructions similar to Sutton et al. (2011). Participants were randomly assigned to one of three experimental conditions: baseline, fake-good, or fake-bad. In the baseline condition, participants were simply asked to answer the single-item scale of conspiracy beliefs (Lantian, Muller, Nurra, & Douglas, 2016). This measure presented participants with a brief preamble followed by the statement: "I think that the official version of the events given by the authorities very often hides the truth". Participants were asked to indicate how true they believed this statement to be (1 = completely false, 9 = completely true). In the fake-good condition, participants again responded to this measure but were asked to answer in the way they thought "a person might if they were trying to appear very well adjusted (e.g., if they wanted to secure release from a psychiatric institution)". In contrast, in the fake-bad condition, participants were asked to answer in the way they thought "a person might if they were trying to appear to have serious psychological or emotional problems (e.g., if they wanted to beat a criminal charge on the basis of diminished responsibility)". Participants then completed some brief demographic questions and were thanked, debriefed, and paid.

1.2. Results and discussion

We used a one-way ANOVA to test for mean differences in reported conspiracy beliefs across the three experimental conditions. We observed a significant main effect of our experimental manipulation, $F(2, 351) = 62.34$, $p < .001$, $\eta_p^2 = 0.26$, 95% CI [0.19, 0.33]. Planned contrasts revealed that between the baseline condition ($n = 119$, $M = 5.08$, $SD = 2.22$) and the fake-bad condition ($n = 116$, $M = 6.99$, $SD = 2.28$; −1 = fake-bad, 1 = baseline, 0 = fake-good), participants reported significantly higher conspiracy beliefs in the fake-bad condition, $t(351) = 6.19$, $p < .001$, $d = 0.81$, 95% CI [0.54, 1.07]. Between the baseline and the fake-good condition ($n = 119$, $M = 3.56$, $SD = 2.57$; 0 = fake-bad, 1 = baseline, −1 = fake-good), participants reported significantly lower conspiracy beliefs in the fake-good condition, $t(351) = 4.99$, $p < .001$, $d = 0.65$, 95% CI [0.39, 0.91].

These findings firstly suggest that people associated the expression of conspiracy beliefs with less favorable interpersonal impressions (in this case, signaling instability). However, a second important conclusion is that people associated the suppression of conspiracy beliefs with more favorable interpersonal impressions (in this case, to appear well-adjusted). Taken together, the results support previous findings indicating that the expression of conspiracy beliefs may entail social costs (e.g., social exclusion; Lantian et al., 2018). In Experiment 2, we focused on how people themselves would use conspiracy theories to create specific impressions. We also expanded the range of impressions potentially associated with sharing conspiracy theories, including the intentions to appear more radical (vs. mainstream) and unique (vs. average).

2. Experiment 2

In support of previous research indicating that there are social costs of expressing conspiracy theories, participants in Experiment 1 predicted that others would strategically share conspiracy theories to create a less favorable social impression. In Experiment 2, we focused on participants' own strategic use of conspiracy theories in a self-presentation context. We also considered other unexplored social consequences of sharing conspiracy theories. For instance, people who want to appear more radical or opposed to mainstream politics (vs. mainstream; e.g., Sapountzis & Condor, 2013), or who wish to appear unique (vs. average) compared to others (Lantian et al., 2017), might pursue those goals by sharing conspiracy theories.

Following the results of Experiment 1, we predicted that people would avoid sharing conspiracy theories to create favorable and stable impressions of themselves. However, since the endorsement of conspiracy theories is associated with some traits that might be desirable in some contexts, such as nonconformity and unconventional political beliefs (Lantian et al., 2017; Sapountzis & Condor, 2013), we predicted that people would share conspiracy theories more when they wished to portray themselves as unique, or politically radical. Finally, we explored the potentially moderating role of interindividual differences in the endorsement of conspiracy theories. Specifically, people who believe strongly in conspiracy theories might expect that it will be advantageous to share them because they feel that they are sharing truthful, reliable information, and that it will make them look good to do so. On the other hand, people who do not believe in conspiracy theories might expect that it will be advantageous to avoid them so as not to share falsehoods or unreliable information. They might anticipate that it does not make them look good to share conspiracy theories. We therefore explored whether people high or low in conspiracy belief share conspiracy theories in different ways to achieve different impression-management goals.

2.1. Method

2.1.1. Participants and design

We obtained 160 responses from Prolific, from May to July 2019. We erased data from 13 cases with duplicated IP addresses (we kept the first response of each duplicated case), as well as 10 cases with incomplete responses. The final sample consisted of 137 participants from the UK (37.96% male, 57.66% female, 0.73% transgender/other; $M_{age} = 29.59$, $SD_{age} = 11.89$, range = 18–74 years). Sample size was determined based on resource constraints. Data were only analyzed once the experiment was complete. Sensitivity analyses revealed that this sample size was sufficient to detect within-subject differences in the expression of conspiracy theories between impression goals as small as $d = 0.28$ in a paired sample *t*-test with 90% statistical power and $\alpha = 0.05$ (two-tailed). Participants were paid a small fee for their time.

The experiment followed a multivariate mixed-factorial design. Through the self-presentation instructions, we manipulated within-subjects the attribute that participants aimed to portray across different potentially correlated measures (e.g., positive vs. negative, politically radical vs. mainstream). The between-subjects factor was the different conspiracy theories we asked participants to express their opinion about. This was one of six possible conspiracy theories taken from Douglas et al. (2016; e.g., MI6's involvement in Princess Diana's death, the creation of AIDS in a laboratory), which were counterbalanced. Participants' own conspiracy beliefs were used as an exploratory moderator of the predicted experimental effects.

2.1.2. Materials and procedure

After providing informed consent, we randomly presented participants with one of the six conspiracy theory statements (MI6's involvement in Princess Diana's death, the creation of AIDS in a laboratory, the 9/11 attacks being a governmental plot, the Apollo moon landings being faked, government suppression of alien contact, or CIA involvement in the JFK assassination).[1] Participants were asked to rate their agreement (1 = strongly disagree, 7 = strongly agree).

We then asked participants how they would respond to the same statement if they were trying to create specific impressions of themselves. In particular, in random order we asked participants to portray themselves "in the best possible light" and "in an unfavorable manner" (i.e., positive vs. negative), as a person with radical political views and as a person with mainstream political views (i.e., radical vs. mainstream), as mentally stable and as mentally unstable (i.e., stable vs. unstable), as a unique or "non-average" person or as a typical or average person (i.e., unique vs. average). Finally, participants completed some brief demographic questions and were finally thanked, debriefed, and paid.

2.2. Results and discussion

2.2.1. Main analyses

We tested whether participants reported higher conspiracy beliefs when aiming to portray specific impressions on others–i.e., negative (vs. positive), politically radical (vs. mainstream), mentally stable (vs. unstable), and unique (vs. average). To do so, we performed a repeated-measures multivariate ANOVA, in which we included the attribute as a within-subject predictor for the four pairs of repeated measures of conspiracy beliefs (i.e., positive vs. negative, radical vs. mainstream, stable vs. unstable, unique vs. average). To check if this effect held across the six different conspiracy theories we used as stimuli, our model further included the different conspiracy theories as a between-subjects factor.

We observed a significant multivariate effect of attribute, $F(4, 127) = 20.53$, $p < .001$, $\eta_p^2 = 0.39$, 95% CI [0.25, 0.49], indicating that participants expressed significantly different levels of conspiracy beliefs depending on the kind of impression they aimed to portray. We also found a multivariate effect of conspiracy theories, $F(20, 520) = 2.22$, $p = .002$, $\eta_p^2 = 0.08$, 95% CI [0.01, 0.09], which suggested that, irrespective of their impression management goals, participants endorsed the six conspiracy theories differently. However, the effect of attribute was similar across the different conspiracy theories, as indicated by the non-significant interaction between attribute and conspiracy theories, $F(20, 520) = 0.90$, $p = .592$, $\eta_p^2 = 0.03$, 95% CI [0.00, 0.03].

In more detail, the univariate effects of attribute (see Table 1) showed that participants expressed significantly higher conspiracy beliefs when they aimed to be perceived negatively ($M = 5.04$, $SD = 2.06$) vs. positively ($M = 2.96$, $SD = 1.81$), $d = 0.63$, 95% CI [0.44, 0.81], as a person with radical ($M = 5.06$, $SD = 2.20$) vs. mainstream ($M = 3.00$, $SD = 1.63$) political views, $d = 0.70$, 95% CI [0.51, 0.88], as mentally unstable ($M = 5.08$, $SD = 2.30$) vs. stable ($M = 3.25$, $SD = 1.95$), $d = 0.47$, 95% CI [0.29, 0.65], and as unique ($M = 4.69$, $SD = 1.68$) vs. average ($M = 3.38$, $SD = 1.54$), $d = 0.56$, 95% CI [0.38, 0.74]. Moreover, the univariate effects of conspiracy theories indicated that differences in the expression of conspiracy beliefs between the conspiracy theories used as stimuli only emerged when participants aimed to present themselves as radical (vs. mainstream) and unique (vs. average). The interaction between attribute and conspiracy theories was not significant across the different dependent variables, which indicated that the univariate effects of attribute remained constant across the different conspiracy theories.

Table 1
Univariate effects of within- and between-subject manipulations on expression of conspiracy theories (Experiment 2).

	F	*df*	*p*	η_p^2	95% CI
Within-subjects					
Attribute					
Positive vs. negative	54.34	1, 130	< 0.001	0.30	[0.17, 0.41]
Mainstream vs. radical	65.12	1, 130	< 0.001	0.33	[0.21, 0.44]
Stable vs. unstable	28.61	1, 130	< 0.001	0.18	[0.08, 0.29]
Average vs. unique	41.47	1, 130	< 0.001	0.24	[0.12, 0.36]
Attribute x CTs					
Positive vs. negative	1.64	5, 130	0.155	0.06	[0.00, 0.12]
Mainstream vs. radical	1.10	5, 130	0.362	0.04	[0.00, 0.09]
Stable vs. unstable	0.88	5, 130	0.496	0.03	[0.00, 0.07]
Average vs. unique	0.30	5, 130	0.914	0.01	[0.00, 0.02]
Between-subjects					
CTs					
Positive vs. negative	1.97	5, 130	0.087	0.07	[0.00, 0.13]
Mainstream vs. radical	2.55	5, 130	0.031	0.09	[0.00, 0.16]
Stable vs. unstable	0.98	5, 130	0.435	0.04	[0.00, 0.08]
Average vs. unique	3.08	5, 130	0.012	0.11	[0.01, 0.18]

Note: CTs = Conspiracy theories.

