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Peer Effects in Risk Aversion and Trust

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Existing evidence shows that risk aversion and trust are largely determined by environmental factors. We test whether one such factor is peer influence. Using random assignment of MBA students to peer groups and predetermined survey responses of economic attitudes, we find causal evidence of positive peer effects in risk aversion and no effects in trust. After the first year of the MBA program, the difference between an individual and her peers' average risk aversion has shrunk by 41%. Finding no peer effects in trust is consistent with recent research showing that distinct cognitive processes govern risk aversion and trust. (*JEL* D81, D83, D01)

Risk aversion and trust are central to much of economic thought, including contract theory, financial economics, and theories of investment.¹ However, our understanding of how these attitudes form and evolve is limited. Recent research finds that genetics explain about 20% of the cross-sectional variation in risk aversion and trust (Cesarini et al. 2008, 2009). This leaves 80% of the variation to be explained by unknown environmental forces. And, unlike genetics, a person's environment is not constant. As environments fluctuate throughout life, attitudes toward risk and trust could shift too. Given that these

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¹ For example, risk aversion is discussed in such seminal works as those by Arrow (1965), Holmstrom (1979), Kihlstrom and Laffont (1979), Mehra and Prescott (1985), and Epstein and Zin (1989). Trust is discussed in theoretical work (Arrow 1972; Williamson 1993), experimental papers (Glaeser et al. 2000; Fershtman and Gneezy 2001) and nonlaboratory empirical research (Knack and Keefer 1997; Guiso, Sapienza, and Zingales 2004; Ahern, Daminelli, and Fracassi 2012).

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attitudes are fundamental elements in economic decision making, a deeper understanding of the environmental influences on risk aversion and trust is essential for advancing economic thought and policy.

In this paper, we study one potential environmental determinant: peer influence. Peers have been shown to affect a wide variety of financial outcomes, including stock market activity (Hong, Kubik, and Stein 2004, 2005; Ivković and Weisbenner 2007; Brown et al. 2008), retirement planning (Duflo and Saez 2002, 2003), CEO compensation and investment (Shue 2013), and entrepreneurship (Lerner and Malmendier 2013). Although there is growing evidence on peer effects in outcomes, there is relatively little direct evidence on peer effects in underlying attitudes. Because economic attitudes underlie observed outcomes, it is plausible that peers influence the attitudes themselves. Alternatively, peers may influence observed actions without influencing the underlying attitudes. Instead, other environmental factors, such as parental guidance or formal education, could explain the nongenetic determinants of attitudes (Dohmen et al. 2012). In either case, evidence for or against peer influence will provide new insight into the forces that shape fundamental attitudes.

One of the primary obstacles that has limited research in peer effects is the challenge of identifying a causal relationship between individuals and peers (Manski 1993). First, correlated group effects, such as endogenously formed peer groups, cause individuals and peers to act similarly for reasons unrelated to social interactions. Second, it is difficult to separate the simultaneous influence of peers on an individual from the influence of the individual on her peers. Finally, the credibility of results depends upon the evidence that peer groups defined by the researcher actually have influential social interactions.

To address each of these empirical obstacles, we design a longitudinal survey of MBA students at the University of Michigan. First, following Lerner and Malmendier (2013) and Shue (2013), we overcome self-selection of peers by exploiting the random assignment of students to one of six sections to create exogenously formed peer groups. Second, we overcome section-specific shocks and simultaneity by eliciting attitudes from the students prior to the start of the MBA program, before social interactions occur. We use these responses to estimate a causal relationship between the predetermined attitudes of a person's peers and the person's own attitudes after a year in the MBA program. Finally, using course enrollment data and self-reported friendship and professional contacts, we empirically verify that section peers have meaningful social interactions. To elicit attitudes towards risk aversion and trust, we use well-established procedures from Holt and Laury (2002) and the World Values Survey.

In linear-in-means regressions, we find strong evidence of positive peer effects in risk aversion. When an individual's randomly assigned peers are relatively more risk averse prior to starting the MBA program, the individual is relatively more risk averse after the first year. A one-standard-deviation increase

in average peer risk aversion causes an increase in one's own risk aversion of 0.2 standard deviations. This result is both statistically significant and economically large.

In contrast, we find no evidence of peer effects in trust. This suggests that trust is more rigid than risk aversion. This result also provides new insight into the relationship between risk and trust. Trust is often considered to be one type of risk taking (Bohnet and Zeckhauser 2004; Kosfeld et al. 2005). However, recent research shows that trust cannot be explained by risk preferences alone. Fehr (2009) reviews both neurobiological and behavioral evidence and concludes that "social risk taking (i.e., trusting) involves neural mechanisms that go beyond what is needed for taking asocial [financial] risks" (244). Our findings provide new evidence that supports this view. However, measurement issues may prevent us from finding peer effects in trust. We discuss this concern in detail below.

We next test for peer effects using the variance decomposition approach of Glaeser, Sacerdote, and Scheinkman (1996). In contrast to linear-in-means regressions, this approach compares the variance of the distribution of students' attitudes between sections to the variance of the distribution within sections. Positive peer effects predict smaller within-section variation, compared with between-section variation, as peer groups conform to the average. In these tests, we find strong evidence of positive peer effects in risk aversion, but no effects in trust, consistent with the regression results.

Though predetermined attitudes and random assignment allow for a causal interpretation of our results, we acknowledge that our design cannot fully address a secondary identification issue. Manski (1993) shows that only under restrictive conditions can one identify whether an individual's choice is driven by a response to her peers' choices (endogenous effects) or to the exogenous characteristics of her peers, such as socioeconomic status or gender (contextual effects). To do so requires the highly unusual occurrence of an exogenous shock to the attitudes of a person's peers, while leaving the person unaffected.

While both endogenous and contextual effects are true causal peer effects, we attempt to provide some insight into the channels through which peer effects operate. In particular, we exploit the panel nature of our study to investigate within-individual changes in attitudes. This approach controls for time-invariant contextual effects from predetermined characteristics of the individual and the peers, such as gender, race, or genetic predisposition. Consistent with our main results, we find that students with greater predetermined risk aversion than their peers exhibit a significant decrease in risk aversion after the first year of the MBA program. Trust remains unaffected by peer influence. Although we cannot claim to separate endogenous and contextual effects, and we do not claim that peer effects only operate through one channel, the peer effects that we identify likely reflect both endogenous and time-varying contextual peer effects.

Our results are robust to a number of alternative hypotheses. First, although random assignment and predetermined attitudes control for most plausible section-specific shocks, it is still possible that an instructor may respond differently to students based on their predetermined attitudes or that instructors are nonrandomly assigned to sections based on their predetermined attitudes. When we control for instructor-fixed effects, we find no substantial changes in our main results. Second, we address the possibility that financial sophistication affects risk preferences by controlling for a student's field of interest and grade point average in finance classes. Third, we verify that our results are robust to a different measure of risk aversion. Finally, because we use a panel study, we also test for attrition bias. We find that there are no significant differences in the demographics of participants across survey waves and that risk aversion and trust do not significantly predict attrition.

