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First Draft: 19 August 2011
This Version: 19 December 2012

JEL Classification: D81, D83, D01

Keywords: Peer effects, risk aversion, altruism, trust

* We thank Francesca Gino, Luigi Guiso, Cam Harvey, Jay Ritter, Kelly Shue, and seminar participants at the 2011 Miami Behavioral Finance Conference, Georgia State University, Michigan State University, Rice University, University of Amsterdam, University of Michigan, University of Arizona, Drexel University, and University of Southern California for helpful comments. Kenneth Ahern is at the Marshall School of Business, University of Southern California (kenneth.ahern@marshall.usc.edu). Ran Duchin is at the Foster School of Business, University of Washington (duchin@uw.edu). Tyler Shumway is at the Ross School of Business, University of Michigan (shumway@umich.edu). We thank Daniel Weagley for excellent research assistance. The project was funded in part by grants from the Ross School of Business and by the National Institution on Aging grant P01-AG026571.
Peer Effects in Economic Attitudes

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 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, 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 policymakers 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 Ikaheimo, 2008), and obesity (Christakis and Fowler, 2007), little evidence exists on peer effects in economic attitudes.

We study three key economic attitudes: risk aversion, 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 trust. Finding evidence of positive or negative peer effects in any of
the three 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 \textit{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 \textit{ex post} own risk aversion by 0.2 standard deviations. In contrast, we find negative peer effects in altruism. 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 altruism causes own altruism to decrease by 0.1 to 0.2 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.

Robustness tests confirm the majority of our results. First, to mitigate concerns about section-specific shocks, we control for instructor fixed effects and find similar results. This means that the results are not driven by differences in the classroom experience of some sections, relative to others. A second concern is that we could be identifying peer effects across all students in all sections, driven 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. Across 1,000 simulated section assignments, we find no significant peer effects. Third, we use a completely different approach to identify peer effects based on the ratio of within-section to between-section variance, following Glaeser, Sacerdote, and Scheinkman (1996). If individual attitudes are independent, the two variances should be equal. We find that the within-section variance of risk aversion relative to between-section variance is smaller after a year of social interaction, consistent with positive peer effects. We find no evidence of changes in variance for altruism, providing contrasting evidence to our results above.

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 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.

A possible concern with our results 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. 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 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:

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

\[ u_{i,s} = \alpha_s + \varepsilon_{i,s} \]  

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 \( \alpha_s \) 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 \( \alpha_s \) 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, 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. Therefore, we control for these and other possible confounding variables in 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 $\alpha_s$ 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:

\[ A_{i,s} = \beta A_{-i,s} + \delta X_{i,s} + \varepsilon_{i,s}. \] (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 cannot 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. We also test for changes in the variance of responses within sections compared to across
sections. This test does not rely on random differences in section average attitudes.

I.B. The Reflection Problem

The model in Equation 2 does not address the reflection problem, and can only be regarded as
correlational evidence. Common omitted factors can lead individuals’ attitudes to change, which
are aggregated to the section-level. In this case, we couldn’t identify a causal link between peer
average attitudes and individual attitudes. To provide causal evidence, we follow Sacerdote (2001)
and Guryan, Kroft, and Notowidigdo (2009), and use predetermined variables to overcome the si-
multaneity inherent in the reflection problem. In particular, we instrument for current peer average
attitudes using attitudes elicited from surveys conducted prior to peer group social interactions:

\[ A_{i,s} = \beta A_{pre}^{P-i,s} + \delta X_{i,s} + \varepsilon_{i,s}. \] (3)
where $\bar{A}_{i,s}^{pre}$ indicates the average predetermined attitude of individual $i$’s future peers. This breaks the reflection problem and uses the exogenous predetermined variation in peer attitudes to explain individual attitudes after the first year of the MBA program.

Though the use of predetermined variables allows us to separate endogenous and exogenous peer effects, we cannot 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):

$$A_{i,s,t} = \tau_t + \beta I \times \bar{A}_{i,s,t}^{pre} + \kappa_t + \xi_{i,s,t},$$

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 $\beta$ 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. 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 three dimensions of economic attitudes studied in this paper: risk aversion, 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

\footnote{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}. 
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 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 Rutström, 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.

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. 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. 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.3. 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 three 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, 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.

