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# The effect of social distancing on trust and solidarity[☆]

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ABSTRACT

This paper investigates the effect of social distancing on behavioral traits (trust, trustworthiness, and solidarity) that are important in interpersonal relationships. Toward this goal, we exploit the exogenous manipulation of social distancing provided by the different timing of stay-at-home orders issued in New York and Arizona during the COVID-19 outbreak. The experimental design also entails a between-subject manipulation in order to distinguish In-group vs. Out-group effects. The results show a positive In-group effect on trust, robust to the degree of compliance to the treatment, while we do not find evidence of significant effects on the other behavioral traits.

## 1. Introduction

A significant body of research investigates the impact of the restrictions implemented in the spring of 2020 to contain the spread of the COVID-19 pandemic. Some direct effects are well-known. For instance, shelter-in-place and stay-at-home orders have increased the feeling of loneliness (Babin et al., 2021; Killgore, Cloonan, Taylor, Miller et al., 2020; Kotwal et al., 2021), which represents a significant mental health concern (Killgore, Cloonan, Taylor and Dailey, 2020; Le & Nguyen, 2020).[1]

Social distancing may also affect individual preferences, particularly those relevant in interpersonal relationships. Preferences are usually modeled as innate and stable traits but there is an extensive literature showing that they react to external factors. Schildberg-Hörisch (2018) proposes a framework that accommodates evidence on systematic changes in preferences due to exogenous shocks or temporary changes in self-control resources, emotions, or stress. Several types of socio-economic and environmental shocks have been shown to affect preferences.[2] Also the sense of belonging, attachment, and identification with one's social group have also been shown to affect individual preferences.[3] The pandemic potentially involved most of these factors and we therefore believe that its effects on interpersonal preferences are worth being investigated.

The goal of this paper is to study how the amount of social relationships affects some of the classic and most relevant interpersonal preferences, i.e. trust and trustworthiness (Berg et al., 1995; Johnson & Mislin, 2011; Schotter & Sopher, 2006) as well as solidarity (Selten & Ockenfels, 1998). Given the huge degree of uncertainty induced by the pandemic, we also elicit the individual level of risk aversion as a potentially crucial control.

☆ The experiment has been pre-registered at the AEA RCT Registry (ID AEARCTR-0005629). We would like to thank Giovana D'Adda, Daniela Di Cagno, Massimiliano Bratti and participants to a seminar at the University of Milan, as well as to participants to the "Special Online Conference on Experimental Insights from Behavioral Economics on COVID-19" for useful suggestions. All remaining errors are ours.

Noemi Pace passed away after the submission of the manuscript. This article, developed thanks to her unwavering commitment despite her illness, reflects her profound passion for her work as well as her intellectual depth, rigor, and creativity as a researcher, and is dedicated to her memory.

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[1] These effects have been observed despite the dramatic increase of interactions through the internet. In principle, these results should therefore be considered as the net effect induced by the restrictions, and the same interpretation should be given to the results in our paper.

[2] Among the others, see Cameron and Shah (2015), Cassar et al. (2017) and Gneezy and Imas (2017) for the effect of environmental shocks, Jakiela and Ozier (2019), Jetter et al. (2020) and Voors et al. (2012) for socio-economic shocks.

[3] See for instance Benjamin et al. (2010), Chen and Li (2009) and Guala and Filippin (2017).

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We investigate the change in these behavioral traits exploiting the exogenous, though imperfect, manipulation of social distancing induced by lockdown measures during the COVID-19 outbreak. The design consists in a two-wave experiment relying upon the different timing of stay-at-home orders issued in the US. In the first wave we administer incentivized tasks to respondents in Phoenix and Tucson, Arizona, on the same day (31th March 2020) in which a stay-at-home order was going to take effect, as well as in New York City where a stay-at-home policy had been already in place for two weeks. We then re-tested the same respondents on the 15th of April, when the stay-at-home policy had displayed its effects also in Arizona. Respondents in Arizona constitute the treatment group, while respondents in New York City represent the control group.[4]

Our experiment entails a mixed (between-within) subject design, described in detail in Section 2 below. The within subject approach outlined above allows us to get rid of the observable and unobservable heterogeneity that is common across experimental groups. The between subject manipulation distinguishes the change of preferences along an In-group vs. Out-group dimension. This distinction is meant to capture what happens at different levels of social distance and proxies a different intensity of interpersonal relationships.

Several determinants linked to social distancing seem to potentially affect the behavioral traits under investigation in a conflicting manner, rendering the overall effect not clear ex ante and therefore interesting to study. On the one hand, there are several reasons to envisage a negative effect of social distancing on trust, trustworthiness and solidarity. First, the implementation of a stay-at-home order may signal an increase in the risk of interacting with other people, something that can erode interpersonal trust, trustworthiness and solidarity. However, there is not such a risk in the experiment, and therefore this effect should be of second order importance. Second, a negative effect could be observed also because of the negative emotions induced by stay-at-home orders, particularly those not characterized by personal control such as anxiety (Dunn & Schweitzer, 2005; Myers & Tingley, 2016). Third and foremost, the behavioral traits under investigation are typically reinforced by proximity to one's counterparts (see for instance Attanasi et al., 2017, 2013) and should therefore be negatively affected by social distancing.

On the other hand, it has been shown that COVID-19 changed moral views inducing people to give priority to society's problems rather than one's own problems (Cappelen et al., 2021). Furthermore, containment measures may also strengthen the ties within a community triggering a feeling of "common fate", generally regarded as an important determinant of group identity (Brewer & Kramer, 1986; Zizzo, 2011). Group identity is defined as a set of beliefs, preferences, norms, or decision heuristics that are dependent on group membership (Akerlof & Kranton, 2000, 2010; Bacharach, 2006). Group identity correlates with pro-social attitudes towards the members of one's group making us more altruistic, egalitarian, trusting, trustworthy, and cooperative towards fellow members (Balliet et al., 2014; Charness et al., 2007; Chen & Li, 2009; Romano et al., 2017). A positive impact on the variables observed should be expected in this case.

The net effect of these counterbalancing forces is uncertain a priori. Consequently, we do not have a directional hypothesis as regards the overall effect of social distancing on trust, trustworthiness, and solidarity.

The In-group vs. Out-group manipulation is meant to mimic a different intensity of interpersonal relationships. In particular, it is meant capture different effects of social distancing inside and outside a community. It is reasonable to assume that common fate effects and some elements of group identity can be observed only at the community level. In this case we have a directional testable implication. We expect a relatively more positive (and possibly positive in absolute terms) effect of social distancing towards In-group rather than towards Out-group members (where a negative effect in absolute terms may be envisaged). It is worth mentioning that hostile attitudes towards out-groups members have been observed during the pandemic even without objective reasons underneath (Bartoš et al., 2021).

As far as risk aversion is concerned, we expect to observe an increase in Arizona (the treatment group) as compared to New York City (the control group). The reason is that the introduction of stay-at-home policy should also signal a high risk of contagion. A risk that cannot be avoided and cannot be insured is known in the literature as background risk. It has been shown that background risk makes individuals less tolerant towards other, avoidable risks (Eeckhoudt et al., 1996; Gneezy & Imas, 2017).

The recent literature studying the effect of the restrictions on our preferences of interest provides mixed results. A short-run decrease in trust is found in the aftermath of the lockdown in Wuhan (Shachat et al., 2021) and in France (Casoria et al., 2024). The fear of the COVID-19 pandemic is negatively correlated with trust also in 3 repeated cross-sections in the US (Alsharawy et al., 2021). Opposite results are obtained by Sibley et al. (2020) comparing matched samples of New Zealanders before and during the first 18 days of lockdown: a higher trust in science, politicians, and police is found in the lockdown group despite higher rates of mental distress. A similar finding emerges among Dutch respondents (Groeniger et al., 2021). An increase of interpersonal trust is also found by Ellena et al. (2021) in Italy and by Esaiasson et al. (2020) in Sweden.[5]

A possible explanation for this lack of consensus is that the evidence is typically based on repeated cross-sections in which heterogeneity and unobservable characteristics play a critical role. Note that even a pure within-subject approach would not be sufficient to identify the causal effect of social distancing on the behavioral traits under investigation. The reason is that a plenty of artifacts may confound the repeated measurement of an outcome over time. This is particularly relevant in a turbulent period such as the COVID-19 outbreak. The empirical strategy of our study (see Section 3) aims to substantially reduce this measurement error adopting a Difference-in-Differences approach, thereby aiming at identifying the causal effect of social distancing.

