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Social Identity and Preferences

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Abstract

We provide evidence that time and risk preference norms tied to social identities help shape observed U.S. demographic patterns in economic outcomes. We identify the marginal effect of norms by measuring how laboratory subjects' choices change when an aspect of social identity is made salient. We find that when ethnic identity is salient to Asian-American subjects, they make more patient choices. When we make race salient to black subjects, non-immigrant blacks (but not immigrant blacks) make more risk-averse choices. Making gender identity salient causes choices to conform to gender norms the subject believes are relatively more common.

JEL Classification: C91, J15, J16, Z10

Keywords: race, ethnicity, gender, identity, norm, stereotype, risk aversion, time preference

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I. Introduction

Relative to white Americans, Asian-Americans accumulate more human capital and are more likely to participate in tax-deferred savings accounts (Sue and Okazaki, 1990; Springstead and Wilson, 2000). 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 (Altonji, Doraszelski, and Segal, 2000; Neal and Johnson, 1996; Fryer and Levitt, 2004; Hurst, Luoh, and Stafford, 1998). However, recent 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). Women invest in more conservative financial assets than men and behave more cautiously in laboratory experiments (Jianakoplos and Bernasek, 1998; Sundén and Surette, 1998; Croson and Gneezy, 2004; Byrnes, Miller, and Schaefer, 1999).

Many social scientists have argued that differences in norms tied to social identities help explain such demographic differences in economic outcomes (Sowell, 1975, 1981, 2005; Murray, 1984; Chiswick, 1983; Barke, Jenkins-Smith, and Slovic, 1997). For example, Sowell (1975, pp. 144-146) writes, "Among the characteristics associated with success is a future orientation—a belief in a pattern of behavior that sacrifices present comforts and enjoyments while preparing for future success… Those groups who [have had] this—the Jews, the Japanese-Americans, and the West Indian Negroes, for example—all came from social backgrounds in which this kind of behavior was common before they set foot on American soil."

Identity effects can be formalized within the theoretical framework of Akerlof and Kranton (2000). Building on a long tradition in the social sciences, they propose that each "social category" constituting part of an individual's identity (such as Asian ethnicity, black race, or male gender) is associated with a set of norms for how someone in that category should behave. Norms influence behavior because they affect the individual's preferences. An individual suffers disutility from deviating from his or her categories' norms, which causes behavior to conform toward those norms.[1]

However, it is difficult to test with non-experimental data whether identity plays a causal role in economic decision-making. In the field, social category affiliations are confounded with

[1] See also Akerlof and Kranton (2002, 2005), Bisin and Verdier (2000, 2001), Fang and Loury (2005), and Bénabou and Tirole (2006) for other theoretical work on social identity effects.

many other factors such as socioeconomic status, opportunity sets, and peer pressure (Austen-Smith and Fryer, 2005; Fryer and Torelli, 2005).

In this paper, we exploit a methodology from social psychology to introduce exogenous variation in identity effects. According to "self-categorization theory," a long-standing idea in psychology (James, 1890; 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. We draw our category-salience manipulations from the psychology literature; for example, we prime gender by asking experimental participants to list advantages of living in a co-ed versus single-sex dorm floor (Shih, Pittinsky, and Ambady, 1999). Control subjects are instead asked neutral questions unrelated to identity. We then elicit subjects' time and risk preferences using incentive-compatible mechanisms standard in experimental economics. Preferences elicited through such mechanisms have been shown to predict variation in real-life impatient and risky behaviors. We test whether the effects of category norms on time and risk preferences are consistent with their contributing to observed mean group differences in economic behavior. Section II describes a theoretical framework for understanding how priming effects allow us to make inferences about norms.

Section III presents the first experiment, which studies the effect of Asian ethnic category norms on time and risk preferences. If Asian category norms help explain high Asian-American capital accumulation, then priming the Asian identity category should cause Asian-American subjects to behave more patiently. Consistent with the identity hypothesis, we find that primed Asian-American subjects make more patient choices than Asian-American control subjects, requiring a much lower interest rate for delaying receipt of payment.

Section IV presents the second experiment, which studies black racial category norms and gender category norms. If low capital accumulation and stock market participation among blacks (but relatively higher capital accumulation among immigrant blacks) is partly the result of identity-induced differences in preferences, then priming the racial identity category should cause native blacks—but not immigrant blacks—to become less patient (explaining lower capital accumulation), more risk averse (explaining lower investment in stocks and other assets commanding positive risk premia, and hence lower long-run capital accumulation), or both. Inconsistent with Sowell's hypothesis, we do not find that priming race has differential effects on discount rates for native blacks, immigrant blacks, and whites. However, we do find identity-related risk aversion effects for black subjects that depend upon how recently their family immigrated to the United States. Blacks with longstanding U.S. roots become more risk averse when primed. In contrast, blacks who have at least one foreign-born parent or who are themselves foreign-born appear, if anything, to become less risk averse. These results suggest that racial risk norms depress native blacks' capital accumulation and stock market participation.

Experiment 2 also examines gender category norms. We do not find a mean difference in gender identity effects on either discount rates or risk aversion. Nevertheless, subjects do conform to risk norms they believe apply to their gender. Priming gender increases risk aversion among both men and women who believe that risk-averse stereotypes about their own gender are relatively more common.

There is a large psychology literature on identity salience (e.g., LeBoeuf, Shafir, and Belyavsky, 2006; Reicher and Levine, 1994; Forehand, Deshpandé, and Reed, 2002).[2] 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. Our work is also related to research on social identity and discrimination against outgroup members (e.g., Hoff and Pandey, 2006; Chen and Li, 2007).