2.2.2. Exploratory analyses

We further considered whether interindividual differences in the endorsement of conspiracy theories reflected different impression goals associated with the expression of conspiracy beliefs. We therefore explored whether participants' baseline measure of conspiracy beliefs ($M = 2.82$, $SD = 1.74$) moderated the effect of the impression goals.[2] We followed a *diff-in-diff* approach in a multivariate multilevel framework, using the R package lme4 (version 1.1–27; Bates, Mächler, Bolker, & Walker, 2015). Specifically, we regressed the differences between repeated measures (i.e., negative - positive, unstable - stable, radical - mainstream, dishonest - honest, unique - average) on participants' conspiracy beliefs as a Level-1 continuous predictor. Note that fitting a multivariate multilevel model in *lme4* requires the inclusion of the different dependent variables (in this case, the different attributes, e.g., negative/positive vs. unstable/stable) as a Level-2 categorical predictor. The model further included the cross-level interaction between the two fixed effects and the participants' number as random intercept.

To simplify the reporting of the results, we looked at the omnibus test of each predictor included in the model. The effect of the different attributes was significant, $F(3, 402) = 6.98$, $p < .001$, $\eta_p^2 = 0.05$, indicating that differences between repeated measures differed across different attributes. More critically, the effect of participants' conspiracy beliefs was significant, $F(1, 134) = 25.72$, $p < .001$, $\eta_p^2 = 0.06$, as well as the cross-level interaction, $F(3, 402) = 6.56$, $p < .001$, $\eta_p^2 = 0.04$, which indicated that the moderating role of participants' conspiracy beliefs differed across attributes. The pattern of this interaction is summarized in Fig. 1, and we present simple slopes analyses at one standard deviation below and above the mean in the Supplementary Materials. Since our sample was on average low in conspiracy beliefs—considering the midpoint of the scale as reference, $t(135) = -7.89$, $p < .001$, $d = -0.68$, 95% CI [−0.87, −0.49]—we also presented simple slopes at two standard deviations above the mean. We observed that it was participants with weaker conspiracy beliefs who drove most effects. The less participants believed in conspiracy theories, the more they modulated their endorsement of conspiracy theories when aiming to appear negative (vs.

[1] Previous research found that people identify these statements as conspiracy theories, even when no label is provided (Douglas et al., 2021, Study 2). Responding to the question "To what extent do you think this statement is a conspiracy theory?", participants rated all statements significantly above the midpoint. In the absence of contextualizing information or the label "conspiracy theory", these statements are therefore rated as conspiracy theories.

[2] For comparisons between the baseline measure of conspiracy beliefs and different impression goals, see the Supplementary Materials.

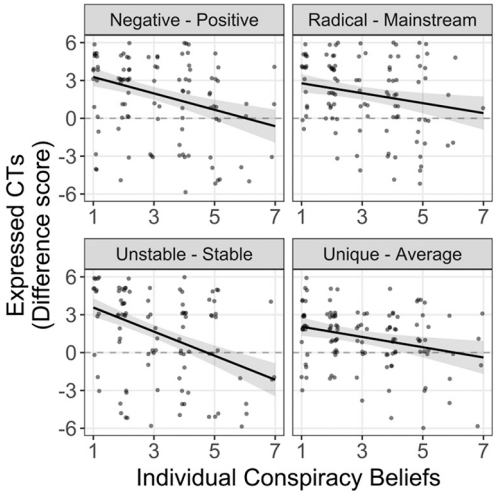

Fig. 1. Moderating effect of individual conspiracy beliefs on the expression of conspiracy theories for different impression formation goals in Experiment 2. Scores above zero show that conspiracy theories were endorsed more regarding the first impression goal (e.g., unstable), whereas scores below zero show they were endorsed more regarding the second impression goal (e.g., stable).

positive), radical (vs. mainstream), and unique (vs. average). Regarding the impression of instability (vs. stability), the interaction showed that both people with low (i.e., -1SD) and people with very high (+2SD) conspiracy beliefs shared these with opposite intentions (i.e., the former to appear unstable and the latter to appear stable).

In summary, as in Experiment 1, Experiment 2 supported the notion that people have distinct conceptions of the communicative value that conspiracy theories may have for creating different types of interpersonal impressions. However, Experiment 2 introduced some nuance. Specifically, our findings suggest that people with weaker conspiracy beliefs mainly associate the expression of conspiracy theories with the creation of unfavorable impressions (i.e., negative, radical, unstable) and the projection of uniqueness, while those with more extreme conspiracy beliefs might express conspiracy theories to appear stable. Based on their own conspiracy beliefs, people therefore seem to have distinct conceptions of the type of impressions that are created by sharing conspiracy theories. Furthermore, the results support existing research indicating that conspiracy theories are generally used to express uniqueness (Lantian et al., 2017), radical views (Sapountzis & Condor, 2013) and, depending on the endorsement of these theories, stability or instability (Hofstadter, 1964).

Like Experiment 1, however, the current experiment did not provide a specific context for participants when creating these impressions. Arguably, different contexts could change people's impression goals, and consequently, their use of conspiracy theories. Therefore, in the next experiment, we aimed to conceptually replicate the findings of Experiment 2, while introducing a specific context (i.e., politics) and including a new impression goal relevant to this context (i.e., appearing honest vs. dishonest).

3. Experiment 3

Experiment 3 was a pre-registered replication of Experiment 2 in which participants were asked to present themselves in a political context (i.e., acting as a politician running for office; https://osf.io/3mvwx/?view_only=96e63724255747e8a3c05dd97af2e26c). The context of politics is important to the study of conspiracy theories for at least two reasons. First, people's beliefs in conspiracy theories are closely related to their political ideology (Imhoff et al., 2022). Second, and critically for the present research, conspiracy theories are often part of the political speech of (populist) politicians, which raises questions about the consequences that conspiracy theories have on people's evaluations of conspiracist political candidates and people's political attitudes more generally (Bergmann, 2018; Castanho Silva et al., 2017). Furthermore, we introduced an additional impression goal, namely, the intention to appear honest (vs. dishonest). Honesty is one of the most important traits argued and observed to predict the evaluation of political leaders and institutions (Bertsou, 2019; Fridkin & Kenney, 2011; Grönlund & Setälä, 2012). We therefore examined whether people choose to share conspiracy theories with the intention of appearing honest (vs. dishonest).

As in Experiment 2, we predicted that participants—acting as a political candidate—would share conspiracy theories more to appear unique (vs. average), politically radical (vs. mainstream), negative (vs. positive), and unstable (vs. stable). Regarding the impression goal to appear honest (vs. dishonest), our initial prediction was based on two assumptions: first, that most people do not believe (or at least, not strongly) in conspiracy theories (taking the distributions of Experiments 1 and 2 as example); and second, that conspiracy theories are typically negatively-evaluated beliefs. If we conceptualize honesty as sharing what one believes to be true, we should expect that, due to the majority of people not believing in conspiracy theories, the average use of conspiracy theories will be oriented to make the negative impression of being dishonest (vs. honest). However, informed by the results from Experiment 2, a more nuanced picture could be drawn when considering people's own conspiracy beliefs (and thus, what each individual considers to be true) as a moderating factor. Therefore, as in the previous study, we also explored the moderating role of participants' own conspiracy beliefs on the formation of an honest (vs. dishonest) impression.

3.1. Method

3.1.1. Participants

Based on our pre-registration, we recruited 150 participants from the USA via Prolific, in April 2022 (52.00% male, 47.30% female, 0.07% other; M_{age} = 35.89, SD_{age} = 12.78, range = 18–74 years). Sensitivity analyses revealed that this sample size was sufficient to detect within-subject differences in the expression of conspiracy theories between impression goals as small as $d = 0.27$ in a paired sample t-test with 90% statistical power and $\alpha = 0.05$ (two-tailed). Participants were paid a small fee for their time.

3.1.2. Materials and procedure

Participants followed the same procedure as in Experiment 2, with minor modifications. First, participants were randomly assigned to one of the six conspiracy theory statements and reported their baseline belief in the respective conspiracy theory. Then, they were asked to imagine that they are a political candidate running in an election, before asking them how they would respond to the conspiracy statement if they were trying to create specific impressions of themselves. Participants responded to the same impression goals as in Experiment 2, as well as an additional impression goal: honest vs. dishonest.

3.2. Results and discussion

3.2.1. Main analyses

We aimed to test whether participants reported higher conspiracy beliefs when aiming to portray specific impressions on others–i.e., negative (vs. positive), politically radical (vs. mainstream), mentally stable (vs. unstable), unique (vs. average), and dishonest (vs. honest). We therefore performed a repeated-measures multivariate ANOVA, in which we included the attribute as a within-subjects predictor for the five pairs of repeated measures of conspiracy beliefs (i.e., positive vs. negative, radical vs. mainstream, stable vs. unstable, unique vs. average, honest vs. dishonest). To check if this effect held across the six different conspiracy theories we used as stimuli, our model further included the different conspiracy theories as a between-subjects factor.

Table 2
Univariate effects of within- and between-subject manipulations on expression of conspiracy beliefs (Experiment 3).

	F	*df*	*p*	η_p^2	95% CI
Within-subjects					
Attribute					
Positive vs. negative	1.54	1, 144	0.216	0.01	[0.00, 0.07]
Mainstream vs. radical	28.29	1, 144	< 0.001	0.16	[0.07, 0.27]
Stable vs. unstable	3.52	1, 144	0.063	0.02	[0.00, 0.09]
Average vs. unique	40.02	1, 144	< 0.001	0.22	[0.11, 0.33]
Honest vs. dishonest	0.70	1, 144	0.403	0.00	[0.00, 0.05]
Attribute x CTs					
Positive vs. negative	1.53	5, 144	0.184	0.05	[0.00, 0.10]
Mainstream vs. radical	1.44	5, 144	0.214	0.05	[0.00, 0.10]
Stable vs. unstable	1.33	5, 144	0.263	0.04	[0.00, 0.09]
Average vs. unique	1.39	5, 144	0.230	0.05	[0.00, 0.10]
Honest vs. dishonest	2.49	5, 144	0.034	0.08	[0.00, 0.15]
Between-subjects					
CTs					
Positive vs. negative	2.14	5, 144	0.063	0.07	[0.00, 0.13]
Mainstream vs. radical	1.09	5, 144	0.368	0.04	[0.00, 0.08]
Stable vs. unstable	1.25	5, 144	0.288	0.04	[0.00, 0.09]
Average vs. unique	2.29	5, 144	0.049	0.07	[0.01, 0.14]
Honest vs. dishonest	0.85	5, 144	0.514	0.03	[0.00, 0.07]

Note: CTs = Conspiracy theories.

