

Social Identity and Preferences^{*}

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Abstract

In two laboratory experiments, we test whether social identities can affect time and risk preferences. We find that when we make ethnic identity salient to Asian-American subjects, they make more patient choices. When we make race salient to white and black subjects, white subjects make more patient and less risk-averse choices, and non-immigrant blacks make more risk-averse choices. Our ethnic and racial identity results are consistent with U.S. demographic patterns in economic outcomes. Making gender identity salient causes risk aversion to conform to the gender risk aversion stereotype the subject believes is relatively more common. Our findings provide support for the hypothesis that cultural differences help explain differences in economic outcomes.

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I. Introduction

Relative to black Americans, white Americans accumulate more financial wealth (Altonji, Doraszelski, and Segal, 2000), accumulate more human capital (Neal and Johnson, 1996; Fryer and Levitt, 2004), and are more likely to invest in the stock market (Hurst, Luoh, Stafford, and Gale, 1998). However, black immigrants from the West Indies and Africa are disproportionately represented among high-income blacks and elite college students (Sowell, 1975; Rimer and Arenson, 2004). Relative to white Americans, Asian-Americans are more likely to participate in tax-deferred savings accounts (Springstead and Wilson, 2000) and accumulate more human capital (Sue and Okazaki, 1990).¹ Women invest in more conservative financial assets than men (Jianakoplos and Bernasek, 1998; Sundén and Surette, 1998).² These differences persist even after controlling for other observable variables like income, family characteristics, and school quality measures.

Some social scientists have argued that these demographic differences result from culturally induced differences in preferences (e.g. Sowell, 1975, 1981, 2005; Murray, 1984; Chiswick, 1983; Barke, Jenkins-Smith, and Slovic, 1997). Unfortunately, culture is confounded with many other factors such as socioeconomic status and peer pressure (Austen-Smith and Fryer, 2005; Fryer and Torelli, 2005). Exogenous cultural variation that permits definitive causal inference is rare.

Social psychology offers a methodology for introducing such exogenous variation. Self-categorization theory, a long-standing idea in psychology (e.g. James, 1890) that was first formalized by Turner (1985), posits that people have multiple social identities: race, ethnicity, gender, occupation, etc. Each of these identities has a different socially prescribed set of preferences. Furthermore, not all of a person's identities are salient at any point in time. Environmental cues trigger certain identities, and a person adopts the preferences prescribed for his or her momentarily salient identities. Akerlof and Kranton (2000, 2002, 2005) formulate a similar theory within an economic framework.

If the self-categorization theory is valid, then one can identify the cultural effect on preferences by exogenously varying the salience of an aspect of cultural identity and seeing how

¹ Carroll, Rhee, and Rhee (1994, 1999) do not find that Asian immigrants save more, but they are hindered by their data quality. Carroll, Rhee, and Rhee (1994) do find that Asian-Canadian immigrants' educational expenditures are 3.6 times the Canadian average.

² See Croson and Gneezy (2004) for a survey of the experimental evidence showing that women are more risk-averse than men.

people's preferences change. We perform such a manipulation in the laboratory by asking subjects questions about their family background or questions about living with individuals of different races or genders. Control subjects were instead asked neutral questions unrelated to identity. We then elicit subjects' time and risk preferences using standard incentive-compatible mechanisms.

In our first experiment, we make Asian ethnic identity salient to Asian-American subjects by asking them about their family background. We find that these primed Asian-American subjects make much more patient choices than Asian-American control subjects. The median primed Asian requires an interest rate much lower than the median unprimed Asian for delaying receipt of payment. Priming race does not affect Asians' risk aversion. Asking about family background has no effect on preferences of white subjects, suggesting that ethnic identity does not influence time and risk preferences for the American whites in our sample.

In our second experiment, we make racial identity salient to white and black subjects by asking questions about living with individuals of different races. We find that primed white subjects become more patient and less risk-averse than control white subjects. Priming whites' racial identity greatly lowers the median interest rate required to delay receipt of payment and the risk premium required to accept gambles. We find some evidence that the risk aversion reduction is particularly large among white subjects who report attaching more importance to their racial identity. For black subjects, the priming effect depends upon how recently their family immigrated to the U.S. Blacks with longstanding U.S. roots become more risk averse when primed, whereas blacks who have at least one foreign-born parent appear to become less risk averse. We find that making race salient has no effect on patience for both black immigrants and non-immigrants.

We also find no mean effect from priming gender identity. However, there is a significant interaction effect on risk preferences. Priming gender increases risk aversion among men who believe that cautious stereotypes about men are more common. Priming gender decreases risk aversion in women who believe that reckless stereotypes about women are more common. These effects reverse for those who hold opposite beliefs about the stereotypes.

Our results are broadly consistent with the hypothesis that social identity plays a causal role in determining economic preferences. The ethnic and racial identity effects we find are

directionally consistent with patterns in economic outcomes among racial groups in the U.S., suggesting that cultural factors contribute to these differences.

We are not the first to show that manipulating identity salience leads to behavioral changes. However, to our knowledge we are the first to explore identity salience effects on primitive preference parameters using incentive-compatible mechanisms. Other researchers have shown that identity salience affects preferences elicited hypothetically over highbrow versus lowbrow activities, collectivist versus individualist behavior, and professional- versus family-oriented activities (LeBoeuf and Shafir, 2005); animal vivisection and ethically questionable experimentation (Reicher and Levine, 1994); and ethnically targeted advertising (Forehand, Deshpandé, and Reed, 2002).

We interpret our results in light of the psychology literature on identity salience, according to which priming identity causes individuals to behave in conformance with socially prescribed preferences. However, psychological work on “stereotype threat” argues that priming identity can cause members of disadvantaged groups to become anxious, which disrupts cognitive processing and causes worse performance on standardized tests (e.g., Steele and Aronson, 1995; Shih, Pittinsky, and Ambady, 1999; Hoff and Pandey, 2006). Conversely, “stereotype lift” increases cognitive performance of a group when negative stereotypes about *other* groups are made salient (Walton and Cohen, 2003). Our results do not appear to be driven by stereotype threat and lift effects. A necessary condition for stereotype threat and lift effects is that subjects perceive the task to be diagnostic of ability, and we do not present our preference elicitation as such. In our second experiment, subjects answered five SAT-style math questions after their preferences were elicited. Primed subjects did not perform differently than unprimed subjects.

Our work contributes to a growing economics literature on how culture affects economic outcomes. Guiso, Sapienza, and Zingales (2006) review this literature, most of which focuses on trust and political preferences rather than risk and time preferences. Guiso, Sapienza, and Zingales (2003, 2006) show that religious belief affects the likelihood a survey respondent reports that thrift is important to teach children, and the percent of people in a country who say thrift is important to teach is positively correlated with the national savings rate. Knowles and Postlewaite (2005) show that savings propensities are transmitted from parents to children, which is consistent with a cultural mechanism.

The paper is organized as followed. Section 2 presents the first experiment, which studied ethnic priming effects among Asian-Americans and whites. Section 3 presents the second experiment, which studied racial priming effects among whites and blacks, as well as gender priming effects. Section 4 concludes.

II. Experiment 1: Asian-American and White Ethnic Identities

Motivated by evidence of high Asian-American savings rates and educational investment, we hypothesized that making Asian ethnic identity salient to Asian-Americans makes them more patient. American stereotypes about East Asian patience and industriousness date back to at least the 19th century (Twain, 1872)³ and persist to this day (e.g., Kasindorf, 1982; Abboud and Kim, 2005). Although there are differences between Asian cultures, Hofstede and Bond (1988) argue that most are high in “Confucian Dynamism,” which emphasizes a “future-oriented mentality.”

We use the method developed by Shih, Pittinsky, and Ambady (1999) to prime ethnic identity. We then elicit time and risk preferences from primed and unprimed subjects using an incentive-compatible mechanism. If Asian ethnic identity plays a causal role in determining preferences, then we expect to see that the priming manipulation causes Asians to behave more patiently. We apply the same prime to white subjects to check that any Asian priming effect is working specifically through the increased salience of *Asian* ethnic identity, rather than through some other channel.

A. Participants

Participants were 159 Harvard College undergraduates, 71 of Asian descent and 66 of white descent. We drop from our analysis three biracial participants and 18 participants who were neither white nor Asian. Within our Asian group, 90% were of East Asian descent, and the remainder were of Asian Indian descent.

We recruited participants by putting up posters in the Harvard psychology building, e-mailing students who reported being members of undergraduate Asian-American clubs on facebook.com, and e-mailing Harvard dormitory lists. There were a small number of subjects

³ Twain wrote, “They are quiet, peaceable, tractable, free from drunkenness, and they are as industrious as the day is long. A disorderly Chinaman is rare, and a lazy one does not exist... Chinamen make good house servants, being quick, obedient, patient, quick to learn and tirelessly industrious.”

who walked into experimental sessions upon observing that they were about to start. At no point did we specify in our recruiting materials that we were looking for white and Asian students.

B. Procedure

The experimenter, a male of black, Mexican, and white descent, ran 15-minute sessions with groups of between one and ten subjects from December 2004 to February 2005. Half the participants were randomly assigned to the ethnicity-salience condition and half to the control condition. At the onset of the experiment, the same instructions describing the experiment and its compensation scheme were read to every subject. Subjects then responded to three sections of questions. As they completed each section, they continued without interruption to the next one. The first section was a “background questionnaire” that varied by condition. The second section elicited participants’ time preferences. The third section elicited their risk preferences. Finally, participants were debriefed, their race was recorded, and payments were made.

