Conspiracist Ideation and Behavior: Evidence from an Incentivized Survey

Seth Emblem

Appalachian State University

March 2024

Abstract

There exists data indicating that issues relating to conspiracy theories have received increased media coverage in recent years. The general concern present in contemporary media coverage is that belief in conspiracy theories might be associated with negative behavioral characteristics. There exist prior studies which suggest that conspiracy theory belief is associated with potential behavioral differences in areas relevant to the workplace and broader economic growth, but these studies did not observe incentivized behavior. The purpose of this study is to test for the associations between conspiracy theory belief and behavior that prior studies suggest might exist. For our methodological approach, we conduct an incentivized online survey to observe subjects' behavior. Ultimately, while conspiracy theory endorsers are more likely to engage in some form of partisan signaling when asked factual questions, they do not, in general, behave differently in the incentivized tasks. These results provide insights into how conspiracy theory belief might (not) impact outcomes in the workplace and broader economic growth, while also suggesting avenues into further research regarding the links between conspiracy theory belief and behavior.

1 Introduction

While conspiracy theories have been an element of the American political sphere since the foundation of the country in the late 18th century, media coverage on issues relating to conspiracy theories has appeared to increase in recent years. An indication of this rise in media coverage can be found in data from the *Media Cloud* database which shows that the percentage of online news stories in the United States containing the phrase "conspiracy theory" has risen ten-fold from 2011 to 2021. While this statistic does not rigorously prove that belief in conspiracy theories is on the rise in the United States, it does provide evidence that there is a growing concern about the potential consequences of conspiracy theory belief. There does not exist clear time series data measuring belief in conspiracy theories and so proving that conspiracy theory belief is on the rise in the U.S. would be difficult, but we can address another important question regarding conspiracy theory belief: are there any real

consequences of conspiracy theory belief? Putting the issue of tracking conspiracy theory belief across time aside, if conspiracy theory belief has real consequences, then we should be able to observe behavioral differences between those who are more likely to endorse conspiracy theories and those who are less likely to endorse conspiracy theories. The purpose of the present paper is to investigate whether there are associations between observed behavior and conspiracy theory belief.

For there to be real consequences of conspiracy theory belief, we need two things. First, there must be behavioral differences between those who are more likely to endorse conspiracy theories and those less likely to endorse conspiracy theories. Second, those behavioral differences must be relevant in contexts such as the workplace. Regarding potential behavioral differences between those more likely to endorse conspiracy theories and others, there are reasonable claims in prior literature that those more likely to endorse conspiracy theories are less trusting, more entitled, more dishonest, more likely to rely on their own judgements about issues, and more creative. The rationale behind these claims is that these personal tendencies are more consistent with being a person who endorses conspiracy theories than not. In the academic literature, we can find studies backing the claims that those who endorse conspiracy theories are less trusting (e.g. Goertzel [1994], Miller et al. [2016]), more entitled (e.g. Cichocka et al. [2016], Enders et al. [2021]), more dishonest (e.g. Jolley et al. [2019]), more likely to rely on their own judgements about issues (e.g. Goertzel [1994], Harambam and Aupers [2017]), and more creative (e.g. Harambam and Aupers [2017], Bonetto and Arciszewski [2021]). These academic studies are suggestive that conspiracy theory endorsers are different from others in the aforementioned ways, but we should point out that none of these studies observe incentivized behavior. This is an important methodological point, as there is a long-standing literature showing that responses to hypothetical scenarios do not always translate to incentivized behavior (e.g. Bohm [1972], Neill et al. [1994], Cummings et al. [1995]). More recently, Esarey et al. [2012] showed how questions relating to a person's self-image (e.g. questions about how much you trust others) are specifically subject to this hypothetical bias. Therefore, if we are going to investigate whether there are real consequences of conspiracy theory belief, we need to observe incentivized behavior in some fashion. The academic studies mentioned above point us towards a set of behavioral areas we need to observe to investigate the issue of whether there are real consequences of conspiracy theory belief. We will now highlight how these suggested behavioral differences can have real consequences.

As an example of how these suggested behavioral differences can have real consequences, consider how employee behavior can influence the environment and productivity of the workplace. Employees who do not trust others are not likely to cultivate the relationships necessary to promote a positive and productive work environment (e.g. Brown et al. [2015]). Furthermore, entitled employees can create a toxic work environment (e.g. Harvey and Harris [2010], Harvey et al. [2014]) and dishonest employees are more likely to steal from the workplace (e.g. Graham et al. [2002]). On a more positive note, employees who rely on their own judgements can help avoid group think and lead workplaces to adopting better practices than currently in place (e.g. Sinaiko and Hirth [2011], Bhargava et al. [2017]). Furthermore, creative employees can help induce innovation in the workplace and consequently increase the productivity of their fellow employees (e.g. Gong et al. [2013]). If conspiracy theory endorsers are less trusting, more entitled, and more dishonest than others, then a surge of

conspiracy theory belief among employees could be concerning for the quality of workplace environments we have in the United States. Of course, if conspiracy theory endorsers are also more likely to rely on their own judgements and are more creative, then the net impact of a rise in conspiracy theory belief on U.S. workplaces is ambiguous. In any case, if conspiracy theory endorsers are different from others in a majority of these behavioral areas, then a rise in conspiracy theory belief could have real consequences, positive and/or negative. What we need to know is whether any of these potential behavioral differences between conspiracy theory endorsers and others exist in reality. To answer this question, we need a methodology that can first give us a measure of who is a conspiracy theory endorser, and second allows us to observe the behavior of participants. A methodology with both of these primary elements will enable us to test whether conspiracy theory endorsers are behaviorally different from others in these aforementioned ways.

One approach to address this issue would be to obtain an instrument for conspiracy theory belief among employees in U.S. workplaces and field data measuring behavior for each of our relevant areas (e.g. rates of petty theft as a measure for dishonesty). However, while one can find field data measuring the amount of petty theft in the workplace, it is more difficult to find field data which provides an incentivized measure of trust or employees' propensity to rely on their own judgements. Given this difficulty, another approach we can take is to design our own instrument that is able to both give us a measure for conspiracy theory belief and provide incentivized measures for each of our relevant behavioral areas.

To test for the potential behavioral differences between those more likely to endorse conspiracy theories and others, we constructed and implemented an online survey instrument. In this survey, subjects make decisions within a set of behavioral measures. The incentivized behavioral measures used are borrowed from previous research studies where possible, and are of our novel design where necessary. The key of the survey instrument is that subjects receive payment for the decisions they make. In this sense, we can observe subjects' behavior rather than just their responses to hypothetical situations where nothing is explicitly at stake. Along with these incentivized behavioral measures, we ask subjects a set of factual political questions as a way to measure for potential non-incentivized behavioral differences between conspiracy theory endorsers and others (e.g. partisan cheerleading). Another important element of the survey instrument is a measure of subjects' belief in conspiracy theories. Measuring conspiracy theory belief for each subject is not as simple as asking subjects whether they think a given conspiracy theory is true (e.g. conspiracy theories about the assassination of JFK), as responses to such questions could depend on factors such as age rather than the subject's general tendency to endorse conspiracy theories. We need a measure of conspiracy theory belief that can more properly measure subjects' propensity to endorse conspiracy theories. Fortunately, there already exists a field validated mechanism for measuring subjects' general tendancy to endorse conspiracy theories: the generic conspiracist beliefs (GCB) scale from Brotherton et al. [2013]. The set of questions we borrow from Brotherton et al. [2013] are meant to identify "conspiracist ideation" in subjects. We can define conspiracist ideation as "individual differences in the general tendency to engage with conspiracist explanations for events."[1] This concept is based on research (e.g. Goertzel [1994], Wood et al. [2012], Enders et al. [2021]) suggesting that individuals who endorse one

[1]Brotherton et al. [2013]

conspiracy theory are more likely to endorse others, even if those other conspiracy theories are unrelated or directly contradictory to the original conspiracy theory. The questions from Brotherton et al. [2013] do not ask about specific conspiracy theories, but rather ask about the general notions involved with most conspiracy theories (e.g. the government is involved in the murder of innocent citizens and/or well-known public figures, and keeps this a secret). With this measure for conspiracy theory belief, we can test whether those more likely to endorse conspiracy theories behave differently than those who do not generally endorse conspiracy theories.

