PEER EFFECTS IN ECONOMIC ATTITUDES*

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Abstract

Recent genetic evidence shows that fundamental economic attitudes, such as risk aversion and altruism, are largely determined by unspecified environmental factors. Using random assignment of MBA students to peer groups and predetermined survey responses of economic attitudes, we provide causal evidence that peer influence is one such environmental factor. We find positive peer effects in risk aversion, consistent with conformity, negative peer effects in honesty and altruism, consistent with self-interest, and no peer effects in trust. Overall, we show that fundamental attitudes, traditionally assumed to be immutable, are, in fact, heavily influenced by social interactions, even in adults.

Recent research shows that genetic traits account for about 20 percent of the variation across individuals in life-long economic attitudes such as risk aversion and altruism (Cesarini et al, 2010). Perhaps the most important implication of this finding is the converse result: economic attitudes are largely determined by environmental factors, contradicting the common theoretical assumption of immutable preferences. Since individual economic attitudes drive economic activity, identifying the environmental factors that influence attitudes is of great importance to researchers and policy-makers alike.

In this paper, we propose that peers may be an important influence on individual economic attitudes. Cultural norms within peer groups help shape individual attitudes. Akerlof (1980) and Bernheim (1994) present models where individuals conform to social norms to avoid a loss of social status and utility. In anthropology, conformity has been shown to be evolutionarily favored, akin to herding (Henrich and Boyd, 1998). These models predict positive peer effects, where individuals conform to peer group norms. In contrast, negative peer effects result when individuals seek status by differentiating themselves from their peers (Ridgeway, 1978; Akerlof, 1997), or when confirmation bias leads to polarized attitudes (Lord, Ross, and Lepper, 1979). Though a vast empirical literature finds evidence consistent with peer effects in a wide range of behaviors, including criminal activity (Kling, Ludwig, and Katz, 2005), educational achievement (Sacerdote, 2001), automobile purchases (Grinblatt, Keloharju, and Ikäheimo, 2008), and obesity (Christakis and Fowler, 2007), little evidence exists on peer effects in economic attitudes.

We study four key economic attitudes: risk aversion, honesty, altruism, and trust. These attitudes are central to economic theory, but relatively little is known about how they are formed. First, Gardner and Steinberg (2005) finds positive peer effects in risky health choices, such as binge drinking and unprotected sex. Since financial and health risk attitudes are correlated (Barsky, Juster, Kimball, and Shapiro, 1997), peer effects may also influence financial risk attitudes. Second, traditional public goods models predict negative peer effects in altruism, as individuals reduce pro-social behavior in the presence of highly pro-social peers. Alternatively, the threat of social sanctions may lead to positive peer effects in altruism (Leider, Mobius, Rosenblat, and Do, 2009). Similar arguments can be made for honesty and trust. Finding evidence of positive or negative peer effects in any of the four attitudes would be an important result because it would suggest

that economic attitudes are not as rigid as traditionally believed, but instead shaped by social interactions.

A number of obstacles hinder the empirical identification of peer effects (Manski, 1993). First, individuals may choose peers who have similar attitudes, which could lead to the false conclusion that peers have influenced an individual's attitudes, when in reality, the attitudes influenced the selection of peers. Second, it is typically difficult to separate the simultaneous influences of peers on an individual from the influence of the individual on her peers. Finally, the definition of salient peer groups, in which peers are likely to have a meaningful influence on an individual, is unclear.

We design a longitudinal survey of MBA students to address each of these empirical obstacles. First, we overcome the self-selection problem by exploiting the random assignment of students to one of six student sections to create exogenously-formed peer groups. Second, to overcome simultaneity, we survey students prior to starting the MBA program to measure predetermined economic attitudes, as well as after the first year of the program. This allows us to provide causal evidence on peer effects by relating the predetermined survey responses of an individual's future peers to her own attitudes *ex post*. Finally, at the end of the first year, we ask students about their self-selected friendships to verify that section peers have meaningful social relationships. On average, over 40 percent of friends in the MBA program are in the same section, compared to 17 percent that would occur randomly.

Using the responses to our survey, we find significant peer effects in economic attitudes. First, we find positive peer effects in risk aversion, consistent with conformity. Using the method of Holt and Laury (2002) to elicit risk aversion, we find that a one-standard deviation increase in predetermined average peer risk aversion increases *ex post* own risk aversion by 0.21 standard deviations. In contrast, we find negative peer effects in honesty and altruism. To elicit honesty, we present a hypothetical situation where the respondent must choose whether to reveal the truth at a personal cost. Altruism is based on standard questions from the Davis Interpersonal Reactivity Index, designed to elicit dispositional empathy. We find that a one-standard deviation increase in peers' average predetermined honesty causes own honesty to decrease by 0.11 standard deviations. For altruism, we find a decrease of 0.21 standard deviations. These results are consistent with self-interested and strategic social interactions. As peers contribute more social goods, an individual

is likely to contribute less. Finally, we find no causal evidence of peer effects in trust, based on a standard question from the World Values Survey. This implies that trust is less flexible than other economic attitudes, at least in our setting.

These findings have far-reaching implications. Positive 'neighborhood' effects in financial risk aversion may help to explain home-bias preferences, excessive comovement in stock prices, and varying rates of stock market participation among different social groups (Hong, Kubik, and Stein, 2004). Positive social effects on risk aversion may also help to explain why some economic activities are clustered in time and within groups, such as industry merger waves. The implications of negative peer effects in altruism and trust are also compelling. These results provide insight into the intersection of social norms and economic interactions. In particular, since our survey elicits anonymous and non-directed attitudes, our results may be interpreted as further evidence that social sanctions are necessary for altruistic behavior (Leider, Mobius, Rosenblat, and Do, 2009). Finally, significant peer effects, both positive and negative, suggest that social interactions are a possible conduit through which individual preferences are aggregated to the level of society.

One concern with our results is that we could be identifying peer effects across all students in all sections, rather than within sections. This would lead us to falsely attribute changes in attitudes to section peers, when in fact the changes are influenced by an omitted factor common to all students. We verify that this is not the case by running placebo tests where we synthetically assign students to random sections, thus creating artificial peer groups. Using these hypothetical peer groups, we re-run our tests 1,000 times and investigate the distribution of outcomes. We find no significant peer effects in the simulations with synthetic peer groups, though the individual-level control variables have highly similar effects in both the tests using synthetic and actual peer groups.

A second concern is that though MBA students are an ideal population for our study, our results might not generalize to the entire population. The MBAs in our study come from many varied ethnic and racial backgrounds, typically have four or five years of work experience, and are about 28 years old, on average. Some are married and some have children. The advantages of this population for studying social networks are that they typically do not know anyone in the program before they begin, they are mature adults and are likely to have established economic attitudes, they are soon to enter the professional population and make important economic decisions, and lastly, they actively

build both professional and personal social networks during their MBA program as a resource for their careers after graduation. At the same time, people who enter MBA programs are likely to be more open to new ideas and could be more receptive to peer influence than the average population. Thus, though our sample could have well-formed economic attitudes, they could be more responsive to peer influence in their economic attitudes than the average population.