A final concern with our research design is the problem of multiple comparisons. In addition to risk aversion and trust, our surveys also elicited attitudes on honesty and altruism, which we do not include in this study. Of the four attitudes, only risk aversion has statistically significant peer effects. Because of the multiple comparisons problem, it is possible that risk aversion is significant by chance alone. However, we believe that the omitted attitudes are insignificant because the measures we used are noisy and subject to participant bias. In Section 1.3 of the paper, we discuss this issue in more detail and provide a full set of results for all variables in the Online Appendix.

We believe that our findings have far-reaching implications. Endogenous peer effects create a social multiplier (Glaeser, Sacerdote, and Scheinkman 2003). A multiplier in financial risk aversion may help to explain how social interactions transmit personal attitudes across society, leading to wide-scale phenomena like home-bias preferences, excessive comovement in stock prices, and varying rates of stock market participation among different social groups. Contextual effects do not create a multiplier but suggest that risk attitudes are influenced by the characteristics of peers. This form of peer effect could occur when a person encounters peers of different backgrounds, such as a student at a university or an employee at a new job.

This paper makes two primary contributions. First, though many papers have investigated peer effects in risky health choices (e.g., Card and Giuliano 2011), to our knowledge, we are the first to study peer effects for financial risk preferences. Trust has received a great deal of attention in economics (e.g., Guiso, Sapienza, and Zingales 2008, 2009), though, to our knowledge, ours is the first study of peer effects in trust. Our findings also provide new evidence that trust is not simply a special case of risk aversion. Subject to measurement error, trust is more immune to the influence of peers and is more stable than risk aversion. This is consistent with the idea that trust is a cultural belief that is passed from generation to generation relatively unchanged through enduring environmental forces, such as family relations (Guiso, Sapienza, and Zingales 2006).

Second, by using subjective survey responses, our results more likely reflect changes in preferences, rather than changes induced by learning from peers. This is consistent with the idea that people conform to social norms of risk aversion to receive utility benefits (Akerlof 1980; Bernheim 1994), rather than because they have updated their expectations about outcomes associated with risk aversion through learning. This provides a microfoundation to the long-run peer effects in outcomes of MBA students documented by Lerner and Malmendier (2013) and Shue (2013) and the role of social networks in business practices and managerial decisions (Cohen, Frazzini, and Malloy 2008).

1. Data Description and Setting

Our data come from a panel survey of students 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. We conducted two online survey waves over eight months. In the first survey wave, denoted wave 0, we collected responses from mid-August until early September 2010. This period represents the preinteraction period and begins before students start the MBA program and ends two weeks after the start of classes. We allowed survey responses after classes began to meet the students in-person to encourage participation.² Because only two weeks had passed, and most students responded by the third day of classes, we consider the responses to be exogenous to meaningful social interaction. The second survey wave was conducted in April 2011. We denote this as wave 1, indicating it is after the first academic year.

In each survey wave, participants were compensated by receiving a \$5 gift card and by being entered in a lottery to win one of three iPads, valued at about \$500. Because MBA students are wealthier and have greater time constraints than undergraduates, we used relatively large compensation to try to induce a higher response rate. We followed standard methods to control for survey response bias: the order of survey questions was counterbalanced in blocks across participants, and 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 at least eight 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 in survey wave 0 and 235 in wave 1. Thus, out of six sections of students, the average section has about 39 students that participated in both waves. Our participation and attrition rates are comparable to other surveys that use MBA students as participants (Kaniel, Massey, and Robinson 2010; Hussey 2011). Most of the attrition probably reflects the increased opportunity cost to MBA students who are preparing for exams and searching for employment.

² Our in-person meetings were not conducted by faculty members who taught any of the first-year classes.

Table 1
Sample demographics

	Wave 0	Wave 1	<i>t</i> -statistic
Female (%)	33.53	34.47	-0.230
Age	28.28	27.88	1.485
Children dummy	7.55	6.63	-0.855
Income bracket (%)			
\$5,000 and under	4.10	5.85	-0.358
\$5,001-15,000	4.10	4.79	0.555
\$15,001-30,000	8.83	7.45	1.027
\$30,001-45,000	9.46	6.91	0.520
\$45,001-60,000	18.30	16.49	-0.429
\$60,001-75,000	20.19	21.81	0.095
\$75,001-90,000	17.35	17.02	-0.894
\$90,001-105,000	9.15	11.70	0.505
\$105,001-120,000	2.84	2.13	-0.016
\$120,001-135,000	1.58	1.60	-0.180
\$135,001-150,000	1.89	2.13	0.060
Over \$150,000	2.21	2.13	-0.806
Marital status (%)			
Never married	71.12	74.36	0.903
Married	27.66	24.10	-0.610
Divorced	0.91	1.54	1.000
Separated	0.30	0.00	0.400
Race (%)			
White	55.29	56.17	-0.208
Black	2.72	3.83	-0.721
Indian	13.90	13.19	0.242
Non-Indian Asian	24.47	22.98	0.411
Hispanic	9.06	5.11	1.851*
Other	1.51	1.28	0.235
Observations	331	235	

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. Values are recorded at wave 0. Differences across waves indicates changes in sample, rather than changes in age, marital status, or children. *t*-statistics are from a difference in mean test. * indicates a significant difference at the 10% level.

Table 1 provides an overview of the demographics of our sample. Roughly one-third of our participants are women and the average age of respondents is about 28 years. About 72% have never been married, 25% are currently married, and a small percentage are divorced or separated. Roughly, 6%–7% of participants have children. Racial background varies, with 55% white, 24% non-Indian Asian, 13% Indian, 9% Hispanic, 3% black, and about 1%–2% other races. Students could select multiple races, or they could prefer not to answer, so percentages do not add to one. There is also variation in the income level of students in the year prior to starting the MBA program. Most commonly, 37% of students made between \$60,000 and \$90,000, though 17% made less than \$30,000, and more than 5% of students made over \$120,000.

Though we do not have a complete demographic profile of the entire student population, using publicly available data, we are able to verify that our sample is highly similar to the total population based on age, gender, and country of origin. In particular, 32% of the sample participants are female, compared

with 30% of the entire student population; 29% of the sample are international students, compared with 28% in the population; and the average student in both the sample and the population is 28 years old.