The results, presented in Section 4, match these expectations only in part. We observe a positive and significant In-group effect on trust, while in the Out-group condition trust decreases but without reaching traditional significance levels. We do not find any evidence of significant effects on trustworthiness, solidarity, and risk aversion. Our analysis then takes into account a possibly imperfect compliance to the treatment that makes the introduction of the stay-at-home order in Arizona simply an intention to treat. Our findings are corroborated by a robustness check in which we include the individual compliance to the treatment. For the respondents who self-reported a decrease of social interactions between the two waves we find a stronger effect of social distancing on trust towards In-group members, while the null results are confirmed across the board.

[4] One could reasonably argue that when the stay-at-home order was enforced in Arizona it was not unexpected. It should be noted, however, that no earlier than on March 23 the governor of Arizona explicitly stated that there was "no immediate plan to adopt shelter-in-place policy due to virus". On the same date the governor also issued an executive order barring local governments from going further than the state. Hence, there are good reasons to believe that the order was somehow unexpected a few days later. In any case, in Section 4.1 we deal with the issue of imperfect compliance to the treatment.

[5] Mixed evidence is also a common feature of other behavioral traits. For instance, risk aversion (Alsharawy et al., 2021; Harrison et al., 2021; Shachat et al., 2021), cooperation (Babin et al., 2021; Buso et al., 2020; Shachat et al., 2021) and pro-social behavior (Aksoy et al., 2021; Branas-Garza et al., 2022; Loeschel et al., 2021).

## 2. The experiment

The experiment has been pre-registered at the AEA RCT Registry (ID AEARCTR-0005629). The sample is composed of respondents in New York City, Tucson (Arizona), and in the metropolitan area of Phoenix (Arizona). Given the impossibility of gathering people during the outbreak, the experiment was conducted online using the Prolific platform, in accordance with all the relevant guidelines and regulations. Subjects provided their explicit consent to participate and were free to withdraw at any time during the experiment. As a purely observational study, the experiment is considered automatically preapproved by the University of Teramo. In what follows we describe the selection of our sample, the experimental tasks, the design, the procedures, and a power analysis.

### 2.1. Selection of the study areas and screening phase

We first filtered the Prolific database in the US based on the state of residence of the respondents. For our treatment condition we preselected only the states with a sufficiently large number of subjects and at the same time not having already implemented a stay-at-home policy at the state level or at least in large cities: Arizona, Kentucky, Florida, and Virginia. Subjects in these states were invited to a screening phase in the week March 22–March 29, 2020 with two goals. The first was to obtain their city of residence in order to run the In-group treatment (see below Section 2.3). The second was to have readily available a list of active and responsive participants, given our need to run Wave 1 as soon as the stay-at-home policy was announced in one of these states. For this second reason we also ran the screening in New York City, where the stay-at-home policy had already been implemented on March 20. The screening paid $0.25 (flat) per respondent, whether selected or not for the main study.

In Kentucky, available participants were scattered around the state, making it impossible to run the In-group treatment at the city level. Jacksonville was the only large city without stay-at-home policy in Florida, but we found only 30 participants. Arizona and Virginia had a sufficient number of subjects. These two states issued a stay-at-home order on the same day (March 30), so we opted for Arizona, where the number of participants available was higher and less disperse.

In Arizona there were 178 subjects living in the metropolitan area of Phoenix (knowing in which city/suburb), or in Tucson. In New York City there were 258 subjects. Assuming a response rate of about two thirds we targeted the study to involve 120 subjects in each state. We therefore invited all the 178 subjects in Arizona as well as 180 subjects in New York City trying to replicate a similar distribution by gender and age.

### 2.2. Incentivized tasks

*Trust and trustworthiness.* Interpersonal trust and trustworthiness are measured by means of a classic mini-trust game involving binary options, as described in Fig. 1.[6] Player A decides whether to keep one dollar or not. If passed, Player B receives $4 and decides whether to keep the whole amount or instead to return $2.2 back.[7]

The subgame perfect equilibrium based on monetary incentives predicts that both players should keep the money. Therefore, choosing to pass is interpreted as a measure of trust and the decision to return as a measure of trustworthiness. Binary choices are often chosen to make easier to administer the game in strategy method. The game is played one-shot in each wave and choices are made both as Player A and as Player B, with resolution of role uncertainty at the end of the whole study.

[6] The detailed instructions of the experiment are reported in Appendix A.

[7] As noted by Ermisch and Gambetta (2006) a structure of the game without equal payoffs constitutes a cleaner measure of trustworthiness by preventing choices to be driven by fairness.

**Table 1**
Experimental conditions (stay-at-home).

| | Wave 1 | Wave 2 |
|---|---|---|
| Arizona (AZ) | Untreated | Treated |
| New York City (NY) | Treated | Treated |

*Solidarity.* Solidarity is measured using a simplified version of the classic solidarity game (Selten & Ockenfels, 1998) played one-shot in each wave. In this three-player game each subject has a 2/3 chance of getting $3 and must decide how much to pass to the ex post loser in case (s)he wins. Each player can pass any amount (from 0 to $3 in 50 cent steps) to the loser with the possibility of giving different amounts to the two opponents, something that is relevant towards the In-group vs. Out-group analysis (see below Section 2.3). The main difference of our version as compared to the classic solidarity game is that a single dice rolling is made by the experimenters for all the three players, rather than resolving uncertainty independently. Assigning the numbers $\{1, 2, 3, 4\}$ to Player A, $\{1, 2, 5, 6\}$ to Player B, and $\{3, 4, 5, 6\}$ to Player C ensures that one and only one player loses in the game. This simplified version retains the incentives and the main features of the original game, while at the same time reducing the range of possible outcomes by fixing at two the number of winners.

*Risk aversion.* Risk aversion is elicited using a simplified version of the static Bomb Risk Elicitation Task (BRET, Crosetto & Filippin, 2013). In this task, participants are presented a winding road in which they earn $0.1 for every step taken in numerical order (Fig. 2).

Participants choose how many steps to take, $k_i \in \{1, 100\}$, knowing that a time bomb is hidden behind one (and only one) of the 100 steps with equal probability. The position of the bomb, $b_i \in \{1, 100\}$, is randomly determined at the end of the study. If $k_i \geq b_i$ it means that the subject stepped on the bomb, which by exploding wipes out the earnings. In contrast, if $k_i < b_i$ the subject receives $0.1 for every step taken. The number of steps taken provides a measure of risk attitudes: the lower the number, the more risk-averse the subject. $k_i = 50$ represents a risk-neutral choice.

The BRET is sufficiently intuitive and does not require to describe explicitly all the lotteries in terms of outcomes and corresponding probabilities. This feature renders the task suitable for an online study where the experimenters have a lower control over the instruction phase. To reduce noise, in the second wave we remind participants the number of steps chosen in the first wave, stressing that they are free to choose any number.

### 2.3. Treatments

The design entails a mixed between-within subject design. The main treatment is the change in the stay-at-home policy in Arizona. We also implement a between subject manipulation in order to differentiate the effect of social distancing along an In-group vs. Out-group perspective that mimics a different intensity of interpersonal relationships.

*Main treatment: Stay-at-home policy.* All the incentivized tasks have been administered in both waves. The main treatment, summarized in Table 1, identifies different conditions according to the different timing in the introduction of the stay-at-home policy. We launched the first wave on the 31th March when it was early morning in Arizona, while the stay-at-home order took effect at 5 pm. Therefore, respondents in Arizona are assigned to the 'Untreated' condition in Wave 1.

We rely upon a Difference-in-Differences in Reverse (DDR) approach (Kim & Lee, 2019) in order to identify the causal effect of social distancing. The DDR is suitable in settings in which one group is always in the "Treated" condition and one group switches from the "Untreated" to the "Treated" condition and has been extensively used in the literature (see Chemin & Wasmer, 2009; Kotchen & Grant, 2011; Rizzica et al., 2020; Rossi & Villar, 2020, among the others).