Reference to contemporary dialogue and the existing academic literature lead us to the hypothesis that conspiracy theory endorsers behave differently from others in the areas regarding trust, dishonesty, entitlement, relying on one's own judgement, and creativity. Our main findings of interest are that, while conspiracy theory endorsers provide different answers to the factual political questions than others, they do not, in general, behave significantly differently from others in our incentivized tasks. In the following sections, we will go through the design and outcomes of our online survey instrument. In Section 2, we will discuss the methodology of the present study. In this section, we will provide details about each behavioral measure, including cases where we designed a novel measure, and state the hypotheses for each measure. In Section 3, we will briefly discuss the data collection process and provide some summary statistics of the data. In Section 4, we will present the results of the study. In Section 5, we will discuss the implications and interpretations of our results and provide concluding remarks.

2 Methodology

To test whether conspiracy theory endorsers are different from others in the aforementioned behavioral areas, we need our survey instrument to include three main parts: first, a set of standard demographic questions to act as control variables for our regression analysis, second, a set of questions which allows us to appropriately identify who is a conspiracy theory endorser, and third, a set of behavioral measures which allow us to effectively observe subjects' behavior in the relevant areas.

As mentioned in the introduction, we borrowed our measure for conspiracy theory belief from Brotherton et al. [2013]. To limit the length of the survey instrument, and to reduce potential experimenter effects, we selected a subset of eight questions from their original 15-item generic conspiracist beliefs (GCB) scale. We used questions that were most closely related to politics and political current events. For instance, we included questions that asked subjects about whether small, secret groups controlled world events/politics, but we did not include questions about extraterrestrials. This way, our conspiracy ideation questions can be framed as a continuation of a section asking subjects about political questions instead of a separate section blatantly asking subjects whether they are conspiracy theorists. The third part of our survey instrument is the incentivized tasks, which we will go through in the following paragraphs.

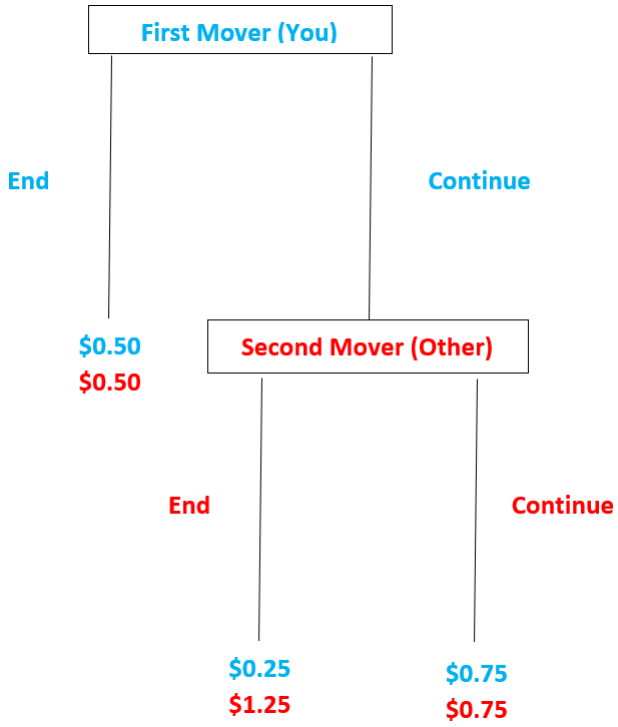

Figure 1: Trust Game Tree

2.1 Interpersonal Trust

The first behavioral measure deals with interpersonal trust. To measure trust, we borrowed a version of the standard investment game from Bohnet and Zeckhauser [2004].[2] As we can see from the game tree in Figure 1, incentives are for Second Mover to play "End," unless he wants to reward First Mover for trusting him, in which case he should play "Continue." By backward induction, First Mover thus has incentive to play "End," unless she believes Second Mover is trustworthy and will reward her for trusting him, in which case she should play "Continue." In this sense, we can view a first mover choosing "Continue" as "trusting," and we can view a second mover choosing "Continue" as "trustworthy."

Whether an individual chooses "End" or "Continue" in the first mover position depends both on her preferences and her subjective beliefs about what the second mover will do. If we find any differences in behavior in the trust game between conspiracy theory endorsers and others, it would be useful to know how much subjects' subjective beliefs might be driving those differences. The design of Andreoni and Sanchez [2020] provides us an incentive compatible elicitation of subjects' beliefs in our trust game. After subjects submit their choice for the trust game, they receive the prompt for the belief elicitation task. For this task, there are two options: Option 1 and Option 2. For each option, subjects could earn either \$0.75 or \$0.25. In Option 1, the probabilities of each payment were predetermined,

[2]Cox [2004] points out that other-regarding preferences could also motivate First Mover to choose Continue. Ultimately, while other-regarding preferences might play a role, his results provide strong support to the conclusion that the standard investment game measures trusting behavior. Given its simplicity, we therefore utilize this version of the standard investment game as our measure of trusting behavior.

	Option 1:		Or	Option 2:	
	Chance of $0.25	Chance of $0.75		Chance of $0.25	Chance of $0.75
1	**0 in 100**	**100 in 100**	*Or*		
2	**10 in 100**	**90 in 100**	*Or*		
3	**20 in 100**	**80 in 100**	*Or*		
4	**30 in 100**	**70 in 100**	*Or*		
5	**40 in 100**	**60 in 100**	*Or*	% second movers chose to end	% second movers chose to continue
6	**50 in 100**	**50 in 100**	*Or*		
7	**60 in 100**	**40 in 100**	*Or*		
8	**70 in 100**	**30 in 100**	*Or*		
9	**80 in 100**	**20 in 100**	*Or*		
10	**90 in 100**	**10 in 100**	*Or*		
11	**100 in 100**	**0 in 100**	*Or*		

Figure 2: Belief Elicitation Table

ranging from a 100% chance of $0.75 and a 0% chance of $0.25 in row 1, to a 0% chance of $0.75 and a 100% chance of $0.25 in row 11. In Option 2, the probabilities of each payment were determined by the results of the trust game. For the first movers, the probability of $0.75 was the proportion of second movers who chose "Continue," and the probability of $0.25 was the proportion of second movers who chose "End." For the second movers, the probability of $0.75 was the proportion of first movers who chose "Continue," and the probability of $0.25 was the proportion of first movers who chose "End."[3] For this task, subjects needed to choose when they would switch from preferring Option 1 to preferring Option 2. Figure 2 shows the table first movers saw when making their choice. Since the subjects did not know the results of the trust game, this task provides an incentive compatible elicitation of their beliefs. As an example, if a first mover thought 35% of second movers chose "Continue," then he would switch from preferring Option 1 to preferring Option 2 in row 8. For each subject, we randomly selected one of the rows to use to determine their payoff. For the row we selected, we used the option the subject indicated they preferred for that row. If in the example above we randomly selected row 6, then we used Option 1 to determine their payoff. In this case, the subject would have a 50% chance of receiving $0.75, and a 50% chance of receiving $0.25.