A possible concern with our study is that MBA students are a small subsample of the entire population. People who enter MBA programs are likely to be more open to new ideas and more receptive to peer influence than the average population. It is also important to note that our participants are students at a prestigious and expensive university. We think the benefits of studying MBA students outweigh these concerns. First, compared with young adults and teenagers, we expect that the population of MBAs is more mature and is less likely to be influenced by peers (Gardner and Steinberg 2005), which biases our results toward finding no peer effects. Second, MBA students are people who are more likely to become CEOs and investment fund managers, making important economic decisions that affect more than just their own economic well-being (Chevalier and Ellison 1999; Graham and Harvey 2001).

As in most panel survey studies, we must consider the possibility that attrition biases our results. Attrition will bias our estimates of peer effects if the change in the composition of the sampled population is related to demographics or attitudes. In Table 1, we test for a statistical difference in demographics between wave 0 and wave 1. We only find a small difference in the fraction of Hispanic participants, with no significant differences in any of the other traits. To provide a more rigorous test of attrition bias, we test for differences in participant characteristics using an analysis of variation test, described in more detail below. In particular, in Online Appendix Table 1, we test whether participants in wave 1 have more similar *ex ante* characteristics than participants in wave 0. We find that students who tend to be more similar in wave 0 are not unusually likely to participate in wave 1.

1.1 Measures of risk aversion and trust

We choose measures of risk aversion and trust that are widely-used and standardized. This means we can compare our results to prior literature and focus attention on the outcomes, rather than the measures.

First, to elicit risk aversion, we use a multiple price list (MPL) design, following the procedure of Holt and Laury (2002). In each survey, participants are asked to choose ten 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 with 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; in the fifth and subsequent lottery choices, the expected payoff is greater for option B. Therefore, a risk neutral person prefers option A in the first four lottery choices and option B in the last six lottery choices. The later a person switches to option B, the more risk averse she is. Our measure of risk aversion is the lottery choice number at

which the person switched from option A to option B. All of the outcomes are hypothetical, as no lottery awards are paid to the participants. The Online Appendix provides more details.

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 question. Therefore, we omit any observations for which the participant chooses option A in the tenth lottery. In addition, to ensure consistency, we drop observations for which the subject switches to option B and then switches back to option A in a subsequent choice.

This measure of risk aversion is appealing for a number of reasons. First, though the complexity of the MPL design 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. Because our subjects are MBA students, the level of mathematical complexity required is unlikely to be a problem.³ 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 using an MPL is that Holt and Laury (2002) show that risk aversion increases when faced with large-scale actual payouts compared with 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 and using actual payouts has a stable correlation over time, we can identify the difference from predetermined risk aversion to ex post risk aversion, across waves.

Second, to measure trust, we use a standard question from the World Values Survey, which has been used in economics (e.g., Guiso, Sapienza, and Zingales 2006, 2008, 2009), as well as other fields, such as political science (Brehm and Rahn 1997) and sociology (Buchan, Croson, and Dawes 2002). In particular, we ask participants to respond to the following question, “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.”

Although the wide usage of this measure of trust makes our results applicable to a large body of prior research, it has limitations. One potential concern with the trust variable is that it is more coarse than the measure of risk aversion. This means that to observe a change in the variable, the underlying latent variable for trust could require a larger shift than risk aversion. We address this concern in the robustness section by creating a binary risk aversion measure for comparability.

³ Because there is some complexity to the MPL design, we cannot rule out the possibility that changes in risk aversion are driven by peer effects in mathematical skill. If social interaction leads a student to improve her skill at calculating expected values, for instance, we may attribute this to a peer effect in risk aversion, when it is actually a peer effect in math skills.

A more fundamental limitation of our trust measure is that the survey question captures generalized trust, rather than context-dependent trust. For instance, people may have differing levels of trust in media (Kioussis 2001), government (Miller 1974), police (Stoutland 2001), and their bosses (Mayer and Gavin 2005). Generalized trust is likely to be a noisy measure of context-dependent trust. Thus, a finding of no peer effect could reflect that our measure of trust is too noisy to identify a peer effect, rather than that social interaction does not influence trust.

1.2 Surveys and preference versus expectation interactions

By using surveys, we follow the advice of Manski (2000, 131), who suggests that surveys provide a more direct measure of attitudes than outcomes. However, survey responses could be a poor proxy for actual behavior if respondents misreport 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 surveys. 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 is stable over time, our measures are valid.

In addition, using subjective attitudes from surveys, rather than observations of outcomes, may help separate the mechanisms by which peer effects operate. The transmission of attitudes between peers must come from observations of behaviors. For example, students are unlikely to observe their peers' choices in a lottery environment directly, but they are likely to infer risk preferences from alternative behavior, such as real or classroom-based investment decisions, job searches, and discussions about business decisions. Empirically disentangling these two forces is difficult (Bursztyn et al. 2012), and we do not claim that our research design allows this. However, we argue that because our survey questions are subjective and relatively private (which we empirically test below), they more likely reflect preference interactions, where a person changes her actions because she gets utility from behaving similarly to her peers, rather than expectation interactions, where a person learns from her peers' outcomes.

1.3 Multiple comparison and spillover effects from omitted variables

As mentioned previously, we elicited two other economic attitudes that we do not include in this study: honesty and altruism. After conducting the surveys, we determined that the survey questions we used do not provide credible measures of these attitudes. In both cases, we are concerned with participant bias, where a student's responses may be biased toward trying to please the researchers. In particular, because honesty and altruism are virtues, students may have

answered strategically to try to impress their professors. For risk aversion and trust, we do not believe the responses will be biased in this way.

One potential concern with the inclusion of these two extra variables is the multiple comparisons problem. It is possible that by random chance alone, one of the four variables is significant. Multiple comparison problems worsen as the number of possible variables increase. Because we only investigate four variables, and omit just two of them, we do not believe our results are driven by random chance. Additionally, as discussed, we have reason to believe the two variables that we omit are insignificant because they are noisy and unreliable measures, not because of multiple comparisons. Nevertheless, for transparency, the full results for all variables are presented in the Online Appendix.

A second concern with omitting these variables is the possibility of spillover effects from one variable to another. To pre-empt this concern, we randomly ordered the risk aversion, trust, altruism, and honesty questions in the survey in counterbalanced blocks across six different versions of the survey. We also counterbalanced questions written with a positive framing with the same question written with a negative framing, when appropriate. These procedures help to alleviate any systematic bias or spillovers that could arise from responding to one type of question before another. In tests reported in the Online Appendix, we also directly tested for spillover effects. After including the measures of honesty and altruism in our tests, all of our main results are unaffected. Based on our procedures and the empirical results, we do not believe that spillover effects influence our main results.

2. Research Design and Identification

There are at least three important identification challenges in peer effects: (1) correlated group effects, (2) the reflection problem, and (3) the definition of peer groups. Next, we describe how we address each of these three challenges.

