ESSAYS ON SOCIAL CAPITAL AND COORDINATION

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My dissertation explores social capital and coordinated investment using experimental methods. Chapter 1 is motivated by the observed relationship between trust attitudes and economic growth (Knack and Keefer, 1997; La Porta et al., 1997; Algan and Cahuc, 2010). I hypothesize that trust as measured in standard survey questions may be capturing the ability of people to coordinate in situations where they both stand to gain if they overcome the risk that the other will not follow suit. This result may help explain (by providing a potential mechanism) the relationship between these survey measures of trust and macroeconomic performance. Chapter 2 examines the importance that expectations of behavior are commonly understood in a coordinated investment setting. That is, how does knowing what others think about what *I* will do affect what I think about them, and how should I respond to these second order beliefs? Chapter 3 is a field study in collaboration with Professor Randall Walsh. We explore the causal impact of trust on investment. We study how quasi-experimental shifts in trust attitudes caused by proximity to crime affect survey respondents' trust attitudes and investment behavior in incentivized tasks.

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1.0 SOCIAL CAPITAL AND EQUILIBRIUM SELECTION IN STAG HUNT GAMES

Surveys of trusting attitudes are found to correlate with growth and development outcomes. The question of why trust attitudes correlate with economic growth remains open however. I argue that trust surveys capture facets of social capital not previously investigated, namely, coordination. Hence a complete investigation of the relationship between trust attitudes in growth must encompass their predictive power in a coordination game. This study shows that affirmative responses to surveys of trust attitudes correlate with and predict efficiency-supporting behavior in a Stag Hunt game.

1.1 INTRODUCTION

Going back to at least Smith's Wealth of Nations, economists have studied why some societies are more prosperous than others. The "social capital" literature focuses on the role played by institutions, norms and beliefs that enable people to participate in mutually beneficial economic activities. Social capital can have many dimensions; but trust is thought to be crucial. Putnam (1993), contrasts local government effectiveness among the regions of Italy following power devolution in the 1970s. He finds that government effectiveness highly correlates with civic engagement[1] and generalized trust in others. Trust attitudes have standard measurement instruments found on surveys such as the General Social Survey and World Values Survey; and are found by cross-country studies, seminally Knack and Keefer (1997), to correlate with growth and other measures of institutional performance.

What mechanisms generate these reduced-form correlations? A literature has arisen in experimental economics seeking to tie trust attitudes to specific behavioral patterns that are candidates for generating macro-level outcomes. One model is the trust game studied by Berg, Dickhaut and McCabe (1995). In that game, a sender decides how much money to transfer to a receiver. Money sent is multiplied; hence social surplus is maximized by sending the entirety of one's endowment. The receiver has the ability, but no obligation, to return some of the resulting surplus to the sender. For the sender to trust the receiver will return a monetary transfer is efficient, but not part of a Nash equilibrium. Glaeser et al. (2000) however find that survey trust questions are not correlated with trusting behavior in the Berg et al. trust game. While trustworthy behavior (the receiver returning a sender's transfer) is correlated with survey trustworthiness questions, this is not the efficiency-generating action. A follow-on literature to Glaeser et al. confirms that behavior in the trust game is not robustly related to survey questions on trust. If the type of trust displayed in the trust game stands behind macro-level outcomes, it does not appear to be a channel through which trust attitude surveys are correlated with growth.

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[1] Putnam measures local government effectiveness as perceived citizen powerlessness, corruption, respect for the law and public safety. Putnam’s civic engagement comprises referendum turnout, newspaper readership, number of sports and cultural associations in the community and the ability of political machines to enforce “preference voting”.

We can respond to this finding in a number of ways. One is to focus on what trust game behavior is correlated with and build on those results. Another might be to dismiss the correlation between trust and prosperity as simply being generated by reverse causation: more prosperous societies instill trust in their citizens because people look backwards at a track record of success. Putnam himself argues against a causal interpretation of his data, emphasizing "path-dependent social equilibria" and saying that "Norms and networks of civic engagement contribute to economic prosperity and in turn are reinforced by that prosperity." Algan and Cahuc (2010) suggest that reverse causality is not solely responsible for observed data patterns, however. Their identification strategy argues for a causal interpretation of trust attitudes on growth – a result which demands further inquiry into the mechanisms generating it.

I argue that the ability to coordinate on efficient actions when they are risky is a form of social capital that can generate growth and prosperity. Social capital could be more than merely a question of finding Pareto-superior deviations from equilibrium play, however. Rather a society with substantial social capital may be successful in coordinating on Pareto-preferred equilibria. If trusting attitudes, revealed through standard survey measures, predict behavior in coordination problems this might explain why they are correlated with growth and other measures of institutional performance. This study establishes that trust surveys do predict behavior in a coordination game.

1.2 SOCIAL CAPITAL

The ability to exploit Pareto-improving opportunities in the face of uncertainty has profound relevance to economic development and entrepreneurship. Social capital is that which connects, directs, or otherwise enables economic activity. Dasgupta (2008) collects overlapping definitions:

> ‘features of social organization, such as trust, norms, and networks that can improve the efficiency of society by facilitating coordinated actions’ – Putnam, Leonardi and Nanetti, (1993, p. 167);
>
> ‘Social capital refers to connections among individuals -- social networks and the norms of reciprocity and trustworthiness that arise from them.’ – Putnam (2000, p. 19); and
>
> ‘Social capital generally refers to trust, concern for one's associates, a willingness to live by the norms of one's community and to punish those who do not.’ – Bowles and Gintis (2002, p. F419).

Social capital is most commonly measured with survey instruments on the General Social Survey or World Values survey. The standard `trust question', found on both, is

> ‘Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?’

and will henceforth be referred to as *GSS_trust*. Accompanying the standard trust question is a variant, also on both the GSS and WVS, the standard ‘fair question’

> ‘Do you think that most people would try to take advantage of you if they got the chance, or would they try to be fair?’

Trust attitudes are oft-used regressors in development and growth studies. Knack and Keefer (1997) correlate trust and civic norms as measured on the World Values Survey with measures of economic performance in a cross-section of 29 countries. These most prominently include growth, but also investment share of GDP, labor force growth, openness to trade, black market penetration, strength of property rights, currency depreciation, creditworthiness, and inequality. Knack and Keefer find that (WVS) trust has a positive correlation with these variables. La Porta et al. (1997) find that standard generalized trust measures track a very broad range of institutional and economic performance outcomes[2]. They argue that most organizations need to maintain trust among their members to function effectively: firms, nonprofits, and governments characterized by high trust and trustworthiness should perform better. Fukuyama (1995) makes a more discursive version of this argument. Higher trust enables organizations to grow larger since large organizations entail imperfect monitoring within the institution and greater reliance on norms of behavior to enforce cooperation among its members.

Reduced-form correlations found in these classic studies hold up in later waves of the World Values Survey as well. The 2000 Wave for example includes a more heterogeneous range of countries and the additional fair question found on the General Social Survey as well. I revisit the findings of Knack and Keefer's Table 1 with data from the 2000 wave of the World Values Survey (aggregated), Penn World Tables national accounts data, and UNESCO cross-country education data. The original Knack and Keefer regressions measure performance in growth and investment that comes after the measurement of trust. Since I use a survey wave 19 years ahead of theirs, all other covariates are 19 years more recent than in that paper. Other explanatory variables included (per Knack and Keefer) are the proportion of eligible students enrolled in secondary (sec79) and primary schools in 1979 (prim79), per capita income at the beginning of

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[2] These include efficiency of the judiciary, corruption, bureaucratic quality, tax compliance, civic participation, participation in professional associations, share of top 20 firms in GNP, adequacy/quality of infrastructure, infant mortality, high school completion, educational system adequacy, inflation, growth and GNP per capita.

the period (gdp99), and the price level of investment goods (pi99). Since the 2000 wave of 41 countries includes many more less-developed countries than the 1981 wave, some have missing data on the control variables used by Knack and Keefer. I therefore estimate the coefficients using STATA's multiple imputation procedure. The dependent variable in equations 1-4 of **Table 1** is average annual growth in per capita income over the 1999-2009 period.

The results, while less precisely estimated than Knack and Keefer's confirm that social capital variables are an important correlate of economic performance in this diverse panel of countries. Results on control variables are as expected; incomes converge conditional on other variables, school enrollment is positively related to growth, and investment goods prices are negatively related to growth. Both the WVS standard trust and fair questions show a positive relationship with growth over the period examined. The coefficients on WVS questions, while not statistically significant, are economically significant in the baseline specifications and qualitatively unchanged when interaction terms that allow the correlation of growth and social capital to differ for poor and rich countries are added. The coefficients on trust and fair in equations 1-4 indicate that a one standard-deviation rise in affirmative responses to either question is associated with a growth acceleration of around half a percentage point per annum. I find marginal significance for trust on Investment as a share of GDP, suggesting that higher trust enables higher levels of investment. Estimated coefficients on the social capital variables are of a similar magnitude to those in Knack and Keefer.

Table 1. Knack and Keefer's regressions on new data

	Dependent variable					
	Growth 1999-2009				Investment/GDP 1999-2009	
Constant	.015 (.0232)	.015 (.0209)	.012 (.0243)	.016 (.0249)	17.7 (7.73)	15.2 (8.58)
GDP99	-.001** (.0005)	-.001** (.0005)	-.001 (.0013)	-.001 (.0010)	-.040 (.1728)	-.023 (.191)
PRIM79	.023 (.0259)	.017 (.0213)	.021 (.0254)	.018 (.0210)	8.23 (8.07)	4.78 (7.97)
SEC70	.003 (.0237)	.003 (.0219)	.002 (.0241)	.003 (.023)	4.12 (7.88)	6.01 (7.57)
PI99	.000 (.0001)	.000 (.0001)	.000 (.0001)	.000 (.0002)	-.020 (.072)	-.023 (.0842)
WVS_Trust	.030 (.0312)		.045 (.0439)		13.5* (9.56)	
WVS_Fair		.032 (.0255)		.028 (.0399)		5.84 (8.96)
Trust×GDP			-.002 (.0033)			
Fair×GDP				.000 (.0016)		

Reproduction of Knack and Keefer (1997) Table 1 using 2000 wave with new questions. Countries are Albania, Algeria, Argentina, Bangladesh, Bosnia and Herzegovina, Canada, Chile, China, Egypt, India, Indonesia, Iran, Iraq, Israel, Japan, Jordan, Kyrgyzstan, Macedonia, Mexico, Moldova, Montenegro, Morocco, Nigeria, Pakistan, Peru, Philippines, Puerto Rico, Saudi Arabia, Serbia, Singapore, South Africa, South Korea, Spain, Sweden, Tanzania, Turkey, Uganda, United States, Venezuela, Vietnam, and Zimbabwe. GDP99 is the country's GDP per capital in 1999, PRIM79 and SEC79 are primary and secondary education enrollment rates for the eligible school age population in 1979. PI99 is the 1999 price index for investment goods. trust and fair are the (respondent-weighted) country-level averages of response to WVS trust questions. ** indicates significance at 5%, * indicates significance at 10%.

Algan and Cahuc (2010) employ a compelling identification strategy to argue that changes in trust that predate changes in economic development can causally explain those later development outcomes. Since the first WVS wave comes only in 1980, Algan and Cahuc use General Social Survey data from the United States, but examine differences in trust question response for people whose grandparents, parents, or themselves immigrated from different countries. GSS respondents whose parents immigrated in say, 1950, are considered to have inherited the 1950 level of trust in the mother country. Since the cultural transmission estimation is done for first-, second- and third-generation Americans and compared with contemporaneous responses to the corresponding WVS question in the country of origin, Algan and Cahuc then impute time series of trust attitudes for countries going back to the 1930s. Furthermore, they argue that since the imputed attitudes are inherited that they are uncontaminated by reverse causality. This paper corroborates and strengthens the hypothesis that higher levels of trust attitudes predict higher growth.

1.3 COORDINATION AS A CANDIDATE MECHANISM?

Economic explanations for why higher levels of generalized trust cause greater prosperity entail specifying how differences in these attitudes lead individuals to make different decisions in settings where their decisions have economic consequences. Any study of whether people who report themselves to be more trusting on a survey might make different decisions than their low-trust counterparts should be grounded in what those surveyed understand "trust" to mean. One definition offered by Merriam-Webster is "[a] dependence on something future or contingent." This understanding comports with the incentives presented by the Stag Hunt game.

A summary of the stag hunt payoffs may be found in **Table 2**. If both players choose to invest then both will receive high rewards ($7). If a player chooses not to invest, she is given a low reward ($5) regardless of the action of her partner. Players who choose to invest but whose partners decline to invest receive $0. For a rational individual to invest she needs to expect that her partner invests as well (that is, both mutual investment and mutual disinvestment are Nash equilibria). Suppose that a player believes her partner will invest with probability p and decline to invest with probability $1 - p$. Her payoffs from investing and declining to invest, respectively, are $p \times \$7 + (1 - p) \times \$0$ and $5. Thus a risk-neutral player is willing to invest only when the probability her partner invests exceeds $p = 5/7$. Summarizing the strategic uncertainty in such games, Chaudhuri (2009) writes "So in a way, this comes down to a question of trust after all."

Behavioral investigations of coordination games reveal that the Pareto-ranked outcome is only sometimes selected. Devetag and Ortmann (2007) survey the literature on laboratory coordination experiments. They find that coordination is aided by higher expected payoffs from the risky action, low deviation costs, more repetitions, fewer players per game, less randomness in matching, adding players to groups known to have coordinated before, expensive talk, cheap talk, richer communication, and loss-aversion.

Table 2. Payoffs

	Invest	Don't Invest
Invest	7, 7	0, 5
Don't Invest	5, 0	5, 5

1.3.1 The importance of coordination to economic prosperity

There are many enterprises where the success of any one entrepreneur's investment depends on whether enough other people invest as well. The ability to coordinate on efficient investment opportunities has profound relevance to economic growth since new ideas, technologies and physical capital created by coordinated investment will increase the productive capacity of the economy and thus prosperity. Rodrik (1996) models a two-sector economy in general equilibrium. There is a low-technology sector that does not require intermediate inputs but in which the marginal product of labor and hence wages are low. The other sector requires intermediate inputs whose production exhibits increasing returns to scale. Two equilibria exist. One entails only the low-productivity sector being active, since the final producers cannot be profitable without inputs and no firms produce inputs because no downstream firms exist to buy them. For the economy to realize greater productivity all firms must be coordinated.

Coordination among individuals is also essential to the internal functioning of firms. Using coordination games to model productivity in worker teams has ample precedence in the literature. Van Huyck, Battalio and Beil (1990) implement a “minimum effort” game in the laboratory: players in a group can exert costly effort to produce a good, but the level of production is determined by the minimum effort exerted among all members in the group. This game’s multiple equilibria can be Pareto-ranked with higher equilibrium effort levels being more efficient. Social capital has also often been examined in workplace environments where we expect coordination to be important. Adler and Kwon (2002) summarize the literature on the importance of social capital to firms. Social capital strengthens networks that create initial matches, fosters continued success (promotion, reduced turnover), supports research and development and generally encourages positive spillovers.

The observed and plausibly causal link between generalized trust attitudes and economic prosperity, the need for individuals to trust their partners will take complementary actions in order to coordinate on efficient equilibria in Stag Hunt games, and the relevance of coordination to growth and productivity, motivate the following hypotheses:

Hypothesis 1: *Trusting attitudes, as revealed in affirmative responses to the GSS trust questions, are positively associated with taking efficiency-supporting actions in the Stag Hunt game.*

Hypothesis 2: *Trusting attitudes, as revealed in GSS questions, represent optimistic beliefs about partner behavior. Individuals who answer affirmatively to the GSS trust questions think that other people are more likely to take efficiency-supporting actions in the Stag Hunt game.*

1.4 DESIGN

Four sessions were conducted in the Pittsburgh Experimental Economics Laboratory at the University of Pittsburgh. 20 undergraduate subjects per session were recruited to participate. Each session lasted approximately 30 minutes. The 20 participants in each session were seated and asked to complete an anonymous survey of demographic information and personal attitudes. The questions asked are a subset of those used in Glaeser et al. (2000) and are included in the supplementary interface screenshots. Subjects were told that they would participate in a decision-making exercise following the survey, but were not given any specific information on the structure of the game before all had completed the survey. The survey questions were designed to elicit opinions on a variety of topics; care was taken not to prime subjects to think about trust issues nor have their responses to the trust questions be particularly salient in their memories.

The survey questions of primary interest are the standard GSS "trust" question, "fair" question, and the GSS "help" question. While the trust and fair questions are common to the GSS and the WVS and referenced earlier, the help question is unique to the GSS but similar to the other two:

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

I also measure trustworthiness on a 6-point scale and attitudinal risk preferences using a subset of the questions developed by Weber et al. (2002). These are discussed in Section 1.5.2. Following the survey, the Stag Hunt game is described to subjects. **Table 2** shows the monetary payoffs used. The full set of instructions are available upon request.

Ten rounds of the same Stag Hunt game are played with absolute-stranger (turnpike) rematching each round. This partner matching institution was chosen because the GSS questions to be correlated with behavior in this game are designed to measure generalized trust between strangers in a society. Subjects were also instructed that following the first round, but before learning its outcome, they would be asked to guess how many of the other people in the room had invested. Correct guesses were incentivized by awarding $3.00 to anyone who guessed the exact number correctly and $1.50 to anyone who guessed within one person. This was done to measure prior beliefs as accurately as possible, since I hypothesize that more trusting people will have higher expectations that others will invest. All responses were entered anonymously via Fischbacher (2007)'s z-Tree software on the lab's computer terminals.

Two of the ten rounds were selected at random with uniform probability for payment. All participants earned $3.00 for completing the survey on top of their $5.00 show-up fee. Median earnings in all sessions were $18.00. The minimum possible earnings for completing the experiment are $8.00 ($5 show-up fee, $3 survey completion fee and $0 in both selected Stag Hunt rounds) while maximum earnings are $25.00 ($8 + $3 guess reward + 2 × $7 in both selected rounds).

1.5 RESULTS

First-round investment frequency is below 50% in all sessions, and all sessions eventually converge to the risk-dominant equilibrium. This is as expected, since the payoffs used require a risk-neutral person to believe that her partner invests with probability greater than 5/7 for her to want to invest as well. Figure 1 shows how the frequency of investment for each session, and all sessions averaged, evolves across the 10 rounds. Figure 2 shows average investment frequencies by round broken down by affirmative answers to *GSS_trust*, *GSS_fair*, *GSS_help*, and self-reported trustworthiness respectively.

The figures show that participants with affirmative answers to all of the GSS questions invest more often across all rounds. I explain and investigate the significance of these findings in the following subsections.

