MZES-GESIS Pre-Registration Challenge: Pre-Registration Template for Proposals Seeking Survey Time in GESIS-Panel

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Study Information

1. Title

    Information Flows and Trust Levels in the 21st Century: A Micro-Panel Re-Investigation.

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3. Abstract

    We test the hypothesis that communication technology mediated information flow drives social trust on a micro-panel that is representative for the German population, using linear panel data regression methods. We extend on previous research by considering a broader set of current communication media—thereby addressing the issue of media substitutability, eliciting data on actual usage over and above access, and tackling the issue of unobserved heterogeneity. The results contribute to an understanding of the determinants of trust and of its effects on the nature of contracting and on organizational structure.

Theory

4. Motivation and research questions

    Why are some regions or countries prosperous and well-governed while others struggle with poverty, corruption, and violence? Why do some teams and organizations work together and perform while others get stuck in mutual sabotage? A popular and recurring hypothesis is that such differences are driven by different levels of social *trust* [1-11]: if people trust one another to adhere to the rules, commit to their promises, and to be innately constrained in acting selfishly, then gains from cooperation are realized more easily and smoothly. In the opposite case, tedious and costly safeguards against cheating—such as complex contracts, monitoring and enforcement—must jump in and cooperation has a hard time.

Indeed, empirical studies have found positive relationships between measures of social trust and economic growth [12-18], development [19-22], quality of government [9,23], organizational performance [24,25], and even—at least if confidence is not excessive—individual economic success [26]. Implicit in studies of this sort—especially if the relationships are interpreted causally—is the assumption that trust levels are by and large exogenous during the period of observation. The assumption is backed by the hypothesis that current levels of social trust are steady state phenomena in a slow-moving process of cultural evolution, such that local trust levels are determined by local cultural history and are thus independent from current conditions [1,5,27]. The opposite is, however, implied by approaches based on rational choice and game theory, in which trust is a strategic equilibrium phenomenon, a (self-fulfilling) rational assessment that is instantly adapted to all particulars of the current situation [28]:[1] trust is the rational expectation—based on what is known about the game—that the other will cooperate. According to this view trust is a purely mediating variable that is highly malleable to current conditions.

The evidence suggests that the truth about trust is somewhere in the middle between the two approaches [29-35]. As suggested by the game theoretic approach, behavioral experiments using the “trust game” or similar paradigms show that information about the interaction partners and their past conduct is first-order important [30,36]. Applied to the field it follows that individuals with access to communication networks should be able to make more accurate trust assessments, in turn select into relations with trustworthy partners, and hence exhibit higher degrees of social trust [37-43]. In a seminal empirical study Fisman and Khanna [44] tested this hypothesis with a cross-country linear regression approach. They combine survey data from the 1990-1993 wave of the World Values Survey (WVS) and from the 1994 International Telecommunications Union (ITU) Yearbook, aggregated at the national level for 40 countries. Their measure of trust is the percentage of respondents in each country who answered that “most people can be trusted” when asked, “Generally speaking, would you say that most people can be trusted or that you can't be too careful when dealing with people?”. As a proxy for the level of two-way communication they use the number of landline phones per capita. Controlling for income (measured by the log of per capita national income) in a robust (heteroskedasticity-corrected) least squares regression with fixed effects for the cultural component of trust,[2] Fisman and Khanna indeed find a strongly positive (coefficient 0.75) and statistically highly significant (error probability less than 1%) relationship between phone connectivity and the social trust measure. The effect is robust to the inclusion of a measure of local community density (measured by the percentage of a country’s population living in urban areas). The coefficient of an interaction term with the phone

[1] In game theoretic terms, the situation is described by the set of available actions, the sequence of moves, the information structure, and the players’ preferences on the set of terminal histories. Implicit in those elements is the institutional environment and possibilities of mutual reward and punishment.
[2] Two sorts of dummies are included to capture cultural effects. The first is six basic cultures according to Huntington [45]: Western, Orthodox, Sinic, Hindu, African, and Latin American. The second is a dummy that captures whether a hierarchically organized religion (Catholic Christian, Orthodox Christian, Muslim) is dominant in the respective country. The latter is based on an argument of Putnam [4] that hierarchically organized religions discouraged the formation of a “habit of trust”.

connectivity measure is also significantly positive, suggesting that the main relationship is stronger in urbanized regions. The result challenges the notion that trust is a cultural primitive and thus exogenous in the short-run.