2.1 Correlated group effects

Manski (1993) shows that peers may appear to be similar for three reasons: (1) correlated group effects, (2) endogenous effects, and (3) contextual effects. Correlated group effects can arise if people endogenously select peers who are similar to themselves or if people in a group all face the same environment or receive common external shocks. Group effects lead to similarity in the outcomes of individuals and peers but do not reflect social interactions. In contrast, endogenous and contextual effects both represent true causal peer effects. Endogenous peer effects occur when a person's attitudes or actions are influenced by peer attitudes or actions. Contextual peer effects occur when a person's attitudes or actions are influenced by exogenous characteristics of peers.

To identify a true causal peer effect, we need to rule out correlated group effects. To remove correlated effects caused by self-selection, we follow Lerner and Malmendier (2013) and Shue (2013) and exploit the random assignment of the MBA students in our sample to one of six sections, which we use to form peer groups. Similar to the random assignment of MBA students at Harvard Business School, as detailed by Lerner and Malmendier (2013), administrators at Michigan try to maximize diversity within sections across a number of dimensions, while keeping the sizes of the sections roughly equal. The following dimensions are equally weighted in the assignment algorithm: gender, ethnicity, citizenship, undergraduate institution, employer, and dual-degree status. In addition, the assignment algorithm separates spouses or partners into different sections. This randomization is highly similar to the one used at HBS, though with fewer dimensions and a less complex algorithm, as described to us in interviews with enrollment administrators at Michigan.

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 and trust (Croson and Gneezy 2009; Sapienza, Toldra-Simats, and Zingales 2013). 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 the demographic characteristics create group-specific effects. Therefore, we control for these and other possible confounding variables in our tests.

Although random assignment removes self-selection bias, it does not necessarily remove the potential for section-specific shocks. If all students in one section have a different experience than do the students in other sections, and the experience influences their economic attitudes such that there is more or less variation in attitudes, our results could be biased. To address this concern, we test for peer effects using peers' predetermined attitudes recorded before starting the MBA program and hence before any section-specific shocks could occur. Later in the paper we run further robustness tests to control for section-specific shocks that could persist even when using predetermined peer variables.

2.2 The reflection problem

A second challenge in identifying peer effects is known as the reflection problem (Manski 1993). The reflection problem is the long-standing econometric problem of simultaneity bias caused by regressing outcomes on outcomes. When two equations, such as supply and demand, are determined simultaneously in equilibrium, then it is not possible to see which outcome causes the other. Moffitt (2001) shows that Manski's reflection problem is embedded in this more general econometric challenge.

To address simultaneity bias, we use predetermined peer attitudes to proxy for current attitudes, following prior research (Sacerdote 2001; Hanushek et al. 2003; Hoxby and Weingarth 2005; Lavy, Paserman, and Schlosser 2012). In particular, in wave 1 we proxy for peer average attitudes using the exogenous attitudes elicited from survey wave 0, prior to social interaction. Using predetermined variables allows for a causal interpretation of the regression results. However, we are careful to note that although using predetermined variables overcomes simultaneity, doing so cannot separately identify endogenous and contextual peer effects.

Following a variation of Graham and Hahn's (2005) work, we estimate a standard linear-in-means model of peer effects as follows:

$$A_{i,s,t} = \alpha + \beta \bar{A}_{-i,s,0} + \delta X_{i,s,t} + \varepsilon_{i,s,t}, \quad (1)$$

where $A_{i,s,t}$ is the attitude of individual i in peer group s in wave t ; $\bar{A}_{-i,s,0}$ is the leave-one-out average attitude of all people in group s , excluding individual i in wave 0; and $X_{i,s,t}$ is a set of individual-level control variables. A positive coefficient for β means that a person's attitudes conform to her peers' predetermined attitudes. If the random peers' attitudes are low before social interaction, then a positive peer effect is observed if the person's attitudes are low after social interactions, relative to other people.⁴

The linear-in-means model of peer effects is a common assumption in prior studies of peer effects. By studying the influence of the average peer, as opposed to the influence of a peer leader, for example, the linear-in-means model makes relatively weak assumptions about the influence of social interaction on attitudes. Instead, if we could identify students with greater influence using predetermined variables, then our tests could have greater power to detect peer effects. Other studies have identified leaders by grade levels or professional standing (Carrell et al. 2007; Waldinger 2012). In our setting with random assignment, we have no *ex ante* measure of peer leadership, so we follow the convention in the literature and make no *a priori* assumptions about which students have greater social influence.

To further test for peer effects, we run regressions of an individual's change in attitude from wave 0 to wave 1 on the individual's difference from her peers in wave 0, as follows:

$$A_{i,s,1} - A_{i,s,0} = \alpha + \beta (A_{i,s,0} - \bar{A}_{-i,s,0}) + \varepsilon_{i,s,t}. \quad (2)$$

This regression controls for all time-invariant aspects of an individual, such as gender, race, and financial sophistication and highlights the change in attitudes over time for each individual based on her predetermined similarity

⁴ An alternative procedure to overcome simultaneity is to follow Brown et al. (2008), who instrument for exogenous variation in stock market participation, using lagged average participation of the community in which an individual used to live. Our proxy is likely to be more powerful because it uses a student's own predetermined attitudes, rather than country-level attitudes, to proxy for her attitudes after the first year of the MBA program.

to the peer average. A negative β indicates positive peer effects. To conform to peer averages, people who report values of A higher than their peers in wave 0 display a decrease in the values of A from wave 0 to wave 1. People with low A scores in wave 0 increase their scores to conform to the average. Because peers are randomly assigned, a person's distance to her peers' average in wave 0 is also random. This means we can interpret the results as causal evidence.

As mentioned above, our research design does not allow us to separate endogenous from contextual peer effects. However, by using within-individual changes in attitudes in Equation (2), we control for all time-invariant contextual factors. This means that the only way that our results would be completely explained by contextual effects is if there is a dynamic, time-varying response to the exogenous peer group contextual characteristics, and no endogenous reactions. Allowing for this possibility, we assume that our results represent a combination of endogenous and contextual effects, as is common in the literature (e.g., Sacerdote 2001).

2.3 Validity of peer groups

Manski (2000) makes an important point that is often overlooked in peer effects research. Peer groups must be defined such that they capture true social interactions. Stinebrickner and Stinebrickner (2006) define relevant peers as those with "potential influence." Because there is no standard definition of a peer group, the likelihood of potential influence must be verified empirically.

We have several reasons to believe that section groups contain influential peers. First, Michigan follows a similar model as does Harvard and encourages students to take pride in their section (Shue 2013). 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, and students are only allowed to change sections under extreme circumstances.

