



Trust games: A meta-analysis

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ABSTRACT

We collect data from 162 replications of the Berg, Dickhaut, and McCabe Investment game (the “trust” game) involving more than 23,000 participants. We conduct a meta-analysis of these games in order to identify the effect of experimental protocols and geographic variation on this popular behavioral measure of trust and trustworthiness. Our findings indicate that the amount sent in the game is significantly affected by whether payment is random, and whether play is with a simulated counterpart. Trustworthiness is significantly affected by the amount by which the experimenter multiplies the amount sent, whether subjects play both roles in the experiment, and whether the subjects are students. We find robust evidence that subjects send less in trust games conducted in Africa than those in North America.

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1. Introduction

Trusting in others and reciprocating that trust with trustworthy actions are everyday aspects of life. In most neighborhoods, most of the time, unlocked doors remain unopened, lost wallets containing cash are returned, and the vast majority of contracts, thankfully, remain incomplete. This is fortunate, since all of this trust and trustworthiness is good for the economy. Trust within organizations increases efficiency by lower monitoring costs (e.g. Frank, 1988), lowering turnover (Dirks & Ferrin, 2002), and increasing uncompensated positive behavior from employees (Dirks & Ferrin, 2002; Konovsky & Pugh, 1994). On a higher level of aggregation, scholars have linked a shared willingness to engage in trusting or trustworthy behavior to better economic outcomes (Arrow, 1972; Fukuyama, 1995; Putnam, 1993). Knack and Keefer (1997) use cross-country data to show a link between higher trust and higher GDP per capita. Higher levels of trust have been associated with more efficient judicial systems, higher quality government bureaucracies, lower corruption, and greater financial development (Guiso, Sapienza, & Zingales, 2004; La Porta, Lopez-de-Silanes, Shleifer, & Vishny, 1997).

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Over the last two decades the measurement of trust and trustworthiness has been revolutionized through the use of laboratory experiments. One of the earliest moves in this direction was a game constructed by [Camerer and Weigelt \(1988\)](#). A simplified version of their experiment designed by [Berg, Dickhaut and McCabe \(BDM, 1995\)](#) has come to dominate the field. The BDM two stage trust game has become a popular and frequently replicated measure of behavioral trust and trustworthiness.¹ It involves a sequential exchange in which there is no contract to enforce agreements. Subjects are endowed with \$10, anonymously paired and assigned to either the role of sender or receiver. At stage one of the game, the sender may either pass nothing, or any portion x of the endowment ($0 \leq x \leq 10$) to the receiver. The sender then keeps $10 - x$, and the experimenter triples the remaining money so that $3x$ is passed onto the receiver. In stage two, the receiver may either pass nothing, or pass any portion y of the money received ($0 \leq y \leq 3x$) back to the sender. The amount passed by the sender is said to capture trust, “a willingness to bet that another person will reciprocate a risky move (at a cost to themselves),” and the amount returned to the trustor by the trustee to capture trustworthiness ([Camerer, 2003, p. 85](#)).

Berg and colleagues identified a considerable willingness to trust and reciprocate trust among subjects engaging in the one-time, anonymous, and controlled exchange setting – a result which deviated substantially from predictions assuming rational and self-interested opportunistic behavior. Some recent work has examined the limitations of this measure, arguing that the amount sent in the game confounds trust with altruism ([Cox, 2004](#)) and betrayal aversion ([Bohnet, Greig, Herrmann, & Zeckhauser, 2008](#); [Fehr, 2009](#)) and omits other important facets of trust ([Ben-Ner & Halldorsson, 2010](#); [Ermiş, Gambetta, Laurie, Siedler, & Noah Uhrig, 2009](#)). Nevertheless, the game remains a popular choice among trust researchers. The trust game has been replicated across numerous countries, often using slightly different experimental protocols than the original BDM game. We take advantage of this diversity to perform a meta-analysis of the trust game in order to identify the effect of different experimental protocols and unobserved factors correlated with geographic region on this behavioral measure of trust and trustworthiness. Our data set covers 162 replications of the game across 35 countries. On average, there were 148 players in each of these replications and a total of 23,924 individuals.

While frequently used in fields such as medicine and psychology, meta-analyses are less common in economics ([Van den Bergh, Button, Nijkamp, & Pepping, 1997](#)). Given the relative youth of the field of experimental economics, it should come as no surprise that there were few meta-studies before 2000 or so.² However, in the last few years there has been a proliferation of informal surveys of experimental results ([Bowles, 2008](#); [Chaudhuri, 2010](#); [Croson & Gneezy, 2009](#); [Kagel & Roth, 1995](#)), many of which are focused on the trust game ([Camerer, 2003, chap. 2](#); [Chaudhuri, 2009](#); [Fehr, 2009, chap. 3](#)). Our paper differs from these in that we systematically identify experimental and geographic variables common to as many iterations of the trust game as possible, create a consistent data set, and employ formal econometric tools to identify the effect of these variables on the amount sent and returned in the trust game. In this sense, our study most resembles those performed by [Zelmer \(2003\)](#) for the public goods game, [Oosterbeek, Sloof, and Van de Kuilen \(2004\)](#) for the ultimatum game, and [Jones \(2008\)](#) for the prisoner's dilemma game.

The most important reason to conduct a meta-analysis of the trust game is to verify the generalizability of its findings.³ It has been well known for some time that small changes to experimental protocols can have dramatic effects on behavior in the lab. For example, “framing effects” involving the alteration of a single word in subject instructions, like those demonstrated by [Burnham, McCabe, and Smith \(2000\)](#) for the public goods game, can significantly affect behavior (see also [Bohnet & Cooter, 2005](#); [Hertwig & Ortmann, 2001](#); [Roth, 1995](#)). Similar observations have been made with respect to frequently used procedures in the trust game such as whether or not the experiment is double blind ([BDM, 1995](#); [Hoffman, McCabe, Shachat, & Smith, 1994](#)) or if the strategy method is used to elicit second mover decisions ([Casari & Cason, 2009](#)). By looking at a large number of games, we identify which experimental protocols matter and by how much.

In addition to asking whether the rules used in the lab bias trust game behavior, we also investigate whether the average subject pool is representative. Most experiments conducted in the West use cheap, readily available, student subjects. There is evidence, however, that students are less trusting and exhibit less trustworthiness than adults ([Fehr & List, 2004](#)). More generally, even if one argues that experimental protocols and subject pools do not generate significant biases, it is possible that there are unobserved characteristics of the local population which make inferences about behavior difficult to generalize into other geographic regions (for examples see [Henrich et al., 2010](#)). Indeed, given the likelihood that trust and trustworthiness are, at least partly, endogenous to local institutions, we would be surprised if there are not systematic differences across geographic regions ([Fehr, 2009](#)). There is evidence for geographic variation in performance in ultimatum games ([Oosterbeek et al., 2004](#)), public goods games ([Gächter, Herrmann, & Thöni, 2010](#)), as well as dictator games ([Henrich et al., 2001](#)) and several studies show variation in trust and trustworthiness across populations as well ([Bohnet et al., 2008](#); [Naef & Schupp, 2008](#)). While these trust game studies are based on relatively small sample sizes, the advantage of our meta-analysis is that it is based on data collected from a much larger population and drawn from countries covering five distinct geographic areas. We document our data, which readers may also like to consult as a source of information about what has been done in the literature (see [Appendices A and B](#)).

¹ This game was originally called the “investment game” and is similar to the trust game in [Kreps \(1990\)](#).

² The exception being [Sally's \(1995\)](#) application of meta-analytic procedures to the Prisoners' Dilemma game.

³ The generalizability of laboratory experiments has recently been the subject of a well publicized exchange between [Levitt and List \(2007\)](#) and [Falk and Heckman \(2009\)](#). While our results bear on some of the claims made within this debate (e.g. the effect of stakes on “irrational” behavior), they are largely tangential to the main thrust of that discussion which focused on the relative merits of lab versus field experiments.

2. Data and methodology

2.1. Meta-analytic procedure

The basic purpose of a meta-analysis is to apply methodological rigor to a literature review through the use of statistical techniques (Glass, 1976; Stanley, 2001). By combining multiple studies, a meta-analysis can reduce the impact of sampling error and improve estimates of the effects on trust game behavior (Hunter, 1997). Meta-analyses are most commonly used to conduct a quantitative literature review of a research finding or “effect” that has been investigated in primary research under a variety of different circumstances. The outcome variable is usually a measure of effect magnitude and is generally captured by a single summary statistic per data set in the form of a standardized mean difference or a correlation coefficient. In addition to this index of effect size, the meta-analysis includes carefully selected characteristics of the experimental conditions that are hypothesized to influence the experimental outcomes.

The meta-analysis in this study has the characteristics of a standard meta-analysis because it examines data across previously analyzed studies with the same outcome measures to determine the relationship between several coded statistics. The basic unit of analysis in this meta-analysis is past studies of the trust game experiments, each consisting of a number of subjects exposed to a unique set of experimental conditions. Our study however, departs from the more common use of meta-analyses to examine effect sizes or correlations because the outcome variable is a proportion (Hedges & Olkin, 1985). We have two effect measures from each replication of the trust game: the average amount sent and average amount returned across replications of the BDM trust game.