One could also be concerned that our measures of attitudes are from self-reported survey responses. Survey responses could be a poor proxy for actual behavior if respondents mis-state their preferences, knowingly or unknowingly. We have reasons to believe that our survey responses are valid. First, the survey questions we use are from well-established national or global surveys, such as the Generalized Social Survey and the World Values Survey. Second, prior research finds that survey responses about risk preferences and subjective probabilities are good indicators of actual behavior (Guiso, Jappelli, and Terlizzese, 1992; Hurd and McGarry, 1995). Third, the panel nature of our data identifies changes in the same measures of attitudes over time. As long as survey responses are correlated with actual behavior and this correlation does not change systematically over time, our measures are valid.

Additionally, survey responses provide benefits that behavioral peer effects studies do not have. By testing for attitudinal changes that are not directly observable, we may better distinguish what Manski (2000) calls 'preference interactions' from 'expectation interactions.' Preference interactions are when an individual's preferences depend on others' preferences. Expectation interactions are when expected outcomes are updated through observational learning from others' outcomes. Our survey of attitudes more likely reflects preference interactions than expectation interactions. While both represent social interactions, they have very different implications for economic theory and policy.

Our paper contributes to two fields of research. First, our paper provides some of the first direct evidence that fundamental economic attitudes are influenced by peers. Though many papers have investigated peer effects in risky health choices (e.g, Card and Giuliano (2011)), to the best of our knowledge, we are the first to study peer effects for financial risk preferences. Peer effects in altruism have been found in numerous papers that employ dictator games, (e.g., Leider, Mobius, Rosenblat, and Do (2009)). Our paper provides complementary evidence on altruism that is based

on attitudinal survey responses. Peer effects in honesty have been studied in limited contexts (i.e., academic cheating (Chang, Chen, and Lai, 2008)), though honesty is not typically measured relative to a monetary cost. Finally, trust has received a great deal of attention in economics, though we are unaware of any prior studies of peer effects in trust. Therefore, our results provide new evidence on the evolution of key economic attitudes.

Second, our paper provides foundational evidence for prior research on the importance of managerial social networks. The most closely related paper in this field is Lerner and Malmendier (2011). Similar to our setting, Lerner and Malmendier exploit random section assignments of Harvard MBAs to reveal peer effects in entrepreneurship. More generally, our results support recent evidence that business practices and managerial decisions are influenced by social networks (Cohen, Frazzini, and Malloy, 2008; Bizjak, Lemmon, and Whitby, 2009). In particular, our results complement the evidence in Shue (2011) which shows that peer influences during MBA programs have long-lasting implications for managerial decisions. Our paper is different than Shue's because we observe economic attitudes prior to the formation of peer groups, which allows us to present causal evidence, whereas Shue observes managerial decisions many years later. However, Shue's results imply that the peer effects we observe are not simply temporary shocks.

The remainder of the paper is organized as follows. Section I presents our empirical identification strategy. Section II describes our data and measures. Section III presents the results of our tests on peer effects. Section IV presents placebo tests of synthetically assigned random sections. Section V concludes.

I. Identification of Peer Effects

Following a variation of Graham and Hahn (2005), a standard linear-in-means model of peer effects is as follows:

$$A_{i,s} = \beta \overline{A}_{-i,s} + \gamma \overline{X}_{-i,s} + \delta X_{i,s} + u_{i,s}$$

$$u_{i,s} = \alpha_s + \varepsilon_{i,s} \tag{1}$$

where $A_{i,s}$ is the attitude of individual i in peer group s; $\overline{A}_{-i,s}$ is the leave-one-out average attitude of all individuals in group s, excluding individual i; X is a vector of predetermined characteristics for either the peers ($\overline{X}_{-i,s}$) or individual i ($X_{i,s}$); and α_s are unobserved correlated group effects.

Manski (1993) highlights three key challenges to identifying peer effects. First, individuals could choose peers who have similar attitudes (correlated effects). This is reflected in Equation 1 by the presence of the unobserved α_s term. This could lead to the false conclusion that peers have influenced an individual's attitudes, when in reality, the attitudes influenced the selection of peer groups. Second, peer effects could include simultaneous influences of peers on an individual, and of an individual on her peers (the reflection problem). The reflection problem means that separating peers' direct influence (endogenous effects ($\overline{A}_{-i,s}$)) from effects based on predetermined characteristics of peers (exogenous effects ($\overline{X}_{-i,s}$)) is only possible under a set of restrictive assumptions. Finally, Manski makes the important point that peer groups must be defined such that they capture true social interactions. The following describes how we address each of these challenges.

I.A. Self-Selection

To address the presence of correlated effects that occur when peers are self-selected, we use randomly-assigned peer groups. Similar to the random assignment of MBA students at Harvard Business School, detailed in Lerner and Malmendier (2011), incoming MBA students at Michigan are randomly assigned to one of six sections. The administrators who make the section assignment informed us that they try to maximize diversity within sections across a number of dimensions, while keeping the sizes of the section roughly equal. The following dimensions are equally weighted in the assignment process: gender, ethnicity, citizenship, undergraduate institution, employer, and dual-degree students. In addition, spouses or partners who are in the program together are separated into different sections. This randomization is highly similar to the one used at HBS, though with fewer dimensions and a less complex algorithm.

For the randomization to eliminate unobserved group effects, the section assignments must be orthogonal to the underlying determinants of the economic attitudes we study. Prior research has identified correlations between gender and risk aversion, altruism, honesty, and trust (Croson and Gneezy, 2009; Andreoni and Vesterlund, 2001; Whitley, Nelson, and Jones, 1999; Sapienza, Toldra,

and Zingales, 2007). In addition, empirical evidence shows that wealth is correlated with risk aversion (Guiso and Paiella, 2008) and cultural background is correlated with trust (Guiso, Sapienza, and Zingales, 2009). Because the randomization specifically attempts to maximize diversity within sections for gender and ethnicity, it is unlikely that there are group-specific effects driven by these demographic characteristics. Wealth is not directly used in the randomization, though other dimensions will likely be correlated with wealth. Though we control for these effects in our tests, the randomization of section assignments based on some of these dimensions will only diminish the power of our tests.

It should be noted that randomization does not remove group-specific shocks. If the experiences of the sections vary over time in a way that affects economic attitudes, our results could be biased. However, this is unlikely to be the case in an MBA program. Students are sensitive to differences in teaching, curriculum, programs, or services across sections, and any deviations will be quickly corrected. Instructors are alert to this sensitivity and try to maintain uniformity across sections. We are unaware of any student complaints about differential treatment between sections in the past or during our sample period. Therefore, we ignore section-specific shocks. Thus, conditional on these factors, and based on the randomization procedure used, we can credibly assume that α_s is zero in our tests.

