

MEASURING TRUST*

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We combine two experiments and a survey to measure trust and trustworthiness—two key components of social capital. Standard attitudinal survey questions about *trust* predict *trustworthy* behavior in our experiments much better than they predict trusting behavior. Trusting behavior in the experiments is predicted by past trusting behavior outside of the experiments. When individuals are closer socially, both trust and trustworthiness rise. Trustworthiness declines when partners are of different races or nationalities. High status individuals are able to elicit more trustworthiness in others.

I. INTRODUCTION

A growing body of research suggests that “social capital” influences a wide range of significant economic and political phenomena. For example, Arrow {1972} and Fukuyama {1995} believe that the level of trust in a society strongly predicts its economic success. Putnam {1993} uses Italian cross-regional data to show that local governments are more efficient where there is greater civic engagement. In recent years, economists have tried to identify the impact of social capital by using attitudinal measures of trust from survey questionnaires. Knack and Keefer {1997}, for example, show that an increase of one standard deviation in country-level trust predicts an increase in economic growth of more than one-half of a standard deviation. La Porta, Lopez-de-Silanes, Shleifer, and Vishny {1997} find that a standard deviation increase in trust increases judicial efficiency by 0.7 of a standard deviation and reduces government corruption by 0.3 of a standard deviation.¹

The great lacuna in this research agenda is the measurement of trust. Much of the social capital research relies upon attitudinal survey questions from the General Social Survey (GSS) such as

* The MacArthur Foundation has generously funded this research which is part of the MacArthur Foundation Network on Norms and Preferences. Glaeser and Laibson also thank the National Science Foundation, the Sloan Foundation, and the Olin Foundation. David Cutler, Lawrence Katz, George Loewenstein, Andrei Shleifer, and Steven Tadelis provided helpful comments. Beth Bellman, Meghana Bhatt, Elizabeth Dunn, Elizabeth Kelsing, Lars Nesheim, Joseph Robbins, and Stephen Weinberg provided excellent research assistance.

1. See also Jacobs {1961}, Loury {1977}, and Coleman {1990} for additional claims about the importance of social capital.

“Generally speaking, would you say that most people can be trusted or that you can’t be too careful in dealing with people?” While these survey questions are interesting, they are also vague, abstract, and hard to interpret. Putnam (1995), for example, laments this problem: “since trust is so central to the theory of social capital, it would be desirable to have strong behavioral indicators of trends in social trust or misanthropy. I have discovered no such behavioral measures.”

In this paper we measure trust and trustworthiness by conducting experiments with monetary rewards. Because we measure subjects’ attitudes, background characteristics, and social connectedness, we can identify individual and situational correlates of trust. For example, we test whether the standard attitudinal trust questions predict actual trusting behavior with real money. The primary methodological point of this paper is that experiments can be integrated with surveys to measure individual-level variation in traditionally hard-to-measure characteristics such as trust and trustworthiness.

We first ask survey questions of a sample of 258 Harvard undergraduates. Three to four weeks later, a subgroup of 196 undergraduates plays two experimental trust games. In the first game, subjects are paired and meet their partner. They are then separated, and one member of the pair (the sender) has the opportunity to send between 0 and 15 dollars to his or her partner (the recipient).² The experimenter doubles each dollar that is sent. After the second player receives the transfer (i.e., twice the amount sent), he or she may return money back to the first player.³ This game (based on Berg, Dickhaut, and McCabe (1995)) is similar to many economically relevant settings such as investment with imperfect contracts or production of a public good. We think of the amount sent by the “sender” as a natural measure of trust. The sender trusts the “recipient” to return a fair share of the amount the recipient receives. Similarly, controlling for the amount sent, the amount returned is a measure of trustworthiness.

In our second game, subjects report valuations for a series of “envelope drops.” Subjects are told that an envelope, addressed to the subject and containing 10 dollars, will be intentionally dropped by an experimenter. If a subject typically places a high

2. One-half of the sample of trustees (chosen randomly) were given the opportunity to promise beforehand to return at least as much money as they were sent by the trustor.

3. All of the rules of the game are common knowledge.

value on the dropped envelopes, then we infer that the subject is more likely to trust the anonymous strangers who will find the apparently lost envelope.

Previous research has explored both types of experiments that we study. The primary difference between our work and most previous work is that we ask whether subject characteristics predict the choices that subjects make in these experiments.

We first examine the predictive power of two types of survey questions: questions about trusting *attitudes* and questions about past trusting *behavior*. In both of our games, the standard *attitudinal* questions generally do not predict subject choices in our experiments.⁴ Of ten different variants of broad attitudinal questions, none has a significant correlation with trusting choices in either experiment that we conducted.⁵ However, we identify two relatively precise attitudinal questions about trusting strangers that did predict trust in both games.

Measures of past trusting *behavior* are better than the abstract *attitudinal* questions in predicting subjects' experimental choices. An index of past trusting behavior—based on specific behavioral questions that we developed—has a 22.4 percent correlation with the amount sent in the trust game, and a 14.6 percent correlation with the average valuation in the envelope drop. These positive correlations suggest that a component of trusting behavior may be stable across time and choice domains. We also find a 15.8 percent correlation (marginally significant) between trust in the envelope drop experiment and trust in the two-person trust game.

Although questions about trusting *attitudes* do not predict trusting behavior, such questions do appear to predict trustworthiness. An index of an individual's response to GSS attitudinal trust questions has a 34 percent correlation with the amount of money that the individual himself gives back. While attitudinal trust surveys at best weakly predict any individual's level of trust, they may be good at predicting the overall level of trustworthiness in society.

While our primary purpose is methodological, the experi-

4. This finding is consistent with numerous studies that report a lack of correlation between attitudes and behavior (e.g., review by Ajzen and Fishbein [1977], although see Kraus [1995] for a different perspective). The classic example is LaPiere's (1934) finding that self-reported unwillingness to serve racial minorities was inconsistent with restaurant or motel owners' actual behavior toward minorities.

5. Because of our small sample size we cannot rule out a small positive relationship between the trust survey questions and trust in the experiments.

ments in this paper also produce three additional substantive results. First, as theory predicts, the degree of social connection between the sender and recipient—the number of friends they have in common and the duration of their acquaintanceship—generally predicts the levels of trust and trustworthiness in the two-person trust game. These results support the idea that repeated play in dense social networks facilitates trust (as in Abreu {1988} and Greif {1993}).⁶ This result is not surprising, and we consider it to be less important as validation for the theory than as validation for the experiments.

Second, we find that subjects who are paired with a partner of a different race or nationality send back less money to their partner.⁷ Eleven out of the twelve times in which the recipient sent back nothing, the sender and the recipient were of different races. These effects are stronger than the social network effects and survive controls for the social connection of the sender and recipient. This finding is also unsurprising, but serves as a reminder of the continuing barriers that racial and national differences may create.

Third, we find that background characteristics capturing the level of status and organization membership—variables meant to serve as proxies for an individual's own social capital—strongly predict the amount of money that senders receive back from recipients, and strongly predict the financial returns for senders. People with better educated parents, students who work fewer hours for pay, individuals with more friends, and members of volunteer organizations all earn more money in the experiment. These results imply that in at least one stylized setting, noncognitive social skills may be important determinants of economic returns.

II. EMPIRICAL MEASUREMENT OF TRUST—THE CURRENT APPROACH

The empirical literature on trust has focused on responses to the question: “Generally speaking, would you say that most people can be trusted or that you can’t be too careful in dealing with people?” This question is taken from the National Opinion

6. Fershtman and Gneezy {1998} also use a variant of this game to examine trust in Israel and find a similar result. They document the importance of group membership by showing that males (not females) are less trusting of Sephardic males. In their version of the game, players do not meet one another, and they are left to deduce ethnic background from their opponents' last name.

7. We use country of residence at age sixteen as a proxy for nationality.

Research Center's General Social Survey (GSS). The survey is the primary source for U. S. evidence on trust and social capital. Since its inception in 1972 the survey has been administered twenty times to a sample of between one and two thousand respondents. The set of questions on the GSS changes from survey to survey, but the GSS trust question has been asked almost continuously.

Subject responses to the GSS trust question are difficult to interpret.⁸ Variation in responses might arise for numerous reasons: e.g., differences in beliefs about the trustworthiness of a common set of people; differences in interpretation of who comprises "most people;" differences in interpretation of what it means to be able to trust someone; or differences in the ability to elicit trustworthy behavior from other people.⁹ Variation may also arise because some respondents are not willing to answer truthfully when asked such a question on a survey. Our experiments suggest a new framework for interpretation of the GSS trust question.