2.2. Identification of studies

The explanatory power of a meta-analytic study depends heavily on the inclusion of all relevant manuscripts. A systematic search strategy must be employed to not only identify all publications that have replicated the trust game since BDM in 1995, but also conference papers, pre-publications work and unpublished replications. Identifying unpublished works allows us to minimize potential bias arising from the fact that studies with significant findings are usually not published (Rosenthal, 1979).

As a first step in our systematic search we used the Web of Science database search engine to identify all published articles that cite BDM. This produced a list of 430 papers which were then carefully reviewed to identify 53 studies which included replications of the BDM trust game. The next step was to search the FirstSearch database which includes four important databases: WorldCat, ArticleFirst, ECO, and WorldCatDissertations. WorldCat is a cooperative database of bibliographic records contributed by more than 50,000 libraries. ArticleFirst is an index of articles from the contents pages of journals containing more than 15 million entries beginning in 1990. ECO is another database of scholarly journals and World Cat Dissertations provides access to all the dissertations available in OCLC member libraries. We searched this comprehensive database for the keywords “trust game” or “investment game”. This produced a list of 275 books, articles and dissertations which were reviewed for replications of the BDM experiment which were also not duplicates of those identified in the first step. This step uncovered seven additional papers.

The next three steps identified working papers that have not been published. We used the Social Science Research Network (SSRN) e-library database, which catalogues over 100,000 searchable working papers and forthcoming journal articles, to search for keywords “trust game” or “investment game” in the title or abstract. This produced a list of 153 papers, and 18 additional replications of the BDM game. Step four identified additional working papers by economists using EconPapers, a search engine that provides access to the largest collection of on-line economics working papers and book chapters, RePEc, and searched for “trust game” or “investment game”. This produced a list of 327 papers which were again carefully reviewed for experiments replicating the BDM game. Twenty-one additional papers were added to our data set. As a final step we posted two appeals for additional papers on the Economic Science Association (ESA) discussion board.⁴ This produced an additional 31 unpublished contributions to our dataset.

An important part of our methodological approach is that the observations in our data are comparable. This necessitated a somewhat narrow definition of what constitutes an iteration of the trust game. In order to maintain consistency in the measurement of our dependant variables, the amount sent and returned in the game, we did not include replications of the ‘binary trust game’ which forces players to make dichotomous choices (e.g. Gambetta, 1988; Snijders, 1996). We also did not include replications with participants younger than 17 years (e.g. Sutter & Kocher, 2007). In order for a replication to be included in our dataset it was also necessary for us to be able to identify a control treatment which was meant to mimic the original BDM environment. An example of a study that did not satisfy this condition would be Slonim and Guillen (2010) in which there were no trust games played in which subjects did not know the gender of their counterpart. In addition, if the replication included multiple rounds played (with the same player dyads) we had to be able to identify all relevant variables from the first round. An example of a paper where we could not do this is Baumgartner, Heinrichs, Vonlanthen, Fischbacher, and Fehr (2008). Finally, we excluded experiments studying group decision making unless there was an identifiable control round played by individuals. An example of a paper excluded for this reason is Akai and Netzer (2009).

⁴ The URL for the ESA discussion board is: <http://groups.google.com/group/esa-discuss>.

The search and criteria for selecting studies resulted in 130 manuscripts providing 162 trust game replications with 161 observations for the mean amount sent and 137 for the mean amount returned. Some manuscripts provide more than one study replicating BDM in more than one country or group of independent subjects. Also, while we identified a total of 162 studies, one of these studies manipulated the amount sent, and therefore only contributed data on the amount returned. A full list of the 130 manuscripts included in this meta-analysis is provided in [Appendix A](#). Our dataset and the corresponding 162 observations is provided in [Appendix B](#).

2.3. Description of variables used to predict amount sent

Our data are cross-sectional with each observation representing a replication of the trust game. Our two dependent variables are the proportions of the endowment sent (measure of trust) and available funds returned (measure of trustworthiness) averaged across each replicated trust game. Methodological variations across trust game studies could systematically influence a sender's expectations or willingness to make themselves vulnerable to 'trust'. Below we describe each of these variables and explain how the existing literature suggests they may impact behavior in the lab.

2.3.1. Amount at stake

BDM endowed their senders with \$10 while other researchers have conducted their studies with smaller and larger stakes. It is possible that the observed deviations from assumptions of self-interested utility maximization by subjects in the lab may simply be due the fact that there is not enough money on the table ([Carpenter, Verhoogen, & Burks, 2005](#); [Levitt & List, 2007](#); [Slonim & Roth, 1998](#)). A study conducted in rural Bangladesh provides some evidence that trust behavior falls as stakes increase ([Johansson-Stenman, Mahmud, & Martinsson, 2008](#)). Research examining the effect of stakes did not, however, find that higher stakes substantially change the proposer's behavior in the ultimatum game ([Cameron, 1999](#)), and stakes were also not found to shift observed preferences in gift exchange games ([Fehr, Fischbacher, & Tougareva, 2002](#)). Several experimental studies which examine the relationship between stakes and individual risk preferences, in contrast, have found that people become more risk averse when there is more at stake (e.g. [Binswanger, 1980](#); [Holt & Laury, 2002](#)). In a lottery choice experiment that measured risk aversion over a wide range of payoffs, [Holt and Laury \(2002\)](#) found that subjects became more risk averse as the stakes increased. Whether due to risk aversion, or simply not enough "money on the table", if stakes matter, then the prediction is that the higher the stakes, the less trusting behavior observed in the lab.

In our data we measure amount at stake as the size of the sender's endowment, represented by the variable "Sender Endowment". The variable is standardized by dividing the endowment in the original currency by a purchasing power parity (PPP) conversion factor developed by the World Bank and published as part of the organization's annual compilation of World Development Indicators.⁵ The PPP conversion factor allows currencies to be converted into a common unit of account that reflects equivalent purchasing power by using information from surveys of prices and expenditures across countries ([Dornbusch & Fischer, 1994](#)). The PPP conversion factor represents an approximation of the number of units of a country's currency required to buy a set bundle of goods and services in the domestic market as a US dollar would buy in the US market. We use the PPP conversion factor for the year in which each study was conducted.

2.3.2. Receiver endowment

In BDM's trust game both the sender and receiver were given equal endowments. The sender then decided whether to pass any of that endowment to the receiver. Many replications by other researchers chose not to endow the receiver in the trust game, presumably to lower the cost of administering the experiment. Models of behavior that incorporate other-regarding preference (or fairness) into players' utilities (e.g. [Fehr & Schmidt, 2002](#)) and theories of equity ([Adams, 1965](#); [Adams & Freedman, 1976](#)) would lead us to predict behavioral differences when the endowments are unequal. Subjects have been randomly assigned to their roles, so unequal endowments mean there is an *inequity*, which can produce feelings of distress and guilt ([Adams, 1965](#); [Adams & Freedman, 1976](#)). Only by passing more to the unendowed receiver can the sender eliminate the feeling of distress and restore equity. The variable "Receiver Endowed" is equal to 1 if the receiver was endowed and 0 if the receiver was not endowed.

2.3.3. Rate of return

BDM argued that trust must produce a welfare gain in order to facilitate exchange – both parties must be better off when trust occurs. The BDM game therefore incorporated a positive rate of return on trust by tripling the money passed to the receiver. While many replications of the BDM trust game maintained the convention of tripling the quantity sent, other researchers doubled the quantity sent simply to lower the cost of running the study. But the potential gains from trust, if the counterpart were to be trustworthy, may enter into a person's calculation of whether to trust ([Coleman, 1990](#)). A higher rate of return in the trust game may therefore increase the likelihood that the sender would risk passing more money to the receiver, while the lower rate of return, in contrast, may lower incentives to pass money. We code the rate of return as "Rate Return". A value of 0 indicates the amount sent was doubled and 1 indicates the amount sent was tripled by the experimenter.⁶

⁵ <http://data.worldbank.org/data-catalog/world-development-indicators>.

⁶ There was one study which multiplied the endowment by 6 ([Ackert, Church, & Davis, 2011](#)). We decided to code this as a 1 rather than drop the observation.

2.3.4. Both roles

Replications of the BDM study sometimes asked subjects to play both the role of the sender and the receiver with different partners. This approach offers the advantage of collecting more testable observations from fewer subjects, but it is frequently employed without reference to possible systematic effects it might have on trust behavior. Burks and colleagues (Burks, Carpenter, & Verhoogen, 2003) found that subjects who played both roles and who were aware of this before the experiment began, on average sent less money to their counterparts than those who only played the sender role in the trust game. They argue that subjects feel less responsibility toward their counterpart since their earnings will depend on two separate interactions and as a result are willing to act more selfishly. The variable “Both Roles” is coded 1 if they played both roles and 0 if they played only one role.

