Social Identity and Preferences[*]

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Abstract

Social identities prescribe behaviors for people. We identify the marginal behavioral effect of these norms on discount rates and risk aversion by measuring how laboratory subjects' choices change when an aspect of social identity is made salient. When we make ethnic identity salient to Asian-American subjects, they make more patient choices. When we make racial identity salient to black subjects, non-immigrant blacks (but not immigrant blacks) make more patient choices. Making gender identity salient has no effect on intertemporal or risk choices.

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Relative to white Americans, Asian-Americans accumulate more human capital and are more likely to participate in tax-deferred savings accounts. Black Americans accumulate less financial wealth, accumulate less human capital, and are less likely to invest in the stock market, even after controlling for observable demographic variables. However, recent black immigrants from the West Indies and Africa are disproportionately represented among high-income blacks and elite college students. Women invest in more conservative financial assets than men and behave more cautiously in laboratory experiments.[1]

Many social scientists have argued that differences in norms tied to social identities help explain such demographic differences in economic outcomes.[2] Asian-American identity is hypothesized to include a norm for patience, non-immigrant black identity is hypothesized to include norms for impatience and risk-averse investing, and a higher degree of risk aversion is hypothesized to be normative for female identity than for male identity. However, it is difficult to test with non-experimental data whether identity norms play a causal role in economic decision-making. Social category affiliations in the field are confounded with many other factors such as socioeconomic status, opportunity sets, and peer pressure (David Austen-Smith and Fryer 2005; Fryer and Paul Torelli 2005).

In this paper, we use a methodology from social psychology to introduce exogenous variation in identity effects. According to "self-categorization theory," a long-standing idea in

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[1] These findings are documented in Thomas Sowell (1975), Stanley Sue and Sumie Okazaki (1990), Derek A. Neal and William R. Johnson (1996), Erik Hurst, Ming-Ching Luoh, and Frank Stafford (1998), Nancy Ammon Jianakoplos and Alexandra Bernasek (1998), Annika E. Sundén and Brian J. Surette (1998), James P. Byrnes, David C. Miller, and William D. Schaefer (1999), Joseph G. Altonji, Ulrich Doraszelski, and Lewis Segal (2000), Glenn R. Springstead and Theresa M. Wilson (2000), Roland G. Fryer Jr. and Steven D. Levitt (2004), Rachel Croson and Uri Gneezy (2004), and Sara Rimer and Karen W. Arenson (2004). It should be noted that Christopher Carroll, Byung-Kun Rhee, and Changyong 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, for example, Thomas Sowell (1975, 1981, 2005), Barry R. Chiswick (1983), Charles Murray (1984), Geert Hofstede and Michael Harris Bond (1988), Richard P. Barke, Hank Jenkins-Smith, and Paul Slovic (1997), and Yankelovich Partners Inc. (1999).

psychology (William James 1890; John C. Turner 1985), environmental cues called "primes" can temporarily make a certain social category more salient, causing a person's behavior to tilt more toward the norms associated with the salient category. If the self-categorization theory is valid, then a researcher can identify the marginal effect of a particular social category by experimentally varying the salience of the category and seeing how an individual's behavior changes.

Our focus is on the effect of ethnic, racial, and gender category norms on time and risk preferences. After randomly manipulating category salience, we elicit subjects' time and risk preferences using incentive-compatible mechanisms standard in experimental economics. Section I describes a theoretical framework for understanding how priming effects allow us to make inferences about norms.

Section II presents the first experiment, which studies the effect of Asian ethnic category norms on time and risk preferences. Consistent with the hypothesis that Asian category norms help explain high Asian-American capital accumulation, we find that primed Asian-American subjects make more patient choices than Asian-American control subjects.

Section III presents the second experiment, which studies black racial category norms and gender category norms. Contrary to the arguments of Sowell (1975, 1981, 2005) and Charles Murray (1984), we find that making race salient causes native blacks—that is, those who have long-standing U.S. roots—to become more patient, whereas it has no effect on immigrant blacks' discount rates.[3] We also find suggestive evidence that native blacks become more risk averse when primed with race, which is consistent with racial risk norms depressing native blacks'

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[3] See Herbert J. Storing (1995), Cornel West (2000), and Bernard W. Bell (2005) for arguments that patience is a central feature of black American norms.

stock market participation, and that whites become more patient when primed with race. Finally, making gender salient has no effect on men and women's risk aversion.

There is a large psychology literature on identity salience.[4] Our work differs from this past research in two important ways. First, we focus on primitive preference parameters measured with incentive-compatible mechanisms, dependent variables that are primarily of interest to economists. Second, psychologists have emphasized testing the validity of self-categorization theory and the cognitive mechanisms through which it operates. Therefore, they prime identities chosen specifically because their norms are *known* so that the theory has a clear prediction about the priming effect. We take self-categorization theory as given and prime identities with *unknown* norms so that we can infer what those norms are via the behavioral response to the prime.

Section IV concludes. A web appendix contains the experimental materials we used.