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 non-Indian 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 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 see this bias, consider the extreme case of two individuals in a section, one with a higher score than the other. The leave-one-out peer average for the low-score individual is the higher score of the other individual. The average for the high-score individual is the lower score of the other. Thus, in the cross-section, there is a negative relation.

To address this bias, Guryan et al. suggest comparing coefficient estimates to empirical distributions of coefficients created from simulations with synthetically-created random assignments.\textsuperscript{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 of predetermined individual level controls including gender, age, a dummy for parenthood, income, marital status, and race. For each attitude, we find insignificant coefficients for the effect of the peer average. 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. These results confirm that students are randomly assigned across sections with respect to economic attitudes. The negative bias in these tests also means that interpretation

\textsuperscript{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 estimates of the correlational relationship in Equation 2 is problematic. Therefore, we only present evidence of the effect of predetermined peer averages on ex post outcomes, which do not suffer from this bias.

III. CAUSAL RESULTS ON PEER EFFECTS

Having established random assignment, we now estimate Equation 3 in Table IV, where we relate predetermined peer attitudes to \textit{ex post} individual attitudes. For each attitude we report results from a parsimonious model and a model with additional control variables including gender, age, a dummy for having children, income, marital status, and race. Because the variance of the predetermined peer averages are different than the variance of the individual responses, we normalize both the individual and peer average variables by their standard deviations. This means that the coefficient estimates provide the marginal effect of a one standard deviation change in the peer average on the standard deviation change in the individual attitudes.

First, the coefficient estimates in Table IV, show that risk aversion displays positive peer effects. A one-standard deviation increase in predetermined peer average risk aversion increases \textit{ex post} own risk aversion by 0.2 standard deviations in the parsimonious model. This result is statistically significant at the 5\% level. Including the additional individual-level control variables leaves the main effect relatively unchanged at 0.23. We find no effects for gender or age, but find that participants with children have are statistically less risk averse than those 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).

Positive peer effects are consistent with a desire for conformity in preferences for risk. These results are consistent with similar evidence of positive peer effects for risk-taking in health choices, cited above. It is worth reiterating that random assignment rules out the hypothesis that this result is caused by students selecting similar peers. It also worth reiterating that random assignment means that OLS estimates are unbiased.

In contrast, we find negative peer effects in altruism. A one-standard deviation increase in predetermined peer average altruism decreases \textit{ex post} own altruism by 0.17 standard deviations
in the parsimonious model and 0.20 standard deviations in the model with controls. Negative peer effects are consistent with models of public goods. 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, the true underlying preferences might 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 more selfishly, according to the preferences of rational economic man. We also find that female participants report statistically higher levels of altruism than men, consistent with prior research.

Finally, 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 and altruism are more flexible.

III.A. Section-Specific Shocks

One limitation of our study design is that we must assume that section-specific shocks are not driving our results. Though we have presented qualitative arguments why section-specific shocks are unlikely to occur, we can never rule out the possibility. To help mitigate this concern, we control for perhaps the largest differences across sections in the MBA program: the instructors. Each course is typically co-taught by two instructors, who each teach three out of the six sections. If instructors are influencing economic attitudes, we should observe systematic differences between instructors’ sections.

In untabulated tests, we control for instructor fixed effects. We include dummy variables for the identity of the instructors during the two Fall half-semester terms. Our main results are unchanged after controlling for instructor effects. Though we cannot control for section-specific shocks, since they would be highly correlated with our leave-one-out peer averages, these tests provide some assurance that one of the main differences across sections, the instructors, is not driving our results.
III.B. Placebo Tests

A second concern with our results is that we could be picking up an omitted factor that is affecting all students' economic attitudes. This would be evidence of a MBA effect, rather than a peer effect. Therefore, we conduct placebo tests to verify that our results are not driven by such an omitted variable. We run a 1,000 simulated causal regressions identical to those in Table IV, but 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 V. 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.

III.C. Exogenous and Endogenous Peer Effects

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. For example, students are unlikely to directly observe their peers’ choices in a lottery environment, but they are likely to infer risk preferences from alternative behavior, such as real or classroom-based investment decisions, job and internship searches, and discussions about business decisions. Therefore, in untabulated tests, 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 untabulated 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.