2.3.5. Real players

Replications of BDM sometimes included a simulated counterpart rather than an actual human being. In such replications, subjects are told that they are interacting with a real counterpart, and it was generally assumed that subjects believed this to be true. Many of these studies did not, however, report the results of a manipulation check to verify that subjects actually believed what they were told – that they were interacting with a real counterpart when in fact they were not. Research has shown that people make different decisions in exchange settings with real counterparts than when they know they are dealing with a computer (e.g. Bottom, Holloway, Miller, Mislin, & Whitford, 2006; Sanfey, Rilling, Aronson, Nystrom, & Cohen, 2003), suggesting that subjects may send less money in the trust game when interacting with a computer counterpart. The variable “Real Person” is equal to 1 if the counterpart was another experimental subject and 0 if it was a computer or confederate.

2.3.6. Random payment

While the subjects in the original BDM game played with real money that corresponded to their earnings, replications of the trust game experiment sometimes involved random rather than guaranteed payments to the subjects. This approach is also employed to reduce costs to the experimenter. For example, rather than pay each subject for the amount ‘earned’ by playing the trust game, sometimes subjects are told that one (or a few) will be randomly selected and awarded the amount earned in the study. While Bolle (1990) found that the use of random payments to subjects does not systematically affect behavior, Bottom’s research (1998) suggests that it might by introducing additional risk to the game. The subject’s upside reward depends not only on whether the counterpart is trustworthy, but also on an unrelated lottery. This added risk would make the average sender less willing to pass money to their counterpart. We code this variable as “Random Payment” where 1 indicates that subject behavior in the trust game is rewarded randomly according to some pre-specified rule of the experimented, and 0 indicates otherwise.

2.3.7. Strategy method

Subjects in some replications of the BDM trust game are asked to state the amount that they would return for each conceivable amount passed by the sender. Trustees are asked to respond to every possible behavior from their counterpart, allowing the experimenter to collect more information about what the subjects might consider to be fair behavior (Bahry & Wilson, 2006). While some researchers have indicated that the process of thinking through the behavioral implications of each possible outcome changes the subjects’ perceptions of the game and leads them to process their decisions differently (Güth, Tietz, & Müller, 2001; Roth, 1995), others have shown that eliciting such responses from subjects does not have a systematic effect on behavior (Brandts & Charness, 2000). In a recent survey of the literature Brandts and Charness (2010) posit that the strategy method does not lead to different experimental results than the standard direct-response method. The variable “Strategy Method” is equal to 1 if the receiver was subjected to the strategy method and 0 otherwise.

2.3.8. Anonymity

The original BDM game guaranteed subjects anonymity in order to hold constant any potential impact of past or possible future interactions between the parties outside the lab. When the guarantee of anonymity among players is eliminated, their behavior may be motivated by factors outside the lab, such as an individual’s concern for their reputation (Kreps, 1990), a motivation to reciprocate past kind acts (Gouldner, 1960), or a fear of retribution (Bies & Tripp, 1996). These concerns suggest that when players are not anonymous they will pass more money to their counterparts in the trust game. The variable ‘anonymous’ is coded 1 if the players in the study are anonymous, and 0 if the setting is not anonymous.

2.3.9. Double-blind

BDM placed great emphasis on the importance of a double blind procedure in their experiment – a procedure to ensure that not only other subjects, but also the experimenter cannot trace decisions back to the individuals who made them. This procedure helps to rule out the possibility that people are engaging in trust behavior because they want to build or protect their reputation with the experimenter as a generous and agreeable individual with whom future exchanges would be desirable. Conveying these characteristics to an experimenter may produce the perception of some probability of benefits in future exchanges with the experimenter, and as a result the future expected payoff from engaging in such exchanges could motivate the subject’s behavior in addition to, or instead of, trust in the counterpart.

Hoffman and colleagues found significantly more self-interested behavior in experimental dictator games using a double-blind procedure (Hoffman et al., 1994). Nevertheless, replications of the BDM trust game usually do not employ the strict

double blind procedure, suggesting that experimental researchers have conflicting views with respect to the effects this protocol has on behavior. The inconsistent use of strict double blind procedures may be artificially inflating proportions sent in the trust game due to an underlying desire of subjects assigned to the role of sender to protect their reputation or impress the experimenter. The variable “Double Blind” is equal to 1 if the experimental protocol followed the strict double-blind procedure, and 0 if the procedure was not strictly double blind.

2.3.10. Student subjects

The subjects in the original BDM study were students. While students are a convenient and frequently employed sample for behavioral laboratory experiments, the external validity of using student subjects has been criticized because it is not a representative sample of the general population. Students are on average younger than random samples of the population and some research, has found that relatively older subjects exhibit less trust behavior in the trust game than student participants (e.g. Bellemare & Kröger, 2003; Fehr, Fischbacher, von Rosenbladt, Schupp, & Wagner, 2003). If the experimental subjects were students, then the variable “Student” takes on a value of 1, and 0 otherwise.

2.4. Description of variables used to predict amount returned

Trustworthiness is motivated by conditional other-regarding preferences (reciprocity), as well as unconditional other-regarding preferences such as altruism and kindness (Ashraf, Bohnet, & Piankov, 2006; Cox, 2004). We expect that while the amount sent by the counterpart will be a major predictor of the amount returned in the trust game, methodological variations to the trust game may also influence this behavior. In particular, stakes, whether the receiver is endowed, and the rate of return are methodological variables that are each expected to separately influence the perceived ‘wealth’ of the receiver. They are all factors that on average increase the receiver’s pot of funds received from the sender in the trust game. Research on philanthropic behavior has identified income as the most important predictor of giving behavior, finding that higher income households donate more (e.g. Yen, 2002). As such, we might expect ‘wealthier’ receivers to exhibit more kindness by giving more money to their counterparts.

Random payment, the use of the strategy method, playing both roles, and having a simulated confederate represent methodological variations that influence the level of responsibility trusted parties may feel toward their counterpart, and therefore affect the amount of money they send back. When subjects play both the sender and receiver roles, Burks et al. (2003) found that subjects passed less money to their counterparts. The strategy method forces subjects to imagine how they would respond to their counterpart’s behavior by asking them to respond to hypothetical actions, which again removes a part of the interpersonal dynamic and responsibility felt for the counterpart (Güth et al., 2001; Roth, 1995). Random payments also mean that the ‘kindness’ and reciprocity exhibited by the receiver may never be experienced or realized by the counterpart, which may also translate into lower felt responsibility to the counterpart. Finally, if subjects suspect the counterpart to be a simulated confederate rather than a real person, we also expect them to reciprocate less and to show less kindness.

Researchers have also found that older people are more generous than younger people and that giving increases with age (e.g. Midlarsky & Hannah, 1989; Nichols, 1992; Yen, 2002). Since students represent a subset of the population that is younger than average, we might expect students to give less money back to their counterparts than older, non-student populations. Fehr and List (2004) as well as Falk, Meier, and Zehnder (2011) find evidence that their student subjects exhibit less trustworthiness than their non-student subjects in experimental studies of trust behavior.

A double-blind protocol is expected to influence subject behavior in the same way that it influences trust behavior. By eliminating any possible reputation effect, we expect trusted parties to reciprocate less and therefore send less in the trust game when there is a double-blind protocol in place.

2.5. Geographic variables

Our dataset on trust contains 162 observation spanning 35 countries: 47 studies from North America, 64 from Europe, 23 from Asia, 15 from Sub-Saharan Africa, and 13 from Latin America. This substantial variation allows us to investigate how unobserved characteristics of these regions may impact the amount sent and returned in the trust game. Fehr (2009) argues that trust is endogenous to a region’s institutions. For example, it is possible that people trust less in regions with more invasive government regulations (Aghion, Algan, Cahuc, & Shleifer, 2010). Naef and Schupp (2008) argue that Americans display more trust than Germans. Yamagishi, Cook, and Watabe (1998) argue that Asians exhibit more altruism and, thus, we would expect them to be more trustworthy. We examine systematic variation across geographic regions in trust game behavior while controlling for variations in methodological variations.

3. Analysis and results

Our measure of behavioral trust is the amount of money passed by the sender divided by endowment. Our measure of trustworthiness is the amount returned by the receiver as a proportion of the amount available to return. When measured this way, both trust and trustworthiness are proportions, falling between zero and one. In order to avoid inefficient coeffi-

Table 1
Descriptive statistics.