However, the study also has several limitations that serve as motivations for our planned study. First, as Fisman and Khanna note upfront, there is a mismatch between the motivating theory—which is micro-level—and their coarsely aggregated macro-level data. Second, a causal interpretation in the direction suggested by the hypothesis is problematic since phones per capita is reasonably endogenous. The correlation may be spuriously generated by omitted third variables that drive both trust and phone connectivity. Causality may also go the reverse direction: more trusting people could have a higher marginal return of communication devices—because they are likely more socially integrated—and hence self-select to be connected. And finally, the information and communication technology infrastructure and usage habits have changed dramatically since the observational period in the early 1990s. Landline phones now have a marginal role, most communication takes place through mobile devices and internet-based messaging services. Alongside the pace of communication has increased significantly. It is not clear whether and how the relationship “carries over to the 21st century”. Based on those observations, our research questions are threefold:

A. Is the positive relationship between information flow—proxied by communication media usage—and social trust replicable at the micro-level?
B. Is the one-way causal interpretation that information flow—proxied by communication media usage—drives trust credible?
C. What about newer communication technologies such as mobile phones and internet-based text and media messaging channels?

5. Hypotheses

We begin with research question (C), because it has notable conceptual implications for the evaluation of the other two. This is because different communication technologies are generally related by complementarity or substitutability. Especially the latter poses a problem for the approach of Fisman and Khanna [44], which is further elevated in a time-dynamic context: what if people just switch from landline phone to another communication medium, such as a cellular phone or an internet-based medium such as Skype or messaging services? Representing the scale of usage of landline phone, mobile phone, and internet-based media of participant i in period t by x_{it}^L, x_{it}^M and x_{it}^W, respectively (see sections 8 and 9 on measurement), we test the substitutability hypothesis with the nulls

SH1: The Kendall rank correlation coefficient between x_{it}^L and x_{it}^M across i and t is non-negative.

SH2: The Kendall rank correlation coefficient between x_{it}^L and x_{it}^W across i and t is non-negative.

SH3: The Kendall rank correlation coefficient between x_{it}^M and x_{it}^W across i and t is non-negative.

If the communication media are substitutes, then the data will reject SH1-SH3. Under this condition, considering only one medium—landline phone for instance—as a predictor of trust will generate substantial measurement bias: *measured* information inflow can go down, while *actual* inflow does not—or even go up. Furthermore, actual information inflow is not only a function of *access* (availability of a phone or internet connection, for instance) but also the extent of *usage*. Based on those considerations, we elicit information about access and usage of a broad set of communication media (see section 8) and construct an aggregate measure of usage (see section 9), which is a proxy for information inflow of much higher quality than just access to one particular medium. It serves as the basis for addressing questions (A) and (B).

Regarding research question (A), consider the statistical model of Fisman and Khanna [44]: they specify the linear model

$$y_i = \alpha + \beta x_i + Z_i \gamma + e_i$$

where i is an index of the observational unit (a country in their case), y_i the measure of trust (the proportion in the country who answered that “most people can be trusted” when asked, “Generally speaking, would you say that most people can be trusted or that you can't be too careful when dealing with people?”), x_i the measure of information flows via communication technology (number of landline phones per capita), Z_i a vector of co-variates that serve as control variables, and e_i is the error term. The control term $Z_i\gamma$ is specified

$$\gamma_1 z_{i1} + \gamma_2 z_{i2} + \gamma_3 z_{i3} + \gamma_4 z_{i4} + \cdots + \gamma_8 z_{i8}$$