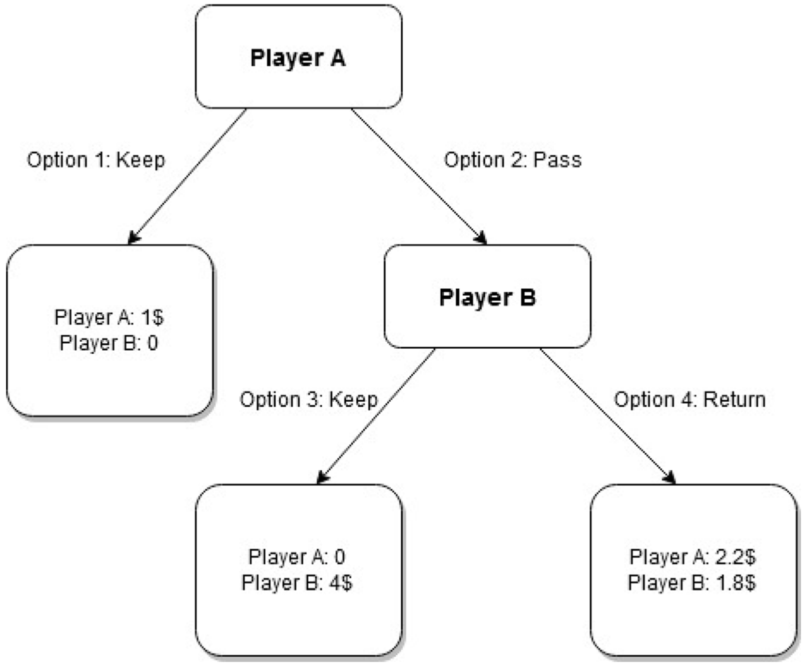


**Fig. 1.** Mini trust game.

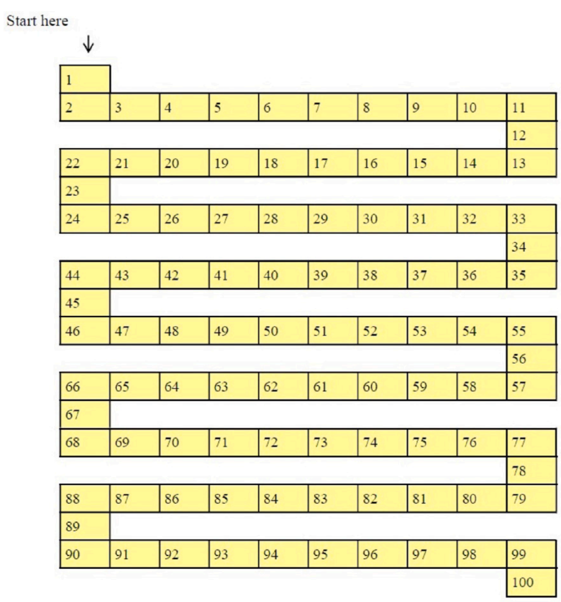


**Fig. 2.** Bomb risk elicitation task.

The DDR differs from a traditional Difference-in-Differences because it is characterized by a comparison group that is always treated, but the underlying assumptions and the interpretation of the results are equivalent. This method is formally described and compared to a traditional Difference-in-Difference approach in Appendix B.[8] In our setting the DDR compares the change over time in the behavioral traits of the subjects in Arizona (the group that switched from being "Untreated" to "Treated") with those of the subjects in New York City (the always "Treated" group). If the difference in the trends is statistically significant we conclude that social distancing as proxied by the stay-at-home policy plays a role.

To shed some light on the causal effect of social distancing the assumption required is that time does not affect differently treated and untreated units. In other words, absent the treatment, both groups should follow a parallel trend. Moreover, the treatment effect needs to be constant. It is admittedly difficult to provide compelling evidence that the common trend assumption holds in a turbulent period such as the outbreak of the pandemic, due both to unobserved heterogeneity and to practical reasons like the different number of tests administered. However, available data show that between the two waves the spread of the pandemic followed indeed a common trend. The fraction of the population testing positive roughly tripled in both states (from 0.18 to 0.54 in Arizona and from 3.89 to 10.99 in New York City).[9] As regards the time invariance of the treatment, we acknowledge that our study is bound to investigate short run effects. While the effect of social distancing may reasonably assumed to be constant in the limited spell of time of our study, our empirical exercise is not equipped to shed light on how the effects of social distancing evolve over time.

Social distancing may react imperfectly to the stay-at-home order, however. Citizens may not adhere to the order, or be exempted because employed in essential services. Furthermore, respondents in Arizona may have reduced social interactions even before the stay-at-home order took place. In other words, compliance to the treatment is not guaranteed and therefore the stay-at-home order should be interpreted as an intention to treat. Thanks to the questions administered at the end of each wave of the experiment we can build a reliable proxy of the compliance to the stay-at-home order. We use this variable to check the robustness of our results in Section 4.1.

[8] In principle, a situation in which several units receive a treatment at different times would require to use a two-way fixed effects DD estimator (see Goodman-Bacon, 2021, among the others). However, we only have two points in time and two groups, with the later group that changes treatment status while the early group does not, so that our exercise falls in the classic difference-in-difference framework (See Equation 6 and Panel D in Goodman-Bacon, 2021).

[9] In the psychophysics of perceptions it is commonly accepted that the perceived change of a stimulus depends on the pre-existing level. The Weber–Fechner laws seem to speak in favor of subjects reacting proportionally to changes in rates, rather than in levels, of contagion. There is evidence that such mechanism also applies to emotional perceptions of the spreading of the pandemic (Dyer & Kolic, 2020).

*Additional treatment: In-group vs. Out-group.* Social distancing does not necessarily affect subjects' attitude towards others in a unique direction, as the effect may depend on the closeness of the interpersonal relationships. For instance, it can strengthen ties within a community while triggering a hostile reaction towards people with which interpersonal relationships were already negligible (Bartoš et al., 2021). For this reason the design of the experiment also entails a between-subject manipulation in order to distinguish In-group vs. Out-group effects.

In the In-group condition of the trust game each participant in Tucson and New York City is told that (s)he is matched with an anonymous participant from the same city. In the Phoenix metropolitan area each participant is told that the In-group member comes from the same city/suburb (Phoenix, Mesa, Scottsdale). In the Out-group condition a participant from Arizona (New York City) knows that the opponent is from New York (Arizona).[10] Summarizing:

- **Trust In-group** (60 AZ, 60 NY): Two players from the same city/suburb are matched.
- **Trust Out-group** (60 AZ, 60 NY): One player from Arizona and one from New York City are matched.

As explained in the Introduction, the effect of social distancing on interpersonal trust is unclear a priori. On the one hand, it may trigger negative emotions that can erode trust. On the other hand, the introduction of a stay-at-home order may generate a feeling of common fate that may strengthen the ties within a community, thereby increasing interpersonal trust. Due to the common fate argument, a relatively more positive (and possibly positive in absolute terms) effect of social distancing should be expected towards In-group rather than towards Out-group members, who can also suffer hostile attitudes (Bartoš et al., 2021). As far as trustworthiness is concerned, we expect an opposite change because trustworthiness should positively correlate with the scarcity of trust.

In the solidarity game we match two players from the same city/suburb and one from the other state. The sample of participants is therefore split in two groups:

- **Solidarity NY-NY-AZ** (40 AZ, 80 NY)
- **Solidarity AZ-AZ-NY** (80 AZ, 40 NY).

Players NY-NY and AZ-AZ come from the same city (in case of Arizona either Tucson, or the same city/suburb within the Phoenix metropolitan area). Also in this case the effect of interest is identified comparing the change in the choices in Arizona as compared to New York City. The In-group vs. Out-group favoritism can be measured as the difference between the amount given to opponents that only differ for their origin. For instance, suppose that in Solidarity AZ-AZ-NY Player A and B are from Tucson and Player C from New York City. Player A has two anonymous opponents and knows that they only differ because Player B is also from Tucson, while Player C lives in a different state (New York). The difference in the amount passed to Player B and Player C can be used to proxy the In-group favoritism of Player A. The same argument applies to Player B. We expect to observe a positive effect of social distancing on solidarity in the In-group choice, because of the increased feeling of common fate with subjects that are geographically closer. In more detail, across waves we expect a larger increase of the difference in the amount passed in the AZ-AZ-NY condition than in the NY-NY-AZ condition. In the example above, Player C faces two identical (Out-group) opponents and therefore we do not expect any systematic difference in the amount passed to them. If social distancing triggers a hostile reaction towards Out-group members, we could observe players C in NY-NY-AZ to decrease the amount passed across waves (relatively to players C in AZ-AZ-NY) and therefore a negative DDR effect.

### 2.4. Procedures

Before the first wave 12 blocks of participants have been predefined, corresponding to 2 × 3 (In-group vs. Out-group in the trust game and Player A,B,C in the solidarity game) experimental conditions in each state. The pre-screened participants, once stratified according to their city/suburb of residence, have been randomly assigned to one of such blocks.

*Matching.* We adopt a Perfect Stranger matching protocol in order to avoid potential carryover effects across waves. Participants are explicitly told since the beginning that they are never matched with the same player across tasks and waves. Given that there is no feedback until all choices are made in both waves, the decisions at the individual level can reasonably be regarded as independent observations.[11]

*Payment protocol.* Payment is conditioned on the successful completion of both waves, and includes a fixed payment of \$1.5 for each wave plus the outcomes in the incentivized tasks as follows:

- The BRET is paid only once, randomly drawing the relevant wave.
- At the end of the whole study we randomly assign the role in the trust game and combine the choices of the respondents in both the trust and the solidarity game. In each wave we chose at random one of these two games and pay the corresponding outcome at the individual level.