Variable name	Obs.	Mean	Std. dev.	Min	Max
<i>Panel A: Sent fraction (trust)</i>					
pSent	161	0.50	0.12	0.22	0.89
Trust	161	0.02	0.54	−1.24	2.04
Sender endowment	161	18.99	45.15	0.00	238.10
Receiver endowed	161	0.54	0.50	0.00	1.00
Anonymous	161	0.86	0.35	0.00	1.00
Rate return	161	0.91	0.29	0.00	1.00
Double blind	161	0.10	0.30	0.00	1.00
Student	161	0.52	0.50	0.00	1.00
Strategy method	161	0.39	0.49	0.00	1.00
Both roles	161	0.20	0.40	0.00	1.00
Random payment	161	0.33	0.47	0.00	1.00
Real person	161	0.96	0.19	0.00	1.00
<i>Panel B: Proportion returned (trustworthiness)</i>					
pSent	137	0.50	0.12	0.22	0.89
pReturn	137	0.37	0.11	0.11	0.81
Trust	137	0.03	0.54	−1.24	2.04
Trustworthy	137	−0.56	0.52	−2.11	1.46
Sender endowment	137	20.46	47.33	0.18	238.10
Receiver endowed	137	0.54	0.50	0.00	1.00
Anonymous	137	0.84	0.37	0.00	1.00
Rate return	137	0.90	0.30	0.00	1.00
Double blind	137	0.11	0.31	0.00	1.00
Student	137	0.49	0.50	0.00	1.00
Strategy method	137	0.38	0.49	0.00	1.00
Both roles	137	0.20	0.40	0.00	1.00
Random payment	137	0.35	0.48	0.00	1.00

cient estimates and an inappropriately specified model using OLS, we apply the logit transformation to both trust and trustworthy in order to map their values from [0,1] to the real line.⁷

To make interpretation easier, Table 1 shows the descriptive statistics for trust and trustworthiness both with and without the logit transformation. We call the variables before transformation “pSent” and “pReturn”. After the transformation they are referred to as “Trust” and “Trustworthiness”. Fig. 1 provides histograms of the distributions of the untransformed variables pSent and pReturn. Standard assumptions on rationality and selfishness predict senders pass no money to receivers and subsequently no money will be returned. Despite this, replications of the trust game have consistently supported the finding that individuals are willing to send and return positive amounts. Furthermore, as Fig. 1 shows, the *amount* they have been willing to send has varied significantly. For example, one study (Masclot & Penard, 2011) found senders passing an average of 22% of their initial endowment, while another had senders passing an average of 89% of their endowment (Swope, Cadigan, Schmitt, & Shupp, 2008). The data presented in Fig. 1 are consistent with this wide range of findings. There is a great deal of variation in both our trust measure and our trustworthiness measure. The coefficients of variation for the untransformed variables are both around 0.30.

Table 2 provides descriptive statistics by region and Table 3 breaks down the data further by country. A cursory review of these tables suggests that there may indeed be systematic variation across geographic regions in trust and trustworthiness. Table 1, panel A shows descriptive statistics for the amount of money sent in the trust game as a proportion of the amount available (pSent). Panel B shows statistics for the proportion returned (pReturn). In particular, on average, subjects send less and return less in Africa than in other regions. Keeping in mind that these observations obviously do not control for any of the experimental protocols discussed above, they do suggest it may be interesting to probe further and investigate whether region dummies can explain trusting and trustworthy behavior.

To examine the relationship between experimental protocols and observed trust and trustworthy behavior, we estimate the following specification using ordinary least squares:

$$y_i = \alpha + B'X_i + \frac{\varepsilon_i}{w_i} \quad (1)$$

where y_i is either our logit transformed trust or trustworthy measure, X_i is a vector of methodological variables, and i represents a replication of the trust game.⁸ Hedges and Olkin (1985) recommend that estimates and test statistics based on

⁷ For a dependent variable, Y , the logit transformed variable is, $\hat{Y} = \ln \left(\frac{Y}{1-Y} \right)$.

⁸ As a robustness check, we also ran all specifications with the non-logit transformed pSent and pReturned measures as dependent variables using a Generalized Linear Model with a Bernoulli family function and a logistic linking function, as suggested by Papke and Wooldridge (1996) for proportional dependent variables. The results were substantially the same as our reported results.

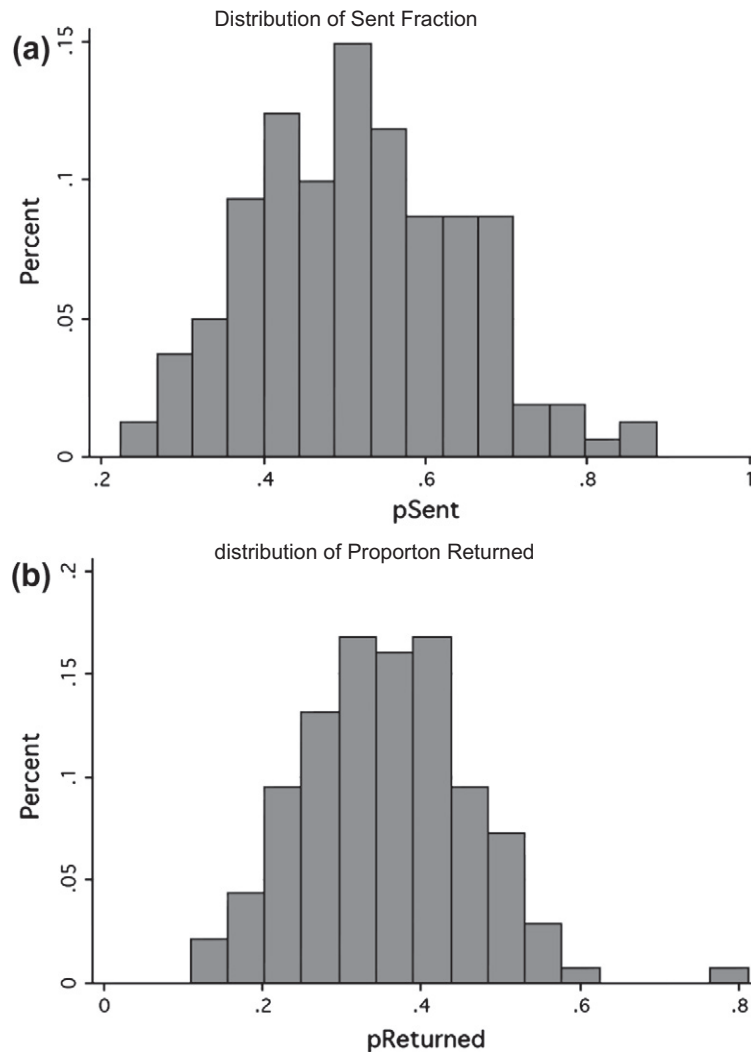


Fig. 1. Distribution of dependent variables. *Notes:* pSent and pReturn refer to the untransformed amount sent as a proportion of amount available by senders and the amount returned as a proportion of amount available by receivers respectively.

averaged data be calculated using weighted regression analysis in order to ensure that study results reflect the sample size in each study examined. As such, the error term, $\varepsilon_i \sim N(0, \sigma^2)$ is weighted by $\frac{1}{w_i}$, where $w_i = \frac{n_i}{\sum n_i}$ with n_i being the number of subjects in treatment i and n_i the sum of subjects across all treatments. In effect, the contribution of study i is given greater weight the larger the underlying sample from which mean trust and trustworthiness are calculated.

It is possible that our coefficient estimates are biased due to unobserved region specific factors that are correlated both with the independent variable and the error term. Thus, we also estimate the specification:

$$y_{ij} = \alpha + B'X_i + \gamma_j + \frac{\varepsilon_{ij}}{w_i} \quad (2)$$

where γ_j is a dummy for region j .

Finally, it is well known that cross section regressions are highly sensitive to the conditioning information set (Levine & Renelt, 1992). In general, coefficient estimates based on cross sectional data are sensitive to both specification and violations of the OLS normality assumption by the underlying data. Our data are no different. Indeed, the distribution of the trust and trustworthy data both fail skewness–kurtosis tests for non-normality at the 5% level. The implication is that there are outliers in our data which may significantly bias the parameter estimates. Thus, in specifications (3) and (6) we implement a robust estimation procedure in order to minimize the effect of outliers. The procedure is a form of iterated weighted least squares regression in which the weights are inversely related to the absolute residuals of an

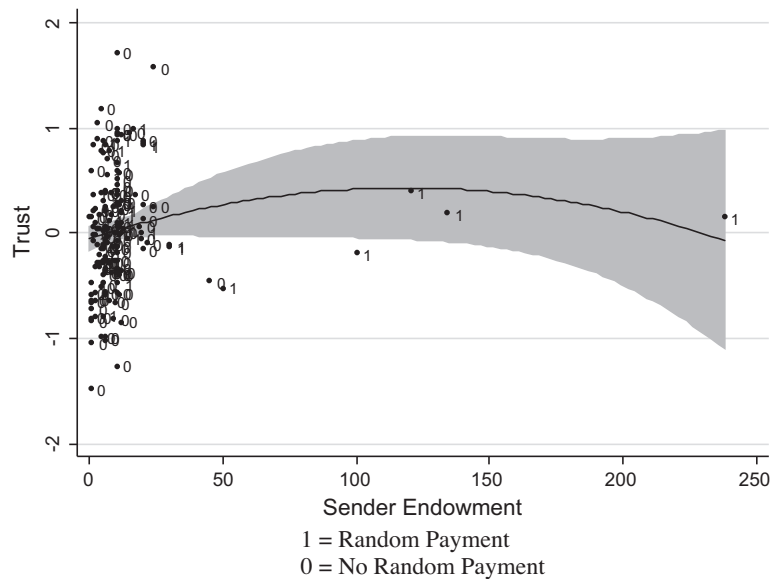


Fig. 2. The effect of sender endowment on trust. *Notes:* Shaded area represents 5% confidence interval. Figure created as follows: (A) Estimated specification (2) in Table 4 without sender endowment included as a regressor. (B) Calculated residuals, r . (C) Estimated the following regression: $r = a + b * (\text{sender endowment}) + c * (\text{sender endowment squared}) + \text{error}$, where a , b , and c are coefficients to be estimated. (D) Generated predictions for trust along with standard errors using estimated coefficients $\hat{a}$, $\hat{b}$, $\hat{c}$ from the previous regression. Line plotted is equal to: $\hat{a} + \hat{b} * (\text{sender endowment}) + \hat{c} * (\text{sender endowment squared})$. Points plotted above are equal to the residuals, r .

