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Brief Report

Smartphone use decreases trustworthiness of strangers☆

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ABSTRACT

Trust is crucial for social, economic, and political relationships. Evidence shows the importance of trust in distinct areas, from the day-to-day forming of personal relations to the stability of democracies. In this paper, we ask how new technologies that compete for our attention affect the formation of trust between strangers. To that end, we study how smartphone use affects interactions with, and subsequent trust in, strangers. In our experiment, we had participants wait in groups of six for approximately 20 minutes, allowing them to interact as they wished. In one treatment, participants could use their smartphones during the wait time, while in the other they did not have access to their phones. We then randomly paired participants within each group to play a trust game and answer a brief survey. As predicted, we find that limiting phone access resulted in higher levels of trustworthiness.

1. Introduction

Mobile devices are ubiquitous. The average American checks their phones about 96 times per day, and spends more than 5 hours a day on it, with an inverse relationship with age (Flynn, 2023). As mobile devices become more omnipresent, an important question arises regarding their impact on social connections. Mobile devices have positive psychological impact through easy access to remote friends and family, as this can increase feelings of social connectedness (Pielot et al., 2014). However, this ubiquitous connection may also have social and emotional costs. For example, a study that tracked individuals throughout the day found that people feel lonelier while socializing over digital platforms such as Facebook (Kross et al., 2013).

When using smartphones, people incur opportunity costs. Virtual social interactions could be replacing in-person interactions, and even diminishing the quality of in-person interactions that do occur, due to being distracted while with others. For example, research has found that the presence of phones at a meal in a restaurant increased distraction, which led to less enjoyment of time spent with family and friends (Dwyer et al., 2018). Given how in-person interactions have been shown to predict subjective well-being and social connectedness, their results have troubling implications (Baumeister & Leary, 1995; Lyubomirsky & Boehm, 2010; Reis et al., 2000). In this paper, we consider a specific type of opportunity cost associated with the use of digital interactions—connecting with strangers. A

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study that manipulated whether participants had access to their phones while waiting in a room with strangers found that participants with phone access self-reported feeling less socially connected than those without phone access (Kushlev et al., 2017). By obviating the need to interact with strangers, smartphones may discourage people from interacting with others in society, even when it would be beneficial for them.

In this paper we focus on a different consequence of smartphones usage—trust. How does trust emerge in a world where smartphones are ubiquitous? Consider two waiting room scenarios. In one, several individuals who do not know each other are sitting, awaiting their turn, with access to their phones. In the other, their phones are out of reach. We expect people in the former scenario would choose to use their phones, avoiding the initial awkwardness but also forgoing opportunities to meet new people and potentially form new relationships. On the other hand, those without their phones might engage in conversation and learn about the other person (s). Although there are many interactions in which people can casually chat with one another—sitting in the back of an Uber, standing in line at a restaurant, waiting for a doctor's appointment, at the DMV, etc.—they may forgo the opportunity and instead seek entertainment using their mobile devices. Such behavior would likely result in greater "social distance" (the degree to which one person feels connected to another) between these individuals compared with the social distance we would expect among individuals who could not access their phone and chose instead to engage in casual conversations with others.

The current paper investigates whether smartphone presence affects interaction with strangers, leading to less trust behavior in a downstream task. Our prediction is that those with access to their phones will interact less with others than participants with no phone access, and that these lower levels of interactions will impact trust behavior. Trust is important from both a psychological and economic standpoint, as it is integral to human society and to economic interactions. As Arrow (1972) wrote, "virtually every commercial transaction has within itself an element of trust." While contractual agreements are important, trust between individuals in the economy is an essential component of many efficient economic interactions, and its importance in the functioning of markets is demonstrated in a large literature (Algan & Cahuc, 2010; Batrancea et al., 2019; Beckert, 2009; Guiso et al., 2008; Jetter & Kristoffersen, 2018; La Porta et al., 1997; Sapienza et al., 2013; Zak & Knack, 2001).