I. A Theoretical Framework

Within our theoretical framework, which is inspired by Akerlof and Kranton (2000), priming a particular social category reveals the marginal effect of increasing the strength of affiliation with that category. Let x be some action choice, such as how much to pursue immediate gratification or how much to avoid risks. An individual belongs to a social category C, such as black race or female gender, with strength $s \geq 0$. Let x_0 denote the individual's

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[4] Identity salience studies include Stephen Reicher and Mark Levine (1994), Mark R. Forehand, Rohit Deshpandé, and Americus Reed II (2002), and Robyn A. LeBoeuf, Eldar Shafir, and Julia Belyavsky (2006). In addition, priming techniques are widely used in psychology to explore broader cognitive processes. See, for example, John A. Bargh, Mark Chen, and Lara Burrows (1996) and Daniel M. Wegner and Bargh (1998). Our work is also related to economic research on social identity and discrimination against outgroup members (e.g., Karla Hoff and Priyanka Pandey 2006; Yan Chen and Sherry Xin Li 2009).

preferred action in the absence of identity considerations, and let x_C denote the action prescribed for members of social category C. The individual chooses x to maximize

$$U = -(1 - w(s))(x - x_0)^2 - w(s)(x - x_C)^2, \tag{1}$$

where $0 \le w(s) \le 1$ is the weight placed on social category C in the person's decision. We assume that $w(0) = 0$ and $w' > 0$. Deviating from the norm prescribed for one's category causes disutility that is increasing in s, the strength of one's affiliation with that category. We assume that s has a steady-state value $\bar{s}$ (perhaps negatively correlated with $|x_0 - x_C|$) but can be temporarily perturbed away from $\bar{s}$ by a category prime $\varepsilon > 0$.

The first-order condition of (1) gives the optimal action, $x^*(s) = (1 - w(s))x_0 + w(s)x_C$, which is a weighted average of the preferred action without identity considerations and the category norm. This condition yields several implications that guide our analysis.

First, the higher the steady-state strength $\bar{s}$ of the category affiliation, the closer x^* is to x_C. Second, a category prime ε also causes x^* to move closer to x_C. Thus, the behavioral effect of priming social category C reveals the marginal behavioral effect of increasing the steady-state strength $\bar{s}$ of C. This is why priming manipulations are a useful experimental procedure for studying how identity affects steady-state preferences. Third, the sign of the priming treatment effect, $x^*(s + \varepsilon) - x^*(s) \approx (dx^* / ds)\varepsilon = w'(s)(x_C - x_0)\varepsilon$, depends on the sign of $x_C - x_0$. Even if the $\bar{s}$, x_0, and $w(\cdot)$ of an experimental sample differ from those of the general population, the directional effects of priming the sample will generalize to the population as long as $x_C - x_0$ has the same sign for both groups.

The hypothesis that an identity norm increases patience corresponds to $\gamma_C < \gamma_0$, where γ is the degree of impatient behavior. Similarly, the hypotheses that an identity norm increases risk aversion corresponds to $\rho_C > \rho_0$, where ρ is the degree of risk-averse behavior.

Some psychologists have expressed the intuition that priming a category should have a stronger effect on those who identify more strongly with that category. Our framework generates the perhaps surprising conclusion that this need not be the case. Let $x_0 < x_C$ without loss of generality. Priming sensitivity increasing with identification strength corresponds to the condition $d^2 x^* / ds^2 > 0$, which holds in a neighborhood of s if and only if $w''(s) > 0$. Depending on the shape of $w(\cdot)$ and the level of s, $d^2 x^* / ds^2$ could take either sign. While it may be the case that individuals with higher $\bar{s}$ are more susceptible to priming ($w'' > 0$), such individuals could instead become saturated with the category ($w'' < 0$). Thus, measuring the interaction between priming and identification strength does not test the theory's validity.

II. Experiment 1: Asian-American Ethnic Norms

A. Procedure

Participants were Harvard College undergraduates, 71 of Asian descent and 66 of white descent. Half the participants were randomly assigned to the ethnicity-salience condition and half to the control condition. Subjects responded to a "background questionnaire" that varied by experimental condition, then a time preference elicitation, and finally a risk preference elicitation.

In the ethnicity-salience condition, the "background questionnaire" included questions about languages spoken by the subject's family and how many generations the subject's family has lived in the United States. This manipulation was originally used by Margaret Shih, Todd L.

Pittinsky, and Nalini Ambady (1999) to make ethnicity salient to Asian-Americans. In the control condition, the "background questionnaire" asked about the school meal plan and cable television subscription.

We measured time preferences by asking participants to make 46 binary choices between a certain amount of money received earlier ($3 or $7) and a larger amount received later (either between $3.05 and $7, or between $7.10 and $15). Half the choices were between an immediate payment and a payment one week later, and half the choices were between a payment one week later and a payment two weeks later.

