Trustful Voters, Trustworthy Politicians: A Survey Experiment on the Influence of Social Media in Politics

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Abstract

Is uncivil discourse in social media reducing political trust among voters? Recent increases in uncivil dialogue and political polarization in social media raise questions about the relationship between negative online messages and the decline in political trust. We implement a *trust game* in a survey experiment with 4,800 respondents in Brazil and Mexico. Our design models the effect of social media on *trust* and *trustworthiness*. Survey respondents alternate as agents (*politicians*) and principals (*voters*), with rewards contingent on their preferred "candidate" winning the election. We measure the extent to which voters place their *trust* in others and are themselves *trustworthy*, that is, willing to honor requests that may not benefit them. Results provide robust support for a negative effect of uncivil partisan discourse on trust behavior and null results on trustworthiness.

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1 Trustful Voters and Trustworthy Politicians

Is uncivil discourse in social media reducing political trust among voters? Is social media engagement making voters less trustworthy? The increases in uncivil dialogue (Mason, 2016; Iyengar, Sood and Lelkes, 2012) and polarization (Banks et al., 2020; Bail et al., 2018) raise questions about the relationship between online exposure and the recent decline in political trust. In this article we report results of an experiment that shows significant declines in trust behavior among users exposed to, and who engaged with, uncivil partisan messages in social media.

Understanding the effect of social media exposure on trust is substantively and theoretically important. Political trust is critical to citizens' commitment to the rule of law, norms and regulations, and democracy. Research shows that politicians perceived as trustworthy are associated with increases in political engagement, higher voter turnout, citizen support for existing policies, support for institutional reforms, compliance with political authorities, and reciprocity (Levi and Stoker, 2000). Unfortunately, measures of trust have shown steady declines across the world over the past decade (Keefer, Scartascini and Vlaicu, 2018; Murtin et al., 2018; Scartascini and Valle L., 2020). These declines are concurrent with the rise of social media as a dominant platform for interpersonal communication and for the delivery of political news. However, there is little research that tests for the relationship between social media exposure, social media engagement, trust, and trustworthiness.[1]

[1]See Witmer and Håkansson (2015) for an overview of this discussion.

In this paper, we describe a survey experiment that implements a variant of the well-known *trust game* in an electoral context.[2] We test whether respondents *trust* others to act on their behalf and whether they are *trustworthy* with respect to the resources entrusted to them. Trusting behavior increases the potential rewards perceived by the participants (more votes) but may carry large costs if the players' trust is betrayed (that is, if their candidate loses the election). By providing prizes associated with winning the election and raffle tickets for each vote they contribute to the win, we ensure that incentives are aligned as in the traditional trust game. After an initial round of the game, we randomly treat a subset of respondents to negative and positive tweets from incumbent and opposition politicians and measure changes in *trust* and *trustworthy* behaviors. We expect negative tweets from out-group politicians to activate partisan identities, even if those identities are orthogonal to the actual game being played. We implement our survey experiments using two large, randomized panels of 2,400 Brazilian and Mexican respondents each.

Results show robust and statistically significant declines in trust among voters exposed to negative messages from out-group political figures (*dissonant messages*). Findings also support an activation of partisan identities and higher memory availability (Kahneman, 2011) of polarization frames.[3] Results are more modest when only the negative tone of the social media post (*uncivil discourse*) is considered. Finally, we do not find a statistically significant effect on trustworthiness. Agents cast entrusted votes at the same rate,

[2] *Trust games* are a well-established methodological strategy for studying economic exchanges in low-information environments (Berg, Dickhaut and McCabe, 1995).

[3] As shown by Bail et al. (2018), exposure to counter-attitudinal arguments may create a "backfire" effect that increases political polarization and induces motivated reasoning on the users' side. Our study builds on these findings to show similar negative effects on interpersonal trust

regardless of the treatments they received. Overall, results provide support for a negative effect of social media exposure on trust but no support for a decline in trustworthiness.

Further testing of our findings indicates that "dosage" matters. We find that incidental exposure to social media has modest effects on trust. Results are pronounced and statistically significant at higher levels of engagement with tweets. Differences in trust between the control and treatment group are great when they "do" Twitter (like, retweet, reply) as opposed to when they "read" it (no engagement). These findings are new and important, as they point to differences between social media platforms and more traditional news outlets.[4] Our findings support "anger" as an important mediator in reducing trust, in line with recent research on negative emotions and polarization (Mason, 2016; Banks, 2014; Banks, White and McKenzie, 2019).

In all, our work offers three novel contributions to scholars interested in the study of social media, trust, and democratic governance. First, we find that social media exposure leads to declines in trust behavior rather than a mere change in attitudes.[5] The decline in trust behavior is self-interested and cannot be explained by the desire of the respondents to interpret the intent of the survey instruments.

Second, we show conclusively that social media engagement magnifies the effect of the experimental treatment. There is a larger decline in trust among respondents who

[4]Research in political communication describes several other important differences, including changes in editorial gatekeeping, in the practices and routines of journalists, and in exposure to different news frames propagated by peers (Shoemaker and Reese, 2013; Tandoc, 2014; Aruguete and Calvo, 2018). Showing that engagement is an important mediator in reducing trust also contributes to current research on incidental consumption of news (Boczkowski, Mitchelstein and Matassi, 2018), indicating that the consequences of incidental exposure may be more modest than previously thought.

[5]See Tucker et al. (2018) for an overview of the recent literature on social media

shared the content of the treatment, compared with those who were simply exposed to partisan messages. This is critically relevant for the burgeoning literature on incidental exposure to news (Boczkowski, Mitchelstein and Matassi, 2018; Fletcher and Nielsen, 2018). Our two-way design, comparing engaged and nonengaged users in the randomized experiment, supports the view that sharing behavior increases the negative effect of social media treatments.

Third, we contribute methodologically to the study of *trust games*, presenting a survey design that replicates important behavioral responses from in-person lab experiments. Our design brings the trust game from the more traditional "investment" setting to an electoral political scenario, which could prove useful for understanding the role of trust in voter-politician interactions. Although the use of online survey experiments reduces the number of measurements taken from each treated individual, it can be rapidly scaled up, rendering results with higher external validity.

The organization of this paper is as follows. First, we describe the substantive importance of testing for the relationship between social media exposure, trust, and trustworthiness. Second, we present our experimental design and its implementation in Mexico and Brazil. Third, we present our general experimental results, with estimates that distinguish between partisan cognitive dissonance, negative tone of the content, and sharing behavior. Fourth, we describe extensions of our results that describe the mediating effect of negative emotions on trust. We conclude with a discussion of possible further extensions of our work.

2 Trust and Trustworthiness

Beginning with the work of Adam Smith, trust and trustworthiness have been recognized as key factors in promoting cooperation and exchange (Smith, 1937). Trust and trustworthiness are fundamental forces that shape societies and institutions (formal and informal) and co-evolve with them (Arrow, 1974; Guiso, Sapienza and Zingales, 2004). Trust and trustworthiness have positive effects on the ability of people to make transactions and on the ability of governments to function (Arrow, 1974; Knack and Keefer, 1997; Gambetta, 1988; Jacobsen, 1999; Zak and Knack, 2001; Algan and Cahuc, 2014; Bjørnskov and Méon, 2015; Algan et al., 2017). High trust correlates with higher growth, social progress, and democratic stability (Algan and Cahuc, 2010, 2014; Aghion et al., 2010; Keefer et al., 2020). Importantly, if citizens do not trust their governments, they will not demand public goods or policies whose benefits materialize only in the long run (Keefer, Scartascini and Vlaicu, 2018; Keefer et al., 2020; Scartascini and Valle L., 2020).

Studying trust has become ubiquitous in recent years. Most studies use well-known survey questions that measure *trust attitudes* rather than *trust behavior*.[6] This is problematic, as there is consistent evidence that trust attitudes and trust behavior are weakly correlated (Wilson, 2017). Importantly, the analytical connection between the social benefits of trust and trustworthiness makes sense in terms of behaviors rather than attitudes. This point is forcefully made by the literature on transaction costs, which establishes the positive effects of individuals who place their trust in others, irrespective of whether they

[6]Examples include agreement questions such as "Most people can be trusted" as well as scale questions of reported trust in family, friends, and neighbors.

agree that "most individuals can be trusted" (Bloom et al., 2012).

In the last two decades, *trust games* have revolutionized the field of behavioral economics and political science, generating data on trust and trustworthy behavior rather than reports on attitudes, which are frequently vitiated by misreporting and desirability biases (Berg, Dickhaut and McCabe, 1995). In the traditional *trust game*, individual trust behavior is studied in group settings where cooperation leaves everyone better off and self-interested behavior can make everyone worse off. In these laboratory games, one individual has an initial endowment that she can retain or pass on to a second individual. The amount she passes on ("invests") is multiplied (usually by 3) by the time it reaches the second individual. A second individual can keep all the receipts or reciprocate by sending back part or all. The amount passed by the sender is said to capture *trust*, and the amount returned to the trustor by the trustee to capture *trustworthiness* (Camerer and Loewenstein, 2003).

Democratic representation is a particular type of trust game in which a *principal* (the voter) sends her vote to an *agent* (the politician), who will then act on her behalf. The *principal-agent* relationship is a difficult one, with decisions made by a politician often hidden from the public's view. This raises the specter of abuse by officeholders, who are expected to be principled and to fulfill their mandates even if these do not align with their preferences or interests. We expect politicians to be *worthy of our trust*, although they frequently deceive us (Hardin, 2002). We also consider ourselves to be *worthy of the trust of others*, although we are often willing to explain away why we default on our promises (Ariely and Jones, 2012). Using this idea of democratic representation as a type of trust

game, our paper models how exposure to social media renders changes in interpersonal trust and trustworthiness behavior. [7]

2.1 Nuts and Bolts of the Electoral Trust Game

In the political trust game that we embedded in our survey experiment, each respondent selects one of two fictional candidates. Respondents are informed that they must collect votes for their candidate of choice throughout the survey. At the end of the survey, those who chose the candidate who won the most votes are allowed to enter a raffle for two new iPads. Respondents are also informed that their number of raffle entries will be equal to the number of votes they contributed to their candidate. Therefore, collecting as many votes as possible is incentive-compatible: it increases the chances that their candidate will win *and* it increases their chances of winning an iPad.

In the survey, respondents act the parts of a politician (agent) and a voter (principal). When answering questions as the agent (*politician*), respondents are asked to cast or discard votes entrusted to them (trustworthiness). When answering in the role of the principal (*voters*), respondents must decide how many votes to cast directly (one vote cast equals one raffle entry) and how many to entrust to another player (one entrusted vote that is eventually cast equals two votes and two raffle entries). However, they are warned that the agent (politician) may discard those votes; these are the same choices they themselves have when playing the other role. To ensure that there is no deception,

[7] For a general discussion of trust and trustworthiness, see Hardin (2002). In Hardin, trustworthiness is described as an instrumental trait, where a politician seeks to build a reputation over repeated interactions. Similar descriptions of trustworthiness as reciprocity are found in Croson and Buchan (1999) and Fehr and Gächter (2000)

our team serves as a universal player that honors all votes, those cast directly as well as those entrusted to another player.

Following Mazar and Ariely (2006), and in order to approximate better the relationship between representatives and voters, the role of the agent is reinforced by an initial pledge: "If other players delegate (entrust) their votes to me, I agree to follow their preferences and to use them to support the candidate of his or her choosing." After reading the initial statement, they are given five votes in support of their candidate of choice. The pledge is not binding. Representatives are required to read it but need not promise to comply with it; nor are there any sanctions for defaulting.

For the voter, placing her *trust* in another player (the agent) offers the possibility of doubling the votes her candidate receives (as well as the raffle tickets that she herself earns). However, the other player may decide not to cast those votes, something that is made clear at the outset. For the agent, being *trustworthy* is the act of casting delegated votes, even if those votes serve the candidate whom the respondent does not support (thereby lowering the likelihood of winning the raffle). As we will show, there are good theoretical reasons for *trust* and *trustworthiness* to be weakly correlated, as they express different types of cognitive beliefs about oneself (*belief-based guilt*) and about the other players (*first-order beliefs*).

2.2 Framing, *Belief-Based Guilt*, and *First-Order Belief*

Why would respondents cast delegated votes if doing so reduces their chances of winning the election and the raffle? Why would respondents delegate votes onto others if

there is no reassurance that these votes will be cast? Given that there is no accountability in this survey, or in many democratic polities, one of the potential explanations for why agents fulfill the mandate entrusted to them is *guilt aversion*, a concept to which we now turn. Understanding *guilt aversion* also provides clear mechanisms to model the effect of social media frames on *trust* and *trustworthiness*.