Ethnicity-salience manipulation. In the ethnicity-salience condition, there were eight questions on the “background questionnaire”:

- (a) What year in school are you?
- (b) Do you live on or off campus?
- (c) Do your parents or grandparents speak any languages other than English?
- (d) What languages do you know?
- (e) What opportunities do you have to speak these languages around campus?
- (f) What percentage of these opportunities is found in the residence halls?
- (g) What language do you speak at home?
- (h) How many generations of your family have lived in the United States?

Questions (c) through (h) are exactly those used by Shih, Pittinsky, and Ambady (1999) to make ethnicity salient to Asian-Americans. Questions (a) and (b) were added to disguise the questionnaire’s intent.

Control condition. In the control condition, the “background questionnaire” began with the same two questions as the ethnicity-salience questionnaire. The remaining six questions were designed to be neutral with respect to ethnic identity:

- (a) What year in school are you?
- (b) Do you live on or off campus?
- (c) How many meals a week do you eat in the residence dining halls?
- (d) From 1 to 7 how satisfied would you say you are with the food?
- (e) If a limited-meals meal plan were offered would it interest you?
- (f) Would you consider subscribing to cable television if it was offered?
- (g) How much would you be willing to pay per month for this service?
- (h) List one or two reasons why you would or would not subscribe to cable television.

These questions are modeled after the control questions of Shih, Pittinsky, and Ambady (1999), modified to be relevant for the Harvard student body.

Measured time preferences. We measured time preferences by asking participants to make a series of binary choices, each of which had some probability of determining their payment. The choices were divided into two 11-question blocks and two 12-question blocks. One of the 11-question blocks required participants to circle either “\$3 today **or** X in 1 week,” where $X = \$3.05, \$3.10, \$3.25, \$3.50, \$3.75, \$4.00, \$4.50, \$5.00, \$5.50, \$6.00,$ or $\$7.00$. The other 11-question block asked about “\$3 in 1 week **or** X in 2 weeks,” where X took on the same values as in the first block. The 12-question blocks were the same as the first two, except that the monetary amounts were larger. The immediate reward was \$7, and the delayed rewards took values $X = \$7.10, \$7.25, \$7.50, \$8.00, \$8.50, \$9.25, \$10.00, \$10.75, \$11.75, \$12.50, \$13.75,$ or $\$15.00$. Half the participants saw the questions in order of ascending X , and half with decreasing. Half answered the today versus one week questions before the one week versus two weeks questions, and half the other way around. It took participants around five minutes to answer the time preference questions.

Although our approach to measuring time preferences is standard (Frederick, Loewenstein, and O’Donoghue, 2002), it has been argued that choices over the timing of monetary rewards should not measure time preference, since people can (in principle) borrow or lend money at the market interest rate regardless of how they discount future utility (Fuchs, 1982). However, in experiments like ours, most participants discount future rewards at a much higher rate than the market interest rate (Frederick, Loewenstein, and O’Donoghue 2002), perhaps because they are liquidity-constrained or do not realize that money is fungible. In either

case, questions involving monetary rewards do measure discounting over utility. Consistent with this interpretation, time preference measured in a manner similar to ours predicts variation in discounting-related behaviors such as drug addiction (e.g., Kirby, Petry, and Bickel, 1999; Kirby and Petry, 2004), cigarette smoking (Fuchs, 1982; Bickel, Odum, and Madden, 1999), excessive gambling (Petry and Casarella, 1999), use of commitment savings devices (Ashraf, Karlan, and Yin, 2004), and rapid exhaustion of food stamps (Shapiro, 2005). (See also Loewenstein, Read, and Baumeister, 2003.)

Some economists are troubled by the fact that subjects in experiments such as ours require extremely high interest rates to delay payment receipt. For example, a subject choosing to receive \$3 today rather than \$3.05 in one week is borrowing at an annualized interest rate of 136%. Although it is difficult to believe that such impatience is normatively justified, even higher interest rates are sometimes observed in real-world markets that involve short-term borrowing. For instance, nine million American households took out a payday loan in 2002 at annualized interest rates typically around 7000% (Tobacman and Skiba, 2005). Therefore, the extreme interest rates we observe are not confined to laboratory settings.

Measured risk preferences. We measured risk preferences with 18 binary choices between a safe option and a gamble: “\$4 guaranteed **or** a $Y\%$ chance at \$8.” Y took all values from 25% through 76% in increments of 3%. Half the participants saw the questions in order of ascending Y and half with decreasing. Each binary choice had some probability of determining the participant’s payout. Answering these questions took about three minutes.

Existing evidence suggests that risk preferences measured through choice tasks are related to real-world risk behaviors. Risk aversion measures derived from real-stakes experimental choices are highly correlated with measures from hypothetical choices (Dohmen et al., 2005), which in turn predict risky behaviors such as smoking, drinking, failing to hold insurance, holding stocks rather than Treasury bills, being self-employed, switching jobs, and moving residences (Barsky et al., 1997; Guiso and Paiella, 2001; Dohmen et al., 2005).

Compensation scheme. Before the participant answered any of the preference elicitation questions, the experimenter explained that after the experiment, the participant would randomly select one of the time or risk preference choices to determine his or her payout by drawing a

number out of a bag.⁴ The bag contained slips of paper numbered 1 to 64, one for each preference elicitation question. If a risk preference question was selected, and if the participant had chosen the gamble in that question, then the participant would randomly draw a number out of a different bag, which contained numbers between 1 and 100. If the drawn number was less than or equal to the $Y\%$ probability of winning, the participant won \$8.⁵

All rewards were paid by a check given to the participant immediately following the debriefing. Delayed payments were implemented by post-dating the check. Subjects were told the post-dated check could not be cashed until the date on the check.⁶

C. Econometric methodology

Our dependent variable for the time preference task is the lowest continuously compounded weekly interest rate that induces the subject to choose the later payment over the earlier payment. For example, if the subject chose the later payment over an earlier \$3 payment whenever the later payment is at least \$3.50, then the dependent variable value is $r = \log(3.50/3) = 0.154$.

Our dependent variable for the risk preference task is the lowest expected return premium that induces the subject to choose the gamble over the certain payout. For example, for a subject who chose to gamble for \$8 rather than accepting the sure \$4 if and only if the probability of winning is at least 58%, the dependent variable value is $\pi = (8 \times 0.58 - 4)/4 = 0.16$.⁷

⁴ Existing evidence suggests that paying subjects for a randomly-chosen question causes subjects to behave as if they were being paid for every question (Hey and Lee, 2005).

⁵ The printed instructions on the risk elicitation sheet stated that gambles would be resolved by drawing from a bag of red and blue marbles. Shortly before sessions were scheduled to begin, it became apparent that it would be logistically infeasible to use marbles, but there was not enough time to receive Institutional Review Board approval for a revised elicitation sheet before the first session. Therefore, if a risk preference question was selected at the end of a session, subjects were informed that instead of drawing a marble, they would be drawing a slip of paper to determine the gamble outcome.

⁶ To secure the promise to pay at the end of a loan term, payday lending companies typically use postdated checks collected from borrowers at the time of loan origination (Potter, 2002). Although a check-issuer's bank bears no legal liability if it pays a postdated check early (provided the check-writer did not notify the bank of the check in advance; see U.C.C. §4-401), many banks will not allow account holders to deposit post-dated checks. Although we did not keep track of check deposit dates in Experiment 1, we found in Experiment 2 that almost all subjects deposited their checks after the check date. (All but one deposited their check into a bank checking account.)

⁷ Only two subjects did not have a threshold such that they chose the earlier payment if and only if the interest rate was below that threshold. These two subjects also did not have a risk premium threshold such that they chose the certain payoff if and only if the risk premium was below that threshold. Our results are unaffected by excluding these two subjects.

We observe choices at a finite number of interest rates and risk premia, and there are a substantial number of subjects whose dependent variable observations are left- or right-censored. Therefore, if the subject chooses the earlier \$3 payment over the later \$3.25 payment, but the later \$3.50 payment over the earlier \$3 payment, we only know that her r is between $\log(3.25/3)$ and $\log(3.50/3)$. A similar problem applies to the risk choices. We therefore use an interval regression (Stewart, 1983), which is a maximum-likelihood procedure that assumes that the latent dependent variable is conditionally distributed normally, has an unknown exact value, but is known to fall within a certain interval.

The normality assumption implies that the dependent variable sometimes takes on negative values. This negativity is not a problem in the risk preference regressions, since we do observe some risk-seeking behavior in our data. We therefore use π as the dependent variable in the risk preference regressions. However, our prior belief is that negative interest rates are perverse and likely to be due to elicitation errors. Therefore, we impose lognormality on the interest rate variable by making $\log(r)$ the dependent variable in the interval regression, thus ruling out negative interest rates. In the interest rate regression tables that follow, if the coefficients imply that a certain set of explanatory variable values are associated with a mean $\log(r)$ of $\hat{\mu}$, then the median r is $\exp(\hat{\mu})$. Because of outliers, we will focus on median interest rates in our analysis.⁸

We observe four r values for each participant, since we elicited four sets of intertemporal preferences. In the time preference results that follow, we report results that pool the four r values together, adding explanatory dummy variables to indicate in which trade-off type (now versus one week, one week versus two weeks, small intertemporal choice, large intertemporal choice) the r value was observed. We cluster standard errors by subject to correct for within-subject correlation of r .