The expected payoff in the BRET is \$2.5 under risk neutrality. In the trust game earnings range between 0 and \$4. The average payoff depends on the fraction of subjects passing the dollar as Player A. Evidence from previous experiments shows that about half of the players do so, implying an average expected payoff of \$1.25 per capita. In the solidarity game there are always two players out of three earning \$3, while the other gets zero. The average earning is therefore \$2 per capita by construction, regardless of the amount of money passed among subjects before the resolution of uncertainty. The expected payoff is therefore about \$5–\$5.50 in addition to the \$3 participation fee. Given an expected duration of the experiment of about 12 min per wave, the incentives are definitely salient. We administer the trust and the solidarity game in Wave 2 without reminding the subjects of their decisions in the first wave, because in this case the choice set is limited (binary in the trust game) and therefore there is less noise involved.

*Summary of the protocol*

**Wave 1:** March 31-April 1, 2020

1. Bomb Risk Elicitation Task;
2. Mini-trust game with role uncertainty and In-group vs. Out-group treatment;
3. Solidarity game with In-group vs. Out-group distinction;
4. Questions, also about interpersonal relationships (expected and retrospective).

**Wave 2:** April 15–16, 2020

[10] The choice of recruiting participants from states that differ along several dimensions has pros and cons. In principle, initial differences across groups do not invalidate the exercise, regardless of whether they occur directly on the level of social distance or in the measured behavioral traits. Nevertheless, one may argue that if groups are heterogeneous it is more likely that their reaction to the same treatment may also differ, in this case violating the common trend assumption. On the other hand, having some differences across groups renders the In-Group vs. Out-Group intervention more salient.

[11] The Perfect Stranger matching protocol is implemented without deception through a pre-defined rotation of players starting from an anonymous grid that relies upon the 12 blocks. Each participant has been randomly assigned (ex post) to the initial position in such a grid.

**Table 2**
Descriptive statistics at baseline by treatment condition.

| | (A) By state | | | (B) Trust game | | | (C) Solidarity game | | |
|---|---|---|---|---|---|---|---|---|---|
| | AZ | NY | P-val. | In-group | Out-group | P-val. | Player A, B | Player C | P-val. |
| Age | 35.65 | 34.20 | 0.68 | 34.53 | 35.15 | 0.50 | 34.31 | 35.67 | 0.32 |
| Male | 0.51 | 0.49 | 0.90 | 0.52 | 0.48 | 0.62 | 0.53 | 0.46 | 0.31 |
| Born in US | 0.91 | 0.86 | 0.34 | 0.90 | 0.87 | 0.45 | 0.88 | 0.88 | 1.00 |
| Fulltime work | 0.42 | 0.55 | 0.05 | 0.46 | 0.52 | 0.39 | 0.48 | 0.51 | 0.70 |
| Student | 0.25 | 0.33 | 0.13 | 0.33 | 0.25 | 0.17 | 0.32 | 0.24 | 0.21 |
| Observations | 119 | 147 | | 130 | 136 | | 161 | 105 | |

Mann Whitney (Fisher) tests on the equality of distribution (proportion) are performed on continuous (binary) variables.
The null hypothesis is that the means(proportions) are equal across groups.
Player A, B face both In-group and Out-group choice; Player C only Out-group choice.

1. Bomb Risk Elicitation Task;
2. Mini-trust game with role uncertainty and In-group vs. Out-group treatment;
3. Solidarity game with In-group vs. Out-group distinction;
4. Questions, also about interpersonal relationships (expected and retrospective) and about the impact of the pandemic on economic conditions.

### 2.5. Power analysis

The size of the sample ($N = 240$) is limited by the availability of subjects in Arizona. Nevertheless, the experiment is powered to detect medium size effects in our treatment variables.

As regards risk aversion, with 120 observations per group we reach a power of 0.8 in a two-tailed $t$-test under the following assumptions: ($i$) an average choice of 40.05 steps with a standard deviation of 13.98, consistently with Crosetto and Filippin (2016) ($ii$) a difference of 5.1 steps in the average choice of the treated group (Cohen's $d = 0.36$).

Using the Cochran–Mantel–Haenszel test (Cochran, 1954; Mantel & Haenszel, 1959), we find that the sample size (60 independent observations per group) grants a power of 0.8 also for a two-tailed test in the trust (trustworthiness) treatment. This power level is reached assuming that the fraction of subjects passing (returning) changes by 5% from a baseline of 0.5 and choosing an odds ratio of 3 that corresponds to a medium size effect (Ferguson, 2016).

Numbers in the solidarity game are more difficult to conjecture ex ante. However, the lower number of independent observations (40 per group) renders the experiment relatively less powered in the test of favoritism. The conventional level of power (0.8) is in fact reached only for a relatively large effect size (Cohen's $d = 0.63$).

## 3. Data and empirical strategy

A total of 266 subjects participated to Wave 1, 147 from New York City (55.3%) and 119 from Arizona (44.7%). Given the ex-ante stratification, the two groups are balanced in terms of gender and age. The two subsamples are also well-balanced in terms of origin, with the large majority of the participants born in the US (see Table 2, Panel A). Nearly half of the sample works fulltime, in this case with a significantly higher fraction in NY (55% vs. 42%).[12] Approximately one fourth of the sample is composed of students. The sample is also well-balanced in terms of assignment to the treatment within each of the two games (trust game, Panel B, and solidarity game, Panel C).

213 subjects participated also to Wave 2, but 8 did not complete the survey. Therefore, we have a balanced panel composed of 205 subjects (116 from New York City and 89 from Arizona). Despite our attempt to minimize attrition conditioning the payment to the completion of the whole study, as well as sending reminders, 61 subjects dropped out from the experiment. This fraction does not significantly differ in the two states (P-val. = 0.427). Descriptive statistics of the panel sample and the attritors (as measured in Wave 1) show that selection was not based on observables (see Table C.8 in Appendix). Nevertheless, since we are interested in the change of behavior, the analysis is conducted using the panel sample only.

The identification strategy adopted in this study exploits the different timing in the implementation of the stay-at-home order in the two states as an exogenous manipulation of social distancing. The DDR method compares the changes in outcome variables over time between subjects in Arizona (the group that switched from being "Untreated" to "Treated") and subjects in New York (the always "Treated" group). The trend in the outcome variables in Arizona is compared with that in New York. A statistically significant difference in the trends constitutes evidence that social distancing as proxied by the stay-at-home policy plays a role. The estimated equation is therefore:

$$Y_{i,t} = \beta_0 + \beta_1 Wave2 + \beta_2 Arizona + \beta_3 Wave2 * Arizona + \sum \gamma X_{i,1} + \epsilon_{i,t} \quad (1)$$

where $Y_i$ is the behavioral trait under investigation for individual $i$ at time $t \in \{1,2\}$, $Wave2$ is a time dummy equal to 1 for observations collected in the second wave ($t = 2$), and $Arizona$ is the indicator equal to 1 for observations in Arizona (zero otherwise). The interaction $Wave2 * Arizona$ captures the switching treatment (from the "Untreated" to the "Treated" condition in Arizona), so that $\beta_3$ represents our effect of interest (DDR). $X_{i,1}$ is a vector of individual characteristics measured at baseline (i.e. in Wave 1), and $\epsilon_{i,t}$ is the error term.

## 4. Results

*Risk aversion.* The number of steps chosen in the BRET, which measure the degree of risk tolerance, appears very consistent across waves at the individual level ($\rho = 0.6$, P-val $< 0.001$). However, the results do not support the assumption that the pandemic, representing a background risk, decreases the risk tolerance of the respondents. Choices appear little changed on average both in Arizona (40.6 in Wave 2 as compared to 40.8 in Wave 1) and in New York City (38.8 and 38.9, respectively). Not surprisingly, Table 3 shows that the DDR coefficient is fairly close to zero. This result holds with the inclusion of controls (Column 2) and does not depend on whether we consider the number of steps chosen (continuous variable 1–100) or a binary indicator equal to one when the number of steps is higher than the median value (Columns 3 and 4).

Reminding participants of their choice in Wave 1 increases the correlation between responses by reducing measurement error. However, such reminders may also activate consistency motives, leading participants to align more closely with their earlier answers and potentially obscuring genuine changes in their preferences. Although there is no evidence of an aggregate effect, risk aversion may have changed at the individual level and therefore we include it as a control in the regressions for trust, trustworthiness, and solidarity. Also in this case, however, we do not find evidence of any significant correlation, as reported below.