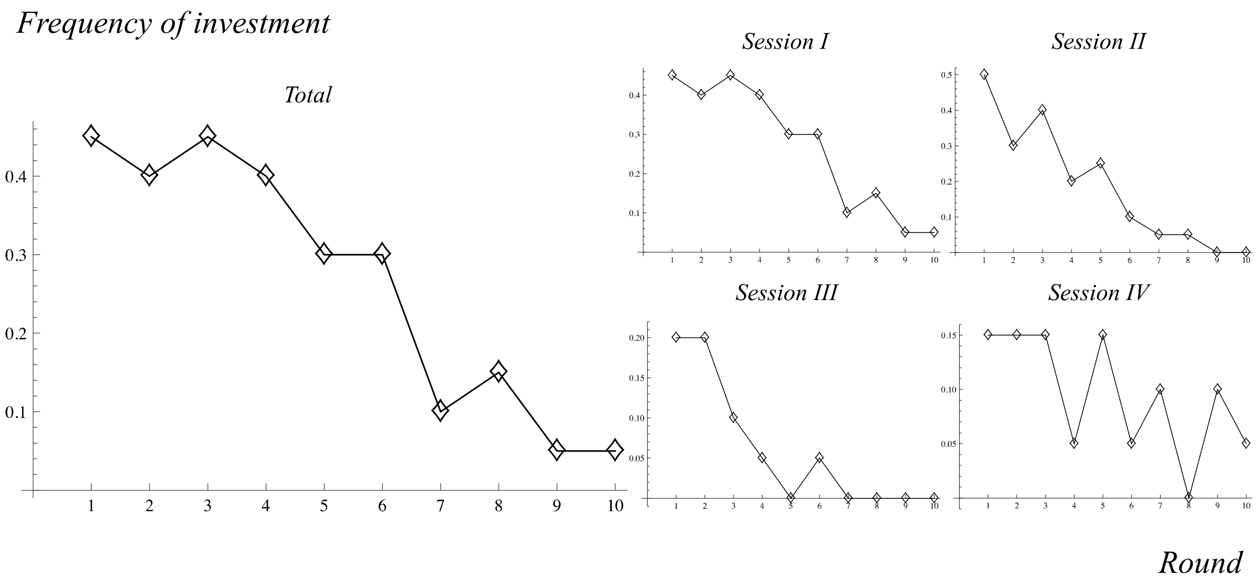

Figure 1. Mean level of investment across rounds in all sessions

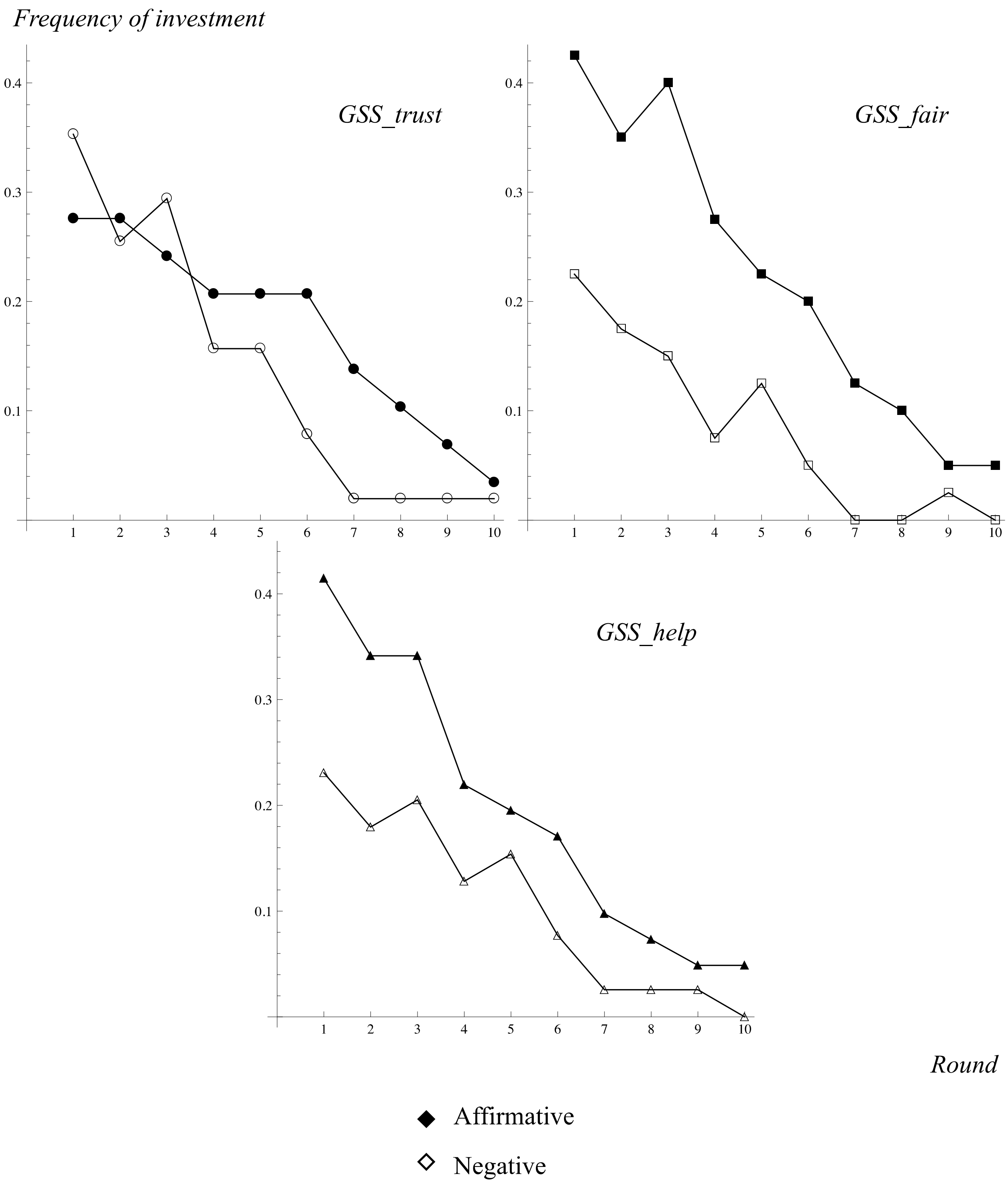

Figure 2. Mean level of investment by response to GSS questions

1.5.1 Beliefs

Table 3 reports subjects' average beliefs that others will invest in the first round, broken down by GSS survey responses as well as first-round investment frequency. Later rounds are contaminated by experience in previous rounds, so the first round is the most clean measure of the influence that beliefs have on the propensity to invest. I later utilize a structural model to incorporate observations from later rounds. Subjects who invest in round 1 expect that 77% of the other subjects in their session will do likewise; those who did not invest in the first round expect that only 19% of fellow session participants would invest that round. Subjects who answer *GSS_trust* in the affirmative expect that 35% of the other subjects in their session will invest in the first round while those who answer in the negative expect that 39% will invest; subjects who answer *GSS_fair* in the affirmative expect that 45% of the other subjects in their session will invest while those who answer in the negative expect that 30% will invest; and subjects who answer *GSS_help* in the affirmative expect that 44% of the other subjects in their session will invest while those who answer in the negative expect that 36% will invest.

Table 3. Beliefs by first-round decision and response to GSS questions

	$Invest_1$	*GSS_trust*	*GSS_fair*	*GSS_help*
Affirmative	.774	.354	.453	.439
Negative	.188	.391	.303	.359

Subject guesses about the probability that others invest in the first round, by investment choices and GSS responses.

To formally test the differences found in Table 3, Table 4 displays nonparametric correlation coefficients and corresponding *p*-values between subject responses to survey measures (*GSS_trust*, *_fair* and *_help*), investing in the first round, and their elicited beliefs that others invest in this round (guess). The results of these simple correlations track the results of Table 3. The belief that others are likely to invest is highly and significantly correlated with investing oneself. The two GSS questions that show significant correlation with prior beliefs are *GSS_fair* and *GSS_help*. The correlation between *GSS_trust* and beliefs is not significantly different from 0. Figure 3 displays cumulative distribution functions of beliefs on how many others invest, broken up by affirmative/negative response to GSS questions. The horizontal axis on these graphs is the elicited belief about how many other people in a subject's session will invest in the first round; and the height of the graph corresponding to *X* on the horizontal axis measures the cumulative proportion of affirmative / negative respondents who report that at least *X* people will invest. For example, in the panel corresponding to the *GSS_fair* question, about 80% of the affirmative responders to GSS_fair believe that at least 10 other people will invest in the first round, while only 60% of negative responders believe that at least 10 other people will invest. These graphs corroborate the findings of Table 3/Table 4. The *GSS_fair* and *GSS_help* questions are associated with expecting more people will invest on the entire distribution of beliefs.

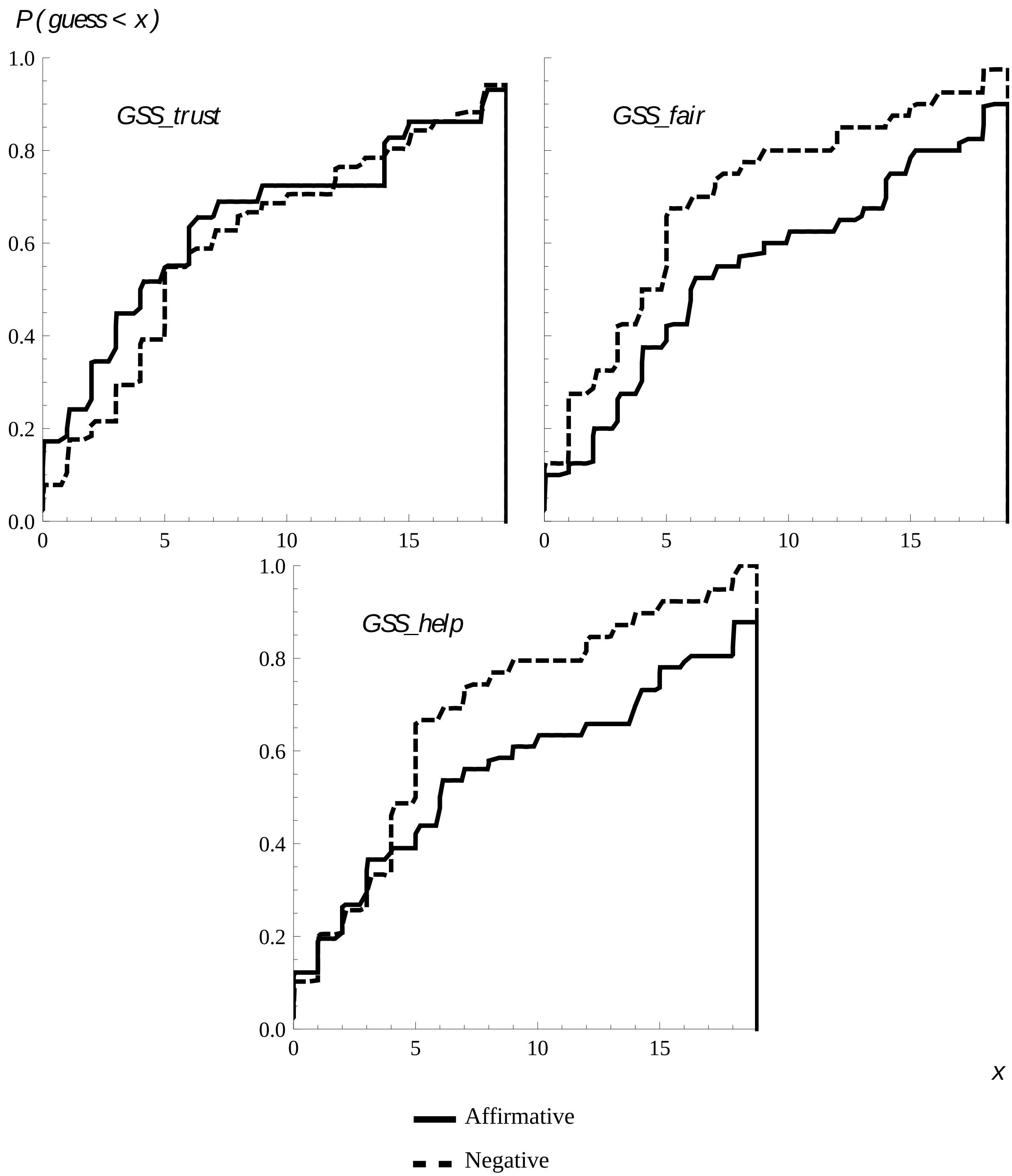

Figure 3. Cumulative distribution of guesses by response to GSS questions

Table 4. Beliefs by first-round decision and response to GSS questions

	$Invest_1$	*GSS_trust*	*GSS_fair*	*GSS_help*	Trustworthy
Guess	.665* (.000)	-.077 (.200)	.187* (.018)	.128* (.091)	.045 (.302)

Kendall-tau correlations between first-round guesses, first-round investment choices and GSS trust questions; * indicates significance at the 10% level; *p*-values are estimated from the simulated empirical distribution of estimates under the null hypothesis.

While results for two of the three GSS questions are significant in the direction predicted by Hypothesis 2, the result on the standard (and most examined in the literature) *GSS_trust* question is not. Figure 3 and Figure 4 suggest that while those with affirmative responses to this question are more likely to invest, it is not due to beliefs that other subjects are more likely to invest. One possible resolution of these conflicting results on *GSS_trust* is that people with affirmative responses to trust attitude questions have preferences that make them more likely to choose investment conditional on beliefs. In Section 1.5.3, I address the consideration that these preferences are related to risk aversion and do not find support for that explanation. Whether these preferences are preferences for efficiency, altruism, as-such preferences for coordination, or otherwise is not knowable from these data. It is however possible to identify these preferences controlling for beliefs and reaction to beliefs by using a simple econometric model.

1.5.2 The interaction with risk aversion

Since I only measure correlations between attitudes and behavior in the Stag Hunt game, it can be argued that these results merely reflect omitted variables bias due to correlation between trust and another determinant of playing Stag. The most obvious such confound is risk aversion, but Neumann and Vogt (2009) show that risk preferences do not account for significant across-subject variation in the Stag Hunt, and here I present evidence that it is not a significant confound in my study either. On the survey, I include the financial risk-seeking measures from Weber et al. (2002), which they show to correlate with the incentivized risk- aversion procedure of Weber, Shafir, and Blais (2004). Figure 4 shows action choices and initial beliefs comparable to those presented in Figure 2 and Figure 3, respectively, but with each outcome measure now the residual from the regression of choices/beliefs on the Weber et al. questions. All qualitative differences between trusters and non-trusters remain unchanged after controlling for risk. To the extent that risk preferences are captured by the index of Weber et al. questions, they appear to not be a confound in this study.

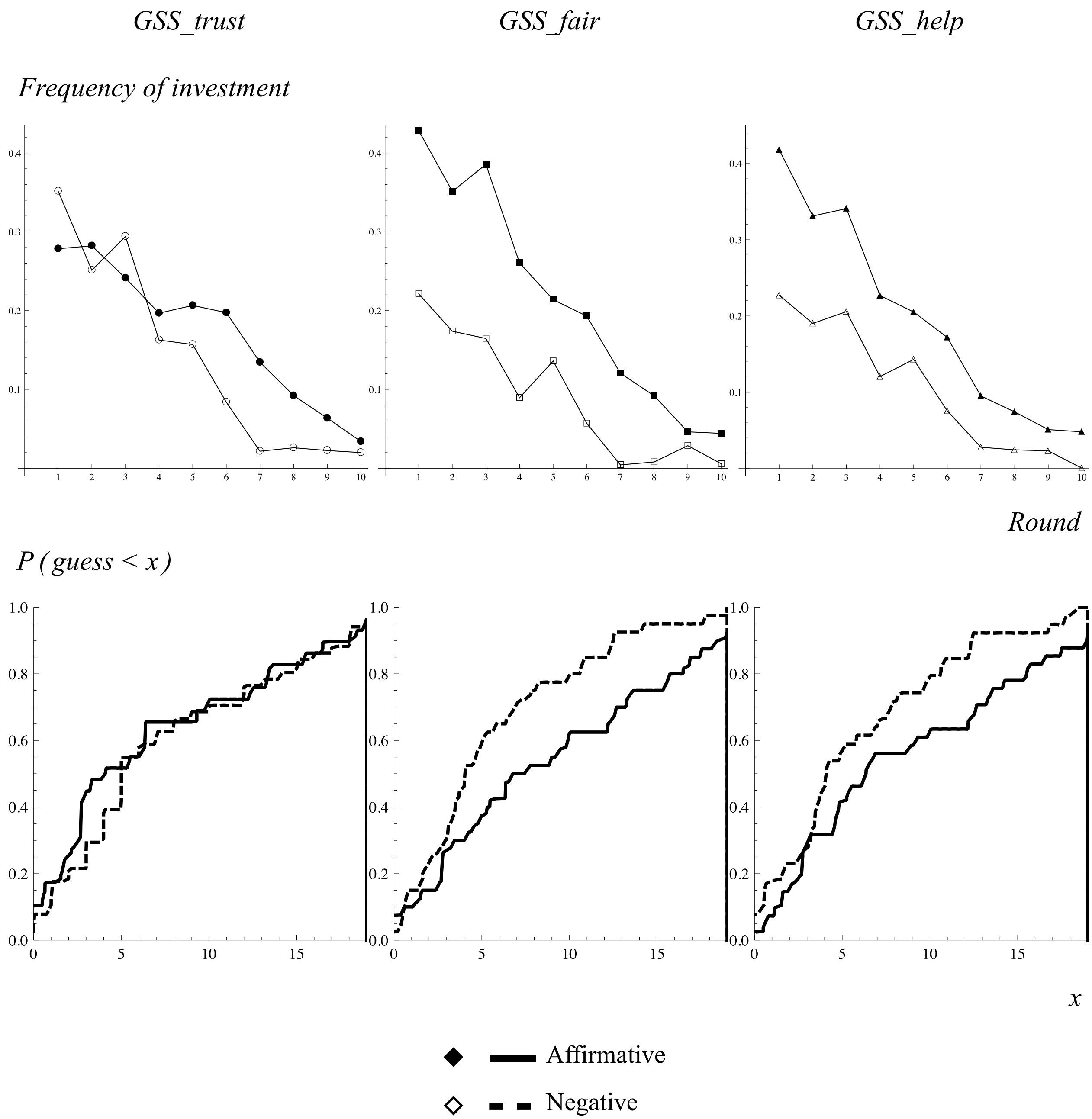

Figure 4. Main results hold after controlling for risk attitudes

1.5.3 Preferences

A way to exploit the power afforded by the repeated setting used in my sessions is to posit a structural model for how agents update and react to beliefs – combining both action and belief data. I assume that the probability that subject i attaches to her partner investing in round t is given:

$$q_{it} = \frac{q_{0i} + I_{i,1} + \cdots + I_{i,t-2} + I_{i,t-1}}{t}$$

where $I_{i,t}$ takes on the value 1 if subject i's partner invests in round t and 0 if her partner does not and q_{0i} is subject i's belief about the probability that others invest in the first period. Given these, player i adopts a cutoff strategy such that she invests when $q_{it} + \varepsilon_{it} > c_i$ for $0 < c_i < 1$ and $E(\varepsilon_{it}|q_{it}) = 0$. Intuitively, this means that players invest when they are reasonably confident that others will invest (their belief exceeds their cutoff c_i), and decline to invest if they believe others unlikely to invest (belief does not exceed c_i). The c_i term parameterizes just how confident player i must be that others invest to want to invest herself: players with high c must be very confident that others invest before they follow suit while those with lower c require less reassurance that others invest before they are willing to. The model with $\varepsilon_{it} = 0$ is a reduced form of a strategy that best responds to beliefs under very generic preference specifications (risk or social preferences may affect the utility from the game payoffs, but to the extent that the game remains a stag hunt in utilities, players still employ cutoff strategies in their beliefs). Relaxing $\varepsilon_{it} = 0$ and specifying an extreme value distribution allows for non-systematic decision errors and allows us to estimate the c_i using logistic regression.