D. Results

Table 1 displays coefficients from a regression of participants' required log interest rate and risk premium on experimental condition and trade-off type. Column 1 shows the main result

⁸ The mean r is $\exp(\hat{\mu} + 0.5\sigma^2)$, where σ is the conditional standard deviation of the $\log(r)$ distribution. Outliers make this mean quite large for many experimental groups. However, the point estimates for the priming effects are directionally similar when we focus on mean interest rates.

from Experiment 1: the interest rate required by Asians to defer payment falls dramatically when Asian ethnic identity is made salient. For example, for trade-offs between \$4 now and money one week from now, the median required interest rate falls from 8.8% to 2.1%. Running separate regressions for each intertemporal choice type (immediate payment amount \times time horizon) reveals that this treatment effect is statistically significant at the 1% level and of similar magnitude for all four types (not shown in tables). Column 3 shows that there is no effect on the risk premium Asians require to accept gambles.⁹ Columns 2 and 4 show, in analogous regressions for white subjects, that whites' choices are not affected by the prime.

Consistent with existing evidence on time preference (Frederick, Loewenstein, and O'Donoghue, 2002), we find that participants demand higher interest rates to defer smaller monetary amounts and lower interest rates to further defer an already-delayed payment. However, the latter effect is not statistically significant for Asians and only marginally significant for whites.¹⁰

III. Experiment 2: White and Black Racial Identities, and Gender Identity

Experiment 1 focused on Asian and white ethnic identities. We ran a second experiment to explore how preferences are related to white racial identity, black racial identity, and gender identity.¹¹ In order to recruit a sufficient number of black participants, we ran the experiment at Temple University, which has one of the largest black student populations in the United States outside of the historically black colleges.¹² Experiment 2 also expanded on the earlier experiment by measuring larger-stakes (in addition to small-stakes) risk preferences and by asking a host of

⁹ We do not place much importance on the finding that control Asians are more risk averse and slightly more impatient than control whites. Many complicated selection effects, including Harvard's admissions process, the way we recruited subjects, and subjects' decisions to participate, confound these direct estimates. Only within-race comparisons between treatment and control groups are reliable for inference.

¹⁰ We also examine whether primed Asians who have been in the U.S. for one or fewer generations, who report speaking only an Asian language at home, or who list an Asian language first when asked what languages they know demand especially low interest rates. The point estimates suggest that primed Asians with the strongest ethnic affiliations are particularly patient, but only the "listing Asian language first" interaction is significant. We are hampered by our not having collected these affiliation strength data for the control group, preventing us from controlling for baseline preference differences associated with differing affiliation strength. We find no significant interaction between the white ethnic salience treatment and the number of generations the white subject's family has been in the U.S.

¹¹ Race is a distinct concept from ethnicity. A German-American's race would typically be white, whereas his ethnicity would be German.

¹² Running the experiment at an historically black college would have precluded our recruiting white subjects from the same population, and we were concerned that students at historically black colleges may be so saturated with their racial identity that a priming manipulation would have no additional effect.

questions that would enable us to test potential mechanisms underlying the identity salience effects. In addition, we introduced variation in the delay between the salience manipulation and the preference elicitation, which allows us to investigate the impulse response function of an identity salience shock. To our knowledge, we are the first to provide evidence on the duration of identity salience effects.

A. Participants

We recruited 280 Temple University students by handing out flyers on campus and providing a \$1 referral fee to participants for each friend they got to sign up for the experiment. At no point did we mention that we were looking for black and white subjects. There were 84 black subjects, 162 non-Hispanic white subjects, and 34 subjects who were neither black nor non-Hispanic white.

B. Procedure

We conducted 19 fifty-minute experimental sessions in Temple classrooms on March 18, 25, and 26, 2006. The smallest session had 6 participants, and the largest had 29.

In order to maximize statistical power for analyzing both the race-salience manipulation and the gender-salience manipulation, we studied gender salience only among non-black participants. Half of the black participants were assigned to the race-salience condition and half to the control condition. On March 18, half the non-black participants were assigned to the race-salience condition and half to the control condition. By the end of March 18, it became apparent that we had recruited a sufficient number of non-black subjects. Therefore, for non-black subjects participating in the March 25 and 26 sessions, we administered the gender-salience treatment to approximately half, the race-salience treatment to approximately a quarter, and the control treatment to approximately a quarter.¹³

The principal experimenter was a male of black, Mexican, and white descent. He was assisted by a white male and an Asian male. After the questionnaire booklet was distributed to

¹³ The reason the proportions are not exact is that we started out giving gender-salience treatments to 2/3 of the non-black subjects, the race-salience treatment to 1/6 of the non-black subjects, and the control treatment to 1/6 of the non-black subjects. When we realized that the number of gender-salience non-black subjects exceeded the number of race-salience non-black subjects, we switched to administering each treatment to 1/3 of the non-black subjects. In our final three sessions, we ran short of identity salience questionnaires because we had many more walk-in subjects than we had anticipated, so we distributed extra control questionnaires.

each participant, the principal experimenter guided session participants through the questionnaire together by reading instructions aloud before each section. The questionnaire was divided into sections (with the neutral labels “Section 1,” “Section 2,” and so on). The first section contained the identity-salience manipulation or control. The next three sections were a time preference elicitation (which took 5 minutes for instructions and responses), a risk preference elicitation (5 minutes), and a six-question version of the Spielberger State-Trait Anxiety Inventory (Marteau and Bekker, 1992) (1.5 minutes). These three sections’ order varied across sessions. The penultimate section was a six-question math quiz with SAT-like questions. The questionnaire’s final section asked a variety of questions about personal and family background, as well as questions unrelated to the study in order to mask its purpose.¹⁴ Each of the time and risk preference measures was incentive-compatible, as explained below. We also paid subjects 10 cents for each math question they answered correctly. Participants were paid for their choices, plus a \$1 show-up fee, by check immediately upon completing the experiment. In order to avoid contaminating future subjects, participants’ debriefing form did not reveal that our study was about racial and gender identities.¹⁵

Race-salience manipulation. In the race-salience condition, we adapted for race the questions that Shih, Pittinsky, and Ambady (1999) used to make gender salient. Specifically, we asked participants the following in the questionnaire’s first section:

- (a) Do you live on campus or off campus?
- (b) Do you have a roommate?
- (c) What is your race?
- (d) If you could live with any roommate you liked, would you prefer to live with a roommate of your own race or a different race?
- (e) Please list three advantages of having a roommate of your own race.
- (f) Please list three advantages of having a roommate of a different race.

¹⁴ In addition to asking subjects to report their race and gender, we surreptitiously recorded most subjects’ race and gender during the experimental sessions. We relied on subjects’ self-reported race and gender except in one case where it seemed clear both from our visual observation and from other parts of the questionnaire that the subject had accidentally circled the wrong gender.

¹⁵ When all sessions were completed, we provided subjects a more complete debriefing via e-mail.

Gender-salience manipulation. In the gender-salience condition, the questions in the first section were nearly identical to those that Shih, Pittinsky, and Ambady (1999) used to make gender salient:

- (a) Do you live on campus or off campus?
- (b) Do you have a roommate?
- (c) What is your gender?
- (d) If you could live anywhere on campus, would you prefer living on a co-ed floor or a single-sex floor?
- (e) Please list three advantages of living on a co-ed floor.
- (f) Please list three advantages of living on a single-sex floor.

Control condition. In the control condition, the first section asked participants questions designed not to make either race or gender salient, but which followed a structure parallel to the race- and gender-salience questions:

- (a) Do you live on campus or off campus?
- (b) Do you have a roommate?
- (c) How old are you?
- (d) If you could live anywhere, would you prefer to live on campus or off campus?
- (e) Please list three advantages of living on campus.
- (f) Please list three advantages of living off campus.

Measured time preferences. We measured time preferences by asking participants to make two sets of 12 binary choices. In the first set of 12 questions, the participant was asked to circle either “(A) I prefer to get \$10 right now,” or “(B) I prefer to get X one week from now,” where $X =$ \$10.10, \$10.25, \$10.50, \$10.75, \$11.00, \$11.25, \$11.50, \$12.00, \$12.50, \$13, \$14, and \$15. The second set of 12 questions was the same as the first set, except that option (A) occurred “one week from now,” and option (B) occurred “two weeks from now.” These questions were presented with the delayed reward X in ascending order.¹⁶

¹⁶ We chose these X amounts based on participant responses in Experiment 1. We wanted to have finer resolution in portions of the interest rate or risk premium space where there was more response clustering, while sacrificing resolution where there were fewer responses in order to shorten the elicitation section.

The section's instructions gave two sample questions and explained that later during the experiment, a participant would roll a 24-sided die to determine which question would count for payment in that session. All payments would be made by check, and if on the chosen question the subject had selected the delayed payment, he would receive that delayed payment as a post-dated check. The experimenter told participants that post-dated checks can be cashed any time on or after the check's date.¹⁷ The final two sentences of the section's instructions made clear that the questions were not intended to evaluate performance: "It's important to keep in mind that there are no right or wrong answers here. Which choice you make is a matter of personal preference." (We used this same wording again in the instructions for both risk preference sections.)