[12] We include this variable as a control in our empirical analysis, without detecting any significant effect on the treatment.

**Table 3**
DDR regression for risk tolerance.

| | (1) Bomb steps | (2) Bomb steps | (3) Steps > median | (4) Steps > median |
|---|---|---|---|---|
| Arizona | 1.903 | 3.096 | 0.051 | 0.070 |
| | (2.916) | (2.911) | (0.071) | (0.072) |
| Wave2 | −0.190 | −0.191 | 0.052 | 0.052 |
| | (1.490) | (1.515) | (0.044) | (0.045) |
| Arizona*Wave2 (DDR) | −0.058 | −0.059 | −0.007 | −0.007 |
| | (2.529) | (2.575) | (0.065) | (0.066) |
| Controls | No | Yes | No | Yes |
| Constant | 38.940*** | 45.391*** | 0.466*** | 0.530*** |
| | (1.718) | (5.841) | (0.047) | (0.157) |
| Observations | 410 | 406 | 410 | 406 |

Note: Linear probability model, fixed effects. Dep. var.: Number of steps chosen (1 and 2); Dummy = 1 (3 and 4).
Standard errors are clustered at the individual level and reported in parentheses.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
Controls: Age, gender, nationality, native English speaker, employment and student status.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

**Table 4**
DDR regression for trust: In-group vs. Out-group.

| | In-group | | Out-group | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Arizona | 0.152 | 0.146 | 0.151 | 0.172* |
| | (0.093) | (0.100) | (0.098) | (0.104) |
| Wave2 | −0.111 | −0.112 | −0.000 | −0.001 |
| | (0.068) | (0.070) | (0.057) | (0.059) |
| Arizona*Wave2 (DDR) | 0.154* | 0.156* | −0.119 | −0.117 |
| | (0.081) | (0.083) | (0.083) | (0.085) |
| Controls | No | Yes | No | Yes |
| Constant | 0.593*** | 0.591*** | 0.516*** | 0.645*** |
| | (0.068) | (0.230) | (0.064) | (0.235) |
| Observations | 202 | 200 | 208 | 206 |

Note: Linear probability model, fixed effects. Dep. var.: Passing the money as Player A.
Standard errors clustered at individual level are reported in parentheses.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
Controls: Age, gender, nationality, mother tongue, employment and student status, risk tolerance.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

**Table 5**
DDR regression for trustworthiness In-group vs. Out-group.

| | In-group | | Out-group | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Arizona | 0.223** | 0.212** | 0.103 | 0.132 |
| | (0.095) | (0.103) | (0.099) | (0.101) |
| Wave2 | −0.000 | 0.004 | −0.065 | −0.072 |
| | (0.059) | (0.062) | (0.073) | (0.076) |
| Arizona*Wave2 (DDR) | −0.021 | −0.032 | 0.065 | 0.082 |
| | (0.098) | (0.103) | (0.105) | (0.108) |
| Controls | No | Yes | No | Yes |
| Constant | 0.500*** | 0.688*** | 0.516*** | 0.979*** |
| | (0.069) | (0.241) | (0.064) | (0.196) |
| Observations | 202 | 200 | 208 | 206 |

Note: Linear probability model, fixed effects. Dep. var.: Returning money as Player B.
Standard errors clustered at individual level are reported in parentheses.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
Controls: Age, gender, nationality, mother tongue, employment and student status, risk tolerance.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

*Trust and trustworthiness.* Subjects display a remarkable degree of trust, particularly high in Arizona where 69% of Players A pass the money to Player B. In New York City the fraction is 53%. Similar numbers are observed also in the decision of Player B to return some money, which proxies trustworthiness (67% in Arizona, 49% in New York City). The analysis of the effect of social distancing is qualitatively aligned with our conjectures, but results are generally weak and approach standard significance levels only for trust.

Table 4 shows positive DDR coefficients in the In-group treatment, significant at 90% confidence level. As a consequence of social distancing, players display an increased level of trust when interacting with opponents living in the same city/suburb. The opposite sign appears in the Out-group condition, but in this case the decrease of trust is not statistically significant.

As far as trustworthiness is concerned, our conjecture is not supported by the data (Table 5). In line with Kovács et al. (2010), we observe a positive relationship between one's own trust and trustworthiness ($\rho = 0.5147$; P-val. $< 0.001$). However, social distancing does not seem to play any role in shaping trustworthiness. The DDR coefficients are small and clearly not statistically significant across the board.

*Solidarity.* In the solidarity game subjects pass on average 81 cents to the loser (86 in Arizona, 77 in New York City), which corresponds to 27% of their endowment. There is evidence of In-group favoritism. Player A and B pass to the In-group opponent a significantly higher amount (Wilcoxon signed-rank test, P-val. $< 0.001$), although the magnitude of the difference is not large on average (7.5 cents). As expected, the amount passed by Player C to the two Out-group opponents is almost always the same. In the about 10% of the observations in which this is not the case we use the average amount in the regressions below.

Our conjecture on the effect of social distancing on solidarity is not supported by the data (Table 6). The DDR coefficients are fairly close to zero across the board. There is no evidence of an increase of In-group favoritism (Column 1 and 2), as the coefficients are even negative though not statistically significant. A similar argument applies to solidarity towards Out-group members (Column 3 and 4). The stay-at-home order does not seem to play any role in shaping solidarity regardless of opponents living in the same city/suburb or in a different state.

*Gender differences.* The existing literature on gender differences in the behavioral traits under investigation shows that men and women sometimes differ. For instance, a meta-analysis shows that men trust more than women, while no gender difference appears in trustworthiness (Van Den Akker et al., 2020). Women show more solidarity than men (Ockenfels & Weimann, 1999; Selten & Ockenfels, 1998). For this reason, in the pre-registration of this study we planned to analyze the results along a gender dimension. Although our sample measured at baseline does not replicate these results, differences can emerge in principle due to a gender specific reaction to the treatment. However, the analysis shows that this is not the case. In a specification including a complete set of interactions, the DDR coefficients of interest do not display any significant gender difference.

**Table 6**
DDR regression for solidarity.

| | Favoritism (Player A, B) | | Out-group (Player C) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Arizona | 0.060 | 0.085 | 0.099 | 0.153 |
| | (0.087) | (0.103) | (0.115) | (0.109) |
| Wave2 | 0.044 | 0.050 | 0.073 | 0.088 |
| | (0.071) | (0.072) | (0.088) | (0.089) |
| Arizona*Wave2 (DDR) | −0.017 | −0.023 | −0.043 | −0.060 |
| | (0.112) | (0.113) | (0.121) | (0.128) |
| Controls | No | Yes | No | Yes |
| Constant | 0.029 | 0.027 | 0.651*** | 0.872** |
| | (0.042) | (0.162) | (0.073) | (0.283) |
| Observations | 248 | 246 | 162 | 160 |

Note: Linear probability model, fixed effects. Dependent variables:
Favoritism = Difference in the amount passed to In- vs. Out-group;
Out-group = Average amount passed to the 2 Out-group opponents.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
Standard errors clustered at individual level are reported in parentheses.
Controls: Age, gender, nationality, mother tongue, employment and student status, risk tolerance.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

### 4.1. Compliance to the treatment

The results presented in the previous section can be criticized on the ground that social distancing may react imperfectly to the stay-at-home order. Accordingly, the DDR coefficients presented in the previous section should be regarded as the intention-to-treat effects and possibly represent a lower bound of the true effects.

As a robustness check we replicate our empirical exercise distinguishing the participants according to their compliance to the treatment. More precisely, we define as "Compliant" an individual that in Wave 2 reports to have interacted (in the previous week) with a strictly lower number of people than in Wave 1. In the regressions we then include a complete set of interactions, in which the DDR coefficient applies to all the treated participants (compliant or not) and the triple interaction (DDR*Compliant) captures the additional effect of being compliant as compared to those participants who are not. Hence, the sum of the triple interaction and of the classic DDR coefficient measures the total impact of being compliant to the stay-at-home order in Arizona.

Table 7 (Column 1 and 2) reports the results for trust, providing additional evidence that social distancing has a positive effect in the In-group treatment. The coefficient of the triple interaction is positive and statistically significant indicating that trust increases significantly more for the participants who are compliant to the treatment. The DDR coefficient shows instead that trust does not increase significantly for the treated respondents who are not compliant. Furthermore, the total effect of the treatment for the compliant participants (DDR*Compliant + DDR) is larger and significant at a higher level of confidence than what observed in Table 4. The results of Table 7 confirm that the positive effect of social distancing on trust within a community is genuine. The null result for trust in the Out-group condition is also confirmed when taking into account the compliance to the treatment (Table 7, Column 3 and 4).