If conspiracy theory endorsers choose the trusting action (Continue) less often than others, then we can check the responses to the belief elicitation task to see if their subjective beliefs are a significant factor driving this difference. Related to subjective beliefs, Douglas and Sutton [2011] suggests that conspiracy theory endorsers engage in projection: they think others might engage in conspiracies because they themselves would engage in conspiracies if given the opportunity. This concept of projection suggests that if conspiracy theory endorsers choose "Continue" less often than others, then we might expect that conspiracy theory endorsers will switch from preferring Option 1 to Option 2 in our belief elicitation task later than others (i.e. conspiracy theory endorsers have more pessimistic beliefs about the trustworthiness of others).

[3]Before subjects are given the task, we ask them a comprehension question as a check to see if they understood the directions. We give them the correct answer to the comprehension question after they submitted their answer.

2.2 Entitlement

The next behavioral measure deals with entitlement. We can define (psychological) entitlement as "a stable and pervasive sense that one deserves more and is entitled to more than others."[4] With this definition in mind, one way to measure entitlement is to utilize a task where a subject can choose to take some sort of prize even though the "rules" state that someone else should receive the prize. Such a task requires two important elements. First, the task needs a prize assignment rule that an entitled person would be willing to ignore. Second, this task needs to be able to distinguish between entitlement and standard selfish preferences. As an example of this distinction, consider a setting where a group of people are deciding on a rule to determine who gets the last slice of pizza from a luncheon. A selfish person would of course want to get the last slice, but as long as she knows that the agreed upon rule is fair, she will accept the outcome of the rule. An entitled person, on the other hand, will only accept the outcome of the rule if he gets the last slice (he would take any outcome where he did not get the last slice as evidence that the rule was unfair). To the best of our knowledge, there is not a preexisting game which distinguishes between entitlement and standard selfish preferences. We therefore designed a novel game that makes this distinction for our measure of entitlement.

For the prize assignment rule of our game, subjects are first asked to tell us which of two paintings they prefer: Painting 1 (Kandinsky) and Painting 2 (Klee). We chose the Kandinsky and Klee painting task as our prize assignment rule because its original intent in Chen and Li [2009] was to create an assignment device that was random[5], but also felt nonrandom to subjects because they were making a choice based on their preferences. We paid subjects \$0.50 for stating their painting preference. Once a subject submitted their painting preference, we informed them that an additional \$1.00 prize was available to take. Subjects who chose Painting 1 were paired with subjects who chose Painting 2.[6] After subjects submitted their painting preference, we informed subjects who chose Painting 2 that the additional \$1.00 prize was intended for them to take, and we informed subjects who chose Painting 1 that the additional \$1.00 prize was not intended for them to take. Regardless of which painting they chose, each subject had the option to try to take the prize, or to defer it to the subject they were paired with. Figure 3 shows the game tree from the perspective of a subject who chose Painting 2. Subjects who chose Painting 2 (i.e. "winning" subjects) effectively go first in this game, so they make a single choice between trying to take the prize and deferring the prize to the other subject. Subjects who chose Painting 1 (i.e. "losing" subjects) effectively go second in this game. Because we are using an asynchronous online survey to observe subjects' behavior, we cannot simply ask losing subjects whether they want to take the prize or defer it back to the winning subject. If we only asked losing subjects this single question, then their subjective beliefs about what the winning subject will do could be relevant to their decisions. This would make it difficult for us to distinguish between stan-

[4] Campbell et al. [2004]

[5] In our sample of 303 subjects, 49% chose Painting 1 (148) and 51% chose Painting 2 (155).

[6] There were seven more subjects who chose Painting 2 (155) compared to Painting 1 (148). So, we had 148 pairs, each with one subject who chose Painting 1 and one subject who chose Painting 2. The seven leftover subjects who chose Painting 2 were each matched with a subject who chose Painting 1. For these seven, the matched actions only affected their own payoff. For the 148 pairs, the paired actions affected both subjects' payoffs. The instructions given to subjects were constructed to allow for this multiple pairing.

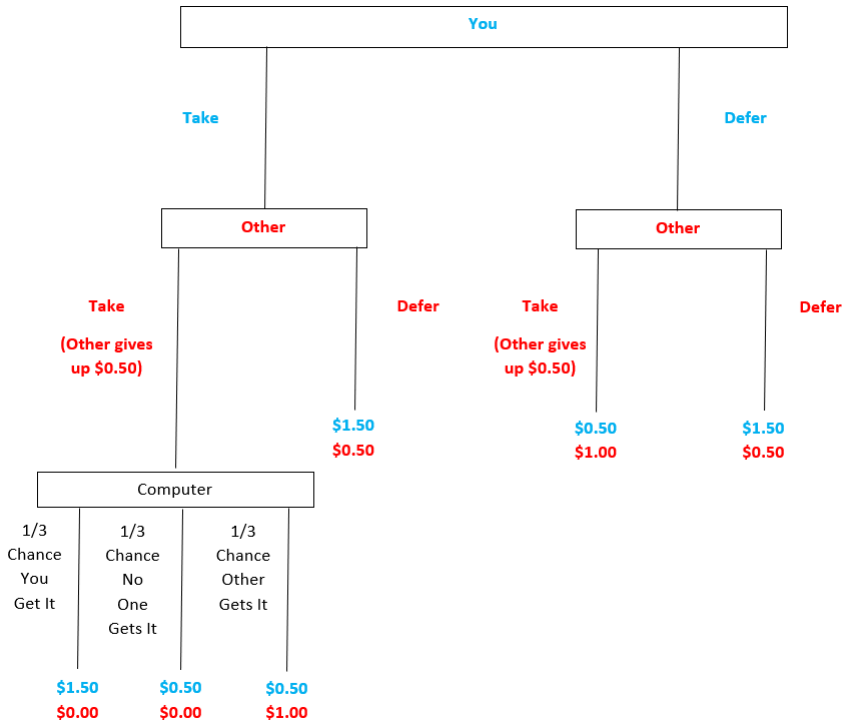

Figure 3: Entitlement Game Tree

dard selfish preferences and entitlement. To avoid this issue, we use the strategy-method where losing subjects have two choices to make: whether to take or defer when the other subject chose to take, and whether to take or defer when the other subject chose to defer. The other main difference between the choices of a winning subject and a losing subject is that a losing subject must give up the $0.50 we paid them to state their painting preference if they want to take the prize. A winning subject does not have to give up anything to take the prize.

As we can see in the game tree, a losing subject who only cares about expected payoffs will only choose to take if the winning subject he is paired with chooses to defer. If the winning subject chooses to take, the losing subject only makes $0.33 in expectation if he also chooses to take, so he is better off deferring and earning $0.50. For an entitled losing subject, his utility from winning the $1 prize may be greater than the consumption value of $1, so the expected utility of taking might be greater than the expected utility of deferring. Furthermore, an entitled losing subject could also gain utility from changing the rules in his favor. Instead of painting preferences choosing who should get the prize, which a losing subject might have qualms with, a "truly" random process gives each subject equal chance of (not) getting the prize. Such motivations would not appeal to a selfish subject, he will try to earn as much as he can, but such motivations might appeal to someone with the trait of (psychological) entitlement.