Before turning to our experimental analysis, we first discuss the standard empirical analysis of the GSS trust question. Putnam (1995) and others have analyzed the GSS data and drawn two important conclusions. First, younger cohorts are dramatically less likely to report that "most people can be trusted." Second, organization membership, an important measure of social capital, exhibits a similar downward trend. These stylized facts have generated a substantial debate about declining social capital in the United States. Using a multiple regression framework, we replicate these earlier findings.

Table I gives mean answers to the trust question for both the entire sample of GSS respondents and for different subpopulations. This crude evidence suggests substantial variation in responses by cohort, education, and race: e.g., 42.3 percent of the cohort with birth years between 1911 and 1915 report that "most people can be trusted," compared with 30.1 percent of the cohort born between 1961 and 1965. Table I also compares GSS data with the responses of Harvard undergraduates who participated in our trust experiment: 42.6 percent of young and college-educated GSS

8. Schwarz (1999) notes that all self-reported instruments are subject to distortion, with various features of the questions such as ambiguity and format readily producing unintended effects on subjects' responses.

9. It seems unlikely that differences in altruism drive answers to this question (although certainly altruistic attitudes might correlate with the answers to this question).

TABLE I
AVERAGE LEVELS OF TRUST

Q. "Generally Speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?"	Percent responding that. . . ". . . most people can be trusted"
US (1972–1996):	39.9%
US (White):	44.2%
US (Black):	16.1%
US (other races):	26.6%
US (1911–1915 birthyear):	42.3%
US (1961–1965 birthyear):	30.1%
US (no high school diploma):	26.3%
US (high school diploma):	45.0%
US (college diploma):	58.5%
US (college diploma, 1996):	49.7%
US (college diploma and birthyear after 1965):	42.6%
Harvard Undergraduates (1997)	44.4%

Harvard data are from the authors' survey.

Source: The National Opinion Research Center's General Social Survey, 1972–1994.

respondents report that "most people can be trusted," close to the rate of 44.4 percent in our Harvard sample.

Mean response rates by demographic group confound many different effects in the GSS data. Table II identifies these separate effects with probit regressions that estimate how different characteristics explain trust.¹⁰ The first regression of Table II presents our benchmark results. Trust is much lower for later cohorts. The gap in the level of trust between individuals who were born before 1915 and those who are born after 1959 is 21.4 percent.¹¹ Trust is much higher among richer and well-educated individuals. College graduates are more than 30 percent more likely to answer yes to the trust question than high school dropouts. Blacks are 21 percent less likely to say that they are trusting than whites.¹² Men are slightly more trusting. Married persons are also more trusting. City size has a slightly negative effect on the amount of trust. Members of more educated (or wealthier) religious denominations

10. Table II reports the marginal effects of the variables.

11. We cannot separately identify age, cohort, and time effects, due to multicollinearity. We identify age and cohort effects by assuming away time effects.

12. The gap between the races diminishes in states where blacks are less of a minority.

TABLE II
GENERAL SOCIAL SURVEY RESULTS

Dependent variable:	GSS trust (1)	Organization membership (# of types of organizations) (2)
Born before 1915	0.2142 (0.0216)	0.4929 (0.0837)
Born between 1915 and 1929	0.1538 (0.0176)	0.5689 (0.0690)
Born between 1930 and 1944	0.1331 (0.0141)	0.3712 (0.0571)
Born between 1945 and 1959	0.0782 (0.0107)	-0.0088 (0.0443)
Age between 25 and 34	-0.0238 (0.0124)	-0.0686 (0.0470)
Age between 35 and 44	0.0229 (0.0141)	0.0940 (0.0547)
Age between 45 and 54	0.0417 (0.0164)	-0.1250 (0.0662)
Age between 55 and 64	0.0219 (0.0185)	-0.2087 (0.0732)
Age between 65 and 99	-0.0175 (0.0198)	-0.3004 (0.0800)
College education	0.1720 (0.0088)	1.1743 (0.0344)
Dropout	-0.1638 (0.0077)	-0.6922 (0.0308)
Log (real income)	0.0154 (0.0051)	0.0715 (0.0182)
Black	-0.2091 (0.0093)	0.0941 (0.0411)
Male	0.0343 (0.0066)	0.2562 (0.0249)
Married	0.0338 (0.0761)	0.0802 (0.0294)
Town	-0.0227 (0.0117)	-0.0734 (0.0455)
Small city	-0.0200 (0.0138)	-0.1174 (0.0542)
Big city	-0.0505 (0.0145)	-0.2208 (0.0575)
Catholic	0.0120 (0.0098)	-0.0194 (0.0372)
Jew	-0.0216 (0.0234)	0.1356 (0.0915)
No religion	-0.0041 (0.0134)	-0.5868 (0.0533)
Baptist	-0.0582 (0.0103)	-0.1116 (0.0532)
Methodist	0.0363 (0.0126)	0.1397 (0.0465)
Lutheran	0.0749 (0.0144)	0.1821 (0.0532)
Presbyterian	0.0842 (0.0177)	0.3122 (0.0638)
Episcopalian	0.0758 (0.0224)	0.2260 (0.0840)
Constant	0.3828	1.3282 (0.0762)
R^2	0.0855	0.1431
Observations	23867	19133

Data are from the General Social Survey, 1972–1994. Standard errors are in parentheses. Regression (1) is a probit. We report derivatives at sample means. Regression (2) is OLS. In regression (1) the constant is the mean probability at mean values of the explanatory variables. R^2 is pseudo R^2 for regression (1) and adjusted R^2 for regression (2). Town population is between 2000 and 100,000. Small City population is between 100,000 and 500,000. Big City population is above 500,000. The omitted category for religion is Protestant, no denomination.

report that they are more trusting, but Jews, a relatively highly educated religious group, report that they are less trusting.¹³

Goldin and Katz (1999) find that the proportion of residents in an area who were Lutheran in the early part of this century strongly predicts current measures of social capital. We find that the proportion of Lutherans in a state is a strong predictor of state-level trust,¹⁴ but that state effects can only explain a small amount of *individual* variation in trust.¹⁵

Most of these results have multiple interpretations. For example, the positive effect of education on trust might occur because more educated people associate with other more educated people who are, for some reason, more trustworthy.¹⁶ Alternatively, education might create individual social capital by raising social skills or because high status increases the ability to reward and punish others.

The second regression of Table II repeats this analysis using membership in nonprofessional organizations—an observable measure of social behavior—as the dependent variable. Specifically, this variable represents the number of different types of organizations in which the individual is a member. The correlation of this variable with the GSS trust measure is high, and many of the same patterns continue to hold. For example, the basic cohort, education, and income effects remain. There are only two real differences. Age effects peak between 35 and 44, and the race dummy (Blacks = 1) now takes a positive coefficient.¹⁷ This racial effect on organization membership follows from the high rate of religious affiliation of African-Americans.

We have run similar regressions outside the United States. The positive effect of education on trust and organization membership is close to ubiquitous. Measured trust does not, however, generally decline for younger cohorts outside of the United States.

We now proceed to our experiments. Putnam and others have

13. La Porta et al. (1997) argue that hierarchical religions reduce social capital at the country level because the operation of these religions entails less cooperative interaction between lay people. We see no evidence for this in the United States. Iannaccone (1991) argues that religion serves a large social purpose, and we might expect to find significantly more trust among members of particular religious groups (see also Glaeser and Glendon (1998)).

14. This analysis is available from the authors upon request.

15. Percent Lutheran explains 30 percent of the variation in the mean level of trust across states.

16. For example, generally acting in a trustworthy manner may be a luxury good, which is consumed disproportionately by people with high incomes.

17. This finding does not change when we control for the number of children in the household.

shown alarming cohort trends in U. S. responses to the GSS trust question, and we have confirmed the existence of these trends. However, it remains unclear whether these trends in survey responses accurately reflect trends in social capital. Our primary goal is to understand what the survey question about trust actually measures.

III. SURVEY AND EXPERIMENTAL DESIGN

Subjects were recruited from Harvard's introductory economics course (Social Analysis 10, or "Ec10") during the first week of class.¹⁸ Out of approximately 1000 Ec10 students, 274 agreed to participate, of whom 189 completed all parts of the study.¹⁹

Our experimental procedure has three components. First, subjects are asked to fill out a survey. Three to four weeks later, subjects participate in two experiments, the "Trust Game" and the "Envelope Drop."

Part 1, Survey. Subjects fill out an anonymous 137-question survey, which typically takes 20 to 30 minutes. The first half of the survey contains demographic and behavioral questions (e.g., gender, parental education, race, college activities, etc.). The second half contains attitudinal and self-reported behavioral measures of subjects' trustfulness and trustworthiness.

The trust questions were chosen from several sources. In our selection we emphasized questions from the General Social Survey, which contains a wide range of attitudinal questions about trust. In addition, we designed numerous trust questions of our own, including three questions to elicit past trusting behaviors. Such behavioral information is not elicited by any of the GSS questions. We ask:

- "How often do you lend money to your friends?"
- "How often do you lend personal possessions to your friends (e.g., CDs, clothes, bicycle, etc.)?"
- "How often do you intentionally leave your rooming group's hallway door unlocked (when nobody is home)?"