We study the effects of phone access on trust behavior in a pre-registered laboratory experiment. In our study, six participants were invited to the lab at the same time and asked to wait for twenty minutes around a joint table. We varied whether participants had phone access (PA) or no phone access (NPA) while they were waiting, and looked at the effect of this manipulation on interaction and on decisions in the Berg et al. (1995) trust game (see Johnson & Mislin, 2011 for a review on the trust game). As in the standard paradigm, the six participants were randomly matched in three pairs and interacted anonymously. After being randomly matched, participants were randomly assigned the role of either an investor or a reciprocator. The investor received a \$5 endowment and moved first, deciding how much of their \$5 endowment, \$x, they wished to send to the reciprocator. They were told the experimenter would triple this amount and give \$3x to the reciprocator. In the second stage, reciprocators chose \$y, the amount of money they wished to send back to the investor, out of the \$3x they received. Hence, at the end of the game, investors' earnings were $\$(5 - x + y)$, and reciprocators' earnings were $\$(3x - y)$. All rules were common knowledge.

The equilibrium of this game, under the assumption that players are selfish profit maximizers, is for both participants to send zero dollars, in which case the investor earns \$5 and the reciprocator \$0. However, this outcome is Pareto inefficient because it is possible to have other outcomes in which both the investor and the reciprocator are better off. Whenever investors send a positive amount and the reciprocators send back more than they received, then both players can earn more than in equilibrium. For such efficient outcomes, investors need to take a risk and trust that the reciprocators will not be selfish, but rather choose to send back more than \$x. Investors are exposing themselves to a risk: If the reciprocator indeed reciprocates, then this investment pays off; if not, investors lose money and suffer the psychological cost that accompanies such an outcome. Many studies have used the trust game to measure prosocial behavior in a variety of domains, including guilt aversion (Bellemare et al., 2019), changes in preferences (Maggioni et al., 2018), and morality and social norms (Bonowski & Minnameier, 2022). The measure has been validated as capturing trust as a psychological construct (Sofianos, 2022).

Originally, we anticipated an effect on both trust and trustworthiness. However, informed by a pilot study (reported in supplement), we pre-registered that investors would not differ significantly in amount sent by treatment. Our main hypothesis is that reciprocators will differ significantly in the amount sent back and the proportion sent back by treatment, such that reciprocators in the no phone access (NPA) will send more/a higher proportion back. Additionally, we predicted that participants in the NPA treatment will interact significantly more with one another than participants in the phone access (PA) treatment, as measured through self-reported interaction and through incentive compatible questions. Given the mixed results with respect to gender differences in trust game outcomes (Van Den Akker et al., 2020), we did not predict any effects of gender on trust or trustworthiness. The preregistration for this study can be found online at https://aspredicted.org/2KX_LNN.

2. Method

Our study varied whether groups of participants had access to their phone during a waiting period, and look at the effect this manipulation had on interaction and on decisions in the Berg et al. (1995) trust game. The study was conducted at the University of California San Diego, and was run from October of 2019 to February of 2020. In the supplement, we report results from an exploratory pilot run in Spring 2019, and we report results from the pooled data for increased power. The only thing that differed between the two studies was that in our pre-registered study, we removed the two scale questions on social connectedness.

Our total sample size was 276 participants in 46 groups, and our final sample size was 240 participants in 40 groups. We drop six sessions using the following procedure: Following a debrief on sessions and prior to seeing any of the data, the second author determined whether groups should be excluded. This was not part of our pre-registration, but followed the same procedure as our pilot.

The reasons for exclusion were: Session 1, 2, 3, 7 and 8: training experimenter; Session 4: non-student with child (see supplement for details).

Participants were recruited in groups of six, and were randomly assigned to either a Phone Access (PA) or a No Phone Access (NPA) treatment. Upon arrival to the lab room, participants in the NPA treatment were asked to take out their phones, turn them off, and place them into their backpacks. Participants in the PA treatment were allowed to use their phone while waiting. This was the only difference in procedure between the NPA and PA treatments. Participants in both condition were seated around a table, and were told that the experimenters would be pulling each participant out to go over the consent form and get some general information. An experimenter then took one participant out at a time to a smaller room, where they reviewed the consent form with another experimenter and completed a survey asking for their name, gender, age, major, and class year (Sutter & Kocher, 2007). This was repeated for all six participants such that in total, each participant waited for approximately 20 minutes with the other participants in the room.

After the last participant was brought back into the room, the experimenter asked all participants to move to their computers and begin the study. Participants were told they would be randomly and anonymously paired with another participant for this part of the study, and that they would receive any payoff they got in cash at the end of the study. Participants then clicked a link in Qualtrics that took them into a trust game session in oTree (Chen et al., 2016), where they were randomly paired to play the standard trust game (Berg et al., 1995). They played the trust game once during the session (i.e., there were no repeated interactions).