Trustworthiness and Guilt Aversion

Battigalli and Dufwenberg (2007) coin the term *guilt aversion* to describe the psychological cost of letting other people down: "Player i's guilt may depend on how much he lets j down. Player i's guilt may also depend on how much j believes i believes he lets j down."(Battigalli and Dufwenberg, 2007, pg. 170). In the absence of information about player j, *simple guilt* emerges, where the decision to cast the entrusted votes is a function of the perceived rewards, m_i, of defaulting on the request made by others, and the general guilt sensitivity, $\theta_i G_{ij}$, when no other information exists about the principal, j.

$$Pr(TWY_{ij}) = \phi(m_i - \theta_i G_{ij}) \tag{1}$$

In equation 1, the probability that we will be *trustworthy* is affected by the subjective value of the reward m_i and by the guilt sensitivity θ_i of *agent* i for a generic *principal* j. We are agnostic about the social, political, or psychological origins of "guilt" and consider the guilt parameter, θ_i, as a placeholder for a simple aversion to defaulting on a mandate. Therefore, simple guilt describes the individual's propensity to act on a generic request. This θ_i parameter is sensitive to a number of exogenous shocks. Framing effects are one

possible mechanism that modulates the guilt sensitivity parameter, θ_i.

For example, consider the experiment proposed by Ariely and Jones (2012), wherein respondents are asked to "read a pledge" before being given the chance to cheat. We may think of this pledge as a heuristic device that increases the relative value of θ_i, the cost of *"letting other people down"*.[8] We expect negative messages from out-group politicians to increase negative feelings toward others (Mason, 2016; Banks, 2014), thereby reducing the value of θ_i (and reducing trustworthy responses). This expectation follows from the literature on generic or procedural frames, where the way in which a problem is presented alters the perceived legitimacy of an actor (Entman, 1993) or event (Iyengar, 1990).

Our approach to trustworthiness differs from the classification proposed by Ashraf, Bohnet and Piankov (2006), who distinguish between "unconditional kindness,', "expectations of reciprocity," and "[instrumental] reciprocity." In our case, guilt is the result of defaulting on a request from another respondent. There is no "kindness" in casting entrusted votes; there is no reciprocity expected or information collected about the individual who entrusts votes to be cast; and, finally, there are no instrumental benefits to be gained from being trustworthy.

Trust and First-Order Belief

Unlike *belief-based guilt*, where we pay the cost or reap the benefit of our decision

[8]In our experiment, individuals are offered a "pledge" and the opportunity to click on the option, "I read the pledge." As in Ariely and Jones (2012), respondents who selected this option were considerably more likely to cast the entrusted votes, even though the question only asked respondents to *read* rather than *sign* the pledge. Further, even if respondents believed they were signing rather than reading the pledge, there were no sanctions for defaulting on it. Therefore, the only cost of defaulting accommodates Battigalli and Dufwenberg (2007)'s definition of *simple guilt*.

instantaneously, trust is a cognitive belief about the future behavior of others. When we entrust our votes to others, we do not know if they will comply with our request. If we were to find out that the *principal* failed to cast our votes we would feel betrayed rather than guilty.

Our decision to trust another person depends on how we evaluate the behavior of other respondents, which may or may not be related to our own guilt sensitivity. We expect others to be less trustful as potential gains from deception increase, m_j^*. We also expect others to be less likely to fulfill their promises if they have been remorseless or dishonest in the past. Trust, therefore, is a cognitive belief about other people's behavior, where the subjective gains from m_j^* and the subjective losses from guilt θ_j^* remain unobserved.

Given that *agent* j decides to cast entrusted votes following Equation 1, an action that is unobserved by *principal* i, the share of delegated votes depends on our belief that $m_j^* - \theta_j^* G_{ij} > 0$, a belief that is unrelated to and not informed by our own guilt aversion parameter θ_i. Notice that not even the likelihood of betrayal depends on $\theta_i - \theta_j^* < 0$, given that others defaulting on their promise to be trustworthy is unrelated to how trustworthy we are. We may feel no remorse when defaulting on the mandate we received; at the same time, we may still be outraged by the failure of others to do so. Therefore, as shown in equation 2, our decision to entrust others or to cast votes ourselves depends on unobserved values of how attractive to the other player is the unobserved prize, m_j^*, and how costly the unobserved guilt, θ_j^*.

$$T_{ij} = \alpha V_i + (1 - \alpha) V_i \phi(m_j^* - \theta_j^* G_{ij}) \tag{2}$$

If we assume an empathetic respondent who will do for others what she expects others to do for her, the *golden rule*, trustworthiness and trust would show a weak positive correlation, $cor(\theta_i, \theta_j^*) > 0$. Notice that θ_j^* remains unobserved by i and does not reflect actual information about the principal. Therefore, the value of m_j^* and the simple guilt parameter, θ_j^*, represent expectations of the respondent and not actual behavioral traits of another player.[9]

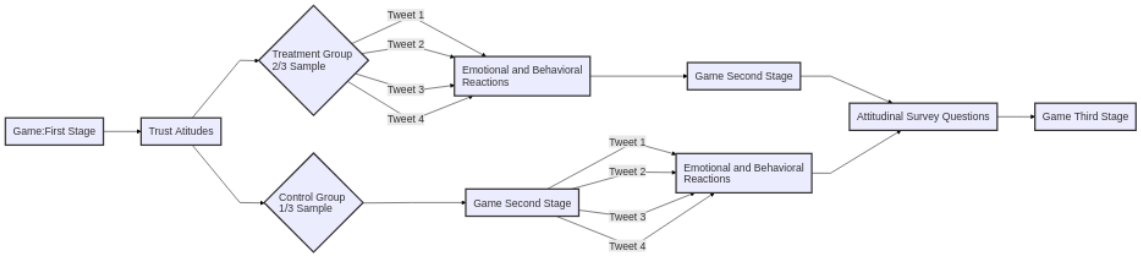
3 Survey Experiment and Hypotheses

In the Supplemental Information File (SIF), we present a full description of the trust game and the embedded framing experiment, including graphics of the survey flow and the execution of each round of the game. Here we present a summary for the readers. Figure 1 illustrates the experimental design and survey flow. We analyze the trustworthy-trust behavior twice, in a baseline round and in a second round. By survey design, respondents' odds of winning a reward are conditional on their candidate winning the overall election and the number of "votes" they contribute to that victory. Fulfilling a request to cast votes for another candidate therefore reduces the likelihood that respondents will personally benefit once the survey is completed.

By comparing the baseline round and the second round, we can measure changes in

[9]In our experiment, the baseline round offers respondents the possibility of duplicating a fraction of the votes entrusted to others. We then treat two-thirds of the respondents to social media frames with the remaining third as controls. Given that we provide no information about the principal, j, and that reward m_i is held constant, we measure the changes in the expected sensitivity of the guilt parameter only in the second round, $\theta_{j,R1}^* - \theta_{j,R2}^*$.

Figure 1 Rounds of the *Trust Game* Embedded in the Survey



Note: All survey respondents take part in the first round. After we measure baseline trust, a third of the respondents are assigned to the control group; the remaining two-thirds are treated with positive and negative political messages from in-group and out-group politicians (four different designs). Emotional and behavioral reactions include propensity to "retweet," "like" or "reply" as well as questions about affective responses ("anger," "joy," "disgust," etc.). The design allows us to isolate the effects of political dissonance and uncivil discourse. Further, in the extensions we provide evidence of behavioral and emotional triggers that reduce trust in respondents in the treatment group but not among the control group.

trust and *trustworthiness*, which depend on the simple guilt parameters, $\theta_j^* G_{ij}$ and $\theta_i G_{ij}$, respectively. Given that the value of the reward (m^*) remains constant and that no other information is provided about likely partners, changes from the first to the second round can occur only through the guilt parameter. Even though θ_j^* remains unobserved, we can measure changes in trustworthiness between rounds 1 and 2, $\theta_{j,2}^* - \theta_{j,1}^*$, because everything else in equation (1) remains constant. Therefore, our model is experimentally distinguished by the use of two rounds administered to treatment and control groups.

After the baseline round, one third of the respondents are set as controls, while two-

thirds are treated with positive and negative political messages (tweets) from in-group and out-group politicians.[10] Negative messages from out-group politicians are expected to decrease *trustworthiness* and *trust*, with social media frames altering the perceived value of the guilt parameters, $[\theta_i, \theta_j^*]$. Our experimental design allows us to compare the baseline trustworthiness and trust of respondents assigned to the treatment and control conditions. We expect "positive" messages to increase baseline trustworthiness and trust, while we expect "negative" messages to decrease trustworthiness and baseline trust.

3.1 Main Hypotheses: Trust and Trustworthiness

The experimental treatments in both Brazil and Mexico present tweets dealing with the COVID-19 crisis. The content of the tweets attributes blame (negative tweet) or signals cooperation (positive tweet). We randomly treat one-third of our sample to negative messages (attribution of responsibility) and one-third to positive messages (interparty cooperation). The control group—the remaining third—is not exposed to social media messages until they have completed the second round. In the two arms of the treatment, we randomly rotate the author of the tweets using two high-level politicians from different (opposing) parties.[11]

[10] The opposition politicians are Fernando Haddad (Brazil) or Francisco Calderon (Mexico), while the incumbent politicians are Eduardo Bolsonaro (Brazil) or Marti Bartres (Mexico). Respondents are informed upon conclusion of the survey that the tweets were edited by our team. Nothing in the tweets constitutes information that, if believed, could harm the respondents. Edited tweets ensure that the treatments are similar in all respects except the endorsement figures and the negative or positive frames. In the Appendix, we present the images and text of the treatments.

[11] All the hypotheses were preregistered, and the preregistration has been uploaded to the AJPS editorial manager for reviewing purposes.

Positive messages report to voters the willingness of political elites to cooperate with rivals to fight the COVID-19 pandemic. The messages signal to respondents the importance of unity and cooperation to manage the crisis. Negative messages blame political opponents for sowing conflict and weakening the needed response to the crisis. These negative tweets activate partisan identities and frame the COVID-19 response as an "us vs. them" problem (Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2015; Mason, 2016). The initial hypotheses of the experiment, as stated, reflect the expectation that positive social media messages will increase trustworthiness and trust, and negative messages will reduce both.

HT_0A**: Positive social media messages increase compliance by agents and trust among principals.**

HT_0B**: Negative social media messages decrease compliance by agents and trust among principals.**

Because positive or negative political messages may be endorsed by politicians who align or not with the preferences of the respondents, we test for the effect of political congruence (in-group) or dissonance (out-group) on *trust* and *trustworthiness*. A broad literature in political behavior shows that partisanship is central to attitude formation in areas as distinctive as candidate evaluation, economic perceptions, support for democracy and authoritarianism, and policy preferences (Green, Palmquist and Schickler, 2004; Arceneaux, 2008; Slothuus and De Vreese, 2010; Evans and Andersen, 2006; Zaller, 1992). However, less is known about the effect of partisanship on trust behavior. Informed by the literature on partisan identities, we expect the endorsement of out-group politicians

to augment the effect of positive and negative messages on trust and trustworthiness:

HT_1A**: Positive social media messages from misaligned politicians result in larger gains in trustworthiness among agents and in trust among principals.**

HT_1B**: Negative social media messages from misaligned politicians result in larger declines in trustworthiness among agents and in trust among principals.**

Considerable research suggests that individuals perceive social media platforms as conduits for increased polarization and mistrust. Therefore, we expect that the mean levels of trustworthiness and trust in individuals in the treatment group will be lower than among the control group. This leads to our third set of hypotheses.

HT_2**: On average, trustworthiness and trust will decline in later rounds of questioning, compared with the baseline measures.**

We also expect attention to the treatment conditions to moderate the effects of framing and cognitive dissonance. Recent scholarship in both political science and psychology suggests that the amount of time spent on a survey question works as a measure of respondent effort. Those who are more cognitively engaged with the treatment receive stronger doses (Berinsky, Margolis and Sances, 2014; Wise and Kong, 2005; Malhotra, 2008). Similar effects for latency, measured as the time spent reading tweets, have been shown to increase the effects of social media framing on polarization (Banks et al., 2020). To capture the effects of attention, we capture the active response of the respondents to each treatment condition. We expect:

HT_3**: Higher engagement, such as lower latency (more time spent reading the tweets) and active responses to tweets (retweet, like, and reply), will increase the effects of the**

treatments.

4 Descriptive Evidence: Trustworthiness and Trust

Descriptive Results for Trustworthiness

Tables 1 and 2 present descriptive information on the decision to cast the five entrusted votes (i.e., our measure of *trustworthiness*). In the first round, a total of 64% of Mexican and Brazilian respondents cast the entrusted votes, which, as noted earlier, reduced their chances of participating in the raffle. In the second round, casting rates declined to 59% and 51%, respectively.[12] Among those who agreed to cast entrusted votes in the first round, 20% in Brazil and 19% in Mexico defected in the second round. Among those who did not agree to cast votes, 22% and 15%, respectively, agreed to do so in the second round.

It is worth highlighting that, although casting votes reduces the chances of winning one of the iPads, a majority of respondents still accepted their role of trustee and cast the votes of their peers as requested.