A complete copy of our survey is available upon request.

Part 2, The Trust Game. Three to four weeks after subjects fill out the survey, they come to a Harvard classroom to participate

18. Our recruitment handout and all of our other subject materials (e.g., consent forms, instructions, and debriefing forms) are available upon request.

19. The Ec10 students are roughly representative of Harvard undergraduates, since nearly two-thirds of the undergraduates take this course.

in two experiments: the “Trust Game” and the “Envelope Drop.” When communicating with the subjects, we refer, respectively, to the “Transfer Game” and the “Bonus Winnings Procedure.” For clarity, we use the former, more descriptive titles here.

Subjects arrive at the experiment site, sign in, fill out a consent form, and are then paired with another subject to participate in the “Trust Game.” Those who arrive together and wish to play together are allowed to do so, raising the likelihood that subjects who know each other will be paired.²⁰ Others are paired in order of arrival. After being paired, the subjects jointly fill out a social connection survey, which includes nine questions about social links between the subjects. For example, we ask the subjects to produce a list of all personal acquaintances whom they have in common. Subjects tally and report the number of names on their joint list, but keep the actual list of names to assure anonymity of the subjects (and their friends).

After filling out the social connection survey, the subjects are separated and told that they will not see each other again during the course of the experiment. The rest of the game (steps 1–5 below) is then explained to the separated subjects. Then the game is played.²¹

In the experimental instructions we refer to the subjects as either RED or GREEN players. Each pair of subjects contains one RED player and one GREEN player. In the current summary, we use more meaningful labels: “sender” and “recipient.” The five steps of the game are as follows.

- (1) The *sender* is given \$15.
- (2) The *recipient* sends the *sender* a message/promise about the *recipient's* intended future actions in the game.
- (3) The *sender* chooses to send all, some, or none of his/her \$15 to the *recipient*. We refer to this transfer as the “amount sent.”
- (4) The experimenters double whatever the *sender* chooses to send. For example, if the *amount sent* is \$4, the *recipient* receives \$8, and the *sender* keeps \$11 of the original \$15.
- (5) The *recipient* chooses to send all, some, or none of the received money back to the *sender*. We refer to this transfer amount as the “amount returned.” For example,

20. In a small pilot study, we found that randomly paired subjects were highly unlikely to know each other at all. Our nonrandom pairing procedure generates more variation in social connection.

21. A formal analysis of this game appears in an earlier version of this paper (Glaeser, Laibson, Scheinkman, and Soutter 1999).

if the *recipient* receives \$8, and the *amount returned* is \$1, then the *recipient* gets to keep \$7, and the *sender* gets to keep $\$1 + \$11 = \$12$.

To implement this procedure, subjects write their decisions on a record sheet, which is placed in a coded envelope and collected by a subset of experimental assistants who never see the contents of the envelope. The other subset of experimental assistants who actually open the envelopes (in a different room) never see the subjects. Hence, subject anonymity (vis-à-vis the experimenters) is assured.

We implement step (2)—which allows the recipient to make a promise—by giving the recipient (GREEN) the opportunity to check one of two statements: (a) “I, person GREEN, promise to repay RED at least as much as RED sends me. For example, if RED sends me \$4, which will be doubled by the experimenters to \$8, then I will repay RED at least \$4,” or (b) “I, person GREEN, make no promise to RED.”²² One of these two statements must be checked, and no other type of message is allowed. Our instructions note that promises, if made, are not binding.

Half of our pairs of subjects are given the opportunity to make a promise in the form described above. The other subjects receive no opportunity to send a message, and no mention of a promise is made in their experimental instructions.

Our procedure differs from the Berg-Dickhaut-McCabe (1995) experiment on which it is based in three ways. First, unlike Berg, Dickhaut, and McCabe we include a promise condition. Second, the Berg-Dickhaut-McCabe procedure guarantees that individual subject decisions are unobservable to all parties (i.e., both experimentalists *and* other subjects). Our game only provides anonymity vis-à-vis the experimenters; the subjects know the identity of the other subject in their pair. This knowledge creates variation in social connection—some subject pairs know each other well, while other pairs have only just met—enabling us to determine how different levels of social connection influence trusting behavior. However, removing subject-to-subject anonymity engenders an undesirable ancillary effect: a sharp rise in the cooperative behavior of senders. This increased cooperation is undesirable because full and universal cooperation eliminates variation in

22. As we intended, all but one of the 47 subjects who were given the chance to make the scripted promise did so. Our investigation differentiates between people randomly given the opportunity to promise, not between people who did promise versus people who could have promised and did not.

trusting behavior. To avoid this possibility, we deviate from the Berg-Dickhaut-McCabe experiment in a third way. We only double—rather than triple—the money that the sender sends to the recipient, thereby reducing the incentive to cooperate.²³

Part 3, The Envelope Drop. After completing the trust game, subjects participate in a second experimental procedure that provides an additional behavioral measure of trust. In this game subjects report valuations for a series of “envelope drops.” Specifically, subjects are told that an experimental assistant will intentionally drop in a public place an envelope containing 10 dollars that is addressed to the subject. The envelope may be dropped in one of several different public places (e.g., Harvard Square) under one of several different conditions (e.g., sealed and stamped). For each place and condition of the envelope drop, the subject reports a valuation. We believe that the subject’s valuation of such an envelope drop primarily measures confidence that a random pedestrian in that location will return the envelope to the subject (say by putting it into a mailbox). The procedure may also measure the subject’s trust that the experimenter will carry out the envelope drop in the first place.

To elicit truthful subject reporting, we use a standard revelation mechanism.²⁴ We randomly generate a payoff that is independent of the subject’s valuation. We give the subject this payoff if the subject’s reported valuation is below the payoff value. If the subject’s reported valuation is above the payoff value, we carry out the envelope drop. We vary properties of the envelope drop (e.g., location, time of day, stamp on envelope, envelope sealed). Each subject provides valuations for fifteen different envelope drop conditions. Subjects report sensible changes in valuations across conditions (see Appendix 3). We average each subject’s responses across the fifteen different envelope conditions offered to each subject, and use this *mean reservation value* as our second measure of trust.

We use the *amount sent* in the Trust Game and the *mean reservation value* in the Envelope Drop as our two experimental measures of trust. To measure trustworthiness, we use

$$\text{return ratio} = \text{amount returned} / \text{amount available to return}.$$

23. Despite this reduction in the incentive to cooperate, we still find that cooperation is “too” high: 71 percent of our sample of *senders* send the upper limit of the amount they are allowed to send (i.e., 15 dollars).

24. See Becker, DeGroot, and Marschak (1964).

observed. The average amount returned is \$12.30, and the average value of *return ratio* is 45.5 percent.

The average *mean reservation value* from the Envelope Drop is normalized to zero. The standard deviation is \$1.77. The distribution of *mean reservation values* approximates a bell curve with little or no apparent truncation at the endpoints.

There exists substantial heterogeneity in trusting and trustworthy behavior, and our survey data enable us to identify the variables that explain this variation.²⁵ The observed heterogeneity could arise from individual fixed effects, situationally specific effects (e.g., partnership-specific attributes in the trust game), or random noise.

We organize our analysis conceptually. First, we identify individual fixed effects in trust and trustworthiness. We look for a relationship between behavior in the experiments and responses to attitudinal survey questions. We also look for a relationship between behaviors in different situations. Second, we identify the effects of social connection between the sender and recipient. Third, controlling for social connection, we ask whether some subjects tend to fare well financially in social interactions, perhaps because of status effects. Such people may be thought of as having individual social capital.

A. Individual Effects in Trust and Trustworthiness

We begin by looking at the level of trust our senders display toward their partners. In Table III we examine whether survey questions and other individual background characteristics predict the *amount sent* in the Trust Game. In all regressions we include six basic control variables: a promise condition dummy, a mixed-gender pair dummy, a male sender dummy, a nonwhite sender dummy, a freshman sender dummy, and an only child sender dummy.²⁶ Generally, these control variables are insignificant, and their inclusion or exclusion does not affect any of our other results.

In the first column of Table III, we evaluate the predictive value of the GSS trust question. *Senders* who believe “that most people can be trusted” send 22 cents more to their partners than *senders* who believe “that you can’t be too careful in dealing with

25. Appendices 1 and 2 give the means and correlations from the survey and the two experiments.

26. The dummy variables were chosen so that approximately 50 percent of the sample had a value of one for the variable. The only child dummy is an exception to this rule.