Once participants finished the game, they returned to the Qualtrics survey and indicated whether they interacted with any of the other participants in the room, or if any of the other participants in the room interacted with one another. If they indicated “Yes” to either question, they were asked to list as many names, class years, and majors they could remember. These interaction questions were incentivized ($0.25 per correct answer).

Our key dependent measures were the proportion of money sent back by the reciprocators in the trust game, whether the participants interacted with one another (Yes/No), and the number of interaction questions participants answered correctly (out of 15).

3. Results

We pre-registered four main analyses to test our hypotheses. Using regressions that clustered standard errors on the session level, we looked at the effect of Treatment on (1) the proportion of money Reciprocators sent back, (2) the amount Reciprocators sent back, (3) the amount of money Investors sent, and (4) the number of correctly answered interaction questions. We ran all regressions with clustered standard errors. There were 20 sessions per treatment, and 60 pairwise observations per treatment. Any control variables used are specified in line.

The proportion sent back reflects the amount reciprocators sent back divided by the amount investors sent. A proportion under one indicates that, on average, reciprocators sent back less money than the amount investors sent to them. A proportion equal to one indicates that, on average, reciprocators sent back the same amount of money investors sent to them. A proportion higher than one indicates that, on average, reciprocators sent back more money than investors sent to them.

Consistent with our main hypothesis, we found a difference in the proportion sent back in the NPA treatment: The average was 1.16, compared with 0.91 in the PA. Notably, reciprocators in the NPA treatment sent back more than the amount they received. This difference is not significant with a two-sided test ($t = -1.90, p = .060$). However, given our preregistered hypothesis was directional, a

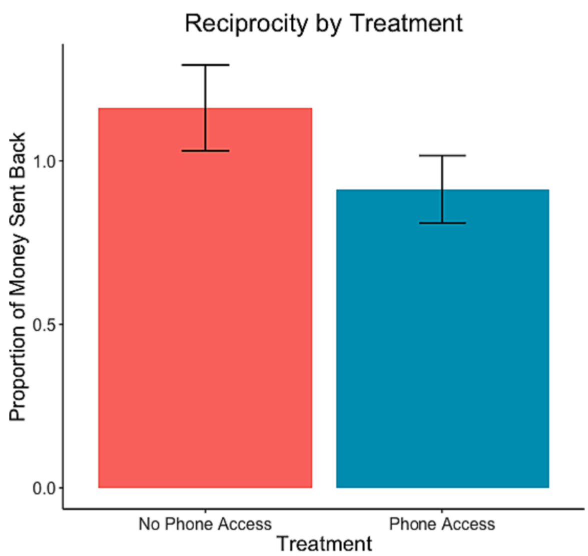

Fig. 1. Participants who did not have access to their phones while waiting (NPA treatment) sent back, on average, 116% of the money they received (left hand bar). This means that on average, they sent back more than the amount the investor sent them (before it was tripled). Participants who had access to their phones while waiting (PA treatment) sent back, on average, only 91% (right hand bar). Bars are standard error bars.

one-sided test is appropriate, providing support for our hypothesis; we report the two-sided test to be both conservative and transparent. The result is presented in Fig. 1. In the supplement, we report results from an exploratory pilot run in Spring 2019 and results from the pooled data for increased power. We find the difference to be significant in the pooled data ($t = -2.38$, $p = .019$).

There is no significant difference in Sent Back Amount between treatments: $4.10 on average in the NPA treatment versus $3.50 in the PA treatment ($t = -0.98$, $p = .330$). This finding is in line with studies using the trust game, which typically use sent back proportion as the primary DV of interest, as conditioning the returned amount on the sent amount better captures reciprocity (Berg et al., 1995; Croson & Gneezy, 2009).

In line with our prediction, we found no difference in Sent Amount by Treatment: In the NPA treatment, Investors sent $3.46 on average, and in the PA treatment, they sent $3.48 on average ($t = 0.09$, $p = .932$). Table 1 below shows the means by treatment.