In Section IV.F, we discuss three potential alternative explanations for our results: "stereotype threat" (Steele and Aronson, 1995), experimenter demand effects, and Type I error. Section V concludes. A web appendix contains the experimental questionnaires we used.

[2] In addition, priming techniques are widely used in psychology to explore broader cognitive processes. See, for example, Bargh, Chen, and Burrows (1996) and Wegner and Bargh (1998).

II. A Theoretical Framework

Here, we outline a theoretical framework inspired by Akerlof and Kranton (2000) that organizes our thinking about identity and priming effects. In this framework, priming a particular social category reveals the marginal effect of increasing the strength of affiliation with that category.

Let $x(\theta)$ be some behavior that depends on a preference parameter θ, which may be the discount rate δ or risk aversion ρ. An individual belongs to some social category C, such as black race or female gender, with strength $s \geq 0$. Let θ_0 denote the individual's preference without identity considerations, and let θ_C denote the preference norm associated with social category C. We denote by $x_0 \equiv x(\theta_0)$ the optimal choice of x without identity considerations, and by $x_C \equiv x(\theta_C)$ the choice of x prescribed for members of C. The individual chooses x to maximize

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

where $0 \leq w(s) \leq 1$ 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.[3] We assume that s has a steady-state value $\bar{s}$ but can be temporarily perturbed away from $\bar{s}$ by a category prime ε; for example, s might follow an AR(1) process, $s_t = (1-\phi)s_{t-1} + \phi\bar{s} + \varepsilon_t$.

The first-order condition of (1) gives the optimal action,

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

which is a weighted average of the optimal choice without identity considerations and the category norm. This condition yields several implications that guide our analysis.

Proposition 1: The higher the steady-state strength $\bar{s}$ of the category affiliation, the closer x^* is to x_C.

Proposition 2: A category prime $\varepsilon > 0$ (whether naturally occurring or experimentally induced) also causes x^* to move closer to x_C.

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[3] For simplicity, we analyze the case where only a single social category is relevant to an individual, but it would be straightforward to add additional categories to the utility function.

Thus, the behavioral effect of priming social category *C* reveals the marginal behavioral effect of increasing the steady-state strength $\bar{s}$ of category *C*. This is why priming manipulations are a useful experimental procedure for studying how identity affects steady-state preferences.

Proposition 3: The priming treatment effect

$$\frac{dx^*}{ds} = w'(s)(x_C - x_0) \tag{3}$$

depends on the sign of $x_C - x_0$.

Even if college students differ from the general population in the shape of their $w(s)$ function and their levels of $\bar{s}$ and x_0, the directional effects of priming on college students will generalize as long as $x_C - x_0$ has the same sign on average for both groups.

Some psychologists have expressed the intuition that priming a category should have a stronger effect on those who identify more strongly with that category. For example, LeBoeuf, Shafir, and Belyavsky (2006, p.19) hypothesize that "evoking an identity will trigger preference assimilation only for those highly identified with that identity." In our framework, suppose without loss of generality that $x_0 < x_C$. Then the hypothesis of priming sensitivity increasing with identification strength corresponds to the condition $\frac{d}{ds}\left(\frac{dx^*}{ds}\right) = w''(s)(x_C - x_0) > 0$. Our formal framework generates a perhaps surprising conclusion about the interaction between priming and category affiliation strength.

Proposition 4: In general it is ambiguous whether the priming treatment effect is stronger or weaker for individuals with a stronger category affiliation. Suppose, without loss of generality, that $x_0 < x_C$. Then $\frac{d^2x^*}{ds^2} > 0$ if and only if $w''(s) > 0$.

Depending on the shape of $w(\cdot)$ and the level of s, d^2x^* / ds^2 could take either sign. Intuitively, while it may be the case that individuals with higher $\bar{s}$ are more susceptible to priming ($w'' > 0$), it could instead be that such individuals become saturated with the category norm ($w'' < 0$). For that reason, even though we test for interactions between priming effects and identification strength, we do not emphasize those empirical results.

In summary, the behavioral response to priming a social category provides directional information about the preference norms associated with that category, but both the magnitude of the effect and its interaction with affiliation strength must be interpreted cautiously.

III. Experiment 1: Asian-American Ethnic Norms

Asian-Americans accumulate more human capital and are more likely to participate in tax-deferred savings accounts than white Americans (Sue and Okazaki, 1990; Springstead and Wilson, 2000).[4] Norms for patient behavior seem to be linked to many Asian ethnic identities. Hofstede and Bond (1988) argue that most Asian cultures are high in "Confucian Dynamism," which emphasizes a "future-oriented mentality." American stereotypes about East Asian patience and industriousness date back to at least the 19th century (Twain, 1872)[5] and persist to this day (e.g., Kasindorf, 1982; Abboud and Kim, 2005). If identity effects on discount rates raise Asian-American financial and educational investment rates—that is, $\delta_C < \delta_0$ for the Asian category, where δ is the discount rate—then priming the Asian identity category should cause Asian-American subjects to behave more patiently.

We also measured how priming the Asian identity category affects risk aversion, although it is unclear what risk preference norm one should expect to find associated with Asian-American ethnicity. Barsky et al. (1997) find that Asian-Americans answer hypothetical survey questions in a less risk-averse manner than whites, and Weber and Hsee (1998) and Hsee and Weber (1999) present evidence that experimental subjects in China are less risk averse than American subjects, suggesting that there may be a risk-tolerant Asian category norm. On the other hand, both Chinese and American subjects in Hsee and Weber (1999) believed that Americans would be more risk seeking, and Hong (1978) finds that Taiwanese experimental subjects are more risk averse than American subjects.

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[4] It should be noted that Carroll, Rhee, and Rhee (1994, 1999) do not find that Asian immigrants save more, but they are hindered by their data quality. However, Carroll, Rhee, and Rhee (1994) do find that Asian-Canadian immigrants' educational expenditures are 3.6 times the Canadian average.