Table 2
Descriptive statistics by region.

Variable name	Obs.	Sum N	Mean	Std. dev.	Min	Max
<i>Panel A: Sent fraction (trust)</i>						
All regions	161	23,900	0.502	0.124	0.224	0.885
North America	46	4579	0.517	0.158	0.259	0.885
Europe	64	9030	0.537	0.121	0.224	0.783
Asia	23	3043	0.482	0.102	0.285	0.710
South America	13	4733	0.458	0.074	0.336	0.857
Africa	15	2515	0.456	0.133	0.300	0.750
<i>Panel B: Proportion returned (trustworthiness)</i>						
All regions	137	21,529	0.372	0.114	0.108	0.812
North America	41	4324	0.340	0.089	0.119	0.496
Europe	53	7596	0.382	0.094	0.108	0.542
Asia	15	2361	0.460	0.114	0.215	0.597
South America	13	4733	0.369	0.147	0.184	0.812
Africa	15	2515	0.319	0.106	0.180	0.514

observation. The iteration process terminates when the maximum change in residuals drops below a specified tolerance limit (Hamilton, 1991).⁹

Results for the regressions are reported in Table 4. Specifications (1), (2), and (3) use Trust as the dependent variable. Whether payment is random exhibits a robustly significant and negative effect on the amount sent. Playing against a real person has a significant, positive, impact on amount sent. Somewhat surprisingly, we do not find any effect of using a double blind protocol. With the exception of Sender Endowment, all of the independent variables are zero-one dummies, which makes their economic significance relatively easy to evaluate. According to specification (2), paying subjects randomly or having them play against a “fake” subject both reduce the amount sent (trust) by close to 60% of a standard deviation. These are large effects induced by protocols that are often considered innocuous.

We get mixed results concerning the effect of receiver endowment and whether the counterpart is anonymous. In specifications (1) and (2) the dummy variable on receiver endowment is negative, but not significant, while in specification (3) it is negative and highly significant. Specification (2) also indicates that the effect of an anonymous counterpart reduces amount sent, but this effect, while it remains negative, is no longer significant in specification (3).

⁹ This form of robust regression allows for neither sample weights of the type we used for averaged data, nor for the use of robust variance–covariance estimators such as Huber–White standard errors or Clustered standard errors. To check if this matters, we ran the OLS specifications without weights or robust standard errors, the results were very similar to those in specifications (1), (2), (4), and (5).

Table 3

Descriptive statistics by country.

Country	Number of studies	Total sample size	Average fraction sent	Average proportion returned
Argentina	3	678	0.43	0.40
Australia	2	196	0.51	0.32
Austria	6	508	0.62	0.38
Bangladesh	4	863	0.46	0.46
Brazil	2	138	0.71	0.45
Bulgaria	2	62	0.57	0.39
Cameroon	2	320	0.70	0.47
Canada	5	432	0.60	0.31
China	5	1036	0.48	0.55
Colombia	2	722	0.37	0.23
Costa Rica	1	425	0.46	0.26
France	9	1008	0.43	0.33
Germany	15	1315	0.51	0.44
Honduras	1	758	0.49	0.42
Hungary	1	74	0.51	0.40
India	1	92	0.49	0.29
Israel	2	535	0.59	0.45
Italy	8	763	0.43	0.31
Japan	2	78	0.58	0.32
Kenya	4	646	0.38	0.32
Netherlands	6	751	0.46	0.33
New Zealand	2	123	0.44	0.22
Paraguay	1	188	0.47	0.43
Peru	2	1245	0.48	0.46
Russia	2	758	0.49	0.37
South Africa	4	775	0.44	0.24
South Korea	1	52	0.67	0.29
Sweden	4	941	0.74	0.37
Switzerland	1	986	0.66	0.53
Tanzania	2	310	0.54	0.40
Uganda	2	246	0.45	0.33
United Kingdom	5	274	0.54	0.28
United States	46	4552	0.51	0.34
Uruguay	1	579	0.45	0.29
Vietnam	2	194	0.33	n/a

We also find a robustly significant and negative coefficient on the Africa region dummy. Both specifications (2) and (3) indicate that subjects pass their counterparts significantly less in trust games conducted in Africa than in North America (North America is the omitted category). The effects for the other geographic regions are more mixed. In Specification (2) the dummy variable on Asia is negative and significant at the 5% level and South America is negative and significant at the 10% level. However, when we control for outliers among the independent variables in Specification (3), Asia and South America are no longer significant, but Europe is at the 10% level.

Specifications (4), (5), and (6) have trustworthiness as their dependent variable. They indicate a robust and statistically significant inverse relationship between rate of return and student. The coefficient on rate of return indicates that increasing the multiplier on amount sent by the “trustor” from two to three *decreases* the amount of money returned by the receiver by a little over one standard deviation. This means that, at least in part, second movers take into account the “size of the pie” by adjusting downwards what they return to the senders. This is in contrast to the coefficient on Trust, which indicates a consistent tendency to return more money the more is sent (a one standard deviation increase in trust, leads to about a 40% increase in trustworthiness). The coefficient on Student is also significant across all three specifications (4), (5) and (6). According to specification (5), student populations return, on average, 80% less of a standard deviation than adult subject pools.

We also find evidence that having players engage in both roles (sender and receiver) in different rounds of the trust game has a negative impact on trustworthiness. This result, however, only shows in specifications (5) and (6) once region dummies are included. The geographic region coefficients in specifications (5) and (6) indicate that players in Sub-Saharan Africa are also send less back than the reference group in North America, though the coefficient in (6) is not significant at conventional levels (p -value = 0.14). The coefficient in (5) implies that participants in Africa return fully half a standard deviation less than participants from the United States and Canada. We also find evidence in specification (5) that Asians send back more than North Americans. But these results do not stand up to the robust estimator in (6).

Our results across all specifications suggest that stakes do not influence behavior in the trust game. None of the coefficients for the sender endowment are significant and they are all close to zero.

Table 4
Regression results.

Variable name	Sent fraction (trust)			Proportion returned (trustworthiness)		
	(1) OLS	(2) OLS	(3) Robust	(4) OLS	(5) OLS	(6) Robust
Sender endowment	0.0011 (0.0011)	0.0012 (0.0011)	−0.0009 (0.0020)	0.0000 (0.0007)	−0.0004 (0.0008)	−0.0007 (0.0015)
Receiver endowed	−0.1073 (0.1331)	−0.1050 (0.1161)	−0.2788*** (0.1021)	−0.0211 (0.1248)	−0.0148 (0.1158)	0.0001 (0.0889)
Anonymous	−0.2889 (0.1993)	−0.3722* (0.1909)	−0.3088 (0.2051)	0.4608 (0.2920)	0.5075 (0.3157)	0.3813* (0.1655)
Rate return	0.1409 (0.2117)	0.0823 (0.1882)	−0.0154 (0.1881)	−0.6083*** (0.1496)	−0.5478*** (0.1334)	−0.5929*** (0.1416)
Double blind	0.1306 (0.1433)	0.1286 (0.1324)	0.0965 (0.1188)	0.0715 (0.1203)	−0.0323 (0.1043)	−0.0315 (0.0911)
Student	0.0931 (0.1305)	−0.1276 (0.1518)	0.2145 (0.1315)	−0.2761** (0.1277)	−0.2690** (0.1087)	−0.2805*** (0.1055)
Both roles	0.2126 (0.2082)	0.2058 (0.1822)	−0.0841 (0.1243)	−0.1923 (0.1508)	−0.2364* (0.1317)	−0.2842*** (0.0986)
Random payment	−0.6080*** (0.1868)	−0.6502*** (0.1802)	−0.2803** (0.1325)	0.0598 (0.1915)	0.0673 (0.1723)	−0.0082 (0.1099)
Strategy method	0.1747 (0.1377)	0.1070 (0.1143)	−0.0392 (0.1050)	0.0335 (0.1282)	0.0162 (0.1214)	0.1132 (0.0886)
Real person	0.3413* (0.1758)	0.3768* (0.1810)	0.4046* (0.2185)			
Trust				0.3163*** (0.0956)	0.2920*** (0.1016)	0.2275*** (0.0681)
Europe		−0.1097 (0.1373)	−0.2110* (0.1091)		0.1218 (0.1312)	0.0351 (0.0877)
Asia		−0.4959** (0.1934)	−0.1878 (0.1552)		0.2724** (0.1324)	0.0582 (0.1373)
South America		−0.3957* (0.2037)	−0.1864 (0.2217)		0.0713 (0.1675)	−0.0824 (0.1681)
Africa		−0.5566** (0.2200)	−0.3171* (0.1919)		−0.2670* (0.1546)	−0.2195 (0.1474)
Observations	161	161	161	137	137	137
F-stat	3.26	3.12	2.04	7.72	10.27	4.07
R-square	0.186	0.274	0.163	0.368	0.434	0.319