A second issue is that the leave-one-out average creates a mechanically high correlation between variables. This occurs because the covariances of the leave-one-out averages are the same as the individual-level averages, but the standard deviations are diminished because the extreme responses are smoothed out with the averages. This leads to highly correlated right hand side variables. Therefore, we exclude the predetermined peer exogenous characteristics from the model and interpret the average peer attitudes as both endogenous and exogenous effects. These considerations produce the following model:

$$A_{i,s} = \beta \overline{A}_{-i,s} + \delta X_{i,s} + \varepsilon_{i,s}. \tag{2}$$

Though random assignment allows us to overcome self-selection bias, it can potentially create a different obstacle to identifying peer effects. If the average attitudes we study do not vary

across sections, we can not identify peer effects. This is because peers would be identical in any randomization and we could not attribute effects to actual peers rather than common shocks across all sections. For instance, if the first-year experience of the MBA program makes all students less risk averse, we could falsely attribute this to peer effects if all peer means were equal across sections. Instead, identification requires randomly occurring variation in mean attitudes across sections, orthogonal to the section assignment procedure. With six sections of moderate size, we do find variation (documented in a later section), though some attitudes have greater variation than others.

I.B. The Reflection Problem

The model in Equation 2 does not address the reflection problem, and can only be regarded as correlational evidence. To provide causal evidence, we follow Sacerdote (2001) and Guryan, Kroft, and Notowidigdo (2009), and use predetermined variables to overcome the simultaneity inherent in the reflection problem. In particular, we run cross-section regressions using attitudes of future peers from surveys conducted prior to peer group social interactions:

$$A_{i,s} = \beta \overline{A}^{Pre}_{-i,s} + \delta X_{i,s} + \varepsilon_{i,s} \tag{3}$$

where $\overline{A}^{Pre}_{-i,s}$ indicates the average predetermined attitude of individual i's future peers.

Though the use of predetermined variables allows us to separate endogenous and exogenous peer effects, we can not include specific exogenous peer effects due to the multicollinearity inherent in the leave-one-out average peer variables. One way to address this issue is by including individual fixed effects. This will control for all time invariant aspects of both individuals and their peers. However, the individual fixed effects would capture all of the variation in the predetermined variables, including the predetermined attitudes. To overcome this problem, we use the following specification, following Stevenson (2010):

$$A_{i,s,t} = \tau_t + \beta I \times \overline{A}^{Pre}_{-i,s,t} + \kappa_i + \varepsilon_{i,s,t}, \tag{4}$$

where I is an indicator variable for the post-randomization period. This allows us to observe the dynamic response from the time before peer group interaction to post interaction, based on the predetermined attitudes of peers. For instance, a positive β indicates that an individual whose peers have higher predetermined risk aversion has a greater increase in own risk aversion than someone whose peers have lower risk aversion, controlling for common shocks and all time-invariant characteristics of the individuals and peers. Thus, this model separates the contextual or exogenous predetermined peer effects from the direct endogenous peer effect. Since our attitudes are not directly observable by peers, it is more reasonable to expect to find contextual rather than endogenous peer effects. We discuss this issue in greater detail later in the paper.

I.C. Formation of Peer Groups

To draw inferences from peer effects models, peer groups need to be relevant for social interactions. Though randomly-assigned peer groups do not suffer from selection bias, they could not actually reflect boundaries of meaningful social interactions. Stinebrickner and Stinebrickner (2006) define relevant peers as those with "potential influence." For instance, we also have data on within-section random project assignments for a number of classes. Given that these groups are only relevant to one single class and could only meet as a group a few times, they are unlikely to have substantial influence on economic attitudes, though they do not suffer from selection bias.

We have several reasons to believe that section groups contain peers of "potential influence." First, Shue (2011) provides detailed descriptive evidence that shows that MBA students take great pride in their sections at Harvard. Michigan follows a similar model and also encourages section pride. Students take all their core classes with section-mates, comprising the large majority of courses taken in the first-year sequence. Non-MBA students are not allowed to take classes attended by the first-year MBA students, but are offered separate sections instead. In addition, students are only allowed to change sections under extreme circumstances.

Beyond this descriptive evidence, we also empirically verify that section peers are important sources of social interaction. In the second wave of the study, we ask students to name up to 15 friends and 15 professional contacts within the first-year MBA cohort. On average, 43 percent of listed friends and 36 percent of listed professional contacts are in the same section, each statistically

different from 16.7 percent, the fraction that would be observed if friends and contacts were drawn from all sections equally. The ability to verify that randomly assigned peer groups reflect true social interactions is one of the advantages of our survey-based study.

We are careful to point out that we can not capture all peer effects using random section assignments. Certainly, influential peers exist in other sections, in other cohort years, and outside of the business school. This means that any peer effects we discover would be a lower bound.

II. Data and Variables

Our data come from two online surveys conducted at the University of Michigan, where we solicited responses from all incoming first-year students in the Day-MBA program of the Ross School of Business. The first survey wave collected responses from mid-August until early September of 2010. This period represents the pre-randomization period and begins before the first classes were held and ends two weeks after classes began. We allowed survey responses after classes began in order to increase the participation rate by meeting the students in-person to encourage participation. Since only two weeks had passed, we do not feel that any significant peer effects would contaminate the survey results. The second survey wave was conducted at the end of the first academic year in April 2011, after seven months of potential social interaction.

In each survey wave, participants were compensated by receiving a $5 giftcard and by being entered to win one of three iPads, valued at about $500. Since MBA students are wealthier and have greater time constraints than undergraduates, we used a relatively large compensation to try to induce a higher response rate.

We follow standard methods to control for survey response bias. First, to reduce order effects, the order of survey questions was counterbalanced in blocks across participants. Second, questions were altered slightly between survey waves to reduce practice effects or fatigue effects, while maintaining the same interpretation of the responses. The survey waves were administered seven months apart, so repeated measurement issues are unlikely to bias our results.

Of the total cohort of 494 first-year MBA students, we received responses from 331 students for the first survey and 196 students in the second wave.[1] However, we necessarily restrict attention to students who completed both surveys with answers to our relevant questions, leaving a response rate of 39.7 percent of the total cohort and an attrition rate of 40.8 percent. These rates compare to other surveys that use MBA students as participants (53% response rate in Kaniel, Massey, and Robinson (2010a, 2010b) and 35.6% attrition rate in Hussey (2011)).

II.A. Measures of Economic Attitudes

We next describe our measures of the four dimensions of economic attitudes studied in this paper: risk aversion, honesty, altruism, and trust.