**III.D. Analysis of Variance Tests**

Finally, we test for peer effects using a completely different approach. Following Glaeser, Sacerdote, and Scheinkman (1996), we compare variation in survey responses within each section to variation across sections. Positive peer effects implies that variation within a section is smaller than what would be observed in independent draws, as peers’ attitudes become more homogeneous. Table VI presents simple tests of this idea. For both the pre- and post-interaction surveys, and for each of the three economic attitudes, we compute the $F$-statistic of the ratio of between-section variance to within-section variance. Because we are interested in group-level variation, we use the entire sample of responses, without restricting attention to only those students who responded to both survey waves. A high $F$-statistic indicates that the variation among peers within a section is less than the variation between section averages across non-peers.

The results in Table VI show that in the first survey wave there are no significant differences between the within-section variance and the between-section variance for any of the three economic attitudes. This is consistent with the assumption that since this survey was taken before the students’ first year of the MBA program, each survey response is independent. In the second survey wave, after seven months of social interactions, we find strong evidence of positive peer effects in risk aversion, but no peer effects in altruism or trust. The $F$-statistic for risk aversion is highly statistically significant at 4.02, indicating that the variance of risk aversion within sections decreased significantly. This is consistent with the regression results presented above. In contrast, though we found negative peer effects for altruism in the regression tests, in the variance tests, we find no evidence of peer effects. Trust continues to display no peer effect.
III.E. Implications of Results

Our 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. It also has implications for corporate investment. If corporate culture influences attitudes towards risk, employees may alter their views about risky investments depending upon their employer.

These results also shed light on the underlying forces behind altruism. 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, 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. However, our findings are weaker for altruism, as we do not find evidence of peer effects in the excess variance tests.

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. 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 altruism, are negatively related to individual pro-social attitudes. This implies that in the absence of social sanctions, as in our
data, individuals could 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, 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 $38,500; 9/10 of $1,000
Option A: 2/10 of $20,000; 8/10 of $16,000 or Option B: 2/10 of $38,500; 8/10 of $1,000
Option A: 3/10 of $20,000; 7/10 of $16,000 or Option B: 3/10 of $38,500; 7/10 of $1,000
Option A: 4/10 of $20,000; 6/10 of $16,000 or Option B: 4/10 of $38,500; 6/10 of $1,000
Option A: 5/10 of $20,000; 5/10 of $16,000 or Option B: 5/10 of $38,500; 5/10 of $1,000
Option A: 6/10 of $20,000; 4/10 of $16,000 or Option B: 6/10 of $38,500; 4/10 of $1,000
Option A: 7/10 of $20,000; 3/10 of $16,000 or Option B: 7/10 of $38,500; 3/10 of $1,000
Option A: 8/10 of $20,000; 2/10 of $16,000 or Option B: 8/10 of $38,500; 2/10 of $1,000
Option A: 9/10 of $20,000; 1/10 of $16,000 or Option B: 9/10 of $38,500; 1/10 of $1,000
Option A: 10/10 of $20,000; 0/10 of $16,000 or Option B: 10/10 of $38,500; 0/10 of $1,000

I.B. 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.C. 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


Table I
SUMMARY STATISTICS OF ECONOMIC ATTITUDES

Panel A. Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>Percentile</th>
<th>Mean</th>
<th>Std Dev</th>
<th>Min</th>
<th>25th</th>
<th>50th</th>
<th>75th</th>
<th>Max</th>
<th>Obs.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Ex Post Individual Attitudes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Risk Aversion</td>
<td></td>
<td>6.506</td>
<td>1.726</td>
<td>1</td>
<td>5</td>
<td>7</td>
<td>8</td>
<td>10</td>
<td>158</td>
</tr>
<tr>
<td>Altruism</td>
<td></td>
<td>3.250</td>
<td>0.707</td>
<td>1.5</td>
<td>2.75</td>
<td>3.25</td>
<td>3.75</td>
<td>4.5</td>
<td>172</td>
</tr>
<tr>
<td>Trust</td>
<td></td>
<td>0.510</td>
<td>0.502</td>
<td>0</td>
<td>0</td>
<td>1</td>
<td>1</td>
<td>1</td>
<td>157</td>
</tr>
<tr>
<td><strong>Pre-Determined Average Peer Attitudes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Risk Aversion</td>
<td></td>
<td>6.382</td>
<td>0.318</td>
<td>5.917</td>
<td>6.052</td>
<td>6.361</td>
<td>6.733</td>
<td>6.918</td>
<td>196</td>
</tr>
<tr>
<td>Altruism</td>
<td></td>
<td>3.318</td>
<td>0.090</td>
<td>3.203</td>
<td>3.250</td>
<td>3.291</td>
<td>3.345</td>
<td>3.534</td>
<td>196</td>
</tr>
<tr>
<td>Trust</td>
<td></td>
<td>0.532</td>
<td>0.068</td>
<td>0.414</td>
<td>0.488</td>
<td>0.538</td>
<td>0.588</td>
<td>0.634</td>
<td>196</td>
</tr>
</tbody>
</table>