TABLE III
 AMOUNT SENT AS A FUNCTION OF SENDER CHARACTERISTICS

	(1)	(2)	(3)	(4)	(5)
Different sexes	-0.670 (1.130)	-0.128 (1.112)	-1.043 (1.120)	-0.358 (1.106)	-0.643 (1.082)
Promise	0.043 (1.024)	-0.097 (1.015)	0.440 (1.040)	-0.038 (.992)	-0.153 (0.995)
Male	0.147 (1.197)	0.623 (1.174)	-0.028 (1.148)	0.457 (1.149)	-0.013 (1.138)
White	-0.330 (1.030)	-0.640 (1.025)	0.055 (1.031)	-0.227 (1.003)	-0.329 (1.006)
Freshman	-0.205 (1.136)	-0.434 (1.125)	-0.254 (1.092)	-0.970 (1.081)	-0.305 (1.086)
Only child	-1.620 (1.53)	-1.724 (1.474)	-1.555 (1.496)	-1.775 (1.530)	-1.569 (1.492)
GSS trust	0.220 (1.022)				
Trust index		-0.094 (0.222)			
Trust strangers			2.209 (1.060)		
Trusting behavior index				0.403 (0.214)	
Mean reservation value					0.417 (0.312)
Constant	13.361 (2.448)	13.009 (1.735)	9.836 (2.272)	12.707 (1.648)	13.336 (1.639)
Adj. R^2	-0.059	-0.050	-0.009	-0.007	-0.034
Observations	93	90	92	93	95

Standard errors are in parentheses. All regressions are ordinary least squares.

people." This effect is not statistically distinguishable from zero. In the next regression we replace the basic GSS trust question with an index of responses to three GSS attitudinal questions: the original GSS trust question and two additional questions that measure one's confidence in other people.

GSS fair: Do you think most people would try to take advantage of you if they got the chance, or would they try to be fair?

GSS helpful: Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?

We form an index of responses to these questions by adding the three variables after they are de-measured, normalized by their standard deviations, and re-signed for comparability. Increases in

this index measure of trust insignificantly *reduce* the actual level of trust.

We included twelve different attitudinal measures of trust in our survey, including two widely used multiquestion trust scales from the psychology literature: the Rosenberg {1957} Faith in People Scale (which has twelve component questions) and the Rotter {1967, 1971} Interpersonal Trust Scale (which has 25 component questions). Of these twelve different attitudinal measures, all but two have no statistically significant covariation with the actual amount of trust in our experiment. Trust survey questions at best only weakly predict trust. However, it is important to highlight the statistical imprecision of these results. Some of our standard errors are quite large, including our standard errors for the GSS trust question.²⁷

The two attitudinal survey questions that do significantly predict trusting behavior in the experiment ask specifically about trust of strangers. In column (3) in Table III we find that subjects who disagree with the statement “you can’t trust strangers anymore” are more trusting. Individuals who disagree with this statement send over an average of \$2.21 more than comparable individuals who agree with the statement. Disagreement with another statement from our survey—“when dealing with strangers, one is better off using caution before trusting them”—is also significantly correlated with the amount of trust observed in our experiment.

There are two plausible interpretations of these findings. First, among so many trust questions it is unsurprising to find two

27. Our attitudinal survey questions may not measure behavioral propensities, but may instead measure subjects’ beliefs about social norms. McClelland, Koestner, and Weinberger (1989) report a distinction between explicit and implicit attitudes in their study of “achievement motivation.” Explicit responses to an attitude survey on achievement appeared to reveal beliefs about social norms for achievement. These self-reports did not reflect durable personal commitments to action and did not correlate with behaviors that could actually enhance achievement. Rather than depending on subjects’ explicit self-reports, McClelland, Koestner, and Weinberger (1989) resorted to an implicit measure. They used independent judgments of the extent to which subjects’ indirectly obtained thought patterns displayed associations typical of high interest in achievement. The explicit measures were uncorrelated with these implicit measures, which did predict actual behavior. The poor performance of explicit measures may reflect limited self-knowledge, since much of the thinking process takes place outside of consciousness (e.g., Nisbett and Wilson {1977} and Hirst {1998}). Alternatively, subjects may distort their self-reports for presentational reasons. There is a growing trend (e.g., Banaji and Greenwald {1994}, and Greenwald and Banaji {1995}) toward use of implicit cognitive measures. For example, Spalding and Hardin (1999) have found behavioral links to implicit but not explicit measures of self-esteem.

that are significant, even if the questions actually have little or no predictive value.²⁸ Second, the attitudinal questions regarding strangers are more precise and meaningful than completely general, nonspecific questions regarding trust. This high degree of specificity might make these variables genuinely more predictive than those of a more general nature.²⁹ To further test whether the connection between these variables and the amount sent is the result of spurious correlation, we examine separately sender-recipient pairs who are composed of two strangers and those who are composed of two friends. We find that the questions about strangers only predict the amount of trust between strangers, which supports their validity as measures of trust. This represents our first piece of evidence in support of the hypothesis that trusting behavior has a component that is a persistent, individual characteristic.

In the fourth and fifth regressions we examine the connection between trusting behavior reported in the survey and trust in the experiment. The fourth regression shows the connection between an index of past trusting behavior and the *amount sent*. This index is a weighted average of responses to questions about lending possessions, lending money, and leaving your door unlocked.³⁰ A one-standard-deviation increase in this variable (2.13) increases the dollar amount sent over by 86 cents (one-fifth of a standard deviation). In the fifth regression we show a positive, but insignificant, correlation between the *mean reservation value* and the *amount sent*. Without controls, this relationship is significant at the 12.5 percent level.

We have also investigated several other potential determinants of variation in the *amount sent*, including variation in altruism and variation in early life experiences. Our measures of

28. However, the fact that the two significant questions have similar conceptual content suggests more than a spurious correlation.

29. Declines in the attitude-behavior link occur when the level of specificity in the survey question does not match that of the behavior under observation. The problem is found here in the lack of correspondence between general GSS questions and specific trusting behavior measured in our two experimental games. McClelland, Koestner, and Weinberger (1989) found a similar discrepancy between self-reports of general value placed on achievement and actual behavior to increase achievement. The problem has been documented in other settings in a review by Oskamp (1991). In a meta-study, Kraus (1995) found that certain types of questions tend to increase attitude-behavior correlation, including those addressing direct experience and those with a focus on specific issues corresponding to the specific behavior under observation. Both of our predictive questions meet these two criteria.

30. The index is formed by de-meaning these three variables, dividing by their standard deviations, and summing the resulting values.

altruism (financial generosity to the poor, hours spent volunteering) do not predict *amount sent* and generally do not affect our other coefficient estimates. To investigate the potential roots of trust, we investigate proxies for past experiences that might have led to a cynical or untrusting outlook (parental divorce, childhood violence, etc.). These also had no effect.

In Table IV we examine the determinants of the level of trust in the Envelope Drop game. We include an extra control variable, *lost mail*, which measures the recency with which the respondent lost an item in the mail. Responses to this question take on a value between one (never) and four (within the last six months). A one-standard-deviation increase in this variable (.99) predicts a

TABLE IV
MEAN RESERVATION VALUES AS A FUNCTION OF SUBJECT CHARACTERISTICS

	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.1680 (0.2810)	0.0975 (0.2890)	0.1469 (0.2820)	0.1923 (0.2850)	0.1237 (0.2784)	0.1716 (0.2870)
White	0.1599 (0.2740)	0.1590 (0.2810)	0.2028 (0.2760)	0.1535 (0.2780)	0.2036 (0.2715)	0.1865 (0.2860)
Freshman	-0.0857 (0.2880)	-0.0924 (0.2990)	-0.0633 (0.2880)	-0.0059 (0.2910)	0.0561 (0.2844)	0.1792 (0.2930)
Lost mail	-0.2336 (0.1370)	-0.2487 (0.1390)	-0.2124 (0.1370)	-0.2488 (0.1380)	-0.2274 (0.1348)	-0.2559 (0.1380)
Only child	0.0243 (0.4380)	0.0203 (0.4400)	-0.2483 (0.4330)	0.2049 (0.4420)	-0.1443 (0.4317)	-0.1429 (0.4290)
GSS trust	0.2020 (0.2620)					
Trust index		0.0396 (0.0610)				
Trust strangers			0.6774 (0.2740)			
Trusting behavior index				0.1354 (0.0640)	0.1299 (0.0621)	0.1324 (0.0650)
Pro-transfer					0.4570 (0.1448)	
Past windfall						0.8779 (0.2720)
Constant	0.7239 (0.6360)	0.5187 (0.5240)	-0.5439 (0.6680)	0.4486 (0.5180)	-0.6265 (0.6092)	1.6145 (0.6290)
Adj. R^2	-0.004	-0.005	0.029	0.022	0.069	0.076
Observations	182	177	183	183	183	166

Standard errors are in parentheses. All regressions are ordinary least squares.

decline of \$.22, or one-seventh of a standard deviation, in the *mean reservation value*. The impact of the *lost mail* variable suggests the importance of past experiences, working presumably through beliefs.³¹

In the first regression of Table IV, we include the basic GSS trust question. Individuals who answer yes to this question are willing to pay 20 cents more for the 10-dollar envelope drop. This effect goes in the right direction but is not statistically significant. In regression (2) we include the index of responses to the three GSS attitudinal questions. A one-standard-deviation increase in this GSS index (2.22) causes an insignificant 9-cent increase in the *mean reservation value*. Ten of the twelve attitudinal trust measures that we included in our survey are not significantly correlated with the *mean reservation value*.