2.3 Dishonesty

The next behavioral measures deal with dishonest behavior. To measure dishonest behavior, we need a set of incentivized tasks that requires subjects to make a choice between being honest and maximizing their monetary payoff. We use two such tasks to measure dishonesty, one of which was of our own design based on a relatively unknown quirk about United States' area codes. Our first lying task is a novel design where we ask subjects to provide the middle digit of the first area code they remember using in the United States. Subjects were paid \$0.10 plus \$0.10 times the number they enter. A feature of U.S. area codes is that none have "9" as their middle digit. As such, any subject who reports "9" as the area code's middle digit is definitely lying. Meanwhile, since all subjects have at least one area code they remember using, each subject can tell the complete truth if they desire.

We also use a version of the classic coin flip experiment (e.g. Jacquemet et al. [2021], Dickinson and McEvoy [2021]) as a second lying task. We ask subjects to find a fair coin and flip it five (5) times. We then ask subjects to report the number of times a flip came up as heads instead of tails. Subjects were paid \$0.10 for each time they report a coin flip came up heads. We obviously cannot verify that each subject took the time to do all five coin flips, nor can we verify the outcomes of the coin flips. As such, subjects are free to lie and say that they flipped five heads. However, although most subjects are probably lying in the sense that they did not take the time to flip a coin five times, some subjects might not want to come off as a selfish liar, so they might only report flipping heads two or three times. So, even though most subjects are likely lying by default, this task gives us another measure about the extent to which subjects are willing to lie.

We utilize the standard coin flip task because it has been verified as a measure for dishonesty in previous studies, but there are issues that arise when using it in an online survey. Because some of the subjects might not have a fair coin readily available when taking our survey, these subjects will be lying by default. Given this possibility, we included our novel area code question to make sure that we have a task that allows subjects to be completely honest if they so desire. Also, having two tasks for measuring dishonesty allows us to provide subjects a higher guaranteed base payment for taking the time to complete our survey.

2.4 Propensity to Rely on Own Judgement

The next task deals with one's propensity to rely on their own judgement. In the words of a conspiracy theory endorser, we can conceptualize this propensity as an individual's commitment "to look at things from multiple perspectives, to consult multiple sources, but mostly to think for yourself and be able to adjust previously held convictions."[7] So, when we refer to a propensity to rely on one's own judgement, we mean one's general propensity to test and explore things for herself rather than simply taking someone's word or advice. To measure this, we need a task where subjects are given incentive to make the "correct" choice among a set of options, about which they can choose to either take the advice of someone else, or to gather information about the options himself or herself. Although there are preexisting tasks that address the issue of advice giving, to the best of our knowledge,

[7]Harambam and Aupers [2017]

there are not any preexisting tasks designed specifically to address this issue of relying on one's own judgement. We therefore designed a novel task appropriate for an asynchronous online survey which identifies a general propensity to explore things for oneself.

The novel task we designed is based on a modified version of a two-armed bandit game. Subjects have the choice between two income generating options, labeled "Option A" and "Option B," respectively. Subjects know that the possible payoffs are \$0.25, \$0.50, \$0.75, and \$1.00, but they do not know the distribution of payoffs for each option nor the expected payoff of each option. Option B has an expected payoff \$0.25 higher than Option A. Subjects are informed that one of the options has an expected payoff \$0.25 higher than the other, but they are not told which one. Subjects in the main sample were given two opportunities to help them pick between the two income earning options. For the first opportunity, subjects could conduct 10 trial rounds where they could choose either Option A or Option B and observe the outcome of each of the draws. For the second opportunity, subjects could instead accept the advice of a random previous participant who had already conducted 10 trial rounds in a pilot survey. Subjects who opted to conduct the 10 trial rounds themselves had to pay a monetary price for the opportunity, while subjects who opted to accept a previous subject's advice did not have to pay any monetary price. Subjects were randomly given one of three treatments: a *low cost* treatment where the price to conduct 10 trial rounds was \$0.01, a *medium cost* treatment where the price to conduct 10 trial rounds was \$0.05, and a *high cost* treatment where the price to conduct 10 trial rounds was \$0.10. A rational, risk-neutral, money-maximizing agent will choose to pay the testing cost if the following inequality holds:

$$p_i - p_a > 4c$$

where p_i is the agent's subjective belief that she will choose the correct option (Option B), p_a is her subjective belief that the advice is correct, and c is the cost to test the options. At the low, medium, and high cost conditions, she must believe that she is at least 4%, 20%, and 40%, respectively, more likely to choose the correct option than the average pilot subject to decide to test the options herself. If a subject had the most pessimistic belief about the accuracy of advice (i.e. 50% chance of correct advice), then she must believe that she is at least 54%, 70%, and 90% likely to choose the correct option to be willing to pay the low, medium, and high cost, respectively. There is also a time cost involved in testing the options for oneself. Subjects can finish the survey sooner and spend their time doing other activities if they choose to simply take the advice of someone else. Given the high belief requirements and additional time costs, a subject would only test the options for herself if she is both highly confident in her ability to choose the higher paying option and thinks that going through the 10 testing rounds is worth her time.

In the pilot survey, $\frac{2}{3}$ of subjects recommended Option B and $\frac{1}{3}$ recommended Option A. If a subject knew that the true probability of correct advice was $\frac{2}{3}$, then she could not justify testing the options herself at the high cost, unless she placed high importance on testing things for herself. Therefore, our design not only allows us to test whether conspiracy theory endorsers have a higher propensity to rely on their own judgement on average, but also the strength of their propensity if it exists.

2.5 Creativity

To measure creativity, we want an incentivized task that can elicit responses from subjects such that we can judge their responses based on how novel and useful they are. Fortunately, there already exists such a measure specifically designed for this purpose, namely the Creative Uses Task. We use the standard Creative Uses Task which dates back to Torrance [1966]. We used the instructions and procedures of Dutcher and Rodet [2018] as the template for our design. On the first screen, we told the subjects that they would have three minutes to come up with alternative uses of an object to be displayed on a following screen. Using a tin can as an example, we gave the subjects potential uses that would and would not receive credit. We also gave the subjects instructions for how they should enter their responses in Qualtrics.[8] On the second screen, we explained to them how they would be paid for this task. We paid subjects \$0.15 for each valid use they provided. A valid use is any plausible use different from the object's primary intended use. Furthermore, we paid subjects an additional \$0.15 for each valid use that was unique among a set of 10 subjects.[9] We also told subjects that their responses would be judged by two independent judges (not the experimenters) to determine whether they satisfy the requirements. On the task screen, subjects were told that their object was an extension cord.[10] Subjects entered their responses into a text box, and they could either click the "next" button to continue when they were finished or be automatically moved to the next task when the three-minute timer hit zero.