A.1. Risk Aversion

To elicit risk aversion, we use a multiple price list (MPL) design, following the procedure in Holt and Laury (2002). In each survey, participants were asked to choose 10 times between two pairs of lotteries. Each lottery includes a high and low payoff, but the first lottery (Option A) has less variability between the payoffs compared to the second lottery (Option B). Proceeding from the first to the tenth lottery-choice, the probability of the high payoff increases equally for both lotteries. As a result, in the first four lottery-choices, the expected payoff is greater for Option A than Option B; in the fifth and subsequent lottery-choices, the expected payoff is greater for Option B. Therefore, a risk neutral individual would prefer Option A in the first four lottery-choices and Option B in the last six lottery-choices. The later the subject switches to Option B, the more risk averse she is. The tenth lottery-choice presents a certain outcome in both options, with Option B providing a higher amount. This means that the tenth choice can be used as a test of subject understanding of the questions. Therefore, we omit any observations where the participant chose Option A in the tenth lottery. In addition, to ensure consistency, we drop observations where the subject switched to Option B and then switched back to Option A in a subsequent choice. Our measure of risk aversion is the lottery-choice number where the subject switched from Option A

[1]Part of the drop-off in response rate could be explained by survey-fatigue from a small intermediate survey we conducted in January 2011 (which is not used in this paper), though most of the drop-off probably reflects the increased opportunity cost to MBA students who are preparing for exams and searching for summer internships).

to Option B. All of the outcomes are hypothetical, as no lottery awards are paid to the survey respondents. We alter the hypothetical payment level slightly from the first to the second survey. More details are available in the appendix.

This measure of risk aversion is appealing for a number of reasons. Though the complexity of the method of Holt and Laury (2002) requires more effort from the participants than other methods of eliciting risk aversion (e.g., Becker, DeGroot, and Marschak (1964)), it is transparent to participants and provides greater variability in outcomes. Since our sample is taken from MBA students, the level of mathematical complexity required is unlikely to be a problem. Second, the MPL design is also stable over time (Harrison and Rutsröm, 2008), which will reduce noise in our panel estimates.

A potential concern with our implementation of an MPL is that Holt and Laury (2002) show that risk aversion increases when faced with large-scale actual payouts compared to hypothetical payouts, as we use in our survey. We appeal to the panel nature of our data to address this concern. As long as risk aversion using hypothetical payouts is correlated with risk aversion using actual payouts, we can identify the difference from predetermined risk aversion to *ex post* risk aversion, within individuals. A final concern with this method is that order effects could confound treatment effects, since the probability weights increase monotonically from the first to the last question. However, presenting the lottery choices in random order would likely be much more confusing for participants.

Table I presents summary statistics for our measure of risk aversion. Using the *ex post* individual responses, we find that participants are risk averse, switching to the risky response (Option B) on lottery-choice 6.5, on average, with the median at seven. This is consistent with the results reported in Holt and Laury (2002) and Holt and Laury (2005), using samples of undergraduates, MBA students, and professors, and both real and hypothetical outcomes. There is also a range of outcomes with a standard deviation of 1.7 and an interquartile range of three compared to a total range of 9.

A.2. Honesty

We measure honesty using responses to a question where the respondent chooses to reveal the truth at a personal cost, or to be dishonest at no cost. Following Innes and Mitra (2009), we ask students whether, as a seller of a used car, they would tell a potential buyer about a hidden mechanical problem and accept a discount or not tell the truth and receive a higher sales price. In the second survey, we changed the setting to selling a computer with a lower than advertised processor speed. See the appendix for details.

As with risk aversion, we use hypothetical costs to elicit honesty, which could not be a true reflection of reality. The concern is that in our survey setting, it is costless to appear truthful, and participants will choose to inform the buyer of the problem to appear noble to the researchers. In the extreme, all of the students will choose to reveal the hidden problem, making the measure useless. Second, wealthy students could find the fixed dollar cost of truthtelling to be small relative to a poorer student. However, as before, we address these issues by relying on the panel nature of our data to capture changes in responses following social interactions.

We find that not all participants choose to reveal the problem. As we report in Table I, 86 percent of respondents indicated that they would reveal the problem. This is slightly more than the 70 percent honesty rate reported in Innes and Mitra (2009) for a sample of undergraduate students.

A.3. Altruism

Altruism is measured as the average response over four questions about empathy. The questions were first formulated as part of the empathic concern subscale in the Davis Interpersonal Reactivity Index (Davis, 1980, 1983), and are also included in the General Social Survey (GSS) of 2002 and 2004, a widely used survey in sociology. Participants are asked to indicate on a scale from one to five how well they are described by each of these four statements:

(1) I often have tender, concerned feelings for people less fortunate than me.

(2) Sometimes I don't feel very sorry for other people when they are having problems.

(3) When I see someone being taken advantage of, I feel protective towards them.

(4) Other people's misfortunes do not usually disturb me a great deal.

The responses to statements two and four are normalized so that all four responses indicate altruistic attitudes and the average is taken. Since these statements are not directly tied to any dollar amount, they have the advantage that wealth effects will not have a mechanical bias, as is possible in the honesty questions. Thus, these questions can be considered to be more general than altruism and measure dispositional tendencies to be responsive to others' needs. Nevertheless, measures of general empathy and economic altruism are highly correlated (Smith, 2003).

The average respondent in our sample has an average score of 3.25 where the standard deviation is 0.71. This mean is somewhat lower than the mean of 3.91 from the GSS for the generalized population, but closer to 3.73 for GSS respondents who were 28 years old, matching the average age in our sample. The lower altruism of our sample also likely reflects the large numbers of unmarried and male students, who also have lower empathy scores in the general population.

A.4. Trust

Finally, to measure trust, we ask participants to respond to the following question, taken from the World Values Survey:

> Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?

Participants could respond, "Most people can be trusted," or "Can't be too careful." This measure of trust has been used widely throughout economics, most notably in a series of papers by Guiso, Sapienza, and Zingales (2006, 2008, 2009).

In Table I, we report that about 51 percent of participants believed that people could be trusted, in general. This is higher than the 26.1 percent reported in the 2005–2007 waves of the WVS, but closer to the 38.7 percent for U.S. respondents aged 30 to 49 years and the 40 percent trustfulness for U.S. males.

II.B. Variation Across Sections

In Panel B of Table I, we present average scores for the four economic attitudes in the *ex post* period by section. If all sections were identical through random assignment, we could not identify any peer effects. Instead, we rely on naturally occurring variation in economic attitudes across

sections that is unrelated to the section-assignment randomization. Panel B reveals that there is variation across the sections. Risk aversion ranges from 5.79 to 7.07, honesty from 0.79 to 0.97, altruism from 3.09 to 3.39, and trust from 0.48 to 0.57. The last column presents the coefficient of variation across the sections to normalize the levels. Honesty has the greatest variation and altruism, the least.