Measured risk preferences. One section of the questionnaire measured risk preferences. This section was split into a portion measuring risk preferences over small stakes and a portion measuring risk preferences over larger stakes.

We elicited small-stakes risk preferences by asking participants to circle either "(A) I get \$1 for sure," or "(B) If the six-sided die comes up 1, 2, or 3, I get X . If the six-sided die comes up 4, 5, or 6, I get nothing." We asked six such questions, where $X = \$1.60, \$2, \$2.40, \$2.80, \$3.20,$ and $\$3.60$. The questions were presented in ascending order of X .

The small-stakes section's instructions gave a sample question and told participants that they would be paid according to *every* choice they made in the small-stakes risk section. Later during the experiment, a participant would roll a six-sided die to determine the outcomes of each question's gamble. Any money the participant earned in this section would be paid with a check that could be cashed immediately.

The larger-stakes risk section choices were analogous, except that the monetary amounts were multiplied by 100. For example, the first question gave a choice between "(A) I get \$100 for sure," and "(B) If the six-sided die comes up 1, 2, or 3, I get \$160. If the six-sided die comes up 4, 5, or 6, I get nothing." The section's instructions explained that we would pay a participant for a randomly selected question in the section *if* the participant could correctly guess in

¹⁷ If participants received a delayed payment, then they also received a separate check with the immediately cashable portion of their payment. If we exclude from our discounting regressions the 15 subjects who deposited their checks more than one business day before the check's date, our results are unchanged.

sequence two roulette wheel spin outcomes which would take place later in the session.¹⁸ Participants submitted written predictions before answering this section's questions. (No one correctly predicted both spins.) The instructions presented a sample question and told the participants that any money earned in this section would be paid by an immediately cashable check.

Self-reported anxiety. The Spielberger State-Trait Anxiety Inventory (STAI) is a standard forty-question psychometric measure of anxiety. We administered the shortened version of the STAI developed by Marteau and Bekker (1992): six questions that ask participants to rate on a four-point numerical scale how much six statements described how they feel “*right now, at this moment.*” They are told that there are no right or wrong answers, and that they should not spend too much time on any one statement. The statements are the following:

- (a) I feel calm.
- (b) I am tense.
- (c) I feel upset.
- (d) I am relaxed.
- (e) I feel content.
- (f) I am worried.

The numerical sum of (a), (d), and (e) answers are subtracted from the sum of (b), (c), and (f) answers to compute an anxiety score.

Math quiz. We gave participants eight minutes to answer six questions similar to those found on the SAT Math exam. The instructions told participants that unlike the previous preference questions, these math questions did have right answers. For each question they answered correctly, 10 cents would be added to the check that they could cash immediately.

Background questions. The last section subjects completed was a background questionnaire that also included questions unrelated to the study to disguise the study's purpose.

¹⁸ Since each roulette wheel spin has 38 possible outcomes, the probability that a participant would be paid for his or her choice was $(1/38)^2 = 1/1444$.

In this section, we asked about the credibility of our payment promises. The first question asked, “Throughout this experiment, you made choices that involved various amounts of money. We said that your responses would affect how much you get paid, but you may not have believed us. Did you believe that your responses would affect how much you get paid?” We drop from our analysis participants who did not circle the answer, “I believed that my responses would matter, exactly as the questionnaire said.”

The second question asked, “Think back to when you were answering questions about getting a certain amount of money today versus getting some different amount of money in a week. Did you believe that you would actually get paid in a week if you chose to take the money in a week?” For our analysis of time preference, we additionally drop participants who did not circle the answer, “I believed that I would get paid in a week if I chose to take the money in a week, as the questionnaire said.”

We asked about the participant’s race, gender, and/or age in the final section if we did not ask about them in the priming section. We also asked in what countries they and their parents were born.

Finally, we asked a series of questions about participants’ beliefs regarding how common various stereotypes were about their race or gender, how strongly the participant identified with his or her race and gender, and how much the participant agreed with four traditional gender role statements. We will discuss these questions further in Section III.E.

C. Econometric Methodology

For intertemporal choices to be called well behaved, a participant has to choose the earlier payment if and only if the later payment is less than some threshold. Similarly, risk preferences are well behaved if a participant chooses the safe payout if and only if the gamble’s expected value is less than some threshold. Respectively, 94% and 87% of participants answered the intertemporal questions and the risk preference questions in a well-behaved manner.

As in Experiment 1, our dependent variables are $\log(r)$ (the log of the lowest interest rate that induced subjects to choose the later payment) and π (the lowest risk premium that induced subjects to choose the gamble), and we use interval regressions for our estimations. We use the lowest interest rate and risk premium that induce the subject to choose the delayed or risky payout as our dependent variable because those are the prices at which transactions are likely to

occur in a market where many profit-maximizing firms are competing for the subjects' investment dollars. Our main results are generally unchanged if, in an OLS regression, we use as the dependent variable the total number of delayed choices or risky choices, measures that do not depend on well-behaved choices.¹⁹

We observe two r values and two π values for each participant. In the regressions reported below, we pool the two r values or the two π values and add dummy independent variables that indicate in which choice situation (now versus one week, one week versus two weeks, small gamble, large gamble) the r or π was observed. Standard errors are clustered by individual. We note in the text whenever there are interesting divergences between the pooled regressions and the regressions run separately by choice situation.

In our risk-preference regressions, we drop participants who reported not believing their choices would affect how much they got paid, In our time-discounting regressions, we also drop participants who reported not believing they would receive promised delayed payments,²⁰ For the race-salience analysis, we drop participants who were not non-Hispanic white or black.

D. Main Results

Table 2 shows summary statistics for the Temple undergraduate sample.²¹ Because the identity-salience manipulations were randomly assigned, there should not be systematic differences between participants across experimental conditions. Statistical comparisons reported in Table 2 confirm that participants generally appear similar across conditions. The only significant difference is that the white race-salience group has somewhat lower SAT Math scores than the white control group. Our subsequent regression results are not affected by adding SAT Math score as a control. Importantly, there is no systematic difference in participants' propensity to believe our payment promises across conditions.

¹⁹ The only difference is that the risk-averse stereotype interaction with the gender-salience treatment loses significance but retains the same sign for women.

²⁰ If we include participants who did not believe our payment promises, the sign of our effects remain unchanged in the baseline regressions, but we lose statistical significance on all but the white priming effect on the required risk premium. These findings are consistent with skeptical participants' choices adding noise to the data.

²¹ Table 2 includes participants who did not believe our payment promises. These participants are excluded from the main analysis.

Table 3 presents the baseline results for whites, blacks, men, and women.²² The main result of Experiment 2 is that making race salient to whites causes them to become much more patient and much less risk averse. The median interest rate required to defer receipt of an immediate payment by one week falls from 10.2% to 3.9%. The priming effect on interest rates is of similar magnitude and statistical significance when separately analyzing choices over immediate versus one-week-deferred payment receipt and one-week-deferred versus two-week-deferred payment receipt (not shown in the tables).

The effect of identity salience on whites' risk preferences is also large. The mean expected return premium required to take a 50-50 gamble instead of \$1 for sure falls from 24.3% to 10.7%. This pooled estimate is marginally statistically significant; the risk aversion effects are concentrated in the smaller-stakes gambles—choices between \$1 for sure or a 50% chance for \$X—where the effect is statistically significant at the 5% level. The effect for larger-stakes gambles—choices between \$100 for sure or 50% chance for \$100X—is one-third smaller and is not statistically significant. (These separate regressions are not reported in the tables.)

Sociological research indicates that blacks whose families have long U.S. roots grow up with a very different cultural heritage than blacks whose families have recently immigrated to the U.S. (e.g., Waters, 1994). Immigrant blacks are also over-represented among the black economic elite (Sowell, 1975; Rimer and Arenson, 2004).²³ For this reason, when analyzing the race priming effect on blacks, it is important to interact the treatment with an immigrant dummy, which we set to 1 if at least one parent or the subject was born abroad. In our sample of blacks, 25% are immigrants.

We find no significant identity salience effect on patience for blacks. The point estimate for the black priming effect is much smaller than for the white priming effect, although our large standard errors prevent us from statistically rejecting equality with the white priming effect (two-

²² We find some demographic variation in how choices respond to time horizons and gamble sizes (within experimental condition). Whites and men in our sample are more patient when making choices between two future payments than when making choices between an immediate payment or a delayed payment, although this difference is not statistically significant for whites. In contrast, blacks and women in our sample are less patient about two future trade-offs than about choices between an immediate payment or a delayed payment, with neither difference being statistically significant. Consistent with Kachelmeier and Shehata (1992) and Holt and Laury (2002), whites and men become more risk-averse when confronted with larger experimental gambles. However, this effect is not statistically significant in blacks, and women show a statistically insignificant opposite effect.

²³ Of course, blacks who successfully immigrate to the U.S. may be selected to be relatively wealthier and better educated. The norms and stereotypes activated by the identity salience manipulation are likely to be related to the immigrant black culture in the U.S., rather than to the prevalent culture in the country of origin.

sided $p = 0.20$ for non-immigrants, 0.32 for immigrants). We do find that non-immigrant blacks become significantly more risk averse, however. Their average required risk premium increases from 7.4% to 23.7%.²⁴ Immigrant blacks react quite differently. Their required risk premium *falls* by 12.4 percentage points (from 11.6% to -0.8%), causing them to become slightly risk-seeking. The difference between immigrant and non-immigrant blacks' risk reaction is statistically significant at the 5% level. However, we cannot reject the hypothesis that the priming effect on immigrant blacks' risk aversion is zero. Examining the choice types separately, it appears that the non-immigrant black priming effect on risk aversion is concentrated in choices over large-stakes gambles (unlike among whites).