We observed a significant multivariate effect of attribute, $F(5, 140) = 19.79$, $p < .001$, $\eta_p^2 = 0.41$, 95% CI [0.27, 0.50], indicating that participants expressed significantly different levels of conspiracy beliefs depending on the kind of impression they aimed to portray. We also found a multivariate effect of conspiracy theories, $F(25, 720) = 1.67$, $p = .021$, $\eta_p^2 = 0.06$, 95% CI [0.00, 0.06], which suggested that participants differed in their expression of conspiracy beliefs toward the different conspiracy theories used as stimuli. However, the effect of attribute was similar across the different conspiracy theories, as indicated by the non-significant interaction between attribute and conspiracy theories, $F(25, 720) = 1.17$, $p = .258$, $\eta_p^2 = 0.04$, 95% CI [0.00, 0.03].

Specifically, the univariate effects of attribute (see Table 2) showed that participants expressed significantly higher conspiracy beliefs when they aimed to be perceived as a politician with radical ($M = 4.58$, $SD = 2.21$) vs. mainstream ($M = 3.10$, $SD = 1.71$) political views, $d = 0.43$, 95% CI [0.26, 0.60], and as unique ($M = 4.52$, $SD = 1.65$) vs. average ($M = 3.20$, $SD = 1.67$), $d = 0.50$, 95% CI [0.33, 0.67]. We did not observe significant differences when people presented themselves negatively as a politician ($M = 4.09$, $SD = 2.30$) vs. positively ($M = 3.69$, $SD = 2.20$) $d = 0.10$, 95% CI [−0.07, 0.26], as unstable ($M = 4.45$, $SD = 2.46$) vs. stable ($M = 3.78$, $SD = 2.30$), $d = 0.15$, 95% CI [−0.02, 0.31], or as dishonest ($M = 3.73$, $SD = 2.33$) vs. honest ($M = 4.13$, $SD = 2.16$), $d = -0.09$, 95% CI [−0.25, 0.07]. Furthermore, the univariate effects of conspiracy theories indicated that differences in the expression of conspiracy beliefs between the conspiracy theories used as stimuli only emerged when participants aimed to present themselves as a politician who is unique (vs. average). The interaction between attribute and conspiracy theories was significant for honest (vs. dishonest), but not significant across the remaining dependent variables, which indicated that the univariate effects of attribute remained constant across the different conspiracy theories except for honesty (vs. dishonesty).

3.3. Exploratory analyses

We again explored whether the individual endorsement of conspiracy theories was associated with different impression goals. To do so, we tested participants' baseline conspiracy beliefs ($M = 2.55$, $SD = 1.54$) as a moderator, following the same *diff-in-diff* multilevel approach as in Experiment 2.[3] The model showed that the effect of the different attributes was not significant, $F(4, 592) = 1.07$, $p = .372$, $\eta_p^2 = 0.01$. However, the significant effect of participants' conspiracy beliefs, $F(1, 148) = 13.58$, $p < .001$, $\eta_p^2 = 0.02$, confirmed that differences between opposing impression goals existed as a function of this variable, whereas the significant cross-level interaction, $F(4, 592) = 3.76$, $p = .005$, $\eta_p^2 = 0.02$, suggested that the moderating role of participants' conspiracy beliefs differed across impression goals. As depicted in Fig. 2 and summarized in the simple slopes analyses presented in the Supplementary Materials, for portraying themselves negatively (vs. positively) as a politician or as unstable (vs. stable), people with weaker conspiracy beliefs (i.e., -1SD) expressed conspiracy theories to a greater extent to present themselves more negatively as a politician and as more unstable. People with stronger conspiracy beliefs (+1SD) were no different in their use of conspiracy theories between these two impression goals. However, we observed that people with extreme conspiracy beliefs (+2SD) expressed conspiracy theories to a significantly greater extent to present themselves more positively as a politician and as more stable. Regarding self-presentation as radical (vs. mainstream) and unique (vs. average), the interaction mainly described how people with weaker conspiracy beliefs expressed conspiracy beliefs to a greater extent to portray themselves as a politician who is more radical and unique. Finally, for the new impression goal of appearing honest (vs. dishonest), those with weaker conspiracy beliefs expressed conspiracy theories to a greater extent to portray themselves as a politician who is dishonest (vs.

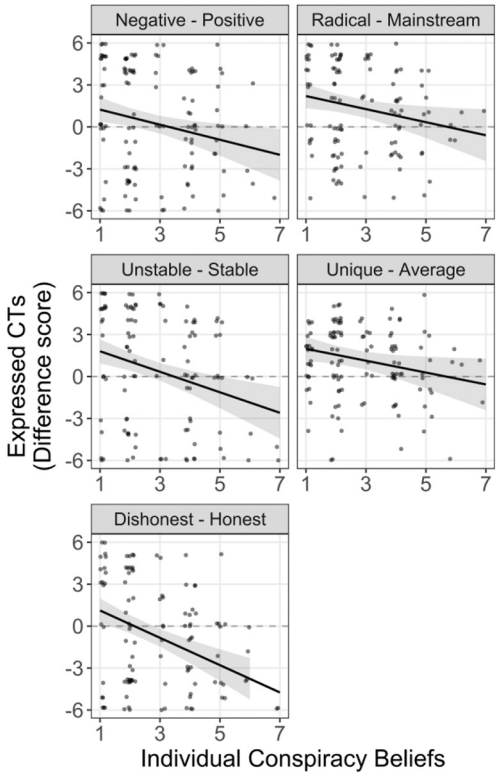

Fig. 2. Moderating effect of individual conspiracy beliefs on the expression of conspiracy theories for different impression formation goals in Experiment 3. Scores above zero show that conspiracy theories were endorsed more regarding the first impression goal (e.g., unique), whereas scores below zero show they were endorsed more regarding the second impression goal (e.g., average).

[3] For comparisons between the baseline measure of conspiracy beliefs and different impression goals, see the Supplementary Materials.

honest), whereas those with stronger and more extreme conspiracy beliefs expressed them to appear honest (vs. dishonest).

Taken together, Experiment 3 partially replicated the results found in Experiment 2. Participants shared conspiracy theories more to create an impression of uniqueness and as having radical views, supporting previous research in the literature (Lantian et al., 2017; Sapountzis & Condor, 2013). However, we did not find differences in the sharing of conspiracy theories with the aim of creating a negative impression as a politician, or to appear unstable or dishonest. According to our exploratory findings, this was the case because the strategic expression of conspiracy theories depended on the individual's own level of conspiracy beliefs. As in Experiment 2, people with weaker conspiracy beliefs mainly associated the sharing of conspiracy theories with creating negative impressions and appearing radical, unstable, and unique as a politician. In contrast, those with very strong conspiracy beliefs shared conspiracy theories to create positive impressions and to appear stable. This interaction pattern is specifically revealing in the case of the new impression goal considered in Experiment 3 (i.e., appearing honest vs. dishonest). People who believed more strongly in conspiracy theories shared them to appear honest as a politician rather than dishonest, while people who had weaker conspiracy beliefs shared them to appear dishonest as a politician rather than honest.

Although the results from Experiment 3 might have important implications regarding the perceived efficacy of conspiracy theories for political impression-management, they also raise questions about the actual efficacy of conspiracy theories, and highlight that the majority of people who do not strongly believe in conspiracy theories might consider the latter as a normalized part of the political discourse. Regarding the actual efficacy of conspiracy theories for impression-management, the next set of experiments examined whether recipients of conspiracy theories do indeed form impressions close to those people thought they would in Experiments 1, 2 and 3. We continued in the domain of politics, where the communication of conspiracy theories appears to be arguably impactful.

4. Experiments 4a and 4b

Experiments 1, 2, and 3 revealed that people have distinct conceptions of the potential social consequences that exist when people share conspiracy theories. When we asked participants to create more favorable impressions, they (especially those with weaker conspiracy beliefs) reported that they and others would share conspiracy theories less. However, they reported that they would share conspiracy theories more to create alternative impressions (i.e., to appear unique and more radical). People therefore seem to believe that sharing conspiracy theories will have social consequences for individuals, and that not all of these consequences will necessarily be unfavorable. However, although this is valuable knowledge, people's implicit theories about the consequences of conspiracy theories may not reflect the reality–i.e., people may think that sharing conspiracy theories creates specific impressions of individuals, but does this actually happen? It is this question that we turned to in the next experiments.

Following on from Experiment 3, we focused again on the domain of politics where growing concern about populist leaders' use of conspiracy theories has raised questions about the political consequences of conspiracy theories (Bergmann, 2018; Castanho Silva et al., 2017). In particular, we addressed our research question in two different political contexts (the USA and the UK), which allowed us to examine whether the social consequences of sharing conspiracy theories in one context where conspiracy theories have arguably been a prominent feature of recent political discourse (the USA) also hold in a sample from a different national context (the UK).

Thus, in Experiments 4a and 4b, we asked participants from the USA and UK, respectively, to read a political speech that had ostensibly been made by a politician running for office. Half of the participants read a conspiracy-related statement and the other half read a statement refuting the conspiracy theories. Participants rated the candidate on a number of dimensions. These variables were like those we examined in Experiments 2 and 3. Specifically, we measured the extent to which the politician can be viewed as a political outsider (i.e., judgments of being radical and unique), and predictability (i.e., stability). Positivity (specifically positive leadership qualities) were examined by measuring perceived trustworthiness, charisma, benevolence, integrity and competence. As in Study 3, we also measured perceived honesty. We predicted that the pro- and anti-conspiracy candidates would be rated differently on each of these dimensions. Specifically, we predicted that the pro-conspiracy candidate would be rated as less honest, trustworthy, benevolent, have less integrity, be less competent and less predictable, than the anti-conspiracy candidate. However, we predicted that the pro-conspiracy candidate would be rated as more likely to effect change, be charismatic, and more likely to be a political outsider, compared to the anti-conspiracy candidate. We further expected that participants would express more support for the anti-conspiracy candidate compared to the pro-conspiracy candidate.

In an exploratory analysis, we also examined the extent to which people's impressions of candidates might be further influenced by their right-wing attitudes. Specifically, do people's initial political leanings determine the impressions they form of candidates who share (vs. refute) conspiracy theories? Previous research has shown that right-wing attitudes, such as authoritarianism, are associated with higher conspiracy beliefs (e.g., Abalakina-Paap et al., 1999; Bruder, Haffke, Neave, Nouripanah, & Imhoff, 2013; Green & Douglas, 2018; Wood & Gray, 2019). This could be explained by a heightened sensitivity toward threats (Duckitt & Sibley, 2009; Grzesiak-Feldman, 2015). Therefore, people with more right-wing attitudes might form more favorable impressions of a candidate who shares conspiracy theories, whereas people with less right-wing attitudes might be unaffected, or indeed favor a candidate who refutes conspiracy theories.