where z_{i1} is a measure of income (the natural logarithm of gross domestic product per capita in 1992 US dollars), z_{i2} a measure of the level of urbanization (percentage of the population living in urban areas), z_{i3} the percentage of the population belonging to a hierarchical religion, and z_{i4} through z_{i8} is an array of dummy variables that represents the classification into the six basic cultures according to Huntington [45]. In an extended model, they also include an interaction term $\delta x_i z_{i2}$ to capture the hypothesis that the marginal effect of phones on trust to be affected by the level of urbanization. Their statistical hypothesis is $\beta \neq 0$, with a reasonable expectation that $\beta > 0$. Fisman and Khanna estimate the parameters α, β, γ, and δ on a cross-sectional dataset with 40 units (countries) by means of robust (heteroskedasticity-corrected) least squares regression. One limitation of this approach is the mismatch between the motivating theory—which is micro-level—and their coarsely aggregated macro-level data.

A direct transfer of the linear model of Fisman and Khanna [44] to micro-level is

$$y_{it} = \alpha + \beta x_{it} + Z_{it}\gamma + e_{it}$$

where i is an index of the individual participant, t is an index of the survey wave, y_{it} the measure of trust, x_{it} the measure of information inflow via communication technology,

Z_{it} a vector of co-variates (explained in section 8), and e_{it} is the error term.[3] In the current study we measure those variables at the individual level (also explained in section 8), and re-evaluate the hypothesis

SH4: $\beta \neq 0$.

with the reasonable expectation that $\beta > 0$.

Regarding research question (B), a causal interpretation of the estimate of β from Fisman and Khanna [44] and the above micro-model specification is problematic. The specification amounts to pooled ordinary least squares (POLS), which rests on six key assumptions [50]:

i. Linearity: the dependent variable is formulated as a linear function of a set of independent variables and the error term.
ii. Exogeneity: the expected value of error is zero, or equivalently, the errors are not correlated with any regressors.
iii. Homoskedasticity: errors have the same variance.
iv. Non-autocorrelation: errors are independent from one another.
v. The observations on the independent variables are not stochastic but fixed in repeated samples without measurement errors.
vi. Full rank: there is no exact linear relationship among independent variables (no multicollinearity).

If those conditions are true, the POLS estimate of β is unbiased and efficient, and hence a sensible basis to test the hypothesis $\beta \neq 0$. However, it is reasonable to expect individual heterogeneity beyond the co-variates accounted for in Z_{it}. Such individual heterogeneity is the major weakness of the cross-sectional approach of Fisman and Khanna [44]. Such individual effects are captured by the extended model

$$y_{it} = \alpha + \beta x_{it} + Z_{it}\gamma + u_i + e_{it}$$

If individual effects u_i are not zero, then assumptions (ii) through (iv) may be violated. Specifically, errors may not have same variance but vary across individual (heteroskedasticity) and/or are related with each other (autocorrelation). Consequently, the POLS estimator is biased and may result in false rejection or false non-rejection of the hypothesis $\beta \neq 0$. On theoretical grounds, we consider this problem to be salient in the present setting. It is very likely that there are individual characteristics—like personal history, cultural background, intelligence or other personality traits—that cannot be properly accounted for in Z_{it}.[4] That is the point where panel data helps, because the individual effects can be estimated from the data (which is not possible with cross-sectional data). Likewise, we can also account for time specific effects, like national policies and regulations or general technological change,

[3] This implicitly assumes that y_{it} is a metric variable. We evaluate the robustness of results with respect to this assumption in section 14.

[4] This is reflected in the efforts of Fisman and Khanna [44] to explicitly account for “culture”, but their proxies are admittedly very crude.

$$y_{it} = \alpha + \beta x_{it} + Z_{it}\gamma + u_i + v_t + e_{it}$$

that would otherwise confound the results due to omitted variable bias. Assuming that this adequately models the data generating process (we continue in section 10 with further details), we re-evaluate SH4 while accounting for possible unobserved individual-specific and time-specific effects. This is a significantly tougher test of the hypothesis and elevates the credibility of a causal interpretation of β by mitigating omitted variable bias. Note that it does *not* address the issue of possible reverse causality. The data available for the current study is not sufficient to do the analysis required to pin down causal direction, we leave this for a follow-up study (see section “Other”).