Our approach to measuring time preferences is standard (Shane Frederick, George Loewenstein, and Ted O'Donoghue 2002). Similar measures predict variation in discounting-related behaviors such as drug addiction, cigarette smoking, excessive gambling, use of commitment savings devices, borrowing on installment accounts and credit cards, and rapid exhaustion of food stamps.[5]

We measured risk preferences with 18 binary choices between a sure $4 and an X percent chance at $8, where X varied from 25 to 76. Risk aversion measures derived from real-stakes experimental choices such as ours are highly correlated with measures from hypothetical choices, 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.[6]

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[5] See Victor Fuchs (1982), Warren K. Bickel, Amy L. Odum, and Gregory J. Madden (1999), Kris N. Kirby, Nancy M. Petry, and Bickel (1999), Petry and Thomas Casarella (1999), Kirby and Petry (2004), Jesse M. Shapiro (2005), Nava Ashraf, Dean S. Karlan, and Wesley Yin (2006), and Stephan Meier and Charles Sprenger (forthcoming).
[6] See Robert B. Barsky et al. (1997), Luigi Guiso and Monica Paiella (forthcoming), Thomas Dohmen et al. (2005), and Claudia R. Sahm (2007).

One of the time or risk preference choices was randomly selected to determine each participant's payout. All payments were made by a check given to the participant immediately following the experiment. Delayed payments were implemented by post-dating the check.[7]

B. Econometric Methodology

In the time preference task, our dependent variable is the log of the minimum continuously compounded weekly interest rate that the subject requires to choose the later payment. That is, we apply the log operator once to transform the reservation gross interest rate into the continuously compounded net interest rate, and then we apply the log operator again. The second application of the log operator causes the estimator we describe below to assume that reservation continuously compounded interest rates are conditionally log-normal, thus ruling out negative discount rates. In the risk preference task, our dependent variable is the minimum risk premium—that is, the expected return offered by the gamble in excess of the risk-free return— that the subject requires to accept the gamble. For example, if the subject would choose to gamble for $8 rather than accept the sure $4 if and only if the probability of winning is at least 58 percent, then the reservation risk premium is $(8 \times 0.58 - 4)/4 = 0.16$.[8]

Because we observe choices at only a finite number of interest rates and risk premia, we use an interval regression (Mark B. Stewart 1983), which is a generalization of the tobit estimator that allows the econometrician to specify that the dependent variable value lies

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[7] If a risk preference choice was selected for payment, the check was immediately cashable.
[8] In this formula, we treat the risk choice as investing a $4 endowment into either the risk-free or risky asset.

somewhere within an interval. Due to outliers, we will focus on median interest rates in our analysis, although means give directionally similar results.[9]

C. Results

Table 1 displays, by experimental condition and race, the average proportion of choices where subjects chose the earlier or safe option. In the unprimed condition, Asians are somewhat more impatient and risk averse than whites. This comparison may be confounded by sample selection (both into the Harvard student body and into the experiment); in the nationally representative Health and Retirement Study, middle-aged and older Asian-Americans appear to be less risk averse than whites on average (Barsky et al. 1997). Even in a representative sample, identity effects cannot be inferred from simple differences in measured preferences because social category affiliations are confounded with many unobserved factors. To learn about identity effects, we instead turn to the comparisons between treatment and control groups.

Even though Table 1 discards all information about the prices involved in each trade-off, the main result of Experiment 1 is apparent: Asians make significantly fewer impatient choices when their ethnicity is primed. The 14 percentage point drop in the proportion of impatient choices is significant at the 1 percent level. In contrast, whites seem to get slightly more impatient in the ethnicity-salience condition, but the difference is not significant. The fact that whites do not respond to the prime is evidence that that the Asian priming effect is not driven by some channel common to both Asians and whites. Neither whites nor Asians change their risk choices in response to the ethnicity prime.

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[9] In the interest rate regressions that follow, if the coefficients imply that a certain set of explanatory variable values is associated with a mean log continuously compounded interest rate of $\hat{\mu}$, then the median continuously compounded interest rate is $\exp(\hat{\mu})$.

Table 2 presents formal regression evidence on priming effects, incorporating price information. We regress participants' reservation log continuously compounded interest rate and risk premium on an experimental condition dummy. The interest rate regression also includes dummies for whether the intertemporal choice is between payments deferred for one week versus two weeks, whether the earlier payout being offered is $7, and the interaction of the above two dummies. We cluster standard errors in the interest rate regression by subject (Kenneth A. Froot 1989; William Rogers 1993). Column 1 confirms that 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 percent to 2.1 percent. Running separate regressions for each intertemporal choice type (immediate payment amount × time horizon) reveals that this treatment effect is significant at the 1 percent level and of similar magnitude for all four types.[10] Column 3 shows that there is no effect on the risk premium Asians require to accept gambles, which is perhaps not surprising given the previous literature's ambiguous findings on Asian risk attitudes.[11] Columns 2 and 4 show, in analogous regressions for white subjects, that whites' choices are not affected by the prime.

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[10] In a regression that includes an interaction between identity salience and an indicator for one's family having lived in the U.S. for fewer than two generations, the interaction is insignificantly negative for Asians. However, this regression must be interpreted cautiously because we did not measure immigration status for the control group and so cannot include immigration status as an uninteracted regressor.

[11] See Lawrence K. Hong (1978), Barsky et al. (1997), Elke U. Weber and Christopher Hsee (1998), and Hsee and Weber (1999).