Descriptive Results for Trust

Figure 2 presents descriptive results on the number of votes [0,10] delegated (entrusted) to others, with the first round shown on the horizontal axis and the second on the vertical. The circles in Figure 2 describe the share of votes entrusted in the second round conditional

[12] We do not analyze the third round of the game here. However, trustworthiness in both countries remained almost unchanged in the third round.

Table 1 Trustworthy, Transition Matrix (Brazil)

	Second Round		
First Round	Agree	Don't Agree	Total
Agree	51% (1213)	12% (295)	64% (1508)
Don't Agree	8% (189)	28% (666)	36% (855)
Total	59% (1402)	41% (961)	100% (2363)

Table 2 Trustworthy, Transition Matrix (Mexico)

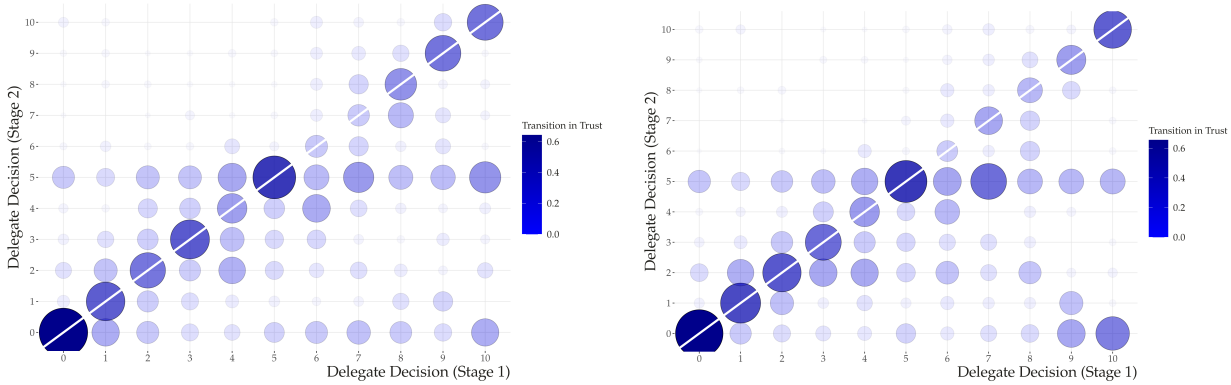
	Second Round		
First Round	Agree	Don't Agree	Total
Agree	51% (1188)	13% (307)	64% (1495)
Don't Agree	5% (129)	31% (722)	36% (851)
Total	56% (1317)	44% (1029)	100% (2346)

on the respondent's decision in the first round. For example, the circles plotted on the diagonal of each figure represent respondents who delegated the same amount of votes in the first and second rounds of the game. By contrast, the upper and lower triangles indicate an increase or decrease in trust, respectively.

Overall, we observe a decline in trust among respondents in our Mexican and Brazilian samples. Between the first and the second rounds of the game, respondents consistently reduced the number of votes entrusted to other players, as the reader can easily see from the more populated lower triangle in Figure 2. Therefore, from the first to the second round we observe that most respondents reduced the votes entrusted to other players and

retained for themselves a larger number of votes.

Figure 2 Trust: First and Second Rounds of the Game, Compared



a) Brazil b) Mexico

Note: The plots present changes in trust (votes delegated) between the first and the second rounds of the game in Brazil and Mexico. The upper triangle in each figures indicates the share of respondents who delegated more in the second round (increase in trust), whereas the lower triangle indicates the share of subjects who delegated less (decrease in trust)

5 Experimental Results

Descriptive evidence in the previous section shows that between the first and second rounds, fewer respondents agreed to cast the votes entrusted to them (lower trustworthiness) and smaller quantities were delegated to other respondents (lower trust). In Brazil, rates of agreement to cast entrusted votes (trustworthiness) declined from 64% to 59%, and in Mexico from 64% to 56%. Similarly, entrusted votes (trust) in Brazil declined from 3.4/10 votes in the first round to 3.17/10 in the second, and in Mexico from 3.75/10 in the first to 3.24/10 in the round. In the next two subsections, we show that social media exposure had no effect on the decline in trustworthiness but a significant effect on trust.

5.1 The Null Effect of Social Media Exposure on Trustworthiness

Table 3 presents our findings on the effect of social media exposure on trustworthiness. We estimate benchmark linear probability models to capture the effect of exposure to social media messages on the binary decision to cast votes entrusted by another player in the second round of the game. In the second round, our models interact the treatments with the subjects' first-round decision. Columns 1 to 3 present the results for Brazil, while columns 4 to 6 present those for Mexico. The baseline condition includes respondents who played the second round of the game without being exposed to social media messages. We then separate by treatment condition (negative/positive and in-group/out-group) and control for the first-round decision to cast votes.

While findings are suggestive and point in the right direction, estimates do not reject the null hypothesis that $\theta_{i,2} - \theta_{i,1} \neq 0$. Accordingly, we report null findings for the trustworthiness (agent) hypotheses, HT_1A and HT_1B. Only hypothesis HT_2 holds, showing a decline in trustworthiness in later rounds, consistent with most in-person implementations of the *trust game*. This decline, however, is not explained by social media exposure.

5.2 The Negative Effect of Social Media Exposure on Trust

Unlike the case for trustworthiness, our model results show that social media exposure reduces overall trust. We begin by presenting conservative estimates of the effect of our experiment on trust, separating dissonant messages (out-group politician) and uncivil messages (negative content) using nonparametric graphical information. Then, we present statistical models and estimate the marginal effects of the treatments. Later, we discuss

Table 3 Regression Models: Treatment Effects of Framing and Endorsement on Trustworthiness

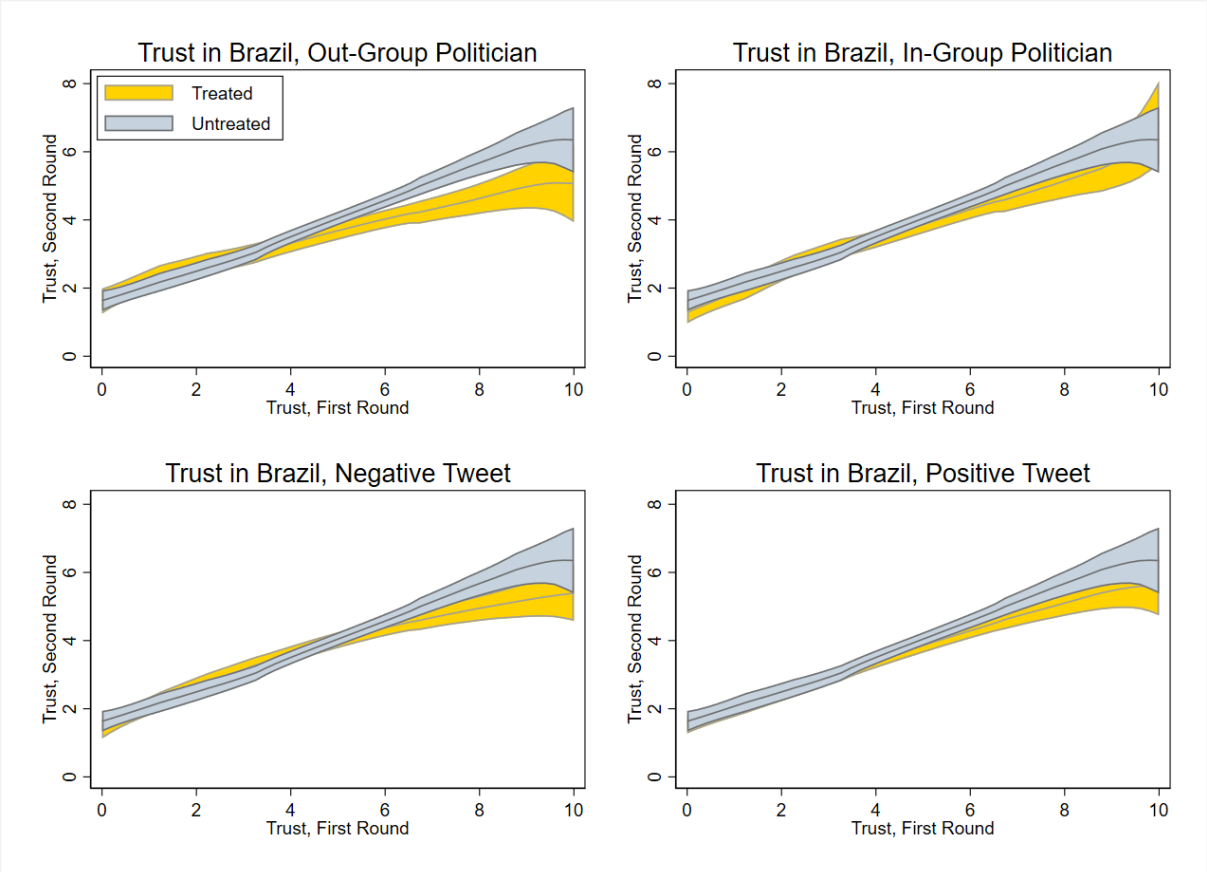
	Brazil			Mexico		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	0.344***	0.416***	0.462***	0.240***	0.303***	0.207*
	(0.074)	(0.082)	(0.097)	(0.076)	(0.095)	(0.107)
Trustworthiness (Round 1)	0.589***	0.591***	0.594***	0.634***	0.632***	0.634***
	(0.032)	(0.032)	(0.032)	(0.029)	(0.029)	(0.029)
Framing: Negative	0.035			−0.027		
	(0.036)			(0.034)		
Framing: Positive	0.005			−0.019		
	(0.036)			(0.033)		
Out-group		−0.028			0.0005	
		(0.040)			(0.043)	
In-group		0.019			−0.030	
		(0.041)			(0.042)	
Negative Out-group			−0.032			−0.011
			(0.052)			(0.063)
Positive Out-group			−0.024			0.005
			(0.050)			(0.052)
Negative x Trustworthiness (Round 1)	−0.021			0.013		
	(0.045)			(0.042)		
Positive x Trustworthiness (Round 1)	−0.010			0.012		
	(0.045)			(0.042)		
Out-group x Trustworthiness (Round 1)		0.028			0.002	
		(0.050)			(0.054)	
In-group x Trustworthiness (Round 1)		−0.007			0.039	
		(0.050)			(0.052)	
Negative Out-group x Trustworthiness (Round 1)			0.015			0.009
			(0.065)			(0.078)
Positive Out-group x Trustworthiness (Round 1)			0.038			−0.001
			(0.062)			(0.066)
N	2,128	1,607	1,156	2,219	1,426	1,084
Adjusted R^2	0.331	0.347	0.346	0.391	0.395	0.379

Notes: The models use benchmark OLS estimation. Models 1, 2, and 3 report results for Brazil; Models 4, 5, and 6 for Mexico. The dependent variable uses the decision to cast votes entrusted by other players, thus measuring subjects' levels of trustworthiness. A battery of individual-level pretreatment controls—such as, age, income, employment, education, gender, and individual level of trust—are controlled for in all six estimations. *p<0.1; **p<0.05; ***p<0.01

the factors that mediate the decline in trust.

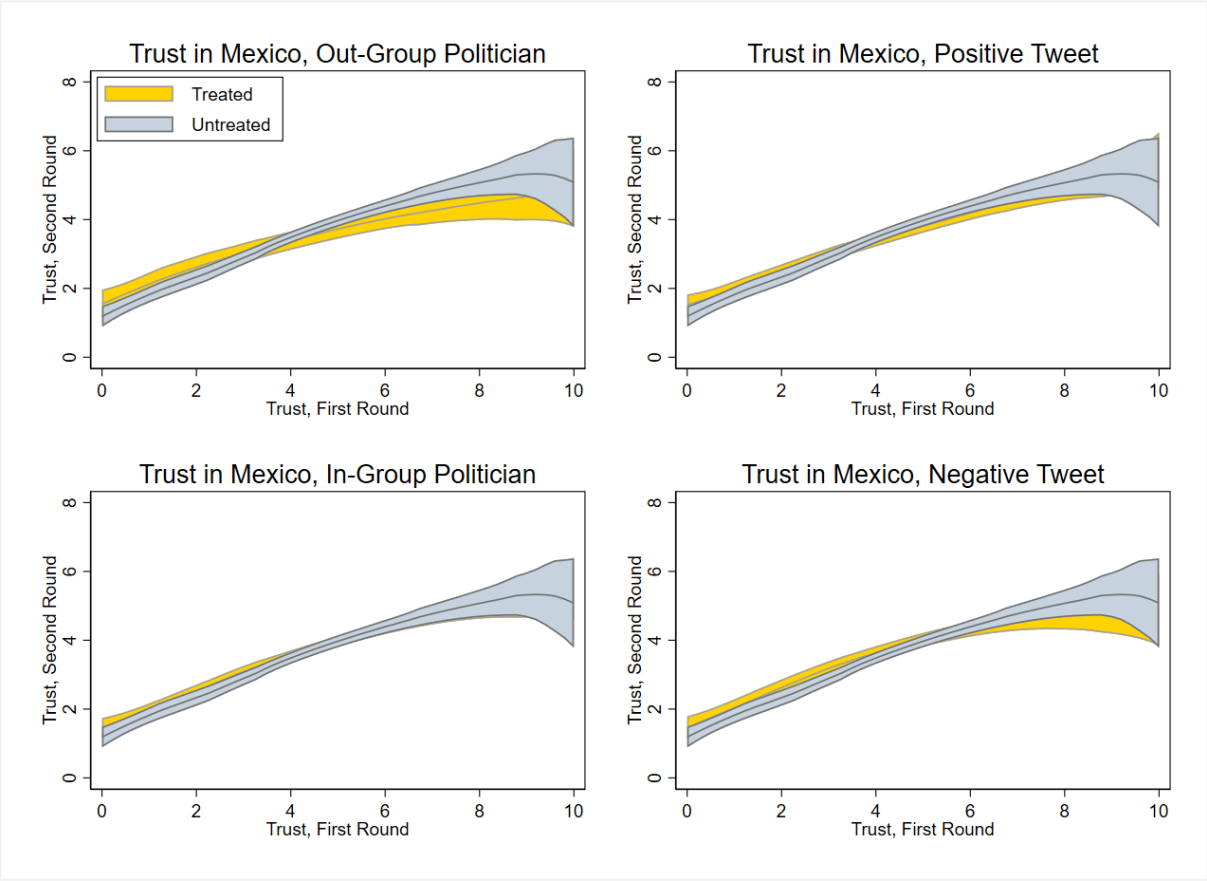
Figures 3 and 4 separate the results of our experiment by out-group/in-group politicians and by the negative/positive conditions. Separating the two treatment conditions, we find robust and statistically significant results when respondents are exposed to messages by out-group politicians (*dissonant messages*). Results are inconclusive when considering only the negative tone of the social media post (*uncivil discourse*), as they are significant for Brazil but not for Mexico.