Because we varied the order of the time preference elicitation, risk preference elicitation, and anxiety scale sections across experimental sessions, we can gain some insight into how quickly our priming effects decayed. Time preference elicitations began approximately 0, 2, or 7 minutes after the end of the priming section. Risk preference elicitations began approximately 0, 5, or 7 minutes after the priming section. All elicitations were completed approximately 12 minutes after the priming section.

Patterns in the treatment effect point estimates suggest that the impulse response to a single identity salience shock does decay over time—perhaps dissipating completely after 12 minutes—although few of our priming effects differ in a statistically significant manner across questionnaire orders due to sample size limitations. (In the real world, of course, people are exposed to a constant flow of identity salience shocks, so the relatively swift decay of a single shock's effect does not imply that identity effects are trivial in practice.) For whites, the median interest rate required to defer an immediate payment by one week drops by 9.5 percentage points when time preferences are elicited immediately after the prime. When 2 or 7 minutes separate the prime from the start of the time preference elicitation, the treatment effect is still present but attenuated (-4.2 percentage points in both cases). (Results not shown in tables.) For non-immigrant blacks, the prime-induced increase in the risk premium drops from 21.7 to 20.8 to 8.0 percentage points as 0, 5, or 7 minutes intervene between the prime and the start of the risk preference elicitation. Similarly, for immigrant blacks, the prime-induced effect on the risk

²⁴ As in Experiment 1, we do not draw any inferences from the fact that our control whites are more impatient and risk averse than our control blacks. Because it is likely that different selection criteria determined whether a black subject chose to participate than a white subject, only comparisons between treatment and control subjects within each demographic group are reliable.

premium goes from -19.6 to $+5.1$ to $+2.5$ (both effectively zero) percentage points. Although in general we find monotonic decays, the risk premium effect for whites decays non-monotonically—from -25.5 to $+3.1$ to -23.5 percentage points—as the prime becomes more distant from the risk preference elicitation.

Finally, we find no mean effect of priming gender identity for men and women. However, we will see in Section III.E that there are significant interaction effects between gender priming and stereotypes subjects believe are common about their gender.

E. Priming Mechanisms and Mediators

In this subsection, we provide some additional evidence on the mechanisms behind our priming results.

Conformance to perceived stereotypes. Self-categorization theory predicts that expressed preferences will move toward the preferences prescribed for the currently salient identities. If there is heterogeneity in what people perceive those prescriptions to be, then making a given identity salient will result in different preference responses. Here, we test the effect of heterogeneity in perceptions about one potential source of prescriptions: societal stereotypes. It is often asserted, for example, that stereotypes about Asian math ability or black athletic ability push members of those races towards math or sports. Gender stereotypes may inform a person's notion of what an idealized man or woman is.

In the questionnaire's final section, we asked participants how common (on a six-point scale from "extremely uncommon" to "extremely common") they thought the following stereotypes were about their *own* race or gender: generous, lazy, frugal, impatient, studious, cautious, artistic, patient, and reckless. If we assume that these numerical ratings are cardinal, then we can compare stereotypes across groups. We find that white participants on average rated whites as more frugal, more patient, more cautious, and less reckless (Mann-Whitney tests, all $p < 0.05$), as well as less impatient ($p > 0.05$, not significant) than black participants rate blacks. Compared to female participants, male participants rated their own sex as more impatient, less patient, less cautious, more reckless (Mann-Whitney tests, all $p < 0.01$), and slightly more frugal ($p > 0.05$, not significant).

For the analysis that follows, we calculate for each participant a patient stereotype belief index pertaining to his or her own race (or gender) by adding the participant’s numerical rating of “patient” and “frugal,” subtracting the “impatient” rating, and standardizing the resulting variable to have mean zero and unit variance within the race or gender group. We create an analogous index for risk-averse stereotypes by subtracting the participant’s rating of “reckless” from the rating of “cautious” and standardizing.

We regress the required log interest rate or risk premium on a constant, a treatment dummy, a stereotype index, the interaction between the treatment dummy and that stereotype index, and a trade-off type dummy. The primary coefficients of interest are the interaction effects of stereotype beliefs with the salience treatments.

Taken as a whole, the results suggest that racial stereotypes do not mediate the race-salience treatment effects. As discussed previously, priming race reduces white risk aversion, even though whites rate their race as *more* stereotypically risk-averse (than blacks rate their own race). Focusing on heterogeneity within the white sample, we see in column 1 of Table 4’s Panel A that the interaction of the patient stereotype index goes in the hypothesized direction—i.e., belief that patient stereotypes about whites are more common causes the race-salience treatment to decrease the required interest rate by more—but the coefficient is insignificant. Similarly, although column 4 of Table 4’s Panel A suggests that believing risk-averse stereotypes about whites are more common attenuates the race-salience treatment on the risk premium, the interaction is not statistically significant. For blacks (Table 4, Panel B, columns 1 and 4), the stereotype belief interactions both go in opposition to the hypothesized direction.

Unlike for race identity, stereotype conformance appears to play an important role for gender identity. Among both men and women, those who believe risk-averse stereotypes about their gender are relatively more common become more risk-averse in response to the gender prime (Column 4 of panels C and D in Table 4). The opposite effect occurs for those who believe risk-averse stereotypes about their gender are relatively less common. The size of this interaction effect is large: a one standard deviation increase in the risk-averse stereotype index is associated with a 19.2 percentage point increase in the gender prime’s risk premium effect among men and an 18.2 percentage point increase among women.²⁵ Separately analyzing the components of the

²⁵ The mean priming effect in the interaction specification is a 5 percentage point decrease for men and a 3 percentage point decrease for women, neither of which is statistically significant.

risk-averse stereotype index, we find that these effects are driven by beliefs about the “cautious” stereotype for men (significant at the 1% level) and the “reckless” stereotype for women (significant at the 5% level). (These stereotype component results are not shown in tables.)

This interaction effect also decays over time. Examining the size of the “cautious” standardized stereotype interaction for men and “reckless” standardized stereotype interaction for women,²⁶ we find that the coefficient goes from 27.2 to 31.4 to 1.8 percentage points for men and from 23.5 to 19.4 to 2.4 percentage points for women as 0, 5, or 7 minutes intervened between the end of the gender prime and the start of the risk preference elicitation.²⁷ (Not shown in tables.) The difference between the interaction effects when 0 versus 7 minutes separated the prime and the elicitation is significant at the 5% level for both men and women.

Stereotype threat, lift, and emotional states. Many researchers have documented the “stereotype threat” phenomenon: making racial or gender identity salient impairs the cognitive performance of groups with stereotypically poor performance (e.g., Steele and Aronson, 1995; Shih, Pittinsky, and Ambady, 1999). Walton and Cohen (2003) present evidence of a “stereotype lift” effect: making negative stereotypes about *other* groups salient improves cognitive performance. It is believed that stereotype threat and lift effects operate through increasing or reducing anxiety that one will confirm negative stereotypes about one’s group. Consistent with this mechanism, these effects dissipate when tasks are presented to subjects as not being diagnostic of ability (see also Croizet and Claire, 1998; Aronson, Quinn, and Spencer, 1998; Kray, Thompson, and Galinsky, 2001).

A possible explanation for our results is that the identity primes induced stereotype lift among Asians in Experiment 1 and whites and immigrant blacks in Experiment 2, improving their ability to compute expected values and interest rates. Conversely, the identity primes may have induced stereotype threat among non-immigrant blacks in Experiment 2. We think this explanation is unlikely because we did not present the preference elicitation questions as being diagnostic of ability. In Experiment 2, we were explicit in telling subjects that there are no right or wrong answers for the preference elicitation questions.

²⁶ We are focusing on the gender-specific components of the risk-averse stereotype index that drove the overall interactions in order to maximize statistical power.

²⁷ Recall that these interaction coefficient represent how much the gender-salience effect changes when belief about the stereotype’s prevalence changes by one standard deviation.

Even if stereotype threat and lift effects on cognitive ability were not present, it is possible that the priming questions induced changes in subjects' emotional states which affected their expressed preferences. For example, if certain priming questions agitated subjects, their willingness to delay payment receipt or take risks may change (Loewenstein, 2000).

To check that our results were not being driven by stereotype threat and lift or emotional changes, we examine how the treatment affected performance on five SAT Math-like questions administered after the elicitations and responses to a shortened version of the Spielberger State-Trait Anxiety Index (a standard psychometric measure of anxiety).²⁸ The first two columns of Table 5 show that the primes had no effect on math quiz performance for whites, blacks, and women. Anxiety for blacks, men, and women is also unaffected. The race prime seems to have increased anxiety scores by 0.5 standard deviations among whites, and men who believe risk-averse stereotypes about their gender are relatively more (less) common perform slightly worse (better) on the math quiz when primed. However, controlling for anxiety, SAT math score, and math quiz score only increases the magnitude and statistical significance of the white priming effects (Table 4, Panel A, Columns 2 and 5) and the interaction effect between the prime and belief about risk-averse gender stereotypes for male risk premia (Table 4, Panel C, Column 5).