Since I am primarily concerned with individual-level heterogeneity, I estimate different parameters for each subject and examine whether those subjects with high trust measures feature greater baseline willingness to invest (conditional on beliefs) as revealed in their estimate of c_i. These are computed by estimating logistic regressions of each subject's investment decisions on the q_{it}. If the subject's estimated (latent) regression function is $\hat{y}^*_{it} = \beta_{0i} + \beta_{1i} q_{it}$, then a reasonable estimate of c_i can be computed by $-\beta_{0i}/\beta_{1i}$. The estimates are summarized in Table 5.

Table 5. Cutoffs at which we expect indifference between investing & not, by GSS question

	GSS_trust		*GSS_fair*		*GSS_help*	
	Affirm.	Negative	Affirm.	Negative	Affirm.	Negative
P (Partner invests)	.558	.436	.448	.615	.502	.502
2-tail MW p-value	.026		.008		.408	
Corr(Cutoff, Guess)	-.061	.161	.047	.047	.063	.137
	(.340)	(.049)	(.327)	(.341)	(.286)	(.111)

Average probability of their partner investing at which subject groups are indifferent between investing and not investing. these were estimated individually by logit regression on beliefs/history of play. If $\hat{y}_i^* = \beta_{0i} + \beta_{1i}\, belief_i$ is the estimated regression for subject i then the belief at which they are indifferent between investing and not is $-\beta_{0i} / \beta_{1i}$. Mann-Whitney ranksum tests between affirmative and negative responders to the GSS questions were performed on the resulting estimates and p-values are reported below the (averaged) estimates. Corr(Cutoff, Guess) is the estimated kendall tau correlation between subject guesses about how many people invest in round 1 and and their estimated cutoffs; two-tailed p-values for the null hypothesis that Corr(Cutoff, Guess) = 0 are in parentheses.

Subjects who answer in the affirmative to *GSS_trust* have significantly higher thresholds than their negatively responding counterparts, while affirmative respondents to *GSS_fair* have a significantly lower threshold required to invest than their negatively-responding counterparts. Specifically, while affirmative respondents to *GSS_trust* will only invest when they believe others will invest with greater than probability .56, negative respondents invest when they believe their partners invest with at least .44 probability. Affirmative respondents to *GSS_fair* will invest when they believe others invest with probability at least .45, negative respondents only invest when they believe their partners invest with at least .62 probability. There are no significant differences in thresholds between affirmative and negative responders to *GSS_help*. Mann-Whitney rank sum p-values are reported in Table 5.

While the estimated thresholds indicate that affirmative responders to *GSS_fair* would be more likely to invest even controlling for their higher beliefs that others invest; affirmative responders to *GSS_trust* have neither higher beliefs that others invest nor greater willingness to invest controlling for beliefs. Why then do affirmative responders to *GSS_trust* appear to invest more often on average than their negatively-responding counterparts? The third row of Table 5 provides an explanation. The only significant correlation found between beliefs that others invest in round 1 and subjects' own threshold required to invest is for negative responders to *GSS_trust*. Intuitively, this means that while the negative respondents to *GSS_trust* seem to have similar beliefs and lower threshold cutoffs as the affirmative respondents, they appear to believe that other subjects have significantly higher thresholds than they do; this may explain why affirmative responders to *GSS_trust* invest more frequently.

1.6 DISCUSSION

Evidence from the Stag Hunt experiment I conduct provide preliminary evidence that social capital, as measured through trust questions, predicts coordination on Pareto-dominant equilibria. Hence, surveys on trust measure an important facet of social capital: coordination. It is furthermore clear that players' expectations of what other players will do is a significant predictor of behavior and that two of the GSS questions capture this motive. In addition, the standard trust question, as well as *GSS_fair* (both also on the World Values Survey) predict a preference for coordinating conditional on beliefs. The qualitative findings on coordination preferences and beliefs by question are consistent with Thöni et al. (2010) as well. The finding that trust attitudes are operative in the Stag Hunt also sheds light on why trust surveys are found to have a relationship to contributions in public goods games (Anderson, Mellor and Milyo, 2004; Thöni, et al., 2010). The findings here corroborate those in the Thöni et al. study to a great extent. In that paper, only the *GSS_trust* (and not *_fair*) question is found to correlate with higher contributions to a linear public good, but that only the *GSS_fair* question (and not *_trust*) is positively related to beliefs about others' contributions. Coordination and linear public good with conditional contribution opportunities share many common features, and in fact most of Thöni et al.'s subjects have contribution schedules that imply they are effectively playing a coordination game. Since solving both free-riding and coordination problems are necessary in the provision of public goods, my paper contributes to the literature on why more trusting people are able to provide more public goods. This leaves open the question of how these two environments (public goods and the Stag Hunt) differ from the classical trust game of Berg et al. (1995), where the relationship between survey and behavioral trust is weak or absent.

Lastly, it should be the goal of this research agenda to take what we know about how social capital operates back into the field. If we can establish both that exogenous increases in trust improve economic outcomes, and that these positive outcomes do indeed feed back into trust, we will be one step closer to explaining elusive cross-country variance in development and human welfare.

2.0 THE IMPORTANCE OF HIGHER-ORDER BELIEFS TO SUCCESSFUL COORDINATION

Beliefs about other players' strategies are crucial in determining outcomes for coordination games. If players are to coordinate on an efficient equilibrium, they must believe that others will coordinate with them. In many settings there is uncertainty about beliefs as well as strategies. Do people consider these 'higher-order' beliefs (beliefs about beliefs) when making coordination decisions? I design a modified Stag Hunt experiment that allows me to identify how these higher-order beliefs and uncertainty about higher-order beliefs matter for coordination. Players prefer to invest especially when they believe that others are 'optimistic' that they will invest; but knowledge that others think them unlikely to invest does not cause players to behave differently than when they do not know what their partners think about them. Thus resolving uncertainty about beliefs can result in marked efficiency gains.

2.1 INTRODUCTION

A vast swathe of economic activity is achieved by coordination among agents, sustained in equilibrium by mutually reinforcing beliefs. Product manufacturers expect inputs to be produced by upstream firms, who in turn must count on the custom of these downstream manufacturers to profitably produce those inputs. Potential investors in a start-up venture will only invest if they expect that the entrepreneur will successfully raise capital from other investors like them. Entire economies can fall into traps where expectations of low incomes lead firms and individuals to forego production and investment that could be profitable under a different set of expectations.

For concreteness, let us focus on a model that captures the strategic incentives present in the situations above in their most essential form: a stag hunt game. In a stag hunt game, players have the opportunity to invest in a speculative venture that will be profitable only if a sufficient proportion of other players do likewise. If a player has a sufficiently strong belief that others will invest, then she will also want to invest, making mutual investment a Nash equilibrium. If she places low probability on others investing, then she will also decline to invest. Thus mutual disinvestment is also a Nash equilibrium.

How do people form the expectations that lead them into better or worse equilibria? The introspective player will reason that since potential partners face the same incentives that she does, they will invest when they expect *her* to invest. In this way, her beliefs about others' actions (what game theorists term *first-order beliefs*) are crucially informed by her beliefs about others' beliefs (*second-order beliefs*, naturally).

Do people make these considerations when deciding whether to invest or not in stag hunt games? Experimental implementations of *global games*, specifically stag hunt games with uncertainty about payoffs, should provide us some evidence (Heinemann et al., 2004; Cabrales et al. 2007; Heinemann and Cornand, 2011). Uncertainty about payoffs to coordination can, under Bayesian reasoning, select a unique equilibrium (Carlsson and Van Damme, 1993). This works because when a subject has low beliefs about the fundamental payoffs, she expects others do as well, that those other players are unlikely to invest, and that they are making the same consideration about her. Global games experiments generally do not observe strong differences across information treatments however.

I design a modified stag hunt game that features exogenous uncertainty about *actions* rather than payoffs. Players' actions are subject to exogenous perturbations with objectively known probabilities. I experimentally manipulate players' expectations that their partner will invest in the modified stag hunt but revealing information about the exogenous perturbations (first-order beliefs); this allows me to also manipulate players' second-order beliefs by revealing what their partners know about *their* probability of investment. Since the perturbations are independently assigned, I can identify how people respond to induced changes in their second-order beliefs. Careful elicitation of subjects' first- and second-order beliefs confirms that the experimental treatments operate through the hypothesized belief mechanism. The paper proceeds as follows: existing literature on stag hunt games is surveyed in Section 2.2, Section 2.3 outlines a model that predicts how rational players respond to changes in second-order beliefs, Section 2.4 describes the design of the experiment in detail, Section 2.5 summarizes the collected data, Section 2.6 presents the results of the experiment and Section 2.7 concludes.

2.2 RELATED LITERATURE

The theoretical and experimental analysis of coordination shows that beliefs are an extremely important determinant of behavior in coordination games (Devetag and Ortmann, 2007); but to not model where they come from is akin to saying that technology is an important determinant of growth but neglecting to model whence technological progress stems – indisputably true but nevertheless unsatisfying. Is common knowledge of a belief important in sustaining that belief?

This question is of great importance in organizing how we think about coordination in heterogeneous societies, but we cannot answer it without knowing how higher-order beliefs influence human decision making.

The importance of higher-order beliefs to coordination is closely related with the notion of how global games (Carlsson and van Damme, 1993) yield equilibrium refinements in these games. This theory states that under an arbitrarily small level of uncertainty about the game payoffs, perfect higher-order reasoning implies that only the risk-dominant equilibrium survives iterated deletion of dominated strategies. In a "global" Stag Hunt investment game, players do not know the precise payoff from investing, but share a common prior belief distribution about what it is. Each player receives private information that narrows the possible range of investment payoffs. Morris and Shin (2003) emphasize that global games yield equilibrium selection results precisely because they generate higher-order uncertainty about partner strategies. Global games shed light on the role that higher-order beliefs play in coordination because, to the extent that people reason in higher orders, they should react to the higher-order beliefs they form. Different private signals in a global game induce changes in higher-order beliefs because a player cannot know what other players think about the signal that she herself received. In a Stag Hunt global game with payoff uncertainty and private signals about payoffs, if I observe the payoff from coordinated investment to be high, there is still some probability that my partner thinks that I observed it to be low. The distribution of the signal induces a distribution of higher-order beliefs given preferences. If preferences are risk-neutral, Bayesian players should invest only when investment is risk-dominant.

We cannot identify how higher-order beliefs influence human decision making without the level of control and observation permissible in a laboratory experiment. Unfortunately, the experimental results from global games call into question whether human subjects respond to induced changes in higher-order beliefs. Heinemann, Nagel, and Ockenfels (2004) test the comparative static predictions of global games theory in an *n*-player Stag Hunt by comparing sessions with public signals about the threshold number of players required to support the risky action to those with private signals of this parameter. While many comparative statics are consistent with global games theory, they do not find significant differences between public and private information conditions. Cabrales, Nagel, and Armenter (2007) examine Stag Hunts with deterministic versus random payoffs with private signals. They find significant differences between the public and private information treatments for only one of two parameterizations. The global games literature provides very weak evidence on the impact of higher-order beliefs. Neither Heinemann, Nagel, and Ockenfels nor Cabrales, Nagel, and Armenter find comparative statics that are not also consistent with players simply preferring to invest when payoffs from successful coordination are higher. Heinemann and Cornand (2011) conduct a similar study in which agents' optimal choices in the Morris and Shin (2002) game are a mix between a public and private signal. They find subjects under-react to the public relative to the private signal and hypothesize that finite levels of higher-order reasoning may be responsible.

The problem with implementing global games as a way to examine the role of higher-order beliefs in coordination games is that higher-order beliefs are only changed by manipulating payoffs to investment, and there is a long literature showing the effects of payoff changes on investment behavior in these games (again, see Devetag and Ortmann, 2007 for a comprehensive review). It is important to establish both that human reasoning about coordination takes place at higher orders, and to investigate how this reasoning works by directly examining changes in beliefs. In the next section I lay out an innovative experimental design that manipulates second-order beliefs without changing investment payoffs and measures precisely how induced changes in second-order beliefs affect first-order beliefs and investment behavior.

2.3 THEORETICAL SETUP

In this section I sketch a simple argument demonstrating how higher order beliefs influence investment behavior in a coordination game. Consider the Stag Hunt game of Table 6:

Table 6. A Stag Hunt game with $H > L > 0$

	Invest	Don't Invest
Invest	H, H	0, L
Don't Invest	L, 0	L, L

Each agent chooses to 'invest' or 'not invest'. If both agents invest then both will receive a high return H. If an agent chooses not to invest, she earns a low return L regardless of whether the other person invests or not. Agents who invest but whose counterparts do not will lose their investment. Both *Invest*, *Invest* and *Not invest*, *Not invest* are Nash equilibria, and though only *Invest*, *Invest* is efficient, a risk-neutral player must believe that her counterpart invests with probability greater than L/H to want to invest. What considerations lead to these beliefs? In classical game theory, beliefs are consistent in equilibrium and yield infinite hierarchies of beliefs (Harsanyi, 1967-68). In the context of coordination, a player will consider her partner more likely to invest if she expects that her partner expects her to invest. If and only if a rational player's belief that the other will invest exceeds some threshold determined by her risk preferences will that player invest[3]. This consideration forms the basis for reasoning about behavior in this game. Thus, for a player to form a first-order belief that models her partner's investment choice, she must form a belief about her partner's beliefs and about her partner's preferences. I take as given, a belief about her partner's belief and her partner's preferences, which in turn induce her first-order belief (the probability she thinks that her partner will invest). Of course we may similarly ask where second-order beliefs come from, and posit (like Harsanyi) that if an extra step of strategic reasoning takes place, then second-order beliefs are themselves derived from third-order beliefs and what players think their partner thinks about their preferences. There is an extensive literature that models human strategic behavior as comprising a finite but arbitrary number of iterated reasoning steps (I think that you think that I think that you think, etc.) (e.g. Stahl and Wilson, 1994; Camerer, Ho, and Chong, 2004). The theoretical argument for why second-order beliefs induce first-order beliefs (to be laid out formally in this

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[3] We maintain the following assumptions: subjects treat the choice to invest as a simple lottery with subjective probabilities over the possible outcomes (0 or *H*); players prefer to invest when their partner invests with probability 1 and prefer not to invest when there is 0 probability their partner invests (each lottery first-order stochastically dominates the other in these two cases); and players' preferences obey the continuity axiom of von Neumann and Morgenstern. Since not investing is preferred to investing when your partner fails to invest, and investing is preferred to not investing when your partner invests, the continuity axiom implies that there is some probability of one's partner investing below which players will prefer not to invest, above which players will prefer to invest, and at which they will be indifferent between investing and not investing.

section) is exactly mirrored in how third-order beliefs would induce second-order beliefs (and fourth third, etc.) if additional reasoning steps take place. This paper examines whether people reason at the second-order in coordination, and so for illustrative purposes we take a player's second-order belief as given.

Since the investment game is symmetric, without loss of generality we consider the decision of one agent who we denote Investor 1, in reaction to her counterpart investor, who we denote Investor 2. Let Investor 1 have second-order belief ρ_1, i.e. Investor 1 expects that Investor 2 thinks that Investor 1 will invest with probability ρ_1, and fail to invest with probability $1 - \rho_1$. Investor 1 is uncertain of the exact parameterization of Investor 2's preferences. Investor 2 will invest if she thinks that Investor 1 will invest with at least probability c_2, and decline to invest if she thinks that Investor 1 will invest with less than probability c_2. Investor 1 of course does not know what c_2 is, but has some sense of what it is that we formally model with cumulative distribution function F_1. ρ_1 and F_1 induce Investor 1's first-order belief that Investor 2 will invest: Investor 1 believes that Investor 2 will invest with probability $F_1(\rho_1)$, and will fail to invest with probability $1 - F_1(\rho_1)$. Since F_1 is a cumulative distribution function, it is clear that Investor 1's first-order belief $F_1(\rho_1)$ is increasing in her second-order belief ρ_1. Investor 1, adopting a cutoff strategy herself, will invest if she believes that Investor 2 will invest with probability at least c_1[4]. Player 1 will invest if $F_1(\rho_1) \geq c_1$ and decline to invest if $F_1(\rho_1) < c_1$.

There exists threshold ρ_1* such that Investor 1 will invest if her second-order belief, ρ_1, is at least ρ_1* and will decline to invest if $\rho_1 < \rho_1$*.

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[4] Note that the three Nash equilibria of this game are *Invest*, *Invest*; *Not invest*, *Not invest*; and Investment with probabilities c_2 and c_1., respectively.

Sketch of proof: Let F_1 be continuous. Since not investing is preferred to investing if Investor 2 invests with less than c_1 probability, and investing is preferred to not investing when Investor 2 invests with probability c_1 or greater, then the continuity axiom and the intermediate value theorem imply that there is some probability $F_1(\rho_1^*)$ of Investor 2 investing below which Investor 1 prefers not to invest, above which she prefers to invest, and at which she is indifferent.

In Section 2.4, I lay out an experimental design for the investment game that can induce changes in second-order beliefs while holding constant all other strategic elements.

2.4 EXPERIMENTAL DESIGN AND PREDICTIONS

I develop four treatments to experimentally induce variation in second-order beliefs and analyze their impact on investment. All treatments implement a modified version of the Stag Hunt game whose payoffs are given in Table 7.