Figure 3 Changes in Trust among Treated and Untreated Respondents in Brazil



Note: Local polynomial lines with confidence intervals. Plots compare changes in trust (votes delegated) between the first and second rounds of the game in Brazil. Four treatment conditions are compared with the control group: dissonant tweets from an out-group politicians, congruent tweets from an in-group politician, negative tweets (*responsibility deflection*), and positive tweets (*cross-the-aisle*). The joint effect of out-group and negative tone is not evaluated in this figure.

Figure 4 Changes in Trust among Treated and Untreated Respondents in Mexico



Note: Local polynomial lines with confidence intervals. Plots compare changes in trust (votes delegated) between the first and second stages of the game in Mexico. Four treatment conditions are compared with the control group: dissonant tweets from an out-group politician, congruent tweets from an in-group politician, negative tweets (*responsibility deflection*), and positive tweets (*cross-the-aisle*). The joint effect of out-group and negative is not evaluated in this figure.

The upper left plot in Figure 3 provides visual confirmation of a statistically significant difference between respondents in the treatment and control groups exposed to messages from out-group politicians. The negative effect of the tweet is larger for respondents who entrusted more than four votes in the first round. Results are substantively similar but less robust in the case of Mexico (Figure 4). By contrast, exposing respondents to tweets from politicians they support yields small effects in Brazil and null results in Mexico.

The lower left plots in Figures 3 and 4 show that, compared with the control group, negative political messages produce a modest decline in trust in Brazil but have no significant effect in Mexico. Given that we are not considering the joint effect of an out-group politician posting a negative tweet, the results reported in this section are very conservative.

In Table 4, we present the results from benchmark ordinary least squares (OLS) analysis to capture the effect of the treatments on declines in trust in the second round of the game. Because changes in trust are heterogeneous, as shown in Figures 3 and 4, we use an interactive linear model between the treatments and the decision to entrust votes in the first round of the game. Columns 1 to 3 present the results for Brazil of each different set of specifications, and columns 4 to 6 for Mexico. [13]

The first models for each country (1 and 4) estimate the treatment effect of the content of the tweets. If we do not take into account the first-round decision to entrust votes, we cannot reject the null hypothesis that after respondents are exposed to the treatment $\theta^*_{j,2} - \theta^*_{j,1} \neq 0$. As in Figures 3, 4, and 5, the effect of the treatment has the expected negative effect once the first-round decision is taken into account.

In models 2 and 5, we estimate the effects of reading a message from an out-group politician. We consider the vote intention of the respondent, "if elections were to take place next week," and the author of the tweet, to distinguish the effect of a message posted by an in-group or out-group politician.

Exposure to a tweet from an out-group politician, independent of the content of the

[13]The control group for all models consists of respondents who played the second round of the game without reading the social media message.

message, yields a statistically significant decrease in trust among respondents in Brazil. After treatment with a tweet from a misaligned politician, respondents decrease the number of votes they entrust to other players. The effect is larger for higher levels of trust in the first round, as reported in Figure 3. Although the results are substantially similar in Mexico, the magnitude of the effects is smaller. Although the interaction term is not statistically distinct from zero, even for the Mexican case, reading a tweet from a misaligned politician has a negative effect on trust.

Finally, models 3 and 6 evaluate hypotheses $H1_A$ and $H1_B$, with respondents playing the role of principals (voters). We estimate the effects of being exposed to a negative message from an out-group politician. Results in both countries show statistically significant declines in trust after respondents are exposed to uncivil/negative social media messages from political opponents.

Results are fully described in Figure 5, with marginal effects for two of our treatment conditions from models 3 and 6. Results describe the marginal change in the number of votes[0,10] entrusted in the second round as a function of trust in the first round. Figure 5 presents the effects of reading a tweet from a misaligned politician (models 2 and 4) and Figure 6 separates the out-group treatment according to the positive and negative framing (models 3 and 5). The figures provide a clear visualization of how out-group messaging, in particular with a negative tone, has a detrimental effect on interpersonal trust. For both cases, we see that reading a negative dissonant message reduces by almost 10% the votes delegated to other players between the first and second stages of the trust game—and marginal effects are statistically different from zero on respondents who in the early stage

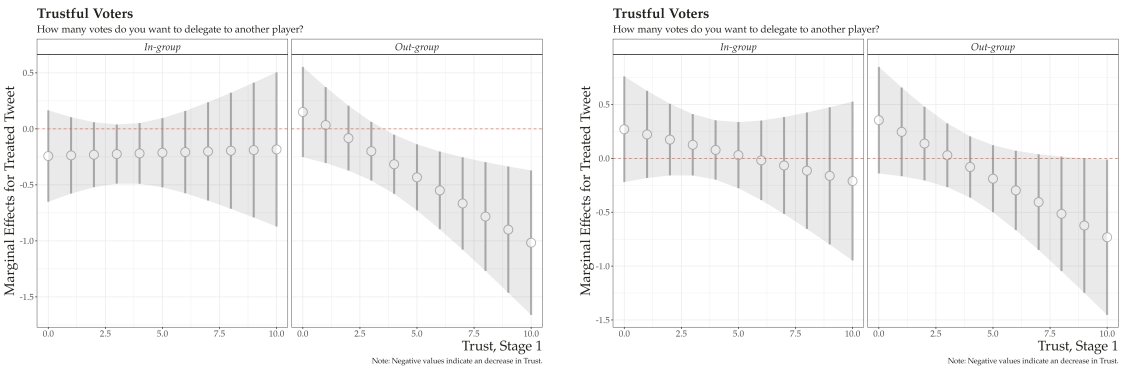
Table 4 Regression Models: Treatment Effects of Framing and Endorsement on Trust

	Brazil			Mexico		
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	2.276***	2.037***	1.985***	2.514***	2.114***	2.322***
	(0.433)	(0.481)	(0.575)	(0.444)	(0.552)	(0.612)
Trust (Round 1)	0.460***	0.460***	0.460***	0.459***	0.462***	0.462***
	(0.031)	(0.031)	(0.031)	(0.032)	(0.032)	(0.032)
Framing: Negative	−0.052			0.299		
	(0.194)			(0.209)		
Framing: Positive	−0.006			0.150		
	(0.195)			(0.204)		
Out-group		0.101			0.300	
		(0.216)			(0.264)	
In-group		−0.315			0.266	
		(0.217)			(0.261)	
Negative Out-group			0.083			0.676*
			(0.283)			(0.366)
Positive Out-group			0.110			0.015
			(0.274)			(0.324)
Negative x Trust (Round 1)	−0.032			−0.050		
	(0.043)			(0.046)		
Positive x Trust (Round 1)	−0.033			−0.039		
	(0.044)			(0.045)		
Out-group x Trust (Round 1)		−0.104**			−0.081	
		(0.048)			(0.057)	
In-group x Trust (Round 1)		0.022			−0.041	
		(0.050)			(0.058)	
Negative Out-group x Trust (Round 1)			−0.126**			−0.164**
			(0.062)			(0.079)
Positive Out-group x Trust (Round 1)			−0.081			−0.018
			(0.062)			(0.069)
N	2,092	1,583	1,140	2,216	1,425	1,083
Adjusted R^2	0.232	0.234	0.218	0.200	0.196	0.202

Notes: The models use benchmark OLS estimation. Models 1, 2, and 3 report results for Brazil; Models 4, 5, and 6 for Mexico. The dependent variable uses the number of votes subjects (principals) entrusted in round 2 to another player to be doubled and cast for the principal's candidate. A battery of individual-level pretreatment controls—such as, age, income, employment, education, gender, and individual level of trust—are controlled for in all six estimations. *p<0.1; **p<0.05; ***p<0.01

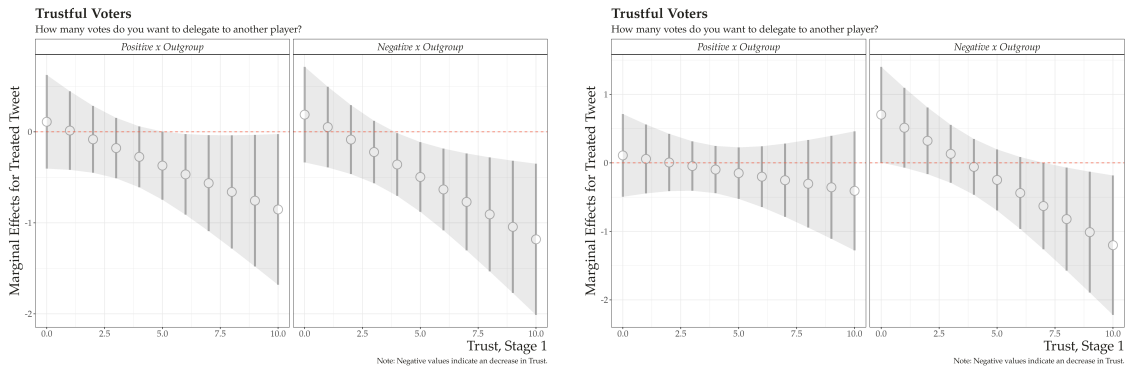
of the game exhibited higher levels of trust. The effect is substantively significant and, more important, describes a low-dosage treatment (one tweet) compared with the large number of tweets that users are exposed to on a daily basis.

Figure 5 Marginal Effects of Cognitive Dissonance on Trust



a) Brazil b) Mexico

Figure 6 Marginal Effects of Negative Treatment from a Misaligned Politician



a) Brazil b) Mexico

6 Mechanisms: The Role of Attention and Emotion

While results from the previous sections confirm the hypothesized effect of social media frames on trust, they provide limited information about the mechanisms that underlie our results or about the differences observed between Brazil and Mexico. Our survey, however, included validation checks to evaluate whether respondents properly interpreted the partisan leaning of the social media frames and, more importantly, questions about respondents' engagement with the partisan treatments.

In this section we analyze these results in greater detail, introducing a double-identification strategy that isolates the effect of attention to social media on declines in trust.

Consider the effect of the treatment among respondents who engaged with the political tweets (by retweeting, liking, or replying) ***before*** answering our trust question (treatment group), compared with those in the control group who engaged with the tweet ***after*** answering the trust question. Given that the treatment consists exclusively of manipulating whether respondents play the trust game **before** or **after** reading the social media messages, our double-identification assumption only needs to assume that respondents assigned to the control group would have engaged with the tweet in the same way if they had been in the treatment group and not answered the trust question before engaging. We believe that this is a reasonable assumption, one that allows us to identify the heterogeneity of the treatment effects conditional on behavioral reactions to the social media message.