2.6 Factual Political Questions

The last part of our survey design to discuss is our set of factual political questions. These factual political questions act both as a segue between the demographic questions and our conspiracist ideation questions, and as an additional behavioral measure for differences between conspiracy theory endorsers and others. These four political questions ask subjects about facts, such as how Joe Biden's 100-day approval rating compared with the 100-day approval ratings of George W. Bush and Donald Trump. Three of these factual political questions were either directly borrowed from or based on questions from Robbett and Matthews [2018]. We constructed the fourth factual political question in the survey instrument ourselves. We normalize responses to these questions such that providing the most democratic-friendly answer is scored as "0," so a low sum across the four questions reflects a bias towards democrats and a high sum reflects a bias towards republicans. As an example, we asked subjects how much of the promised border wall was built during the four years of the Trump administration. Subjects were given five options: less than 150 miles, between 150 and 300 miles, between 300 and 450 miles, between 450 and 600 miles, and more than 600 miles.[11] So, scores for this question could range from "0" if a subject answered with

[8] We asked the subjects a comprehension question about these directions as a check to see if they were reading carefully. We gave subjects the correct answer to the comprehension question on the following screen.

[9] Since there were 303 subjects, three of the groups had 11 subjects.

[10] Of the objects used in Dutcher and Rodet [2018], extension cord was the only one where a Google search for creative uses did not immediately provide a list of valid uses. Note that Dutcher and Rodet [2018] was conducted in a laboratory setting, so they did not need to worry about subjects trying to do a Google search for creative uses.

[11] The correct answer was "between 450 and 600 miles."

"less than 150 miles," to "5" if a subject answered with "more than 600 miles."

As mentioned above, these four factual political questions not only act as a segue between our demographic questions and our conspiracist ideation questions, but they also provide us an additional, non-incentivized, test for behavioral differences between conspiracy theory endorsers and others. The political mindset of a conspiracy theory endorser may be different from someone who does not endorse conspiracy theories. For instance, a conspiracy theory endorser might believe that the mainstream consensus on a relevant issue is incorrect, while others would generally have opinions more in line with the mainstream consensus. As such, we might expect that conspiracy theory endorses will provide answers that are more extreme than those who generally do not endorse conspiracy theories. For our measure of this, we take the sum of each subject's responses to the four factual political questions. So for the purposes of our survey instrument, our expectation is that the total answers for conspiracy theory endorsers will be significantly higher (or lower) than the total answers for others.

2.7 Hypotheses

Before we discuss our data and results, we will now explicitly state the hypotheses our survey instrument was designed to test. These hypotheses reflect the suggestions from the prior literature on how conspiracy theory endorsers might behave.

Hypothesis 1. *(Trust) Conspiracy theory endorsers in the position of "First Mover" will choose "Continue" less often than others.*

This hypothesis is based on the expectation that conspiracy theory endorsers are less trusting than others. Since a first mover choosing "Continue" in our trust game indicates trusting behavior, we therefore expect conspiracy theory endorsers to choose "Continue" less often than others.

Hypothesis 2. *(Trustworthiness) Conspiracy theory endorsers in the position of "Second Mover" will choose "Continue" less often than others.*

Hypothesis 3. *(Subjective Beliefs) Conspiracy theory endorsers' elicited beliefs about the percentage of second movers choosing "Continue" will be lower than the elicited beliefs of others.*

These two hypotheses are based on the expectation that conspiracy theory endorsers will engage in projection. Assuming that conspiracy theory endorsers are less trusting than others, projection first requires that conspiracy theory endorsers are less trustworthy than others, and second that conspiracy theory endorsers are more pessimistic about the trustworthiness of others than others are. Since a second mover choosing "Continue" in our trust game indicates trustworthy behavior, we therefore expect conspiracy theory endorsers in the position of "Second Mover" will choose "Continue" less often than others, and furthermore that conspiracy theory endorsers' elicited beliefs about the percentage of second movers choosing "Continue" will be lower than the elicited beliefs of others.

Hypothesis 4. *(Entitlement) Conspiracy theory endorsers who picked Painting 1 (i.e. a "losing subject") will be more likely to take the* \$1 *prize than others.*

This hypothesis is based on the expectation that conspiracy theory endorsers are more entitled than others. Since a losing subject choosing to always take the $1 prize in our entitlement task indicates entitlement, we therefore expect conspiracy theory endorsers who picked Painting 1 will choose to always take the $1 prize more often than others.

Hypothesis 5. *(Dishonesty 1) Conspiracy theory endorsers will report "9" as their area code's middle digit more often than others.*[12]

Hypothesis 6. *(Dishonesty 2) Conspiracy theory endorsers will report a higher average number of heads than others.*

These hypotheses are based on the expectation that conspiracy theory endorsers are more likely to engage in dishonest behavior than others. Since reporting a high number of heads or reporting "9" as one's area code middle digit is an indication of dishonesty, we therefore expect conspiracy theory endorsers will report a higher average number of heads than others and conspiracy theory endorsers will report "9" as their area code's middle digit more often than others.

Hypothesis 7. *(Relying on Own Judgement) Conspiracy theory endorsers will choose to test the options more often than others.*

This hypothesis is based on the expectation that conspiracy theory endorsers are more likely to rely on their own judgements than others are. Since choosing to test the options in our modified two-armed bandit game is an indication of relying on one's own judgement, we therefore expect conspiracy theory endorsers will choose to test the options more often than others.

Hypothesis 8. *(Creativity) On average, conspiracy theory endorsers will provide a greater number of valid/unique uses than others.*

This hypothesis is based on the expectation that conspiracy theory endorsers are more creative than others. Since providing a high number of valid/unique uses in the Creative Uses Task is an indication of creativity, we therefore expect that conspiracy theory endorsers will provide a greater number of valid/unique uses than others.

Hypothesis 9. *(Political Bias) The average answer conspiracy theory endorsers give to the factual political questions will be higher (or lower) than the average answers of others.*

This hypothesis is based on the expectation that conspiracy theory endorsers will provide more extreme answers to the factual political questions than others. Since we expect that conspiracy theory endorsers are of a different political mindset than others, we expect to see conspiracy theory endorsers provide answers further away from the "mainstream consensus" (i.e. more extreme answers).

[12]We could also test for the average area code response. The issue with this test, however, is that conspiracy theory endorsers could have randomly lived in locations with higher/lower area code middle digits, so the average area code response might not be an informative measure of dishonesty.

	Low Score (0-9)	Medium Score (10-17)	High Score (18-32)
Democrat	68	50	45
Republican	13	16	23
Independent	22	29	37
Bachelor's (or more)	65	48	49
Income ($100,000+)	35	22	27
Employed	62	57	66
Male	50	46	52
White	77	61	69
Urban	77	70	75
Median Age	54	41	35
N	103	95	105

Table 1: Demographic Summary Statistics by Conspiracy Score

3 Data

303 subjects participated in our online survey in September 2021. Subjects were recruited via the online recruiting service, *Prolific Academic*. A breakdown of the summary statistics for our sample can be found in Table 1. For our specification, we group subjects into one of three categories based on their score on the conspiracy ideation questions: low score (0-9), medium score (10-17), and high score (18-32). The score cutoffs were selected so that each category had as close to one-third of the subject sample as possible, without having any overlap in scores. Looking at Table 1, we see that the main demographics that vary some across categories are political affiliation, education, and age. Such variation is in line with prior literature on the demographics of conspiracy theory endorsers. Regarding political affiliation, the existing literature (e.g. Douglas et al. [2019], Smallpage et al. [2020]) suggests that identifying as an independent tends to be positively correlated with being a conspiracy theory endorser. Furthermore, this literature suggests that those on the losing end of the political process (e.g. in elections) tend to be more likely to endorse conspiracy theories. As such, we are not surprised by the negative correlation between the conspiracy ideation score and identifying as a democrat, nor with the positive correlation between the conspiracy ideation score and identifying as a republican or independent. The aforementioned literature also suggests that there is a negative correlation between conspiracy ideation scores and education, which we see in our data. Finally, Galliford and Furnham [2017] suggests that conspiracy theory endorsers tend to be younger, which is reflected in the median age of those in each of our categories.