Identity strength. Theoretically, it is not clear whether a given identity-salience manipulation should have a stronger or weaker effect if the primed identity is one that the individual affiliates with more strongly. On the one hand, increasing the salience of a more strongly held identity by one unit should have a larger effect on preferences. On the other hand, it may be more difficult for a prime to increase a strongly held identity's salience because the identity is likely to already be salient. In addition, identity strength interaction estimates may be confounded if people with a strong affiliation to an identity have a systematically different conception of what that identity's prescriptions are than people with a weak affiliation to the identity.

In the context of socialite, scholar, or family-oriented identities, LeBoeuf and Shafir (2005) find that behavior changes more towards the identity's prescriptions in response to a

²⁸ Although our priming effects appeared to largely dissipate after 12 minutes, stereotype threat and lift effects have been shown to be more persistent. Blascovich et al. (2001) report that blacks in stereotype-threat conditions exhibit elevated blood pressure, and this elevation shows no signs of attenuation even 16 minutes after the prime (when their measurements end). Similarly, whites exhibit lower blood pressure up to 16 minutes after the prime. Therefore, if stereotype threat and lift were present in our experiment, we would expect to see some of their effects in our math quiz.

prime if the identity is an important part of the subject's self-concept. Keeping the caveats outlined above in mind, we look to see if we find a similar interaction in our experiment.

For racial identity, we asked participants in the questionnaire's final section how much they agreed (on a six-point scale from "strongly agree" to "strongly disagree") with each of the following statements:

- (a) My racial identity is an important part of my self-image.
- (b) My racial identity is an important reflection of who I am.
- (c) My racial identity has very little to do with how I feel about myself.
- (d) My racial identity is unimportant to my sense of what kind of person I am.

For gender identity, the questions were analogous, but we substituted "being a woman/man" for "my racial identity" in the statement text. These questions are taken from the "private collective self-esteem subscale" (Luhtanen and Crocker, 1992), a standard psychological instrument for measuring identity affiliation.

We assign a value from 1 to 6 to the responses to each statement, where 6 corresponds to the response indicating the highest degree of identification. We sum the race (or gender) responses and standardize this race (or gender) identification variable to be mean zero and variance one within each regression we run.

Although responses to these identity strength questions are not generally thought to be affected by momentary primes, our evidence hints that they might be. Column 3 of Table 5 shows that the gender-salience treatment increases women's identity strength score by 0.5 standard deviations. In addition, the gender-salience treatment decreases the identity strength score of men who believe risk-averse stereotypes about men are relatively common. The point estimates of the priming effect on racial identification strength are positive for whites and immigrant blacks, but insignificant. Since the identity identification occurred near the end of the questionnaire, this evidence might suggest that priming has longer-lasting effects on self-reported identity identification than on choices, perhaps because answering identity-related questions retriggers earlier primes.

We find a number of statistically significant interactions between identity strength and priming effects, but no clear patterns, except perhaps for immigrant blacks. For men, stronger gender identification greatly attenuates the interaction effect between belief about risk-averse stereotypes and the priming effect (Table 4, panel C, column 6), and primed men with stronger

gender identification scores become more risk averse on average. On the other hand, there are no significant interaction effects for women (Table 4, panel D, columns 3 and 6). For whites, although we find that stronger racial identification magnifies the race-salience effect on the required risk premium (Table 4, panel A, column 6), the interaction for time preference is insignificant and goes in the opposite direction (Table 4, panel A, column 3). For native blacks, there appear to be no interaction effects. However, immigrant blacks with strong racial identification become more impatient in response to the prime, and their risk premium response is (insignificantly) closer to native blacks' (Table 4, panel B, columns 3 and 6). Interestingly, this last finding may be consistent with the ethnography of Waters (1994), who reports that second-generation immigrant blacks who feel stronger racial solidarity are those who have assimilated more thoroughly into native black American culture and hence feel that the rewards for individual effort and initiative are low.

Traditional gender roles. Societal prescriptions for identities can come in the form not only of stereotypes, but also in the form of explicit normative expectations. To see if subjects' attitudes towards traditional gender roles influenced the gender-salience effect, we asked subjects to indicate their agreement (on a six-point scale from "strongly disagree" to "strongly agree") with four statements about traditional gender roles:

- (a) The man should always pay for the first date between a man and a woman
- (b) A pre-school child is likely to suffer if his/her mother works outside the home.
- (c) Men shouldn't cry.
- (d) Ultimately, the husband is responsible for making sure the family is financially secure.

Statement (b) is taken from the 1970 National Fertility Study. We formulated the other statements based on introspection about which gender role statements would evoke both substantial agreement and disagreement among college students today.

We assign a value from 1 to 6 for the response to each statement, with 6 corresponding to the greatest agreement with the traditional gender role. We sum the responses and standardize this traditional gender role variable to be of mean zero and unit variance within each gender. We find no significant interactions between agreement with traditional gender roles and the gender treatment (not shown in tables).

IV. Conclusion

Our findings suggest that culture matters for economic behavior not only because it affects trust and political institutions (Guiso, Sapienza, and Zingales, 2006), but also because it influences fundamental preferences. We found that priming Asian-American subjects' ethnic identity causes them to exhibit more patient preferences. We also found that priming racial identity among white subjects caused the white subjects to exhibit more patient and less risk-averse preferences. Priming racial identity among black subjects did not affect time preference, but it increased risk aversion among those who had longstanding roots in the U.S. and may have decreased risk aversion among those who had at least one parent born abroad. Priming gender identity appears to cause both men and women to behave more closely in accordance with risk stereotypes they hold about their own gender. Overall, our results are broadly supportive of the view that preferences are influenced by the interaction between internal identity affiliations, cultural norms and stereotypes, and environmental identity primes.

In our experiments, we varied identity primes exogenously in order to begin to understand the relationship between social identity and preferences. Of course, in actual markets, interested parties such as sellers, employers, churches, and governments have an incentive to manipulate the identity primes that individuals are exposed to. To the extent that an individual can control which of these primes affect behavior by "investing" in different identity affiliations (Bénabou and Tirole, 2006; Becker and Mulligan, 1997), an individual will in turn have an incentive to shape his or her own identities. These possibilities suggest that the process by which preferences are determined and expressed in markets may be richer than economists have traditionally imagined.

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**Table 1. Ethnic-Saliency Treatment Effect on
Asian and White Log Interest Rate and Risk Premium**

This table presents interval regressions for Asians and whites in Experiment 1, where the dependent variable is the log interest rate required to defer payment receipt or the risk premium required to accept a gamble. We pool each subject's four intertemporal choices. *Ethnicity Saliency* is a dummy for the subject received the ethnicity-saliency treatment. *1 Week vs. 2 Weeks* is a dummy for if the intertemporal choice was between payments deferred for one week versus two weeks. *Larger Stakes* is a dummy for if the earlier payout in the intertemporal choice was \$7. σ is the conditional standard deviation of the dependent variable. Standard errors appear in parentheses below the point estimates. Huber-White standard errors, clustered by subject, are reported for the log interest rate regressions.

	Log interest rate		Risk premium	
	Asians (1)	Whites (2)	Asians (3)	Whites (4)
<i>Ethnicity Salient</i>	-1.4165*** (0.3783)	0.4220 (0.3713)	-0.0336 (0.0704)	-0.0210 (0.0662)
<i>1 Week vs. 2 Weeks</i>	-0.0605 (0.1560)	-0.3272* (0.1796)		
<i>Larger Stakes</i>	-0.3909*** (0.1006)	-0.5592*** (0.1269)		
<i>Larger Stakes</i> × <i>(1 Week vs. 2 Weeks)</i>	-0.0584 (0.1512)	0.0887 (0.1773)		
Constant	-2.4322*** (0.2448)	-2.7841*** (0.3110)	0.2060*** (0.0509)	0.0887** (0.0440)
σ	1.6360 (0.1352)	1.6461 (0.1456)	0.2918 (0.0283)	0.2652 (0.0250)
<i>N</i>	284	264	71	66

* Significant at the 5% level. ** Significant at the 5% level. *** Significant at the 1% level

Table 2. Summary Statistics for Experiment 2 Subjects

This table provides summary statistics for the participants in each experimental condition in Experiment 2. We use two-sample *t*-tests to identify differences in means between control and treatment groups, and Pearson chi-square tests to test for differences in proportions. The *p*-values for each test statistic are reported in the last column. “Believed Choices Mattered” is the percent of subjects who reported believing experimental choices would affect payments. “Also Believed Deferred Payment” is the percent of subjects who also reported believing that deferred payment promises were credible.