Design Plan

6. Study design

The present study is observational, that is, we do not directly intervene into the data generating process. The variables described in section 8 are measured repeatedly in a constant sample of participants that is representative for the German population (see section “Sampling Plan”). If there is no missing data or panel attrition, we obtain a short ($N > T$) balanced panel dataset. Missing data (see section 15) or panel attrition may render the dataset unbalanced, but we expect these effects to be negligibly small. The study corresponds to a mixed trial design with a between-subject (unpaired) and a within-subject (paired) dimension but without controlled treatment condition assignment. We aim at two, possibly three waves of data collection within the GESIS Panel with at least six months between individual waves.

7. Randomization

A randomized trial would be ideal for causal inference. Such a trial would require assigning the different communication technology access levels randomly to the participants and expose them to that “treatment” for an extended period. Implementing such treatments and monitoring compliance is excessively costly and legally problematic. It is not possible in the context of the GESIS panel.

We therefore let participants self-select into the “treatments” during their everyday choices and employ appropriate statistical methods to account for selection on observables and non-observables in order to construct a reasonable counterfactual (see below). The GESIS Panel sample includes online (web-based) and offline (by mail) participation with about 33 percent of respondents taking part offline.

Variables

8. Measured variables

We measure trust with an improved variant of the original WVS survey instrument used by Fisman and Khanna [44]. It consists of three questions and has been developed as a response to the problem that the two response categories in the original question are not mutually exclusive, since respondents might consistently agree to both answers [46,47]. The first two questions just split the WVS question up into two parts:

a. “On the whole one can trust people” and
b. “Nowadays one can't rely on anyone”,

with four answer categories, respectively, “fully agree”, “rather agree”, “rather disagree”, and “fully disagree”. The third item is included to address a criticism of the WVS question that answers may significantly depend on how people understand “most people”; in particular, whether they include people they know personally or not [48]. The last item therefore explicitly asks about strangers:

c. “If one is dealing with strangers, it is better to be careful before one can trust them”,

with the same answer categories as before. The question text reads as follows: “Please indicate for each statement whether you agree or disagree.” It has been shown that this three-question version has a high degree of reliability and behavioral validity [49]. The answer categories are coded as integers from 1 (“disagree totally”) through 4 (“totally agree”), respectively. The instrument was asked in the recruitment interview of the GESIS Panel (6/2013–12/2013).

To measure communication technology usage we implement a new question battery that captures usage of different media (landline phone, mobile phone, and internet-based services) along three dimensions: (i) access, (ii) usage, and (iii) size of personal network (number of communication partners). Usage is the focal measure, access and network size are elicited for exploratory purposes. The battery will be tested via cognitive interviewing methods at the GESIS Pretest Lab to optimize the questions prior to their administration in the actual survey and thereby to improve the quality of data.

Question 1: Access

On an average day, do you have access to the following media?

	Yes	*No*
Landline telephone	❑	❑
Mobile telephone	❑	❑
Internet-based telephone (e.g. Skype, Facetime etc.)	❑	❑
Internet-based messaging services (e.g. WhatsApp, Facebook Messenger, SMS, e-mail etc.)	❑	❑

Question 2: Usage

On an average day, how much time do you spend communicating through the following media, whether for work or personal use?

Please give your answer in hours and minutes. If you don’t use the respective media, please enter 0.

	Hours	Minutes
Landline telephone	_____	_____
Mobile telephone	_____	_____
Internet-based telephone (e.g. Skype, Facetime etc.)	_____	_____
Internet-based messaging services (e.g. WhatsApp, Facebook Messenger, SMS, e-mail etc.)	_____	_____

Question 3: Number of communication partners

On an average day, with how many different people do you communicate through all of the previously mentioned media (landline telephone, mobile telephone, internet-based telephone, internet-based messaging)?