[5] 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."

We used the method developed by Shih, Pittinsky, and Ambady (1999) to prime the Asian ethnic identity category in Asian-American subjects. We then elicited time and risk preferences from primed and unprimed subjects using an incentive-compatible mechanism. To check that any Asian priming effect is working through the increase salience of the *Asian* ethnic identity category, we applied the same prime to white subjects.

A. Participants

Participants were 158 Harvard College undergraduates, 71 of Asian descent and 66 of white descent. Within our Asian group, 89% were of East Asian descent, and the remainder were of Asian Indian descent.[6] All of our results continue to hold if we drop Asian Indians from the sample. At no point did we specify in our recruiting materials that we were looking for white and Asian students, so our sessions included three biracial participants and 18 participants who were neither white nor Asian. We drop these subjects from our sample.

B. Procedure

Half the participants were randomly assigned to the ethnicity-salience condition and half to the control condition. After hearing the instructions and compensation scheme, subjects responded to a "background questionnaire" that varied by experimental condition, then a time preference elicitation, then a risk preference elicitation. Finally, participants were debriefed and payments were made.[7]

Ethnicity-salience manipulation. In the ethnicity-salience condition, there were eight questions in 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 used by Shih, Pittinsky, and Ambady (1999) to

[6] Specifically, there were 41 Chinese, 7 Indians, 7 Koreans, 5 Taiwanese, 2 Japanese, 1 Filipino, 1 Thai, 1 Vietnamese, and 6 unspecified Asians.
[7] The experimenter was a male of black, Mexican, and white descent.

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, designed to be neutral with respect to ethnic identity, asked about satisfaction with the school meal plan and subscription to cable television, modeled after Shih, Pittinsky, and Ambady (1999).

Measured time preferences. We measured time preferences by asking participants to make a series of binary choices between money received at different times. The choices were divided into two 11-question blocks and two 12-question blocks. One of the 11-question blocks required participants to choose between "$3 today **or** *X* in 1 week"; in the other block, participants chose between "$3 in 1 week **or** *X* in 2 weeks." In both cases, *X* varied from $3.05 to $7.00. The 12-question blocks were the same as the 11-question blocks, except that the monetary amounts were larger. The immediate reward was $7, and the delayed rewards varied from $7.10 to $15.00. Half the participants saw the questions in ascending order of *X*, and half in descending order. Half answered the today versus one week questions before the one week versus two weeks questions, and half the other way around.

Our approach to measuring time preferences is standard (Frederick, Loewenstein, and O'Donoghue, 2002). Similar measures predict variation in discounting-related behaviors such as drug addiction (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, 2006), borrowing on installment accounts and credit cards (Meier and Sprenger, 2006), and rapid exhaustion of food stamps (Shapiro, 2005).[8]

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

[8] Subjects in experiments such as ours require extremely high interest rates to delay payment receipt. Although it is difficult to believe that such impatience is normatively justified, the real-world payday loan market typically features a two-week interest rate of 18% (Morse, 2006; Skiba and Tobacman, 2007), which annualizes to 7295%.

76% in increments of 3%. Half the participants saw the questions in order of ascending *Y* and half in descending order.

Risk aversion measures derived from real-stakes experimental choices such as ours 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; Sahm, 2007).

Compensation scheme. Before the participant answered any of the preference elicitation questions, the experimenter explained that at the end of the experiment, the participant would randomly select which one of the time or risk preference choices would determine his or her payout.[9] 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.[10] If a risk preference choice was selected for payment, the check was immediately cashable.

C. Econometric Methodology

In our time preference task, we would like to use as our dependent variable the minimum continuously compounded weekly interest rate that the subject requires to choose the later payment over the earlier payment (i.e., the reservation price for accepting later payment). For example, if the subject would choose the later payment over an earlier \$3 payment if and only if the later payment is at least \$3.50, then the reservation interest rate is $r = \log(3.50/3) = 0.154$. [11]

[9] 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; Laury, 2005).

[10] 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 deposit dates in Experiment 1, we found in Experiment 2's Temple sample that 86% of subjects deposited their checks on or after the check date. (Because of how we ensured anonymity, a similar analysis in Experiment 2's Michigan sample was impossible.)

[11] In Experiment 1, only 4 subjects did not choose the earlier payment if and only if the interest rate was below a unique threshold, and 6 subjects did not have a unique risk premium threshold. Our results are unaffected by excluding these subjects. In our regressions, we use the interval corresponding to the lowest interest rate and lowest risk premium below which the subject behaved impatiently or cautiously, respectively. In Experiment 2, 31 and 88 subjects did not have a unique interest rate and risk premium threshold, respectively; our results are qualitatively unchanged if we exclude these participants. The results in Tables 1 and 4, which are consistent with the regression evidence, do not depend upon a separate assumption about how to treat poorly behaved choices.

Similarly, in our risk preference task, we would like to use as our dependent variable the minimum expected return premium that the subject requires to accept the gamble over the certain payout. 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%, then the reservation risk premium is $\pi = (8 \times 0.58 - 4)/4 = 0.16$.

In reality, we observe choices at only a finite number of interest rates and risk premia, and there are a substantial number of subjects whose observations are left- or right-censored. We therefore use an interval regression (Stewart, 1983), which assumes that the latent dependent variable is conditionally distributed normally, has an unknown exact value, but is known to fall within a certain interval. We make log(r) the dependent variable in the interval regression to rule out negative interest rates. Because of outliers, we will focus on median interest rates in our analysis. 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(r) of $\hat{\mu}$, then the median r is $\exp(\hat{\mu})$.[12]

We observe four r (interval) values for each participant, since we elicited four sets of intertemporal preferences. We report interest rate regression results that pool the four r values together, adding explanatory dummy variables to indicate for which trade-off type (now versus one week, one week versus two weeks, small intertemporal choice, larger intertemporal choice) the r value was observed. We cluster standard errors by subject to correct for within-subject correlation of r (Froot, 1989; Rogers, 1993).