III. Experiment 2: Black Racial Norms and Gender Norms

A. Procedure

We recruited 280 Temple University students and 231 University of Michigan students. We randomly assigned participants to the race-salience, gender-salience, or control conditions. No blacks were assigned to the gender-salience condition due to their scarcity in the sample.

The first section of the questionnaire contained the category-salience manipulation or control questions, modeled after Shih, Pittinsky, and Ambady (1999). In the race-salience condition, this section asked subjects to identify their race and opine on living with roommates of the same or different race. In the gender-salience condition, subjects identified their gender and opined on living on a co-ed versus single-sex dormitory floor. In the control condition, the first section asked about living on or off campus.

The next three sections were a time preference elicitation, a risk preference elicitation, and a shortened version of the Spielberger State-Trait Anxiety Inventory (Theresa M. Marteau and Hilary Bekker 1992). These three sections' order varied across sessions. The penultimate section was an eight-minute math quiz with six SAT-like questions. We paid participants 10 cents for each correctly answered math question. The final section asked questions about personal and family background. Participants were paid for their choices by check immediately upon completing the experiment.

We measured time preferences by asking participants to make 24 binary choices between $10 earlier and some larger amount later, ranging from $10.10 to $15. Half the choices were between an immediate payment and a payment one week later, and half the choices were between a payment one week later and a payment two weeks later. One question in this section was randomly chosen for payment. Delayed payments were implemented via post-dated check.

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The section's instructions made it 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 the risk preference elicitations.

We elicited small-stakes risk preferences by asking participants to make six binary choices between $1 for sure and a 50 percent chance at a larger amount, ranging from $1.60 to $3.60. All six choices affected each subject's payment. We measured larger-stakes risk preferences with analogous choices, except that the monetary amounts were multiplied by 100 and there was only a small chance that the subject's choice would be implemented.[12]

B. Main Results

Table 3 displays, by demographic group × treatment group, the average percent of intertemporal or risk choices where subjects chose the earlier or safe payment, respectively.[13] (A Web appendix table reports descriptive statistics for each demographic × treatment group.) We classify blacks as immigrants if they responded in the debriefing survey that either they or at least one of their parents were born outside the U.S. Because no blacks were assigned to the gender-salience condition, we exclude all blacks from the control group when making comparisons to a gender-salience treatment group.

As in Experiment 1, our student subjects are a highly selected population, and this is reflected in their baseline choices. For instance, blacks are on average more risk averse than whites in nationally representative data (on middle-aged and older Americans; see Sahm 2007),

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[12] Any money participants earned from their risk choices was paid with a check that could be cashed immediately.
[13] To be clear that they are not driving our results, we have dropped the four native blacks who were over 22 years old, all of whom were randomized into the race-salience treatment. These subjects—ages 23, 23, 34, and 47—differed from other subjects along many dimensions. Our priming results are qualitatively similar if we include these four subjects.

whereas our unprimed native black subjects choose the safe payment less often than our white subjects. We identify identity effects by comparing unprimed participants with primed participants within each demographic group.

Table 3 shows that when we discard all price information, we do not find any treatment effects that reach statistical significance. Table 4 presents regression evidence incorporating price information. Our econometric methodology is similar to that used for Experiment 1. The dependent variable is either the log of the continuously compounded reservation interest rate or the reservation risk premium. The explanatory variables are a dummy for whether a social category is being made salient, a tradeoff type dummy (whether the intertemporal choice is between payments deferred for one week versus two weeks, or whether the sure payout in the risky choice is $100), a dummy for whether the subjects were recruited at Michigan, and an interaction between the tradeoff type and Michigan dummies.

In Panel A, we see that making race salient to native blacks significantly lowers their reservation interest rate for deferring receipt of payment.[14] For example, fitted regression estimates from the table indicate that when choosing between money now and money one week from now, the median continuously compounded reservation interest rate among Temple students falls from 23.2 percent to 10.9 percent. In contrast, race salience has no effect on white and immigrant black reservation interest rates.[15] It appears that racial identity effects on discount

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[14] The difference in the statistical significance of the native black priming effect in Table 3 versus Table 4 is mainly due to the fact that the reservation continuously compounded interest rate is close to log-normally distributed. Therefore, using the log of the continuously compounded interest rate as the dependent variable in a regression results in an efficient estimate of the median. The comparison of percentages in Table 3 did not utilize such a transformation, resulting in an inefficient estimate. Note that we had subjects make many more intertemporal choices in Experiment 1 than in Experiment 2, so the inefficient estimator used in Table 1 still has sufficient power to detect the Asian priming effect.

[15] The difference between the native black coefficient and the white coefficient is significant at the 10 percent level ($p = 0.06$), as is the difference between the native black and immigrant black coefficients ($p = 0.10$). However, these comparisons must be interpreted carefully, since the dependent variable has been transformed by two applications of the log function. A one unit movement in the transformed interest rate does not translate to an equivalent size

rates do not cause the capital accumulation gap between blacks and whites, and between native and immigrant blacks.