- ❑ *0-3 persons*
- ❑ *4-6 persons*
- ❑ *7-10 persons*
- ❑ *11-20 persons*
- ❑ *21-30 persons*
- ❑ *31 or more persons*

The responses to question 2 measure x_{it}^{L} (landline telephone), x_{it}^{M} (mobile telephone), and x_{it}^{W} (sum of the two internet-based media). To control for the likely presence of an income effect on communication technology access and usage, we use the variable measuring the respondent's household income in EUR per month, which is measured with nine categories in the GESIS Panel (Standard Edition). To capture complementarity between residential density and communication technology access and usage, we use a variable (already measured in the GESIS panel) that asks for the type of residential area (big city, suburban, town or small town, rural area, farmstead or freestanding house in the countryside).

The cross-country study of Fisman and Khanna [44] required statistical control of the widely different cultural backgrounds of the countries. By restricting the study to a single country (Germany) we get rid of this problem to some extent. We do control for individual differences in cultural imprint by gender, age, dummies for religious confession (none, roman catholic, protestant, other, prefer not to disclose), and a dummy whether the respondent lives in eastern Germany to capture possible long-term effects of different institutions in the former GDR. All those variables are included in the Standard Edition of the GESIS Panel. Residual individual heterogeneity is absorbed by the individual-specific and time-specific effects in the panel model (see section 10).

9. Indices

We follow Fehr [30] in constructing variable y_{it} by coding the responses to the three trust questions from 0 (lowest trust) to 3 (highest trust), respectively, and adding up the values. Variable x_{it} is total communication time (the sum of responses to question 2).

Sampling Plan

A panel dataset is sampled as part of the continuously operating GESIS panel. The GESIS Panel provides a probability-based mixed-mode access panel infrastructure located at GESIS Leibniz Institute for the Social Sciences in Mannheim, Germany. The dataset has multiple observational units, which is individual survey respondents, each of which has repeated measurements at different time periods. The data collection procedures and sample size are contingent on decisions by the primary investigators of the GESIS Panel. We do not restrict the samples collected by the GESIS panel. Static panel data models require in principle at least two waves, three if first-differencing is applied. We therefore aim at two, possibly three waves of data.

Analysis Plan

10. Statistical models

We fit the linear panel data model

$$y_{it} = \alpha + \beta x_{it} + Z_{it}\gamma + u_i + v_t + e_{it}$$

with the dataset explained above. SE4 is independent from whether individual and time effects are modeled as intercept components (fixed effects, FE)

$$y_{it} = (\alpha + u_i + v_t) + \beta x_{it} + Z_{it}\gamma + e_{it}$$

or error-components (random effects, RE)

$$y_{it} = \alpha + \beta x_{it} + Z_{it}\gamma + (u_i + v_t + e_{it})$$

or as a mixture where one effect is fixed and the respective other random. But the specification choice has implications for the quality of the estimate, and hence the statistical test of SE4. The choice is not straightforward and must be done on theoretical and statistical grounds. A FE model examines individual/time differences in intercepts, assuming the same slopes and constant variance across individuals/time. Since an individual specific effect is time invariant and a time specific effect common to all individuals, they are a part of the intercept and hence can be correlated with other regressors (OLS assumption ii is not violated). A RE model assumes that individual effect (heterogeneity) and time effect is *not* correlated with any regressor and then estimates error variance specific to individuals and times. Hence, u_i and v_t are components of a composite error term. The intercept and slopes of regressors are the same across individual. The difference among individuals or time periods lies in their individual specific errors, not in their intercepts. On theoretical grounds, we consider a full FE specification suitable for the current setting. First, it is reasonable to expect that there are time specific "shocks" that affect trust in all individuals, such as critical events or national policies or politics. Second, it is also reasonable to expect there to be "baseline trust" levels that are stable personality traits, and hence differ across individuals. Third, it is to be expected that those specific effects are also related with (self-selected) communication technology connectivity and usage, and possibly other regressors such as income. Based on those considerations a RE specification seems not appropriate.