Using course enrollment records, we empirically verify these claims. In the first year of the program, two students in the same section take classes worth 17.4 credit hours together, on average, compared with 0.37 credit hours for two students in different sections.⁵ This means that our peer groups interact with their section mates for the vast majority of their class time and spend almost no class time with students in other sections.⁶

Though the results show that students in the same section spend a considerable amount of time together in the first year, they may not actually form social ties. Using our survey-based study, we address this question empirically.

⁵ In the final half-semester of the first year, students complete a large-scale field assignment worth 7.5 credits. All students are officially in the same course section, though they work in teams that are only partially randomized. We do not count this assignment in our calculations.

⁶ During the second year, students choose elective courses and do not remain with their section peers. Therefore, we focus only on the first year.

Table 2
Summary statistics of risk aversion and trust by section and wave

	Section						All
	1	2	3	4	5	6	
Risk aversion							
Wave 0	6.405 (1.499) [37]	6.800 (1.702) [50]	6.027 (2.048) [37]	6.733 (1.924) [45]	6.219 (1.845) [32]	6.047 (1.573) [43]	6.402 (1.781) [244]
Wave 1	7.000 (1.272) [22]	7.121 (1.474) [33]	5.844 (1.547) [32]	6.394 (1.694) [33]	7.048 (1.774) [21]	6.000 (1.259) [30]	6.520 (1.581) [171]
Trust							
Wave 0	0.525 (0.506) [40]	0.424 (0.498) [59]	0.556 (0.504) [36]	0.500 (0.506) [44]	0.588 (0.500) [34]	0.619 (0.492) [42]	0.525 (0.500) [255]
Wave 1	0.516 (0.508) [31]	0.588 (0.500) [34]	0.600 (0.497) [35]	0.531 (0.507) [32]	0.517 (0.509) [29]	0.500 (0.508) [34]	0.544 (0.499) [195]

Data are from survey responses of MBA students assigned to one of six sections. “Risk Aversion” is measured following Holt and Laury (2002). “Trust” is measured following the World Values Survey. Wave 0 survey responses are taken before the first year of the MBA program. Wave 1 responses are from surveys conducted at the end of first academic year of the MBA program. The first entry in each cell is the average; the second entry, appearing in parenthesis, is the standard deviation; and the third entry, appearing in brackets, is the number of observations.

In the second wave of the study, we ask students to name up to 15 friends and 15 professional contacts from a list of all MBA students in their year-cohort. After the first year, 43% of listed friends and 36% of listed professional contacts are in the same section, on average, compared with 17%, the fraction that would be observed if friends and contacts were drawn from all sections equally. The results provide solid evidence that peer groups based on section assignments define a collection of people who actually interact socially. They spend a significant amount of time with each other and they are more likely to form friendships and professional ties.

3. Empirical Results

3.1 Summary statistics

Table 2 presents summary statistics of survey responses for risk aversion and trust by section and by survey wave. On average, we find that participants are risk averse, switching to the risky response (option B) on lottery choice 6.5. This is consistent with results using samples of undergraduates, MBA students, and professors and both real and hypothetical outcomes, as reported by Holt and Laury (2002, 2005). In unreported statistics, we find a standard deviation of 1.7 and an interquartile range of 3. We also find that the average risk aversion across all participants is stable over the two survey waves, though there is greater variability within sections.

Turning to trust, about 53% of participants believe that people could be trusted, in general. This is higher than the 26.1% reported in the 2005–2007 waves of the WVS but is closer to the 38.7% for U.S. respondents aged 30 to 49 years and the 40% trustfulness for U.S. males. The average level of trust increases one percentage point from survey wave 0 to 1, and there is variation across sections.

In Online Appendix Table 2, we report correlations between risk aversion, trust, and demographic characteristics. We find a positive, but statistically insignificant, correlation between trust and risk aversion. We find no significant correlation between trust and any of the demographic variables. For risk aversion, we find no relation to age, gender, income, marital status, or children but find that Asian students report higher risk aversion, and Hispanic students report lower risk aversion. Finding no relation between risk aversion and gender is consistent with Croson and Gneezy's (2009) evidence, which shows that women are more risk averse than men in the general population, but not in populations of managers and professionals. Though the correlations with demographics are not strong, we still control for individual-level characteristics in subsequent tests.

In all subsequent tests, we normalize the trust and risk aversion by subtracting their mean and dividing by their standard deviation, where we calculate means and standard deviations by wave at either the individual or peer-average level. We do this because the variance of the peer averages are much smaller than the variance of the individual responses, making it hard to interpret regression coefficients. The normalization means that the coefficient estimates are interpreted as the marginal effect of a one-standard-deviation change in the peer average on standard deviation changes in the individual attitudes.

3.2 Tests of random assignment

Before investigating the presence of peer effects, we test for random assignment across sections with respect to economic attitudes. Following Sacerdote (2001), we regress predetermined individual attitudes on predetermined average peer attitudes in wave 0. Because these are measured prior to any meaningful social interaction, coefficient estimates that are different than zero imply a nonrandom assignment procedure. 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, Kroft, and Notowidigdo (2009) show that this test of random assignment is negatively biased. To see this, consider the extreme case of two people in a section: one with a higher score than the other. The leave-one-out peer average for the low-score person is the high score of the other person. The peer average for the high-score person is the low score of the other. This implies that in the cross-section there is a negative relation between individual and peer averages.

Table 3
The relationship between own and peers' predetermined attitudes: Random assignment

	Risk aversion				Trust			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predetermined peer average	0.041 (0.040) [0.349]	0.045 (0.053) [0.347]	0.060 (0.046) [0.266]	0.065 (0.055) [0.268]	-0.037 (0.085) [0.910]	-0.042 (0.076) [0.947]	-0.026 (0.082) [0.819]	-0.037 (0.084) [0.887]
Trust		0.115* (0.057)		0.093 (0.072)				
Risk aversion						0.097 (0.058)		0.080 (0.079)
Female			0.135 (0.112)	0.110 (0.133)			-0.170 (0.166)	-0.175 (0.142)
Age			-0.029 (0.024)	-0.022 (0.030)			0.014 (0.015)	-0.014 (0.044)
Children			-0.197 (0.250)	-0.302 (0.339)			-0.328 (0.358)	-0.287 (0.327)
Additional controls	no	no	yes	yes	no	no	yes	yes
Adjusted R ²	-0.002	0.004	0.026	0.020	-0.003	0.004	-0.030	-0.045
Observations	244	215	232	206	255	215	244	206

Coefficients from OLS regressions, where dependent and explanatory variables are from surveys conducted by MBA students in wave 0, before the MBA program began. "Peer average" indicates the leave-one-out average of the dependent variable. "Trust" and "Risk aversion" are individual attitudes in wave 0. "Additional controls" include income, marital status, and race dummy variables. Dependent variables and "Peer average" are normalized so that coefficients are the standard deviation changes in individual attitudes for a one-standard-deviation-change in peer averages. Standard errors clustered by section are in parentheses. *p*-values from Monte Carlo placebo tests are in brackets. Significance at the 1%, 5%, and 10% level is indicated by ***, **, and *.