Panel B. Mean Variation Across Sections

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>Hi–Lo</th>
<th>CV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Risk Aversion</td>
<td>6.833 (24)</td>
<td>6.818 (33)</td>
<td>5.792 (24)</td>
<td>7.071 (28)</td>
<td>6.381 (28)</td>
<td>6.000 (28)</td>
<td>1.279</td>
<td>0.079</td>
</tr>
<tr>
<td>Altruism</td>
<td>3.194 (27)</td>
<td>3.270 (37)</td>
<td>3.385 (24)</td>
<td>3.217 (30)</td>
<td>3.087 (23)</td>
<td>3.323 (31)</td>
<td>0.298</td>
<td>0.032</td>
</tr>
<tr>
<td>Trust</td>
<td>0.478 (23)</td>
<td>0.500 (36)</td>
<td>0.565 (23)</td>
<td>0.481 (27)</td>
<td>0.500 (20)</td>
<td>0.536 (28)</td>
<td>0.087</td>
<td>0.067</td>
</tr>
</tbody>
</table>

Data are from survey responses of 196 first-year MBA students. ‘Risk Aversion’ is measured following Holt and Laury (2002). ‘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

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

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). ‘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

<table>
<thead>
<tr>
<th></th>
<th>Risk Aversion</th>
<th>Altruism</th>
<th>Trust</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Peer Average</td>
<td>0.226</td>
<td>0.332</td>
<td>-0.627</td>
</tr>
<tr>
<td>(0.366)</td>
<td>(0.400)</td>
<td>(0.504)</td>
<td>(0.528)</td>
</tr>
<tr>
<td>Female</td>
<td>0.240</td>
<td></td>
<td>0.239**</td>
</tr>
<tr>
<td>(0.273)</td>
<td></td>
<td></td>
<td>(0.095)</td>
</tr>
<tr>
<td>Age</td>
<td>-0.052</td>
<td></td>
<td>0.029</td>
</tr>
<tr>
<td>(0.060)</td>
<td></td>
<td></td>
<td>(0.023)</td>
</tr>
<tr>
<td>Children</td>
<td>-0.350</td>
<td></td>
<td>-0.349</td>
</tr>
<tr>
<td>(0.636)</td>
<td></td>
<td></td>
<td>(0.230)</td>
</tr>
<tr>
<td>Additional Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>-0.002</td>
<td>0.026</td>
<td>0.002</td>
</tr>
<tr>
<td>Observations</td>
<td>244</td>
<td>232</td>
<td>276</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Risk Aversion</th>
<th>Altruism</th>
<th>Trust</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>25%</td>
<td>-1.234</td>
<td>-1.094</td>
<td>-1.250</td>
</tr>
<tr>
<td>50%</td>
<td>-0.354</td>
<td>-0.331</td>
<td>-0.361</td>
</tr>
<tr>
<td>75%</td>
<td>0.076</td>
<td>0.106</td>
<td>0.116</td>
</tr>
<tr>
<td>95%</td>
<td>0.409</td>
<td>0.488</td>
<td>0.447</td>
</tr>
<tr>
<td>99%</td>
<td>0.602</td>
<td>0.659</td>
<td>0.559</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

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). ‘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: Causal Evidence