4.1. Method

4.1.1. Participants and design

4.1.1.1. Experiment 4a. Participants were recruited in November 2018 using Prolific. Eligibility for participation was restricted to Prolific workers from the USA who were at least 18 years of age and had an approval rate of at least 95% across prior assignments. The sample consisted of 248 participants (50.81% male, 47.58% female, 0.81% transgender, 0.81% rather not say; $M_{age} = 34.22$, $SD_{age} = 11.11$, range = 18–70 years). Sample size was determined based on resource constraints. Data were only analyzed once the experiment was complete. Sensitivity analyses revealed that this sample size was sufficient to detect effect sizes as small as $d = 0.41$ in an independent sample *t*-test with 90% statistical power and $\alpha = 0.05$ (two-tailed). Participants were paid a small fee for their time. The experiment was a two-group between-subjects design.

4.1.1.2. Experiment 4b. Participants were recruited in January 2019 using Prolific. Eligibility for participation was restricted to Prolific workers from the UK who were at least 18 years of age and had an approval rate of at least 95% across prior assignments. The sample consisted of 250 participants (68.40% female, 31.20% male, 0.04% rather not say; $M_{age} = 38.33$, $SD_{age} = 12.55$, range = 18–77 years). Sample size was determined based on resource constraints. Data were only analyzed once the experiment was complete. Sensitivity analyses revealed that this sample size was sufficient to detect effect sizes as small as $d = 0.41$ in an independent sample t-test with 90% statistical power and $\alpha = 0.05$ (two-tailed). Participants were paid a small fee for their time. As in Experiment 4b, this was a two-group between-subjects experimental design.

4.1.2. Materials and procedure

Experiments 4a and 4b followed the same procedure and used the same materials. After providing informed consent, participants were randomly allocated to one of two experimental conditions: pro-conspiracy and anti-conspiracy.[4] In both conditions, participants were asked to read a brief statement from a politician's speech, which was ostensibly made during their political campaign when running for office. In the pro-conspiracy condition, the politician's statement argued for the "deep state" conspiracy theory (e.g., "…A group of bureaucrats, intelligence agency personnel and other government entities controls all national policy behind the scenes…"). In the anti-conspiracy condition, the politician's statement argued against the "deep state" conspiracy theory (e.g., "…There is no evidence that a group of bureaucrats, intelligence agency personnel and other government entities controls all national policy behind the scenes…").

All participants were then asked to indicate their impression and support for the politician across 10 different measures, which were presented in random order: three items were created to measure support for politician (e.g., "I would vote for this politician"); effect change (e.g., "This politician will change things"); honesty (e.g., "This politician is honest"); trustworthiness (e.g., "This politician is trustworthy"); charisma (e.g., "This politician is charismatic"); and outsider (e.g., "This politician is a rogue"). Participants also completed an adapted Trust in Leaders scale (Adams & Sartori, 2005), which consisted of four five-item subfactors: benevolence (e.g., "I have confidence in the motivations of this politician"); integrity (e.g., "I believe this politician is fair"); predictable (e.g., "I know how this politician is going to act"); and competence (e.g., "This politician will perform their job well"). Higher scores indicated higher agreement with these measures (1 = completely disagree, 7 = completely agree). Statistics for the internal consistency of the different scales are presented in Table 3.

Participants then completed measures about themselves. First, participants indicated their right-wing attitudes by completing a shortened version of the Authoritarianism-Conservatism-Traditionalism (ACT) scale to measure right-wing attitudes (Duckitt, Bizumic, Krauss, & Heled, 2010). There were 12 statements (e.g., "What our country needs most is discipline, with everyone following our leaders in unity", 1 = strongly disagree, 7 = strongly agree). Finally, participants then answered some socio-demographic questions: age, gender (male, female, transgender, rather not say), and political orientation (1 = very left-wing, 7 = very right-wing), before being debriefed, thanked, and paid.

4.2. Results and discussion

4.2.1. Main analyses

For Experiments 4a and 4b, the means, standard deviations, zero-order correlations, and degrees of freedom for the main variables of interest can be found in Table 3. In both experiments we used a multivariate ANOVA model to test for mean differences in impressions of the politician across two experimental conditions (−1 = anti-conspiracy, 1 = pro-conspiracy). In both experiments, there was a significant difference in impressions found between the two conditions (Experiment 4a, $F(10, 237) = 16.111$, $p < .001$; Pillai's trace = 0.405, $\eta_p^2 = 0.94$; Experiment 4b, $F(10, 238) = 12.88$, $p < .001$; Pillai's trace = 0.351, $\eta_p^2 = 0.351$).

In Experiment 4a, compared to the anti-conspiracy condition, participants in the pro- conspiracy condition rated the politician as being less trustworthy ($d = 0.37$), less benevolent ($d = 0.29$), less predictable ($d = 0.58$), less competent ($d = 0.42$), more able to effect change ($d = 0.29$), and more of an outsider ($d = 1.28$). Participants also reported less support for the pro-conspiracy politician compared to the anti-conspiracy politician ($d = 0.26$). No significant differences between the two conditions were found for impressions of honesty, charisma, and integrity (see Table 4). In Experiment 4b, compared to the anti-conspiracy condition, participants rated the pro-conspiracy politician as being less trustworthy ($d = 0.35$), less predictable ($d = 0.55$), less competent ($d = 0.41$), and more of an outsider ($d = 1.34$). No significant differences between conditions were found for impressions of ability to effect change, honesty, charisma, benevolence, integrity, or support for the politician (see Table 4).

4.2.2. Exploring right-wing attitudes as a moderator

We then used PROCESS Model 1 (Hayes, 2022) to test whether the impressions of the anti-conspiracy vs. pro-conspiracy politicians were moderated by right-wing attitudes.[5] For Experiment 4a, significant interactions between the experimental condition and right-wing attitudes were found for impressions of ability to effect change, honesty, trustworthiness, benevolence, integrity, predictability, competence, and support for the politician, but not for impressions of charisma or being an outsider (see Table 5 for conditional effects and the Supplementary Materials for an illustration of simple slopes of significant interactions). Specifically, people with weaker and moderate right-wing attitudes showed lower support for the pro-conspiracy (vs. anti-conspiracy) politician, whereas people with stronger right-wing attitudes showed no difference in support for either politician. A similar pattern of conditional effects was found for impressions of trustworthiness, benevolence, predictability, and competence. People with weaker right-wing attitudes reported lower, and people with stronger right-wing attitudes reported higher, impressions of honesty and integrity for the pro-conspiracy (vs. anti-conspiracy politician), whereas no differences were found for people with moderate right-wing attitudes. Finally, people with moderate and stronger right-wing attitudes reported higher impressions of ability to effect change for the pro-conspiracy (vs. anti-conspiracy) politician, whereas no differences were found for people with weaker right-wing attitudes.

For Experiment 4b, no significant interactions between the experimental conditions and right-wing attitudes were found for support for the politician, impressions of ability to effect change, honesty, trustworthiness, charisma, benevolence, integrity, predictability, competence, or being an outsider (see Table 5 for conditional effects).

In Experiments 4a and 4b participants rated a fictitious politician differently depending on whether they shared (vs. refuted) conspiracy theories. Not all dependent measures yielded significant differences, but in both experiments, core impressions (i.e., trustworthiness, predictability, competence, and appearing as a political outsider) were affected by the communication of conspiracy theories. Note that in both Experiments 4a and 4b, we observed the largest effects for ratings of being a political outsider. This suggests that when the voting public want an outsider, a politician using conspiracy theories might be especially attractive. Our findings therefore provide some support for the hypotheses. Furthermore, in Experiment 4a, the effect of sharing conspiracy theories on the impressions the participants formed of the fictitious politician was often moderated by participants' own levels of right-wing attitudes. With the exception of ratings as a political outsider, attitudes toward the anti-conspiracy candidate (vs. the conspiracy candidate) tended to be more favorable at lower levels of right-wing attitudes. However, among people higher in right-wing attitudes, attitudes toward the pro-conspiracy candidate (vs. the anti-conspiracy candidate) tended

[4] The current design is conceptually similar to Jolley and Douglas (2014a, 2014b). In these experiments, no difference in the main dependent variables (i.e., intentions to engage in politics) were found between the anti-conspiracy condition and control, but a difference was found between these conditions and a pro-conspiracy condition. Guided by these findings, we opted to only include anti- and pro-conspiracy conditions in the current studies.

[5] Independent-samples *t*-tests showed that the experimental manipulations did not significantly affect right-wing attitudes in Experiment 4a, $t(246) = 0.401$, $p = .689$, or Experiment 4b, $t(247) = -0.830$, $p = .407$.

Table 3
Means, standard deviations, Cronbach's alpha, and zero-order correlations (Experiments 4a [US participants] and 4b [UK participants]).