	Mean	Std. Dev.	Mean	Std. Dev.	<i>p</i> -value
<i>Whites</i>					
	<i>Control Condition</i>		<i>Race-Salience Condition</i>		
Age	19.6	1.5	19.7	1.2	0.742
SAT I Math Score	607.1	79.4	569.6	63.5	0.011**
SAT I Verbal Score	600.9	75.9	602.4	72.1	0.917
Household Income > \$80,000	60.0%		51.0%		0.348
Believed Choices Mattered	86.7%		83.7%		0.660
Also Believed Deferred Payment	65.0%		55.1%		0.293
<i>N</i>	60		49		
<i>Blacks</i>					
	<i>Control Condition</i>		<i>Race-Salience Condition</i>		
Age	19.4	1.3	21.0	5.9	0.107
SAT I Math Score	541.1	80.6	515.1	87.6	0.186
SAT I Verbal Score	540.3	66.4	571.4	101.1	0.120
Household Income > \$80,000	30.0%		33.3%		0.746
Believed Choices Mattered	77.5%		74.4%		0.743
Also Believed Deferred Payment	55.0%		55.8%		0.941
<i>N</i>	40		43		
<i>Men</i>					
	<i>Control Condition</i>		<i>Gender-Salience Condition</i>		
Age	19.9	1.4	19.8	3.4	0.960
SAT I Math Score	605.2	100.3	596.5	78.2	0.672
SAT I Verbal Score	594.2	82.5	592.6	76.1	0.933
Household Income > \$80,000	54.9%		52.8%		0.845
Believed Choices Mattered	82.4%		75.0%		0.404
Also Believed Deferred Payment	62.7%		50.0%		0.236
<i>N</i>	51		36		
<i>Women</i>					
	<i>Control Condition</i>		<i>Gender-Salience Condition</i>		
Age	19.3	1.4	19.2	1.3	0.764
SAT I Math Score	569.1	76.5	555.9	62.0	0.471
SAT I Verbal Score	570.7	73.0	576.8	83.3	0.749
Household Income > \$80,000	43.6%		46.2%		0.822
Believed Choices Mattered	82.3%		74.1%		0.377
Also Believed Deferred Payment	59.7%		51.9%		0.493
<i>N</i>	62		27		

** Significant at the 5% level.

Table 3. Baseline Identity-Salience Treatment Effects in Experiment 2

This table presents interval regressions for whites, blacks, men, and women in Experiment 2, where the dependent variable is the log interest rate required to defer payment receipt or the risk premium required to accept a gamble. We pool each subject's two intertemporal choices together, and each subject's two risk choices together. *Race Salient* and *Gender Salient* are dummies for whether the subject received the race- or gender-salience treatment. *Immigrant* is a dummy for if the subject or at least one parent was born abroad. *1 Week vs. 2 Weeks* is a dummy for if the intertemporal choice was between payments deferred for one week versus two weeks. *Large Stakes* is a dummy for if the sure payout in the risky choice was \$100. σ is the conditional standard deviation of the dependent variable. Huber-White standard errors, clustered by subject, are reported for below the point estimates.

	Log interest rate		Risk premium	
	<i>Whites</i>	<i>Blacks</i>	<i>Whites</i>	<i>Blacks</i>
<i>Race Salient</i>	-0.9669** (0.4420)	-0.2396 (0.3657)	-0.1356* (0.0700)	0.1626* (0.0889)
<i>Race Salient</i> × <i>Immigrant</i>		-0.1555 (0.5226)		-0.2866** (0.1366)
<i>Immigrant</i>		-0.4739 (0.3870)		0.0421 (0.1070)
<i>1 Week vs. 2 Weeks</i>	-0.1935 (0.1463)	0.0139 (0.1947)		
<i>Large Stakes</i>			0.3097*** (0.0494)	0.0777 (0.0573)
Constant	-2.2813*** (0.2409)	-1.9158*** (0.2610)	0.2430*** (0.0458)	0.0742 (0.0708)
σ	1.6973 (0.1801)	1.0807 (0.1339)	0.3961 (0.0279)	0.3428 (0.0385)
<i>N</i>	134	92	188	126
	<i>Men</i>	<i>Women</i>	<i>Men</i>	<i>Women</i>
<i>Gender Salient</i>	-0.3562 (0.3845)	-0.4170 (0.5145)	-0.0451 (0.0911)	-0.0523 (0.0831)
<i>1 Week vs. 2 Weeks</i>	-0.5004** (0.2199)	0.2695 (0.2056)		
<i>Large Stakes</i>			0.2638*** (0.0648)	0.0722 (0.0555)
Constant	-1.8226*** (0.2243)	-2.7677*** (0.2785)	0.2123*** (0.0548)	0.2075*** (0.0489)
σ	1.4367 (0.1992)	1.6375 (0.1822)	0.4346 (0.0347)	0.3827 (0.0321)
<i>N</i>	100	102	138	142

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Table 4. Mechanisms and Mediators of Identity-Salience Effects in Experiment 2

This table presents interval regressions for whites, blacks, men, and women in Experiment 2, where the dependent variable is the log interest rate required to defer payment receipt or the risk premium required to accept a gamble. We pool each subject's two intertemporal choices together, and each subject's two risk choices together. *Race Salient* and *Gender Salient* are dummies for whether the subject received the race- or gender-salience treatment. *Immigrant* is a dummy for if the subject or at least one parent was born abroad. *1 Week vs. 2 Weeks* is a dummy for if the intertemporal choice was between payments deferred for one week versus two weeks. *Large Stakes* is a dummy for if the sure payout in the risky choice was \$100. σ is the conditional standard deviation of the dependent variable. *Female* is a gender dummy. All the variables below are standardized to have zero mean and unit variance within each regression. *Race Patient Stereotypes* and *Gender Patient Stereotypes* are the extent to which the subject believes "patient" and "frugal" stereotypes are common and "impatient" stereotypes are uncommon about his or her race or gender. *Race Risk-Averse Stereotypes* and *Gender Risk-Averse Stereotypes* are the extent to which the subject believes "cautious" stereotypes are common and "reckless" stereotypes are uncommon about his or her race or gender. *Race ID Strength* and *Gender ID Strength* are the importance of race and gender to the subject's identity. *Math Quiz Score* is the number of math quiz questions answered correctly. *Anxiety* is the subject's self-reported anxiety (STAI). Huber-White standard errors, clustered by subject, are reported for below the point estimates.

Panel A: Whites						
	Log interest rate			Risk premium		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Race Salient</i>	-0.9295** (0.4407)	-1.2673*** (0.4111)	-0.9736** (0.4459)	-0.1292* (0.0668)	-0.1622** (0.0748)	-0.1115 (0.0717)
<i>Race Salient</i> × <i>Race Patient Stereotypes</i>	-0.0745 (0.5185)					
<i>Race Salient</i> × <i>Race Risk-Averse Stereotypes</i>				0.0647 (0.0747)		
<i>Race Salient</i> × <i>Race ID Strength</i>			0.0412 (0.4637)			-0.1307* (0.0781)
<i>Race Patient Stereotypes</i>	-0.2556 (0.2705)					
<i>Race Risk-Averse Stereotypes</i>				0.0568 (0.0629)		
<i>Race ID Strength</i>			0.0737 (0.3013)			0.0075 (0.0597)
<i>SAT Math Score</i>		-0.1343 (0.3310)			-0.0171 (0.0524)	
<i>Math Quiz Score</i>		0.3296 (0.2347)			0.0375 (0.0461)	
<i>Anxiety</i>		-0.1271 (0.2085)			0.0262 (0.0387)	
<i>1 Week vs. 2 Weeks</i>	-0.1890 (0.1458)	-0.2277 (0.1668)	-0.1925 (0.1461)			
<i>Large Stakes</i>				0.3107*** (0.0496)	0.2939*** (0.0519)	0.3101*** (0.0494)
Constant	-2.2630*** (0.2343)	-2.2099*** (0.2352)	-2.2767*** (0.2464)	0.2396*** (0.0451)	0.2574*** (0.0478)	0.2432*** (0.0477)
σ	1.6772 (0.1779)	1.6853 (0.1930)	1.6953 (0.1799)	0.3857 (0.0267)	0.3962 (0.0282)	0.3882 (0.0262)
<i>N</i>	134	120	134	188	168	188

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Panel B: Blacks						
	Log interest rate			Risk premium		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Race Salient</i>	-0.2146 (0.3290)	-0.4967 (0.3655)	-0.1415 (0.3515)	0.1740** (0.0873)	0.1920** (0.0951)	0.1613* (0.0879)
<i>Race Salient</i> × <i>Immigrant</i>	-0.1663 (0.5174)	-0.0765 (0.5407)	-0.1436 (0.5125)	-0.3057** (0.1297)	-0.3469** (0.1409)	-0.2911** (0.1374)
<i>Race Salient</i> × <i>Race</i> <i>Patient Stereotypes</i>	0.4110 (0.2518)					
<i>Race Salient</i> × <i>Race</i> <i>Risk-Averse Stereotypes</i>				-0.0075 (0.0843)		
<i>Race Salient</i> × <i>Race ID Strength</i>			-0.4805 (0.3175)			-0.0246 (0.0928)
<i>Race Salient</i> × <i>Race ID Strength</i> × <i>Immigrant</i>			1.1525*** (0.4325)			0.0859 (0.1257)
<i>Immigrant</i>	-0.4297 (0.3723)	-0.1086 (0.3375)	-0.4227 (0.3838)	0.0299 (0.1011)	0.0632 (0.1046)	0.0411 (0.1114)
<i>Race Patient</i> <i>Stereotypes</i>	-0.1465 (0.1488)					
<i>Race Risk-Averse</i> <i>Stereotypes</i>				0.0754 (0.0707)		
<i>Race ID Strength</i>			0.4906** (0.2034)			-0.0384 (0.0671)
<i>Race ID Strength</i> × <i>Immigrant</i>			-0.6513** (0.3194)			0.0405 (0.0857)
<i>SAT Math Score</i>		-0.0948 (0.1166)			0.0506 (0.0596)	
<i>Math Quiz Score</i>		0.0189 (0.1572)			0.0318 (0.0475)	
<i>Anxiety</i>		0.4040*** (0.1126)			0.0511 (0.0328)	
<i>1 Week vs. 2 Weeks</i>	0.0140 (0.1944)	0.0048 (0.2151)	0.0178 (0.1956)			
<i>Large Stakes</i>				0.0733 (0.0591)	0.0889 (0.0621)	0.0777 (0.0575)
Constant	-1.9386*** (0.2374)	-2.0366*** (0.2212)	-2.0153*** (0.2088)	0.0842 (0.0732)	0.0427 (0.0712)	0.0760 (0.0709)
σ	1.0571 (0.1245)	1.0389 (0.1308)	1.0286 (0.1363)	0.3409 (0.0382)	0.3429 (0.0385)	0.3400 (0.0385)
<i>N</i>	92	84	92	122	114	126