The last two columns of Table 4's Panel A show that making gender salient has no significant effects on either men's or women's patience. Panel B shows that there are no significant priming effects on any of the groups' risk aversions either. In particular, there is no indication that male identity contains a stronger risk-seeking norm than female identity.

Subjects who did not believe that our preference elicitations were incentive-compatible may have made low-quality decisions or acted strategically (rather than reporting their true preferences), adding noise and bias to our treatment effect estimates. Therefore, an interesting subsample is the set of subjects who reported in the debriefing questionnaire that they believed that they would be paid exactly as promised in the experimental instructions.[16]

Our first question regarding payment credibility 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?" 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?"

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movement in the untransformed interest rate, which is the price that is relevant to subjects. To get a sense of the magnitudes in the untransformed scale, we perform a calculation similar to the one in the main text and find that primed Temple whites' median reservation interest rate falls by only 0.4 percentage points, and primed Temple immigrant blacks' median reservation interest rate falls by only 0.7 percentage points. Analogous calculations for Michigan subjects yield similar results.

[16] We did not measure our credibility among Experiment 1 subjects. Experiment 1 was conducted on Harvard undergraduates by the third author, who was also a Harvard undergraduate at the time. We had no affiliation with Temple, and only a temporary non-faculty affiliation with the University of Michigan. In subsequent experiments similar to Experiment 2 (but with a different priming manipulation) at one of our home institutions, we find belief rates around 80 percent, suggesting that credibility is in fact much higher when social distance is smaller.

In a Web appendix table, we show that the category salience manipulations had no statistically significant effect on belief in our payment promises, but between 35 and 53 percent of subjects within each demographic group × experimental condition cell reported some disbelief. Other studies have shown that without financial incentives, experimental participants behave more randomly and exert less effort (see Colin F. Camerer and Robin M. Hogarth 1999 for a literature review). We have suggestive evidence that this is true among our skeptics. One measure of participants' cognitive effort is whether their choices are "well-behaved"—choosing the delayed payment if and only if the interest rate exceeds exactly one threshold, and choosing the gamble if and only if the risk premium exceeds exactly one threshold.[17] Respectively, 7 percent and 21 percent of participants who did not believe our payment promises failed to answer the intertemporal and risk questions in a well-behaved manner. These proportions are greater than the 5 percent and 16 percent of believers who made poorly behaved intertemporal or risk choices, but it should be noted that these differences are not statistically significant.[18] Because dropping self-reported skeptics causes us to discard so much data, subjects endogenously chose their response to the belief questions, and merely asking questions about payment credibility may have generated skepticism *ex post* even if none was present during the preference elicitations, the subsample results that follow should be interpreted with caution.

In Table 5, we exclude subjects who did not believe that their choices would affect their payment. For our time preference regressions, we additionally drop subjects who did not believe they would receive deferred payments. We find that restricting our sample to self-reported

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[17] Our results are qualitatively similar when excluding subjects whose choices were poorly behaved.

[18] Non-believers also made a higher fraction of impatient choices ($p = 0.02$) and cautious choices (not statistically significant), which is consistent with their exerting less cognitive effort (Daniel J. Benjamin, Sebastian A. Brown, and Jesse M. Shapiro 2006). Charles A. Holt and Susan K. Laury (2002) find that subjects become less risk averse when they are not offered incentives. Our results may not be inconsistent with Holt and Laury's because they explicitly told their subjects that the choices were hypothetical, whereas our non-believing subjects apparently believed we were deceiving them about incentives.

believers qualitatively changes some of our results. Although the point estimate for the effect of priming race on time preference is nearly identical in the full native black sample and in the restricted native black sample (which is 41 percent smaller), the effect loses statistical significance in the restricted sample. Priming race also causes white discount rates to drop significantly among believers, suggesting that there may be a patient white racial norm as well.[19]

In the risk domain for the restricted sample, we find that priming racial identity raises the reservation native black risk premium among believers by 23 percentage points, a change that is significant at the 1 percent level. In contrast, immigrant blacks' required risk premium falls by 11 percentage points when race is salient, although the drop is not statistically different from zero. The native black priming effect on risk aversion is statistically different from the white and immigrant black priming effects (both *p*-values < 0.01). This result is consistent with the hypothesis that racial risk norms depress native blacks' stock market participation.

C. Potential Alternative Explanations

Stereotype threat, lift, and emotional states. Many researchers have documented the "stereotype threat" phenomenon: making race or gender salient impairs the cognitive performance of groups with stereotypically poor performance. Conversely, making negative stereotypes about *other* groups salient improves cognitive performance, a finding dubbed "stereotype lift."[20] It is believed that stereotype threat and lift effects operate through increasing or reducing anxiety that

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[19] Race (e.g. white) is distinct from ethnicity (e.g. German). The priming effect on whites in Experiment 2 should be interpreted as a race salience effect on whites. For whites in Experiment 1, the priming effect should be interpreted as an ethnicity salience effect for the various ethnicities relevant to the white subjects.