Measure		1	2	3	4	5	6	7	8	9	10	11
1. Support		–	0.61 (< 0.001)	0.79 (< 0.001)	0.84 (< 0.001)	0.65 (< 0.001)	0.10 (0.114)	0.81 (< 0.001)	0.83 (< 0.001)	0.39 (< 0.001)	0.81 (< 0.001)	0.02 (0.777)
2. Effect Change		0.69 (< 0.001)	–	0.61 (< 0.001)	0.63 (< 0.001)	0.60 (< 0.001)	0.33 (< 0.001)	0.67 (< 0.001)	0.71 (< 0.001)	0.23 (< 0.001)	0.60 (< 0.001)	0.03 (0.695)
3. Honest		0.76 (< 0.001)	0.61 (< 0.001)	–	0.86 (< 0.001)	0.55 (< 0.001)	0.21 (< 0.001)	0.79 (< 0.001)	0.82 (< 0.001)	0.31 (< 0.001)	0.74 (< 0.001)	0.01 (0.971)
4. Trustworthy		0.79 (< 0.001)	0.64 (< 0.001)	0.80 (< 0.001)	–	0.60 (< 0.001)	0.08 (0.200)	0.86 (< 0.001)	0.89 (< 0.001)	0.45 (< 0.001)	0.83 (< 0.001)	0.02 (0.785)
5. Charismatic		0.57 (< 0.001)	0.65 (< 0.001)	0.50 (< 0.001)	0.51 (< 0.001)	–	0.21 (< 0.001)	0.60 (< 0.001)	0.63 (< 0.001)	0.33 (< 0.001)	0.62 (< 0.001)	−0.04 (0.582)
6. Outsider		−0.05 (0.396)	0.14 (0.032)	−0.08 (0.191)	−0.16 (0.011)	0.20 (0.001)	–	0.12 (0.053)	0.15 (0.019)	−0.11 (0.073)	0.04 (0.563)	0.09 (0.170)
7. Benevolent		0.77 (< 0.001)	0.66 (< 0.001)	0.77 (< 0.001)	0.79 (< 0.001)	0.54 (< 0.001)	−0.03 (0.624)	–	0.93 (< 0.001)	0.47 (< 0.001)	0.84 (< 0.001)	0.04 (0.582)
8. Integrous		0.77 (< 0.001)	0.66 (< 0.001)	0.79 (< 0.001)	0.78 (< 0.001)	0.55 (< 0.001)	−0.07 (0.302)	0.89 (< 0.001)	–	0.46 (< 0.001)	0.86 (< 0.001)	0.02 (0.805)
9. Predictable		0.31 (< 0.001)	0.26 (< 0.001)	0.32 (< 0.001)	0.45 (< 0.001)	0.22 (< 0.001)	−0.28 (< 0.001)	0.43 (< 0.001)	0.47 (< 0.001)	–	0.51 (< 0.001)	−0.01 (0.984)
10. Competent		0.77 (< 0.001)	0.67 (< 0.001)	0.71 (< 0.001)	0.77 (< 0.001)	0.57 (< 0.001)	−0.21 (< 0.001)	0.77 (< 0.001)	0.83 (< 0.001)	0.56 (< 0.001)	–	−0.01 (0.980)
11. Right-wing attitudes		0.09 (0.148)	0.13 (0.043)	0.08 (0.207)	0.09 (0.159)	0.21 (< 0.001)	−0.06 (0.315)	0.12 (0.057)	0.18 (0.004)	0.09 (0.153)	0.20 (0.001)	–
Experiment 4a	*M*	3.20	3.26	3.60	3.30	3.41	3.59	3.12	3.22	3.36	3.47	3.14
	SD	1.58	1.52	1.60	1.56	1.36	1.69	1.48	1.41	1.38	1.43	1.20
	α	0.97	0.93	0.94	0.95	0.84	0.91	0.94	0.93	0.92	0.93	0.90
Experiment 4b	*M*	3.22	3.38	3.61	3.36	3.37	3.86	3.11	3.27	3.21	3.48	3.63
	SD	1.52	1.38	1.41	1.36	1.27	1.57	1.31	1.27	1.34	1.25	1.10
	α	0.97	0.94	0.96	0.97	0.86	0.90	0.97	0.95	0.92	0.95	0.86

Note: Experiment 4a zero-order correlations ($df = 246$) are displayed on the upper diagonal, while those for Experiment 4b ($df = 248$) are displayed on the lower diagonal. Exact *p*-values can be found below each correlation in the parenthesis.

Table 4
Univariate effects of experimental manipulation on different types of impressions (Experiments 4a [US participants] and 4b [UK participants]).

		Anti-Conspiracy	Pro-Conspiracy				
		M (*SD*)	*M* (*SD*)	*F*	*p*	η_p^2	95% CI
Support	Exp. 4a	3.40 (1.41)	2.99 (1.72)	4.34	0.038	0.02	[0.00, 0.06]
	Exp. 4b	3.40 (1.30)	3.03 (1.68)	3.70	0.056	0.02	[0.00, 0.06]
Effect Change	Exp. 4a	3.04 (1.33)	3.48 (1.66)	5.25	0.023	0.02	[0.00, 0.07]
	Exp. 4b	3.30 (1.32)	3.46 (1.43)	0.86	0.356	0.01	[0.00, 0.03]
Honest	Exp. 4a	3.66 (1.47)	3.54 (1.74)	0.36	0.552	0.00	[0.00, 0.03]
	Exp. 4b	3.70 (1.35)	3.52 (1.46)	1.02	0.315	0.01	[0.00, 0.03]
Trustworthy	Exp. 4a	3.58 (1.50)	3.01 (1.57)	8.54	0.004	0.03	[0.00, 0.09]
	Exp. 4b	3.59 (1.30)	3.13 (1.38)	7.44	0.007	0.03	[0.00, 0.08]
Charismatic	Exp. 4a	3.41 (1.22)	3.40 (1.50)	0.01	0.955	0.01	[0.00, 0.00]
	Exp. 4b	3.21 (1.16)	3.51 (1.36)	3.67	0.056	0.02	[0.00, 0.06]
Outsider	Exp. 4a	2.70 (1.32)	4.53 (1.53)	101.51	< 0.001	0.29	[0.20, 0.38]
	Exp. 4b	2.97 (1.30)	4.73 (1.32)	111.94	< 0.001	0.31	[0.22, 0.39]
Benevolent	Exp. 4a	3.33 (1.43)	2.91 (1.51)	5.09	0.025	0.02	[0.00, 0.07]
	Exp. 4b	3.19 (1.28)	3.02 (1.32)	1.06	0.305	0.01	[0.00, 0.03]
Integrous	Exp. 4a	3.33 (1.37)	3.10 (1.45)	1.60	0.207	0.01	[0.00, 0.04]
	Exp. 4b	3.37 (1.22)	3.15 (1.32)	1.86	0.174	0.01	[0.00, 0.04]
Predictable	Exp. 4a	3.74 (1.33)	2.98 (1.31)	20.40	< 0.001	0.08	[0.02, 0.15]
	Exp. 4b	3.57 (1.28)	2.85 (1.31)	19.14	< 0.001	0.07	[0.02, 0.14]
Competent	Exp. 4a	3.76 (1.33)	3.17 (1.48)	10.81	< 0.001	0.04	[0.01, 0.10]
	Exp. 4b	3.74 (1.12)	3.23 (1.32)	10.57	0.001	0.04	[0.01, 0.10]

Note: For Experiment 4a, $df = 1, 246$ (anti-conspiracy, $n = 127$; pro-conspiracy, $n = 121$). For Experiment 4b, $df = 1, 247$ (anti-conspiracy, $n = 122$; pro-conspiracy, $n = 127$).

to be more favorable.

There were several differences in findings between Experiments 4a and 4b and it is worth commenting on them here. Several of the effects that were significant in Experiment 4a were not significant in Experiment 4b, and right-wing attitudes did not moderate any of the differences in Experiment 4b. We can only speculate at this point why these findings were different. The most obvious point of difference between the two experiments is that they were carried out in different countries (USA vs. UK) and therefore in political contexts that differ in the visibility of conspiracy theories (Walter & Drochon, 2022). To follow up Experiments 4a and 4b, we therefore felt that it was important to replicate the findings, and we chose to do so in the US context. In doing so, we could check that the findings in this context were robust over time. This was the aim of Experiment 5.

5. Experiment 5

Experiment 5 was a pre-registered replication of Experiments 4a and 4b. The procedure was almost identical and the pre-registered hypotheses were the same with respect to differences between the pro- and anti-

Table 5
Conditional univariate effects of experimental condition at different levels of Right-Wing Attitudes (Experiments 4a [US participants] and 4b [UK participants]).

Conditional effects (X on Y) at ±1 SD of W		Experiment 4a: Right-Wing Attitudes as Moderator				Experiment 4b: Right-Wing Attitudes as Moderator			
		Interaction	-1SD	Mean	+1SD	Interaction	-1SD	Mean	+1SD
Support	*b* (*SE*)	**0.35 (0.08)**	−0.63 (0.14)	−0.21 (0.10)	0.22 (0.14)	0.02 (0.09)	−0.22 (0.14)	−0.19 (0.10)	−0.17 (0.14)
	95% CI	**[0.19, 0.51]**	[−0.90, −0.36]	[−0.40, −0.02]	[−0.05, 0.49]	[−0.15, 0.19]	[−0.48, 0.05]	[−0.38, −0.01]	[−0.44, 0.10]
Effect Change	*b* (*SE*)	**0.25 (0.08)**	−0.08 (0.13)	0.22 (0.09)	0.52 (0.13)	−0.04 (0.08)	0.11 (0.12)	0.07 (0.09)	0.03 (0.12)
	95% CI	**[0.09, 0.40]**	[−0.34, 0.19]	[0.04, 0.41]	[0.25, 0.78]	[−0.19, 0.12]	[−0.13, 0.35]	[−0.10, 0.24]	[−0.21, 0.28]
Honest	*b* (*SE*)	**0.37 (0.08)**	−0.50 (0.14)	−0.06 (0.10)	0.38 (0.14)	0.03 (0.08)	−0.13 (0.13)	−0.10 (0.09)	−0.06 (0.13)
	95% CI	**[0.20, 0.53]**	[−0.77, −0.22]	[−0.25, 0.13]	[0.10, 0.66]	[−0.13, 0.19]	[−0.38, 0.12]	[−0.27, 0.08]	[−0.31, 0.19]
Trustworthy	*b* (*SE*)	**0.34 (0.08)**	−0.69 (0.13)	−0.28 (0.09)	0.12 (0.13)	−0.05 (0.08)	−0.19 (0.12)	−0.24 (0.08)	−0.29 (0.12)
	95% CI	**[0.18, 0.50]**	[−0.95, −0.43]	[−0.47, −0.10]	[−0.14, 0.39]	[−0.20, 0.11]	[−0.43, 0.05]	[−0.41, −0.07]	[−0.53, −0.05]
Charismatic	*b* (*SE*)	0.14 (0.07)	−0.18 (0.12)	−0.01 (0.09)	0.16 (0.12)	−0.08 (0.07)	0.23 (0.11)	0.14 (0.08)	0.06 (0.11)
	95% CI	[−0.01, 0.29]	[−0.42, 0.06]	[−0.18, 0.16]	[−0.08, 0.41]	[−0.22, 0.07]	[0.01, 0.44]	[−0.02, 0.29]	[−0.16, 0.27]
Outsider	*b* (*SE*)	−0.13 (0.08)	1.08 (0.13)	0.92 (0.09)	0.76 (0.13)	−0.14 (0.08)	1.04 (0.12)	0.89 (0.08)	0.74 (0.12)
	95% CI	[−0.28, 0.01]	[0.83, 1.33]	[0.74, 1.09]	[0.51, 1.01]	[−0.29, 0.01]	[0.81, 1.27]	[0.72, 1.05]	[0.51, 0.97]
Benevolent	*b* (*SE*)	**0.34 (0.08)**	−0.61 (0.13)	−0.21 (0.09)	0.20 (0.13)	−0.09 (0.08)	0.01 (0.12)	−0.09 (0.08)	−0.20 (0.12)
	95% CI	**[0.19, 0.49]**	[−0.86, −0.36]	[−0.38, −0.03]	[−0.05, 0.45]	[−0.24, 0.05]	[−0.22, 0.24]	[−0.26, 0.07]	[−0.43, 0.03]
Integrous	*b* (*SE*)	**0.36 (0.07)**	−0.54 (0.12)	−0.11 (0.09)	0.32 (0.12)	−0.04 (0.07)	−0.07 (0.11)	−0.12 (0.08)	−0.17 (0.11)
	95% CI	**[0.22, 0.50]**	[−0.78, −0.30]	[−0.28, 0.06]	[0.08, 0.55]	[−0.19, 0.10]	[−0.29, 0.15]	[−0.28, 0.03]	[−0.39, 0.05]
Predictable	*b* (*SE*)	**0.20 (0.07)**	−0.61 (0.12)	−0.38 (0.08)	−0.15 (0.12)	−0.01 (0.08)	−0.36 (0.11)	−0.37 (0.08)	−0.37 (0.12)
	95% CI	**[0.06, 0.33]**	[−0.85, −0.38]	[−0.54, −0.22]	[−0.38, 0.09]	[−0.15, 0.14]	[−0.59, −0.13]	[−0.53, −0.21]	[−0.60, −0.15]
Competent	*b* (*SE*)	**0.35 (0.07)**	−0.71 (0.12)	−0.29 (0.09)	0.13 (0.12)	0.08 (0.07)	−0.35 (0.11)	−0.27 (0.08)	−0.18 (0.11)
	95% CI	**[0.21, 0.49]**	[−0.95, −0.47]	[−0.46, −0.12]	[−0.11, 0.37]	[−0.06, 0.22]	[−0.56, −0.14]	[−0.42, −0.12]	[−0.39, 0.03]