We initially expected effects on trust because we thought that all NPA groups would chat. It is possible that trust is a bit harder to move, and that our manipulation was too conservative in that we allowed participants to choose whether to interact in both groups. Informed by our pilot study, we ran a pre-registered exploratory test to examine why no difference in sent amount emerges. We regressed Sent Amount on Treatment, and added the term Investor Interacted (i.e., whether the investor self-reported interacting with anyone else in the room) to the clustered regression. We find that when investors in the NPA treatment indicated no interaction with other participants, they sent significantly less (mean = $2.55, sd = 1.61) than when they indicated interaction (mean = $3.66, *sd* = 4.26). Put simply, investors in the NPA treatment sent less on average than those in PA treatment when they indicated no interaction ($t = -2.42$, $p = .017$). Once investors in the NPA treatment interacted, they sent significantly more ($t = 2.50$, $p = .014$). In other words, relationships in the NPA were more extreme than in the PA treatment, with sent amount as a proxy for relationships. While we did not specify a hypothesis for this finding, the intuition is that when participants sit with a group of people and no one is talking, and they do not have their phone as an alternative, participants might sit awkwardly and dislike the experience. But as soon as someone interacts, the experience gets better. Table 2 and Fig. 2 depict this result. We note that this result replicates in the pilot data and the pooled data as well, and may be an interesting direction for future studies to explore.

We next looked at backtransfers conditional on whether the treatment led to interactions, and find that interactions did not have an effect on backtransfers. More specifically, we regress Sent Back Amount on Treatment, and add the term Reciprocator Interacted (i.e., whether the reciprocator interacted with anyone else in the room) to the clustered regression. We find no effect of Treatment ($t = 0.83$, $p = .411$) and no effect of Reciprocator Interacted ($t = -0.70$, $p = .483$) on Sent Back Amount. We find no interaction effect of Reciprocator Interacted and Treatment on Sent Back Amount ($t = -0.29$, $p = .772$).

We regress Sent Back Proportion on Treatment, and add the term Reciprocator Interacted (i.e., whether the reciprocator interacted with anyone else in the room) to the clustered regression. We find a significant effect of Treatment ($t = 2.94$, $p = .004$) on Sent Back Proportion, such that those in the NPA treatment sent back proportionally more. This supports our main finding. We do not find a significant effect of Reciprocator Interacted ($t = -0.52$, $p = .601$) on Sent Back Proportion. We find no interaction effect of Reciprocator Interacted and Treatment on Sent Back Proportion ($t = -1.58$, $p = .116$).

Next we examine the more basic question of whether interaction varied by treatment, using a two proportions z-test. Interaction was measured using a self-report yes/no answer to the question, *Did you speak to any of the other participants in the room?* We found that a significantly greater proportion of participants in the NPA treatment indicated they interacted with other participants (99/120; 82.5 %) than in the PA treatment (75/120; 62.5 %) ($\chi^2 = 11.055$, $p < .001$). Clustering standard errors on the session level, we regress self-reported interaction on treatment and do not find that significantly more participants in the NPA group reported interacting than those in the PA group ($t = -1.73$, $p = .085$) (see Fig. 3).

We additionally look at our incentive compatible measure of interaction. We examine the number of incentivized questions answered correctly by treatment, and find no significant difference ($t = -0.97$, $p = .332$); this result does not support our hypothesis. A potential reason for this result is that we concentrated on the wrong information, namely information that undergraduates plausibly trade when first meeting one another. Our data supports this idea; we find that participants got on average only 3.94 names/dates/class years correct out of a possible 15 (*sd* = 3.55). In open ended text fields that were not incentivized, we asked participants what they talked with others in the room about. We find that in many cases, participants reported exchanging no basic information, but rather talking about the weather, about participating in lab experiments, etc.

We pool our pilot data with data from the main study and report results in the supplement. In this larger dataset, we regress self-reported interaction on treatment and find that significantly more participants in the NPA group reported interacting than those in the PA group ($t = -3.12$, $p = .002$) (see supplement pp 11–12). Looking at the number of incentivized questions answered correctly, we also find a significant difference ($t = -2.13$, $p = .034$). Analyses from this larger dataset support the idea that without phone access, participants are more likely to interact and share information about one another, which may decrease social distance, and thus increase trustworthiness.

In the supplement, we report the results of other analyses for which we did not have pre-registered hypotheses. Specifically, we test whether interaction mediates the relationship between phone presence and trust, again operationalizing interaction in two ways: (1) whether participants indicated they interacted with anyone else in the room, and (2) the number of interaction questions participants

Table 1
Average amount sent, sent back, and proportion sent back by treatment, with 60 pairs in each treatment. Standard deviations in parentheses.