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Panel C: Men						
	Log interest rate			Risk premium		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Gender Salient</i>	-0.2461 (0.3893)	-0.3922 (0.3731)	-0.2512 (0.4666)	-0.0462 (0.0843)	-0.0464 (0.0875)	-0.0652 (0.0729)
<i>Gender Salient</i> × <i>Gend. Patient Stereotypes</i>	0.4056 (0.3687)	0.3738 (0.3467)	0.6307 (0.4454)			
<i>Gender Salient</i> × <i>Gender Patient Stereotypes</i> × <i>Gender ID Strength</i>			-0.0057 (0.3893)			
<i>Gender Salient</i> × <i>Gend. Risk-Averse Stereotypes</i>				0.1917* (0.1054)	0.2329** (0.1068)	0.2070*** (0.0520)
<i>Gender Salient</i> × <i>Gender Risk-Averse Stereotypes</i> × <i>Gender ID Strength</i>						-0.1850** (0.0827)
<i>Gender Salient</i> × <i>Gender ID Strength</i>			-0.7074 (0.4881)			0.1359** (0.0573)
<i>Gender ID Strength</i> × <i>Gender Patient Stereotypes</i>			-0.0312 (0.2420)			
<i>Gender ID Strength</i> × <i>Gender Risk-Averse Stereotypes</i>						-0.0280 (0.0751)
<i>Gender Patient Stereotypes</i>	-0.2359 (0.2517)	-0.1493 (0.2451)	-0.3149 (0.2629)			
<i>Gender Risk-Averse Stereotypes</i>				-0.0062 (0.0718)	-0.0328 (0.0730)	0.0137 (0.0389)
<i>Gender ID Strength</i>			0.3529 (0.2673)			-0.0091 (0.0453)
<i>SAT Math Score</i>		0.1528 (0.1963)			0.0530 (0.0572)	
<i>Math Quiz Score</i>		-0.0831 (0.2137)			-0.0106 (0.0528)	
<i>Anxiety</i>		-0.0513 (0.2185)			-0.0222 (0.0400)	
<i>1 Week vs. 2 Weeks</i>	-0.4987** (0.2188)	-0.5210** (0.2313)	-0.4925** (0.2181)			
<i>Large Stakes</i>				0.2620*** (0.0649)	0.2443*** (0.0626)	0.2617*** (0.0649)
Constant	-1.8805*** (0.2629)	-1.7355*** (0.2386)	-1.8587*** (0.2705)	0.2117*** (0.0540)	0.2137*** (0.0551)	0.2238*** (0.0528)
σ	1.4186 (0.1844)	1.3772 (0.1808)	1.3751 (0.1750)	0.4187 (0.0311)	0.4072 (0.0301)	0.3971 (0.0311)
<i>N</i>	100	94	100	138	132	138

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Panel D: Women						
	Log interest rate			Risk premium		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Gender Salient</i>	-0.2692 (0.5030)	-0.6753 (0.5272)	-0.1653 (0.4947)	-0.0259 (0.0842)	-0.0470 (0.1003)	-0.0021 (0.0885)
<i>Gender Salient</i> × <i>Gend. Patient Stereotypes</i>	0.6789 (0.4982)	0.7981 (0.4937)	0.2622 (0.9750)			
<i>Gender Salient</i> × <i>Gender Patient Stereotypes</i> × <i>Gender ID Strength</i>			0.2205 (1.3580)			
<i>Gender Salient</i> × <i>Gend. Risk-Averse Stereotypes</i>				0.1824** (0.0913)	0.1884** (0.0856)	0.2092** (0.0810)
<i>Gender Salient</i> × <i>Gender Risk-Averse Stereotypes</i> × <i>Gender ID Strength</i>						-0.0600 (0.0896)
<i>Gender Salient</i> × <i>Gender ID Strength</i>			0.1835 (0.8062)			0.0377 (0.0817)
<i>Gender ID Strength</i> × <i>Gender Patient Stereotypes</i>			0.2715 (0.1922)			
<i>Gender ID Strength</i> × <i>Gender Risk-Averse Stereotypes</i>						-0.0097 (0.0490)
<i>Gender Patient Stereotypes</i>	0.0775 (0.2016)	0.1227 (0.2115)	0.1209 (0.2017)			
<i>Gender Risk-Averse Stereotypes</i>				-0.0241 (0.0619)	-0.0464 (0.0579)	-0.0510 (0.0380)
<i>Gender ID Strength</i>			-0.2972 (0.2089)			-0.0933** (0.0469)
<i>SAT Math Score</i>		-0.8912*** (0.2737)			-0.0643 (0.0613)	
<i>Math Quiz Score</i>		0.4192** (0.2123)			0.0819* (0.0441)	
<i>Anxiety</i>		0.4264** (0.1781)			-0.0195 (0.0311)	
<i>1 Week vs. 2 Weeks</i>	0.2695 (0.2072)	0.2777 (0.2234)	0.2665 (0.2108)			
<i>Large Stakes</i>				0.0722 (0.0553)	0.0543 (0.0603)	0.0726 (0.0553)
Constant	-2.7709*** (0.2842)	-2.3850*** (0.2377)	-2.8194*** (0.2730)	0.2094*** (0.0483)	0.2043*** (0.0539)	0.1824*** (0.0493)
σ	1.6216 (0.1784)	1.4889 (0.1574)	1.5739 (0.1865)	0.3741 (0.0332)	0.3693 (0.0335)	0.3623 (0.0318)
<i>N</i>	102	96	102	142	128	142

* Significant at the 10% level. ** Significant at the 5% level. *** Significant at the 1% level.

Table 5. Identity-Salience Effects on Math Performance, Anxiety, and Group Identification Strength

This table reports OLS regressions where the dependent variables are the number of questions answered correctly in our math quiz, self-reported anxiety (STAI), and group identification strength. For the white and black regressions, the group identification strength refers to race identification. For men and women, the group identification strength refers to gender identification. The dependent variables are standardized to have zero mean and unit variance within each regression. *Race Salient* and *Gender Salient* are dummies for whether the subject received the race- or gender-salience treatment. *Immigrant* is a dummy for if the subject or at least one parent was born abroad. *Gender Risk-Averse Stereotypes* is the extent to which the subject believes “cautious” stereotypes are common and “reckless” stereotypes are uncommon about his or her gender.

	Math Score	Anxiety	Group ID Strength
	(1)	(2)	(3)
<i>Whites</i>			
<i>Race Salient</i>	-0.2097 (0.2224)	0.4745** (0.2077)	0.2816 (0.2011)
Constant	0.1484 (0.1469)	-0.1973 (0.1379)	-0.0900 (0.1328)
<i>N</i>	94	93	94
<i>Blacks</i>			
<i>Race Salient</i>	-0.0575 (0.3457)	0.2793 (0.3250)	-0.0399 (0.3680)
<i>Race Salient</i> × <i>Immigrant</i>	0.3414 (0.4920)	-0.1363 (0.4625)	0.5069 (0.5237)
<i>Immigrant</i>	0.0608 (0.3501)	-0.6210* (0.3291)	-0.4311 (0.3727)
Constant	0.0429 (0.2593)	0.2649 (0.2437)	0.0463 (0.2760)
<i>N</i>	63	63	63
<i>Men</i>			
<i>Gender Salient</i>	-0.1615 (0.2247)	0.0469 (0.2432)	0.1099 (0.2527)
<i>Gender Salient</i> × <i>Gender Risk-Averse Stereotypes</i>	-0.5081** (0.2310)	0.0791 (0.2501)	-0.5091* (0.2597)
<i>Gender Risk-Averse Stereotypes</i>	-0.0373 (0.1592)	0.0053 (0.1723)	0.4611** (0.1790)
Constant	0.1160 (0.1424)	-0.1099 (0.1541)	-0.1055 (0.1601)
<i>N</i>	69	69	69
<i>Women</i>			
<i>Gender Salient</i>	0.2752 (0.2841)	0.2644 (0.2857)	0.4949** (0.2576)
<i>Gender Salient</i> × <i>Gender Risk-Averse Stereotypes</i>	-0.0129 (0.3249)	0.3468 (0.3261)	-0.1628 (0.2947)
<i>Gender Risk-Averse Stereotypes</i>	0.0460 (0.1701)	0.0335 (0.1710)	0.0841 (0.1543)
Constant	0.1058 (0.1491)	-0.1362 (0.1509)	-0.2885** (0.1352)
<i>N</i>	71	70	71

* Significant at the 10% level. ** Significant at the 5% level.