Table 7. Payoffs

	Invest	Don't Invest
Invest	18, 18	0, 10
Don't Invest	10, 0	10, 10

Subjects must choose to either invest or not invest. No investment guarantees a payoff of $10, while investment only pays when the subjects' partner does likewise. The subject must expect her partner to invest to choose it over not investing, which carries no risk. The actions chosen by each player are subject to uncertainty: chosen actions only sometimes correspond to the same final action. Final actions determine payoffs. That is, there is some probability that those who intend to invest fail to successfully do so, and those who intend not to invest may find themselves by twist of fate in fact investing. This process is described to subjects as the outcome of a fair coin flip and an independent six-sided die roll. If the coin comes up heads then that person's investment probability is high: if a 5 or 6 is rolled, the person's final action will be investment; 1-4 do not modify the player's chosen action. If the coin comes up tails then that person's investment probability is low: if a 5 or 6 is rolled the final action will be non-investment; 1-4 do not modify the player's chosen action. Table 8 summarizes how noise is operative in all possible cases:

Table 8. The *action* carried out as a function of subjects' *choice* and uncertainty

		Die roll					
		1	2	3	4	5	6
Coin flip	Heads (high investment probability)	*Choice* is not modified				*Invest*	
	Tails (low investment probability)					*Not invest*	

The uncertainty in how subjects' chosen actions are carried out as final actions is necessary to induce controlled changes in beliefs. The difference between treatments lies in the information investors have about whether their partner's investment probability is high or low, as well as the information they have on their own investment probability. In Treatment *BK* (*both know*), subjects know their own investment probability and their partners know this as well; in Treatment *PK* (*partner knows*) subjects do not know their own investment probability but their partner does (and by symmetry their know their partner's); in Treatment *IK* (*I know*) subjects know their own investment probability but their partner does not (nor they their partner's); and in Treatment *NK* (*neither knows*) neither subject nor their partner knows their investment probability. Subjects are made fully aware of the information structure of the treatment they participate in. Beliefs are elicited before information about the round is revealed. Table 9 summarizes the design of this experiment. Knowing whether one's partner has a high or low investment probability will make a potential investor more optimistic or pessimistic that the partner will indeed invest. Since the attractiveness of investing is weakly increasing in the belief that one's partner invests, optimistic investors will be more likely to invest than investors who don't know their partner's investment probability, who in turn will be more likely to invest than pessimistic investors. Less obviously, knowing one's own investment probability can induce what shall henceforth be referred to as second-order-optimism (second-order-pessimism): the knowledge that your partner is optimistic (pessimistic) about you.

Treatments *BK* and *PK* implement three distinct information sets that differ only in what subjects know about their partner's beliefs (partner's beliefs optimistic, partner's beliefs pessimistic, partner's beliefs unknown). If investors have strategic models of their partners, then they should recognize that differing beliefs held by their partner will cause that partner to be more or less likely to invest herself. Treatments *IK* and *NK* implement appropriate controls for Treatments *BK* and *PK* because the same information provided in Treatment *BK* (relative to not knowing this information in Treatment *PK*) does not give rise to changes in beliefs that are predicted to generate a behavioral response. Investors should not condition on their own investment probability because (since their partner cannot see this) it does not change their partner's beliefs. Hence using the 2-by-2 treatment design we will be able to test for the role of second-order beliefs. While information on a person's own investment probability should have no effect on behavior in the *IK*/*NK* comparison, it should have a substantial effect in the *BK*/*PK* comparison.

Table 9. Information structure of the treatments

	Do you know…	
	Your investment prob.	Partner's investment prob.
Treatment *BK*	YES	YES
Treatment *PK*	NO	YES
Treatment *IK*	YES	NO
Treatment *NK*	NO	NO

2.4.1 Predictions

Invest, Invest and *Not invest*, *Not invest* remain Nash equilibria under all joint investment probability realizations in all treatments if preferences are risk-neutral. *Invest*, *Invest* also remains efficient relative to *Not invest*, *Not invest* since expected payoffs in the *Invest*, *Invest* equilibrium are higher than those in the *Not invest*, *Not invest* equilibrium. Our simple model of Section 2.3 additionally generates out-of-equilibrium predictions (i.e. when beliefs are not consistent). Predictions are broken out by treatment. Without loss of generality, we consider Investor 1 playing the modified coordination game of this section with Investor 2. Investor 1 has incomplete knowledge of Investor 2's beliefs as well as incomplete knowledge of what probability would make Investor 2 indifferent between investing and not in- vesting; but has subjective prior beliefs that we denote ρ_1 and F_1 as before.

Let us examine the predicted behavior in each of the treatments. We start with Treatment *NK* since players receive no information on investment probabilities in that treatment. Note now that since final actions and not chosen actions are strategically relevant, Investor 1 should think that Investor 2 expects her to (finally) invest with probability $\frac{2}{3}\rho_1 + \frac{1}{6}$. Investor 2 will invest if and only if $\frac{2}{3}\rho_1 + \frac{1}{6} > c_2$, so the probability that Investor 2 will choose to invest is $F_1(\frac{2}{3}\rho_1 + \frac{1}{6})$ and hence the final probability that Investor 2 invests is $\frac{2}{3} F_1(\frac{2}{3}\rho_1 + \frac{1}{6}) + \frac{1}{6}$.

In Treatment *IK*, Investor 1 learns her own investment probability, but knows that this information is not known by Investor 2. Her belief in the final investment probability of Investor 2 therefore remains $\frac{2}{3} F_1(\frac{2}{3}\rho_1 + \frac{1}{6}) + \frac{1}{6}$.

In Treatment *PK*, Investor 1 learns what investor 2's investment probability is. Since Investor 2 does not know her own investment probability, Investor 1's model of Investor 2's beliefs should remain unchanged relative to baseline ($\frac{2}{3}\rho_1 + \frac{1}{6}$). Investor 2's final investment probability depends on her investment probability, which Investor 1 knows. Suppose that she observes Investor 2's investment probability to be high and so she is optimistic. This means that the final investment probability is $\frac{2}{3} F_1 (\frac{2}{3}\rho_1 + \frac{1}{6}) + \frac{1}{3}$. If Investor 1 had instead observed Investor 2's investment probability to be low, then she would be pessimistic and her belief would be $\frac{2}{3} F_1 (\frac{2}{3}\rho_1 + \frac{1}{6})$.

In Treatment *BK*, Investor 1 learns her own investment probability, which means that her posterior weight on Investor 2 investing depends on both her own investment probability and Investor 2's. Since Investor 1 knows what Investor 2 knows about her investment probability, this will be incorporated into her model of Investor 2's beliefs. If her own investment probability is high, then Investor 2 will have belief $\frac{2}{3}\rho_1 + \frac{1}{3}$ and if it is low the belief will be $\frac{2}{3}\rho_1$. We denote these as second-order optimism and second-order pessimism, respectively. High vs. low partner investment probability changes Investor 1's first-order beliefs by the same reasoning as in Treatment *PK*. The final investment probability for Investor 2 in all treatment is given in Table 10. Since the likelihood of investment is weakly increasing in the belief that one's partner will invest, our belief predictions generate the following comparative static predictions for investment.

Hypothesis 1: *All else equal, players who are second-order optimistic in Treatment BK will be more likely to invest than players in Treatment PK who have similar information about their partner's investment probability. On the other hand, players who are second-order pessimistic will be less likely to invest than their counterparts in Treatment B.*

Hypothesis 2: *All else equal, subjects in Treatment IK invest with the same frequency as subjects in Treatment NK. While subjects in Treatment IK observe their own investment probability, this* *induces neither second-order-optimism nor second-order-pessimism as subjects realize that their partner does not know this information.*

Table 10. Player 1's beliefs that her partner's final action is investment, by her own investment probability and her partner's investment probability in all treatments

	Player 1's investment probability	Player 2's investment probability	
		High	*Low*
Treatment *BK*	*High*	$\frac{2}{3}F_1\left(\frac{2}{3}P_1+\frac{1}{3}\right)+\frac{1}{3}$	$\frac{2}{3}F_1\left(\frac{2}{3}P_1+\frac{1}{3}\right)$
	Low	$\frac{2}{3}F_1\left(\frac{2}{3}P_1\right)+\frac{1}{3}$	$\frac{2}{3}F_1\left(\frac{2}{3}P_1\right)$
Treatment *PK*	*High*	$\frac{2}{3}F_1\left(\frac{2}{3}P_1+\frac{1}{6}\right)+\frac{1}{3}$	$\frac{2}{3}F_1\left(\frac{2}{3}P_1+\frac{1}{6}\right)$
	Low		
Treatment *IK*	*High*	$\frac{2}{3}F_1\left(\frac{2}{3}P_1+\frac{1}{6}\right)+\frac{1}{6}$	
	Low		
Treatment *NK*	*High*	$\frac{2}{3}F_1\left(\frac{2}{3}P_1+\frac{1}{6}\right)+\frac{1}{6}$	
	Low		

2.5 DATA

Sessions of each treatment have subjects play 10 rounds of the modified Stag Hunt with random rematching between rounds. Eight sessions were conducted in the Pittsburgh Experimental Economics Laboratory (PEEL) at the University of Pittsburgh. Three sessions each of treatments *BK* and *PK* are implemented, as are one session each of treatments *IK* and *NK*[5]. Twenty undergraduate subjects per session were recruited to participate. The total number of subjects per treatment is shown in Table 11. All subjects were recruited from the general population of the

[5] Per Table 10, Treatment *BK* has four times as many strategically distinct cells as Treatments *IK*/*NK*, and twice as many as Treatment *PK*. Sample sizes were chosen with the goal of distinguishing predicted treatment differences with reasonable power.

PEEL participant database. Each session lasted approximately 80 minutes. The 20 participants in each session are seated and read the instructions. Following the instructions there is a short comprehension quiz that all subjects are required to complete before any decision-making begins. All participants earn a $5.00 show-up fee.

Table 11. Sessions

Treatment	*BK*	*PK*	*IK*	*NK*
# of subjects	3	3	1	1
# of subjects per session	20	20	20	20
Total # of subjects	60	60	20	20
# of rounds per session	10	10	10	10

The instructions start by telling subjects that they will participate in 10 rounds, and that each round consists of a Decision Task, followed by Estimation Task 1, and then by Estimation Task 2. Only one component of each round is paid: one randomly selected round of the Decision Task, a different randomly selected round of Estimation Task 1, and yet another randomly selected round of Estimation Task 2[6]. Then the Decision Task (the investment game) is described to subjects. Subjects are matched with a randomly selected partner from the room each round. Table 7 shows the monetary payoffs used. The full set of instructions are available upon request.

Subjects are made aware of the transformation of their choices into final actions, and what information they will be able to see. Again, in Treatment *BK* subjects are informed whether

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[6] This design was chosen primarily to minimize the concern that subjects can hedge by stating beliefs different from their true ones. Subjects' payoff from the Decision Task of round t depends on the probability their partner chooses to invest that round, denoted p_t: $\frac{2}{3} p_t + \frac{1}{6}$. Note that $Corr(\frac{2}{3} p_t + \frac{1}{6}, p_t) \geq Corr(\frac{2}{3} p_t + \frac{1}{6}, p_{-t})$ for beliefs p_t, p_{-t} that differ *at all* between periods t, $-t$. Higher correlation between payoffs presents a greater incentive to hedge.

their and their partner’s investment probabilities are high or low before making their choices, while in Treatment *PK* they are only informed of their partner’s investment probability. In Treatment *IK* they are only informed of their own investment probability; and in Treatment *NK* they do not learn any investment probability information before making their decisions. The joint sequence of partner matchings, investment probability realizations and whether choices actually become modified before they are carried out as final actions was randomly generated ex-ante and was constant across sessions.

Following the decision task in Treatments *BK*/*PK* subjects are told how many people in the room (besides themselves) are optimistic, and how many are pessimistic. Subjects are asked to guess the number of each group choosing to invest in the preceding Decision Task. If they guess the exact number of people who actually chose to invest among people who are optimistic they earn $1.50, otherwise they earn $0. Likewise if they guess the exact number of people who actually chose to invest among people who are pessimistic they earn $1.50, otherwise they earn $0. In Treatments *IK*/*NK* subjects are simply asked how many people in the room invested (as all subjects are neither optimistic nor pessimistic in Treatments *IK*/*NK*) and rewarded $3.00 if and only if their guess is correct[7]. We take the elicited fractions choosing to invest as measures of subjects’ probabilistic beliefs that their partner will invest in the relevant case[8]. Subjects are also

[7] We do not elicit counterfactual beliefs about how many people *would* invest if they knew their partner's investment frequency as no one in sessions of treatments *IK* and *NK* ever had this information – making incentivized elicitation impossible.

[8] Since all subjects are matched randomly we can think of this number as the probability that one’s partner will choose to invest since partners are a random draw from this set of 19. This

reminded what their guesses imply about the potentially modified final investment actions of their partners.

Estimation Task 2 asks subjects what they believe other people responded to Estimation Task 1. All of the possible responses to each component of Estimation Task 1 are listed in rows, and subjects must guess the number of people in the room (themselves excluded) that gave that response to Estimation Task 1[9]. The computer software ensures that subjects' responses add up to the total number of other people in the room. Like in Estimation Task 1, if subjects guess the actual number of people giving a particular response then they earn $0.25 per correct guess but $0 otherwise. There are 22 total such guessing tasks in Estimation Task 2, meaning that subjects could earn up to $5.50 for exactly correct second-order guesses.

Average earnings in all sessions were $17.42 including show-up payment. The minimum possible earnings for completing the experiment is the $5 show-up payment (everyone made more than this) while maximum earnings are $31.50 ($5 + $18 in the Decision Task + $3 in Estimation Task 1 + $5.50 in Estimation Task 2).

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method of belief elicitation is preferable to a proper scoring rule (e.g. quadratic scoring) because scoring rules are only incentive compatible under very specific forms of risk preferences. Incentivizing only correct guesses is robust to risk aversion. A proof of the incentive-compatibility of this elicitation mechanism may be found in Appendix A.

[9] This elicitation mechanism draws strong inspiration from the work of Neri and Manski (2012).

2.6 RESULTS

As we seek to understand the decisions and beliefs of human investors, we refer here only to chosen actions and elicited beliefs about these choices, not final actions that may result from a modification of that choice.

2.6.1 Summary

Summary graphs of investment frequency can be found in Figure 5. There are four possible joint realizations of own and partner investment probabilities for any given subject. Investment frequencies are reported by which of these cases a given decision was made under; note that a subject will face different cases across rounds so round-to-round variability in each series is partially driven by variable composition of the group of subjects facing that particular noise realization (though the sequence of noise realizations across all sessions was identical).

In Treatment *NK*, subjects are unaware of both their own investment probability and that of their partner. Thus, we should not expect to see any differences in investment frequency between own or partner investment probability. This corresponds to what we see in the data, with no persistent differences and a high level of variation that decreases slightly in later rounds. Investment rates decline from over 50% to an average of around 30%.

The graph of investment frequency for Treatment *IK* tells a similar story. While subjects in this treatment observe their own investment probability, they do not see that of their partner – hence information on their own investment probability is not strategically relevant.

If subjects reacted to this information for other reasons, we would see noticeable differences in investment between the upward-pointing triangles (high own investment probability) and downward-pointing triangles (own investment probability low), but we do not. Investment levels hover around 50% in all noise realizations, albeit with wide variation. The large variation is most likely caused by small numbers of subjects in each of the joint investment probability cells in treatments *IK* and *NK* (on average 5).

Since subjects can see their partner's investment probability, the graph of Treatment *PK* investment levels is one where we should expect to see differences in investment behavior across information, and do. Subjects who observe that their partner's investment probability is high (the solid triangles, optimistic) are around 20% more likely to invest than those who observed their partner's investment probability to be low (unfilled triangles, pessimistic). It is this difference that provides the foundation for differences in investment levels between subjects who are second- order-optimistic and those who are second-order-pessimistic in Treatment *BK*; since they can expect their partners to respond to the observed investment probability. Since subjects do not know their own investment probability, we do not expect nor find differential investment behavior between subjects whose own investment probability was high (upward-pointing triangles) and those for whom it was low (downward-pointing triangles) in Treatment *PK*.

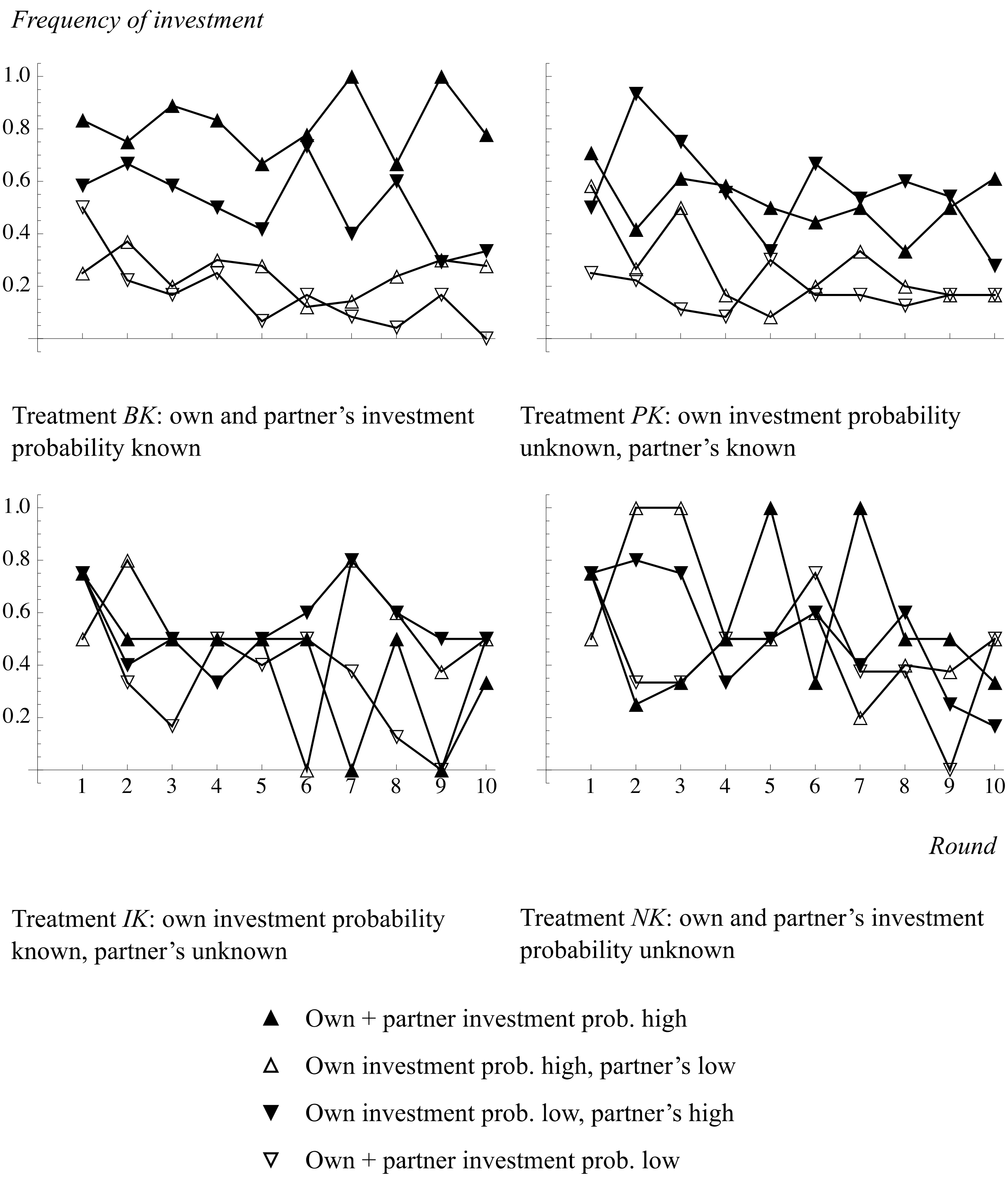


Figure 5. Investment frequencies by treatment and investment probability

Investment probabilities in Treatment *BK* are common knowledge, and thus Treatment *BK* displays a clear separation of investment levels among all four investment probability realizations. Like in Treatment *PK*, optimistic subjects (solid triangles) invest up to 40% more frequently than pessimistic subjects (unfilled triangles) in Treatment *BK*. This difference is larger in magnitude than the corresponding difference from Treatment *PK*, as we might expect it to be[10]. Second-order-optimistic subjects (upward-pointing triangles) are 10-15% more likely to invest than second- order-pessimistic ones (downward-pointing triangles). Thus, we see that induced changes in first-order beliefs have a first-order effect on investment behavior while the induced changes in second-order beliefs had a comparably second-order effect on investment frequency.