Treatment	Sent Amount	Sent Back Amount	Sent Back Proportion	# of Pairs
No Phone Access	$3.46 (1.49)	$4.10 (3.09)	1.16 (0.71)	60
Phone Access	$3.48 (1.52)	$3.50 (2.74)	0.91 (0.64)	60

Table 2
Breakdown of the average amount invested by treatment and whether the investor self-reported indicating interacting with others.

Treatment	Investor Interacted	N	Average Amount Invested	Standard Deviation	Standard Error
Phone Access	No	25	3.72	1.76	0.352
Phone Access	Yes	35	3.31	2.66	0.450
No Phone Access	No	11	2.55	1.61	0.485
No Phone Access	Yes	49	3.66	4.26	0.609

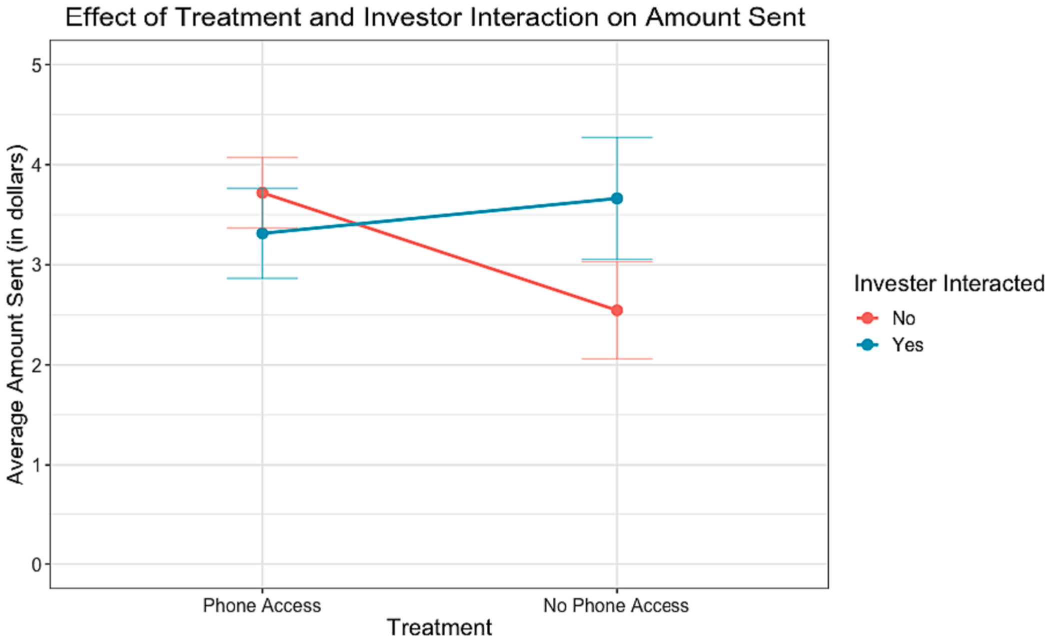

Fig. 2. Investors in the NPA treatment sent significantly less (mean = $2.55) than investors in the PA treatment (mean = $3.72) when they indicated no interaction. However, when investors in the NPA treatment indicated interaction, they sent just as much as investors in the PA treatment, and significantly more than investors in the NPA treatment who indicated no interaction (mean = $3.66). Bars are standard error bars.

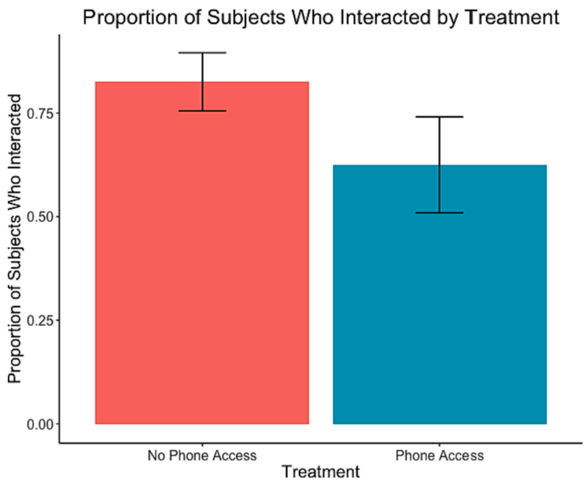

Fig. 3. The left-hand bar depicts the proportion of participants (0.825) in the NPA treatment who self-reported interacting with others in the room. The right-hand bar depicts the proportion of participants (0.625) in the PA treatment who self-reported interacting with others in the room. Bars are standard error bars.

answered correctly. We also examine questions like whether participants differed by treatment in their willingness to participate in future studies, whether gender impacted any results, etc. Given the lack of pre-registered hypotheses, we relegate these analyses to the supplement and encourage interpretation with caution.