Throughout this section, we repeat the same double identification strategy (engaged

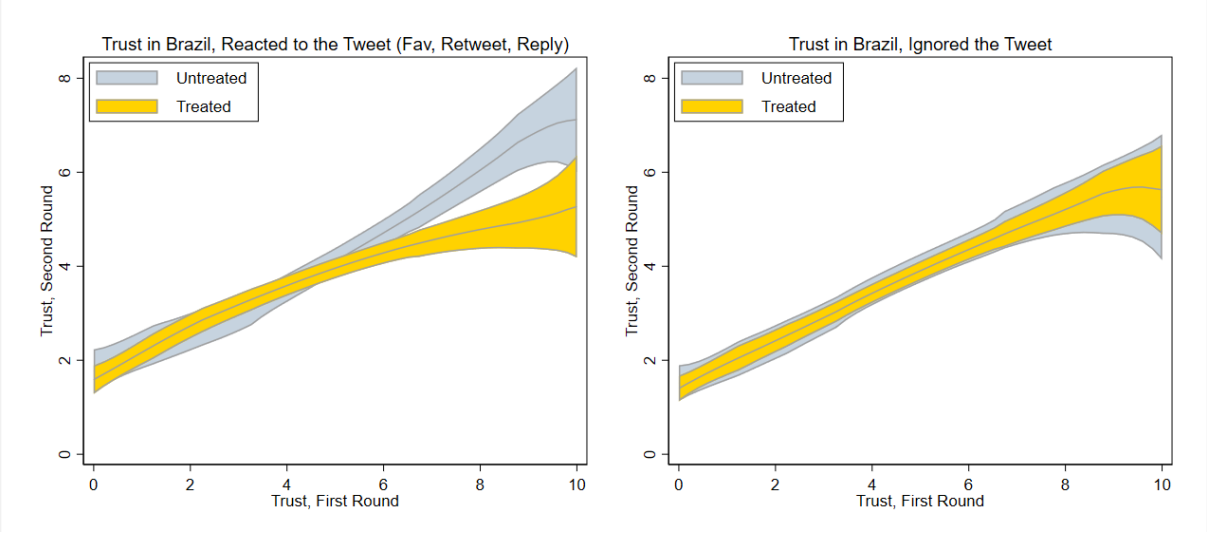
treatment/engaged control, ignore treatment/ignore control) to isolate the mechanisms that explain a decline in trust. Consider Figure 7 which, as in the previous section, plots the trust decision in the second round (vertical axis) against the decision in the first round (horizontal axis). In Figure 7, the left plot compares the effect of the *treated-engaged* group (like, retweet, reply) against the *control-engaged* group. Meanwhile, the right plot describes the *treatment/ignore* group against the *control/ignore* group. Notable is the significant decline in trust among respondents who like, retweet, or reply to a tweet in the treatment group compared with respondents in the control group who were equally engaged with the tweet. As important is the fact that those who ignore the tweet are almost indistinguishable between groups.

Results are revealing, showing a significant decline in trust only among respondents who engaged with the tweet ***before*** the second round (treatment), and null effects for respondents who engaged with the tweet but did so ***after*** the second round (control). In other words, if we consider only respondents who felt strongly about the tweet, the effect is large and significant only for the treatment group.

Described in terms of our theory, our test results show that $(\theta^*_{j,2} - \theta^*_{j,1}|E) < (\theta^*_{j,2} - \theta^*_{j,1}|\neg E)$, given that $(\theta^*_{j,2} - \theta^*_{j,1}|E) < 0$, while $(\theta^*_{j,2} - \theta^*_{j,1}|\neg E) = 0$. By splitting the sample between those who engage with the tweet (treatment and control) and those who did not (treatment and control), we prove hypothesis HT_3 and are able also to test for the different mechanisms that explain the decline in trust.

Figure 8 depicts similar two-way comparisons, focusing on messages from out-group politicians (dissonant trait). Among those who like, retweet, or reply to the message

Figure 7 Changes in Trust When Respondents Engage with the Tweet (left) or Ignore It (right)

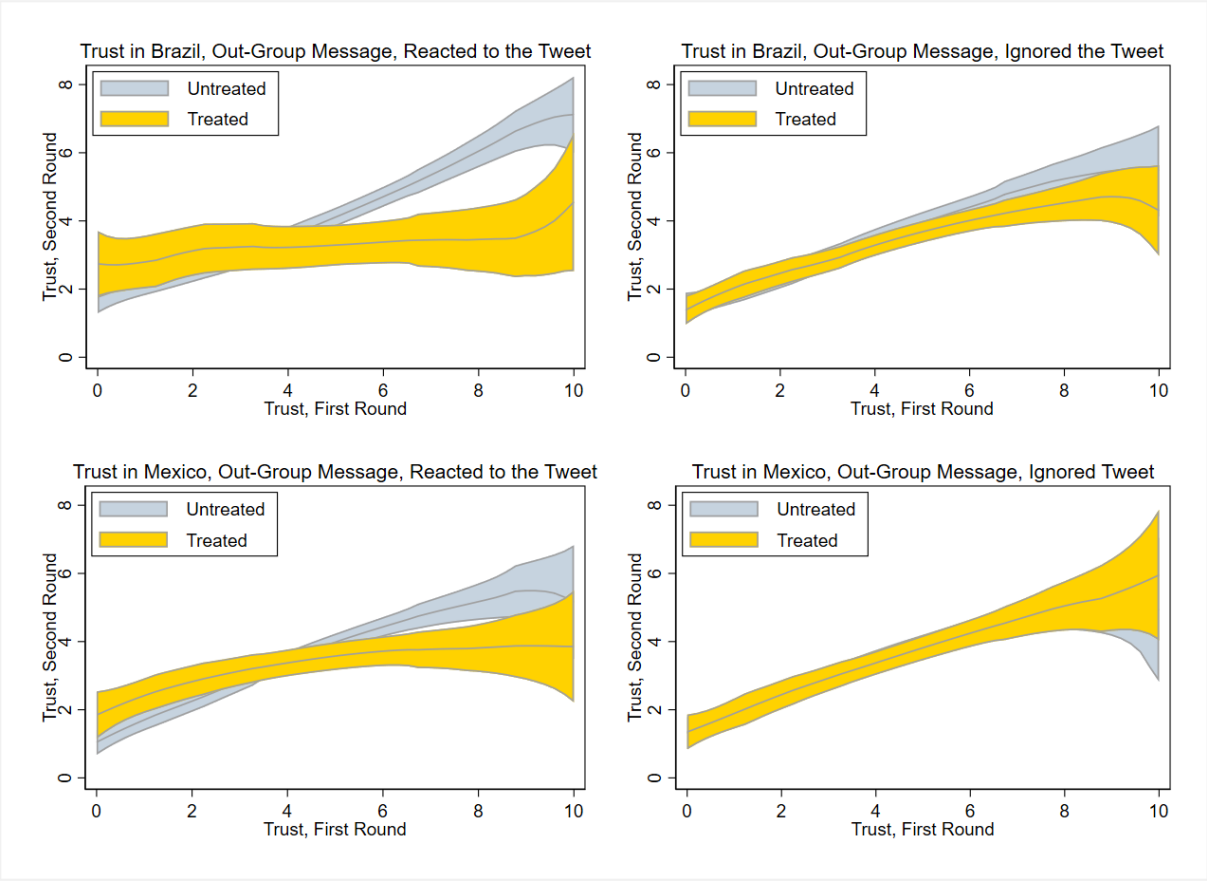


Note: The left plot estimates the treatment effect among voters who engaged with the tweet (like, retweet, or reply). The right plot estimates the treatment effect among those who did not engage (ignore). Results show a decline in trust only among respondents who saw and engaged with the tweet *before* the second round of the experiment. Those who engaged with the tweet *after* the experiment showed no decline in trust. We also find no effect among treatment and control respondents who ignored the tweet.

(left plot), we see larger treatment effects. By contrast, incidental exposure (Boczkowski, Mitchelstein and Matassi, 2018) to the tweet, as shown in the plots to the right of Figure 8, has modest effects in Brazil and a null effect in Mexico. Indeed, conditioning on both treatment and attention provides the strongest evidence yet of the effect of social media on trust.[14] The comparisons between treatment and control groups with respondents who reacted or ignored our social media treatments is illuminating. Given that all other parameters are held constant, we can confidently state that $\theta^*_{j,R1} - \theta^*_{j,R2} < 0$.

[14] Appendix D presents the results using a linear parameterization of the treatment effects using OLS. The results are similar.

Figure 8 Changes in Trust When Respondents Engage with Dissonant Tweets



Note: The left plot estimates the treatment effect among Brazilian and Mexican voters who engaged with the tweet (like, retweet, or reply). The right plots estimate the treatment effect among those who ignore the tweet. Results show large declines in trust for dissonant tweets only among treated respondents who engaged with the tweet *before* the second round of the experiment. There is no effect for partisan dissonance in the control group and no difference in the treatment and control respondents who ignored the tweet.

7 Concluding Remarks

Are polarization and uncivil dialogue reducing trust and trustworthiness? Results from our survey experiment provide compelling evidence of a negative effect of social media on trust behavior. Negative messages from out-group politicians reduce the propensity of survey respondents to entrust votes to their peers.

The negative effect on trust is considerably greater among randomly treated respondents who engage with social media messages through likes, retweets, and replies. By contrast, respondents in the control group who were equally engaged show no change in trust behavior. Together, the double-identification strategy discussed in Section 6 provides robust evidence of a social media effect on trust behavior.

We also find no evidence of a social media effect on trustworthiness. After exposure to negative social media messages, respondents are less likely to entrust resources to others but no less likely to cast votes entrusted to them.

Interesting extensions of the proposed model could be deployed to understand why social media exposure decreases trust but has negligible effects on trustworthiness. Indeed, results are consistent with social media reducing the association between θ_j^* and θ_i after exposure. Recent research by Corbacho et al. (2016) has shown that individuals who perceive others as corrupt are also more likely to engage in corruption themselves. By contrast, our experiment finds no equivalent association between perceiving others to be deceitful and behaving deceitfully. The dissociation between trust and trustworthiness in the treatment group, therefore, raises new questions about the connections between θ_j^* and θ_i.

In our view, the implementation of the proposed trust game as a survey experiment in two countries was a success. We find consistent estimates of trust behavior that were readily comparable across the two cases, with a design that allows us to distinguish the quality of the treatment (i.e., stronger in Brazil than in Mexico) as well as the importance of the mediating factors involved (i.e., anger). We believe that the survey design can be

easily replicated and, as with the laboratory version of the traditional trust game, used to explore differences within and across countries.

Evidence has mounted that trust is important for thriving democracies and economies. Latin America and the world as a whole, however, have seen large drops in trust over the past few decades. Failures by governments to deal with several economic crises (and a pandemic), growing inequality, and unfulfilled expectations may be the main drivers. Still, the quantity of information and how it is distributed matters as well. Social media was expected to bring additional transparency, higher accountability, and, hence, higher political trust. Unfortunately, the evidence does not seem to bear out these expectations.

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Trustful Voters, Trustworthy Politicians: A Survey Experiment on the Influence of Social Media in Politics

Supplemental Information File

Contents

A Appendix A: Survey Flow and Treatment Assignment

In our survey, *trust* and *trustworthiness* are repeated independent behavioral responses to the *potential for realizing gains* by entrusting one's votes to others or of *incurring losses* by casting other respondents' votes. There are no expected future interactions and no gains in reputation. As in Cox (2004), we isolate *trustworthiness* from other preferences such as reciprocity or altruism, given that respondents have no information about the individual who entrusted votes to them or about the individual to whom votes will be entrusted.[15]

Game Sequence and Trust/Trustworthiness interventions

To capture the role of social media on changes in trust and trustworthiness, we embed a survey experiment in a political trust game and expose respondents to contextually appropriate tweets from government or opposition political figures in Mexico and Brazil. The opening question of the survey invites respondents to select one of two fictional cartoon candidates and informs them that they will be able to collect votes for their candidate throughout the survey. Once all respondents answer the survey, those who supported the candidate who wins most votes are entered into a raffle for two new iPads.

