

Replication of Jachimowicz et al. 2017

“Community trust reduces myopic decisions of low income individuals”

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<http://www.pnas.org/content/114/21/5401>

The original paper includes several studies but only one on MTurk that fits our criteria. In this between-subject experiment, participants are randomized to one of four conditions in a 2x2 design (low/high felt-income x low/high community trust) and are then asked to make decisions related to temporal discounting. We focus on the comparison of myopic decisions between the low felt-income, low-community trust and the low-felt income, high community trust conditions. Low-felt income is induced by asking participants to imagine situations of severe financial implications, while low and high trust are induced by prompting participants to list 2 (low trust) or 10 (high trust) examples of experiences of justified community trust. Participants make repeated choices between a smaller payoff closer in time and a larger payoff farther in the future (e.g., €10 today vs. €50 in a month), measuring myopia. Participants in the low felt-income, low levels of community trust condition are more myopic than participants in the low felt-income, high levels of community trust condition.

Hypothesis to replicate and bet on: Low felt-income individuals with low levels of community trust are more myopic than low felt-income individuals with high levels of community trust. The authors test the above hypothesis using a two-sided independent samples *t*-test ($t(60) = 2.79, p = 0.007$; since the article did not include test-statistics, the results were re-estimated based on the original data and analysis scripts provided by the authors. Note that the re-estimated *p*-value deviates from the *p*-value mentioned in the article ($p = 0.04$)).

Criteria for replication: The criteria for replication are an effect in the same direction as the original study and a *p*-value < 0.05 in a two-sided independent samples *t*-test.

Power analysis: The original study had 62 participants for the two conditions. The standardized effect size (Cohen’s *d*) was $d = 0.708$. To have 90% power to detect 67% of the original effect size, a sample size of $n = 189$ is required. The two high-income conditions from the original study will be added for manipulation check purposes. Therefore, the overall sample size is doubled, resulting in $n = 378$.

Sample: The original paper mentions no restrictions on who could participate. Following advice by the original authors, we will exclude participants who provide nonsensical responses to the writing prompts (as indicated by two independent raters). Specifically, two raters will read all open responses to verify that participants provided adequate answers. If both coders agree that a particular participant provided nonsensical responses, data from this participant will be excluded. We will make sure that participants can only participate once from the same account in this specific study, and we will only recruit participants with a HIT approval rate of 95% or above. We will also check all IP addresses via <https://www.ipqualityscore.com/>; and we will remove any participants where one or more of the following is true: fraud score ≥ 85 ; TOR = True; VPN = True; Bot = True; abuse velocity = high. The replication sample size is the sample size after any exclusions of participants.

Materials: We will use the same material as in the original study, kindly provided by the original authors. In particular, we have received the exact wording of the items and implemented those in *Qualtrics*.

Procedure: We will closely follow the procedure of the original experiment. The following summary of the experimental procedure is therefore largely based on the description of the experiment in the section “Online Pilot Study” and “Study 3: Experimental Evidence” (pp. 1–2) in the Supplementary Information.

Participants will first be shown a Captcha, and will thereafter provide informed consent. After this we will include an attention check that participants will need to pass to continue to the study. This attention check is in addition to any other potential attention check(s) used in the original study. To induce low (or high) levels of felt income, participants will be presented with four different scenarios with severe financial implications that ask them to make decisions (e.g. “The economy is going through difficult times; suppose your employer needs to make substantial budget cuts. Imagine a scenario in which you received a 15% (5%) cut in your salary. Given your situation, would you be able to maintain roughly your same lifestyle under those new circumstances?”).

After each scenario, participants will then be asked to elaborate, being prompted “Why or why not? If not, what changes would you need to make? Would it impact your leisure, housing, or travel plans?”

Participants will then read a definition of community trust: “the extent to which you trust your community.” Levels of community trust will be manipulated as follows: participants will be asked to either list 2 (low) or 10 (high) examples where community trust was justified.

Next, we will assess temporal discounting factors using DEEP (Dynamic Experiments for Estimating Preferences), using the software implementation for *Qualtrics* as available via <https://sites.google.com/a/decisionssciences.columbia.edu/cds-wiki/deep-software>. A new, enhanced version of the DEEP software will be used to generate the stimuli and estimate the temporal discounting rate. The original authors agreed to apply this updated method (the old one is no longer available) and the developers of the software offered to assist us with conducting the preprocessing analyses. Following the original study, we will include 8 items that dynamically measure temporal discounting. DEEP measures temporal discounting by presenting participants with repeated choices between a smaller payoff that is received closer to the present (smaller/sooner) and a larger one that is received farther into the future (larger/later), e.g., “receive \$30 in 3 months or receive \$5 today”. Finally, we will collect demographic control variables.

Following the original study (and as advised by the original authors) we will also conduct one manipulation check for the income manipulation and two manipulation checks for the community-trust manipulation. For the income manipulation, we will validate by means of an independent samples *t*-test that participants in the low felt-income condition are more myopic than participants in the high felt-income condition. For the community trust manipulation, we will check that a) Participants in the high trust condition (i.e., the 10-example condition) have written more characters than participants in the low trust condition (i.e., the 2-example condition). An independent samples *t*-test will be used to validate this difference. b) For participants in the 10-example condition, the number of characters written for the first and the last example should not differ significantly. This will be validated by means of a paired samples *t*-test, for which a non-significant result is required. The conclusion about whether the study replicates or not will only be based on the main replication test (i.e., the results of the manipulation checks a) and b) do not affect this conclusion). The tests of a) and b) are mainly relevant for understanding why the study failed to replicate if it should fail to replicate.

When preparing this report, the original author Jachimowicz was concerned by the sample used in this replication. More specifically, he noted that the sample—a random group of MTurk participants, without any income representativeness quotas—is likely to have an overrepresentation of individuals with higher levels of financial hardship, given the ongoing economic effects of the pandemic particularly on individuals toward the bottom of the income distribution. He notes that given that the underlying sample may be systematically skewed toward higher financial hardship, the target manipulation which seeks to vary ‘felt income’ may be ineffective, being overridden by the true reality participants face in their everyday lives. As a result, Jachimowicz expressed his reservations about running the replication on this sample, given that the manipulation which is at the focus of the current replication efforts may not make a meaningful psychological difference to participants.

Following the original study, we will also measure levels of household income through 14 income buckets ranging from \$0-\$10k all the way up to \$200,000 or more. We will use the midpoint of each bucket for each participant, and compare the mean incomes across the replication and original study using an independent samples *t*-test (the highest open ended bucket will be assigned an income of \$225,000). We will also ask participants about the number of household members and adjust the household income by the square root of household members, as suggested by original author Jachimowicz. We will adjust the income in the replication study for inflation between the time of the original study (2016) and the replication study using the US CPI. Note that even if we cannot reject the null hypothesis of no difference in mean income between the samples, or even if the income in the replication sample is higher than in the original study, financial hardship may still differ between the two samples, given the economic effects of the pandemic (which may manifest, for example, in higher debts in one’s personal life as well as the broader community; as well as spending deficits accrued over the past year and a half; neither of which we capture here; we are grateful to the original author Jachimowicz for pointing this out).

Analysis: The analysis will be performed as in the original paper. That is we will compare myopia between participants in the high and low community-trust conditions with an independent samples *t*-test.

Subject payments:

We are standardizing payments across all replications so that studies have a certain show-up fee depending on the expected length of the study, with an hourly wage from the show-up fee of \$8 and a minimum payment of \$1 (for studies with incentive payment we use the same incentive payment as in the original study; and this payment is paid in addition to the show-up fee). If we have problems recruiting, we will increase the show-up fee.