4 Results

Recall that our fundamental question is whether there are general behavioral differences between conspiracy theory endorsers and others. To address this question, we want to first summarize the outcomes of our behavioral measures across conspiracist ideation scores. This

Conspiracist Ideation Score	Low (0-9)	Medium (10-17)	High (18-32)
Mean Total Political Question Response	5.57	7.00	8.01
% FM Choosing Continue (Trust)	56.60%	73.47%	65.38%
% SM Choosing Continue (Trust)	72.00%	54.35%	75.47%
FM Median Belief about SM (Trust)	50-60%	60-70%	50-60%
SM Median Belief about FM (Trust)	50-60%	50-60%	50-60%
% Always Take (Entitlement)	16.36%	15.22%	23.40%
% Reporting "9" (Dishonesty)	4.85%	6.32%	10.48%
Mean Reported # of Heads (Dishonesty)	2.80	2.81	2.70
% Choosing to Test (Own Judgement)	23.30%	24.21%	28.57%
% Tester Chose Correct (Own Judgement)	60.87%	70.83%	63.33%
Average # of Valid Uses (Creativity)	6.02	4.34	4.31
Average # of Unique Uses (Creativity)	2.98	1.86	1.92
Average Earnings	$4.71	$4.42	$4.49

Table 2: Behavioral Measures Summary

will give us the broad picture of how conspiracy theory endorsers behave compared to others. In Table 2, we summarize the outcomes of the factual political questions and the incentivized tasks. Given demographic differences by conspiracist ideation score, we will ultimately need to conduct regression analysis to test for differences, controlling for demographic variables. That said, the information in Table 2 gives us an indication about the structure of our data. In the first row, we report the average total response in the factual political questions for the three conspiracy score categories. As we can see, those with low scores on the conspiracy ideation questions tended to give answers that were more congenial towards democrats, while those with high scores tended to give answers that were more congenial towards republicans. On average, those with medium scores did not tend to give answers that favored either democrats or republicans. Looking at the outcomes for the incentivized tasks, we notice that for the most part, there is not much of a difference between the average outcomes of those with low scores and those with high scores. There are a couple of cases, specifically the outcomes for the Creative Uses Task and the responses to the area code question, where there could possibly be a significant difference, but overall the outcomes look similar. So, the summary of our measures suggests that if there are any general behavioral differences between conspiracy theory endorsers and others, these differences are small and seemingly not economically significant, contrary to the majority of our hypotheses. Now that we have the broad picture that conspiracy theory endorsers do not seem to have significant behavioral differences from others, we will run regressions with control variables to make sure this suggestion from our summary holds up to more careful analysis.

In our regression analysis, we want to run regressions for each of our stated hypotheses. The structure for each of our regressions is the same, the only thing changing across regressions is the dependent variable (and as a consequence, whether we utilize an OLS regression or a logit/probit regression). For these regressions, we utilize the following set of explanatory variables. Our main variables of interest are indicator variables for having a

medium conspiracy score and having a high conspiracy score. We therefore use those with a low conspiracy score as the reference group. The control variables we include are indicator variables for identifying as a republican and identifying as an independent, an indicator variable for having at least a Bachelor's degree, the subject's self-reported risk preferences, and the subject's age. We chose these as our set of control variables because these were the demographics that differed most across conspiracist ideation scores. Therefore, if the coefficients on the conspiracist ideation score indicator variables are significantly different from zero, we would have indication of a pure effect of being a conspiracy theory endorser rather than the effect of a (negatively) correlated demographic like education or age. We will now go through the result statements for each of our hypotheses.

Result 1. *(Trust) Conspiracy theory endorsers in the position of "First Mover" do not choose "Continue" less often than others.*

In column (2) of Table 3, we have the results of the probit regression for the behavior of first movers in our trust game. As we can see, the coefficients on our conspiracy variables are not statistically significant, suggesting that conspiracy theory endorsers were not significantly less trusting than others, which is contrary to our hypothesis.

Result 2. *(Trustworthiness) Conspiracy theory endorsers in the position of "Second Mover" do not choose "Continue" less often than others.*

In column (3) of Table 3, we have the results of the probit regression for the behavior of second movers in our trust game. As we can see, the coefficients on our conspiracy variables are not statistically significant, suggesting that conspiracy theory endorsers were not significantly less trustworthy than others, which is contrary to our hypothesis.

Result 3. *(Subjective Beliefs) Conspiracy theory endorsers' elicited beliefs about the percentage of second movers choosing "Continue" is not lower than the elicited beliefs of others.*

In columns (4) and (5) of Table 3, we have the results of the OLS regressions for the belief elicitation task for our trust game. As we can see, the coefficients on our conspiracy variables are not statistically significant, which suggests that conspiracy theory endorsers did not have significantly more pessimistic beliefs than others, contrary to our hypothesis.

Result 4. *(Entitlement) Conspiracy theory endorsers who picked Painting 1 (i.e. "losing subjects") are not more likely take the* \$1 *prize than others.*

In column (6) of Table 3, we have the results of the probit regression for the behavior of losing subjects in our entitlement game. As we can see, the coefficients on our conspiracy variables are not statistically significant, suggesting that conspiracy theory endorsers were not significantly more entitled than others, which is contrary to our hypothesis.

Result 5. *(Dishonesty 1) Conspiracy theory endorsers do not report "9" as their area code's middle digit more often than others.*

Result 6. *(Dishonesty 2) Conspiracy theory endorsers do not report a higher average number of heads than others.*

In column (1) of Table 4, we have the results of the probit regression for the behavior of subjects in our area code task. In column (2) of Table 4, we have the results of the OLS regression for the behavior of subjects in our coin flip task. As we can see for both tasks, the coefficients on our conspiracy variables are not statistically significant, suggesting that conspiracy theory endorsers were not significantly more dishonest than others, which is contrary to our hypothesis.

Result 7. *(Relying on Own Judgement) Conspiracy theory endorsers do not choose to test the options more often than others.*

In column (3) of Table 4, we have the results of the probit regression for the behavior of subjects in our modified two-armed bandit game. As we can see, the coefficients on our conspiracy variables are not statistically significant, suggesting that conspiracy theory endorsers were not significantly more likely to rely on their own judgements compared to others, which is contrary to our hypothesis. In light of this null result, we further test whether, for those who did test the options for themselves, conspiracy theory endorsers were more likely to choose the correct option (Option B). The results of this probit regression are in column (4) of Table 4. As we can see, conspiracy theory endorsers who tested the options for themselves were not more likely to choose the correct option.

Result 8. *(Creativity) Conspiracy theory endorsers provide fewer valid/unique uses than others.*

In columns (5) and (6) of Table 4, we have the results of the OLS regressions for the behavior of subjects in the Creative Uses Task. As we can see, the coefficients on our conspiracy variables are in fact statistically significant, but they are negative, suggesting that conspiracy theory endorsers were *less* creative than others, which is contrary to our hypothesis that conspiracy theory endorsers would be *more* creative than others.

Result 9. *(Political Bias) The average answer conspiracy theory endorsers give to the factual political questions is higher than the average answers of others.*

In column (1) of Table 3, we have the results of the OLS regression for our factual political questions. As we can see, even when controlling for political affiliation, conspiracy theory endorsers provide answers to the factual political that are a higher than others, which matches with our hypothesis.