II.C. Demographic Profile of Sample

Next, we present the demographic background of our sample, as well as comparison samples in Table II. The average age of respondents is 27.9 years, with 33 percent women, and 6.6 percent who have children. Racial background is varied, with 55 percent white, 24 percent Asian, 13 percent Indian, six percent Hispanic, three percent black, and 2 percent other races. Students could select multiple races or they could prefer not to answer, so percentages do not add to one. In untabulated results, country of origin and ancestry are also varied. There is also variation in the income level of students in the year prior to starting the MBA program. Most commonly, 37 percent of students made between $60,000 and $90,000, though 17 percent made less than $30,000 and over five percent of students made over $120,000. These statistics reveal that our sample includes a wide variety of backgrounds. In addition, these data reveal that our sample represents mature adults who are less likely to be influenced by their peers than are young adults and teenagers.

Attrition in responses between the survey waves could lead to sample bias if those who choose to respond to both surveys differed from than those who only responded to the first survey in a way that is correlated with the determinants of economic attitudes. Column (2) of Table II presents averages for the sample of respondents who only responded to the first survey and column (3) indicates statistically significant differences between the two samples. Those who only responded to the first survey are statistically older, but not meaningfully so (29.9 vs. 27.9). They are also more likely to be Hispanic and to fall in one of the lower income categories. In our tests, we control for income and race, to help alleviate any bias this could cause.

Comparing economic attitudes between the subsamples, we find a minor statistically significant difference for altruism, though not for any of the other three attitudes. The difference means that we could not be able to generalize our results on altruism to larger samples, though we do not

feel this to be a major concern as the explanatory variables are roughly equal across subsamples. In unreported tests, we also verify that our sample is similar to the entire 494 student population across the dimensions that we can observe, namely age, gender, and race.

It is an important caveat that all of our participants are MBA students at a prestigious and expensive university. Therefore, our results could not apply to the entire population. However, given the variety of backgrounds our our sample, we feel that our results can generalize beyond just MBA students and are likely a better reflection of the population making important economic decisions than is a sample of undergraduate students, typically used in survey-based research.

II.D. Tests of Random Assignment

Before investigating the presence of peer effects, we test for random assignment across sections with respect to economic attitudes. As is customary, we regress predetermined individual attitudes on predetermined mean peer attitudes, following Sacerdote (2001). Since these attitudes are measured prior to any meaningful social interaction, coefficient estimates that are different than zero would imply a non-random assignment procedure with respect to attitudes. However, as noted by Guryan, Kroft, and Notowidigdo (2009), the leave-one-out average creates a mechanical negative relationship between individual and peer attitudes within sections. In Monte Carlo simulations, Guryan et al. show that this test of random assignment is negatively biased.

To address this bias, Guryan et al. suggest comparing coefficient estimates to empirical distributions of coefficients created from simulations with synthetically-created random assignments.[2] Therefore, we run 1,000 simulations of each regression where students are synthetically assigned to different sections, with equal likelihood. To test for random assignment, we compare the peer effect coefficient estimated using the true section assignment to the empirical distribution of the coefficient from the simulations.

Results on randomization are presented in Table III. Panel A presents the regression results from the true section assignments and Panel B presents the empirical distribution of the peer effect coefficient from the simulations. For each attitude, we show results with no controls and with a host

[2]Guryan, Kroft, and Notowidigdo (2009) provide a less computationally intensive correction by including the average outcome measure for all potential peer group members, selected or otherwise. However, this requires having multiple sub-populations of potential peers. Since we only have one common source of peers, this correction is infeasible.

of predetermined individual level controls including gender, age, a dummy for parenthood, income, marital status, and race. For risk aversion, altruism, and trust, we find insignificant coefficients for the effect of the peer average. For honesty, we find significantly negative coefficients in both models. However, Panel B reveals a negative bias in these tests, as shown by Guryan, Kroft, and Notowidigdo (2009).

Using the empirical distribution of the coefficient from the Monte Carlo simulations reveals that none of the attitudes have significant relationships between individual and peer averages in the predetermined period. For honesty to have a significant relationship at the 10 percent level, the coefficient would need to be less than -3.73 or greater than 0.500, in the specification including controls. Instead, the coefficient using the true randomization is -2.23. These results confirm that students are randomly assigned across sections with respect to economic attitudes.

III. Results on Peer Effects

Having established random assignment, we now estimate peer effects. We begin by presenting the estimates of Equation 2 in Table IV. Recall that this model can not separate endogenous from exogenous effects and so we can not regard these results as causal. Instead these results provide correlational evidence of peer effects in the *ex post* period.

First, we find significant positive peer effects for risk aversion. In the *ex post* period, individuals with more risk averse peers are more risk averse than individuals with less risk averse peers. Though not causal, this result is consistent with a desire for conformity in preferences for risk. It is worth reiterating that random assignment rules out the hypothesis that this result is caused by students selecting similar peers. We find no correlation between risk aversion and gender or age, but students with children are significantly less risk averse than students without children. Finding no difference in risk aversion based on gender confirms a number of studies that show that gender differences vanish when samples are restricted to professionals such as mutual fund managers, general managers, and entrepreneurs (Croson and Gneezy, 2009).

Next, we find significant negative peer effects for altruism. Consistent with models of public goods, an individual's pro-social attitudes are lower in the presence of peers with more pro-social attitudes. As opposed to observed altruism in experiments, the anonymous and non-directed nature

of our attitudinal survey questions provides evidence that though altruistic behavior could change, underlying preferences could not. This supports the evidence in Leider, Mobius, Rosenblat, and Do (2009) that altruism is driven in part by expected reciprocity. In the absence of explicit reciprocity and the disutility from the shame of one's peers, individuals seem to act according to rational economic man. We also find that women have higher scores of altruism than men.

Trust also shows a negative peer effect. Individuals that have peers that are more trusting, tend to be less trusting themselves. This is also consistent with the traditional view that a self-interested agent strategically free-rides on peer behavior. Finally, we find no significant effect peer effect in honesty, though we do find that having children is associated with greater honesty.

The magnitudes of the peer effects are substantial. Correct interpretations of the coefficients must account for the lower variance in the peer averages, compared to the individual levels. A one-standard deviation increase in the peer average risk aversion increases individual risk aversion by 0.18 standard deviations. A one-standard deviation increase in average peer altruism is associated with a decrease of 0.29 standard deviations of own altruism. A one unit increase in peer trust is associated with a 0.30 decrease in own trust.

III.A. Endogenous Versus Exogenous Peer Effects

Though the results presented so far are suggestive, they are not causal because of the reflection problem. Therefore, in this section, we estimate Equation 3, where we relate predetermined peer attitudes to *ex post* individual attitudes. For each attitude we report results from specifications with the same set of controls as before, and also estimates of Equation 4, which includes individual fixed effects. We first discuss the results that do not include fixed effects.