Again, the two survey questions that specifically ask about trusting strangers positively predict trust. As regression (3) illustrates, individuals who believe that you can trust strangers are willing to pay 68 cents more for the envelope. Again, this result may be spurious, but if it is real, it suggests the presence of stable individual attributes that create trust.

The fourth regression in Table IV shows the connection between our index of past trusting behavior and the *mean reservation value*. This index has a modest, significant effect. A one-standard-deviation increase in the index raises the level of *mean reservation value* by 29 cents. The strongest connection occurs between lending possessions and the *mean reservation value*.

In the fifth regression we show that being in favor of redistribution to the poor predicts one's *mean reservation value*.³² This may occur because dropping the envelope itself is seen as an act of charity. Alternatively, people who favor redistribution may simply have a higher level of general trust or a higher level of confidence in the behavior of individuals with low income who might pick up the envelope.

To further explore the predictive value of the redistribution question, we examine the influence of this variable on subject reservation values for envelope drops in low income areas (e.g., Central Square, a relatively less affluent community near Har-

31. Unlike the rest of our survey questions, the *lost mail* question was asked after the experiment, as part of a debriefing.

32. Behavioral measures of altruism (hours spent on volunteer work or dollars given to charity) are orthogonal to this redistribution variable.

vard Square) and high income areas (e.g., Harvard Square). The strongest connection between the responses to the redistribution question and *reservation value* occurs in low income areas. Indeed there was no connection between the redistribution question and reservation values for the envelopes dropped in Harvard Square. Hence, subjects who favor redistribution appear either to believe that dropping the envelope is an act of charity when the envelope is dropped in a low income community, or to have greater confidence in the honesty of low income individuals who find dropped envelopes.

In the sixth regression we show that there appears to be some cognitive basis for higher trust. People who say that they have benefited from the generosity of an anonymous stranger in the past give more. This suggests that trust does not just reflect altruism or risk tolerance, but also beliefs about others which are formed by past experiences.

Turning now from the senders' trust, we look at the recipients' trustworthiness. In Table V we examine the effect of recipient characteristics on the value of *return ratio* (i.e., *amount returned* divided by *amount available to return*). We add the amount of money sent over in the first round (i.e., *amount sent*) as a control variable and find that this variable has a statistically significant but economically small positive effect on *return ratio*. As Figure I shows graphically, on average recipients return as much as they are sent. This might reflect reciprocal altruism as in Rabin {1993}, but it might also reflect prevailing norms about fair play.³³

These norms were influenced by the promise condition. Figures II and III plot histograms of *return ratio* values for subject pairs in the "no promise" and "promise" conditions. In the no promise condition 48 percent of recipients returned exactly as much as they were sent. By contrast, in the promise condition 68 percent of recipients returned exactly as much as they were sent. But on average *return ratio* did not vary between the two conditions. Instead, the promise condition seemed to anchor responses on the rule, "send back as much as you were sent," thereby reducing heterogeneity in the distribution of *return ratio*. The promise pulled mass away from both the selfish and generous tails of the distribution.

33. This finding is harder to reconcile with inequality aversion which would generally imply that senders who send 5 dollars or less should not receive anything in return. Even if nothing is returned, the sender will have at least as much experimental earnings as the recipient.

TABLE V
RETURN RATIO AS A FUNCTION OF RECIPIENT CHARACTERISTICS

	(1)	(2)	(3)	(4)
Amount sent	0.018 (0.007)	0.018 (0.006)	0.019 (0.007)	0.014 (0.008)
Different sexes	0.003 (0.053)	-0.007 (0.052)	0.006 (0.055)	0.001 (0.065)
Promise	-0.043 (0.051)	-0.007 (0.051)	-0.031 (0.052)	0.017 (0.063)
Male	0.027 (0.059)	0.048 (0.058)	0.013 (0.061)	-0.015 (0.073)
White	0.075 (0.054)	0.072 (0.052)	0.074 (0.055)	0.061 (0.065)
Freshman	-0.072 (0.055)	-0.052 (0.055)	-0.083 (0.056)	-0.009 (0.071)
Only child	-0.217 (0.092)	-0.242 (0.089)	-0.218 (0.088)	-0.191 (0.112)
GSS trust	0.106 (0.051)			
Trust index		0.043 (0.012)		
Self-reported trustworthi- ness			-0.026 (0.026)	
Honesty index				0.010 (0.008)
Constant	0.414 (0.149)	0.212 (0.120)	0.386 (0.185)	0.246 (0.147)
Adj. R^2	0.161	0.232	0.138	0.036
Observations	90	88	91	64

Standard errors are in parentheses. All regressions are ordinary least squares.

By examining the effect of individual characteristics on the level of trustworthiness, we can test whether trustworthiness is a stable characteristic. In the first regression in Table V, we find that our controls actually have some impact. Being nonwhite and being a freshman slightly depress trustworthiness. More significantly, only children are much less likely to return money. Being an only child reduces the amount sent back by 22 percent. This result would be even stronger without control variables. The average subject without siblings returns 40 percent of the amount that was sent over. While this result is certainly among the most provocative of this study, our small sample size (ten only children) leads us to withhold judgment until there is further independent verification of an only child effect on trustworthiness.

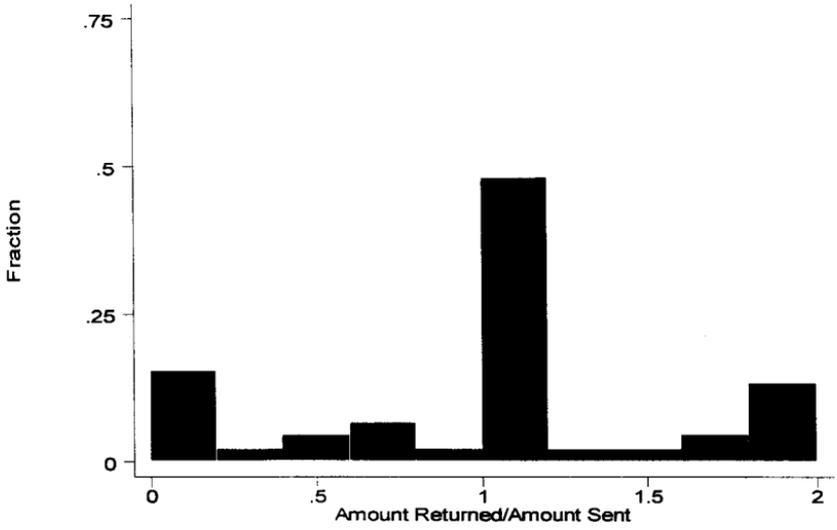


FIGURE II
Distribution of Return Ratio in "No Promise" Condition

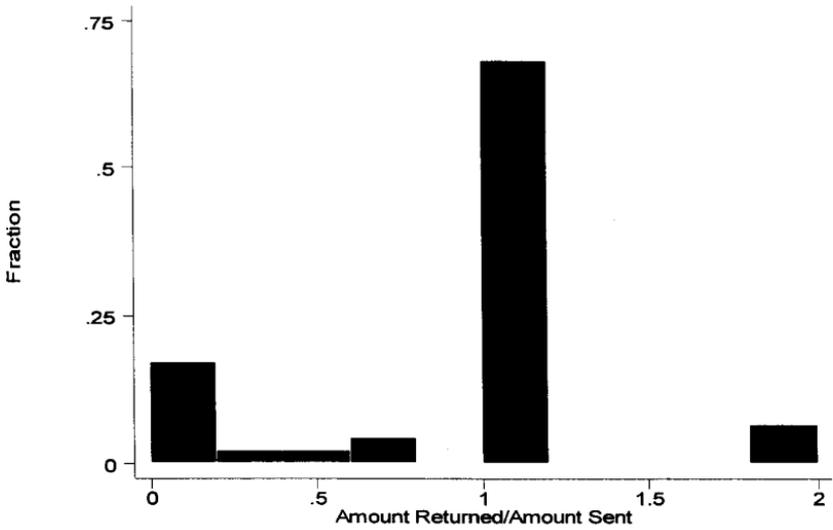


FIGURE III
Distribution of Return Ratio in "Promise" Condition

We also find a significant correlation between the ratio of money returned and the GSS trust question. In regression (2) in Table V we find an even stronger relationship between the level of trustworthiness and the index of trust-related questions (GSS Trust, GSS Helpful, and GSS Fair). The strongest subcomponent of that index is the question about whether a subject thinks other people are fair. There are significant correlations between many of the attitudinal measures of trust and the experimental measure of trustworthiness.