Notes: Huber–white robust standard errors are reported in parentheses for specification (1), (2), (4), and (5). The dependent variables are transformed using the logit transformation as described in the text. “Real Person” is not included in the trustworthy regressions due to insufficient observations. “Robust” regressions are estimated as explained in the text. The robust regression technique does not allow for observation weights or robust standard errors.

*** Coefficient is significant at the 1% level.

** Coefficient at the 5% level.

* Coefficient at the 10% level.

4. Discussion

Our results have a variety of important implications for the potential impact of methodological and geographic variations on trust game behavior. We find that when only one or ‘some’ out of a group of subjects are paid the actual stakes described in the experiment, participants pass significantly less money to their counterpart. Random payment schemes appear to be motivating participants to behave differently, possibly due to the added risk associated with the final payment (Bottom, 1998).

We also find that playing with a real person is associated with significantly more sent. While researchers sometimes employ simulated confederates to play the role of the receiver in the trust game, these studies rarely use a manipulation check to confirm that the experimenter’s attempts to deceive the participants have been successful. Our findings suggest that participants in such trust game experiments may in fact not all believe, as the experimenters wish them to, that they are playing with real counterparts. This could be due to flaws in the experimental procedures employed or even early participants informing later participants of the deception. Certainly if trust game subjects do not believe they are playing with a real counterpart, we expect them to behave differently (e.g. Bottom et al., 2006; Sanfey et al., 2003). This also raises the question of what the trust game is measuring if subjects do not think they are playing with real counterparts. Researchers who wish to either have subjects play against a computer, or, want to exogenously manipulate the amount sent back, may benefit from employing a manipulation checks to verify that their subjects really do believe they are playing against another human being.

The large and negative coefficient on the Africa dummy is one of our more robust results. Given that only 30% of the African experiments used student subjects (compared to 50% for the entire data set) it seems that these results are not being driven by a privileged and non-representative subset of the population. Furthermore, there is support for finding low levels of trust in Africa from the broader social science literature. Nunn and Wantchekon (2009), for example, argue that there should be lower levels of trust in regions that suffered from a more active slave trade during the Colonial Period. To the extent that people rely on heuristic decision making strategies, or, rules-of-thumb that are embodied in culture (Boyd &

Richerson, 1995), individuals from slave trading regions would have adopted a strategy of “mistrusting” rather than “trusting” outsiders. Nunn and Wantchekon find significant empirical support for this hypothesis.

We also find some support that endowing the receiver with funds and ensuring anonymity are associated with lower amounts sent in the trust game. While these findings are not as robust, they do support the underlying concerns that motivated the original BDM experimental design. Endowing both parties controls for the possibility that senders pass money simply out of concern for equity, and anonymous exchange controls for the impact of past and future exchange relations between parties outside the lab.

Given the emphasis placed on the double-blind procedure by BDM and other researchers (e.g. Hoffman et al., 1994), we were surprised to find no evidence that implementing a double blind procedure affects the amount sent or returned. Our results imply that experimenters can avoid excess expense and complexity in their experiments by ignoring double blind protocols.

Another surprising null result in Table 4 is that there is no effect of stakes on experimental trust. A closer look at the data, however, reveals that while we had significant variation on the sender's endowment, ranging from nothing (Piff, Kraus, Cote, Cheng, & Keltner, 2010) to \$238 (converted to USD, Migheli, 2008), only eight observations endowed subjects with more than \$25, and of those eight only one study (Johansson-Stenman et al., 2008) guaranteed subjects their trust game earnings (not random payment).

Fig. 2 shows the effect of sender endowment on trust, holding constant the other control variables (i.e. the partial effect of endowment on trust). The scatter plot shows actual observations on sender endowment against the residuals from estimating specification (2) in Table 4 without sender endowment as a control. We also included the quadratic fitted regression line of endowment on trust along with a 95% confidence interval. As with the regression results in Table 4, there is no obvious relationship between stakes and trust. However, we also indicate in Fig. 2 whether or not each experimental observation pays subjects randomly. A “1” indicates random payment was used, a “0” that it was not. There is only one observation with ‘very high’ stakes for which subjects were actually endowed and paid the amount ‘earned’ by playing the trust game, rather than told that one (or a few) would be randomly selected and awarded the amount earned in the study.

That one observation is Johansson-Stenman et al. (2008). They find that higher stakes lead to lower amounts sent in the trust game in Bangladesh. Unfortunately, we do not have enough high stakes observations with non-random payment to make any claims about what happens to trust or trustworthiness as the endowment gets very large. This is an area which deserves more investigation.

One of the strongest results from the regression on trustworthiness is that when the experimenter only doubles the amount sent, then the proportion returned by the receiver declines less than proportionately. This finding is a puzzle, especially considering that some claim that trust increases when the rate of the return goes up. For example, Lenton and Mosley (2009) argue that increasing the rate of return in the trust game reduces the senders “risk efficacy”, or the amount of resources at the senders disposal to deal with a potential breach of their trust. Our finding suggests that, while the average amount returned may increase as the rate of return is increased, on the margin, reciprocity is declining. This, in turn, implies that there should be diminishing returns to trying to increase trust by raising the rate of return as suggested by Lenton and Mosley (2009). Further study may help disentangle why, exactly, receivers respond to an increase in the rate return in a disproportionate manner.

Our regressions on trustworthiness also indicate that student subjects playing the trust game send back less money than non-student subjects. Since students represent a younger subset of the average population, this finding may be attributable to the fact that younger people give less and are less generous than older people (e.g. Midlarsky & Hannah, 1989; Nichols, 1992; Yen, 2002).

Although less robust, we also find evidence consistent with Burks et al. (2003) that participants asked to play both the role of sender and receiver in the trust game tend to send back less than those who only play the role of receiver.

5. Conclusion

Given the increasing importance of experimental methods in economics, we must continue to test the boundary conditions and generalizability of the results generated by these powerful techniques. Meta-analyses are a useful tool to uncover what matters and what does not in experimental technique. That is not to say that the results we report necessarily invalidate any experiments, or even any specific experimental protocol. As pointed out by Falk and Heckman (2009), well designed lab studies typically have both control and treatment groups and draw conclusions based on average treatment effects which account for biases common to each group. However, it is not clear under what conditions the biases we identify may interact with a treatment so as to result in incorrect estimates of a treatment effect.¹⁰ Indeed, given the large variety of experimental treatments tested in the lab today, it would be very difficult for a study such as this one to identify any specific interaction effect.

Our findings suggest that relatively minor variations in protocol can produce substantial shifts in measured trust behavior. Subjects send less in the trust game if they are paid randomly, and if the counterpart is a simulated confederate. Subjects return less when the rate of return is lower and if they are drawn from a student population.

Finally, the negative relationship between the Sub-Saharan Africa indicator variable and amount sent is one of our most robust results and highlights the importance of local differences in “other-regarding” behavior. Identifying the factors which contribute to these differences is an important area for future research.

¹⁰ For example, it's possible that the ability of a subject to “feel” a treatment may be influenced by the overall level of initial trust in a manner analogous to Fechner (1860); see also Bardsley et al. (2010, chap. 7). This would, in turn, impact the likelihood of a researcher identifying a significant treatment effect.