The imperfect compliance opens the possibility to observe self-selection into the treatment and therefore also reverse causality between social distancing and trust. If we consider trust as a component of social capital (Glaeser et al., 1999), it can be the case that the higher social capital the more likely to be compliant to the stay-at-home order. Furthermore, people with higher social capital may reduce social interactions even independently from the treatment (Campos-Mercade et al., 2021; Durante et al., 2021; Fang et al., 2022). While certainly true in principle, this mechanism does not apply to our results for two reasons. First, our analysis focuses on the *change* of trust. Second, in our data this channel does not appear to work in levels either. Those who pass as Player A in Wave 1 are not more likely to reduce social interactions than those who keep the dollar (Fisher exact test, P-val = 0.672).

**Table 7**
DDR regression for trust controlling for compliance.

| | In-group | | Out-group | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Arizona | 0.306*** | 0.296** | 0.183 | 0.206 |
| | (0.115) | (0.120) | (0.152) | (0.157) |
| Wave2 | −0.111 | −0.112 | 0.000 | −0.001 |
| | (0.069) | (0.070) | (0.057) | (0.059) |
| Arizona*Wave2 (DDR) | 0.073 | 0.075 | −0.133 | −0.128 |
| | (0.079) | (0.080) | (0.106) | (0.107) |
| Compliant | 0.129 | 0.118 | 0.065 | 0.033 |
| | (0.115) | (0.124) | (0.116) | (0.122) |
| Compliant*Arizona | −0.356** | −0.365* | −0.065 | −0.061 |
| | (0.174) | (0.188) | (0.193) | (0.204) |
| DDR*Compliant | 0.181** | 0.189** | 0.022 | 0.017 |
| | (0.087) | (0.091) | (0.121) | (0.123) |
| Controls | No | Yes | No | Yes |
| Constant | 0.540*** | 0.524*** | 0.484*** | 0.627*** |
| | (0.090) | (0.243) | (0.087) | (0.246) |
| DDR + DDR*Compliant | 0.254** | 0.264** | −0.111 | −0.111 |
| | (0.104) | (0.109) | (0.100) | (0.102) |
| N. Obs. | 202 | 200 | 208 | 206 |

Note: Linear probability model, fixed effects. Dep. var.: Passing the money as Player A.
Standard errors clustered at individual level are reported in parentheses.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
DDR*Compliant: Interaction between DDR and Compliance to the treatment.
Controls: Age, gender, nationality, mother tongue, employment and student status, risk tolerance.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

In principle, the imperfect compliance to the treatment may also have blurred significant effects on the other behavioral traits. Repeating the same exercise seen in Table 7 for risk aversion, trustworthiness, and solidarity shows that this is not the case. Compliant subjects are never characterized by a significantly different effect, and the total effect of the treatment remains fairly close to zero in all the circumstances (see Tables C.9–C.11 in Appendix).

## 5. Conclusion

Previous literature provides mixed evidence about the effect of social distancing on individual preferences. Contributions usually rely upon repeated cross sections, however, in which individual heterogeneity and unobservable characteristics play a critical role. This is particularly true in a turbulent period like that of the COVID-19 outbreak.

This paper investigates experimentally whether trust, trustworthiness, and solidarity react to social distancing. We improve upon the existing literature using a panel structure and exploiting an exogenous manipulation of social distancing constituted by the different timing of introduction of stay-at-home orders.

The treatment group is composed by respondents in Phoenix and Tucson, Arizona, where the stay-at-home order took effect on the day of the first wave. Respondents in New York City, where a stay-at-home policy had been already in place for about two weeks, represent the control group. The same individuals were re-tested two weeks later, when both groups were treated. The presence of an always treated comparison group configures a Difference-in-Differences in Reverse, equivalent to a classic Difference-in-Differences. Our design also entails an In-group vs. Out-group (between subject) manipulation meant to capture different effects of social distancing inside the same city/suburb and outside such a community.

We observe that social distancing has a positive and marginally significant In-group effect on trust, while in the Out-group condition trust decreases but the change does not reach traditional significance levels. We then consider that social distancing may react imperfectly to the stay-at-home order. Toward this goal we build a proxy for the compliance to the treatment based on the reduction of social contacts between the two waves. A replication of the analysis including a complete set of interactions with the proxy for compliance improves the strength of the result that reaches traditional significance levels. In more detail, trust increases significantly more for those who are compliant to the treatment, while the increase is not significant for those who are not compliant. This robustness check supports the goodness of the result against the possibility of a false positive.

In contrast, we find no evidence that social distancing affects the other behavioral traits. In the Outgroup condition (for both trust and trustworthiness) the effect may be smaller than what the experiment is powered to detect. In all other cases the estimated coefficients are instead so small to suggest that the null results are genuine rather than false negatives. In principle, we cannot exclude that these null results stem from methodological limitations of the incentivized tasks used, which may fail to extend to effects outside the domain of monetary incentives. However, the significant effect on trust seems to speak against this possibility. The different significance of the results between trust and trustworthiness may be ascribed to the fact that trust depends on beliefs about others more than trustworthiness. Moreover, we show that the null effect of social distancing in this cases is robust to restricting the analysis to the respondents compliant to the treatment.

Overall, our results display that the pandemic had a limited effect on preferences that are relevant in interpersonal relationships. Nevertheless, the positive effect on trust in the In-group condition in Arizona is in our opinion interesting, because it is driven by an increased distancing under common fate, rather than by proximity with the counterparts as usually found. Unfortunately, our experiment is not equipped to measure whether this increase of trust within a community represents a transitory effect of the COVID-19 outbreak or has instead long-run consequences.

## CRediT authorship contribution statement

**Antonio Filippin:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Investigation, Formal analysis, Conceptualization. **Noemi Pace:** Writing – original draft, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

## Appendix A. Instructions

[*Wave 1:*] Welcome to our study! The study is composed of two parts, and this is the first. The second will be conducted on April 15.

[*Wave 2:*] Welcome back! This is the second part of our study that started two weeks ago.

Please read the instructions carefully, stay focused and complete the experiment without distractions. Your understanding will contribute to your final earnings, and will determine the robustness and credibility of our study that is conducted for academic research.

This part of the study will be available online only for 36 h and will take about 12 min to complete. **The payment is conditional on the completion of both parts and it will made by April 21.** You will be paid a **flat amount of $3 ($1.5 each) plus an additional bonus payment. The bonus payment will be about $5.5 on average (min 0, max $18)** and will depend on your choices, on the choices of other participants, and on random events in both parts. This part of the study is composed of three sections of similar length and a few final questions. The outcome of Section 1 will enter your final bonus, together with one between Section 2 and Section 3 (randomly chosen at the end of the study).

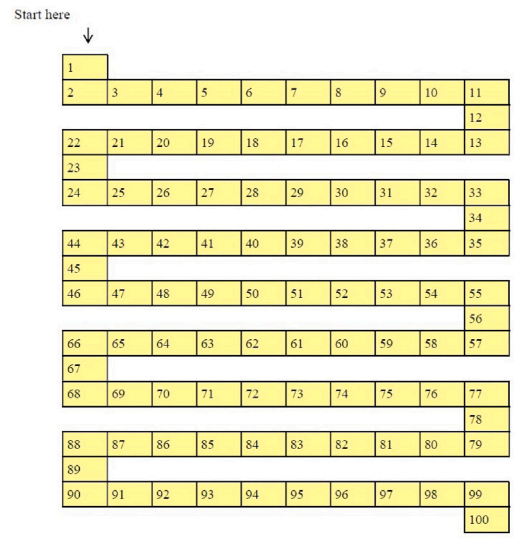


**Fig. A.3.** Bomb risk elicitation task.

We greatly appreciate your participation.

Antonio Filippin (University of Milan)

Noemi Pace (University of Teramo)

For inquiries write to antonio.filippin@unimi.it or npace@unite.it mentioning Prolific in the subject of the email.

**SECTION 1**

Imagine that you are in a minefield, and on the winding road that you see below exactly 1 bomb is hidden behind one of the 100 numbers(see Fig. A.3).

You may gain 10 cents for every step you take. You have to start at step 1, then 2, 3 and so on and you continue until you reach the step where you want to stop. You do not know the bomb's location. You only know that it is equally likely to be behind any of the hundred steps on the winding road. At the end of the whole study (i.e. after the second part) the location of the bomb will be determined randomly. You may write us to get the list of numbers drawn for every participant, to check the fairness of the procedure.