In order to make these second-order effects more apparent, the next figure pools investment decisions over high or low partner investment probability but separates cases of high or low own investment probability. The left-hand pane of Figure 6 shows the investment frequencies from both treatments *BK* and *PK*. Treatment *BK* data is separated by whether own investment probability is high (upward-pointing triangles), or low (downward-pointing triangles), while Treatment *PK* data is pooled as subjects do not know their own investment probability (circles). It appears that subjects who know that their own investment probability is high (second-order- optimistic) are significantly more likely to invest than subjects who know that their own investment probability is low or those who do not know their own investment probability – who invest with about the same frequency. This is intriguing; it suggests that not knowing one's own investment probability generates the same behavior as knowing that it is

[10] Refer to Section 2.6.5 for a detailed and quantitative treatment of why this is might be.

low. Would we expect this result using our model of Sections 2.3/2.4.1? I examine whether observed investment patterns are explained by beliefs in the next subsection, and whether they are explained by a combination of beliefs and preferences in Section 2.6.5.

The right-hand pane of Figure 6 is a comparable graph of data from treatments *IK* and *NK*. Subjects in Treatment *IK* invest about as frequently as those in Treatment *NK*, regardless of whether they know their own investment probability to be high or low.

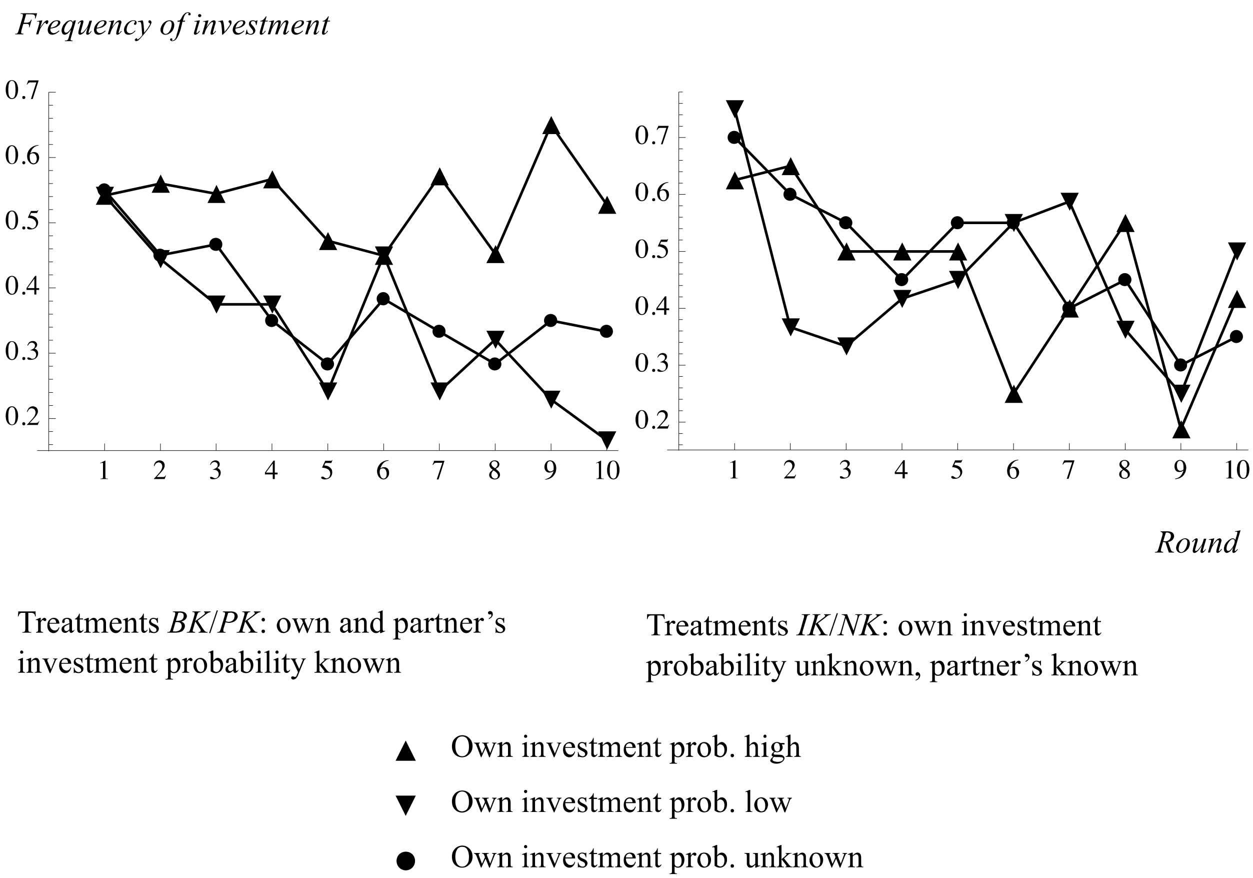


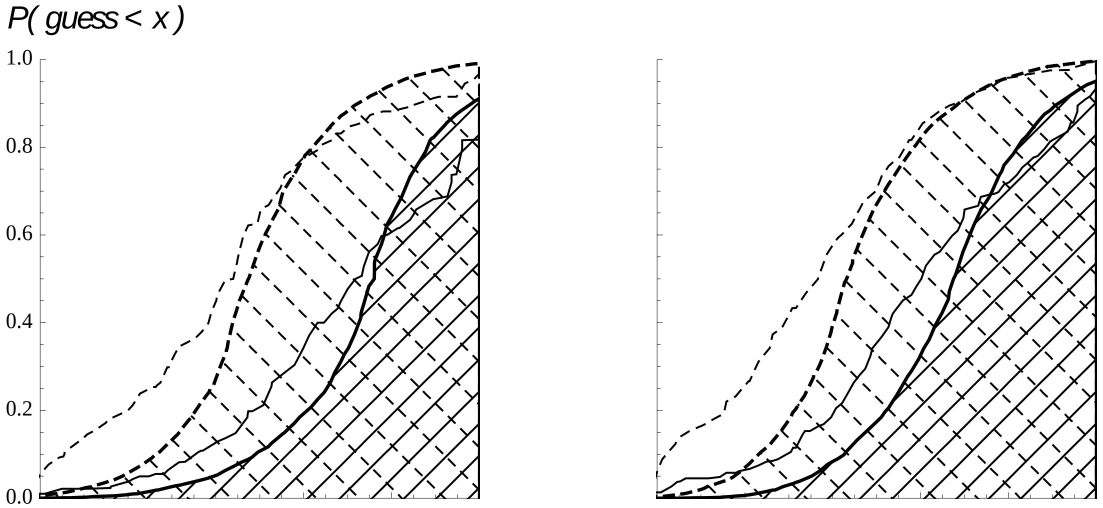
Figure 6. Investment frequencies by own investment probability, pooling across partner investment probability

2.6.2 Beliefs

To strengthen our inference on the role of second-order beliefs, we proceed to examine whether there is evidence that observed differences in investment frequencies can be explained by how the treatments induce changes in beliefs. Beliefs are elicited every round; Appendix B describes how subjects update beliefs across all rounds. Figure 7 shows CDFs of elicited second-order beliefs in all four treatments, with CDFs of elicited

first-order beliefs shown for comparison purposes. Treatments *BK* and *PK* (the left and right panes of the top row, respectively) have second-order beliefs broken down by whether subjects were trying to predict the beliefs of their optimistic counterparts or pessimistic counterparts. First-order beliefs were elicited separately for these two groups, so four CDFs per treatment are shown for comparison: the expected distribution of first-order beliefs for optimistic investors, the actual distribution of first-order beliefs for optimistic investors, the expected distribution of first-order beliefs for pessimistic investors, and the actual distribution of first-order beliefs for pessimistic investors. Since subjects receive no feedback on belief accuracy until all decisions have been made, beliefs are surprisingly accurate. As seen in the rightward shift of the distribution of guesses about guesses relative to that of elicited first-order beliefs, subjects generally tend to think that others place greater likelihood on their partners investing than those others actually do. This could be driven by subjects thinking (on average) that other people are more optimistic than they are. In addition, as seen by the steeper slope of the CDF of guesses about guesses relative to that of elicited first-order beliefs, subjects also tend to think that others' beliefs are more tightly distributed than they are in actuality. Most importantly however, is the finding that subjects expect other subjects to respond to partner investment probability. This means that subjects who observe their own investment probability to be high in Treatment *BK* think their partners place a higher likelihood on them investing. Subjects in Treatment *PK* would of course display this pattern of beliefs if they could observe their own investment probability – this is why elicited beliefs in Treatment *PK* look the same as those from Treatment *BK*. The bottom row of Figure 7 shows elicited second- order beliefs from treatments *IK* (left pane) and *NK* (right pane) graphed with actual first-order belief distributions for comparison. Since subjects in Treatments *IK*/*PK* do not observe their partner's investment probability, second (like first-) order beliefs are not broken down by optimism/pessimism. We do see, like in treatments *BK* and *PK* that on average subjects think that others are more optimistic than they themselves are and that generally beliefs about beliefs tend to be less diffuse than the actual distribution of (first-order) beliefs.

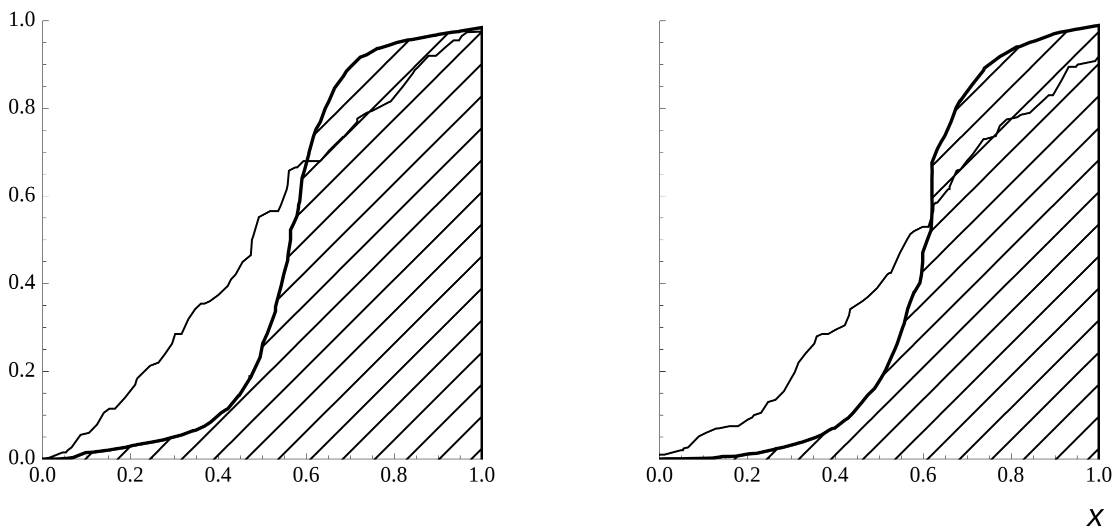
Induced changes in second-order beliefs induce changes in first-order beliefs as is theoretically predicted. That is, since subjects believe that the beliefs of their optimistic counterparts are indeed more optimistic than their pessimistic ones, they in turn believe that these optimistic counterparts will be more likely to invest, which in turn explains why second-order-optimistic subjects are themselves more likely to invest. Figure 8 displays these findings. The diamond-shaped points display average expectations of how many others will invest. For the top row showing Treatments *BK*/*PK*, these are broken out by beliefs about optimistic versus pessimistic investors (solid vs unfilled points, respectively). Circular points on all graphs show that these beliefs are roughly accurate with respect to chosen investment actions (on average in Treatments *IK*/*NK*, bottom row; and between optimistic and pessimistic investors on average in Treatments *BK*/*PK*).



Treatment *BK*: own and partner's investment probability known

Treatment *PK*: own investment probability unknown, partner's known

- Belief that those whose partner's investment probability is high invest
- Belief that those whose partner's investment probability is low invest
- Guess about distribution of guesses – high investment probability
- Guess about distribution of guesses – low investment probability



Treatment *IK*: own investment probability known, partner's unknown

Treatment *NK*: own and partner's investment probability unknown

- Belief that others will invest
- Guess about distribution of guesses

Figure 7. CDFs of elicited second-order beliefs with actual distributions of elicited first-order beliefs shown for comparison

Of note, subjects seem to think that pessimistic investors will invest more often than they actually do; though this cannot explain why second-order-pessimistic subjects invest as frequently as Treatment *PK* subjects who do not know their own investment probability however. Treatment *PK* subjects have similar first-order beliefs as Treatment *BK* subjects, they simply have yet to resolve the uncertainty of whether their partner is optimistic or pessimistic before deciding whether to invest. Treatment *PK* subjects' reduced belief that their partner invests is higher than second-order-pessimistic investors in Treatment *BK*; they should be more willing to invest.

2.6.3 Within-treatment comparisons

Table 12 shows the parameters from several estimated equations explaining investment decisions in a linear probability model (where 1 represents investment). Columns 1, 3 and 5 reproduce comparisons gleaned from Figure 5 and Figure 6. Optimistic subjects in Treatments *BK*/*PK* are more likely than pessimistic subjects (the omitted category) to invest, and second-order-optimistic subjects in Treatment *BK* are also more likely to invest, though this effect is lower in magnitude than that from changes in first-order beliefs, as the coefficient on own investment probability is smaller than that on partner investment probability. There is no significant impact on likelihood to invest for subjects in Treatment *IK* who observe their own investment probability is high relative to low. We should not expect any such difference.

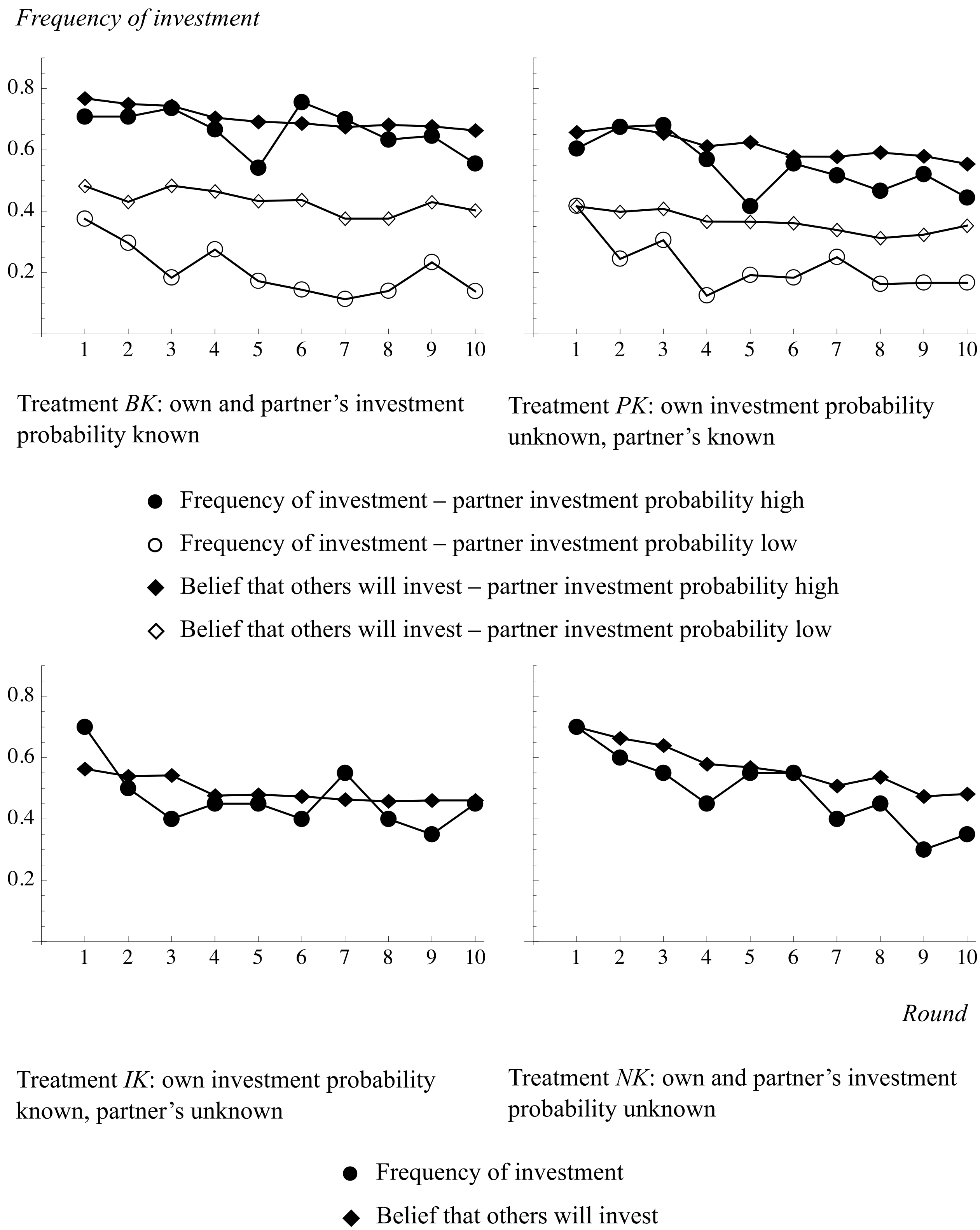


Figure 8. First-order beliefs and investment choice frequencies

The second, fourth and sixth columns of Table 12 control for beliefs. The seventh column explains investment decisions in Treatment *NK*, where there is obviously no investment probability information to condition on but belief that others will invest is quite significant. Treatment *IK* investment decisions are explained in the fifth and sixth columns of Table 12, where subjects can condition on their own investment probability, but should not. Indeed this information does not have a significant impact on investment decisions. The effect of partner- and own-investment probability on investment appear to be significantly reduced once controlling for beliefs as we expect these treatments to operate by inducing changes in beliefs. Furthermore, it does not appear that there is any particular joint effect for investment pairs where both partners have high investment probability (Table 12, Treatment *BK*, own×partner investment probability). We do not expect this coefficient to be significantly different from zero since knowledge of own and partner investment probability have additively separable predicted impacts on beliefs (Table 10).