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Appendix A. Papers included in meta-analysis

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Appendix B. Data

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Ackert, Church et al. (2011)	USA	19	10.00	64	156	6		✓	✓	✓					✓
Ackert, Church et al. (2011)	USA	21	10.00	64	98	3		✓	✓	✓					✓
Altmann, Dohmen et al. (2008)	Germany	240	2.35	48	163	3		✓	✓	✓	✓	✓	✓	✓	✓
Anderson and Dickinson (2010)	England	16	7.62	70	114	3			✓		✓	✓	✓	✓	✓
Anderson, Mellor et al. (2006)	USA	48	10.00	50	101	3			✓	✓					✓
Apicella, Cesarini et al. (2010)	Sweden	684	5.38	78	112	3		✓	✓		✓	✓		✓	✓
Ashraf, Bohnet et al. (2006)	Russia	118	134.17	49	88	3			✓	✓	✓		✓	✓	✓
Ashraf, Bohnet et al. (2006)	South Africa	128	120.37	43	81	3			✓	✓	✓		✓	✓	✓
Bahry, Whitt et al. (2003)	Russia	640	10.73	50	116	3			✓					✓	✓
Barclay (2004)	Canada	40	0.54	39		3			✓	✓	✓	✓	✓	✓	✓
Bauernschuster and Niels (2010)	Germany	26	4.66	44	113	3		✓	✓	✓				✓	✓
Becchetti and Degli Antoni (2007)	Italy	256	5.95	45	100	3		✓	✓	✓					✓
Becchetti and Conzo (2009)	Argentina	152	13.82	35	244	3		✓	✓		✓		✓		✓
Bellemare and Kroger (2007)	Netherlands	100	0.60	29	28	2	✓	✓	✓	✓				✓	
Ben-Ner and Halldorsson (2010)	USA	204	1.40	55	120	3		✓	✓	✓					✓
Berg, Dickhaut et al. (1995)	USA	64	10.00	52	90	3	✓	✓	✓	✓					✓
Boero, Bravo et al. (2009)	Italy	120	0.18	39	99	3		✓	✓	✓				✓	✓
Bohnet and Meier (2005)	USA	64	10.00	28	110	3		✓	✓	✓	✓			✓	✓
Bonein and Serra (2009)	France	136	4.76	44	74	3		✓	✓	✓		✓	✓	✓	✓
Bornstein, Sutter et al. (2010)	Austria	64	8.17	66	75	3			✓	✓					✓
Bouma, Bulte et al. (2008)	India	92	3.51	49	87	3	✓		✓			✓		✓	✓
Bourgeois-Gironde and Corcos (2011)	France	186	11.40	49	124	3			✓	✓			✓	✓	✓

Appendix B (continued)

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Buchan, Croson et al. (2002)	USA	22	10.00	65	85	3	✓	✓	✓	✓					✓
Buchan, Croson et al. (2008)	USA	78	10.00	68	85	3	✓	✓	✓	✓					✓
Buchan, Croson et al. (2002)	East Asia	66	10.00	69	96	3	✓	✓	✓	✓					✓
Buchan, Johnson et al. (2006)	China	50	2.92	71	103	3	✓	✓	✓	✓					✓
Buchan, Johnson et al. (2006)	USA	44	10.00	65	85	3	✓	✓	✓	✓					✓
Buchan, Johnson et al. (2006)	Korea/S. Korea for CPI	52	1.30	67	88	3	✓	✓	✓	✓					✓
Buchan, Johnson et al. (2006)	Japan	44	12.01	69	95	3	✓	✓	✓	✓					✓
Burks, Carpenter et al. (2003)	USA	44	10.00	65	131	3	✓	✓	✓	✓					✓
Burns (2006)	South Africa	308	8.31	33	69	3		✓	✓	✓					✓
Butler (2010)	USA	144	7.00	51	77	3			✓	✓		✓	✓	✓	✓
Butler, Paola et al. (2009)	Italy	124	12.50	50	92	3			✓	✓	✓	✓	✓	✓	✓
Cardenas (2003)	Colombia	160	8.00	50	118	3		✓	✓	✓					✓
Cárdenas, Chong et al. (2008)	Columbia	562	10.51	34	55	3		✓			✓		✓	✓	✓
Cárdenas, Chong et al. (2008)	Uruguay	579	9.39	45	86	3		✓			✓		✓	✓	✓
Cárdenas, Chong et al. (2008)	Argentina	496	19.33	45	79	3		✓			✓		✓	✓	✓
Cárdenas, Chong et al. (2008)	Costa Rica	425	10.71	46	78	3		✓			✓		✓	✓	✓
Cárdenas, Chong et al. (2008)	Peru	541	11.71	50	98	3		✓			✓		✓	✓	✓
Carter and Castillo (2011)	South Africa	283	2.77	53	72	3			✓			✓		✓	✓
Castillo and Carter (2003)	Honduras	758	6.83	49	126	3			✓					✓	✓
Charness, Cobo-Reyes et al. (2008)	USA	96	5.00	37	125	3	✓	✓	✓	✓			✓	✓	✓
Chaudhuri and Gangadharan (2007)	Australia	100	7.10	43	95	3		✓	✓	✓	✓	✓		✓	✓

Appendix B (continued)

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Chaudhuri and Sbai (2011)	New Zealand	82	0.64	33	65	3		✓	✓	✓		✓			✓
Chaudhuri, Sopher et al. (2002)	USA	90	5.00	65	108	3			✓	✓			✓	✓	✓
Chesney, Chuah et al. (2009)	UK	96	6.25	56	81	3			✓	✓					✓
Chuah (2010)	England	96	6.23	56	65	3			✓	✓			✓	✓	✓
Cochard, Van et al. (2004)	France	40	9.28	40	115	3	✓	✓	✓	✓					✓
Coricelli (2002)	UK	30	6.38	38	125	3	✓	✓	✓	✓	✓		✓		✓
Courtiol, Raymond et al. (2009)	France	417	3.25	44		3		✓	✓	✓					
Cox (2004)	USA	64	10.00	60	83	3	✓	✓	✓	✓				✓	✓
Cox (2009)	USA	60	10.00	60	120	3	✓	✓	✓	✓			✓	✓	✓
Cox (2009)	USA	112	100.00	42	70	3			✓	✓	✓		✓	✓	✓
Cox and Hall (2010)	USA	64	10.00	56	124	3	✓	✓	✓	✓	✓			✓	✓
Cox, Ostrom et al. (2009)	USA	68	10.00	57	118	3	✓	✓	✓	✓				✓	✓
Cronk (2007)	Kenya	50	3.39	38	98	3		✓	✓	✓				✓	✓
Danielson and Holm (2007)	Tanzania	110	17.01	56	138	3		✓	✓		✓				✓
Di Cag and Sciubba (2010)	Italy	36	11.90	58	32	3		✓	✓	✓		✓		✓	✓
Dubois and Willinger (2007)	France	36	0.32	38	59	3		✓	✓	✓		✓			✓
Dubois and Willinger (2007)	France	36	0.32	50	75	3		✓		✓		✓			✓
Eckel and Petrie (2011)	USA	32	10.00	34	54	3		✓	✓	✓	✓		✓		✓
Ensminger (2000)	Kenya	262	3.91	44	54	3			✓	✓					✓
Etang, Fielding et al. (2010)	Cameroon	40	3.98	75	123	3			✓						✓
Etang, Fielding et al. (2011)	Cameroon	280	3.15	69	143	3			✓					✓	✓
Evans and Reville (2008)	USA	26	10.00	39		3			✓	✓					
Falk and Zehnder (2007)	Switzerland	986	11.77	66	160	3	✓	✓		✓				✓	
Farina, O'Higgins et al. (2008)	Italy	64	11.90	36	150	3	✓	✓	✓	✓				✓	✓
Fehr and Rockenbach (2003)	Germany	48	3.18	58	120	3		✓	✓	✓				✓	✓
Fershtman and Gneezy (2001)	Israel	483	5.28	58	135	3				✓				✓	✓

Appendix B (continued)

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Fiedler and Haruvy (2009)	Germany	40	2.91	51	104	3		✓	✓	✓					✓
Füllbrunn, Richwien et al. (2011)	Germany	81	4.66	56	96	3	✓	✓	✓	✓	✓				✓
Garbari and Slonim (2009)	USA	441	30.00	50	118	3			✓		✓	✓	✓	✓	✓
Glaeser, Laibson et al. (2000)	USA	196	15.00	83	99	2				✓					✓
Gneezy, Güth et al. (2000)	Netherlands	32	5.51	56	91	2		✓	✓					✓	
Gong (2009)	China	600	5.26	42	119	2		✓	✓						✓
Greig and Bohnet (2008)	Kenya	270	1.72	30	82	2		✓	✓						✓
Güth, Levati et al. (2008a)	Germany	28	11.66	44	99	3		✓	✓	✓				✓	✓
Güth, Levati et al. (2008b)	Germany	32	5.88	44	98	3		✓	✓	✓					✓
Haile, Sadrieh et al. (2008)	South Africa	56	5.41	55	84	3		✓	✓	✓	✓	✓			✓
Harbring (2010)	Germany	180	0.58	43		3			✓	✓	✓	✓		✓	✓
Hargreaves-Heap, Tan et al. (2009)	Europe	144	1.44	55	87	3		✓	✓	✓				✓	✓
Hargreaves-Heap and Zizzo (2009)	UK	36	1.50	52	91	3			✓	✓		✓	✓	✓	✓
Hargreaves-Heap, Verschoor et al. (2009)	Uganda	60	7.49	33	105	3			✓						✓
Hennig-Schmidt, Selten et al. (2009)	MiddleEast	60	5.00	53		3		✓	✓	✓	✓				✓
Hennig-Schmidt, Selten et al. (2009)	Germany	30	5.00	44		3		✓	✓	✓	✓				✓
Holm and Danielson (2005)	Sweden	110	21.39	51	105	3		✓	✓	✓				✓	✓
Holm and Danielson (2005)	Tanzania	200	10.21	53	111	3		✓	✓	✓				✓	✓
Holm and Nystedt (2005)	Sweden	81	10.71	62	99	3			✓		✓				✓
Houser, Schunk et al. (2010)	Germany	48	5.77	44		3		✓	✓	✓				✓	✓
Huang and Murnighan (2010)	USA	36	5.00	45		3			✓	✓				✓	
Innocenti and Paziienza (2006)	Italy	69	5.77	42	62	3	✓	✓	✓	✓				✓	✓
Innocenti and Paziienza (2008)	Italy	62	5.73	34	66	3		✓	✓	✓		✓			✓