Note: Significant interactions between experimental condition and moderation variables are highlighted in bold.

conspiracy candidates. We also predicted the moderated effects by right-wing attitudes that were exploratory in the previous studies (https://osf.io/b87fd/?view_only=0e77cc512c8e485fb229409df99761a5). We expanded on the experiment to test for other potential moderators, in some exploratory analyses. First, we examined if the differences in evaluations of a pro- and anti-conspiracy candidate could be moderated by participants own belief in conspiracy theories. Experiments 2 and 3 showed that people's own conspiracy beliefs moderated their sharing of conspiracy theories in order to create specific impressions of themselves. Specifically, the less participants believed in conspiracy theories, the more they expressed these theories to portray themselves more negatively (vs. positively), and as a radical (vs. mainstream), unstable (vs. stable), and unique (vs. average) person. We therefore explored whether people with weaker conspiracy beliefs were more favorable toward the anti- vs. pro-conspiracy candidate, whereas people with stronger conspiracy beliefs were more favorable toward a pro- vs. anti-conspiracy candidate.[6]

5.1. Method

5.1.1. Participants and design

Participants were recruited in February 2022 using Prolific. Eligibility for participation was restricted to Prolific workers from the USA who were at least 18 years of age and had an approval rate of at least 95% across prior assignments. As specified in the pre-registration, we followed a sequential approach to data collection (Lakens, 2014). We aimed to collect a total of 621 responses, which would ensure 90% statistical power to detect an effect size of $d = 0.26$ (i.e., the smallest effect on support for the politician observed in Experiments 4a), assuming $\alpha = 0.05$. We pre-registered three equally-spaced interim analyses, establishing the following adjusted α boundaries (i.e., 0.0167, 0.0218 and 0.0278, respectively). After collecting the second batch (i.e., $N = 417$), we found the reference effect on support for the politician to be significant at the pre-specified alpha level and therefore decided to stop data collection. Thus, the sample consisted of 417 participants (50.81% male, 47.58% female, 0.81% transgender, 0.81% rather not say; $M_{age} = 34.80$, $SD_{age} = 13.47$, range = 18–79 years). Sensitivity analyses revealed that this sample size was sufficient to detect effect sizes as small as $d = 0.32$ in an independent sample *t*-test with 90% statistical power and $\alpha = 0.05$ (two-sided). Participants were paid a small fee for their time. The experiment was a two-group between-subjects design.

5.1.2. Materials and procedure

After providing informed consent, participants were randomly allocated to either the pro- or anti-conspiracy condition as in Experiments 4a and 4b. The remainder of the procedure remained the same, but we also measured belief in conspiracy theories using the single-item scale used in Experiment 1 (Lantian et al., 2016). Statistics for the internal consistency of the different scales are presented in Table 6. After providing demographic details, participants were then debriefed, thanked, and paid.

5.2. Results and discussion

5.2.1. Main analyses

The means, standard deviations, reliability statistics, and zero-order correlations for the main variables of interest can be found in Table 6. We used a multivariate ANOVA model to test for mean differences in impressions of the politician across two experimental conditions (−1 = anti-conspiracy, 1 = pro-conspiracy). There was a significant difference in impressions found between these two conditions, $F(11, 405) = 14.132$, $p < .001$; Pillai's trace = 0.25, $\eta_p^2 = 0.28$. Compared to the anti-conspiracy condition, participants in the pro- conspiracy condition rated the politician as being less honest, less trustworthy, less integrous, less benevolent, less predictable, less competent, less ethical, and more of an outsider. Participants also reported lower support for the pro-conspiracy politician compared to the anti-conspiracy politician. No significant differences between the two conditions were found for impressions of charisma or ability to effect change. After correcting the observed *p*-values for the sequential approach to data collection (Lakens, 2014), differences in trustworthiness, perceptions as an outsider, predictability, and competence remained statistically significant, whereas differences in support, honesty, benevolence, integrity and ethical leadership

[6] We explored other potential moderators (see the Supplementary Materials for these analyses). With three items each (1 = completely disagree, 7 = completely agree), we measured participants' feelings of political uncertainty (Jolley & Douglas, 2014a, e.g., "I feel uncertain about the future of US politics"; α = 0.79), and political powerlessness (Neal & Groat, 1974; e.g., "It's foolish to vote as it won't make a difference; α = 0.67). We then asked participants the extent to which they trust various institutions (Goertzel, 1994, i.e., the police, neighbors, relatives; 1 = not at all, 7 = very much; α = 0.66).

Table 6
Means, standard deviations, Cronbach's alpha, and zero-order correlations (Experiment 5).

Measure		1	2	3	4	5	6	7	8	9	10	11	12	13
1. Support		–												
2. Effect Change		0.66 (< 0.001)	–											
3. Honest		0.84 (< 0.001)	0.61 (< 0.001)	–										
4. Trustworthy		0.87 (< 0.001)	0.64 (< 0.001)	0.90 (< 0.001)	–									
5. Charismatic		0.63 (< 0.001)	0.58 (< 0.001)	0.57 (< 0.001)	0.59 (< 0.001)	–								
6. Outsider		0.14 (0.004)	0.38 (< 0.001)	0.19 (< 0.001)	0.13 (0.010)	0.26 (< 0.001)	–							
7. Benevolent		0.86 (< 0.001)	0.69 (< 0.001)	0.85 (< 0.001)	0.88 (< 0.001)	0.63 (< 0.001)	0.18 (< 0.001)	–						
8. Integrous		0.86 (< 0.001)	0.68 (< 0.001)	0.88 (< 0.001)	0.90 (< 0.001)	0.60 (< 0.001)	0.18 (< 0.001)	0.94 (< 0.001)	–					
9. Predictable		0.57 (< 0.001)	0.38 (< 0.001)	0.54 (< 0.001)	0.59 (< 0.001)	0.39 (< 0.001)	−0.07 (0.136)	0.61 (< 0.001)	0.65 (< 0.001)	–				
10. Competent		0.86 (< 0.001)	0.63 (< 0.001)	0.80 (< 0.001)	0.86 (< 0.001)	0.60 (< 0.001)	0.06 (0.221)	0.87 (< 0.001)	0.89 (< 0.001)	0.72 (< 0.001)	–			
11. Ethical		0.83 (< 0.001)	0.63 (< 0.001)	0.85 (< 0.001)	0.88 (< 0.001)	0.58 (< 0.001)	0.15 (0.003)	0.88 (< 0.001)	0.89 < 0.001)	0.55 (< 0.001)	0.83 (< 0.001)	–		
12. Right-Wing Attitudes		0.23 (< 0.001)	0.29 (< 0.001)	0.18 (< 0.001)	0.20 (< 0.001)	0.19 (< 0.001)	0.12 (0.011)	0.24 (< 0.001)	0.24 (< 0.001)	0.20 (< 0.001)	0.25 (< 0.001)	0.16 (< 0.001)	–	
13. Conspiracy Belief		0.06 (0.247)	0.04 (0.444)	0.06 (0.225)	0.02 (0.744)	0.11 (0.025)	0.04 (0.460)	0.01 (0.860)	−0.01 (0.947)	0.02 (0.673)	0.04 (0.442)	0.01 (0.847)	0.10 (0.041)	–
Experiment 5	*M*	3.22	3.45	3.63	3.38	3.36	3.78	3.17	3.35	3.53	3.56	3.62	3.25	5.52
	SD	1.74	1.55	1.64	1.66	1.35	1.51	1.59	1.51	1.36	1.43	1.44	1.16	2.37
	α	0.97	0.94	0.96	0.97	0.86	0.90	0.97	0.95	0.92	0.95	0.86	0.90	–

Note: $df = 415$. Exact p-values can be found below each correlation in the parenthesis.

Table 7
Univariate effects of experimental manipulation on different types of impressions (Experiment 5).

	Anti-Conspiracy (n = 212)	Pro-Conspiracy (n = 205)						
	M (*SD*)	*M* (*SD*)	*F*(1, 415)	*p*	Adj. *p*	*d*	95% CI	Adj. *d*
Support	3.52 (1.59)	2.92 (1.82)	12.66	< 0.001	0.059	0.35	[0.14, 0.53]	0.26
Effect Change	3.40 (1.44)	3.49 (1.65)	0.32	0.572	0.945	0.06	[−0.21, 0.17]	–
Honest	3.81 (1.54)	3.45 (1.71)	5.36	0.021	0.573	0.22	[0.05, 0.43]	0.13
Trustworthy	3.70 (1.61)	3.07 (1.64)	15.71	< 0.001	0.019	0.39	[0.21, 0.59]	0.35
Charismatic	3.38 (1.22)	3.34 (1.47)	0.06	0.813	0.989	0.03	[−0.16, 0.22]	–
Outsider	3.15 (1.29)	4.39 (1.46)	83.61	< 0.001	< 0.001	−0.90	[−1.03, −0.63]	0.69
Benevolent	3.35 (1.51)	3.00 (1.65)	5.19	0.023	0.360	0.22	[0.06, 0.45]	0.18
Integrous	3.54 (1.44)	3.17 (1.55)	6.22	0.013	0.377	0.25	[0.07, 0.46]	0.18
Predictable	3.91 (1.23)	3.16 (1.37)	34.00	< 0.001	0.001	0.58	[0.38, 0.77]	0.51
Competent	3.91 (1.35)	3.23 (1.44)	25.06	< 0.001	0.009	0.49	[0.30, 0.69]	0.41
Ethical	3.86 (1.41)	3.39 (1.43)	11.22	< 0.001	0.200	0.33	[0.15, 0.53]	0.23

Note: Adj. *p* and Adj. *d* respectively refer to *p*-values and Cohen's *d* corrected for the two preregistered interim analyses performed during data collection, using the R package *GroupSeq* (version 1.4.0) and following the instructions by Lakens (2014). Two of the adjusted Cohen's *d* could not be calculated due to convergence problems in the estimation of the drift parameter.