If the number where the bomb is located is higher than the number of steps you have taken it means that you have not stepped on the bomb. In this case we will add to your final bonus 10 cents for each of the steps you have taken. If the number where the bomb is located is lower than or equal to the number of steps you have chosen, then you have stepped on the bomb and you will earn nothing. Summarizing, the more steps you take, the higher your potential earning but also the risk that you step on the bomb and get zero.

[*PART INCLUDED ONLY IN WAVE 2*]

In the first part of this study you chose a number of steps equal to X. Now you have the opportunity to revise that choice. You can confirm the choice made two weeks ago by choosing the same number, or choose any other number as you prefer. Your bonus will include the outcome based only on the revised choice that you make today.

How many steps do you want to take?_______

**SECTION 2** *[Framing for In-group condition]*

In this section two participants interact: Player A and Player B. The Figure on your screen summarizes all the possible choices that players can make, and that are explained below(see Fig. A.4).

Player A is endowed with $1 and must decide between Option 1 and Option 2.

- Option1: Player A keeps the dollar and nothing is passed to Player B. In this case Player B does not make any decision, the game ends and earnings are $1 for Player A and 0 for Player B, respectively.

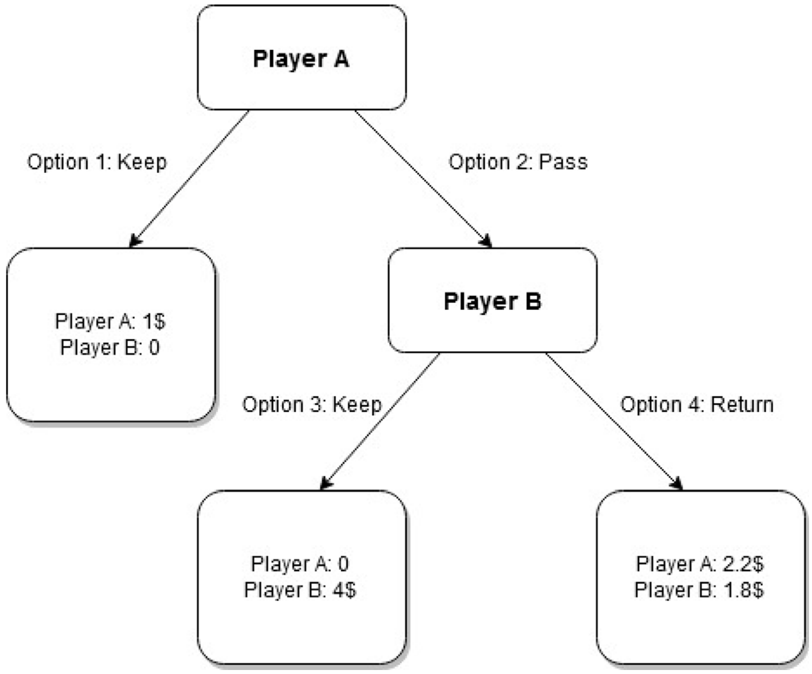


**Fig. A.4.** Mini trust game.

- Option 2: Player A passes the dollar to Player B. The dollar is multiplied by 4, thus Player B receives $4.

In this case the earnings of the game are decided by Player B, who must choose between Option 3 and Option 4.

- Option 3: Player B keeps the $4 and nothing is passed back to Player A. Earnings in this case are 0 for Player A and $4 for Player B, respectively.
- Option 4: Player B keeps the $1.8 and passes back $2.2 to Player A. Earnings in this case are $2.2 for Player A and $1.8 for Player B, respectively.

N.B. Pairs will be randomly formed and roles (Player A vs. Player B) will be randomly assigned only at the end of the second part of the study. In this game you will be matched with another anonymous participant from your city. Thus, you do not know your role now. For this reason, you are asked to make a decision both as Player A and as Player B. At the end of the study pairs will be formed and roles assigned. At that point earnings will be automatically computed combining the decision taken by each participant in the role that has been assigned. This amount will enter your final bonus in case Section 2 is drawn as the relevant section for payment (remember that only one between Section 2 and Section 3 will be randomly chosen to count towards your bonus).

Suppose you are Player A, what is your decision?

☐ I keep $1
☐ I pass $1 - which becomes $4 - to Player B

Suppose you are Player B, what is your decision?

☐ I keep the $4
☐ I pass $2.2 to Player A and I keep $1.8

**SECTION 3** *[Framing for Player A in New York City]*

In this section three participants interact (Player A, Player B, and Player C). They are assigned different numbers (see Fig. A.5):

At the end of the second part of the study we will draw one number rolling a dice. Each player who has the number drawn gets $3. As you can see, for every number that can be drawn 2 players win and one loses. You are Player A and you will be matched with two other anonymous participants (different from the player in the previous section). You only know that Player B lives in your own city, while Player C in Arizona. Your task is to decide how much (from 0 to $3 in 50 cent steps) you want to give to the loser. You can give a different amount in case the loser is Player B or C. The other two participants will make a similar decision. Therefore, if your numbers contain the number drawn you will get $3 minus the amount that you decide to transfer to the player that will actually lose. If your numbers do not contain the number drawn you are the loser and will receive the sum of the amounts that the two winners (Player B and C) decided to transfer to you (Player A). This amount will enter your final bonus in case Section 3 is drawn as the relevant section for payment (remember that only one between Section 2 and Section 3 will be randomly chosen to count towards your bonus).

Suppose Player B (from your city) is the loser. Amount given:

☐ $0
☐ $0.5
☐ $1
☐ $1.5
☐ $2
☐ $2.5
☐ $3

Suppose Player C (from Arizona) is the loser. Amount given:

☐ $0
☐ $0.5
☐ $1
☐ $1.5
☐ $2
☐ $2.5
☐ $3

**SHORT SURVEY**

We conclude the experiment with a short survey.

How many people did you meet outside last week as compared to a usual week?

| Player | | Number Assigned | | | | | |
|---|---|---|---|---|---|---|---|
| Player A | You | 1 | 2 | 3 | 4 | - | - |
| Player B | Player from your city | 1 | 2 | - | - | 5 | 6 |
| Player C | Player from Arizona | - | - | 3 | 4 | 5 | 6 |

**Fig. A.5.** Solidarity game.

- ☐ About the same
- ☐ Fewer, but still many
- ☐ Roughly half
- ☐ Just a few
- ☐ Nobody

How many people do you expect to meet outside in the next two weeks as compared to a usual week?

- ☐ About the same
- ☐ Fewer, but still many
- ☐ Roughly half
- ☐ Just a few
- ☐ Nobody

[*PART INCLUDED ONLY IN WAVE 2*]

Suppose that in normal times you meet outside 100 people a day on average (colleagues, friends, relatives, etc.):

How many people did you meet in a normal day at the end of March? _______

How many people are you meeting in a day now? _______

When do you think the stay-at-home order will be lifted where you live?

- ☐ By the end of April
- ☐ By mid May
- ☐ By the end of May
- ☐ By mid June
- ☐ Not before the end of June

Have you experienced any change of income in the last two weeks?

- ☐ Yes, a positive change
- ☐ No
- ☐ Yes, a negative change

Do you think you will experience income changes in the next two weeks as compared to last week?

- ☐ Yes, a positive change
- ☐ No
- ☐ Yes, a negative change

Thank you very much for your participation!

## Appendix B. Difference-in-difference in reverse

Our identification strategy entails a Difference-in-differences in reverse (DDR) approach that exploits the different timing in the implementation of the stay-at-home order in the two states as an exogenous manipulation of social distancing. The key difference between a standard DD and the DDR stands on the control group, which is never treated in DD and instead always treated in the DDR.

Let us consider formally the simplest possible case – a 2 × 2 DD strategy – with two periods $t \in \{0;1\}$, a treatment and a control group $G_i \in \{T;C\}$, and a treatment $D_{i,t} \in \{0;1\}$ depending on the introduction of the stay-at-home order. The difference between DD and DDR lies on the treatment status of the control group: the former reads $D_{i,t} = 0 \; \forall i;t \mid G_i = C$ – in words, the control group never receives the treatment – while in the latter case (the one we implement) $D_{i,t} = 1 \; \forall i;t \mid G_i = C$, i.e. an always treated control group. To complete the notation, we label the outcome of interest, e.g. trust, with the treatment status, the group, and the time (i.e., $Y^D_{t,G}$).