[20] Studies documenting stereotype threat and lift include Claude M. Steele and Joshua Aronson (1995), Shih, Pittinsky, and Ambady (1999), Gregory M. Walton and Geoffrey L. Cohen (2003), David M. Marx and Diederik A. Stapel (2006), and Thomas S. Dee (2008).

one will confirm negative stereotypes about one's group. Consistent with this mechanism, these effects vanish when tasks are presented to subjects as not being diagnostic of ability.

A possible explanation for our results is that the category primes affected subjects' ability to compute expected values and interest rates. We think this explanation is unlikely because we did not present the preference elicitation questions as being diagnostic of ability. In Experiment 2, we explicitly told subjects that there are no right or wrong answers for the preference elicitation questions.

However, 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 are not being driven by stereotype threat, stereotype lift, or emotional changes, we examine how the primes affected performance on the five math quiz questions administered after the elicitations and responses to the anxiety inventory. The primes had no effect on math quiz performance or anxiety (results not shown in tables).[21]

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[21] Stereotype threat and lift effects have been shown to last at least 16 minutes after race is made salient (Jim Blascovich et al. 2001). Hence, these effects (if we had activated them) would be expected to affect performance in our math quiz, which was administered approximately 12 minutes after the prime. We can confirm that our priming effect on time preference persists for at least 12 minutes because we varied the order of the preference elicitations (in Experiment 2). In fact, the priming effect is *largest* immediately before the math quiz, although this is mainly driven by control subjects becoming more impatient, rather than primed subjects becoming more patient. We thank a referee for pointing out that, even if stereotype threat did not affect the preference elicitations, one might have expected stereotype threat to be activated for the math questions, since they are clearly diagnostic of ability. The lack of such an effect here may be due to the many non-diagnostic tasks performed between the prime and the math quiz. Stereotype threat studies typically administer the diagnostic task immediately after the identity-relevant questions and present the task as the focus of the researchers' interest, making it more likely that subjects will infer that their performance on that task will be used to judge their race or gender.

Experimenter "demand effects." If participants understood the purpose of the experiment, then our priming effects could be explained by a "demand effect" that caused participants to behave in the way they thought the experimenters wanted them to behave.

In the Michigan sample, we asked in the final questionnaire section, "Think back to when you were making choices about money. While you were making those choices, were you thinking about what we *wanted* you to do?" Ninety percent of subjects circled the answer, "No, I was making the choice I wanted to make. I was not thinking about what the experimenter might want me to choose." Of those who instead circled yes, most made innocuous guesses about what the experimenters wanted (like "to see whether or not we were risk takers with money"), and no one made a guess related to race or gender. Our results are qualitatively similar when we drop subjects who reported thinking about what the experimenter wanted them to do.

Type I error. Although our main priming effects are highly statistically significant, we also tested many hypotheses. One way to gain a sense for the likelihood that our results are driven by Type I error is to see if the priming effects are consistent at both schools in which we collected data. Running regressions separately for each school, we find that the native black discount rate effect is strong among the Michigan subjects, where the identity salience coefficient is -1.628 and the p-value is 0.006. The effect is also negative among the native black subjects at Temple, although the identity salience coefficient is smaller (-0.281) and the p-value is larger (0.353).

In the subsample of subjects who reported believing our payment promises, priming race in native blacks caused them to become more risk averse in both the Temple sample (coefficient $= 0.153$, $p = 0.094$) and the Michigan sample (coefficient $= 0.358$, $p = 0.013$). The point estimate for the race priming effect on immigrant blacks' reservation risk premium is negative at both

Temple (–0.125) and Michigan (–0.035), and the *p*-value for the difference between the native and immigrant black race priming effects on the risk premium is 0.045 at Temple and 0.059 at Michigan. The white believer subsample's discount rate priming effect is negative at both schools, but stronger at Temple (coefficient = –0.967, *p* = 0.029) than at Michigan (coefficient = –0.418, *p* = 0.368).

IV. Conclusion

Our results suggest that social identity affects fundamental economic preferences. We find that making Asian-American subjects' ethnicity salient causes them to exhibit more patient preferences. Making race salient to black subjects decreases discount rates among those who have longstanding roots in the U.S. There is also suggestive evidence that native blacks become more risk averse and whites more patient when their racial identity is salient.

Understanding identity salience effects is important for at least two reasons. First, as we have emphasized in this paper, identity salience manipulations are an empirical tool that economists can use to test theories about how steady-state identity affiliations matter for behavior. Second, identity primes may in themselves have important real-world behavioral consequences. If being the only female in line at the polling booth primes the gender identity category, then it may influence the woman to vote for a female candidate.[22] An American-born black worker who is enrolling in his 401(k) may, due to a transitory racial prime, choose a higher savings rate. Even though his discount rate was only temporarily depressed, a large body of empirical evidence has shown that most households' retirement savings decisions are highly inertial (William Samuelson and Richard Zeckhauser 1988; Brigitte C. Madrian and Dennis F.

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[22] Jonah Berger, Marc Meredith, and S. Christian Wheeler (2008) find that voters who vote in a school are more likely to vote to raise taxes to support education.