<table>
<thead>
<tr>
<th></th>
<th>Risk Aversion</th>
<th>Altruism</th>
<th>Trust</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Predetermined Peer Average</td>
<td>0.203**</td>
<td>0.233**</td>
<td>−0.166**</td>
</tr>
<tr>
<td></td>
<td>(0.084)</td>
<td>(0.092)</td>
<td>(0.078)</td>
</tr>
<tr>
<td>Female</td>
<td>0.215</td>
<td></td>
<td>0.676***</td>
</tr>
<tr>
<td></td>
<td>(0.198)</td>
<td></td>
<td>(0.161)</td>
</tr>
<tr>
<td>Age</td>
<td>−0.030</td>
<td></td>
<td>−0.032</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td></td>
<td>(0.047)</td>
</tr>
<tr>
<td>Children</td>
<td>−1.483***</td>
<td></td>
<td>−0.296</td>
</tr>
<tr>
<td></td>
<td>(0.429)</td>
<td></td>
<td>(0.393)</td>
</tr>
<tr>
<td>Additional Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.036</td>
<td>0.181</td>
<td>0.019</td>
</tr>
<tr>
<td>Observations</td>
<td>132</td>
<td>124</td>
<td>153</td>
</tr>
</tbody>
</table>

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). ‘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. The dependent variable and ‘Predetermined Peer Average’ are normalized by subtracting the mean and dividing by the standard deviation. Thus the coefficients provide estimates of the change in individual attitudes for a one-standard deviation-change in predetermined peer averages. ‘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 **.
### Table V

**Placebo Tests From Synthetic Randomizations**

<table>
<thead>
<tr>
<th></th>
<th>Risk Aversion</th>
<th>Altruism</th>
<th>Trust</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Predetermined Peer Average</td>
<td>−0.220</td>
<td>−0.304</td>
<td>−0.553</td>
</tr>
<tr>
<td></td>
<td>(0.609)</td>
<td>(0.684)</td>
<td>(0.705)</td>
</tr>
<tr>
<td>Female</td>
<td>0.381</td>
<td></td>
<td>0.475***</td>
</tr>
<tr>
<td></td>
<td>(0.313)</td>
<td></td>
<td>(0.120)</td>
</tr>
<tr>
<td>Age</td>
<td>−0.043</td>
<td>−0.031</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.073)</td>
<td>(0.033)</td>
<td></td>
</tr>
<tr>
<td>Children</td>
<td>−2.397***</td>
<td>−0.156</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.751)</td>
<td>(0.297)</td>
<td></td>
</tr>
<tr>
<td>Additional Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

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). ‘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.
Table VI
ANALYSIS OF VARIANCE OF ECONOMIC ATTITUDES: PRE AND POST-INTERACTION BY SECTION

<table>
<thead>
<tr>
<th></th>
<th>Pre-Interaction</th>
<th></th>
<th></th>
<th>Post-Interaction</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Sum of Squares</td>
<td>Degrees of Freedom</td>
<td><em>F</em></td>
<td>Sum of Squares</td>
<td>Degrees of Freedom</td>
</tr>
<tr>
<td><strong>Risk Aversion</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Between sections</td>
<td>24.572</td>
<td>5</td>
<td>1.570</td>
<td>46.113</td>
<td>5</td>
</tr>
<tr>
<td>Within sections</td>
<td>746.068</td>
<td>238</td>
<td>(0.170)</td>
<td>378.565</td>
<td>165</td>
</tr>
<tr>
<td><strong>Altruism</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Between sections</td>
<td>1.873</td>
<td>5</td>
<td>0.730</td>
<td>0.485</td>
<td>5</td>
</tr>
<tr>
<td>Within sections</td>
<td>137.955</td>
<td>270</td>
<td>(0.599)</td>
<td>107.870</td>
<td>198</td>
</tr>
<tr>
<td><strong>Trust</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Between sections</td>
<td>1.174</td>
<td>5</td>
<td>0.940</td>
<td>0.292</td>
<td>5</td>
</tr>
<tr>
<td>Within sections</td>
<td>62.411</td>
<td>249</td>
<td>(0.458)</td>
<td>48.087</td>
<td>189</td>
</tr>
</tbody>
</table>

‘Risk Aversion’ is measured following Holt and Laury (2002). ‘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. Sections are six randomly assigned section assignments for the MBA student participants. Pre-interaction variables are from surveys taken before the first year of the MBA program. Post-interaction statistics are from surveys completed after the first academic year of the MBA program. Numbers in parentheses below *F* statistics are *p*-values.