D. Results

Of the participants who received the priming manipulation, 92% of the Asians reported having families who lived in the U.S. for two or fewer generations, and 84% reported a non-English language spoken at home. In contrast, only 34% of the primed white subjects had families who lived in the U.S. for two or fewer generations, and a mere 3% had homes where a non-English language was spoken. Therefore, the priming questions may not have made ethnicity salient to many white participants. Nonetheless, comparing the effect of the manipulation on white versus Asian participants allows us to check that any priming effect on

[12] The mean r is $\exp(\hat{\mu} + 0.5\hat{\sigma})$, where $\hat{\sigma}$ is the (estimated) 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 for mean interest rates.

the Asians is working through the increased salience of the Asian ethnic identity category, rather than through some other channel that would affect the whites as well.

Pooling across stake sizes and horizons, each subject made 46 intertemporal choices and 18 risk choices. Table 1 displays, by experimental condition and race, the average proportion of those choices where subjects chose the earlier or safe option. First examining choices in the unprimed condition, the Asian participants are somewhat more impatient and risk averse than the white participants. This non-experimental comparison is 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). To learn about identity effects, we instead turn to the comparison 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 immediately 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% level. In contrast, whites seem to get slightly more impatient in the ethnicity-salience condition, but the difference is not significant. Neither whites nor Asians change their risk choices in response to the ethnicity prime.

Table 2 presents formal regression evidence on priming effects. We regress participants' reservation log interest rate and risk premium on experimental condition and trade-off type. Column 1 confirms the result from the first table: 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 × 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. Columns 2 and 4 show, in analogous regressions for white subjects, that whites' choices are not affected by the prime.[13]

[13] It would be interesting to examine whether the priming effect is particularly strong for Asians who have recently immigrated. In a regression that includes an interaction between identity salience and an indicator for having lived in the U.S. for fewer than 2 generations, the interaction term is insignificantly negative (we continue to find a significant main effect of identity salience). 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. Therefore, while the negative interaction coefficient is consistent with identity salience increasing

IV. Experiment 2: Black Racial Norms and Gender Norms

While Experiment 1 focused on Asian ethnic category norms, Experiment 2 explores how preferences are affected by black racial category norms and gender category norms. Sowell (1975, 1981, 2001) and Murray (1984) have argued that black category norms encourage impatient behavior. Yankelovich Partners Inc. (1999) describes a "culture of conservatism" among higher-income blacks with regards to investing, which accords with Sahm's (2007) finding that, controlling for demographics, blacks in the Health and Retirement Study are significantly more risk averse over hypothetical wealth gambles than whites. If identity-related differences in time or risk preferences explain why black Americans accumulate less financial wealth (Altonji, Doraszelski, and Segal, 2000), accumulate less human capital (Neal and Johnson, 1996; Fryer and Levitt, 2004), and are less likely to invest in the stock market (Hurst, Luoh, and Stafford, 1998) than white Americans, then we expect that priming the racial identity category among blacks would increase discount rates, increase risk aversion, or both.[14] That is, in terms of the model in Section II, we hypothesize that for blacks, $\delta_C > \delta_0$ and $\rho_C > \rho_0$ where δ is the discount rate and ρ is risk aversion.

Immigrant blacks—whom we define as blacks who were born abroad or who have at least one parent who was born abroad—comprise a substantial minority (41%) of our black subjects. Sociological research indicates that blacks whose families have recently immigrated to the U.S. grow up with a very different cultural heritage than blacks whose families have long-standing U.S. roots (e.g., Waters, 1994). Because 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) and often identify themselves in contrast to American-born blacks (Waters, 1994), we examine whether the effects of priming race on immigrant blacks differ from the priming effects on blacks with long-standing U.S. roots.

Finally, because women invest in more conservative financial assets than men (Jianakoplos and Bernasek, 1998; Sundén and Surette, 1998) and act more cautiously in

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patience more strongly among recent Asian immigrants, it is also consistent with recent Asian immigrants simply being more patient at baseline (even when not primed).

[14] On the other hand, blacks seem to be more likely to engage in risky health behaviors than whites (Hahn, Vesely, and Chang, 2000), perhaps suggesting that black identity is associated with a risk-seeking norm, at least in the health domain.

laboratory experiments (Croson and Gneezy, 2004; Byrnes, Miller, and Schaefer, 1999), we test whether priming gender causes women to become more risk averse and men to become less risk averse.

Experiment 2 also expands on the earlier experiment by measuring larger-stakes (in addition to small-stakes) risk preferences and by asking a host of questions that enable us to test potential mechanisms underlying the category-salience effects. In addition, we introduce variation in the delay between the salience manipulation and the preference elicitations, which allows us to investigate the impulse response function of a category-salience shock.

A. Participants

We recruited 280 Temple University students and 231 University of Michigan students. In order to avoid pre-priming participants with their racial identity category, we did not at any point mention that we were looking for black and white subjects. There were 128 black subjects, 296 non-Hispanic white subjects, and 87 subjects who were neither black nor non-Hispanic white. Among our participants, 44% were male.

B. Procedure

We randomly assigned participants to the race-salience, gender-salience, or control conditions. Because of the scarcity of black subjects, we did not assign any black participants to the gender-salience condition.[15]

The first section of the questionnaire contained the category-salience manipulation or control questions. 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 measurement of the subject's anxiety (1.5 minutes). These three sections' order varied across sessions. The penultimate section was a math quiz with six SAT-like questions. The 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. Participants were paid for their choices, plus a $1 show-up fee, by check immediately upon completing the experiment.