To address this bias, Guryan, Kroft, and Notowidigdo (2009) suggest comparing the coefficient estimates from regressions using the actual section-assignments to the empirical distribution of regression coefficients estimated in Monte Carlo simulations using synthetically created random assignments.⁷ Therefore, for all subsequent regressions, we form an empirical distribution of peer effects regression coefficients from 10,000 simulations. In each simulation, we create synthetic peer groups by randomly assigning students to one of six sections with equal likelihood. From the simulations, we calculate a *p*-value of the true coefficient using the empirical distribution of simulated coefficients.

Table 3 presents the results on randomization and Online Appendix Table 3 presents the empirical distribution of the peer effect coefficients from the simulations. For each attitude, we run tests with no controls and with a host of individual-level controls used in all remaining tests, including gender, age, and dummies for parenthood, income, marital status, and race. Though recent research shows that risk aversion and trust are unique attitudes (Eckel and Wilson 2004; Fehr 2009; Houser, Schunk, and Winter 2010), we also control for contemporaneous correlations between an individual's trust and risk aversion.

⁷ 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 multiple subpopulations of potential peers. Because we only have one common source of peers, this correction is infeasible.

Table 4
Peer effects in economic attitudes: Causal evidence

	Risk aversion				Trust			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predetermined peer average	0.181*** (0.098) [0.008]	0.207** (0.091) [0.012]	0.221*** (0.069) [0.009]	0.247*** (0.069) [0.005]	-0.110 (0.023) [0.333]	-0.103 (0.039) [0.429]	-0.072 (0.035) [0.645]	-0.124 (0.052) [0.371]
Trust		0.014 (0.120)		-0.065 (0.089)				
Risk aversion						-0.012 (0.128)		-0.130 (0.128)
Female			0.181 (0.139)	0.263** (0.101)			0.119 (0.127)	0.119 (0.117)
Age			-0.042 (0.061)	-0.051 (0.059)			0.021 (0.049)	-0.013 (0.049)
Children			-1.376*** (0.287)	-1.372*** (0.308)			-0.173 (0.362)	-0.380 (0.348)
Additional controls	no	no	yes	yes	no	no	yes	yes
Adjusted R^2	0.029	0.032	0.179	0.184	0.007	-0.001	-0.065	-0.082
Observations	171	156	142	131	195	156	158	131

OLS regressions, where the dependent variable is attitude after the first academic year from surveys of MBA students. "Predetermined peer average" indicates the leave-one-out average of the dependent variable from survey wave 0, conducted before the MBA program began. Dependent variables and "Predetermined peer average" are normalized so that coefficients are the standard deviation changes in individual attitudes for a one-standard-deviation change in predetermined peer averages. "Trust" and "Risk aversion" are individual attitudes in wave 1. "Additional controls" include income, marital status, and race dummy variables. Standard errors clustered by section are in parentheses. p -values from Monte Carlo placebo tests (described in the text) are in brackets with significance at the 1%, 5%, and 10% level indicated by ***, **, and *.

We report both conventional standard errors, clustered at the section-level, in parentheses, and also the p -value from the simulated empirical distribution. The negative bias and skew of the simulated coefficients reported in Online Appendix Table 3 is consistent with Guryan, Kroft, and Notowidigdo (2009) and shows that the p -values from the simulated distributions are better measures of statistical significance.

In Table 3, we find insignificant coefficients for the effect of the peer average for both risk aversion and trust in all specifications. The results confirm that students are randomly assigned across sections with respect to economic attitudes.

3.3 Causal results of peer effects

Having established random assignment, we now estimate Equation (1), where we relate predetermined peer attitudes to ex post individual attitudes. Table 4 presents the results. As before, we report clustered standard errors in parentheses and p -values from Monte Carlo simulations in brackets.

First, the coefficient estimates in Columns 1–4 reveal positive peer effects in risk aversion, whether or not we include demographic and trust variables as controls. A one-standard-deviation increase in predetermined peer average risk aversion increases ex post own risk aversion by between 0.18 and 0.25

standard deviations. These results are highly statistically significant. We find no effects for age but find that participants with children are statistically less risk averse than those without children. We find that women are more risk averse, after controlling for trust.

In contrast, in Columns 5–8 of Table 4, we find no evidence of peer effects in trust. In addition, we find no significant relationships between gender, age, parenthood, or risk aversion and trust. We also calculate seemingly unrelated regressions (SUR) by jointly estimating the risk aversion and trust regressions in Table 4. In panel A of Online Appendix Table 4, we find that the coefficients on peer average risk aversion are significantly different than the coefficients on peer average trust.

These results provide strong evidence that peers influence risk aversion, but not trust. The positive peer effect in risk aversion is consistent with similar evidence of positive peer effects in risky health choices (Gardner and Steinberg 2005). Our results imply that there is a desire for conformity in preferences of financial risk as well. The stark difference between trust and risk aversion is consistent with recent research showing that the two preferences are governed by different cognitive processes. As discussed already, we acknowledge that these results also may be influenced by measurement error in trust.

3.4 Within-individual changes in attitudes

Table 5 presents the results from the individual-level regressions in Equation (2). In Columns 1 and 2, the dependent variable is the change in individual risk aversion. In Columns 3 and 4, it is the change in trust. The explanatory variables of interest are the difference in attitude between an individual and her peers in wave 0.

Controlling for time-invariant characteristics of a person and her peers, we find evidence of a positive peer effect in risk aversion, consistent with our previous results. In Column 1, the negative coefficient on risk aversion implies that individual risk aversion scores move toward the average. Those people who have higher than average risk aversion before the MBA program, relative to peers, tend to experience a decline in risk aversion by the end of the first year of the program. Those with lower than average risk aversion tend to have an increase in risk aversion after the first year. This effect is strengthened after controlling for trust in Column 2. The results show that the magnitude of the change in a person's risk aversion is nontrivial. The coefficient estimates imply that after one year, the difference between a student's risk aversion and the risk aversion of her peers is just 41% as large as the difference before the start of the MBA program.