Respondents are informed that the number of their entries in the raffle will be equal to

[15]To avoid deception, our survey experiment institutes a universal respondent who carries out all requests received from respondents. Therefore, all votes entrusted by respondents were doubled and counted toward the respective candidates' total tally. As in Cox (2004), we effectively create a triad where respondents' decisions to cast or entrust votes are independent from one another.

the number of votes they personally contribute to their candidate. Therefore, collecting as many votes as possible is incentive compatible: by making sure their candidate wins they become eligible to participate in the raffle, and by collecting more votes for their candidate they increase their chances of winning an iPad. At the time of the survey, the local price of an iPad was approximately 1.5 times times the median monthly salary in Brazil and half the median salary in Mexico.[16]

When answering questions as the agent (*politician*), respondents are asked to cast or discard votes entrusted to them (trustworthiness). When answering in the role of the principal (*voters*), respondents decide how many votes to cast directly and how many to entrust. In the first round, all voters first play the role of the agent (trustworthiness). As in our theory, we are interested in setting participants' guilt sensitivity parameter, θ, before they entrust votes to others. All votes cast by the agents count toward the candidate favored by the trustee. After playing as agents, respondents decide how many votes to cast directly or to entrust to others. All votes cast (single votes) or entrusted (double votes) are accepted by our "universal" respondent.[17]

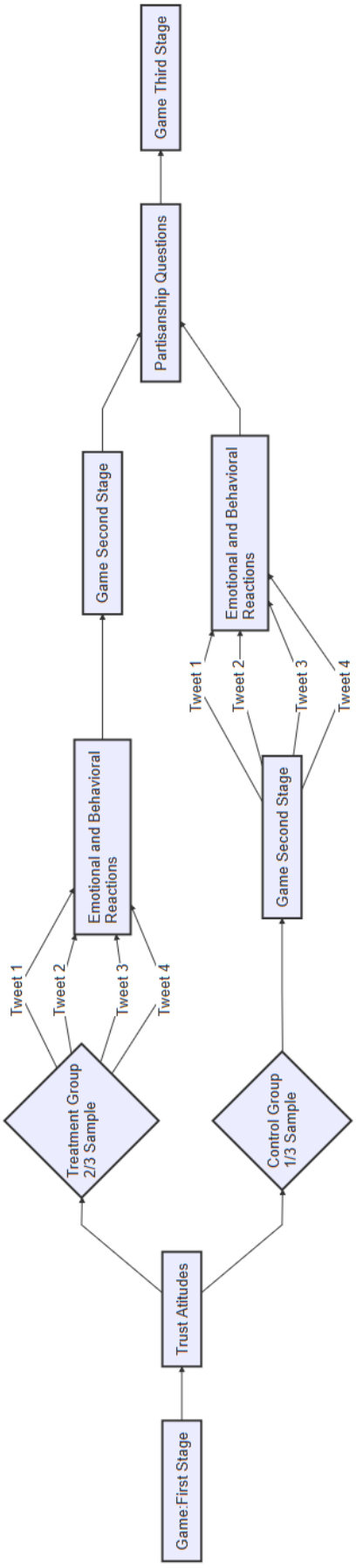
[16] A total of four iPads were distributed in the raffles in Mexico and Brazil, making the odds/price ratio very attractive.

[17] The rounds of casting and entrusting are repeated three times, with some respondents assigned to social media treatments and others to the control group. In this paper, we focus on the first treatment sets, which compare the baseline stage (first round) to the first treatment stage (second round). Comparing the first and second rounds of our two identical experiments in Brazil and Mexico minimizes cross-effects from survey questions that could account for changes in levels of *trust* or *trustworthiness*. Separate analyses model the effect of frame elements on trust and trustworthiness in a third round.

Rounds

We capture the effect of social media on *trustworthiness* and on *trust* using a *trust game* that unfolds in three rounds. In the game, administered in Brazil and Mexico, respondents are told that they may cast their votes directly or entrust them to other respondents for casting (measuring trust) and then to cast any votes entrusted to them by other respondents (measuring trustworthiness). Two-thirds of the respondents in each country constitute a treatment group that is exposed to one of four randomly selected, edited tweets from high-profile politicians in the respondents' country. We vary the framing of the tweet (positive or negative tone) and whether its author is from an in-group or an out-group. The intervention occurs before respondents in the treatment group play the second round of the trust game. The control group plays the second round without reading any tweets, thus providing a straightforward comparison group. Figure 9 illustrates the basic design of the game and the embedded experiment.

Figure 9 Survey Diagram



First Round

Figure 10 summarizes the game. First, respondents are asked to select one of two candidates, Laura or Juan,[18] who have no distinctive markers other than their gender. (The two images of the candidates appearing in the figure were used in all administrations of the game.) When selecting a candidate, respondents are informed that they will have multiple opportunities to increase the votes they allocate to their candidate of choice and, more importantly, that respondents who have supported the overall survey winner (Laura or Juan) are eligible to participate in a raffle for one of two new iPads. The figure summarizes the first round of investment in the two candidates. The next two instructions tell respondents how to win extra votes for their candidates, so that the mechanics of the process are well understood.

The fourth instruction tells respondents that they can win five more votes for their candidate if they read the following pledge: "*If other players delegate their votes to me, I agree to follow their preferences and to cast their votes for the candidate of their choosing.*"

To win the five votes, players are asked to answer either, "*I read the pledge*" or "*I did not read the pledge.*" We do not require them to sign the pledge, only to read it. We then introduce the baseline questions for the agent and the principal, where we inform respondents about casting the votes entrusted to them by another respondent.

Respondents are then offered 10 votes and asked how they would like to handle them. Respondents have two options: (i) they can cast any number of the ten votes directly to support their candidate or (ii) they can entrust any number of their votes to another

[18] We used the respective translations for the names in Portuguese in the Brazilian survey.

respondent to cast on their behalf. Each entrusted vote is doubled. The question explicitly states that the other respondent has read the pledge and will have the same options for casting or discarding them.

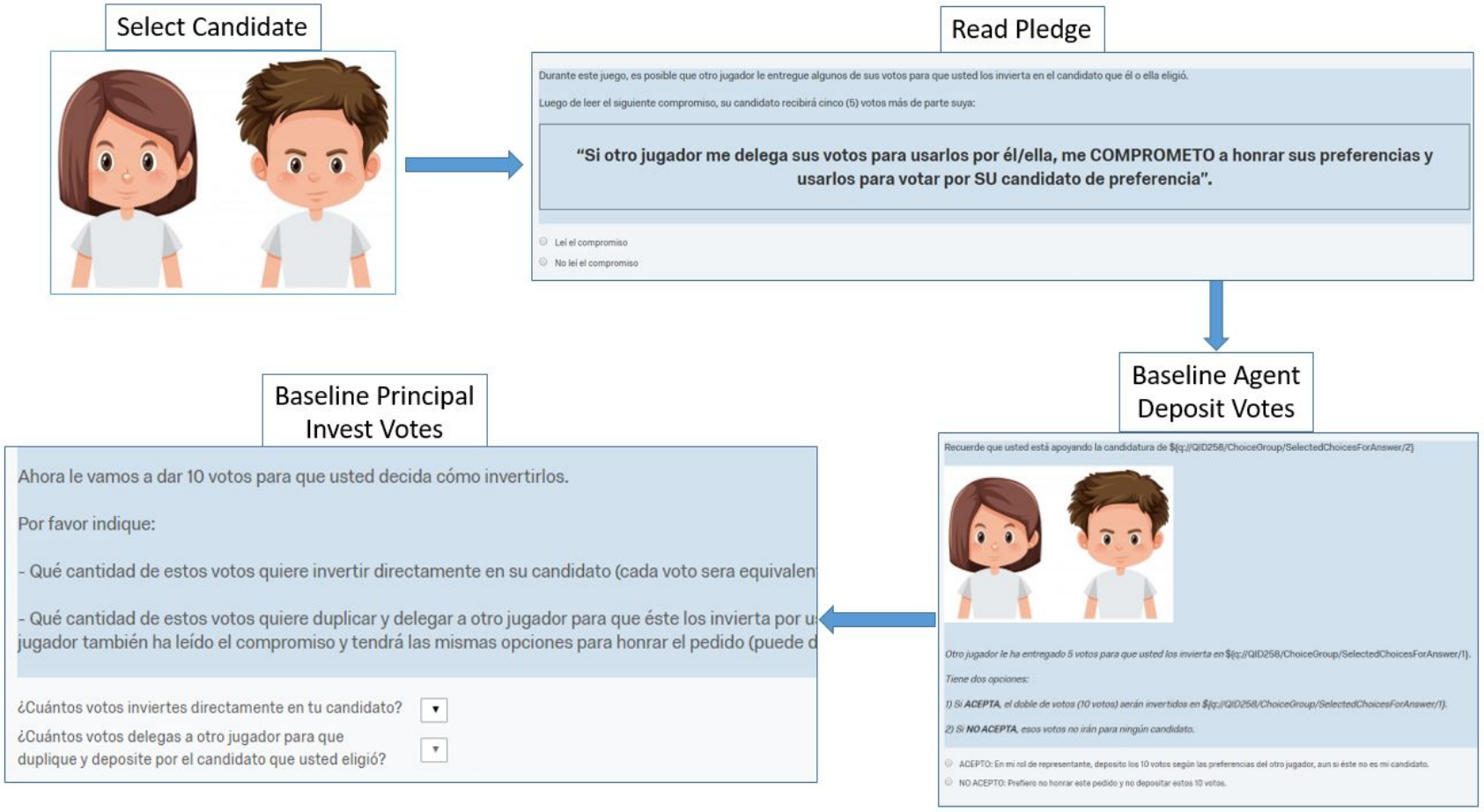


Figure 10 Experimental design: Respondents are asked to select a candidate whom they will support throughout the survey. Succeeding instructions show them how to accumulate more votes for their candidates, with each vote accumulated representing a ticket for a raffle to be held among the supporters of the winning candidate. Respondents may cast their votes directly (one vote = one raffle ticket) or entrust the votes to another player to cast. Entrusted votes count double votes, both toward selecting the winning candidate and in for accumulating raffle tickets.

Second Round: The Tweet Experiment

Once a baseline for the experiment is set, we distract the respondent by asking a variety of attitudinal, behavioral, and socio-demographic questions. These include a battery of standard questions about pro-market and pro-government attitudes, political knowledge, and perceptions of personal trust and trust in institutions.

Then, we randomly select two-thirds of respondents to be exposed to tweets, with the remaining third remaining unexposed and serving as a control, as depicted in Figure 9. After exposing respondents to the tweets we ask if they would "like", "retweet", "reply", or "ignore" the tweet they just read. We follow up with a question that asks how the tweet made them feel ("angry", "sad", "hopeful", etc.). Finally, we again measure *trustworthiness* and *trust* for the two-thirds of respondents exposed to tweets (the treatment group). They are then asked to invest in their candidates and to cast votes as shown in Figure 3.

The treatment group reads the tweets before playing the second round of the game. The entire control group plays the first round of the game before a subset is randomly selected to read the tweets, a step that ensures us behavioral responses to the tweets for the entire sample.

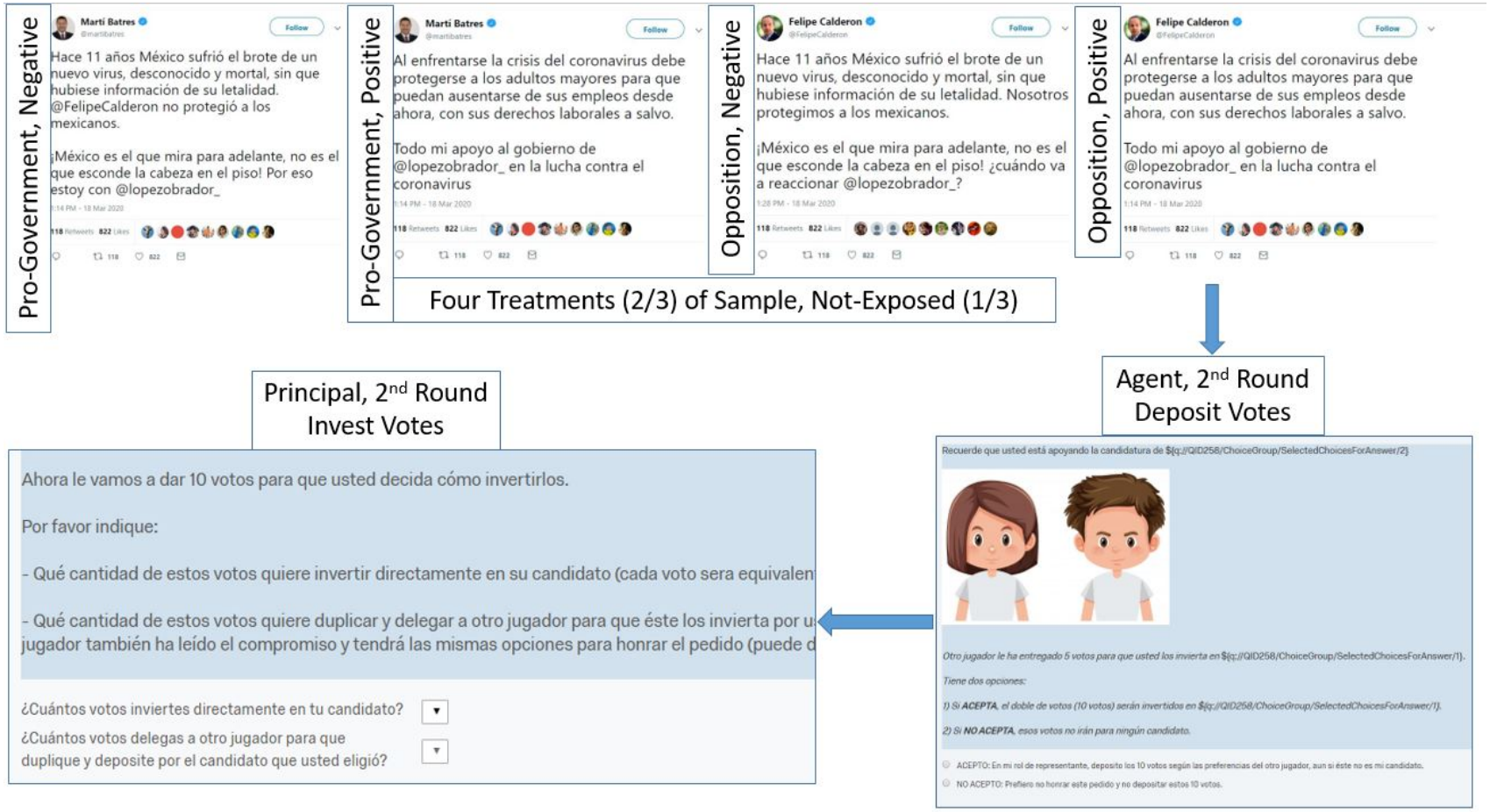


Figure 11 Later rounds: Two-thirds of respondents are exposed to messages before a second round of the game in which respondents are invited to entrust or cast votes as described above. The remaining one-third of the respondents serve as a control group and receive the political messages after the second round. Four different tweets frame the coronavirus response of a leading government or opposition figure. In the case of Mexico, those figures were Marti Bartres for the government and Felipe Calderon for the opposition.