Appendix B (continued)

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Innocenti and Pazienza (2008)	Italy	32	5.73	36	59	3	✓	✓	✓	✓		✓			✓
Jack (2009)	Kenya	64	3.44	42	154	3		✓	✓		✓			✓	✓
Jamison, Karlan et al. (2008)	USA	132	20.00	64	131	3			✓	✓			✓	✓	✓
Ji and Guillen (2010)	Australia	96	20.23	59	96	3			✓	✓	✓	✓	✓	✓	✓
Johansson-Stenman, Mahmud et al. (2005)	Bangladesh	128	1.80	55	138	3		✓						✓	
Johansson-Stenman, Mahmud et al. (2005)	Bangladesh	124	44.94	38	114	3		✓						✓	
Johansson-Stenman, Mahmud et al. (2005)	Bangladesh	118	8.99	46	138	3		✓						✓	
Johansson-Stenman, Mahmud et al. (2009)	Bangladesh	493	9.45	46	145	3	✓	✓						✓	
Kamas and Preston (2009)	USA	226	10.00	51	92	3			✓	✓		✓	✓	✓	✓
Kanagaretnam, Mestelman et al. (2009)	Canada	60	0.81	59	78	3		✓	✓	✓					✓
Karlan (2005)	Peru	704	2.04	46	111	2		✓	✓						✓
Kausel (2010)	USA	132	20.00	68		3			✓	✓				✓	✓
Keck and Karelaia (2010)	France	33	4.35	46	147	3		✓	✓	✓				✓	✓
Keser (2002)	Canada	80	0.57	37	96	3		✓	✓	✓				✓	✓
Koford (2003)	Bulgaria	30	19.12	55	123	3	✓	✓	✓	✓				✓	✓
Koford (2003)	Bulgaria	32	18.69	58	112	3	✓	✓	✓	✓				✓	✓
Kovács and Willinger (2010)	Hungary	74	11.94	51	120	3		✓	✓	✓	✓	✓	✓		✓
Kurzban, Rigdon et al. (2008)	USA	24	2.40	39	147	3	✓	✓	✓	✓				✓	✓
Kurzban, Rigdon et al. (2008)	USA	36	2.40	50	104	3	✓	✓	✓	✓				✓	✓
Lazzarini, Madalozzo et al. (2005)	Brazil	69	24.12	86	100	2				✓					✓
Lazzarini, Madalozzo et al. (2005)	Brazil	69	24.12	56	80	2	✓		✓	✓					✓
Lönnqvist, Verkasalo et al. (2010)	Germany	232	6.00	47		3		✓	✓	✓	✓			✓	✓

Appendix B (continued)

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Lount (2010)	USA	45	10.00	69		3		✓	✓	✓					
Lount Jr. and Murnighan (2005)	USA	108	10.00	62		3		✓	✓	✓			✓		✓
Mak and Zwick (2009)	China	70	8.79	55		3			✓	✓	✓			✓	✓
Malhotra (2005)	USA	38	10.00	49	112	3			✓	✓				✓	✓
Masclet and Penard (2011)	France	64	0.36	22	65	3		✓	✓	✓	✓			✓	✓
Migheli (2008)	Europe	851	4.76	46	103	3			✓	✓			✓		✓
Mosley and Verschoor (2003)	Uganda	186	6.89	49	99	3			✓						✓
Netzer and Sutter (2009)	Austria	36	0.93	44		3		✓	✓	✓	✓	✓	✓	✓	✓
Netzer and Sutter (2009)	Japan	34	1.03	45		3		✓	✓	✓	✓	✓	✓	✓	✓
Ortmann, Fitzgerald et al. (2000)	USA	32	10.00	44	51	3	✓	✓	✓	✓					✓
Ortmann, Fitzgerald et al. (2000)	USA	24	10.00	58	125	3	✓	✓	✓	✓	✓				✓
Piff, Kraus et al. (2010)	USA	155	0.00	43		3			✓						
Rosenboim and Shavit (2010)	Israel	52	5.57	67		3			✓	✓					
Sapienza, Toldra et al. (2007)	USA	502	50.00	38	96	3			✓	✓	✓	✓	✓	✓	✓
Schechter (2007)	Paraguay	188	5.29	47	130	3			✓		✓		✓	✓	✓
Schotter and Sopher (2006)	USA	486	5.00	26	67	3			✓	✓			✓	✓	✓
Schwieren and Sutter (2003)	Austria	58	5.65	62	90	3			✓	✓					✓
Schwieren and Sutter (2003)	Netherlands	60	10.79	70	133	3		✓	✓					✓	
Servátka, Tucker et al. (2008)	New Zealand	41	6.49	66		3			✓	✓	✓				✓
Simpson and Eriksson (2009)	USA	24	10.00		52	3			✓	✓			✓		
Simpson, McGrimmon et al. (2007)	United States	147	6.00	59	114	3			✓	✓	✓				✓
Slonim and Guillen (2010)	USA	56	30.00	44		3				✓	✓	✓	✓	✓	✓
Solnick (2007)	USA	30	10.00	69	63	3		✓	✓	✓		✓	✓	✓	✓
Solnick (2007)	USA	30	10.00	62	137	3		✓	✓	✓				✓	✓
Solnick (2007)	USA	30	10.00	69	137	3		✓	✓	✓	✓			✓	✓
Solnick (2007)	USA	30	10.00	76	142	3		✓	✓	✓				✓	✓

Appendix B (continued)

Paper	Country	Nr. of subjects	Sender end in USD	Av % sent	Av % ret	Rate of ret	Double blind	Receiver endowed	Anony-mous	Student	Strategy method	Play both roles	Payment random	Play additional games	Real counter-part
Song (2007)	Canada	144	4.07	67	139	3	✓	✓	✓	✓				✓	✓
Song (2008)	Canada	108	4.15	75	36	3		✓	✓	✓	✓			✓	✓
Sutter and Kocher (2007)	Austria	220	5.65	66	108	3			✓	✓					✓
Sutter and Kocher (2007)	Austria	68	5.65	54	161	3			✓						✓
Sutter and Kocher (2007)	Austria	62	5.65	66	137	3			✓	✓					✓
Swope, Cadigan et al. (2008)	USA	94	10.00	89	123	3			✓	✓					✓
Tan and Vogel (2008)	Germany	48	5.58	35		3			✓	✓	✓	✓	✓		✓
Tanaka, Camerer et al. (2010)	Vietnam	110	4.24	29		3			✓		✓				✓
Tanaka, Camerer et al. (2010)	Vietnam	84	4.24	39		3			✓		✓				✓
Tanis and Postmes (2005)	Netherlands	139	8.80	28		3		✓	✓						
Tu and Bulte (2010)	China	286	2.89	55	147	3			✓			✓			✓
Vollan (2007)	Africa	218	1.99	30	68	3		✓	✓						✓
Vyrastekova and Onderstal (2005)	Netherlands	248	2.75	53	105	3	✓	✓	✓	✓	✓		✓	✓	
Vyrastekova, Onderstal et al. (2010)	Netherlands	172	11.66	51	108	3		✓	✓	✓	✓	✓	✓	✓	
Walkowitz, Oberhammer et al. (2006)	Argentina	30	20.00	57	103	2			✓	✓	✓	✓			✓
Walkowitz, Oberhammer et al. (2006)	Germany	30	20.00	58	68	2			✓	✓	✓	✓			✓
Walkowitz, Oberhammer et al. (2006)	China	30	20.00	58	103	2			✓	✓	✓	✓			✓
Wibral (2011)	Germany	192	0.41	68		3		✓	✓	✓				✓	✓
Willinger, Keser et al. (2003)	France	60	10.88	42	117	3	✓	✓	✓	✓					✓
Willinger, Keser et al. (2003)	Germany	60	10.47	66	130	3	✓	✓	✓	✓					✓
Zak, Kurzban et al. (2005)	USA	96	10.00	55	126	3		✓	✓	✓					✓
Zethraeus, Kocoska-Maras et al. (2009)	Sweden	66	16.19	77	113	3	✓		✓		✓	✓	✓	✓	✓

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