[15] The Michigan sessions were conducted by various experimenters of white, black, Hispanic, and Asian descent and both genders. Unfortunately, we have little power to test directly for experimenter race and gender effects.

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.

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 varied from \$10.10 to \$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.

One question in this section was randomly chosen for payment, made by checks given to subjects immediately after the session. Delayed payments were implemented via post-dated check, which participants were told can be cashed any time on or after the check's date.[16] 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. 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* varied from \$1.60 to \$3.60 in ascending order. Participants knew they would be paid according to *every* choice they made in the small-stakes risk section. Any money the participant earned in this section would be paid with a check that could be cashed immediately.

We measured larger-stakes risk preferences with analogous choices, except that the monetary amounts were multiplied by 100. For example, the first question gave a choice between \$100 for sure and a 50% chance of winning \$160. The section's instructions explained that we would pay the participant for a randomly selected question in the section *if* the participant could correctly guess (via written prediction) two roulette wheel spin outcomes which would take place later in the session.[17]

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): participants rate on a four-point numerical scale how much they feel (a) calm, (b) tense, (c) upset, (d) relaxed, (e) content, (f) worried. The numerical sum of (a), (d), and (e) answers are subtracted from the sum of (b), (c), and (f) answers to compute a score that increases with anxiety.

[16] 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 34 Temple subjects who deposited their checks more than one business day before the check's date, our results are unchanged. We find that Temple subjects who chose more patiently in the experiment also took longer to deposit their checks. A similar analysis of Michigan subjects is impossible because of how we ensured anonymity there.

[17] Since each roulette wheel spin has 38 possible outcomes, the probability that a participant would be paid for his or her choice was $(1/38)^2 = 1/1444$. Therefore in terms of expected value, our "larger-stakes" risk questions were actually played for smaller stakes than our "small-stakes" questions. Our terminology reflects the fact that, under expected utility theory, choices with larger monetary outcomes should reflect curvature of the utility function over larger amounts of money, regardless of the probability that the choice will be implemented.

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 was a background questionnaire that also included questions unrelated to the study to disguise the study's purpose. 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.

We asked a series of questions about participants' beliefs about norms for their race or gender, and how strongly the participant identified with his or her race and gender. We will discuss these questions further in Section IV.E.

Finally, we asked about the credibility of our payment promises because we had an *ex ante* belief that subjects who did not think their choices were incentive-compatible would make low-quality decisions or act strategically (rather than report their true preferences). 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?" 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?"

C. Econometric Methodology

As in Experiment 1, the dependent variables we are interested in identifying 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 observe two r intervals and two π intervals for each participant. We pool the two r intervals or the two π intervals and add dummy explanatory variables that indicate in which trade-off type (now versus one week, one week versus two weeks, small gamble, large gamble) the r or π interval was observed. In addition, we control for the school at which the subject was recruited, as well as an interaction between the school and trade-off type. Standard errors are clustered by individual.

For the race-salience analysis, we drop participants who are neither non-Hispanic white nor black. For the gender-salience analysis, we drop participants from the control group who are black, since no black subjects received the gender-salience treatment.

D. Main Results

As expected due to randomization, the summary statistics in Table 3 show that participants generally appear similar across conditions once we control for university attended.[18] The fifth and sixth rows of the table suggest that the category salience manipulation did not affect belief in our payment promises (except possibly for men). However, the level of belief was strikingly low. Between 35% and 53% of subjects within a demographic group × experimental condition cell reported either not believing that their choices would affect their payment or not believing that deferred payments would actually be received. Experimental economists have long thought that laboratory choices have low validity unless subjects' monetary payoffs depend upon their choices. In our case, subjects' payments did in fact depend on their choices, but subjects with incorrect beliefs about our promises may have behaved as if there were no relationship between choices and payoffs. Therefore, we drop from our regressions 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 examine at the end of this subsection the impact of retaining these skeptics in the sample.

In total, each subject in Experiment 2 made 24 intertemporal choices (pooling across horizons) and 12 risk choices (pooling across stake sizes). Table 4 displays, among subjects who pass our belief filters, the average percent of these respective choices where subjects chose the earlier or safe payment. As in Experiment 1, our student subjects are a highly selected population, and this is reflected in their baseline choices. Blacks are on average more risk averse than whites in nationally representative data (on middle-aged and older Americans; Sahm, 2007),

[18] We control for university because the proportion of Michigan students in each experimental group is not equal. To be clear that they are not driving our results, we have dropped from our sample 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—clearly differed from the rest of our sample along many dimensions. Our priming results are unchanged if we include these four subjects.

whereas our unprimed native black subjects are on average less risk averse than our white subjects. We instead identify identity effects by comparing the behavior of unprimed participants with the behavior of primed participants.

Although Table 4 discards all information about the prices involved in each trade-off, the main result of Experiment 2 is apparent: native blacks choose the safe payment significantly more often under race salience. In contrast, immigrant blacks and whites, if anything, choose the safe payment *less* often under race salience. These results are consistent with the hypothesis that identity effects play a role in native blacks' reluctance to invest in high-expected-return risky assets.

Table 5A presents formal regression evidence on the baseline category priming effects in Experiment 2. We see that making race salient to native blacks raises their required risk premium by 23 percentage points. 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. We also find no significant white identity risk aversion effect. The native black priming effect on risk aversion is statistically different from the white and immigrant black priming effects (both *p*-values < 0.01).