In contrast, in Columns 3 and 4, we find no significant peer effects in trust. We do find that an individual with a greater difference from her peers in risk aversion in wave 0, has a decrease in trust after the first year. Because we find that more risk-tolerant individuals are also less trusting, this evidence suggests that trust is not simply a risky gamble. In SUR models reported in Online

Table 5
Individual changes in attitudes by distance from predetermined peer averages

Independent variable: Attitude _{<i>i</i>,0} - Attitude _{-<i>i</i>,0}	Dependent variable: Attitude _{<i>i</i>,1} - Attitude _{<i>i</i>,0}			
	Risk Aversion		Trust	
	(1)	(2)	(3)	(4)
Risk aversion	-0.391* (0.088) [0.054]	-0.408** (0.101) [0.028]		-0.155*** (0.056) [0.010]
Trust		-0.166 (0.081) [0.127]	-0.183 (0.067) [0.164]	-0.217 (0.048) [0.551]
Constant	0.009 (0.158)	0.004 (0.148)	0.027 (0.143)	0.051 (0.099)
Adjusted <i>R</i> ²	0.179	0.198	0.083	0.125
Observations	132	115	134	122

This table presents OLS regression results, where the dependent variable is an individual's change in attitude from wave 0 to wave 1. The dependent variables are the differences between an individual's attitude and her peers' average attitude in wave 0. Variables are normalized so that coefficients are the standard deviation changes in individual attitudes for a one-standard-deviation change in predetermined peer averages. Standard errors clustered by section are in parentheses. *p*-values from Monte Carlo placebo tests (described in the text) are in brackets with significance at the 1%, 5%, and 10% level indicated by ***, **, and *.

Appendix Table 4, we find that the coefficients on risk aversion are statistically different than are the coefficients on trust. Overall, the individual fixed effects results are consistent with our prior findings.

3.5 Analysis of variation tests

Following Glaeser, Sacerdote, and Scheinkman (1996), we compare variation in survey responses within each section to variation across sections. Positive peer effects imply that variation within a section is smaller than what would be observed in independent draws, as peers' attitudes become more homogeneous. Because these tests are based only on the second-moment of the distribution of attitudes, they do not depend upon random variation in the average level of responses in attitudes across the six sections. Thus, these tests provide additional evidence beyond the linear-in-means regressions presented above, which rely on the first moment of the distribution.

For each survey wave, and for risk aversion and trust, we compute an *F*-statistic of the ratio of between-section variation to within-section variation. A high *F*-statistic indicates that the variation among peers within a section is less than the variation between sections, consistent with positive peer effects. The null hypothesis of no peer effects is rejected if *F* does not equal one.

The results in Table 6 show that in the first survey wave there are no significant differences between the within-section variance and the between-section variance for risk aversion or trust, consistent with random assignment. In the second survey wave, we find strong evidence of positive peer effects in risk aversion (*F*=4.02) and slightly negative peer effects in trust (*F*=0.23). However, the only significant change in *F*-statistics from wave 0 to wave 1 is

Table 6
Analysis of variance of economic attitudes

	Preinteraction			After first year			Difference
	Sum of squares	d.f.	F_0	Sum of squares	d.f.	F_1	$F_1 - F_0$
Risk aversion							
Between sections	24.57	5	1.57	46.11	5	4.02***	2.45***
Within sections	746.07	238	(0.17)	378.57	165	(0.00)	(0.01)
Trust							
Between sections	1.17	5	0.94	0.29	5	0.23*	-0.71
Within sections	62.41	249	(0.46)	48.09	189	(0.95)	(0.81)

“Risk Aversion” is measured following Holt and Laury (2002). “Trust” is measured following the World Values Survey. Sections are six randomly assigned section assignments for the MBA student participants. “Preinteraction” variables are from surveys taken before the first year of the MBA program. “After first year” variables are from surveys conducted at the end of first academic year of the MBA program. “d.f.” is the degrees of freedom. F_0 and F_1 are F -statistics. Numbers in parentheses below the F statistics indicate $1 - \Pr(X \leq x)$. F -statistics in the extreme 1%, 5%, and 10% are indicated by ***, **, and *. Significance of the difference of the F -statistics is computed using Monte Carlo simulations with 10 million observations.

for risk aversion. The change in the F -statistic for trust is insignificant. These results provide complementary evidence that risk aversion exhibits a positive peer effect and trust exhibits no peer effects.

3.6 Implications of the results

The evidence we have presented provides a consistent result: social interaction leads to conformity in individual risk aversion, but not in trust. As stated previously, we argue that it is reasonable that some of these effects are attributable to endogenous peer effects. This means that risk aversion may spill over to others and cause a social multiplier, which could cause feedback effects that transmit risk aversion attitudes across a society. In addition, we find peer effects after just one year in the MBA program. This suggests that risk preferences could fluctuate often, especially when there is more social interaction or if peer groups change frequently. At the individual level, time-varying risk aversion provides an alternative explanation for people’s trading behaviors, beyond rebalancing and information-related trading (Guiso, Sapienza, and Zingales 2013).

Our results have important implications for understanding how social interaction at the individual-level affects macroeconomic outcomes. For instance, changes in aggregate risk aversion could be driven by the spread of local risk aversion through social interaction. This could lead to time-varying risk premiums, a concept that is central to stock and bond return predictability (Ferson and Harvey 1991) and excess comovement (Gourio, Siemer, and Verdelhan 2013). More generally, accounting for the transmission of risk preferences through social interaction can help to explain situations in which individual traders’ actions appear to have greater commonality than is expected.

At the same time, finding no peer effects in trust also has important implications. This suggests that although trust is likely influenced by other environmental forces, such as parental guidance, it may be more influenced by persistent social environments, such as institutions (Knack and Keefer 1997; Zak and Knack 2001). One implication is that when people are put in new social environments, such as at college or in a new job, they may take more risky actions, but not become more trusting in general. The distinction between risk aversion and trust may also help illuminate the underlying forces behind stock market participation, which has been tied to both risk aversion (see Barberis, Huang, and Thaler 2006) and trust (Guiso, Sapienza, and Zingales 2008).

Third, by eliciting subjective attitudes, our results might help to disentangle two underlying mechanisms that drive peer effects: expectation and preference interactions (Manski 2000). Expectation interactions occur when people update their beliefs based on observational learning of peers. Preference interactions occur when peers' preferences influence an individual's preferences. It is beyond the scope of this paper to completely separate these mechanisms, but because ours is one of the first investigations of peer effects in subjective attitudes, rather than observed outcomes, our results may provide some insight. In particular, because our measures are based on unobserved subjective preferences in individual surveys, rather than specific observable outcomes, it is possible that the peer effects in our study more likely reflect preference interactions than expectation interactions. This interpretation is consistent with the idea that people conform to social norms of risk aversion to receive utility benefits (Akerlof 1980; Bernheim 1994), rather than because they have learned about risk aversion. This means that if policy makers want to influence risk preferences at the population level, education may be insufficient. Instead, changes in social norms of risk taking may be required.

4. Robustness Tests

In this section, we present a number of empirical tests that address alternative explanations of our results. These include section-specific shocks, financial sophistication, differences in the coarseness of our variables, and the possibility that students completed the survey together.