The DD compares the differences between treatment and control group at $t = 1$ with the same difference at $t = 0$:

$$DD = \underbrace{[E(Y^1_{1,T}) - E(Y^0_{1,C})]}_{\text{Post}} - \underbrace{[E(Y^0_{0,T}) - E(Y^0_{0,C})]}_{\text{Pre}} \tag{B.1}$$

$$= \underbrace{[E(Y^1_{1,T}) - E(Y^0_{0,T})]}_{\text{T group}} - \underbrace{[E(Y^0_{1,C}) - E(Y^0_{0,C})]}_{\text{C group}} \tag{B.2}$$

To identify the effect of the treatment the DD requires only the parallel trend assumption. Without the treatment the outcome variable should change by the same amount in the two experimental groups:

$$ID_{DD} \Rightarrow \underbrace{[E(Y^0_{1,T}) - E(Y^0_{0,T})]}_{\text{Conterfactual T group}} = \underbrace{[E(Y^0_{1,C}) - E(Y^0_{0,C})]}_{\text{C group}}. \tag{B.3}$$

Substituting the Counterfactual $T$ Group in Eq. (B.2), the DD yields an estimate of the treatment effect $E(Y^1_{1,T}) - E(Y^0_{1,T})$ based on the (unobservable) outcome $Y^0_{1,T}$.

In the DDR case, the definition in Eqs. (B.1) and (B.2) differs with respect to the treatment status of the control group, which is always 1. Consequently, the counterfactual identification in the DDR case depends on $Y^1_{0,T}$, i.e. the (unobservable) outcome in the treatment group at $t = 0$ present the treatment:

$$ID_{DDR} \Rightarrow \underbrace{[E(Y^1_{1,T}) - E(Y^1_{0,T})]}_{\text{Conterfactual T group}} = \underbrace{[E(Y^1_{1,C}) - E(Y^1_{0,C})]}_{\text{C group}} \tag{B.4}$$

In turn, the DDR provides an estimate of $E(Y^1_{0,T}) - E(Y^0_{0,T})$.

Absent any issue concerning time varying treatment effects, DD and DDR yield the same measure under a mild condition, namely that time does not affect treated and untreated units in a different way. The only formal difference is that the DDR estimate identifies the treatment effect on the treated at $t = 0$ instead of $t = 1$.

**Table C.8**
Descriptive statistics: panel sample vs. attritors.

| | Attritors | Panel sample | P-val. |
|---|---|---|---|
| Age | 34.07 | 35.08 | 0.44 |
| Male | 0.42 | 0.52 | 0.43 |
| Born in US | 0.90 | 0.88 | 0.82 |
| Fulltime work | 0.43 | 0.51 | 0.29 |
| Student | 0.30 | 0.28 | 0.87 |
| Steps in bomb | 40.07 | 39.77 | 0.66 |
| Trust | 0.57 | 0.62 | 0.55 |
| Trustworthiness | 0.66 | 0.58 | 0.30 |
| Solidarity | 1.60 | 1.66 | 0.39 |
| N. Obs | 61 | 205 | |

Mann Whitney (Fisher) tests on the equality of distribution (proportion) on continuous (binary) variables.
The null hypothesis is that the means(proportions) are equal across groups.

## Appendix C. Additional analysis

See Tables C.8–C.11.

**Table C.9**
DDR regression for risk tolerance controlling for compliance.

| | (1) Bomb steps | (2) Bomb steps | (3) Steps > median | (4) Steps > median |
|---|---|---|---|---|
| Arizona | 0.434 | 1.448 | 0.006 | 0.027 |
| | (4.139) | (4.032) | (0.099) | (0.098) |
| Wave2 | −0.190 | −0.191 | 0.052 | 0.052 |
| | (1.496) | (1.521) | (0.044) | (0.045) |
| Arizona*Wave2 (DDR) | 1.677 | 1.679 | 0.021 | 0.021 |
| | (3.069) | (3.099) | (0.085) | (0.086) |
| Compliant | −1.851 | −1.186 | −0.036 | −0.029 |
| | (3.324) | (3.357) | (0.083) | (0.083) |
| Compliant*Arizona | 3.006 | 3.300 | 0.090 | 0.086 |
| | (5.794) | (5.797) | (0.135) | (0.137) |
| DDR*Compliant | −3.217 | −3.254 | −0.052 | −0.052 |
| | (4.037) | (4.117) | (0.096) | (0.098) |
| Controls | No | Yes | No | Yes |
| Constant | 39.785*** | 45.936*** | 0.482*** | 0.547*** |
| | (2.208) | (5.980) | (0.060) | (0.161) |
| DDR + DDR*Compliant | −1.539 | −1.575 | −0.031 | −0.031 |
| | (3.370) | (3.460) | (0.077) | (0.079) |
| N. Obs | 410 | 406 | 410 | 406 |

Note: Linear probability model, fixed effects. Dep. var.: Number of steps chosen (1 and 2); Dummy = 1 (3 and 4).
Standard errors clustered at individual level are reported in parentheses.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
DDR*Compliant: Interaction between DDR and Compliance to the treatment.
Standard errors are clustered at the individual level and reported in parentheses.
Controls: Age, gender, nationality, native English speaker, employment and student status.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

**Table C.10**
DDR regression for trustworthiness controlling for compliance.

| | In-group | | Out-group | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Arizona | 0.246* | 0.265** | 0.068 | 0.079 |
| | (0.127) | (0.130) | (0.155) | (0.156) |
| Wave2 | 0.000 | 0.004 | −0.065 | −0.072 |
| | (0.060) | (0.062) | (0.073) | (0.077) |
| Arizona*Wave2 (DDR) | −0.038 | −0.048 | 0.065 | 0.106 |
| | (0.120) | (0.124) | (0.154) | (0.161) |
| Compliant | 0.038 | 0.019 | −0.032 | −0.055 |
| | (0.124) | (0.125) | (0.106) | (0.105) |
| Compliant*Arizona | −0.055 | −0.133 | 0.062 | 0.097 |
| | (0.183) | (0.193) | (0.192) | (0.193) |
| DDR*Compliant | 0.038 | 0.036 | 0.000 | −0.037 |
| | (0.158) | (0.166) | (0.164) | (0.170) |
| Controls | No | Yes | No | Yes |
| Constant | 0.484*** | 0.667** | 0.532*** | 1.007*** |
| | (0.091) | (0.252) | (0.085) | (0.206) |
| DDR + DDR*Compliant | −0.000 | −0.011 | 0.065 | 0.068 |
| | (0.133) | (0.143) | (0.118) | (0.120) |
| N. Obs | 202 | 200 | 208 | 206 |

Note: Linear probability model, fixed effects. Dep. var.: Returning money as Player B.
Standard errors clustered at individual level are reported in parentheses.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
DDR*Compliant: Interaction between DDR and Compliance to the treatment.
Controls: Age, gender, nationality, mother tongue, employment and student status, risk tolerance.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

**Table C.11**
DDR regression for solidarity controlling for compliance.

| | Favoritism (Player A, B) | | Out-group (Player C) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Arizona | 0.114 | 0.124 | 0.375** | 0.399** |
| | (0.151) | (0.159) | (0.185) | (0.174) |
| Wave2 | 0.044 | 0.050 | 0.073 | 0.090 |
| | (0.071) | (0.072) | (0.088) | (0.090) |
| Arizona*Wave2 (DDR) | −0.008 | −0.020 | −0.208 | −0.220 |
| | (0.174) | (0.173) | (0.195) | (0.212) |
| Compliant | 0.042 | 0.039 | 0.256* | 0.252* |
| | (0.091) | (0.090) | (0.136) | (0.139) |
| Compliant*Arizona | −0.114 | −0.088 | −0.509** | −0.476** |
| | (0.179) | (0.168) | (0.232) | (0.218) |
| DDR*Compliant | −0.018 | −0.005 | 0.272 | 0.262 |
| | (0.173) | (0.175) | (0.187) | (0.210) |
| Controls | No | Yes | No | Yes |
| Constant | 0.011 | −0.002* | 0.529*** | 0.841** |
| | (0.050) | (0.160) | (0.091) | (0.281) |
| DDR + DDR*Compliant | −0.026 | −0.025 | 0.064 | 0.042 |
| | (0.100) | (0.104) | (0.113) | (0.122) |
| N. Obs | 248 | 246 | 162 | 160 |

Note: Linear probability model, fixed effects. Dependent variables:
Favoritism = Difference in the amount passed to In- vs. Out-group*;
Out-group = Average amount passed to the 2 Out-group opponents.
DDR: Interaction between treated group (Arizona) and treatment period (Wave2).
DDR*Compliant: Interaction between DDR and Compliance to the treatment.
Standard errors clustered at individual level are reported in parentheses.
Controls: Age, gender, nationality, mother tongue, employment and student status, risk tolerance.
* P-val. < 0.1, ** P-val. < 0.05, *** P-val. < 0.01.

## Data availability

Data will be made available on request.

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