These findings suggest that the standard trust questions may be picking up trustworthiness rather than trust. Tables I and II take on a substantially different meaning when viewed in that light. The micro-evidence on cohorts or education might then be interpreted as suggesting that these groups differ in their trustworthiness, not their trust. When aggregated up to the country or region level, presumably a measure of trustworthiness is as good (or better) a measure of social capital as a measure of trust.³⁴

In regression (3) of Table V we show that there is a *negative*, insignificant connection between self-reported trustworthiness and actual trustworthiness. We are not surprised that those people who are willing to admit to being untrustworthy are not the least trustworthy of our subjects. This result adds further weight to our earlier findings that subjects' responses to attitudinal questions can rarely be taken at face value. Instead, our results imply that the best way to determine whether or not a person is trustworthy is to ask him whether or not he trusts others.

The final regression in this table presents an index of past honesty. This is a normalized average of subject answers to four questions about the frequency of lying to parents, roommates, acquaintances, and close friends. This reverse-scored self-reported behavioral measure of honesty weakly predicts the level of trustworthiness. The important components of the index relate to lying to acquaintances and lying to close friends. Again, it

34. One possible explanation for our trustworthiness effects is that people asked to report how much they trust others must search for information with which to answer the question. Their first step might be to determine whether they think people are trustworthy. People regularly overestimate the extent to which others are like them (the "false consensus effect" [Ross, Green, and House 1977]), so people who are themselves trustworthy would be likely to construe others as trustworthy. Having decided that others are usually trustworthy, it would be a reasonable final step to conclude that one's trust of others was high. Schwarz (1999) suggests that a process such as this lies at the heart of all attitudinal self-evaluations.

appears that asking about past actions is more successful than asking about opinions.

B. Social Connections

The social capital literature assumes that social connections (e.g., clubs, organizations, friendships, etc.) mitigate the free-rider problem and facilitate cooperative social interaction. In Table VI we determine whether social connection between sender and recipient increases trust and trustworthiness in our experiment.

We use two basic measures of social connections: months since first meeting and number of friends that the sender and recipient have in common. To lessen the impact of outliers, we have topcoded the number of months at 36 and the number of friends at 30. We include both the index of past trusting behavior and the GSS survey question on trust as added controls.

The first and fifth regressions of Table VI show the effect of months since first meeting on *amount sent* and *return ratio*. The coefficient in the *amount sent* regression is significant at the 90 percent level; the coefficient in the *return ratio* regression is significant at the 95 percent level. The coefficients are not small. A one-standard-deviation increase in time known raises *amount sent* by 80 cents and *return ratio* by 5 percent.

In the second and sixth regressions we look at the number of friends in common. There is a positive, insignificant effect of this variable on *amount sent* and *return ratio*. The economic magnitudes of the coefficients, though, are not inconsequential. For example, ten extra friends apparently raises *return ratio* by 2.6 percent, at least in our relatively small sample.

In the third and seventh regressions we test whether individuals from different countries trust each other less. We find a small but insignificant negative effect on *amount sent*, but a large and very significant effect on *return ratio*. People are much more likely to return low amounts if they are facing someone from a different country. This result is much stronger than the results on social connection, and it is unchanged when we control for social connection.

The differences between regressions (3) and (7) pose a puzzle. If trustworthiness between nationalities is so low, why is trust itself not affected by national differences within a subject pair? Perhaps our coefficient in the trust equation is imprecisely measured. Alternatively, senders may truly fail to infer that they will receive less back from recipients from a different national-

TABLE VI
INFLUENCE OF SOCIAL CONNECTION ON AMOUNT SENT AND RETURN RATIO

	Amount sent as function of sender and pair characteristics				Return ratio as function of recipient and pair characteristics			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Amount sent					0.0143 (0.0069)	0.0151 (0.0069)	0.0170 (0.0066)	0.0140 (0.0069)
Promise	-0.0796 (1.0060)	0.0308 (1.0153)	0.0416 (1.0319)	-0.0597 (1.0380)	-0.0548 (0.0511)	-0.0539 (0.0515)	-0.0358 (0.0501)	-0.0691 (0.0519)
Different sex	-0.2899 (1.1372)	0.0455 (1.1890)	-0.2118 (1.1750)	-0.2243 (1.1657)	0.0067 (0.0530)	0.0331 (0.0577)	0.0328 (0.0528)	0.0050 (0.0536)
Male	0.3777 (1.1950)	0.5750 (1.2007)	0.6190 (1.2168)	0.6453 (1.2251)	0.0477 (0.0589)	0.0622 (0.0621)	0.0458 (0.0570)	0.0448 (0.0584)
White	-0.3568 (1.0082)	-0.1940 (1.0203)	-0.2490 (1.0274)		0.0589 (0.0541)	0.0497 (0.0553)	0.0567 (0.0527)	
Freshman	0.3029 (1.1270)	-0.0344 (1.1251)	0.0387 (1.1484)	0.0215 (1.1406)	-0.0557 (0.0549)	-0.0668 (0.0550)	-0.0730 (0.0532)	-0.0641 (0.0553)
Only child	-1.9766 (1.5499)	-1.6304 (1.5724)	-1.8404 (1.5906)	-1.7288 (1.5966)	-0.2222 (0.0933)	-0.2447 (0.0944)	-0.2149 (0.0909)	-0.2302 (0.0950)
Trusting behavior index	0.3997 (0.2157)	0.3788 (0.2199)	0.4020 (0.2206)	0.3964 (0.2216)	0.0027 (0.0153)	0.0061 (0.0153)	0.0021 (0.0148)	0.0061 (0.0153)
GSS trust	0.1581 (1.0136)	0.1978 (1.0260)	0.1580 (1.0323)	0.2080 (1.0461)	-0.1004 (0.0530)	-0.1057 (0.0537)	-0.1048 (0.0516)	-0.0800 (0.0541)
Months since first meeting	0.1016 (0.0614)				0.0060 (0.0032)			
Number of common friends		0.0310 (0.0321)				0.0026 (0.0017)		
Different nationality			-0.2174 (1.2509)				-0.1749 (0.0616)	
White sender, Nonwhite recipient				-0.7496 (1.4726)				-0.1092 (0.0623)
Nonwhite sender, white recipient				-0.2350 (1.1579)				-0.1231 (0.0665)
Nonwhite sender, non-white recipient				-0.1040 (2.5213)				-0.0893 (0.1250)
Constant	12.0169 (2.4486)	11.6161 (2.5578)	12.2148 (2.4951)	12.1904 (2.5012)	0.4327 (0.1501)	0.4058 (0.1527)	0.4533 (0.1462)	0.5433 (0.1455)
Adjusted R ²	-0.0006	-0.0224	-0.0337	-0.0433	0.159	0.1457	0.2029	0.1471
Observations	92	92	92	92	89	89	89	89

Standard errors are in parentheses. All regressions are ordinary least squares.

ity.³⁵ Perhaps this failure arises because of the novelty of the experiment and the complexity of implementing backward induction. Perhaps this failure arises because Harvard students are loathe to assume/acknowledge that their partners may exhibit nationalistic preferences.

Regressions (4) and (8) of Table VI look at the impact of within-pair racial differences. In our sample, more than 35 percent of the subjects are Asian, so there is considerable racial heterogeneity. To analyze race, we divide the subject pairs into four groups: white sender/white recipient (the omitted category), white sender/nonwhite recipient, nonwhite sender/white recipient and nonwhite sender/nonwhite recipient. Note that subject pairs in the last group may still exhibit within-pair heterogeneity, since the nonwhite category is quite broad.

None of the group effects are significant in the *amount sent* regressions, but all three groups have lower *return ratios* than the white/white omitted category. The lowest return ratios occur when white recipients return money to nonwhite senders. Most strikingly, 92 percent of the cases where the recipient sent back nothing occurred when the individuals were of different races, while only 59 percent of the pairings were racially diverse.

The negative effect of different races may occur because people from different races or countries have a lower probability of interacting in the future, but the nationality and race effects change little when we control for the two social connection variables. Alternatively, lower levels of trustworthiness between races may instead reflect lower levels of reciprocal altruism. Whatever the interpretation, these results suggest that heterogeneity may decrease trustworthiness in social groups.³⁶

C. Social Status and Individual Social Capital

Social capital has two rival conceptualizations. Many original users of the term, James (1904), and more recently Loury (1977) saw social capital as an individual-specific variable reflecting

35. These results support those of Fershtman and Gneezy (1998), but differ in important ways. First, they examine trust of all people toward a low status minority and find that there is less trust. Second, their subjects have no possibility of repeated interaction after the game. Third, they allow no personal contact between subjects, who must infer ethnicity from the last name of their partners. Fershtman and Gneezy find no ethnic effects on amounts returned, but they look only at the effect of the sender being Sephardic, not at any effects of sender-recipient ethnic similarity on the amount returned.