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 priming effects decay. Keeping in mind that the standard errors of our estimates are large since we are dividing our sample roughly in thirds, we find no evidence that the native black priming effect on risk aversion decays over the course of the experimental session. The risk premium gap between control and primed native blacks is 18, 20, and 33 percentage points, respectively, at 0, 5, and 7 minutes after the prime (the times the risk preference elicitations began); the difference between the 0 and 7 minute priming effect is insignificant, with a *t*-statistic of only 0.8.

Priming gender does not appear to differentially affect men's and women's average risk aversion (although we find in Section IV.E below that priming gender causes both men and women to conform to what they believe their own gender norms to be). Priming social category appears to have caused all groups we tested to become more patient (though only statistically significantly for whites), perhaps suggesting that a low discount-rate norm is common to all of these categories. However, because we do not find statistically distinguishable differences in the priming effect across groups, we conclude that identity effects on discount rates do not contribute to the capital accumulation gap between blacks and whites.

Other studies have shown that without financial incentives, experimental participants behave more randomly and exert less effort (see Camerer and Hogarth, 1999, for a literature review). Our experiment adds to this body of evidence. 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. Respectively, 7% and 21% of participants who did not believe our payment promises failed to answer the intertemporal and risk questions in a well-behaved manner, compared with 5% and 16% of believers. Non-believers also behaved substantially more impatiently and cautiously (results not shown in tables), which is consistent with their exerting less cognitive effort (Benjamin, Brown, and Shapiro, 2006).

Table 5B shows the interest rate and risk premium regression coefficients when we keep non-believers in the sample. The main results—the priming effect on risk aversion for native blacks and the difference between the priming effect on native and immigrant blacks—become weaker relative to Table 5A and lose statistical significance. This is because non-believers are more cautious, and they were disproportionately (but not statistically significantly) randomized into the control condition among native blacks and into the category salience condition among immigrant blacks (see Table 3). Because the non-believers appear to have taken the preference tasks less seriously, we emphasize the results that exclude them. The sensitivity of statistical significance to the exclusion of non-believers warrants caution, but the overall evidence suggests that experimenters may be able to increase statistical power and reduce bias by asking their subjects *ex post* about the credibility of the study's payment promises and dropping those who were skeptical.

E. Within-Group Heterogeneity in Category Norms and Affiliation Strength

Proposition 3 shows that the magnitude of the priming effect, $dx^*/ds = w'(s)(x_C - x_0)$, depends on the level of x_C. Thus, the theory predicts that *ceteris paribus*, the priming effect will be larger for individuals with larger x_C, and that if x_C is sufficiently heterogeneous, priming will move people at opposite extremes of the x_C distribution in opposite directions. If a given set of beliefs—such as stereotypes—about a category are a source of category norms, then there should be a positive interaction effect between the priming treatment and those beliefs.

We also report in this subsection the interaction between the priming effect and strength of category affiliation, d^2x^*/ds^2 , even though Proposition 4 indicates that the sign is theoretically ambiguous.

Conformance to perceived stereotypes. 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 $p < 0.01$), 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 frugal, more impatient, less patient, less cautious, and more reckless (Mann-Whitney tests, all $p < 0.01$).[19]

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. The more common the subject believes patient stereotypes are, the higher this index value. We create an analogous index for risk-averse stereotypes by subtracting the participant's rating of "reckless" from the rating of "cautious" and standardizing. The risk-averse index value increases with the perceived prevalence of risk-averse stereotypes.

We regress the reservation log interest rate or risk premium on a constant, a treatment dummy, a stereotype belief index, the interaction between the treatment dummy and the stereotype belief index, a trade-off type dummy, a school dummy, and the interaction between

[19] Black subjects' responses are not included in these gender stereotype comparisons, since no black subjects are in the gender priming analysis.

the trade-off type and school dummy. The primary coefficients of interest are the interaction effects of stereotype beliefs with the treatment dummy.[20]

The results suggest that the stereotypes we measure do not affect racial category norms. Panels A and B in Table 6 show that stereotype beliefs do not significantly alter the priming effect on the reservation interest rate and risk premia for any of the racial categories.

Unlike for race, stereotypes appear to play an important role for gender norms, perhaps because gender stereotypes are considered more socially acceptable and valid than racial stereotypes. 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 (Columns 4 and 5 of Table 6's Panel B). The size of this interaction effect is large: a one standard deviation increase in the risk-averse stereotype index is associated with a 16.6 percentage point increase in the gender prime's risk premium effect among men and a 7.6 percentage point increase among women. Using the aggregated stereotype index, this interaction is statistically significant for men but not for women. Separately analyzing the components of the risk-averse stereotype index (not shown), we find that the interaction effects are driven by beliefs about the "cautious" stereotype for men (significant at the 5% level) and the "reckless" stereotype for women (significant at the 10% level).

This interaction effect between priming and gender risk stereotypes decays over time more quickly than the main effect of priming on native blacks' risk aversion. Examining the size of the "cautious" standardized stereotype interaction for men and "reckless" standardized stereotype interaction for women,[21] we find that the coefficient goes from 20.6 to 14.7 to 5.1 percentage points for men and from 21.6 to 21.2 to -12.1 percentage points for women as 0, 5, or 7 minutes passed between the end of the gender prime and the start of the risk preference elicitation. (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 10% level for women.

Normative childhood messages and traditional gender roles. Besides societal stereotypes, another possible source of category norms is explicit normative messages. To explore this, we

[20] Since we measured stereotype beliefs after manipulating category salience, cognitive dissonance could generate a correlation between stereotype beliefs and the *level* of the reservation log interest rate or risk premium. However, this simple cognitive dissonance mechanism would not bias our key coefficient of interest, the interaction term.