The coefficient estimates in Table V, show that risk aversion displays positive peer effects and honesty and altruism display negative peer effects. A one-standard deviation increase in predetermined peer average risk aversion increases *ex post* own risk aversion by 0.21 standard deviations. For honesty, the magnitude is -0.11, and for altruism it is -0.21. These are substantially large magnitudes, especially considering that our participants are mature adults, rather than teenagers or young adults.

Though trust displays peer effects in the correlations, we do not find a significant causal relationship between peers' trust and own trust. This confirms a number of recent papers that use trust as a central measure of culture, arguing that cultural beliefs do not vary over time (Gorodnichenko and Roland, 2010; Guiso, Sapienza, and Zingales, 2010). In contrast, our evidence suggests that risk aversion, honesty, and altruism are more flexible.

Though we can credibly assume that students are randomly assigned to their peers, and we control for a host of individual level controls, we would like to understand the mechanism through which peers are influenced. Since our measures are attitudes, they are not directly observable, but are likely manifested in a myriad of different observable behaviors. Therefore, we estimate Equation 4, where we use individual fixed effects to control for all possible exogenous peer effects based on predetermined characteristics of one's peers. Since the fixed effects capture all personal as well as peer time-invariant characteristics, a significant coefficient on peer averages implies a direct endogenous peer effect.

In all of the fixed effects regressions, we find insignificant coefficients on the peer effect interaction term. Thus the peer effects we identify are driven by predetermined characteristics of peers, rather than through direct endogenous effects. In our setting, this is what we would expect, since we are studying attitudes rather than behaviors. As Hanushek, Kain, Markman, and Rivkin (2003) points out, the distinction between endogenous and exogenous peer effects is confusing when measures of observed behavior are simply empirical proxies for pre-existing attitudes. Thus, we would expect to find greater exogenous contextual peer effects than peer effects driven by behaviors when the focus is on attitudes.

In sum, these results provide evidence of a sizable and causal relationship between the economic attitudes of one's peers and one's own attitudes. The positive peer effect for risk aversion, suggests that individuals conform to group risk-taking norms. This is consistent with the evidence for conformity in health risk among adolescents discussed previously. This result has important implications for how people choose to allocate financial portfolios and how asset prices are formed. Consistent with Hong, Kubik, and Stein (2004), our results suggest that peers could have substantial influence on the types of investments individuals make. In addition, our results could provide insight into the findings in Kumar (2009), which show that preferences for owning risky stocks are

related to the presence of local state lotteries. It is possible that this relationship is affected by social interaction, rather than simply correlations of an individual's preferences for gambling and stock ownership.

These results also shed light on the underlying forces behind altruism and honesty. Since our study elicits preferences with less social stigmas attached than experimental studies, we are able to provide new evidence that pro-social behaviors, such as altruism and honesty, are driven in part by social sanctions. In the absence of social sanctions, individuals appear to act according to the predictions of rational economic man.

Finally, as argued in the introduction, the finding of any peer effects on economic attitudes, positive or negative, is important. Since our sample is comprised of mature individuals who are likely to have well-established economic attitudes, it is clear evidence that social interactions have an immediate impact on even the most fundamental beliefs of economic actors. This has important implications for a wide-range of economic phenomena. For instance, these results could allow us to move towards a better understanding of large-scale trends in financial markets, such as bubbles, or clusters of corporate activity such as merger and IPO waves.

IV. Placebo Tests

As a robustness check, we conduct placebo tests to verify that our results are not driven by omitted variables. We run a 1,000 simulated causal regressions where students are randomly assigned to sections synthetically. Since peers in these groups are not actual peers, on average, we expect to find no significant causal effects.

The results of the placebo tests are presented in Table VI. Averages of coefficient estimates and standard errors over the 1,000 simulations are presented. The coefficients on the control variables are consistent with our prior findings. In contrast, the coefficient estimates of the peer effects are insignificant for all variables in all specifications. These results provide evidence that the peer effects we detect in our main tests are not driven by an omitted variable.

V. Conclusions

Peer influence is an important determinant of MBA students' economic attitudes. Average peer attitudes of risk aversion are positively related to individual risk attitudes, consistent with conformity. In contrast, greater pro-social peer attitudes, such as honesty and altruism, are negatively related to individual pro-social attitudes. This implies that in the absence of social sanctions, as in our data, individuals are likely to free-ride on their peers' pro-social behavior. Individual trust is not causally influenced by peer trust. These findings are robust to self-selected peers and simultaneity in peer influence.

The results demonstrate that fundamental individual preferences, central to much of economic theory, are malleable. Given that our sample is comprised of mature adults, rather than adolescents or children, finding meaningful and immediate peer influences for individual attitudes implies that attitudes are likely to vary over time in response to changes in peer group attitudes. This has important implications for economics and finance. For instance, these results could help to explain why a culture of fraud exists in one industry and not another, why stock prices exhibit excess comovement, or why merger waves cluster in industries and time. Finally, our study provides a groundwork for understanding the formation and evolution of long-lasting social networks of executive managers.

Appendix A. Survey Measures of Economic Attitudes

This appendix provides the questions used to elicit risk aversion, honesty, altruism, and trust.

I.A. Risk Aversion

"Below is a list of 10 decisions where you are asked to choose Option A or Option B. Each option is a different lottery with different probabilities of winning different amounts. For example, in the first decision, Option A is a lottery that pays \$20,000 with a probability of 10% and pays \$16,000 with a probability of 90%. Option B is a lottery that pays \$38,500 with a probability of 10% and pays \$1,000 with a probability of 90%. You are asked to choose which of these two lotteries you would prefer to play. In summary, you will make 10 choices: for each row you will choose either A or B."

Option A: 1/10 of \$20,000; 9/10 of \$16,000	or	**Option B:** 1/10 of \$38,5000; 9/10 of \$1,000
Option A: 2/10 of \$20,000; 8/10 of \$16,000	or	**Option B:** 2/10 of \$38,5000; 8/10 of \$1,000
Option A: 3/10 of \$20,000; 7/10 of \$16,000	or	**Option B:** 3/10 of \$38,5000; 7/10 of \$1,000
Option A: 4/10 of \$20,000; 6/10 of \$16,000	or	**Option B:** 4/10 of \$38,5000; 6/10 of \$1,000
Option A: 5/10 of \$20,000; 5/10 of \$16,000	or	**Option B:** 5/10 of \$38,5000; 5/10 of \$1,000
Option A: 6/10 of \$20,000; 4/10 of \$16,000	or	**Option B:** 6/10 of \$38,5000; 4/10 of \$1,000
Option A: 7/10 of \$20,000; 3/10 of \$16,000	or	**Option B:** 7/10 of \$38,5000; 3/10 of \$1,000
Option A: 8/10 of \$20,000; 2/10 of \$16,000	or	**Option B:** 8/10 of \$38,5000; 2/10 of \$1,000
Option A: 9/10 of \$20,000; 1/10 of \$16,000	or	**Option B:** 9/10 of \$38,5000; 1/10 of \$1,000
Option A: 10/10 of \$20,000; 0/10 of \$16,000	or	**Option B:** 10/10 of \$38,5000; 0/10 of \$1,000

I.B. Honesty

In the first wave of the survey, students were asked to answer the following question:

> "Suppose that you have been visiting a country called Bayeb. Before leaving the country permanently, you must sell your used car. A local person (unknown to you) agrees to buy the car for US \$2,000 and pay you in cash. However, you know that the radiator in your car is not functioning properly and the problem will only

become noticeable after 2 months. The buyer does not know about the problem. If you tell him/her about the problem, then you have to reduce the price of the car by US $250 and sell it for US $1,750. However, if you do not reveal the problem, then you can sell the car for US $2,000 and the buyer will have to fix the car after 2 months, spending US $250. Would you tell the buyer about the radiator problem?"