4.1 Section-specific shocks

Though random assignment accounts for correlated shocks driven by self-selected peers, one limitation of our study design is that we must assume there are no omitted section-specific shocks. Using predetermined peer averages as our explanatory variable controls for the most likely section-specific shocks because these variables are recorded before any section-specific shock could occur. However, it is still possible that section-specific effects are correlated with the predetermined variables if instructors self-select into sections based

on students' predetermined attitudes or if instructors alter their teaching styles based on predetermined attitudes.

Though we think these channels are unlikely, to be conservative, we control for instructor fixed effects—the most likely source of section-specific shocks. Each course is typically cotaught by two instructors, who each teach three of the six sections. We control for instructor fixed effects in two ways. First, in Online Appendix Table 5, we run analysis of variation tests as in Table 6 of the main paper, but where peer groups are formed based on shared instructors, rather than section assignments. We find no significant peer effects in these tests, indicating that the main results are not driven by the influence of instructors.

In the second set of tests, we run tests equivalent to those in Table 4 but include dummy variables for the identity of the instructors. Controlling for instructor effects also does not change our main results in these tests.

4.2 Student field of interest and finance knowledge

Though we have argued that the survey outcomes we study reflect preferences, not expectations, one could be concerned that risk aversion is related to the course content in a business school. Perhaps taking finance classes, where financial risk is discussed at length, could influence the survey responses. To address this concern, we run additional tests where we control for a student's stated field of interest in wave 0 (accounting, marketing, finance, etc.) and her cumulative grade point average in finance classes taken during the first year. Grade data are only available for a subset of the sample population.

In Online Appendix Table 6, we include field of interest and GPA as independent variables and also as interactions with predetermined peer average risk aversion. For field of interest, we find no changes to our main results. Those with an interest in finance report higher risk aversion, but there is no significant interaction. Controlling for GPA in finance classes does not change the main result either. However, because many students would not allow us to access their GPAs, the sample size is reduced considerably. This may explain why none of the variables are significant, once an interaction term is included. Overall, the results do not suggest that the peer effects in risk aversion are driven by financial interest or skill.

4.3 Binary risk aversion measure

As stated already, we specifically use standard survey questions to elicit risk aversion and trust because they have been vetted by numerous prior studies. One drawback to these measures is that they differ in their coarseness. Risk aversion is measured on a ten-point scale. Trust is measured on a binary scale. It is possible that for the same change in underlying attitudes, we may be able to empirically identify changes in risk aversion, but not in trust. Because we

have no way to expand the measure of trust, we instead collapse our measure of risk aversion to a binary measure for comparability.

In Online Appendix Table 7, we present results from regressions computed identically to the main regressions in Table 4, except that risk aversion is a binary variable. We create a dummy variable for risk aversion equal to one if a student switched from the safe to the risky choice on the fifth or later gamble in the multiple price list, and zero otherwise. Consistent with our main findings, the results using binary risk aversion show significant and positive peer effects. Using a dummy based on switching to the safe choice in the sixth or seventh gamble provide similar results.

4.4 Relationship between survey responses and actual behavior

In an ideal world, the peer effects we observe using survey-based measures of risk aversion and trust would translate to actual behaviors. Evidence of behavioral changes would be strong corroborating evidence. However, finding such a relationship is highly unlikely. Though Barsky et al. (1997) find a positive relationship between financial risk aversion and risky behaviors, such as smoking, drinking, and failing to have insurance, they point out that “risk tolerance explains only a small fraction of the variation of the studied behaviors.” Their sample includes roughly 11,000 respondents in the Health and Retirement Study. A second example comes from Anderson and Mellor (2008). They find negative relationships between risky health choices (such as smoking and not using a seat belt when driving) and financial risk aversion using a measure similar to ours, but the statistical significance of the relationship is weak or insignificant in a sample of close to a 1,000 participants, roughly four times as large as the size of our sample. With our much smaller sample, it is not surprising that in unreported tests we also do not find significant relationships between peer effects in survey attitudes and peer effects in actual risky behavior, such as choosing career paths with greater salary uncertainty.

4.5 Timing of survey responses and peers

Because our surveys are administered online, we do not directly observe students when they complete the surveys. Though we use strict controls to authenticate the participants’ identities, as required by the Family Educational Rights and Privacy Act (FERPA), one concern is that peers may complete the survey together, rather than in private. In this case, we may find a positive peer effect simply because students are discussing their survey responses, rather than because peers have influenced true preferences.

To test this alternative hypothesis, we calculate the absolute difference of two students’ risk aversion and trust responses in survey wave 1 for all possible pairs of students within each section. Then we use the precise date-time that students completed the survey to calculate whether (1) the two students were taking the survey at the same time and (2) the absolute difference in date-time that two students began the survey. We regress the pair’s absolute difference of

risk aversion and trust on these measures of proximity of survey timing. The results in Online Appendix Table 8 show no relation between the similarity of section peers' survey responses and the proximity of the time in which the surveys were completed. This evidence implies that our main results are not driven by students completing the surveys together.

4.6 Placebo tests

We conduct placebo tests to verify that our results are not driven by chance alone. Using estimates from the 10,000 Monte Carlo simulations used to construct confidence intervals, we test for evidence of peer effects. Because the synthetic section assignments do not reflect social interaction, we expect to find no results. In Online Appendix Table 9 we find that the average coefficient estimates and standard errors for the peer effects are insignificant for all variables in all specifications.

5. Conclusion

Social peers influence risk aversion, but not trust of MBA students. When a student is randomly assigned to a peer group that has high average risk aversion, the student's risk aversion increases. In addition, across each of six student sections, we find evidence that the within-section variance in students' risk aversion diminished after a year of social interaction, consistent with positive peer effects. In contrast, we find no evidence of peer effects in trust, implying that trust is more rigid than risk aversion. The results are robust to alternative explanations based on self-selected peers, sample attrition, instructor fixed effects, financial sophistication, and other alternative explanations.

The results of this paper demonstrate that risk aversion, central to much of financial economic theory, is malleable. Given that our sample consists of mature adults, rather than adolescents or children, our results suggest that risk aversion is likely to be influenced by peers throughout adulthood. Changes in environments, such as a new job, geographic relocation, and retirement, could lead to changes in risk aversion as new peer groups are formed. In contrast, our results show that generalized trust is a cultural value that is not easily changed, as argued by Guiso, Sapienza, and Zingales (2006). This suggests that other environmental factors are more important for the transmission of generalized trust than peer influence.

In addition, we conjecture that risk aversion is affected by endogenous peer effects, which can lead to a social multiplier. This implies that attitudes toward risk can spread through social connections. This has interesting ramifications. For example, if corporate culture influences attitudes toward risk, employees may alter their views about risky investments depending upon the social norms of their employer. More broadly, social interactions can cause local risk aversion to aggregate to macroeconomic changes in investment, consistent with Hong, Kubik, and Stein (2004).

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