Our specification hypothesis can be broken down into a set of statistical auxiliary hypotheses that can be tested with the dataset at hand. First, after estimating the POLS model and the FE model by least squares dummy variable (LSDV) regression (or equivalently within-effect estimation methods), the hypothesis that all the specific effects are zero,

AH1: $u_1 = \cdots = u_{N-1} = 0$

AH2: $v_1 = \cdots = v_{T-1} = 0$

can be tested with the *F*-specification-test (which is based on loss of goodness-of-fit). The alternative for AH1 is that at least one individual-specific effect is not zero, the alternative to AH2 that at least one time-specific effect is not zero. Thus, if AH1 is rejected individual-specific effects matter, if AH2 is rejected time-specific effects matter; if none is rejected POLS is fine.

Second, after estimating the POLS model and the RE model by generalized least squares (GLS) regression, the hypothesis that all the specific variance components are zero,

AH3: $\sigma_u^2 = 0$

AH4: $\sigma_v^2 = 0$

can be tested with a Breusch-Pagan Lagrange Multiplier (LM) test (which follows the chi-squared distribution with one degree of freedom). If AH3 is rejected there is significant individual-specific random effect in the data, if AH4 is rejected there is significant time-specific random effect; again, if none is rejected POLS is fine.

Finally, to select between FE and RE, the null hypothesis that

AH5: All individual-specific effects u_i are uncorrelated with any of the other regressors.

AH6: All time-specific effects v_i are uncorrelated with any of the other regressors.

can be evaluated with the Hausman test. If the null hypothesis of no correlation is not violated, LSDV and GLS are consistent, but LSDV is inefficient; otherwise, LSDV is consistent but GLS is inconsistent and biased [50]. The estimates of LSDV and GLS should not differ systematically under the null hypothesis. If AH5/AH6 is rejected, it can be concluded that individual-specific/time-specific effects are significantly correlated with at least one regressor in the model and thus the RE model is problematic.

According to our theoretical considerations, we expect the rejection of AH1, AH2, AH5 and AH6, which implies that a FE specification is adequate. Otherwise, we will also report the results of the POLS model, the RE model, or a mixed model, depending on which subset of hypothesis of AH1-AH6 cannot be rejected.

11. Inference criteria

We evaluate a null hypothesis as rejected if the *p*-value of the applicable test is 0.01 or less, that is, the error of a false negative is not larger than 1%. We then call the associated result statistically “significant”. For *p*-values strictly above 0.01 but strictly below 0.05 we consider the null hypothesis to be “likely rejected” and call the associated result “marginally significant”. Those criteria are at the stricter end of standard practice in the social sciences [51].

12. Data exclusion

We do not exclude any data except if there are missing data points (see next section). Each observation is treated and weighted equally in all analysis.

13. Missing data

We apply “listwise deletion” in case of missing data, that is if there is at least one value of a variable missing in a record due to non-response, recording errors or other reasons, the entire record is excluded. This procedure affects statistical power and, in principle, may affect the results, since—for instance—participants may self-censor by non-responding. However, since the non-response rate in the GESIS panel is generally low and we do not ask for sensitive information (except income), we expect those effects to be negligible.

14. Exploratory analysis and robustness checks

Implicit in the linear specification of our model in section 10 is the assumption that y_{it} is a metric variable. This is not a problem for the statistical estimation of the parameters, but the interpretation of the sizes of effects (other than their sign) could be problematic at the boundaries of the trust variables’ domain. We therefore complement the linear model with an ordered-probit model—which assumes rank-order in y_{it}. We do not expect the conclusions from this model to be qualitatively different than the ones from the linear model, but they provide another perspective for interpretation.

Other

For the current study we are constrained on two, possibly three waves of data. This is enough for fixed and random effects panel analysis, but insufficient for maximum likelihood structural equation models (ML-SEM) or dynamic panel analysis (DPD) that are powerful tools to address the issue of reverse causality. We plan to continue data collection to do such analysis in a follow-up study.

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