In the second wave, students were asked to answer this question:

"Imagine that your business is selling personal computers and that you recently sold a computer for $2,000. The computer was sold as having a 3.20 GHz processor. Afterwards you realized that the computer had a 2.80GHz processor. Would you tell the buyer about the discrepancy and offer a $250 rebate?"

I.C. Altruism

"The following statements ask about your thoughts and feelings in various situations. For each item indicate how well it describes you by choosing the number, where 1 indicates that it does not describe you very well and 5 means that it does describe you very well. Of course numbers 2-4 indicate that how well it describes you are in between these points."

- Sometimes I don't feel very sorry for other people when they are having problems.
- I often have tender, concerned feelings for people less fortunate than me.
- Other people's misfortunes do not usually disturb me a great deal.
- When I see someone being taken advantage of, I feel kind of protective towards them.

I.D. Trust

"Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?"

- Most people can be trusted
- Can't be too careful
- Don't know

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TABLE I
SUMMARY STATISTICS OF ECONOMIC ATTITUDES

Panel A. Summary Statistics

				Percentile				
	Mean	Std Dev	Min	25th	50th	75th	Max	Obs.
Ex Post Individual Attitudes								
Risk Aversion	6.506	1.726	1	5	7	8	10	158
Honesty	0.864	0.343	0	1	1	1	1	177
Altruism	3.250	0.707	1.5	2.75	3.25	3.75	4.5	172
Trust	0.510	0.502	0	0	1	1	1	157
Pre-Determined Average Peer Attitudes								
Risk Aversion	6.382	0.318	5.917	6.052	6.361	6.733	6.918	196
Honesty	0.849	0.029	0.784	0.825	0.862	0.870	0.896	196
Altruism	3.318	0.090	3.203	3.250	3.291	3.345	3.534	196
Trust	0.532	0.068	0.414	0.488	0.538	0.588	0.634	196

Panel B. Mean Variation Across Sections

	1	2	3	4	5	6	Hi−Lo	CV
Risk Aversion	6.833	6.818	5.792	7.071	6.381	6.000	1.279	0.079
	[24]	[33]	[24]	[28]	[21]	[28]		
Honesty	0.793	0.892	0.800	0.903	0.792	0.968	0.176	0.086
	[29]	[37]	[25]	[31]	[24]	[31]		
Altruism	3.194	3.270	3.385	3.217	3.087	3.323	0.298	0.032
	[27]	[37]	[24]	[30]	[23]	[31]		
Trust	0.478	0.500	0.565	0.481	0.500	0.536	0.087	0.067
	[23]	[36]	[23]	[27]	[20]	[28]		

Data are from survey responses of 196 first-year MBA students. 'Risk Aversion' is measured following Holt and Laury (2002). 'Honesty' is measured following a variation of Innes and Mitra (2009). 'Altruism' is the average response to four questions (each with a five-point Likert scale) from the empathic concern subset of the Davis Interpersonal Reactivity Index. 'Trust' is measured following the World Values Survey. Panel B presents means in the first row for each of six student sections. Numbers in brackets are observations. 'Hi-Lo' indicates the highest section average minus the lowest section average. 'CV' indicates the coefficient of variation across section averages.

TABLE II
SAMPLE DEMOGRAPHIC STATISTICS AND ATTRITION

	Response to Both Survey Waves (1)	Response to First Survey Only (2)	Significant Difference (3)
Demographic			
Female	33.16	34.07	
Age	27.88	28.85	**
Children Dummy	6.63	8.89	
Race			
White	54.59	56.30	
Asian	24.49	24.44	
Indian	13.27	14.81	
Hispanic	6.12	13.33	**
Black	3.06	2.22	
Other	2.04	1.48	
Income			
$30k or less	17.35	14.81	
$30k – $60k	22.45	32.59	**
$60k – $90k	37.24	34.07	
$90k – $120k	13.27	8.89	
Over $120k	5.61	5.19	
Economic Attitudes			
Risk Aversion	6.51	6.21	
Honesty	86.44	82.73	
Altruism	3.25	3.41	*
Trust	50.96	55.10	
Observations	196	135	

Data are from survey responses of first-year MBA students. Students can indicate multiple races or prefer not to provide race or income, so percentages do not add to one. 'Children Dummy' is one if the student has ever had any children. 'Risk Aversion' is measured following Holt and Laury (2002). 'Honesty' is measured following a variation of Innes and Mitra (2009). 'Altruism' is the average response to four questions (each with a five-point Likert scale) from the empathic concern subset of the Davis Interpersonal Reactivity Index. 'Trust' is measured following the World Values Survey. Significant differences are from $t-$tests, where $***$, $**$, and $*$ indicate significant differences at the 1%, 5%, and 10% level.

TABLE III
THE RELATIONSHIP BETWEEN OWN AND PEERS' PRE-DETERMINED ATTITUDES: EVIDENCE OF RANDOM ASSIGNMENT

Panel A. Regression Results

	Risk Aversion		Honesty		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peer Average	0.226	0.332	−2.248***	−2.225***	−0.627	−0.714	−0.274	−0.193
	(0.366)	(0.400)	(0.712)	(0.730)	(0.504)	(0.528)	(0.454)	(0.501)
Female		0.240		0.053		0.239**		−0.085
		(0.273)		(0.048)		(0.095)		(0.075)
Age		−0.052		−0.013***		0.029		0.007
		(0.060)		(0.004)		(0.023)		(0.005)
Children		−0.350		0.119		−0.349		−0.164
		(0.636)		(0.077)		(0.230)		(0.150)
Additional Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	−0.002	0.026	0.031	0.071	0.002	0.041	−0.003	−0.030
Observations	244	232	287	275	276	263	255	244

Panel B. Empirical Distribution of Peer Average Coefficient from Monte Carlo Tests