Table 8
Conditional univariate effects of experimental condition at different levels of right-wing attitudes and conspiracy beliefs (Experiment 5).

Conditional effects (X on Y) at ±1 SD of W		Right-Wing Attitudes as Moderator				Conspiracy Beliefs as Moderator			
		Interaction	-1SD	Mean	+1SD	Interaction	-1SD	Mean	+1SD
Support	*b* (*SE*)	**0.22 (0.07)**	−0.54 (0.11)	−0.29 (0.08)	−0.03 (0.11)	**0.21 (0.03)**	−0.80 (0.11)	−0.31 (0.08)	0.19 (0.11)
	95% CI	**[0.08, 0.35]**	[−0.76, −0.31]	[−0.44, −0.13]	[−0.26, 0.19]	**[0.14, 0.28]**	[−1.03, −0.58]	[−0.47, −0.15]	[−0.04, 0.41]
Effect Change	*b* (*SE*)	0.08 (0.06)	−0.04 (0.10)	0.06 (0.07)	0.16 (0.10)	**0.15 (0.03)**	−0.32 (0.10)	0.04 (0.07)	0.40 (0.11)
	95% CI	[−0.03, 0.21]	[−0.24, 0.16]	[−0.08, 0.20]	[−0.05, 0.36]	**[0.09, 0.21]**	[−0.53, −0.11]	[−0.11, 0.18]	[0.19, 0.61]
Honest	*b* (*SE*)	**0.20 (0.07)**	−0.41 (0.11)	−0.18 (0.08)	0.06 (0.11)	**0.23 (0.03)**	−0.73 (0.11)	−0.19 (0.08)	0.35 (0.11)
	95% CI	**[0.07, 0.33]**	[−0.62, −0.19]	[−0.33, −0.02]	[−0.16, 0.27]	**[0.17, 0.29]**	[−0.94–0.52]	[−0.34, −0.04]	[0.14, 0.56]
Trustworthy	*b* (*SE*)	**0.23 (0.07)**	−0.57 (0.11)	−0.30 (0.08)	−0.04 (0.11)	**0.22 (0.03)**	−0.85 (0.11)	−0.32 (0.08)	0.21 (0.11)
	95% CI	**[0.10, 0.36]**	[−0.79, −0.36]	[−0.46, −0.15]	[−0.25, 0.18]	**[0.16, 0.29]**	[−1.06, −0.64]	[−0.47, −0.17]	[−0.01, 0.42]
Charismatic	*b* (*SE*)	**0.12 (0.06)**	−0.14 (0.09)	−0.01 (0.06)	0.13 (0.09)	**−0.08 (0.03)**	−0.23 (0.09)	−0.03 (0.07)	0.17 (0.09)
	95% CI	**[0.01, 0.23]**	[−0.32, 0.04]	[−0.13, 0.12]	[−0.05, 0.31]	**[0.03, 0.14]**	[−0.41, −0.04]	[−0.16, 0.10]	[−0.01, 0.35]
Outsider	*b* (*SE*)	**−0.12 (0.06)**	0.77 (0.09)	0.63 (0.07)	0.49 (0.09)	−0.02 (0.03)	0.66 (0.10)	0.62 (0.07)	0.57 (0.10)
	95% CI	**[−0.23, −0.01]**	[0.58, 0.95]	[0.49, 0.76]	[0.30, 0.67]	[−0.08, 0.04]	[0.47, 0.85]	[0.49, 0.75]	[0.39, 0.76]
Benevolent	*b* (*SE*)	**0.23 (0.06)**	−0.44 (0.11)	−0.16 (0.07)	0.11 (0.11)	**0.20 (0.03)**	−0.64 (0.11)	−0.18 (0.07)	0.29 (0.11)
	95% CI	**[0.11, 0.36]**	[−0.64, −0.23]	[−0.31, −0.02]	[−0.10, 0.31]	**[0.14, 0.26]**	[−0.85, −0.44]	[−0.32, −0.03]	[0.08, 0.50]
Integrous	*b* (*SE*)	**0.22 (0.06)**	−0.43 (0.10)	−0.17 (0.07)	0.09 (0.10)	**0.19 (0.03)**	−0.64 (0.10)	−0.18 (0.07)	0.28 (0.10)
	95% CI	**[0.10, 0.34]**	[−0.62, −0.23]	[−0.31, −0.03]	[−0.11, 0.28]	**[0.14, 0.25]**	[−0.84, −0.45]	[−0.32, −0.04]	[0.08, 0.47]
Predictable	*b* (*SE*)	**0.15 (0.05)**	−0.54 (0.09)	−0.36 (0.06)	−0.19 (0.09)	**0.09 (0.03)**	−0.60 (0.09)	−0.38 (0.06)	−0.16 (0.09)
	95% CI	**[0.04, 0.25]**	[−0.71, −0.36]	[−0.49, −0.24]	[−0.37, −0.02]	**[0.04, 0.15]**	[−0.77, −0.42]	[−0.50, −0.25]	[−0.34, 0.02]
Competent	*b* (*SE*)	**0.19 (0.06)**	−0.55 (0.09)	−0.33 (0.07)	−0.11 (0.09)	**0.17 (0.03)**	−0.74 (0.09)	−0.39 (0.07)	0.04 (0.09)
	95% CI	**[0.08, 0.30]**	[−0.74, −0.37]	[−0.46, −0.20]	[−0.29, 0.08]	**[0.11, 0.22]**	[−0.92, −0.56]	[−0.48, −0.22]	[−0.14, 0.23]
Ethical	*b* (*SE*)	**0.15 (0.06)**	−0.40 (0.09)	−0.23 (0.07)	−0.05 (0.10)	**0.16 (0.03)**	−0.61 (0.10)	−0.23 (0.07)	0.14 (0.10)
	95% CI	**[0.04, 0.27]**	[−0.59, −0.21]	[−0.36, −0.09]	[−0.24, 0.14]	**[0.10, 0.21]**	[−0.79, −0.42]	[−0.37, −0.10]	[−0.05, 0.32]

Note: Significant interactions between condition and moderation variables are highlighted in bold.

became non-significant (see Table 7 for observed and adjusted *p*-values and effect sizes).

We then used PROCESS Model 1 (Hayes, 2022) to test whether the impressions of the anti-conspiracy vs. pro-conspiracy politicians were moderated by right-wing attitudes and participants' own belief in conspiracy theories.[7]

5.2.2. *Right-wing attitudes as moderator*

Significant interactions between the experimental condition and right-wing attitudes were found for impressions of honesty, trustworthiness, charisma, benevolence, integrity, predictability, competence, ethics, being an outsider, and support for the politician, but not for impressions of ability to effect change (see Table 8 for interaction statistics and conditional effects and Supplementary Materials for an illustration of simple slopes of significant interactions). Specifically, people with weaker and moderate right-wing attitudes showed lower support for the pro-conspiracy (vs. anti-conspiracy) politician, whereas people with stronger right-wing attitudes showed no difference in support for either politician. A similar pattern of conditional effects was found for impressions of honesty, trustworthiness, charisma, benevolence, integrity, competence, and ethical leadership. Finally, all levels of right-wing attitudes predicted higher impressions of the pro-conspiracy (vs. anti-conspiracy) politician as being a political outsider, but this effect was weakened at higher levels of right-wing attitudes. Conversely, all levels of right-wing attitudes showed lower impressions of the pro-conspiracy (vs. anti-conspiracy) politician as being predictable, but this effect was weakened at higher levels of right-wing attitudes.

5.2.3. *Conspiracy beliefs as moderator*

Significant interactions between the experimental condition and conspiracy beliefs were found for impressions of ability to effect change, honesty, trustworthiness, charisma, benevolence, integrity, predictability, competence, ethics, and support for the politician, but not for impressions of being an outsider (see Table 8 for interaction statistics and conditional effects and Supplementary Materials for an illustration of simple slopes of significant interactions). Specifically, weaker and moderate conspiracy believers showed lower support for the pro-

[7] Independent-samples *t*-tests showed that the experimental manipulations did not significantly affect right-wing attitudes, $t(415) = 0.673$, $p = .501$, or conspiracy beliefs, $t(415) = 1.833$, $p = .067$.

conspiracy (vs. anti-conspiracy) politician, whereas stronger conspiracy believers showed no difference in support for either politician. A similar pattern of conditional effects was found for impressions of trustworthiness, predictability, competence, and ethical leadership. Weaker and moderate conspiracy believers reported lower impressions of honesty, benevolence, and integrity for the pro-conspiracy (vs. anti-conspiracy) politician, whereas stronger conspiracy believers reported these impressions as higher for the pro-conspiracy (vs. anti- conspiracy politician). Weaker conspiracy believers reported lower, and stronger conspiracy believers reported higher, impressions of ability to effect change for the pro-conspiracy (vs. anti-conspiracy) politician, whereas no differences in this impression were found for moderate conspiracy believers. Finally, weaker conspiracy believers reported lower impressions of charisma for the pro-conspiracy (vs. anti-conspiracy) politician, whereas no differences in this impression were found for moderate and stronger conspiracy believers.

To examine whether right-wing attitudes and conspiracy beliefs were independent moderators, we simultaneously included the condition x right-wing attitudes and the condition x conspiracy beliefs interaction terms into the model for each dependent measure. Except for ratings of the ability to effect change and ratings of charisma—where the right-wing attitudes x condition interaction was not significant—in every other model both interaction terms were statistically significant ($p < .05$). This suggests that the impressions of the anti-conspiracy vs. pro-conspiracy politicians were largely moderated by both right-wing attitudes and participants' own belief in conspiracy theories."

The findings of Experiment 5 largely support those of Experiments 4a and 4b, demonstrating that people's impressions of politicians seem to be influenced by the extent to which the politicians share conspiracy theories. Not all of these impressions are unfavorable. For populist leaders, sharing conspiracy theories may therefore indeed enable them to create the impression of an anti-establishment "rogue" or "one of the people" outside of the political elite. Indeed, we again observed the largest effects for ratings of being a political outsider. In other words, there may be both costs and benefits for politicians who share conspiracy theories.