Once again we then distract the respondent by posing a variety of attitudinal and behavioral questions, including preferences about voting, partisanship, and the competence of specific political figures. We conclude the survey with a third round of the game. Before we conclude, we ask the entire sample to play the game once more. The paper does not offer an analysis of the results of this third round as its focus was the differences between changes in the control and treatment groups from the first round of the game to the second.

B Appendix B: Treatment Intervention

Below we present the complete wording of the tweets used in each treatment condition. Table 6 and Figure 13 present the tweets just as Mexican respondents see them. Table 5 and Figure 12 present the same information for Brazil.

Our experiment uses edited tweets. Although external validity is slightly reduced by the decision to use edited content, we worded the messages using as a reference tweets, public statements, and interviews from the politicians identified as authors of the tweets. The randomization procedures guarantee internal validity. In each country, we vary only two features of each tweet: author and tone.

For the author, we use two prominent political figures in each country. In Brazil, we use Eduardo Bolsonaro, a member of the legislature and son of President Jair Bolsonaro, and Fernando Haddad, the leading candidate of the Workers' Party in the 2018 national election. For the Mexican case, we use Martí Batres, current senator from the ruling party, the National Regeneration Movement (MORENA); and Felipe Calderón, Mexico's president from 2006 to 2012, a leader of the opposition to the current government.

To vary the tone of the message, we use a positive and a negative framing related to the COVID-19 crisis. In both countries, we use the same wording for the positive message, varying the negative message to increase congruence between the content and the political context in each case. Positive messages frame the crisis as a moment of national union in which the president should lead the country; in the negative message, the author avoids blame for the crises and shifts responsibility to the opponent.

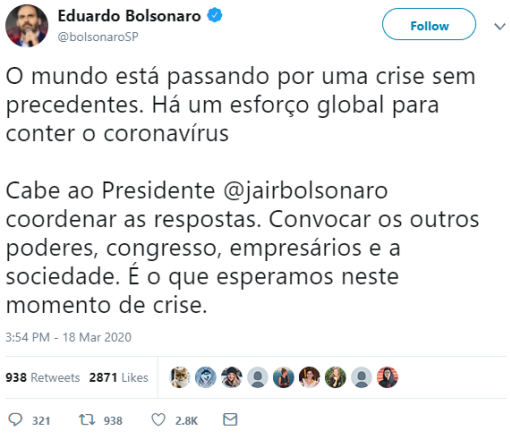
Table 5 Treatment Conditions in Brazil

	Positive Tweet	Negative Tweet
Eduardo Bolsonaro	The world is currently living through an unprecedented crisis. Countries all over the world are rallying to fight the coronavirus. It is the responsibility of President @jairbolsonaro to coordinate our response. He must act together with Congress, business leaders, and civil society. This is what we expect in such critical times.	The world is currently living through an unprecedented crisis. Countries all over the world are rallying to fight the coronavirus. But we have seen such viruses before, and they did not lead to all this hysteria. The fault lies with the President. Don't panic. Switch off the pandemic of misinformation from the media.
Fernando Haddad	The world is currently living through an unprecedented crisis. Countries all over the world are rallying to fight the coronavirus. It is the responsibility of President @jairbolsonaro to coordinate our response. He must act together with Congress, business leaders, and civil society. This is what we expect in such critical times.	The world is currently living through an unprecedented crisis. Countries all over the world are rallying to fight the coronavirus. But President @jairbolsonaro has been slow to respond. He is more concerned about attacking his opponents and taking part in protests that put the health of the Brazilian people at risk.

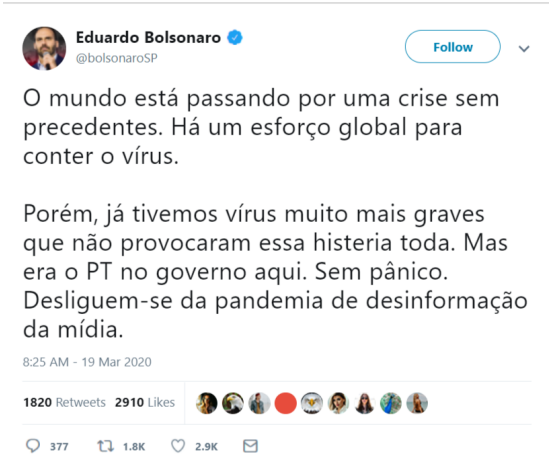
Table 6 Treatment Conditions in Mexico

	Positive Tweet	Negative Tweet
Felipe Calderón	When confronting the crisis of the coronavirus, we must protect the elderly so they can stop working today, while preserving their labor rights. All my support to @lopezobrador_ in his fight against the coronavirus.	Eleven years ago Mexico suffered from the emergence of a new virus, unknown and deadly, without any information about its lethality. We protected Mexico's citizens. Mexico is looks forward; it doesn't bury its head in the sand! When will @lopezobrador_ react?
Marti Batres	When confronting the crisis of the coronavirus, we must protect the elderly so they can stop working today, while preserving their labor rights. All my support to @lopezobrador_ in his fight against the coronavirus.	Eleven years ago Mexico suffered from the emergence of a new virus, unknown and deadly, without any information about its lethality. @FelipeCalderon did not protect Mexico's citizens. Mexico looks forward; it doesn't bury its head in the sand! That's why I support @Lopezobrador_

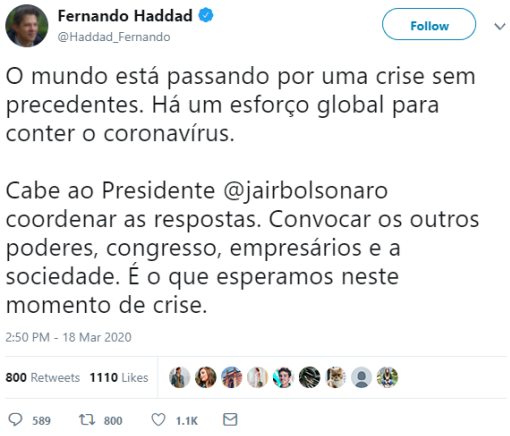
Figure 12 Tweets for the Treatment Conditions in Brazil



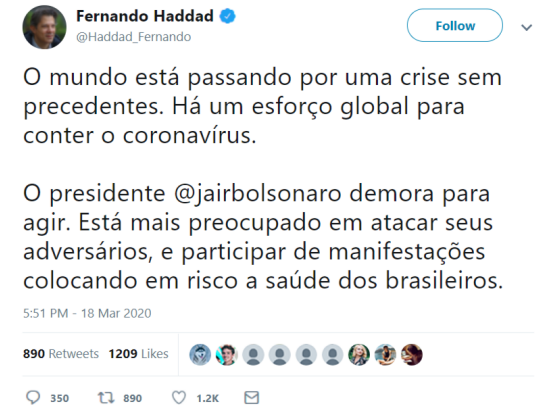
a) Eduardo Bolsonaro x Positive Tweet (T1)



b) Eduardo Bolsonaro x Negative Tweet (T2)



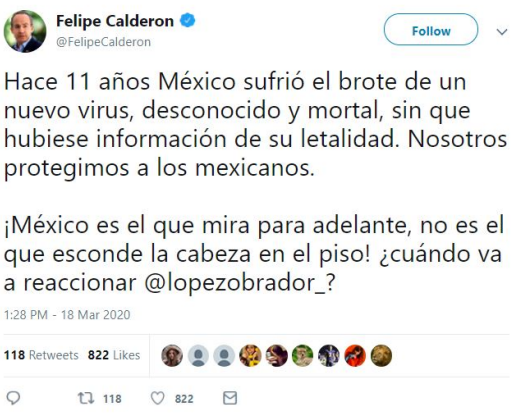
a) Fernando Haddad x Positive Tweet (T3)



b) Fernando Haddad x Negative Tweet (T4)

Figure 13 Tweets for the Treatment Conditions in Mexico

a) Felipe Calderón x Positive Tweet (T1)



b) Felipe Calderón x Negative Tweet (T2)

a) Marti Batres x Positive Tweet (T3)

b) Marti Batres x Negative Tweet (T4)

C Appendix C: Socio-Demographics and Attitudes Across the Samples

In this section, we present a set of individual level socio-demographic, political, and attitudinal characteristics distributed by survey group (treatment or control). As the reader will see, there are no significant differences across the treatment groups in our sample. Since most of these variables are nominal, the values are not susceptible to direct interpretation. Tables 7 and 8 present the sample mean and standard deviation for relevant covariates, organized by group.

Table 7 Sample Demographics by Treatment Assignment in Brazil

Variable	Quantity	Treatment	Control
Age			
	Mean	3.03	3.18
	Standard Error	1.53	1.54
Education			
	Mean	4.45	4.54
	Standard Error	1.26	1.27
Gender			
	Mean	1.48	1.49
	Standard Error	0.50	0.50
Ideological Placement			
	Mean	6.52	6.31
	Standard Error	3.30	3.33
Receive Income Assistance			
	Mean	2.29	2.30
	Standard Error	0.89	0.89
Relative Income			
	Mean	5.30	5.43
	Standard Error	2.10	2.05
Government Voters			
	Mean	1.37	1.35
	Standard Error	0.48	0.48
Opposition Voters			
	Mean	1.73	1.70
	Standard Error	0.44	0.46
Trust Bolsonaro			
	Mean	4.72	4.98
	Standard Error	3.54	3.60
Trust Lula			
	Mean	5.00	5.16
	Standard Error	2.50	2.59
Employment			
	Mean	1.37	1.35
	Standard Error	0.48	0.48
Total Cases			
	Total Number of Cases	1594	768

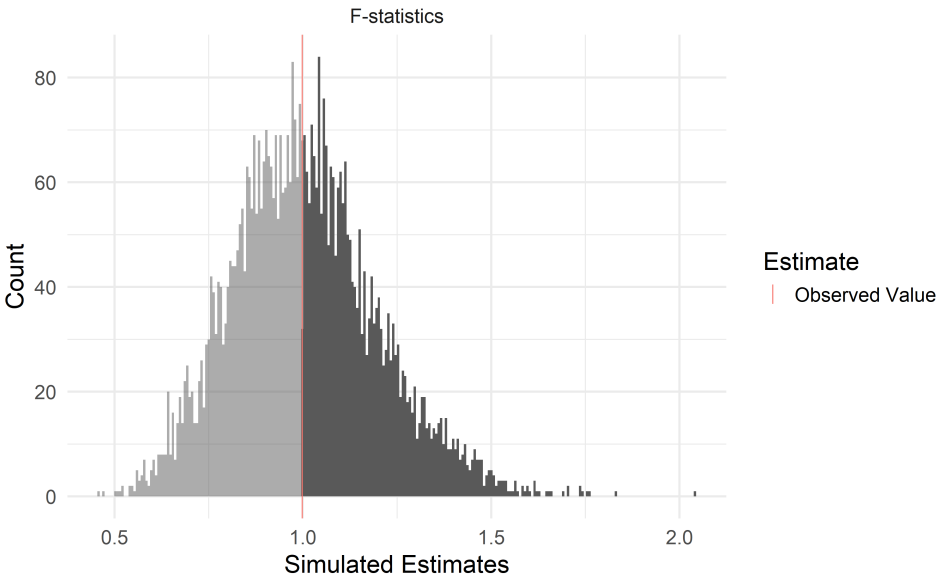
Table 8 Sample Demographics by Treatment Assignment in Mexico

Quantity	Treatment	Control	
Age			
	Mean	3.62	3.70
	Standard Error	1.79	1.81
Education			
	Mean	2.27	2.20
	Standard Error	1.29	1.29
Gender			
	Mean	1.54	1.51
	Standard Error	0.50	0.50
Ideological Placement			
	Mean	5.24	5.38
	Standard Error	3.24	3.20
Receive Income Assistance			
	Mean	1.17	1.18
	Standard Error	0.42	0.41
Relative Income			
	Mean	5.35	5.48
	Standard Error	1.93	1.82
AMLO Trust			
	Mean	5.91	5.93
	Standard Error	3.09	3.03
Government Voters			
	Mean	1.29	1.31
	Standard Error	0.46	0.46
Opposition Voters			
	Mean	1.15	1.15
	Standard Error	0.36	0.36
Employment			
	Mean	1.40	1.42
	Standard Error	0.49	0.49
Total Cases			
	Total Number of Cases	1533.00	813.00