Table 12. Probability of investing (OLS)

Treatment	*BK*		*PK*		*IK*		*NK*
Own prob. High	.267** (.052)	.118** (.061)			.045 (.052)	.083** (.041)	
Partner prob. High	.351** (.063)	.338** (.060)	.334** (.053)	.334** (.053)			
Guess		.490** (.095)		.740** (.123)		.966** (.166)	1.02** (.173)
Own & partner prob. High	.010 (.076)	.022 (.070)					

Round and subject fixed effects, jackknifed standard errors (in parentheses) treat the subject as the unit of observation, ** indicates significance at 5%

2.6.4 Between-treatment comparisons

What are the consequences for investment of resolving higher-order uncertainty about beliefs? Is it simply the case that knowing your partner is optimistic causes more investment while knowledge that your partner is pessimistic causes a similar drop in investment levels? Table 13 examines the effect of knowing one's own investment probability on investment – relative to not knowing this information. Pooling choice data from treatments *BK* and *PK* (*IK* and *NK*) in the same regression allows us to examine the effect of own investment probability conditional on partner investment probability (or not, as in Treatments *IK*/*NK*). Individual subject fixed effects are lost since subjects in treatments *PK* and *NK* never know their own investment probability; this is the omitted category. The first column confirms what we see in the graph from Figure 6 comparing investment decisions in Treatment *BK* to those in *PK*. second-order-optimistic subjects in Treatment *BK* invest significantly more than second-order-pessimistic subjects, who invest about as often as subjects in Treatment *PK*. The second column of Table 13, like the first column of Table 12, shows that this effect is largely mediated through induced changes in beliefs as all of the estimated coefficients become insignificant when beliefs are added as explanatory variables. There are no significant differences in investment frequency between Treatment *IK* subjects observing their own investment probability to be high or low and Treatment *NK* subjects. We conclude that a mean-preserving resolution of higher-order uncertainty can have asymmetric consequences for efficiency.

Table 13. Probability of investing (OLS)

Treatments	*BK+PK*		*IK+NK*	
Own prob. High	.236** (.058)	.021 (.051)	-.003 (.140)	.115 (.080)
Own prob. Low	-.062 (.054)	-.004 (.044)	-.044 (.139)	.036 (.084)
Partner prob. High	.337** (.039)	.338** (.037)		
Guess		.851** (.070)		1.25** (.066)

Round and subject fixed effects, jackknifed standard errors (in parentheses) treat the subject as the unit of observation, ** indicates significance at 5%

2.6.5 Probability models

The linear probability models estimated above provide some easily-interpretable facts on subject reactions to treatments, but a more micro-level specification provides further insight into how subjects react. In Stag Hunt investment games, a rational player i will prefer to invest if her subjective belief that her partner invests, $P\,(\,Invest\,)_{-i}$, exceeds some threshold probability c_i, and will prefer not to invest if it does not. Thus decisions made in this game lend themselves to modeling with a fixed-effects logit specification.

Formally, player i invests if $P\,(\,Invest\,)_{-i,t} + \varepsilon_{it} > c_i$, where ε_{it} is a logistically distributed decision error with mean zero and unknown scale parameter β. I estimate this model by explicitly solving for the fixed effects using standard maximum likelihood, rather than the conditional likelihood formulation usually employed to estimate fixed-effects logit models (Cameron and Trivedi, 2005). Note that the incidental parameters problem inherent in this method requires us to use a bias-correcting procedure à la Hahn and Newey (2004) (the delete-1 jackknife is employed). The advantage of explicitly estimating the fixed effects c_i in this model is that we may generate predicted investment probabilities under counterfactual situations, which will be necessary to predict whether the asymmetry of the treatment effect estimated in the first column of Table 12 would be expected using our model of investment behavior.

2.6.5.1 Third-order beliefs?

In Treatment *BK*, common knowledge of investment probabilities should induce even higher (third-order) changes in beliefs. This works as follows: if players in Treatment *BK* expect that their second-order-optimistic partners will be more likely to invest than second-order-pessimistic partners (they do; and elicited second-order beliefs indicate that subjects recognize this) then subjects should be more likely to invest when seeing that their partner's investment probability is high. Furthermore the response should be above and beyond the exogenously increased probability of investment the treatment induces.

I focus on just how much any additional third-order effects from knowledge of partner investment probability have on $P(\, Invest \,)_{-i}$ for subjects in Treatment *BK* versus those in Treatment *PK*, whose first-order beliefs should mechanically be higher by .33 when their partner's investment probability is high. Since first-order beliefs are elicited with respect to chosen actions, we model information changing these beliefs as

$$P\,(\,Invest\,)_{-i,t} = \tfrac{2}{3}\, guess_{it} + \delta \cdot optimistic$$

since investment choices are carried out with ⅔ probability. We expect δ to be ⅓ in Treatment *PK* and higher in Treatment *BK*. Specifically, since second-order-optimistic subjects in Treatment *BK* are 27% more likely on average to invest than their second- order-pessimistic counterparts (Table 12, first column), we should expect δ to be around ⅓ + ⅔ (.272) = .515 in Treatment *BK*.

Results are shown in Table 14. The estimate of δ from Treatment *PK* is significantly less than .33. Furthermore, while the estimate of δ from Treatment *BK* is no greater than .33, it is significantly higher than the Treatment *PK* estimate (p = .08). The difference between these estimates is less than we would expect however, indicating that there is significant under-reaction to the treatment at this level of reasoning (third-order) relative to lower orders[11]. We conclude that consideration of beliefs even up to the third order may be relevant in predicting investment decisions.

Table 14. Fixed-effects logit regressions of investment choice on beliefs and observable factors

Treatment	*BK*	*PK*
β	8.42 (1.02)	14.8 (2.89)
δ	.332 (.049)	.237 (.045)

Round fixed effects, jackknifed standard errors (in parentheses) treat the subject as the unit of observation

2.6.5.2 Asymmetry of the treatment effect

The asymmetry of the treatment effect shown in previous subsections merits deeper investigation. Specifically, since we have shown that different higher-order beliefs matter for whether people successfully coordinate, confirming the mechanism by which this happens is important for deriving predictions. Does the asymmetry in actions result from an asymmetric or behaviorally anomalous response, or is observed data consistent with the model of Section 2.3. What are the social welfare con- sequences of resolving higher-order uncertainty about beliefs?

[11] As an interesting aside, if global games experiments require a certain number of reasoning iterations before we should expect to observe equilibrium selection results, then these much-diminished third-order effects found here offer support for Heinemann and Cornand (2011)'s assertion that weak selection results in their game arise from a limited number of iterated reasoning steps.

Is it simply the case that knowing your partner is optimistic causes more investment while knowledge that your partner is pessimistic causes a similar drop in investment levels? Would investors in Treatment *PK* continue to invest at the same levels if they knew their own investment probability was high or low? More importantly, would they invest with the same frequencies as subjects in Treatment *BK*? Figure 9 provides some evidence on this question. The left pane displays model predictions for Treatment *BK* subjects. The series marked with upward-pointing and downward-pointing triangles are simply the estimated investment probabilities predicted by the model whose parameters are shown in the first column of Table 14 (for knowledge of own investment probability high and low, respectively). The third series in this graph, indicated by circles, is a counterfactual estimate of Treatment *BK* subjects' investment probability if they did not know their own investment probability. We should in fact expect subjects to be only marginally more likely to invest when not knowing their own investment probability relative to knowing that it is low. The right-hand pane of Figure 9 corroborates this finding. Here, the series indicated by circles is the predicted investment probability of Treatment *PK* subjects in Treatment *PK*, using the model whose parameters are given in the second column of Table 14. The two other series in this graph are counterfactual investment probabilities computed by supposing that subjects in Treatment *PK* did know their own investment probability to be high (upward-pointing triangles) or low (downward-pointing triangles). Again, the same pattern emerges: we should expect higher levels of investment when own investment probability is known to be high and lower (but comparable) for those who do not know their own investment probability or know it to be low. This implies that (for the preferences and beliefs of my experimental subjects in this setting at least) that it is completely rational for resolution of higher-order uncertainty about others' beliefs to generate significantly higher levels of coordination.

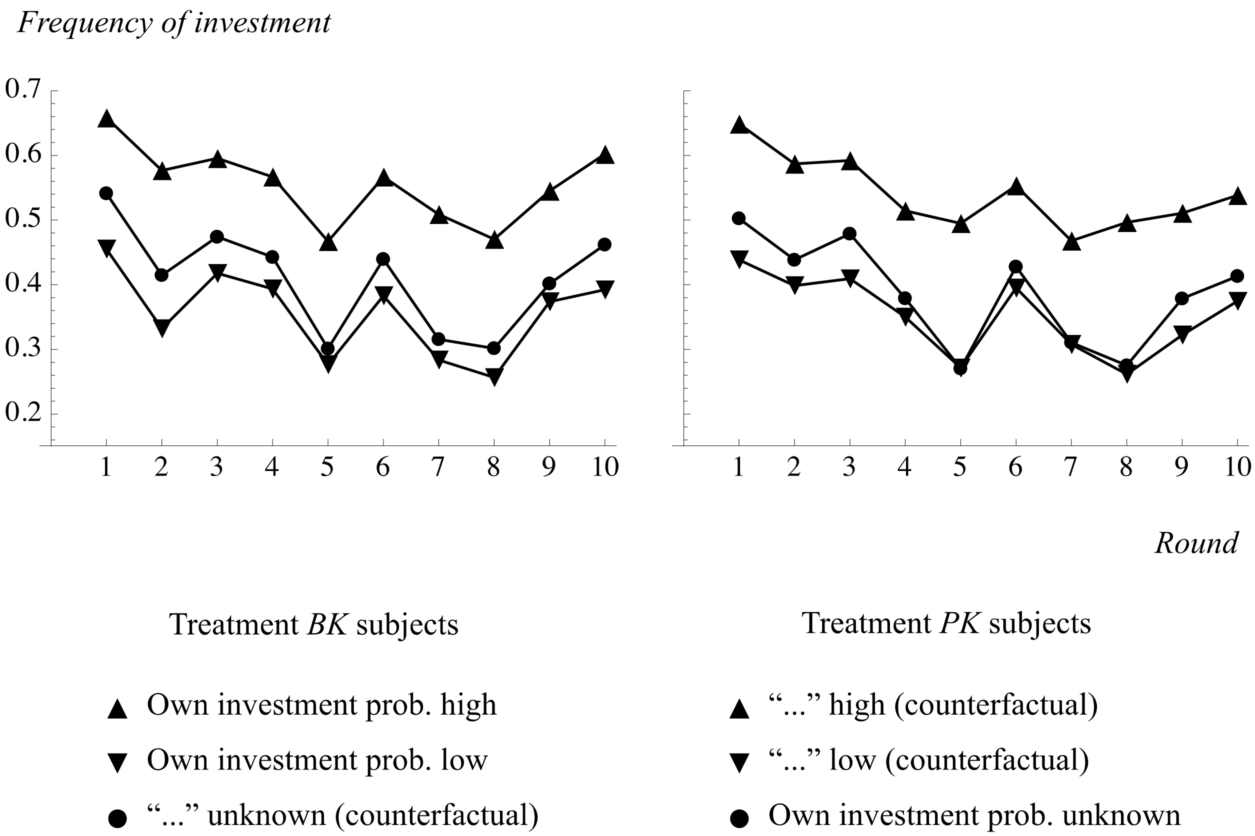


Figure 9. First-order beliefs and investment choice frequencies

2.7 DISCUSSION

The results of the experiment allow us to conclude that consideration of what others think about your beliefs significantly influences investment decisions in a coordination game. Knowing that others think you likely to invest causes people to be more likely to invest themselves. My unique experimental design exogenously shifts beliefs holding fundamental payoffs constant and allows us to identify how higher-order beliefs matter when deciding whether to invest. The experimental treatments change peoples' second-order beliefs by varying the information that others know about them. Differential second-order beliefs in turn change first-order beliefs and in turn decisions, because beliefs determine what rational agents choose to do in coordination games, and so beliefs about beliefs determine beliefs about choices. There is also some evidence that subjects under-react to induced changes in beliefs, particularly to partner investment probability in Treatment *BK* (where responding optimally entails consideration of third-order beliefs).

Broadly, this means that common knowledge of prevailing attitudes is necessary in sustaining equilibria. Heterogeneous societies' inability to coordinate on norms can explain why they have lower social cohesion (e.g. Alesina et al., 1999; Alesina and La Ferrara, 2000-02). Not knowing what others think about your beliefs frustrates coordination. The finding that human subjects reason about coordination at at least the second level reinforces the plausibility of this explanation which, unlike simple in-group out-group identification, conforms to more nuanced recent findings such as those from Putnam (2007) who documents less *in-group* social cohesion in more diverse communities. The alignment of social and individual incentives in the Stag Hunt game means that conventional economic models of decision-making explain the data quite well. The strategic incentives present in this game are relevant to any setting where social norms or technology create multiple equilibria however. For example, Greiff and Paetzel (2012) show in a finitely repeated public good setting that information about the subjective ratings that previous partners have given you is important only when they are common knowledge.

Comparison between treatments indicates that subjects behave equivalently under unresolved higher-order uncertainty and higher-order uncertainty that is resolved as the worst outcome. This is apparently consistent with rational decision making. It also suggests some policy implications. The meeting of different cultures may destabilize efficient social norms; but we might expect society to reestablish coordination as people learn more about each other. This is because resolving uncertainty in favor of the efficient norm may increase adherence while resolving it in favor of the other has no effect. Cross-cultural understanding is of course a core mission of several existing civic organizations. These range from the 1893 Parliament of the World's Religions at the World Columbian Expedition in Chicago, to the United Nations declaration of 2001 as the "Year of Dialogue Among Civilizations". To the naive economist, these may seem like puzzling enterprises; but the results herein suggest that efforts to promote common knowledge of cultural norms are quite worthwhile. As societies across the world become more diverse, we can benefit from specialization and trade while coping with the stress this places on social cohesion.

3.0 THE IMPACT OF SHOCKS TO SOCIAL CAPITAL: EVIDENCE FROM BURGLARIES (WITH RANDALL WALSH)

A body of evidence links social capital, particularly measures of generalized trust, with better macroeconomic and institutional performance. Microeconomists have found correlations between higher social capital and efficiency-promoting behavior in economically important situations. We develop a unique quasi-experimental survey design that allows identification of the structural relationships between trust attitudes and incentivized economic behavior. As an exogenous shock to social capital we identify households located in the same city block that are both proximate and further from property crimes. We find that neighbors of burglary victims have lower levels of generalized trust than their socio-demographically similar neighbors living marginally further from the burglary. We also find suggestive evidence that their lower generalized trust causes them to invest less with a principle who may abscond with their investment and additionally leads them to contribute less to a public good.

3.1 INTRODUCTION

Economists have come to recognize the importance that social capital plays in a wide range of economic activity. The connections that bind people to their communities can allow them to realize gains from trade that do not necessarily arise from narrow self-interest, explicit contracts, or government provision. When people feel connected to their communities, they can expect continuing relationships of mutual benefit, will adhere to norms that support those relationships, and can trust that others in their community will also uphold these norms. These connections, norms and the expectations of trust that arise therefrom are termed *social capital*. Social capital is shown in a variety of studies to potentially have significant economic consequences, ranging from individual-level studies correlating social connections with higher incomes (Narayan & Pritchett, 1999), to cross-country studies showing that societies with higher rates of generalized trust experience more economic growth (Knack & Keefer, 1998; Algan & Cahuc, 2010) as well as better institutional performance along a wide range of metrics (Putnam, 1993; La Porta et al., 1998). Less is known about how the level of social capital in a society can change and what factors might effect such changes. While studies such as Putnam (1993)'s suggest differences in social capital across societies can persist over centuries, Algan and Cahuc (2010) show that *changes* in a society's generalized trust can predict changes in economic growth. There is also much more to be learned about the precise mechanisms – that is specific forms of social and economic interaction – through which social capital might generate higher growth and better institutional performance. Studies in experimental economics have shown that generalized trust predicts greater voluntary contributions to public goods (Anderson et al., 2004; Thöni et al., 2012) as well as greater likelihood to invest in coordination games (Bosworth, forthcoming 2013), and provide mixed evidence that people with more social capital invest more in trust games (Berg et al., 1995). These studies use pre-existing differences in social capital to explain behavior in various investment situations.

In order to explain how changes in social capital resulting from real life experiences translate into changes in behavior, we report results from a novel field experiment through which we examine the influence of property crime on social capital. Survey measures indicate that participants who live closer to burglarized houses suffer adverse shocks to their social capital. We then investigate whether these exogenous shocks to social capital affect behavior in an incentivized trust game, a coordinated investment (stag hunt) game, and a charitable donation task. Participants who score higher on survey measures of social capital send more in the trust game and donate more to a charitable cause; and we find suggestive evidence that subjects who experience shocks to their surveyed social capital send less in the trust game and donate less to charity. We thus shed light on the hidden external costs of property crime on communities' social capital, and argue that erosion of generalized trust may have material consequences.

3.2 SOCIAL CAPITAL

Aspects of social interaction that enable, connect and coordinate economic activity have been grouped under the concept of *social capital*. The New Palgrave Dictionary of Economics gives overlapping definitions (Dasgupta, 2008):

> 'features of social organization, such as trust, norms, and networks that can improve the efficiency of society by facilitating coordinated actions' Putnam, 1993, p. 167;
>
> 'social capital refers to connections among individuals – social networks and the norms of reciprocity and trustworthiness that arise from them.' Putnam (2000, p. 19);

and

> 'Social capital generally refers to trust, concern for one's associates, a willingness to live by the norms of one's community and to punish those who do not' Bowles and Gintis, 2002, p. F419.