36. La Porta et al. (1997) argue that ethnic heterogeneity may decrease the quality of government by limiting the formation of aggregate social capital.

one's ability to do well in social situations. In the 1990s Coleman {1990} and Putnam {1993, 1995} have used the term to describe group-level attributes, like the existence of a social network among a group of friends. We examine evidence for the former conceptualization in this subsection.

In Table VII we test whether people with high levels of social status systematically realize high returns from social interactions. Many different mechanisms could support such regularity. For example, high status individuals may elicit trustworthy behavior because they are relatively skilled at socially punishing or rewarding others.

Table VII identifies individual characteristics that predict high individual returns in the trust experiment. Conceptually, we demonstrate the existence of individual-specific social capital. We focus on characteristics of the sender that predict financial success for the sender. Characteristics of the recipient that predict success generally work because the characteristics predict cheating (i.e., returning less than the *amount sent*). This cheating may be punished after the experiment ends, during routine social interactions among Harvard students. So the total returns to the recipient, including nonfinancial returns, are likely not to be observed.

In our first regression we examine two measures of the sender's family status: (1) hours spent working for pay and (2) whether one's father has a college degree. Hours spent working for pay as a college student is strongly correlated with low family income (44 percent), and we believe it is a significantly better measure than the categorical family income variable that we have. We also include two social connection variables. First, we include the self-reported number of close friends that the individual says he has (top-coded at 20). Second, we include the number of hours per week that the individual spends volunteering. Approximately two-thirds of Harvard undergraduates volunteer regularly, and this is a particularly general form of social interaction. We also include two proxies for "coolness" or charisma in this subject population: beers drunk per week and whether the individual has a sexual partner.³⁷

In the first regression we find that working for pay has a negative effect on *amount sent* but this effect is only marginally

37. This variable takes on a value of one if the individual has a current girlfriend or boyfriend and if the individual has had sex with at least one other person in the past five years.

TABLE VII
SOCIAL CAPITAL, STATUS, AND THE RETURNS TO SOCIAL CAPITAL

	Amount sent as a function of sender characteristics (1)	Return ratio as a function of sender characteristics (2)	Financial returns to sender as a function of sender characteristics (3)	Financial returns to recipient as a function of sender characteristics (4)
Promise	-0.0450 (0.1010)	-0.0103 (0.0491)	0.1881 (1.2238)	-0.2331 (1.5552)
Different sex	-0.8162 (1.1147)	0.0887 (0.0541)	2.0264 (1.3510)	-2.8426 (1.7168)
White	0.2571 (1.0743)	0.0114 (0.0519)	0.4036 (1.3021)	-0.1470 (1.6546)
Male	-0.1849 (1.1733)	0.1008 (0.0565)	2.8013 (1.4220)	-2.9861 (1.8070)
Freshman	0.3741 (1.2394)	-0.0174 (0.0602)	0.4294 (1.5021)	-0.0553 (1.9088)
Only child	-2.5365 (1.5545)	0.0578 (0.0780)	0.7949 (1.8840)	-3.3314 (2.3941)
Father with college degree	-0.3324 (2.0752)	0.1407 (0.0987)	2.3978 (2.5151)	-2.7302 (3.1961)
Hours worked for pay	-0.1441 (0.0869)	-0.0092 (0.0043)	-0.2273 (0.1053)	0.0832 (0.1338)
Number of close friends	-0.0919 (0.0658)	0.0112 (0.0032)	0.1684 (0.0798)	-0.2603 (0.1014)
Hours spent volun- teering	0.0306 (0.2031)	0.0261 (0.0099)	0.6893 (0.2462)	-0.6586 (0.3128)
Sexual partner	2.8618 (1.4861)	0.1726 (0.0723)	4.7588 (1.8012)	-1.8969 (2.2889)
Beer servings	0.0471 (0.1243)	0.0136 (0.0063)	0.3399 (0.1507)	-0.2929 (0.1915)
Amount sent		0.0118 (0.0065)		
Constant	13.7772 (2.7993)	-0.0690 (0.1591)	5.8516 (3.3926)	22.9256 (4.3112)
Adjusted R^2	-0.0061	0.308	0.2006	0.0735
Observations	93	89	93	93

Standard errors are in parentheses. All regressions are ordinary least squares.

significant at the 10 percent level.³⁸ The effect of father's education is insignificant. Of the other variables, only having a sexual partner positively predicts trust.

In the second regression we regress *return ratio* on characteristics of the sender. This regression identifies several sender

38. The impact of the working for pay variable may reflect standard income effects.

characteristics that significantly predict which senders will be well treated by the recipients. We find significant coefficients on all variables except for father's education. Father's education, however, becomes significant if hours worked for pay is dropped from the regression. The effects are often quite large. For example, people without sexual partners receive 17 percent less back than people who do have sexual partners (this is true of both men and women).

These two regressions provide only partial snapshots of the impact of social capital, because the regressions evaluate separately *amount sent* and *return ratio*. The artificial but controlled setting of our experiment has the advantage that we can precisely measure total financial returns from this particular social interaction. We next regress total financial returns (from the Trust Game) on personal characteristics of our subjects. We perform this regression separately for individuals in the sender and the recipient position. All returns are measured in dollars.

Column (3) in Table VII reports the results of the sender regression. Again, all variables except for father's college status are significant. Without controls, the father's education variable becomes much stronger. The average return for senders whose father graduated from college is 14.75 dollars. The average return for senders whose fathers did not graduate from college is 9.29 dollars. It appears that senders from families with lower human capital would have been better off financially if they had not trusted their partners. Comparing *senders* from high and low income families also demonstrates these effects. The average returns for *senders* from high income families was more than 2 dollars greater than the average returns for *senders* from low income families. These higher returns appear to come solely from the ability to elicit trustworthy behavior.

We find that *senders* who work for pay receive less from the social interaction. *Senders* with sexual partners take home over 4 dollars more than their celibate classmates. Individuals who drink beer earn more. Finally, a one-standard-deviation increase in the volunteering variable raises financial returns from the game by almost 2 dollars.

Since social capital of the sender does not predict higher values of *amount sent*, but does predict higher values of *return ratio*, it seems likely that social capital of the sender has negative financial externalities for the recipient. Regression (4) in Table VII confirms this conjecture. We regress the total financial returns

of the recipient on our measures of the sender's social capital. We find that all of the social capital variables that increase the financial returns for the sender decrease the returns to the recipient. In this experiment these types of social capital lead to redistribution from one player to another. As such, the social capital that we have identified appears to generate private, not group, returns. Findings like this underscore the importance of distinguishing between individual and group-level conceptualizations of social capital.

Finally, we note that these results may not generalize. Social capital could have increased everyone's returns in the Trust Game only if it had increased the level of trust in the first period. But a large share of our sample of senders chose to send the maximal amount. As such, there may have been insufficient opportunity for the positive effects of social capital to operate on group returns. Naturally, the earlier warnings about the limited size of our sample and the special features of our games and subject population apply here as well.

V. CONCLUSION

Using two experiments, we investigated whether trust, trustworthiness, and social capital are characteristics of individuals as well as groups. Past trusting behavior correlates with trusting behavior in our experiments. The levels of trust in the two experiments are weakly correlated with each other. We also found that two of our attitudinal survey questions about trusting strangers both predict trust. Hence, trusting behavior has a stable individual-specific component. However, this component is at best weakly measured by typical attitudinal questions about trust, including the widely studied GSS trust question. Trustworthiness also seems to be stable and is strongly predicted by attitudinal survey questions about trust (not trustworthiness) and by having siblings.

In summary, to determine whether someone is trusting, ask him about specific instances of past trusting behaviors. To determine whether someone is trustworthy, ask him if he trusts others.

There are two additional sets of results from these experiments. First, social connection strongly predicts trustworthiness and weakly predicts trust. In particular, national and racial differences between partners strongly predict a tendency to cheat one another.

Second, individual characteristics that relate to family status, social skill, and charisma strongly predict one's total financial returns in the trust experiment. These variables matter because people in our sample are less likely to cheat individuals with these characteristics. These results suggest that some people have "individual social capital," a subcomponent of human capital that reflects an ability to earn returns from social situations.

There are three major implications of this paper for future research. First, social capital is a meaningful, individual-level variable that can be studied with the tools of price theory.³⁹ Our evidence supports the view that human capital includes not only cognitive and physical abilities but also social capital, e.g., interpersonal skills, status, and access to social networks (as in Bowles and Gintis (1976)).