4. Discussion

People use their phones even during social interactions. Extant literature has shown how such smartphone use can affect our brain. Ward et al. (2017) show that even the mere presence of one's own smartphone may reduce available cognitive capacity. Their "brain drain" hypothesis experimentally demonstrates how the presence of smartphones depletes our limited-capacity cognitive resources, thereby leaving fewer resources available for other tasks and undercutting cognitive performance. Apart from its cognitive cost, the presence of smartphones during social interactions can impact subjective well-being and social connectedness (Baumeister & Leary, 1995; Lyubomirsky & Boehm, 2010; Reis et al., 2000).

Yet, to the best of our knowledge, the economic consequences of phone use with respect to social interactions have not been studied. The case we examine here focuses on interactions with strangers. The data presented above, as well as our daily experiences, suggest that short interactions with strangers have changed dramatically since smartphones became omnipresent in life. Does this change affect economic outcomes? We hypothesized that the efficiency of outcomes in trust-related interactions may be reduced if people, instead of engaging in conversations with others, engage with their smartphones. Note that our sample is WEIRD (Western, Educated, Industrialized, Rich, and Democratic), which could impact the findings. For example, a recent study found that WEIRD participants have a lower willingness to discriminate in their investment behavior (Chuah et al., 2023).

In line with our pre-registration, we find that phone access impacts trustworthiness, such that participants without access to their phones sent a higher proportion back than those with. A potential explanatory mechanism for this difference comes from the literature on social distance—the degree to which one person feels connected to another (Bogardus, 1933). This literature shows that people (are expected to) act more favorably to those they feel closer to in a given situation. Studies have shown that social distance in the lab affects behavior in similar games, such as the dictator game. In these papers, social distance is typically manipulated by revealing details about the opponent. Social distance can affect the extent to which fairness considerations are active (Bohnet & Frey, 1999; Charness & Gneezy, 2008). With anonymity, one has a purely intrinsic motivation to behave fairly; when people can identify each other, the fairness norm is partially activated; and when people can also communicate with each other, the fairness norm is strongly active. Building on Johnson and Mislin (2011), who found some support that anonymity negatively affects trust game outcomes, and Scharlemann et al. (2001), who find that a simple smile can elicit cooperation, we argue that the presence of smartphones increases the social distance that the participants experience.

We attempt to capture social distance with our incentive compatible measure of interaction. We note that the lack of treatment differences in the accuracy with which participants recalled personal information about one another may be explained by the idea that social bonds are created through informal conversation. It is difficult to capture soft information, such as paralinguistics and context (e.g., how one speaks and what one speaks about), in an incentive compatible or even tractable way. However, this type of informal conversation has been found to impact trust behavior. Studies have found that participants exhibit greater trust and trustworthiness when gossip (i.e., trustor receives messages about the trustee's behavior from a third party) is present than not (Fehr & Sutter, 2019), and even when inaccurate gossip is present than when communication is impossible (Fonseca & Peters, 2018). It is possible that a different operationalization of interaction may be necessary to show that smartphones accessibility may decrease informal conversation amongst strangers, which in turn may detrimentally impact trustworthiness.

Given the omnipresence of smartphones in our day to day lives, we denote the importance of studying how they might affect social interactions and the formation of new ties among individuals in society. Our research takes a step forward in this direction and suggests that the presence of smartphones negatively impacts certain aspects of trust behavior, namely, trustworthiness.

CRediT authorship contribution statement

Sandy Campbell: Conceptualization, Data curation, Writing – original draft, Writing – review & editing, Visualization, Investigation, Validation, Formal analysis, Methodology, Project administration, Software. **Uri Gneezy:** Conceptualization, Funding acquistion, Writing – review & editing, Superivison, Resources, Project administration, Software.

Data availability

The data is publically available via OSF

Appendix A. Supplementary material

Supplementary material to this article can be found online at https://doi.org/10.1016/j.joep.2024.102714.

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