5 Conclusion

Given the concern present in media pieces about conspiracy theory belief in the United States, we need to better understand whether conspiracy theory endorsers are behaviorally different from others as prior authors have claimed, as such behavioral differences could have real consequences such as in the workplace. In this study, we implemented an incentivized online survey designed to test for behavioral differences between conspiracy theory endorsers and others in the areas of interpersonal trust, (psychological) entitlement, dishonesty, propensity to rely on one's own judgement, and creativity. To be able to test for behavioral differences

VARIABLES	(1) Political Bias	(2) Trust	(3) Trustworthy	(4) FM Belief	(5) SM Belief	(6) Entitled
Medium Score	1.161***	0.358	-0.538*	-0.695	-0.131	-0.017
	(0.389)	(0.273)	(0.282)	(0.473)	(0.521)	(0.327)
High Score	1.945***	-0.028	0.090	-0.442	-0.620	0.345
	(0.397)	(0.276)	(0.294)	(0.494)	(0.523)	(0.327)
Republican	3.337***	-0.063	0.432	1.276**	1.430**	-0.103
	(0.436)	(0.292)	(0.346)	(0.512)	(0.602)	(0.331)
Independent	0.944***	-0.029	-0.188	0.433	0.547	-0.732**
	(0.362)	(0.268)	(0.257)	(0.463)	(0.476)	(0.342)
Bachelor's (or more)	-0.520	0.232	0.072	0.275	0.691	0.064
	(0.323)	(0.217)	(0.260)	(0.377)	(0.470)	(0.242)
Risk Preferences	0.068	0.051	-0.111**	-0.001	0.198*	0.063
	(0.071)	(0.047)	(0.057)	(0.083)	(0.103)	(0.060)
Age	0.009	-0.018***	-0.011	-0.019	-0.020	0.005
	(0.010)	(0.007)	(0.008)	(0.012)	(0.014)	(0.008)
Observations	303	154	149	154	149	148

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Political Bias, Interpersonal Trust, and Entitlement

VARIABLES	(1) Lie Area	(2) Lie Coin	(3) Own Judge	(4) Correct Judge	(5) Creative Valid	(6) Creative Unique
Medium Score	0.071	-0.007	0.035	0.149	-1.582***	-1.122***
	(0.313)	(0.145)	(0.205)	(0.432)	(0.524)	(0.314)
High Score	0.251	-0.142	0.175	-0.263	-1.558***	-1.065***
	(0.306)	(0.148)	(0.207)	(0.424)	(0.536)	(0.321)
Republican	0.173	-0.111	0.175	-0.512	0.163	0.005
	(0.320)	(0.163)	(0.218)	(0.399)	(0.588)	(0.352)
Independent	-0.198	-0.240*	0.015	0.624	1.662***	0.893***
	(0.293)	(0.135)	(0.191)	(0.441)	(0.488)	(0.293)
Bachelor's (or more)	0.709**	0.073	-0.092	-0.121	0.086	0.165
	(0.275)	(0.121)	(0.168)	(0.341)	(0.436)	(0.261)
Risk Preferences	0.032	0.073***	0.057	0.131	0.011	0.059
	(0.057)	(0.026)	(0.037)	(0.083)	(0.096)	(0.057)
Age	-0.020**	-0.002	0.010*	-0.004	0.037***	0.014*
	(0.009)	(0.004)	(0.005)	(0.011)	(0.014)	(0.008)
Observations	303	303	303	77	303	303

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Dishonesty, Relying on Own Judgement, and Creativity

Measure	Hypothesis	Result
Political Bias	+	+
Trust	-	0
Trustworthiness	-	0
Trust Game Belief	-	0
Entitlement	+	0
Dishonesty	+	0
Relying on Own Judgement	+	0
Creativity	+	-

Table 5: Hypothesized Sign Versus Result for Each Measure

in some of these areas, namely the areas of entitlement, dishonesty, and the propensity to rely on one's own judgement, we needed to construct novel measures of our own design. To measure for entitlement, we constructed a dynamic game where a "losing" subject could choose to take a prize that subjects were told was intended for a "winning" subject. A crucial feature of this game is that it can distinguish between standard selfish preferences and entitlement. To measure for dishonest behavior, on top of a standard coin-flip task, we constructed a second task based on a little known feature of U.S. area codes, namely that no U.S. area codes have "9" as the middle digit. Since subjects were likely lying on the coin-flip task by default, having this second task allowed subjects to be completely honest if they desired. To measure for a propensity to rely on one's own judgement, we constructed a modified version of the two-armed bandit game where subjects could either accept the advice of someone else, or pay a small fee to test the options for themselves. As a way to test for the strength of a propensity to rely on one's own judgement, *if* one exists, we randomized the size of the fee subjects had to pay.

From our data, we found that while conspiracy theory endorsers tended to give more extreme answers to the factual political questions, as we hypothesized, they did not generally behave differently from others in our incentivized tasks. Table 5 provides a summary of how our hypotheses compared to our results. For each of our behavioral measures, we hypothesized that conspiracy theory endorsers would be significantly different from others. For instance, we hypothesized that conspiracy theory endorsers would be less (-) trusting than others, and that conspiracy theory endorsers would be more (+) dishonest than others. As we can see from Table 5, for the majority of our behavioral measures, we instead found a null result (0). These results are striking considering that our expectations, which were backed by the existing literature regarding the behavior of conspiracy theory endorsers, were that conspiracy theory endorsers would significantly differ in each of our behavioral measures. The primary implication of our findings is that, when it comes to behaviors relevant to the workplace and broader economic growth, if there are any general behavioral differences between conspiracy theory endorsers and others, these differences are small and seemingly not economically significant. As such, conspiracy theory belief might not have much direct impact on outcomes in the workplace or broader economic growth. To be clear, we are not claiming that a growth in conspiracy theory belief would be inconsequential to American society, only that conspiracy theory endorsers do not seem to have large behavioral difference

from others that would be directly pertinent to the workplace or broader economic growth. With this implication in mind, our results point us towards a couple of avenues for further research into the behavior of conspiracy theory endorsers.

One possible avenue for further investigation relates to the creativity of conspiracy theory endorsers. Although we found a null result for the majority of our incentivized behavioral measures, one case where the coefficients were statistically significant was the Creative Uses Task. The first thing to point out here, as we can see in Table 5, is that our hypothesis was that conspiracy theory endorsers would be *more* (+) creative than others. However, the coefficients on the conspiracy variables for this task were negative. So, relative to our expectation, we fail to reject the null hypothesis that conspiracy theory endorsers are not *more* creative than others. This is similar to the conclusions we make from the results for the other incentivized behavioral measures. However, since the coefficients are statistically significant, there is potentially a stronger interpretation that conspiracy theory endorsers are *less* creative than others. Under the assumption that this stronger interpretation is correct, we can first point out how the creative uses task is slightly different from the other incentivized behavioral measures. While for the other incentivized behavioral measures we are measuring whether a subject takes a particular action (e.g. whether a subject chooses "End" or "Continue" in the trust game), for the Creative Uses Task, we are measuring a subject's ability. Related to the ability of conspiracy theory endorsers, Brotherton and French [2014] suggests that conspiracy theory endorsers are more likely to commit the conjunction fallacy than others. So, there is an existing literature suggesting that the ability of conspiracy theory endorsers might be different from others. Having said that, we should not overemphasize this single set of significant coefficients. Perhaps these significant coefficients are meaningful, but the appropriate conclusion from our results is that the general behavior of conspiracy theory endorsers does not tend to widely differ from the general behavior of others in these behavioral areas relevant to the workplace and broader economic growth. To be able to say more about the creativity of conspiracy theory endorsers, we need to conduct further research directly focused on testing for the creativity of individuals.