	Risk Aversion		Honesty		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1%	−7.454	−8.334	−8.166	−8.041	−7.894	−9.010	−8.970	−8.481
5%	−3.882	−3.435	−4.284	−3.726	−3.848	−3.289	−4.117	−3.827
25%	−1.234	−1.094	−1.252	−1.153	−1.250	−1.044	−1.214	−1.169
50%	−0.354	−0.331	−0.351	−0.313	−0.361	−0.322	−0.379	−0.310
75%	0.076	0.106	0.098	0.136	0.116	0.114	0.099	0.119
95%	0.409	0.488	0.446	0.500	0.447	0.502	0.474	0.474
99%	0.602	0.659	0.625	0.639	0.559	0.695	0.614	0.686
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Coefficients from OLS regressions are presented in Panel A. The dependent variable is listed at the top of the column. 'Risk Aversion' is measured following Holt and Laury (2002). 'Honesty' is measured following a variation of Innes and Mitra (2009). 'Altruism' is the average response to four questions (each with a five-point Likert scale) from the empathic concern subset of the Davis Interpersonal Reactivity Index. 'Trust' is measured following the World Values Survey. 'Peer Average' indicates the leave-one-out average of the dependent variable. 'Additional Controls' include income, marital status, and race dummy variables. Heteroskedasticity-robust standard errors are in parentheses. Significance at the 1%, 5% and 10% level is indicated by ***, **, and **. Observations include up to all 331 student responses from the first survey wave, whether or not they completed the second wave survey, where missing variables reduces the sample size. Panel B presents percentiles of the distribution of the Peer Average coefficients corresponding to the OLS regressions, where the distribution is generated from 1,000 simulations of synthetically created peer groups.

TABLE IV
PEER EFFECTS IN ECONOMIC ATTITUDES: CORRELATIONAL EVIDENCE

	Risk Aversion		Honesty		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peer Average	0.520**	0.670**	0.165	0.601	−5.424***	−4.238***	−3.735***	−4.215***
	(0.250)	(0.286)	(0.367)	(0.467)	(1.013)	(1.038)	(0.765)	(0.963)
Female		0.340		0.038		0.396***		0.061
		(0.307)		(0.046)		(0.123)		(0.103)
Age		−0.026		−0.012		−0.041		0.004
		(0.069)		(0.010)		(0.027)		(0.023)
Children		−2.470***		0.188**		−0.084		−0.011
		(0.692)		(0.088)		(0.262)		(0.236)
Additional Controls	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.023	0.169	−0.005	0.048	0.142	0.216	0.096	0.078
Observations	132	124	157	150	153	145	134	128

Coefficients from OLS regressions are presented where the dependent variable is listed at the top of the column. 'Risk Aversion' is measured following Holt and Laury (2002). 'Honesty' is measured following a variation of Innes and Mitra (2009). 'Altruism' is the average response to four questions (each with a five-point Likert scale) from the empathic concern subset of the Davis Interpersonal Reactivity Index. 'Trust' is measured following the World Values Survey. 'Peer Average' indicates the leave-one-out average of the dependent variable. 'Additional Controls' include income, marital status, and race dummy variables. Heteroskedasticity-robust standard errors are in parentheses. Significance at the 1%, 5% and 10% level is indicated by ***, **, and **. Observations include up to 196 student responses from the second survey wave, where missing variables reduce the sample size.

TABLE V
PEER EFFECTS IN ECONOMIC ATTITUDES: CAUSAL EVIDENCE

	Risk Aversion		Honesty		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predetermined Peer Average	1.155**		−1.286**		−1.625**		−0.960	
	(0.454)		(0.613)		(0.656)		(0.684)	
I×Predetermined Peer Average		0.174		0.452		−0.300		−0.063
		(0.463)		(0.896)		(0.522)		(0.561)
Female	0.340		0.021		0.494***		0.006	
	(0.313)		(0.042)		(0.118)		(0.111)	
Age	−0.048		−0.014		−0.023		−0.002	
	(0.068)		(0.010)		(0.035)		(0.024)	
Children	−2.344***		0.203**		−0.216		0.011	
	(0.678)		(0.091)		(0.287)		(0.216)	
Additional Controls	Yes	No	Yes	No	Yes	No	Yes	No
Individual Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.181	0.293	0.059	0.155	0.158	0.592	−0.031	0.532
Observations	124	264	150	314	145	306	128	268

Coefficients from OLS regressions of responses to the second survey wave are presented where the dependent variable is listed at the top of the column. 'Risk Aversion' is measured following Holt and Laury (2002). 'Honesty' is measured following a variation of Innes and Mitra (2009). 'Altruism' is the average response to four questions (each with a five-point Likert scale) from the empathic concern subset of the Davis Interpersonal Reactivity Index. 'Trust' is measured following the World Values Survey. 'Predetermined Peer Average' indicates the leave-one-out average of the dependent variable from the first survey, conducted before social interactions occurred. 'I' indicates a dummy for the second survey wave. 'Additional Controls' include income, marital status, and race dummy variables. Heteroskedasticity-robust standard errors are in parentheses for non-fixed effects specifications. Non-fixed effects specifications include student responses from the second survey wave, where missing variables reduce the sample size. Specifications that include individual fixed effects have standard errors clustered at the individual level and include observations from both survey waves. Significance at the 1%, 5% and 10% level is indicated by ***, **, and **.

TABLE VI
PLACEBO TESTS FROM SYNETHETIC RANDOMIZATIONS

	Risk Aversion		Honesty		Altruism		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predetermined Peer Average	−0.220	−0.304	−0.074	−0.069	−0.553	−0.393	−0.456	−0.445
	(0.609)	(0.684)	(0.446)	(0.488)	(0.705)	(0.719)	(0.699)	(0.814)
Female		0.381		0.029		0.475***		0.027
		(0.313)		(0.043)		(0.120)		(0.110)
Age		−0.043		−0.012		−0.031		−0.003
		(0.073)		(0.010)		(0.033)		(0.025)
Children		−2.397***		0.183**		−0.156		0.019
		(0.751)		(0.088)		(0.297)		(0.228)
Additional Controls	No	Yes	No	Yes	No	Yes	No	Yes

This table presents average coefficients and standard errors from 1,000 Monte Carlo simulations of causal OLS regressions. Individuals are randomly assigned to one of six synthetic peer groups in each simulation. The dependent variable is listed at the top of the column. 'Risk Aversion' is measured following Holt and Laury (2002). 'Honesty' is measured following a variation of Innes and Mitra (2009). 'Altruism' is the average response to four questions (each with a five-point Likert scale) from the empathic concern subset of the Davis Interpersonal Reactivity Index. 'Trust' is measured following the World Values Survey. 'Predetermined Peer Average' indicates the leave-one-out average of the dependent variable from the first survey, conducted before social interactions occurred. 'Additional Controls' include income, marital status, and race dummy variables. Averages over simulations of heteroskedasticity-robust standard errors are in parentheses. Significance at the 1%, 5% and 10% level is indicated by ***, **, and **. Observations include up to 196 student responses from the second survey wave, where missing variables reduce the sample size.