Social capital is most commonly measured with survey instruments such as those found in the General Social Survey or World Values survey. The standard `trust question', found in both, is

> ‘Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?’.

Henceforth we refer to this question as *GSS_trust*.

3.2.1 Social capital and society-level outcomes

The literature argues that the economic relevance of social capital can be seen in its importance to institutions and macroeconomic performance. Putnam (1993) contrasts local government effectiveness among the regions of Italy following power devolution in the 1970s. He correlates several measures of perceived government effectiveness; most importantly citizen powerlessness, corruption, respect for the law and public safety; with measures of civic engagement; including referendum turnout, newspaper readership, number of sports and cultural associations, political machines influence, and perceived trust in others. He finds that higher levels of civic engagement are associated with more effective governance. Putnam argues against a causal interpretation of his data, emphasizing that “norms and networks of civic engagement contribute to economic prosperity and in turn are reinforced by that prosperity.” Knack and Keefer (1998) find correlations between trust and civic norms as measured on the World Values Survey with measures of economic performance in 29 countries. These include growth, investment share of GDP, labor force growth, openness to trade, black market penetration, strength of property rights, currency depreciation, creditworthiness, and inequality. La Porta et al. (1998) find that standard generalized trust measures track a very broad range of institutional and economic performance outcomes[12]. They argue that most organizations need to maintain trust among their members to function effectively: firms, nonprofits, and governments characterized by high trust and trustworthiness should perform better. Higher trust enables organizations to grow larger since large organizations entail imperfect monitoring within the institution and greater reliance on norms of behavior to enforce cooperation among its members.

Numerous studies have shown social capital is associated with desirable microeconomic outcomes. Narayan and Pritchett (1999) examine data from the Social Capital and Poverty Survey on households in rural Tanzanian villages. They find that households with more group memberships (e.g. political, religious, farmers' groups or women's groups) have higher incomes as measured by consumption expenditures. Supplemental data on the villages from the Human Resource Development Survey suggest that village-level social capital also raises household incomes and predicts access to better public services, use of advanced agricultural practices, participation in communal activities, and access to credit. Adler and Kwon (2002) argue that social capital increases productivity in workplaces. Social capital strengthens networks that foster employer-employee matches, is associated with internal promotion reduced turnover, supports research and development and encourages workers to share ideas and work on projects requiring collaboration.

[12] These include efficiency of the judiciary, corruption, bureaucratic quality, tax compliance, civic participation, participation in professional associations, share of top 20 firms in GNP, adequacy/quality of infrastructure, infant mortality, high school completion, educational system adequacy, inflation, growth and GNP per capita.

3.2.2 Mechanisms

Why do societies with high social capital exhibit better economic and institutional performance? Individuals in high-trust societies may make different decisions than their low-trust counterparts. As discussed above, social capital is conventionally assessed using questionnaires that measure generalized trust or specific outcomes of social capital, such as those questions contained in the World Values Survey and General Social Survey. While these surveys cover large samples, they do not inform us about how people with high generalized trust make different decisions than those with low generalized trust. This prompts behavioral decision researchers to ask whether generalized trust measures predict more trusting choices in controlled economic experiments.

Fehr et al. (2003) combine a demographic and trust attitude survey with the 'sender-receiver' trust game (Berg et al., 1995) implemented via postal mail with a representative sample of Germans. In the trust game, a *sender* makes a choice on how much money to transfer to a *receiver*. Money sent is multiplied; hence sending the entirety of one's endowment maximizes social surplus. The receiver has the ability, but no obligation, to return some of the resulting surplus to the sender. Fehr et al. find that affirmative responses to the WVS trust attitude question[13] do predict the amount transferred by the sender in the trust game, but not the amount returned by the receiver.

In contrast, Glaeser et al. (2000) do not find that affirmative responses to the GSS trust questions predict behavior in either the trust game or an 'envelope drop'. The envelope drop activity elicits subjects' valuations for an envelope that was to be anonymously left in Harvard Square but addressed to them. Thus, the more trusting a subject is, the more they should be

[13] The English language version of this question corresponds to *GSS_trust*.

willing to pay for the envelope. The authors find some support that survey questions correlate with receiver transfers in the trust game, which indicates trust*worthy* behavior. However, they find no correlation between trust*ing* behavior (as measured by the amount transferred by the sender in the trust game or willingness to pay for the dropped envelope) and survey questions about trusting attitudes. This study casts doubt on surveys' ability to measure social capital and instead suggests that the behavioral responses examined are better metrics of social capital.

Karlan (2005) conducts a field study that replicates the null result of Glaeser et al., but suggests that responses to survey trust questions do predict other economic decisions where generalized trust is relevant. He has participants in a Peruvian microcredit program play both the Berg et al. trust game and a linear public good game with voluntary contributions. He is able to link participants' behavior in these games to observations of each participant's history with the microcredit program and attitudes measured by WVS/GSS trust questions. While returning money that one has been sent in the trust game is correlated with microloan repayment; sending money in this game (the behavior that Glaeser et al. find is unrelated to trust attitudes) does not predict the amount of money that individuals invest with the microcredit program. Survey trust questions do however predict the amount saved. Karlan does not find that behavior in the public good game correlates with either repayment or saving in the microcredit program or with trust attitude questions.

Anderson et al. (2004), find that the GSS trust question administered in a post-game survey predicts the amount invested in the public account of a voluntary contribution public good game. Thöni, Tyran and Wengström (2012) find that affirmative answers to the trust question predict being a *conditional contributor* to a linear public good (Fischbacher et al., 2001).

Responses to the standard GSS trust question is not robustly related to beliefs in the Thöni et al. experiment, but it seems to boost contributions at all levels of belief about other contributions.

These studies indicate that people with more social capital may behave in ways that secure more efficient outcomes in a variety of settings that require generalized trust. Our study seeks to examine how a novel source of exogenous variation, crime, may shock social capital, as such these outcomes of social capital are quite relevant. We therefore implement a trust game and a stag hunt investment game, as well as ask participants to contribute to a real public good in our study. Our aim is to test whether shocks to social capital can predict less trusting behavior in these settings.

3.2.3 Crime and social capital

To assess the impact that negative shocks to social capital have on behavior, we turn to property crime as a plausible influence. There are several reasons to believe that crime may adversely affect social capital. For instance, the existence of crime could signal to individuals that they are not safe from expropriation, and that other individuals are willing to transgress social norms against expropriation. The occurrence of crime also may signal an implicit failing of society's crime deterrence institutions, as an effective criminal justice system (police and courts) should be able to identify expropriators and deliver appropriate punishment to deter others from engaging in similar crimes. The expectation that one can trust the judicial system to protect one's interests effectively and equitably is a key prerequisite to the functioning of a market economy. For example, North and Weingast (1989) prominently argue that English constitutional developments protecting property rights promoted investment early in its economic development. Additionally, since crime is often deterred through the private initiative of other community members, the occurrence of crime erodes confidence in other individuals in one's community. Jane Jacobs (1961) observes that cohesive communities can deter crime through simple awareness. Crime that is visible to third-party observers will vary in success depending on the actions taken by those observers. Whether people who witness crime intervene, or alert others in the community or authorities, can have dramatic consequences for criminals and their victims.

There are also reasons to suspect that crime and social capital are co-determined. Communities with higher social capital may more effectively deter crime because their institutions perform better, they have stronger norms discouraging expropriation and promoting good samaritanship, or simply because they feel safer they will encourage "more eyes" to be out on the street. Lederman et al. (2002) show that generalized trust and membership in secular organizations predicts lower homicide rates in a cross-section of countries, controlling for economic growth and inequality. Akçomak and ter Weel (2012) report that variation in crime rates across Dutch municipalities is negatively associated with social capital indicators such as charitable giving, voter turnout, blood donation, affirmative responses to the standard generalized trust questions, and measures of social stability (migration, divorce rates, and presence of immigrants). The association between crime and social capital remains when instrumenting for the latter with historical data on the presence of foreigners and percentage of protestants in the municipality in 1859. Buonanno, et al. (2009) report similar results in a comparison of property crime rates across Italian regions. Blood donation rates and prevalence of civic associations are negatively correlated with property crime rates controlling for judicial effectiveness, socioeconomic and demographic factors. They also instrument for social capital using the historical data on local civic associations constructed by Putnam (1993) for his study.

The rarity or prevalence of crime may also lead people to place more or less trust in their communities and institutions, and to more confidently form relationships which yield social or economic benefits. For example, Blanco and Ruiz (2013) show that trust in state institutions in Colombia is negatively related to violent crime victimization and perceptions of insecurity. We therefore design our study with these inference difficulties clearly in mind.

3.3 DESIGN

Our approach examines the effect that very localized proximity to crime has on social capital and economic decision making. Attitudes and decisions are elicited on a postal survey which combines attitudinal questions related to social capital and generalized trust with three incentivized economic games. Participants' responses and decisions are then compared by dividing the sample into those very close to burglarized homes and those a few houses away in the same sample of city blocks. We observe burglaries in low-crime neighborhoods and then survey households on the block where the crime occurs, shortly after it happens. We then examine within-block variation in attitudes and behavior related to social capital. Our procedure is as follows: we monitor police blotters in the city of Pittsburgh for burglaries. The blotters identify the city block, but not the specific address where the burglary takes place. We then determine if the blocks where burglaries happen are candidates for surveying. Candidate blocks must be composed entirely of single-family residential structures, must not have experienced any violent or property crimes in the preceding 12 months, and must contain at least 10 owner-occupant[14] households to whom surveys can be mailed. We choose these criteria for identification purposes. Residents of neighborhoods where crime is prevalent will already have incorporated this knowledge into their attitudes. By surveying lower-crime neighborhoods of single family houses we aim to observe people who are invested in their communities and whose perceptions might be changed by crime happening very close to them. Additionally, the single-family house criterion ensures that we do not survey blocks where burglaries happen at commercial structures or inside apartment buildings (where the crime may be less observable or residents may be less concerned), and that we can address the householder(s) by name. We seek at least 10 households to mail per block to increase the likelihood of observing multiple houses per block.

Surveys are mailed to owner-occupant households on the candidate block the day following the reported burglary. These surveys are designed to elicit demographic information, attitudinal measures related to social capital, and behavior in three incentivized economic situations. Households that complete and return the survey are promised a $10 debit card and a 1 in 10 probability that the decisions they make on the economic game tasks carried out for real compensation. This randomization is accomplished by printing a digit on each survey that must match the final digit of a future drawing of the Pennsylvania Lottery's Daily Number game for the respondent to receive payment for her decisions in the three games. Surveys are identified by number, and responses are kept separate from the crosswalk of addresses to ID numbers. Some

[14] We define owner-occupancy with respect to public real estate records: if the owner of the dwelling's listed address is the same as that of the dwelling itself we assume that she is an owner-occupant and not otherwise.

time after completed surveys are returned, we learn the specific address where each crime happened, and then code the proximity of each respondent's house to the victim's. The full survey is available upon request. The first 14 questions are demographic in nature. These comprise questions on gender, race, age, citizenship, marital status, number of children, education and employment status. We then ask a series of survey questions designed to asses how respondents' social capital is affected by crime. Of particular interest are questions on community engagement, trust in institutions, and generalized trust in others. These are all written so as to be forward-looking rather than backward-looking as respondents are surveyed shortly after the shock occurs (surveys are mailed the day after the crime is reported). Many of these questions are adapted from the General Social Survey or World Values Survey, and indeed the primary questions of interest are taken verbatim from them, most notably *GSS_trust* (*Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?*). We have respondents choose from a 5-point Likert scale where 1 represents *"need to be very careful"* and 5 represents *"most people can be trusted"*[15]. Also adapted from the WVS are "civic-mindedness" questions assessing the relative appropriateness of various antisocial behaviors such as claiming government benefits not entitled to, avoiding public transport fares, tax evasion, keeping found money, and failing to report damage to parked vehicles. These are prominently examined by Knack and Keefer's growth study along with *GSS_trust*.

The incentivized economic games are as follows: the first is a *trust game* (Berg et al., 1995) in which half of the survey participants, who have been assigned the role of "sender" are

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[15] Both the mean and the median of *GSS_trust* are 3.0 on the 5-point scale.

endowed with \$30 that they can allocate between themselves and a “receiver” (the other half of the mailed surveys are assigned the receiver role). Every dollar sent to the receiver is tripled, and receivers can (but are not obligated to) send some portion of this money back to the sender at their discretion. Since this is a sequential move game, receiver decisions are elicited for each *possible* amount a sender could have transferred (i.e. the strategy method pioneered by Selten, 1967 is used). The amount transferred by the sender is frequently interpreted as a measure of trust and the amount returned by the receiver is interpreted as a measure of trustworthiness. It is efficient for the sender to trust the receiver with her entire endowment, though not rational if she believes the receiver to have only pecuniary motives. The second task is known as the *Stag Hunt*, and is a symmetric two-person game in which the participants have a risky but profitable investment opportunity and a safe outside option. If both invest, they realize positive profits, but if either person declines to invest, the investment will not be successful and a solitary investor will realize a payoff of zero from foregoing her outside option. The game payoffs are summarized in Table 15. When both people invest, they both earn \$9. If one person invests but the other does not, then the person who chose to invest will earn nothing. If a person decides not to invest, she earns \$5, regardless of the other person’s decision. The efficient outcome in this game is for both people to invest, though for this to be rational, one must believe that the other person will also invest. In the final task, participants are given an additional \$30 endowment that they may keep for themselves, or donate some portion of to a charitable cause (the Greater Pittsburgh Community Food Bank). Each dollar donated to the food bank is additionally matched by the researchers. This task is meant to elicit charitable behavior and willingness to finance a public good (feeding the needy).

Table 15. Payoffs in the stag hunt game

		Player 2	
		Invest	Don't invest
Player 1	Invest	\$9, \$9	\$0, \$5
	Don't invest	\$5, \$0	\$5, \$5

3.4 ANALYSIS AND RESULTS

Over the period of February 2012 through June 2013, 40 qualifying burglaries were observed. 622 survey packets were mailed to these 40 blocks. From those, we received 92 returned surveys (a response rate just above 15 percent). Of these, there were 79 white respondents, 11 black, 2 Hispanic, and 2 who identified as “other” (i.e. not one of the US Census race or ethnicity categories). The mean respondent’s age was 53 years old; 45 respondents were male and 47 were female.

Data from the Pittsburgh Police Department identify the address of the burglarized house. Our treatment classification is constructed based on the possible ability of a respondent to observe the burglarized house. We expect that respondents who are able to see the burglarized house from their own will be more likely to know about the crime and thus have likely to have negative shocks to their social capital. The variable *treated* variable therefore takes on the value of 1 for burglary victims themselves, any house next door to the burglarized house, and the five houses on the opposite side of the street in front of the burglarized house. This variable takes on the value 0 for all other observations.

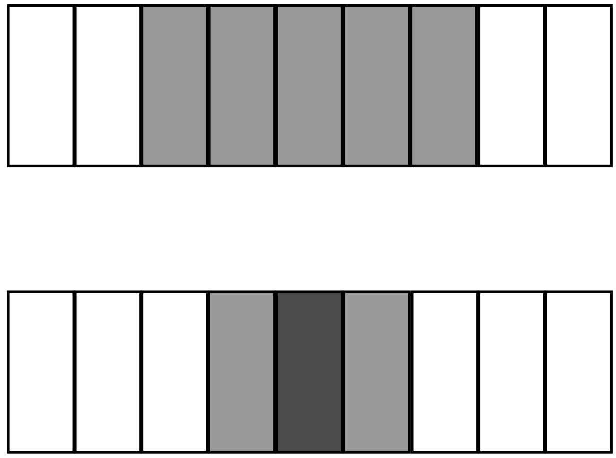

Figure 10. *Victim* (dark grey) and *neighbors* (lighter)

The questions on our survey can be classified into three categories: time-invariant demographic controls that we maintain are exogenous to within-block burglary incidence, a set of attitudinal survey questions informed by extant studies on social capital, and the three incentivized investment decisions. At present, we have 76 fully identified observations, and the small sample size guides much of the analysis to follow (though 92 surveys were returned between February 2012 and June 2013; incomplete responses and discrepancies between the police blotter and geographically identified police data reduce this total)[16].

The set of explanatory variables is sufficiently rich that a standard regression approach utilizing all of the data would more than exhaust the available degrees of freedom. With respect to the social capital survey questions, we address this issue by adopting a systematic approach incorporating factor analysis. For reference the social capital responses are associated with questions 15-30 from the survey packet and correspond to the 21 variables listed in Table 16.

[16] 84 of the 92 returned surveys are geographically identified. 76 are geographically identified and answer all questions. The analysis to follow is restricted to the 76 respondents that are identified and answer all questions.

These questions were all designed to elicit behaviors and attitudes related to social captial. Table 16 reports the factor loadings from a factor analysis on these questions revealing that the responses to these questions all seem to reflect social capital as an underlying factor. We focus on only the most explanatory factor. Our variables' factor loadings on this factor have the signs we would predict from the social capital literature – with two exceptions. These are "If you had a dispute with one of your neighbors or coworkers, how likely do you think it is that any of your friends or family members would find out about it from someone other than yourself?" and the relative acceptability of "Claiming government benefits which you are not entitled to". These two questions are omitted and another factor analysis is performed. We use this to predict values of the underlying factor and retain as the variable sc, which we standardize to have zero mean and standard deviation 1.