[21] 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.

asked Michigan subjects the following question in the questionnaire's final section: "As children, we constantly receive messages from parents, teachers, and society about how we *should* behave (whether or not we actually behave that way). How commonly do you think **white** [**black / male / female]** children receive messages that they should behave in the following ways?" The messages subjects rated were the same as the stereotypes we asked about: generous, lazy, frugal, impatient, studious, cautious, artistic, patient, and reckless.

Although our statistical power is limited due to the small sample, these results (not shown in tables) are broadly consistent with those obtained in the stereotype prevalence regressions. Beliefs about childhood norms do not appear to affect racial category norms. Men who believe boys are frequently given messages to be risk averse become relatively more risk averse when primed, and this interaction is driven by the "cautious" message (the cautious interaction's *p*-value is 0.06). Women who believe girls are frequently given messages to be risk averse also become relatively more risk averse when primed, although this interaction is not statistically significant.

To see if attitudes towards traditional gender roles influenced the gender-salience effect, we asked Temple subjects to indicate their agreement with four statements about traditional gender roles.[22] We find no significant interactions between agreement with traditional gender roles and the gender priming effect (not shown in tables).

Identification strength. Recall from Proposition 4 that it is theoretically ambiguous whether a given category-salience effect will be stronger or weaker for individuals who identify more strongly with the primed category. Nonetheless, for completeness we report these interaction effects here for the significant priming effects that we found previously. We measured racial and gender identification strength using questions from the "private collective self-esteem subscale" (Luhtanen and Crocker, 1992), a standard psychological instrument for measuring identity affiliation. We do not find that identification strength affects the native black risk aversion priming effect. Nor do we find that it affects the interaction between beliefs about female risk-averse stereotypes and the priming effect on women's risk aversion. (In both cases, point

[22] The statements were, "The man should always pay for the first date between a man and a woman," "A pre-school child is likely to suffer if his/her mother works outside the home," "Men shouldn't cry," and "Ultimately, the husband is responsible for making sure the family is financially secure." The second statement is taken from the 1970 National Fertility Study.

estimates are near zero; results are not shown in tables.) However, for men, stronger gender identification greatly attenuates the interaction between beliefs about male risk-averse stereotypes and the priming effect on men's risk aversion ($p = 0.013$; results not shown in tables).

F. Alternative Explanations

In this subsection, we consider alternative explanations for why our priming manipulations caused changes in time and risk preferences in Experiment 2.

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 (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 (see also Marx and Stapel, 2006b). 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 vanish when tasks are presented to subjects as not being diagnostic of ability (e.g., Steele and Aronson, 1995; Walton and Cohen, 2003).

A possible explanation for our results is that the category primes induced stereotype lift among Asians in Experiment 1, improving their ability to compute expected values and interest rates, and stereotype threat among native blacks in Experiment 2, impairing their cognitive ability, which may lead to more risk-averse behavior (Benjamin, Brown, and Shapiro, 2006). 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 were not being driven by stereotype threat, stereotype lift, or emotional changes, we examine how the treatment affected performance on the five SAT Math-like questions administered after the elicitations and responses to the shortened version of the Spielberger State-Trait Anxiety Index (a standard psychometric measure of anxiety).[23] The primes had no effect on math quiz performance for whites, blacks, and women (results not shown in tables). Anxiety for all groups is also unaffected. Although the gender prime does seem to decrease math quiz performance among men who believe risk-averse stereotypes about their gender are relatively more common, this relationship does not explain the male risk-averse stereotype interaction effect on risk premia. That interaction effect in fact strengthens after controlling for anxiety, SAT math score, and math quiz score (the coefficient is 0.199, $p = 0.037$; not reported in tables).

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. This seems unlikely because participants were unaware that the first section of the questionnaire (which contained the race prime, the gender prime, or the identity-neutral control) varied across participants.

Nonetheless, in the Michigan sample, we asked directly about what motivated participants' choices. In the final questionnaire section, we asked, "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 the purpose of the experiment (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 generally strengthen when we drop subjects who report thinking about what the experimenter wanted them to do.

Type I error. We tested many hypotheses using our data. Even in the absence of any true priming effects, we would expect that 5% of regressions would reject the null of no priming effects.

[23] Although some of 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.

One way to ascertain the likelihood of our results being driven by Type I error is to see if the priming effects are consistent in both schools. Our Michigan data were collected after the Temple data had been initially analyzed and so provide something of an out-of-sample test.

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 0.070 at Michigan.

The signs of the gender interaction results are also consistent across the Temple and Michigan samples, but they are less robust than the race priming results. The interaction between men's beliefs about male cautious stereotypes and the gender priming effect at Temple has a coefficient of 0.213 ($p = 0.038$), whereas the Michigan interaction coefficient is 0.052 ($p = 0.761$). The interaction between women's beliefs about female reckless stereotypes and the gender priming effect on female risk aversion is 0.128 ($p = 0.197$) at Temple and 0.035 ($p = 0.823$) at Michigan.

V. Conclusion

Our findings suggest that social identity matters for 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 did not affect time preference relative to whites, but it increased risk aversion among those who had longstanding roots in the U.S. and weakly decreased risk aversion among those whose family had recently immigrated to the U.S. Making gender salient caused both men and women to adhere more closely to the risk norms they hold about their own gender. Overall, our results support the view that identity effects contribute to the differences between U.S. demographic groups in economic behaviors and outcomes.

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. For example, 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.[24] An American-born black worker who is enrolling in his 401(k) may, due to a transitory racial prime, choose a more conservative asset allocation. Even though his risk aversion was only temporarily heightened, a large body of empirical evidence has shown that most households' retirement savings decisions are highly inertial (Samuelson and Zeckhauser, 1988; Madrian and Shea, 2001; 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.