Second, standard survey questions about trust do not appear to measure trust. However, they do measure trustworthiness, which is one ingredient of social capital. This means that most work using these survey questions needs to be somewhat reinterpreted. If future surveys hope to measure trust, then other instruments, including questions about past trusting behavior, should be developed and empirically validated.⁴⁰

Finally, we believe that this paper demonstrates the value of using experiments and surveys together. Experiments measure preferences, behavioral propensities, and other individual attributes much more convincingly than surveys, since experiments provide direct observations of behavior. By connecting experiments and surveys, we can determine the socioeconomic correlates of hard-to-measure individual attributes, and test the validity of survey measures of these attributes.

39. However, aggregation will be much more difficult than for other forms of capital.

40. Indirect psychological measures of attitudes have been shown to be more effective than direct measures (McClelland, Koestner, and Weinberger 1989).

APPENDIX 1: DEFINITIONS OF VARIABLES

Variable name	Question/description	Answer range	Mean (standard deviation)
Amount sent	Amount sent from the sender to the recipient in the trust game	0–15	12.41 (4.54)
Beer servings	The number of beers consumed per week	Positive real numbers	2.03 (4.13)
Behavioral index	Normalized index of lend money, lend possessions, and leave door unlocked	Real numbers	-.03 (2.13)
Close friends	Number of close friends	Positive real numbers capped at 30	7.24 (6.32)
Common friends	Number of friends the two participants of the trust game have in common	Positive real numbers capped at 50	12.54 (16.08)
Different nationality	Are the participants in the trust game from different countries?	No—0 Yes—1	0.25 (0.43)
Different sex	Are the participants in the trust game of opposite sex?	No—0 Yes—1	0.42 (0.49)
Different race	Are the participants in the trust game of different races?	No—0 Yes—1	0.58 (0.49)
Door unlocked	"How often do you leave your door unlocked?"	Very often—1 Often—2 Sometime—3 Rarely—4 Never—5	4.26 (1.11)
Father w/BA	Father with a college degree	No—0 Yes—1	0.90 (0.28)
Freshman	Freshman	No—0 Yes—1	0.68 (0.46)
GSS fair	"Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?"	Would take advantage of you—1 Would try to be fair—2	1.56 (0.49)
GSS help	"Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?"	Try to be helpful—1 Just look out for themselves—2	1.61 (0.49)
GSS trust	"Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?"	Most people can be trusted—1 Can't be too careful—2	1.51 (.50)
GSS index (trust index)	Normalized average of GSS fair, GSS trust, and GSS help	Positive real numbers	0.00 (2.22)
Honesty index	Average of five questions rating frequency of lying to parents, roommates, acquaintances, close friends and partners on a scale of 1 (very often) to 5 (never).	{2.2, 5}	3.99 (.50)
Hours worked for pay	The number of hours worked for pay per week	Positive real numbers capped at 45 hours	4.67 (6.50)

APPENDIX 1: (CONTINUED)

Variable name	Question/description	Answer range	Mean (standard deviation)
Hours volunteering	The number of hours spend volunteering per week	Positive real numbers capped at 60 hours	1.95 (2.65)
Lend money	"How often do you lend money to friends?"	More than once a week—1 About once a week—2 About once a month—3 Once a year or less—4	2.85 (1.15)
Lend possessions	"How often do you lend personal possessions to friends?"	More than once a week—1 About once a week—2 About once a month—3 Once a year or less—4	2.44 (1.18)
Lost mail	"Have you or someone close to you recently lost something in the mail?"	Never—1 Not in several years—2 Within 1–2 years—3 Within the last 6 months—4	2.54 (0.99)
Male	Male	No—0 Yes—1	0.68 (0.46)
Mean reservation value	Average of reservation values over all the envelope drop conditions	0–10	0.002 (1.77)
Only child	Only child	No—0 Yes—1	0.11 (0.32)
Past windfall	"Have you every spontaneously benefitted from the generosity of someone you never knew before?"	No—0 Yes—1	0.57 (0.50)
Pro-transfer	"Personal income shouldn't be determined by work."	Disagree strongly—1 Disagree somewhat—2 Agree somewhat—3 Agree strongly—4	2.17 (0.89)
Promise	Promise to return at least as much to the sender as sent	Agreed—0 Didn't agree—1	0.48 (0.50)
Return ratio	(Amount returned to sender)/(amount recipient received)	{0,1}	0.46 (0.27)
Sexual partner	Does the participant have a sexual partner?	No—0 Yes—1	0.17 (0.38)
Stranger (trust stranger)	"You can't count on strangers anymore."	More or less agree—1 More or less disagree—2	1.39 (0.05)
Time since met	The number of months the sender and recipient have known each other. Capped at 36 months	Positive real value	2.73 (7.83)
Trustworthy (self-reported trustworthiness)	"I am trustworthy"	Disagree strongly—1 Disagree somewhat—2 Disagree slightly—3 Agree slightly—4 Agree somewhat—5 Agree strongly—6	5.31 (0.93)
White	White	No—0 Yes—1	0.61 (0.49)

APPENDIX 2a: CORRELATIONS BETWEEN DIFFERENT MEASURES OF TRUST

	Mean reservation value	Amount sent	GSS trust	GSS index	Trust stranger
Amount sent	0.1580 (0.1222)				
	97				
GSS trust	-0.0636 (0.3899)	-0.0117 (0.9103)			
	185	95			
GSS index	0.0663 (0.3767)	-0.0582 (0.5815)	-0.7505 (0.0000)		
	180	92	253		
Trust stranger	0.1874 (0.0104)	0.1894 (0.0674)	-0.3665 (0.0000)	0.3530 (0.0000)	
	186	94	257	251	
Behavioral index	0.1457 (0.0472)	0.2240 (0.0291)	-0.2036 (0.0010)	0.0930 (0.1418)	0.1355 (0.0293)
	186	95	257	251	259

First entries are correlation coefficients. *P*-values are in parentheses. Last entries are numbers of observations.

APPENDIX 2b: CORRELATIONS BETWEEN OUTCOMES AND INDEPENDENT VARIABLES

	Mean reservation value	Amount sent as a function of sender characteristics	Amount sent as a function of recipient characteristics	Return ratio as a function of sender characteristics	Return ratio as a function of recipient characteristics
Male	0.0462 (0.5274)	0.0478 (0.6419)	-0.0890 (0.3887)	0.1181 (0.2597)	0.0241 (0.8198)
	189	97	96	93	92
White	0.0805 (0.2686)	-0.0031 (0.9758)	-0.1212 (0.2370)	0.1095 (0.2962)	0.1387 (0.1850)
	191	97	97	93	93
Freshman	-0.0259 (0.7219)	-0.0573 (0.5774)	-0.0222 (0.8289)	-0.1265 (0.2268)	-0.0812 (0.4390)
	191	97	97	93	93
Only child	-0.0485 (0.5078)	-0.0129 (0.2066)	-0.0256 (0.8047)	-0.0020 (0.9845)	-0.2394 (0.0215)
	189	97	96	93	92
Number of common friends	-0.0045 (0.9511)	0.1455 (0.1551)	0.1242 (0.2255)	0.1583 (0.1296)	0.1441 (0.1681)
	191	97	97	93	93
Hours volun- teering	0.0113 (0.8768)	0.0573 (0.5773)	0.0198 (0.8470)	0.2085 (0.0449)	0.0854 (0.4158)
	191	97	97	93	93
Hours worked for pay	-0.0499 (0.4928)	-0.1407 (0.1691)	0.0935 (0.3624)	-0.2397 (0.0207)	0.0183 (0.8617)
	191	97	97	93	93
Different coun- tries		-0.0628 (0.5414)	-0.0628 (0.5414)	-0.3210 (0.0017)	-0.3210 (0.0017)
		97	97	93	93
Different races		-0.0143 (0.8898)	-0.0143 (0.8898)	-0.2447 (0.0181)	-0.2447 (0.0181)
		97	97	93	93
Number of close friends		-0.0945 (0.3574)	-0.0376 (0.7150)	0.2412 (0.0199)	0.0003 (0.9980)
		97	97	93	93

First entries are correlation coefficients. Numbers in parentheses are *p*-values. Third entries are number of observations.

APPENDIX 3: THE EFFECT OF DIFFERENT SCENARIOS ON RESERVATION PRICE

	Reservation value
Constant	5.82 (0.109)
Post office	2.29 (0.128)
No postage	-2.50 (0.116)
Unsealed	-2.03 (0.116)
Night	-0.98 (0.128)
Porter Square	-0.36 (0.116)
Central Square	-0.49 (0.116)
Adjusted R^2	0.279
Observations	2850

Standard errors are in parentheses. Results are from ordinary least squares regressions where each person-condition reservation value is an observation.

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