Shea 2001; James J. Choi et al. 2002), so the momentary effect of the prime could have long-run

consequences. Priming effects raise the possibility that a benevolent policymaker could

intentionally use identity primes as an instrument for encouraging desirable behaviors in various

domains, such as savings and investing. Of course, policymakers and other market players will

have self-interested motives as well for manipulating the primes to which individuals are

exposed.

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TABLE 1—PERCENT OF IMPATIENT OR SAFE CHOICES, EXPERIMENT 1

	Percent impatient choices		Percent safe choices	
	Asians	Whites	Asians	Whites
Control	26.37	20.90	66.67	57.96
	(17.49)	(17.94)	(21.54)	(25.00)
Ethnicity Salient	12.63	27.14	64.41	57.28
	(16.28)	(17.78)	(25.07)	(16.34)
p-value of difference	0.0010	0.1639	0.6872	0.8998
N	71	66	71	66

Notes: This table shows the percent of intertemporal choices in which subjects chose the earlier payment, and the percent of risk choices in which subjects declined the gamble. The percentages are reported separately for Asians and whites by experimental condition. Cross-subject standard deviations of the percentages are in parentheses. The penultimate row shows *p*-values of the *t*-test for equality of means in those percentages between the ethnicity-salience and control conditions. The final row shows the number of subjects in each demographic group.

TABLE 2—ETHNICITY-SALIENCE TREATMENT EFFECT ON
ASIAN AND WHITE LOG INTEREST RATE AND RISK PREMIUM, EXPERIMENT 1

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

Notes: This table presents interval regressions where the latent dependent variable is the log continuously compounded 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 Salient* is a dummy for the subject receiving the ethnicity-salience 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. The estimated conditional standard deviation of the latent dependent variable is denoted by $\hat{\sigma}$. The final row reports the number of choices in the regressions. Standard errors appear in parentheses below the point estimates. Huber-White standard errors, clustered by subject, are reported for the log interest rate regressions. * Significant at the 5 percent level. ** Significant at the 1 percent level

TABLE 3—PERCENT OF IMPATIENT OR SAFE CHOICES, EXPERIMENT 2

Panel A: Percent of choices that were impatient

	Whites	Native blacks	Immigrant blacks	Men	Women
Control	41.54	63.58	52.30	46.54	36.06
	(31.04)	(23.07)	(25.07)	(31.64)	(28.39)
Social category salient	40.10	50.53	48.94	49.20	40.32
	(32.03)	(32.30)	(26.62)	(30.13)	(28.33)
p-value of difference	0.6967	0.1390	0.6591	0.7293	0.4594
N	222	71	53	129	134

Panel B: Percent of choices that were safe

	Whites	Native blacks	Immigrant blacks	Men	Women
Control	51.42	48.46	50.60	49.19	49.80
	(21.43)	(18.20)	(19.37)	(20.94)	(21.41)
Social category salient	47.13	55.87	48.33	45.74	44.88
	(20.98)	(18.47)	(22.18)	(22.85)	(19.36)
p-value of difference	0.1226	0.1757	0.7585	0.2823	0.1207
N	222	71	53	129	134

Notes: This table shows the percent of intertemporal choices in which subjects chose the earlier payment, and the percent of risk choices in which subjects declined the gamble. The percentages are reported separately for each demographic group by experimental condition. "Social category salient" refers to the race-salience treatment (first three columns) or the gender-salience treatment (last two columns). Cross-subject standard deviations of the percentages are in parentheses. In order to test for differences between the control and social category salient percentages, we run an OLS regression of each variable of interest on a treatment dummy, a dummy for whether the subject was recruited at Michigan (because the proportion of Michigan students is not equal across treatment cells), and a constant. The *p*-values reported in the penultimate row of each panel are for the treatment dummy coefficients. The final row in each panel shows the number of subjects.

TABLE 4—CATEGORY-SALIENCE TREATMENT EFFECTS, EXPERIMENT 2

Panel A: Log Interest Rate

	Whites	Native blacks	Immigrant blacks	Men	Women
Social Category Salient	-0.0563	-0.7587**	-0.0899	-0.1376	0.3213
	(0.2400)	(0.2896)	(0.2868)	(0.2942)	(0.2834)
1 Week vs. 2 Weeks	-0.1905	-0.1431	0.2863	-0.3897*	0.1501
	(0.1190)	(0.2305)	(0.2473)	(0.1695)	(0.1648)
UMich	-0.2307	-0.5858	-0.0234	-0.9167**	0.0981
	(0.2407)	(0.3684)	(0.4541)	(0.2976)	(0.2903)
1 Week vs. 2 Weeks × *UMich*	0.0275	0.3317	0.6190	0.3867	-0.4079
	(0.1628)	(0.3061)	(0.3450)	(0.2039)	(0.2178)
Constant	-2.6663**	-1.4605**	-2.4737**	-1.9818**	-3.1398**
	(0.1984)	(0.2147)	(0.2575)	(0.2147)	(0.2688)
$\hat{\sigma}$	1.7980	1.4328	1.2002	1.6166	1.6726
	(0.1012)	(0.1517)	(0.1176)	(0.1252)	(0.1154)
N	444	142	106	258	268