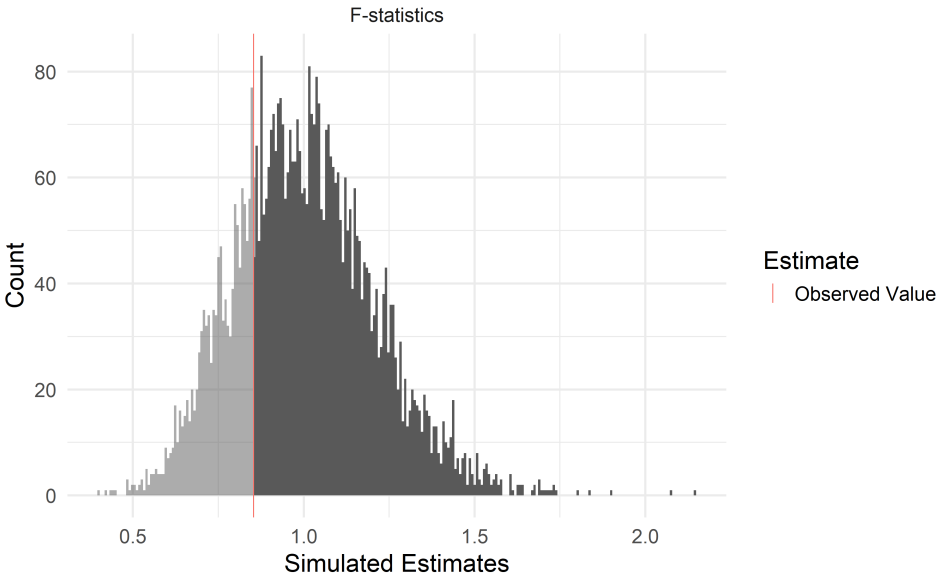
We use randomization inference to assess the balance of covariates across treatment arms (Gerber and Green, 2012; Coppock, 2019). of observed F-statistics. The null distribution of F-statistics is calculated through randomization inference using a linear probability model that regresses the treatment assignment on the covariates from Tables 7 and 8.

Both graphs indicate that the null hypothesis—namely, that no covariates have any effect on treatment assignment—cannot be rejected; in other words, the p-value for the randomization inference does not allow us to reject the possibility that the randomization assignment in the survey worked properly. Approximately 48% (P-value=0.489) and 75% (P-value=0.756) of the simulated F-statistics were larger than the observed F-statistic in Brazil and Mexico, respectively. The vertical red line on both graphs denotes the observed F-statistic, while the shaded regions denote simulated estimates more extreme than the one observed. For both cases, we use 5,000 simulations under the the null hypothesis, implied by random assignment, that no covariates produce effects on receiving the treatment.

Figure 14 Randomization Inference for Covariate Balance



a) Brazil



b) Mexico

D Appendix D: Regression Models by Engagement with Treatment

Table 9 Regression Models: Treatment Effects of Framing and Endorsement on Trust Split by Reaction to the Treatment (Brazil)

	Reacted to the Treatment				Ignored the Treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	3.208***	3.211***	3.278***	3.960***	1.128*	1.107*	0.313	−0.343
	(0.541)	(0.542)	(0.577)	(0.745)	(0.635)	(0.635)	(0.734)	(0.856)
Trust (Stage 1)	0.487***	0.487***	0.489***	0.496***	0.438***	0.438***	0.437***	0.436***
	(0.039)	(0.039)	(0.039)	(0.041)	(0.045)	(0.045)	(0.044)	(0.044)
All Treatments	0.110				−0.113			
	(0.222)				(0.237)			
Framing: Negative		−0.067				−0.039		
		(0.276)				(0.261)		
Framing: Positive		0.223				−0.217		
		(0.244)				(0.286)		
Out-group			0.315				−0.0002	
			(0.331)				(0.277)	
In-group			−0.113				−0.465	
			(0.254)				(0.352)	
Negative Out-group				0.507				0.037
				(0.612)				(0.331)
Positive Out-group				0.230				−0.028
				(0.383)				(0.366)
All Treatments x Trust (Round 1)	−0.091*				0.016			
	(0.049)				(0.054)			
Negative x Trust (Round 1)		−0.052				−0.008		
		(0.060)				(0.059)		
Positive x Trust (Round 1)		−0.117**				0.049		
		(0.054)				(0.064)		
Out-group x Trust (Round 1)			−0.116				−0.078	
			(0.073)				(0.062)	
In-group x Trust (Round 1)			−0.062				0.199**	
			(0.057)				(0.086)	
Negative Out-group x Trust (Round 1)				−0.152				−0.108
				(0.119)				(0.075)
Positive Out-group x Trust (Round 1)				−0.115				−0.033
				(0.091)				(0.081)
N	1,214	1,214	991	552	1,074	1,074	769	652
Adjusted R^2	0.231	0.230	0.245	0.262	0.246	0.245	0.246	0.208

Notes: The models use benchmark OLS estimation. Models 1, 2, and 3 report results for respondents who reacted to the treatment (retweeted, replied, or liked); Models 4, 5, and 6 for those who ignored it. The dependent variable uses the number of votes subjects (principals) entrusted in round 2 to another player to be doubled and cast for the principal's candidate. A battery of individual-level pretreatment controls—such as, age, income, employment, education, gender, and individual level of trust—are controlled for in all six estimations.*p<0.1; **p<0.05; ***p<0.01

Table 10 Regression Models: Treatment Effects of Framing and Endorsement on Trust Split by Reaction to the Treatment (Mexico)

	Reacted to the Treatment				Ignored the Treatment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	2.181***	2.174***	1.631**	2.091**	2.894***	2.866***	2.786***	2.718***
	(0.613)	(0.614)	(0.748)	(0.856)	(0.620)	(0.620)	(0.788)	(0.853)
Delegate (Round 1)	0.476***	0.476***	0.479***	0.479***	0.442***	0.442***	0.443***	0.442***
	(0.043)	(0.043)	(0.043)	(0.043)	(0.048)	(0.048)	(0.048)	(0.048)
All Treatments	0.444*				0.036			
	(0.249)				(0.248)			
Framing: Negative		0.264				0.271		
		(0.305)				(0.282)		
Framing: Positive		0.576**				−0.231		
		(0.281)				(0.289)		
Out-group			0.758*				0.001	
			(0.392)				(0.353)	
In-group			0.417				0.128	
			(0.335)				(0.410)	
Negative Out-group				1.294**				0.249
				(0.576)				(0.478)
Positive Out-group				0.402				−0.233
				(0.471)				(0.442)
All Treatments x Delegate (1)	−0.101*				−0.003			
	(0.054)				(0.057)			
Negative x Delegate (1)		−0.066				−0.033		
		(0.067)				(0.064)		
Positive x Delegate (1)		−0.125**				0.026		
		(0.060)				(0.066)		
Out-group x Delegate (1)			−0.231***				−0.0005	
			(0.083)				(0.078)	
In-group x Delegate (1)			−0.100				0.038	
			(0.073)				(0.092)	
Negative Out-group x Delegate (1)				−0.283**				−0.093
				(0.129)				(0.102)
Positive Out-group x Delegate (1)				−0.182*				0.077
				(0.097)				(0.097)
N	1,130	1,130	798	552	1,162	1,162	684	565
Adjusted R^2	0.184	0.184	0.176	0.198	0.206	0.207	0.213	0.208

Notes: The models use benchmark OLS estimation. Models 1, 2, and 3 report results for respondents who reacted to the treatment (retweeted, replied, or liked), and Models 4, 5, and 6 for those who ignored it. The dependent variable uses the number of votes subjects (principals) entrusted in round 2 to another player to be doubled and cast for the principal's candidate. A battery of individual-level pretreatment controls—such as, age, income, employment, education, gender, and individual level of trust—are controlled for in all six estimations.*p<0.1; **p<0.05; ***p<0.01

E Appendix E: Human Subjects

Human Subjects approval by IRB was granted on February 17, 2020. The project approval is registered under the identification code [1552091-1] Transparency, trust, and Social Media: A Research Proposal. Consent was requested at the beginning of the survey and a disclaimer provided respondents with information on how to contact the researchers or IRB if needed. Details of the application, recruitment, consent, and disclaimers follow:

Subject Selection

a. Recruitment: The survey respondents were recruited by Netquest for the on-line survey, from their panel of Brazilian and Mexican respondents.

b. Eligibility Criteria: Participants were at least 18 years old of age and nationals from Brazil or Mexico.

c. Enrollment Numbers: A total of 2,400 respondents in each country, for a total of 4,800 respondents. The number of participants met national representative samples for each country and enough statistical power for the different experimental treatments in the survey.

Procedures

The final survey took 25 to 30 minutes. As described in the main document, it established a trust baseline where individuals were asked to select a ficticious "cartoon" candidate. They were also informed that if their candidate won the aggregate vote among

all survey respondents, they will be entered in a lottery to win an iPad. For each survey we distributed two (2) iPads, which means that the odds of winning an iPad were 2/2400 in each survey. The iPad lottery ensured that respondents saw a benefit in maximizing votes for their candidates over the curse of the survey. Respondents were informed that they would have different opportunities to increase the number of votes for their candidate, such as asking another respondent to deposit TWICE the number of votes they entrusted (trust). We honor all request by respondents when they asked votes to be deposited. Therefore, there was no deception on the tally of votes. The raffle was conducted online in the website RandomPicker.com according to published guidelines supervised by Netquest.

Respondents were also asked to read a statement that requests to deposit entrusted votes would be met (trustworthy). Voters were informed that they could decide if they wanted to deposit or not the votes entrusted to them. This exercise was repeated three times over the course of the survey.

Our team serve the roll of a universal player, accepting all deposits. Therefore, there was no deception and all votes were counted and used in the raffle, as reported to respondents.All decisions made by respondents were honored, all votes were counted, and that there was no deception. Once all surveys were completed, votes were tallied and two winners in each country received the iPad. As described in the article, the iPads represented between 1/2 and 1 1/2 times a minimum was in Mexico and Brazil respectively.

Risks

We anticipate only minimal discomfort associated with this procedure in case participants do not agree with the survey statements, or the topics covered by it. We mitigate this risk by allowing respondents to skip questions they do not feel comfortable answering, as indicated in the consent form.

Benefits

The findings are very important for increasing our general understanding of how social media affects political trust. There is no existing research modeling the effect of social media on political *trust behavior*. The experiment is of significant interest to NGOs and international donor agencies interested in understanding the effect of social media on democratic governance.

Confidentiality

The PI and team receive a de-identified respondent ID number. No private identifying information was stored in the servers of the PI or any other member of the team. Netquest will provide the emails of the winners of the raffle to deliver the awards. At no point during the recruitment, consent, or research procedures, will Vanderbilt, Qualtrics, or Netquest provide us with personally identifying information for participants. Thus for the full survey we will be able to adequately ensure the anonymity of all survey respondents.

Consent Process

The informed consent procedure provides participants explicit consent to proceed and informs of their right to skip questions and to discontinue the survey.

The online consent was granted by IRB by waiving written consent, given the following criteria: 1. Our research involves no more than minimal risk to the subjects. As we have stated, the only potential risk is minimal discomfort due to the nature of the questions asked, and we mitigate this discomfort by allowing participants to skip questions. 2. The waiver will not adversely affect the rights and welfare of the subjects. All subjects in these pre-test and survey will be fully informed about their rights as participants and the nature of the study, and they will have access to the consent form online to save and print for their records. 3. This research could not practicably be carried out without the waiver because it is entirely performed online. Therefore, none of the co-PIs could gather written consent forms for all participants. 4. Whenever appropriate, the subjects will be provided with additional information after participation. Participants will have access to contact information for both co-PIs and IRB, allowing them to reach out in case they have any further questions.

Research Outside of the United States

Both PIs to this project have extensive experience doing field research and working in Latin America. Both PIs are native Spanish and Portuguese speakers, which allows for our communication with all stakeholders involved in this study. We have reviewed existing regulations in Argentina, Brazil, and Mexico on data protection and privacy. We

are complying with this regulation by providing participants with all relevant information about the project and survey, by protecting data and participants' privacy through ensuring anonymity of the observations and data collected, and by making sure only co-PIs have access to the data collected. Given that the data collection process will be anonymous and no sensitive information is required as part of this process, we believe there is no additional risk participants in Argentina, Brazil, or Mexico will be exposed to, nor that they will be placed at risk of criminal or civil liability.

IRB Approval letter

The official approval letter is attached to this application. During the review process we include an anonymous version of the letter. It will be replaced by the formal approval letter after review is completed.

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