The findings further show, however, that the impressions that people form also depend on their own right-wing attitudes and conspiracy beliefs, replicating the results of Experiment 4a with another US sample, but not of Experiment 4b which had a UK sample. Specifically, there tended to be more favorable attitudes toward the anti-conspiracy candidate (vs. the pro-conspiracy candidate) at lower levels of right-wing attitudes, whereas at higher levels of right-wing attitudes, attitudes toward the politician did not depend on whether they supported or refuted the conspiracy theories. Similarly, there tended to be more favorable attitudes toward the anti-conspiracy candidate (vs. the pro-conspiracy candidate) at lower levels of conspiracy belief, whereas at higher levels of conspiracy belief, attitudes toward the pro-conspiracy tended to be more favorable. Just over three years after running Experiment 4a in the USA, we therefore largely replicated the effects observed in that experiment. We also extended these results to show that participants' own belief in conspiracy theories also influences the impressions they form of individuals who share vs. refute conspiracy theories.

6. General discussion

In six experiments, we examined some of the potential social consequences of sharing conspiracy theories. In Experiment 1, participants perceived that other people would more likely avoid conspiracy theories when they wish to make a favorable impression on others, but would share conspiracy theories to make a less favorable impression. Experiments 2 and 3 revealed that the participants themselves would share conspiracy theories in order to create specific impressions. In a general context (Experiment 2), participants shared conspiracy theories more when aiming to be perceived negatively, radical, unstable, and unique. In a political context (Experiment 3), participants shared conspiracy theories more when aiming to be perceived as radical and unique. Additionally, participants' conspiracy beliefs moderated these effects—low conspiracy believers shared conspiracy theories more to portray themselves negatively, radical, unstable, unique (Experiments 2 and 3), and dishonest (Experiment 3). Conversely, higher believers shared conspiracy theories more to be perceived as stable and honest (Experiment 3).

In Experiments 4a, 4b, and 5, participants rated a fictitious politician who supported (vs. refuted) conspiracy theories as less trustworthy, predictable and competent, but more of a rogue political outsider. In Experiment 4a, participants were also less likely to support a pro-conspiracy candidate. In addition, in Experiments 4a and 5, right-wing attitudes moderated some of these effects such that participants lower in right-wing attitudes tended to be more favorable toward an anti-conspiracy candidate, whereas participants higher in right-wing attitudes were either more favorable toward a pro-conspiracy politician (Experiment 4a), or were unaffected by the conspiracy theories (Experiments 5). Further, in Experiment 5, some effects were also moderated by pre-existing conspiracy beliefs such that stronger conspiracy believers were more favorable toward a pro-conspiracy candidate, but weaker conspiracy believers were more favorable toward an anti-conspiracy candidate. Taken together, the current findings support previous research highlighting the social costs of sharing conspiracy theories. However, they go further by demonstrating that there are potential benefits too.

Conspiracy theories are widely thought of as stigmatizing beliefs (e.g., Lantian et al., 2018). Research suggests that people who share conspiracy theories are perceived unfavorably (e.g., Cassam, 2016; Klein et al., 2015). One key implication of the current research is that although this may often be the case, conspiracy theories do not seem to exclusively indicate negative qualities. They may create the impression that a person is unique, or a radical outsider–someone who may challenge the system and make things happen. People may therefore be able to use conspiracy theories for specific impression-management purposes. Crucially, people might be able to use conspiracy theories to create desired images in audiences that matter to them. This might help explain the proliferation of conspiracy theories and their use to mobilize political support by politicians who are well aware that they look bad to opponents or mainstream audiences. At the same time, lay people also seem to show some understanding that this impression-management is possible.

Studies on the communication of conspiracy theories are rare, and even more so in psychology (Douglas et al., 2019). However, understanding when and why people share conspiracy theories, and what the consequences of sharing conspiracy theories are, is crucial to understanding how they affect individuals, groups, society and politics. We argue that scholars should make efforts to integrate studies on the communication of conspiracy theories with others that examine the psychological and other factors that influence individuals' beliefs in conspiracy theories.

It is also important to understand the consequences of conspiracy communication in different contexts. In the current research, the findings from Experiments 4a and 5 were different in some ways to those of Experiment 4b. As we mentioned earlier, the main difference between these two experiments is the country in which they were conducted. Experiments 4a and 5 were conducted in the USA and Experiment 4b was conducted in the UK. At the time of conducting Experiments 4a and 4b, Donald Trump was president of the USA and–compared to the UK–political discourse was rife with conspiracy theories. We had witnessed QAnon emerge as an influential political movement, with former President Trump one of its biggest supporters (Nguyen, 2020). It is therefore likely that participants in Experiment 4a (and also Experiment 5) were more familiar with, and potentially more affected by, conspiracy theories than their UK counterparts. Whatever the reason or reasons for the differences in findings across the studies, they nonetheless highlight

that the communication of conspiracy theories is likely not to be uniform across contexts. Likewise, the consequences of conspiracy theories in political discourse are unlikely to be the same across contexts. Indeed, what might be negative and costly in one context could be positive and beneficial in another, and vice versa. For example, if a political candidate were to share their belief in a particular conspiracy theory whilst canvassing an election, then they may come across as unstable and decrease their chances of winning the vote. On the other hand, a politician sharing conspiracy theories during political proceedings might create the impression of being radical, thus convincing their opponents that they are willing to use extreme tactics to get their way. In this light, the audience and context may both play important roles in determining when and how sharing conspiracy theories comes with costs or benefits. Future research could investigate these ideas further.

One limitation of the current research is that all participants were recruited from the crowdsourcing platform Prolific. These samples are not nationally representative and tend to be relatively homogenous, including few strong conspiracy believers. These studies can therefore tell us how "average people" tend to perceive and respond to conspiracy theories, but not how people with strong conspiracy beliefs perceive and respond to them. To some extent this is a problem in the literature on the psychology of conspiracy theories more broadly (see Franks, Bangerter, & Bauer, 2013) but is nevertheless important to bear in mind when interpreting the present results. Future research could attempt to recruit more diverse samples–including stronger conspiracy believers–to examine if the effects observed here still hold.

Another limitation is that the current experiments have limited ecological validity. Participants were asked to think about when others might communicate conspiracy theories (Experiments 1, 2, and 3), or to evaluate a fictitious politician who does, or does not share conspiracy theories (Experiments 4a, 4b, and 5). Experiments like this can inform us how people might perceive and respond to conspiracy theories in real-world situations–and isolating the effects of conspiracy theories in experimental studies is of course advantageous–but real-world situations are much more complex. For example, it is possible that people might evaluate politicians based on the combination of their communication strategies (including their use of conspiracy theories), and other features such as their political stance, or position on important issues. Future research might therefore introduce more detailed experimental designs to examine some of these interactions, or even conduct observational studies on social media.

We also know from other research that belief in conspiracy theories correlates with political extremism (Imhoff et al., 2022). The present work suggests that political attitudes also come into play when people are evaluating individuals who share conspiracy theories. Specifically, evaluations of pro- and anti-conspiracy candidates were sometimes moderated by right-wing attitudes (Experiments 4a and 5). We conducted a meta-analysis of Experiments 4a, 4b, and 5 with the purpose of increasing our statistical power for detecting this moderating effect,[8] and found that the latter was context-specific. Specifically, right-wing attitudes moderated the impressions that participants formed of pro- and anti- conspiracy politicians, but only in the US context and not in the UK. This finding also deserves further investigation. We have mentioned previously that the political context may differ in important ways in the US and UK and that the prominence of conspiracy theories in politics may also differ (Walter & Drochon, 2022). Future research could therefore dig deeper into the role of political context and also examine the role of other potential moderators. Overall, these analyses suggest again that context is important.

Another potential methodological limitation is that Experiments 4a, 4b, and 5 did not have a control condition where the politician's speech did not communicate anything related to a conspiracy theory. Previous research using a conceptually similar design to Experiments 4a, 4b, and 5 found no differences between an anti-conspiracy condition and a control (Jolley & Douglas, 2014a, 2014b), but future research could nevertheless examine whether this would still be the case for impression formation.

Another potential issue is whether or not participants in the current experiments perceived the conspiracy theory statements as such. The statements we used are indeed rated as conspiracy theories on average (Douglas et al., 2021). However, previous research has shown that conspiracy statements are labeled as conspiracy theories more by people who reject them (Douglas et al., 2021), but less by people who accept them (Wood & Douglas, 2013). In our experiments, lower conspiracy believers were found to create more unfavorable, and higher believers were found to create more favorable, impressions of themselves (Experiments 2 and 3) and others (Experiment 5) when sharing conspiracy theories. Therefore, future studies could examine these effects again when the statements are explicitly labeled as conspiracy theories, or given an alternative label. Such research would help to determine whether the effects found in the current research are unique to conspiracy theories, or whether they speak to a more general effect of creating unfavorable impressions when expressing beliefs that differ from the mainstream.

It would also be useful to examine the social consequences of sharing conspiracy theories over longer time periods. The current studies provide a snapshot of these consequences but again, political contexts are much more complex and rely on many different events over longer periods of time. With some exceptions (Bierwiaczonek, Kunst, & Pich, 2020; Golec de Zavala & Federico, 2018; Kofta, Soral, & Bilewicz, 2020; Liekefett, Christ, & Becker, 2021), research on the psychology of conspiracy theories lacks this longer-term perspective. Longitudinal studies would be particularly important to examine the effects of communicating conspiracy theories over extended periods of time and changing circumstances. For example, research could examine the longer-term effects of conspiracy theories on voting behavior and the extent to which conspiracy theories can trigger social change. Future research could also examine how sharing conspiracy theories influences interpersonal relationships, or relationships within the workplace. We know very little about the effects of conspiracy theories in these contexts (e.g., Douglas & Leite, 2017), and still less about their effects over time.

7. Conclusion

Conspiracy theories are often viewed as stigmatizing beliefs and that people are negatively evaluated for sharing them. The current research provides further support for this view, but importantly that not all of the consequences of sharing conspiracy theories are negative. By sharing conspiracy theories, people appear to have an opportunity to present themselves in a variety of different ways to others, some of which may have more positive overtones depending on the context. This research opens up new lines of research on the potential benefits and pitfalls for individuals who share conspiracy theories.

Open science practices

Experiments 3 and 5 were pre-registered on OSF, including hypotheses and planned analyses.

- Experiment 3: https://osf.io/3mvwx?view_only=96e63724255747e8a3c05dd97af2e26c
- Experiment 5: https://osf.io/b87fd/?view_only=0e77cc512c8e485fb229409df99761a5

Data access statement

Experimental materials and data are openly available on OSF under the following link: https://osf.io/dqhb3/?view_only=56d77e903bfa4

[8] For further details about statistical power and the meta-analysis, see the Supplementary Materials.

747aa53a32f35f353f9. All measures, manipulations, and exclusions in the experiments are disclosed, as well as the method of determining the final sample sizes in each case.

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Declaration of Competing Interest

The authors declare no conflict of interest.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jesp.2022.104398.

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