Panel B: Risk Premium

	Whites	Native blacks	Immigrant blacks	Men	Women
Social Category Salient	-0.0698	0.0986	0.0474	-0.0528	-0.1091
	(0.0468)	(0.0885)	(0.0940)	(0.0660)	(0.0598)
Larger Stakes	0.3176**	0.0470	0.0634	0.3211**	0.1301*
	(0.0436)	(0.0837)	(0.0751)	(0.0582)	(0.0572)
UMich	0.0125	-0.0091	0.1827	-0.0093	-0.1119*
	(0.0456)	(0.1166)	(0.1138)	(0.0625)	(0.0568)
Larger Stakes × *UMich*	-0.0724	0.2454	0.1215	-0.0842	0.0812
	(0.0596)	(0.1371)	(0.1774)	(0.0852)	(0.0819)
Constant	0.2002**	0.1732*	0.0976	0.1891**	0.2808**
	(0.0394)	(0.0822)	(0.0767)	(0.0491)	(0.0532)
$\hat{\sigma}$	0.3954	0.4341	0.4053	0.4087	0.3954
	(0.0184)	(0.0387)	(0.0460)	(0.0246)	(0.0236)
N	444	142	106	258	268

Notes: This table presents interval regressions where the latent dependent variable is the log continuously compounded 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. *Social Category Salient* is a dummy for the race-salience treatment (first three columns) or the gender-salience treatment (last two columns). *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 sure payout in the risky choice was $100. *UMich* is a dummy for whether the subject was recruited at the University of Michigan. The estimated conditional standard deviation of the latent dependent variable is denoted by $\hat{\sigma}$. Huber-White standard errors, clustered by subject, are reported in parentheses below the point estimates. The final row of each panel reports the number of choices in the regressions. * Significant at the 5 percent level. ** Significant at the 1 percent level.

TABLE 5—CATEGORY-SALIENCE TREATMENT EFFECTS EXCLUDING
SUBJECTS SKEPTICAL ABOUT PAYMENTS, EXPERIMENT 2

Panel A: Log Interest Rate

	Whites	Native blacks	Immigrant blacks	Men	Women
Social Category Salient	-0.7006*	-0.6919	-0.3800	-0.4524	-0.0925
	(0.3184)	(0.4074)	(0.3454)	(0.3960)	(0.3967)
1 Week vs. 2 Weeks	-0.1964	-0.0943	0.0968	-0.4452	0.1797
	(0.1468)	(0.2228)	(0.3586)	(0.2289)	(0.2271)
UMich	-0.3345	-0.1665	-0.3111	-0.6460	-0.0089
	(0.3012)	(0.5126)	(0.5760)	(0.3728)	(0.3928)
1 Week vs. 2 Weeks × *UMich*	0.0348	0.0519	0.9917	0.3709	-0.4447
	(0.2163)	(0.2954)	(0.5141)	(0.2735)	(0.2943)
Constant	-2.3958**	-1.6564**	-2.4392**	-1.8242**	-3.0606**
	(0.2317)	(0.2967)	(0.3570)	(0.2663)	(0.3521)
$\hat{\sigma}$	1.7736	1.4725	1.0918	1.4852	1.7317
	(0.1291)	(0.2079)	(0.1386)	(0.1626)	(0.1554)
N	272	84	56	152	160

Panel B: Risk Premium

	Whites	Native blacks	Immigrant blacks	Men	Women
Social Category Salient	-0.0583	0.2298**	-0.1062	-0.0888	-0.1133
	(0.0507)	(0.0778)	(0.0887)	(0.0725)	(0.0674)
Larger Stakes	0.3098**	0.0442	0.1088	0.3027**	0.0938
	(0.0488)	(0.0920)	(0.0741)	(0.0658)	(0.0660)
UMich	-0.0083	0.0332	0.1101	-0.0522	-0.1616*
	(0.0493)	(0.1171)	(0.0868)	(0.0684)	(0.0628)
Larger Stakes × *UMich*	-0.0308	0.2042	0.0915	-0.0383	0.1596
	(0.0673)	(0.1480)	(0.2166)	(0.0972)	(0.0929)
Constant	0.2091**	0.0474	0.0919	0.2171**	0.3025**
	(0.0413)	(0.0818)	(0.0832)	(0.0511)	(0.0554)
$\hat{\sigma}$	0.3986	0.4087	0.3513	0.4047	0.3961
	(0.0196)	(0.0391)	(0.0525)	(0.0272)	(0.0256)
N	374	118	80	208	214

Notes: This table presents interval regressions where the latent dependent variable is the log continuously compounded 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. *Social Category Salient* is a dummy for the race-salience treatment (first three columns) or the gender-salience treatment (last two columns). *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 sure payout in the risky choice was $100. *UMich* is a dummy for whether the subject was recruited at the University of Michigan. The estimated conditional standard deviation of the latent dependent variable is denoted by $\hat{\sigma}$. Huber-White standard errors, clustered by subject, are reported in parentheses below the point estimates. The final row of each panel reports the number of choices in the regressions. * Significant at the 5 percent level. ** Significant at the 1 percent level.