Another potential avenue for future research is to investigate whether conspiracy theory endorsers might be different from others in the behavioral areas we studied, focusing on specific contexts rather than general behavior. For instance, our study does not find any large differences in general trust between conspiracy theory endorsers and others, but the notion of a difference is still intuitively appealing. One possibility is that while conspiracy theory endorsers might not be less trusting in general, they might be less trusting of specific types of people. One could conduct a separate study focused on testing for differences in trust between conspiracy theory endorsers and others in specific contexts. We also do not find that conspiracy theory endorsers have a higher general propensity to rely on their own judgement, but perhaps conspiracy theory endorsers are more likely to rely on their own judgement in specific contexts (e.g. politically charged contexts like climate change). One could also conduct a separate study where we place subjects into stylized contexts to test whether conspiracy theory endorsers might have a higher propensity to rely on their own judgements in specific contexts. What our study contributes to this literature on the behavior of conspiracy theory endorsers is to narrow the possibility of economically significant behavioral differences into smaller categories. Our study suggests that if there are any general differences between conspiracy theory endorsers and others in these behavioral areas,

they are small and not economically significant, so we can approach the next step of testing for behavioral differences in specific cases if we have good reason to think conspiracy theory endorsers will differ from others in those specific contexts.

References

James Andreoni and Alison Sanchez. Fooling myself or fooling observers? avoiding social pressures by manipulating perceptions of deservingness of others. *Economic inquiry*, 58 (1):12–33, 2020.

Saurabh Bhargava, George Loewenstein, and Justin Sydnor. Choose to lose: Health plan choices from a menu with dominated option. *The Quarterly Journal of Economics*, 132 (3):1319–1372, 2017.

Iris Bohnet and Richard Zeckhauser. Trust, risk and betrayal. *Journal of Economic Behavior & Organization*, 55(4):467–484, 2004.

Eric Bonetto and Thomas Arciszewski. The creativity of conspiracy theories. *The Journal of Creative Behavior*, 2021.

Robert Brotherton, Christopher C. French, and Alan D. Pickering. Measuring belief in conspiracy theories: The generic conspiracist beliefs scale. *Frontiers in psychology*, 4:279, 2013.

Sarah Brown, Daniel Gray, Jolian McHardy, and Karl Taylor. Employee trust and workplace performance. *Journal of Economic Behavior & Organization*, 116:361–378, 2015. doi: https://doi.org/10.1016/j.jebo.2015.05.001. URL `https://www.sciencedirect.com/science/article/pii/S0167268115001365`. ID: 271649.

Aleksandra Cichocka, Marta Marchlewska, Agnieszka Golec de Zavala, and Mateusz Olechowski. 'they will not control us': Ingroup positivity and belief in intergroup conspiracies. *British Journal of Psychology*, 107(3):556–576, 2016.

James C. Cox. How to identify trust and reciprocity. *Games and Economic Behavior*, 46 (2):260–281, 2004.

David L. Dickinson and David M. McEvoy. Further from the truth: The impact of moving from in-person to online settings on dishonest behavior. *Journal of Behavioral and Experimental Economics*, 90:101649, 2021.

Karen M. Douglas and Robbie M. Sutton. Does it take one to know one? endorsement of conspiracy theories is influenced by personal willingness to conspire. *British Journal of Social Psychology*, 50(3):544–552, 2011.

Karen M. Douglas, Joseph E. Uscinski, Robbie M. Sutton, Aleksandra Cichocka, Turkay Nefes, Chee Siang Ang, and Farzin Deravi. Understanding conspiracy theories. *Political Psychology*, 40:3–35, 2019.

E. G. Dutcher and Cortney S. Rodet. Which two heads are better than one?: Uncovering the positive effects of diversity in creative teams. *Unpublished Manuscript (November 12, 2018)*, 2018.

Adam M. Enders, Joseph E. Uscinski, Casey A. Klofstad, Michelle I. Seelig, Stefan Wuchty, Manohar N. Murthi, Kamal Premaratne, and John R. Funchion. Do conspiracy beliefs form a belief system? examining the structure and organization of conspiracy beliefs. *Journal of Social and Political Psychology*, 9(1):255–271, 2021.

Natasha Galliford and Adrian Furnham. Individual difference factors and beliefs in medical and political conspiracy theories. *Scandinavian Journal of Psychology*, 58(5):422–428, 2017.

Ted Goertzel. Belief in conspiracy theories. *Political Psychology*, 15(4):731–742, 1994. doi: 10.2307/3791630. URL `http://www.jstor.org/stable/3791630`. 23.

Yaping Gong, Jing Zhou, and Song Chang. Core knowledge employee creativity and firm performance: The moderating role of riskiness orientation, firm size, and realized absorptive capacity. *Personnel Psychology*, 66(2):443–482, 2013.

Carol Graham, Robert E. Litan, and Sandip Sukhtankar. *The bigger they are, the harder they fall: an estimate of the costs of the crisis in corporate governance*. Brookings Institution Washington, DC, 2002.

Jaron Harambam and Stef Aupers. 'i am not a conspiracy theorist': Relational identifications in the dutch conspiracy milieu. *Cultural Sociology*, 11(1):113–129, 2017.

Paul Harvey and Kenneth J. Harris. Frustration-based outcomes of entitlement and the influence of supervisor communication. *Human Relations*, 63(11):1639–1660, 2010.

Paul Harvey, Kenneth J. Harris, William E. Gillis, and Mark J. Martinko. Abusive supervision and the entitled employee. *The Leadership Quarterly*, 25(2):204–217, 2014.

Nicolas Jacquemet, Alexander G. James, Stéphane Luchini, James J. Murphy, and Jason F. Shogren. Do truth-telling oaths improve honesty in crowd-working? *PloS one*, 16(1): e0244958, 2021.

Daniel Jolley, Karen M. Douglas, Ana C. Leite, and Tanya Schrader. Belief in conspiracy theories and intentions to engage in everyday crime. *British Journal of Social Psychology*, 58(3):534–549, 2019.

Joanne M. Miller, Kyle L. Saunders, and Christina E. Farhart. Conspiracy endorsement as motivated reasoning: The moderating roles of political knowledge and trust. *American Journal of Political Science*, 60(4):824–844, 2016. doi: https://doi.org/10.1111/ajps.12234. URL `https://doi.org/10.1111/ajps.12234`. https://doi.org/10.1111/ajps.12234; 23.

Andrea Robbett and Peter Hans Matthews. Partisan bias and expressive voting. *Journal of Public Economics*, 157:107–120, 2018.

Anna D. Sinaiko and Richard A. Hirth. Consumers, health insurance and dominated choices. *Journal of health economics*, 30(2):450–457, 2011.

Steven M. Smallpage, Hugo Drochon, Joseph E. Uscinski, and Casey Klofstad. *Who are the Conspiracy Theorists?: Demographics and conspiracy theories*, pages 263–277. Routledge handbook of conspiracy theories. Routledge, 2020.

Michael J. Wood, Karen M. Douglas, and Robbie M. Sutton. Dead and alive: Beliefs in contradictory conspiracy theories. *Social psychological and personality science*, 3(6):767–773, 2012.