Table 16. Factor loadings from factor analysis of social capital survey measures

	Factors									
Variables	**1**	2	3	4	5	6	7	8	9	10…
city	**.443**	.195	.155	.369	-.247	-.170	.093	-.015	.088	.074
nonprofit	**.341**	-.081	.338	.291	-.213	.282	.035	-.023	-.092	.120
residents	**.331**	.126	.272	.496	-.086	-.117	.048	-.162	-.087	-.111
important_ charity	**.482**	-.251	.040	-.367	.143	.303	.088	-.091	.050	.065
volunteering	**.449**	.097	.074	-.034	.460	-.280	.114	-.124	-.027	.105
vote	**.391**	-.308	.332	-.232	-.133	.188	.085	-.189	.126	-.074
meetings	**.370**	-.175	.290	-.250	-.174	-.271	-.135	.030	-.012	.139
club	**.324**	-.064	-.140	.004	.381	-.072	.052	-.047	-.298	-.028
gss_trust	**.696**	.257	-.317	-.137	.060	.065	-.220	-.052	-.047	-.136
gss_fair	**.653**	.308	-.416	-.072	-.225	-.007	-.070	.016	-.043	.078
gss_help	**.715**	.209	-.189	.045	-.102	.167	-.073	.128	.052	.072
trustworthy	**.408**	-.227	.238	.006	.172	-.030	.359	.254	.020	-.102
newspaper	**.194**	.017	-.028	-.057	.062	-.143	-.033	.360	.128	-.049
dispute	**-.052**	.181	.353	.220	.101	.117	-.349	.139	-.072	-.038
invest	**.125**	.121	.192	.174	.361	.169	-.008	.126	.038	.185
job	**.274**	.107	.137	.158	.309	.080	-.162	-.083	.208	-.208
benefits	**.120**	.731	.108	-.181	-.098	-.185	.148	-.052	.081	-.063
fare	**-.188**	.635	.309	-.261	-.032	-.007	.064	-.029	.002	.036
taxes	**-.150**	.487	-.199	.075	-.072	.301	.310	.083	-.101	-.071
keep	**-.219**	.130	-.413	.317	.235	.033	.060	-.130	.220	.145
hit_run	**-.293**	.543	.345	-.224	.187	.120	-.101	-.018	-.034	.063
Eigenvalues	**3.18**	2.06	1.42	1.11	.978	.649	.533	.372	.271	.229

Table 17 reports how means of our variables of interest, as well as means of the collected demographic control variables, vary by treatment group status (i.e. victims and their neighbors vs. other respondents). Three of 20 demographic variables, namely *black*, *mother_us* (respondent's mother was born in the United States), *divorced* and *widowed* have significant differences in means. Subsequent analysis controls for either the full set of demographic controls or just these four where degrees of freedom permit, with results qualitatively unchanged.

We begin our analysis by utilizing our proximity instrument to estimate how the social capital composite measure *sc* and the standard *GSS_trust* question respond to proximity to crime in **Table 18**. Our analysis suggests that respondents living in close proximity to the crime (*neighbors+victims*) have lower social capital. Specifically, these respondents have measured social capital composite index about half of a standard deviation lower than their neighbors (columns 1-4) or over a half point lower on the 5-point Likert scale for *GSS_trust* (columns 5-8). Differences in *sc* are significant at at least the 5% level, and differences in *GSS_trust* range from marginally significant in the baseline specification to significant at the 10% level in the most powerful specification wich includes block fixed effects and a full set of demographic controls. Columns 2 and 6 of **Table 18** demonstrate that the estimated effects are robust to controlling for block characteristics using fixed effects instead of clustering, columns 3 and 7 of **Table 18** control for demographic information[17], and columns 4 and 8 include fixed effects and controls. Results are generally robust across specifications.

3.4.1 Decision tasks

We now turn to the link between social capital and incentivized game behavior. Respondents assigned the "sender" role in the trust game transferred an average of $15.49 to the "receivers", who sent back averages of $8.53, $16.52, $24.39, $32.52, $39.48, and $48.73 from (pre-multiplied) transfers of $5, $10, $15, $20, $25, and $30, respectively. This means that any amount senders transferred would have been more than repaid, and in fact senders were left with slightly more than half of the post-multiplied surplus. Furthermore, the return schedule implies that it is rational from an ex-post perspective for risk-neutral senders to transfer their entire endowment to the receiver. This contrasts with laboratory trust game experiments, where senders typically transfer small amounts and are returned slightly less than they transferred on average

[17] Means of control variables are reported in Table III.

(e.g. Berg et al., 1995). In the Fehr et al. (2003) trust game field experiment that studies a representative sample of German adults, senders do transfer about half of their endowment, though they are also returned slightly less than the amount they transfer. Thus our sample appears to be rather more trusting and trustworthy than might be expected. Next, in the stag hunt game around 55.3% of our subjects choose to invest. Interestingly, the fraction that should make a risk-neutral participant indifferent between investing and not investing is 5/9 or 56%, meaning that participants in the stag hunt are roughly best-responding to the empirical frequency of others' choices. Finally, the mean amount of the $30 endowment that respondents transferred to the Greater Pittsburgh Community Food Bank was $23.09, with a majority of respondents transferred the maximum amount of $30.

Table 19 reports baseline correlations between social capital attitude measures and behavior in the incentivized decision tasks estimated by ordinary least squares. Columns 1-3 have as a dependent variable the amount sent in the trust game[18], columns 4-6 whether or not the respondent invested in the stag hunt game, and columns 7-9 donations to the food bank. Regressions reported in the top panel of **Table 19** take our composite index *sc* as an explanatory variable, while those in the bottom panel use *GSS_trust*. Respondents with higher surveyed social capital and generalized trust send more on average in the trust game, and donate more to the food bank. A one standard deviation increase in the *sc* index predicts that the respondent will send an average of $2.59 more in the trust game. The bottom panel of **Table 19** shows that a one point increase in the 5-point *GSS_trust* Likert scale predicts similar increases in amount

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[18] Only 41 respondents participate in the sender role of the trust game, while all respondents participate in the stag hunt and donation task.

transferred by the sender in the trust game ($3.02). Estimates of the effects of increased social capital measures on donations to the food bank are also economically significant. A one standard-deviation increase in the *sc* index predicts food bank donation that is $3.10 higher on average, and a one-point increase in *GSS_trust* predicts a food bank donation $2.65 higher on average. Regressions reported in columns 2, 5, and 8 include the full set of demographic variables; and those in columns 3, 6, and 9 include block fixed effects. The estimated coefficients from these regressions are of a similar magnitude to those that do not include demographics or fixed effects, though some of them lose statistical significance[19]. Results from the stag hunt game are too imprecisely estimated to draw statistically or economically significant conclusions, though it should be noted that incentives on this task were set much lower than the trust game or food bank donation tasks due to constraints on the research budget, to the extent that the size of the maximum social surplus in the stag hunt was 5 times smaller than in the trust game. It is also possible that the subject pool may have found the stag hunt's abstract bi-matrix presentation too unfamiliar to devote much thought to. Indeed, some respondents communicated their difficulty understanding this task.

We finally consider crime-induced shifts in our attitude measures as instruments for the incentivized behavior in the three decision tasks. **Table 20** displays two-stage least squares regressions analyzing the amount sent in the trust game (columns 1-3), whether or not the respondent invested in the stag hunt game (column 4-6), and donation to the food bank (column 7-9). Columns 1, 4 and 7 do not include either fixed effects or controls, columns 2, 5, and 8

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[19] The regression of *send* on *sc* with demographics yields a *p*-value of .110; and those of *GPCFB* on *sc* and *GSS_trust* yield *p*-values of .162 and .243, respectively.

include block fixed effects, and columns 3, 6, and 9 control for a limited set of demographic information. Specifically, we only control for *black*, *mother_us*, *divorced*, and *widowed* since we have limited degrees of freedom and weak instruments (these are the demographic variables that **Table 17** indicates differ significantly between the treatment and control groups). Point estimates suggest that respondents with higher surveyed social capital and generalized trust send more on average in the trust game, and donate more to the food bank. These effects are imprecisely estimated, and while not significant, they are of the same magnitude as those estimated under OLS. A one standard deviation increase in the *sc* index predicts that the respondent will send an average of $7.56 more in the trust game and donate $6.58 more to the food bank (*p*-values are .120 and .153, respectively). Recall for comparison these estimates were $2.59 and $3.10 under OLS. The bottom pane of **Table 20** shows that a one point increase in the 5-point *GSS_trust* Likert scale predicts a $8.88 increase in amount transferred by the sender in the trust game, again with a large standard error (*p*-value .192). A one-point increase in *GSS_trust* predicts a food bank donation $9.04 higher on average (*p*-value .216). Compared to the OLS estimates, it now seems that respondents with higher social capital are *less* likely to invest in the stag hunt, though the fact that such effects only show up under instrumental variables (and like all the IV estimates, those for the stag hunt are rather imprecise, though generally at least as precise as those for the trust game and food bank donation). That the OLS estimates are indistinguishable from zero implies that only the treated respondents display this correlation between attitudes and behavior[20]. Including either block fixed effects or demographic variables in IV specifications does not discernibly affect the estimates.

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[20] When the regressions of **Table 19** are restricted to the subsample born after 1970 and

Table 17. Differences in means by treatment group

	Treated	Control	Difference (p-value)
sc	-.278	.190	-.468 (.024)**
gss_trust	2.77	3.15	-.375 (.170)
send	13.2	16.7	-3.45 (.176)
stag	.681	.500	.181 (.146)
gpcfb	20.7	24.1	-3.39 (.211)
male	.455	.481	.027 (.835)
black	.273	.074	.199 (.066)*
hispanic	.000	.019	-.019 (.322)
other_race	.000	.019	-.019 (.322)
year_born	1958	1953	4.76 (.278)
born_us	1.00	.963	.037 (.159)
mother_us	1.00	.944	.056 (.083)*
father_us	1.00	.926	.065 (.044)
children	.318	.333	.015 (.932)
people_over_18	.773	1.13	-.357 (.127)
married	.500	.389	.111 (.391)
divorced	.000	.148	-.148 (.004)***
widowed	.000	.111	-.111 (.013)**
separated	.000	.037	-.037 (.159)
education	15.3	15.7	-.431 (.506)
employed	.636	.574	.062 (.587)
years_in_pgh	14.0	13.7	.333 (.706)
assess12	67927	68093	-165 (.985)
assess13	108673	92660	16013 (.545)
n	22	54	

possessing at least some college education (a comparable population to that studied in Chapter 1), we do find that respondents who score higher on *sc* and *GSS_trust* are significantly more likely to invest in the Stag Hunt game. Results for the trust game and food bank donation are qualitatively similar between the full sample and the young and educated subsample. IV regressions on this subsample yield no significant results, though IV coefficients are similar to the OLS coefficients in this subsample much like in the full sample.

Table 18. Differences in means by treatment group

	Dependent variable							
	sc				*GSS_trust*			
	1	2	3	4	5	6	7	8
treated	-.516*** (.184)	-.601*** (.185)	-.665** (.320)	-.874*** (.288)	-.375 (.277)	-.485 (.333)	-.609* (.373)	-.920* (.459)
constant	.205 (.102)	.229 (.053)	-19.6 (29.1)	-3.61 (35.8)	3.15 (.127)	3.18 (.096)	-22.5 (26.5)	3.50 (32.9)
Controls	N	N	Y	Y	N	N	Y	Y
Fixed effects	N	Y	N	Y	N	Y	N	Y
n	76				76			
F	7.84	10.57	–	–	1.83	2.12	–	–

Table 19. OLS relationships between surveyed social capital and incentivized decision tasks

	Dependent variable								
	send			*stag*			*gpcfb*		
	1	2	3	4	5	6	7	8	9
sc	2.59**	3.16	3.60**	-.039	-.046	-.027	3.10***	3.38***	1.81
	(1.25)	(1.89)	(1.53)	(.057)	(.066)	(.073)	(1.09)	(1.26)	(1.26)
constant	15.9	85.8	16.0	.554	-13.4	.554	22.9	226	23.0
	(1.35)	(214)	(.218)	(.058)	(11.1)	(.004)	(1.05)	(254)	(.070)
GSS_trust	3.02***	3.90***	3.14*	-.067	-.096*	-.046	2.65**	2.87**	1.76
	(1.11)	(1.28)	(1.55)	(.054)	(.058)	(.067)	(1.23)	(1.43)	(1.47)
constant	7.15	35.0	6.84	.755	-14.2	.691	15.0	234	17.7
	(2.90)	(214)	(4.28)	(.176)	(10.3)	(.204)	(4.03)	(262)	(4.48)
n	41			76			76		
Controls	N	Y	N	N	Y	N	N	Y	N
Fixed effects	N	N	Y	N	N	Y	N	N	Y

Table 20. 2SLS regressions of incentivized task behavior on social capital measures using crime proximity as instrument

	Dependent variable								
	send			*stag*			*gpcfb*		
	1	2	3	4	5	6	7	8	9
sc	7.56	10.4	5.37	-.352	-.469*	-.412	6.58	7.01	4.20
	(4.87)	(17.3)	(7.25)	(.239)	(.285)	(.345)	(4.60)	(6.37)	(4.57)
constant	16.6	23.6	16.3	.572	.264	.575	22.7	20.3	22.9
	(2.05)	(6.55)	(1.56)	(.052)	(.684)	(.081)	(1.10)	(6.30)	(1.07)
GSS_trust	8.88	6.84	6.52	-.484	-.522	-.511	9.04	7.80	5.21
	(6.80)	(8.78)	(9.85)	(.338)	(.343)	(.472)	(7.30)	(7.67)	(5.96)
constant	-8.98	3.32	-2.49	2.02	1.60	2.11	-4.37	.311	7.27
	(18.9)	(23.3)	(27.2)	(1.02)	(.926)	(1.44)	(21.9)	(20.6)	(18.1)
n	41			76			76		
Controls	N	Y	N	N	Y	N	N	Y	N
Fixed effects	N	N	Y	N	N	Y	N	N	Y

3.5 CONCLUSION

Social capital is considered to be relevant to the effective functioning of societies. We present a novel experimental design that explores the impact of property crime on several dimensions of social capital. Our findings are threefold. First, we explore the use of factor analysis to measure social capital. The survey we develop measures a broad set of attitudes that are both relevant to how people see themselves in and interact with their community, and which are also plausibly disrupted by crime. Factor analysis indicates that it may be possible to link the responses to this wide range of attitude questions to a single underlying index of social capital. We compare this index to a question that is used as a standard measure of social capital and find that they are closely related and have similar explanatory potential, with the broader index seeming to be more powerful. Secondly, our quasi-random empirical design lets us evaluate the impact of one potential type of shock to social capital, namely property crime. We find that both our broad measure and the standard measure are significantly and adversely impacted by proximity to burglaries. This result is robust to controlling for respondent demographics and city block-level variation. Finally, there is suggestive evidence indicating that shocks to social capital may have material consequences. Ordinary least squares shows that respondents with lower baseline levels of social capital send less in an incentivized trust game and donate less to charity. Instrumental variables regressions attempt to show whether those respondents whose social capital is adversely impacted by proximity to crime are adjusting their behavior in these tasks. These regressions yield qualitatively compatible results with those from OLS, but coefficients are not precise enough to draw definitive inferences.

APPENDIX A

INCENTIVIZING CORRECT BELIEF REVELATION

Suppose that a subject holds the belief that, on average, there is probability p that others will choose to invest. Denote the realized number of n other people who will choose to invest by m_a and the guess submitted by the subject m_g. We can then write the subject's payoff from Estimation Task 1 as

$$\pi_1 = P\,(\, m_g = m_a\,) \cdot u\,(\,\$3 \text{ or } \$1.50\,)$$

where u is the subject's utility of money. Since partners are ex-ante identically drawn from the population of other people in the room, the probability above is binomial:

$$E\,(\,\pi_1\,) = P_{\mathrm{BIN}}\,(\,n, p;\, m_g\,) \cdot u\,(\,\cdot\,).$$

Maximizing expected payoff with respect to m_g yields the mode of the binomial distribution:

$$m_g = \lfloor\ (\,n + 1\,)\,p\,\rfloor$$

on all but an unmeasurable set of possible p. Since the experimenter observes n, it is possible to identify p as lying within a reasonably small interval.

A.1 SECOND-ORDER ELICITATION

A similar argument will be applied to the second-order belief elicitation, but we will need to maintain a more restrictive expected utility assumption. As actions could be considered binomial, we may consider responses to Estimation Task 1 to be multinomial with n trials (other people) and $n + 1$ possible responses ($m_g \in \{0, 1, \ldots, n\}$). Denote the probability of each response $p_0, p_1, \ldots, p_n$ with $\sum p_i = 1$. Subjects must guess the number of people giving each response, denote this vector $g = g_0, g_1, \ldots, g_n$ with $\sum g_i = n$. Denote the vector of actual responses to Estimation Task 1 $a = a_0, a_1, \ldots, a_n$ with $\sum a_i = n$. The subject's expected payoff from Estimation Task 2 is

$$E(\pi_2) = \sum P(g_i = a_i) \cdot u(\$0.25).$$

If we maximize with respect to g we simply get the modes of the marginal distributions (as the utility is linear in probabilities):

$$g_i = \lfloor (n + 1) p_i \rfloor .$$

APPENDIX B

BELIEF UPDATING

Table 21 displays estimated parameters from equations explaining elicited first-order beliefs. In all of these equations, current-period beliefs are regressed on last period's beliefs and whether or not one's partner invested, interacted with the conditions prevailing at the time (subjects in all treatments see the joint investment probabilities at the end of the round). There are individual subject and round fixed- effects. Since this is a lagged dependent variable model, the equation is estimated by two-stage least squares (Cameron and Trivedi, 2005). Round interactions are added since prior beliefs become more important relative to new information in later rounds. Since beliefs about both optimistic and pessimistic subjects are elicited each round, these appear as separate equations for treatments *BK* and *PK*. The right- most column pools beliefs from all treatments. Results are as we would expect given reasonable belief updating. Subjects whose partners invest are more likely to think that other people invest, though they discount observations of optimistic or second-order-optimistic partners investing.

Table 21. Instrumental variables regressions of beliefs on observable factors

Treatment	*BK*	*PK*	*BK*	*PK*	*IK*	*NK*	*All*
Dependent variable	Guess (opt.)		Guess (pess.)		Guess		
$Guess_{-1}$	.040 (.094)	.349** (.096)	.130 (.116)	.082 (.092)	.330** (.102)	.356** (.105)	.089** (.041)
Partner invests	.063 (.044)	.195** (.089)	.137 (.074)	.001 (.078)	.093 (.059)	-.055 (.118)	.043** (.021)
Partner invests×own prob.	-.012 (.056)	-.168 (.098)	-.119 (.079)	.003 (.097)	-.027 (.071)	.096 (.111)	-.014 (.022)
Partner invests×partner prob.	.032 (.046)	-.242** (.094)	-.087 (.076)	-.015 (.093)	-.133** (.065)	.016 (.100)	-.042 (.026)
Partner invests×round	.003 (.012)	-.035 (.025)	-.034 (.033)	-.011 (.021)	-.014 (.015)	.027 (.027)	-.003 (.008)
Partner invests×own prob. ×partner prob.	-.040 (.073)	.230** (.108)	.087 (.098)	.069 (.119)	.127 (.097)	-.094 (.122)	.040 (.034)
Partner invests×own prob. ×round	-.004 (.014)	.041 (.026)	.041 (.026)	.016 (.024)	.006 (.018)	-.025 (.021)	.004 (.006)
Partner invests×partner prob. ×round	-.020 (.012)	.045 (.024)	.023 (.032)	.017 (.023)	.024 (.017)	-.014 (.024)	.005 (.007)
Partner invests×own prob. ×partner prob.×round	.010 (.016)	-.053 (.030)	-.019 (.033)	-.037 (.030)	-.025 (.026)	.023 (.028)	-.010 (.009)

Round and subject fixed effects, jackknifed standard errors (in parentheses) treat the subject as the unit of observation, ** indicates significance at 5%

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