In our experiments, we varied primes exogenously in order to begin to understand the relationship between social identity and preferences. In actual markets, interested parties such as sellers, employers, politicians, and churches have an incentive to manipulate the 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 (Becker and Mulligan, 1997; Fang and Loury, 2005; Bénabou and Tirole, 2006), 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.

[24] Berger, Meredith, and Wheeler (2006) find that voters who vote in a school are more likely to vote to raise taxes to support education.

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Table 1. Percent of Impatient or Safe Choices, Experiment 1

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.

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

Table 2. Ethnicity-Salience Treatment Effect on
Asian and White Log Interest Rate and Risk Premium, Experiment 1

This table presents interval regressions where the latent 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 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. $\hat{\sigma}$ is the estimated conditional standard deviation of the latent dependent variable. 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.

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

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

Table 3. Summary Statistics, Experiment 2

This table reports summary statistics for the subjects in each experimental condition. "Social category salient" refers to the race-salience treatment (first three columns) or the gender-salience treatment (last two columns). In order to test for differences between the control and treatment groups, we run an OLS regression of each variable of interest on a treatment dummy, an indicator for recruitment location, and a constant. The *p*-values reported are for the treatment dummy coefficients. "Believed choices mattered" is the percent of subjects who believed their experimental choices would affect their payments. "Also believed deferred payment promise" is the percent of subjects who believed the above and believed that deferred payment promises were credible. The last row reports the number of subjects in each demographic group. Some statistics are calculated using fewer subjects because of non-response.

		Whites	Native blacks	Immigrant blacks	Men	Women
Age (mean)	Control	20.0	19.3	19.5	20.1	19.8
	Social category salient	19.6	19.9	19.8	20.0	19.6
	p-value of difference	0.289	0.207	0.437	0.747	0.892
SAT I Math score (mean)	Control	632.9	532.6	551.4	665.6	616.0
	Social category salient	606.7	529.5	534.3	615.3	614.5
	p-value of difference	0.113	0.707	0.450	0.058	0.995
SAT I Verbal score (mean)	Control	624.1	523.7	559.5	623.9	624.7
	Social category salient	622.6	583.0	567.1	605.3	606.9
	p-value of difference	0.791	0.112	0.915	0.661	0.288
Household income > $80,000 (%)	Control	64.1%	26.9%	36.0%	63.4%	61.4%
	Social category salient	61.5%	31.6%	36.0%	55.3%	49.0%
	p-value of difference	0.810	0.914	0.962	0.430	0.218
Believed choices mattered (%)	Control	84.5%	77.8%	82.1%	85.4%	82.1%
	Social category salient	83.9%	86.4%	68.0%	72.3%	76.0%
	p-value of difference	0.880	0.466	0.243	0.053	0.415
Also believed deferred payment promise (%)	Control	64.3%	51.9%	57.1%	64.6%	61.9%
	Social category salient	57.0%	63.6%	48.0%	48.9%	56.0%
	p-value of difference	0.272	0.270	0.534	0.074	0.544
N		222	71	53	129	134

Table 4. Percent of Impatient or Safe Choices, Experiment 2

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. The penultimate row in each panel shows *p*-values of the *t*-test for equality of means in those percentages between the social category salient and control conditions. The final row in each panel shows the number of subjects.

Panel A: Percent of choices that were impatient					
	Whites	Native blacks	Immigrant blacks	Men	Women
Control	44.58	60.12	51.43	50.31	36.62
	(29.69)	(25.30)	(25.60)	(29.26)	(28.28)
Social category salient	33.57	51.93	38.42	46.38	33.90
	(32.39)	(33.50)	(18.70)	(29.47)	(27.90)
p-value of difference	0.0402	0.4325	0.1757	0.3802	0.6081
N	136	42	28	76	80

Panel B: Percent of choices that were safe					
	Whites	Native blacks	Immigrant blacks	Men	Women
Control	51.53	43.25	50.00	49.64	49.52
	(21.34)	(12.25)	(19.78)	(20.78)	(21.34)
Social category salient	47.65	57.02	38.73	44.61	43.92
	(21.36)	(18.54)	(16.12)	(22.83)	(19.86)
p-value of difference	0.2068	0.0083	0.0783	0.1782	0.1236
N	187	59	40	104	107

Table 5A. Baseline Category-Salience Treatment Effects, Experiment 2

This table presents interval regressions where the latent 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. *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. $\hat{\sigma}$ is the estimated conditional standard deviation of the dependent variable. 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.

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

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

Table 5B. Baseline Category-Salience Treatment Effects
Including Subjects Skeptical About Payments, Experiment 2

This table presents interval regressions where the latent dependent variable is the log interest rate required to defer payment receipt or the risk premium required to accept a gamble. The samples include subjects who did not believe that our payment promises were credible. 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. $\hat{\sigma}$ is the estimated conditional standard deviation of the dependent variable. 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.

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 ×	0.0275	0.3317	0.6190	0.3867	-0.4079
UMich	(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 ×	-0.0724	0.2454	0.1215	-0.0842	0.0812
UMich	(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

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

Table 6. Category-Salience Interaction Effects with Stereotype Prevalence Beliefs, Experiment 2

This table presents interval regressions where the latent 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. *Social Category Salient* is a dummy for the race-salience treatment in the first three columns or the gender-salience treatment in the last two columns. *Patient Stereotype* is the extent to which the subject believes "patient" and "frugal" stereotypes are common and "impatient" stereotypes are uncommon about his or her race (first three columns) or gender (last two columns). *Risk-Averse Stereotype* is the extent to which the subject believes "cautious" stereotypes are common and "reckless" stereotypes are uncommon about his or her race (first three columns) or gender (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. $\hat{\sigma}$ is the estimated conditional standard deviation of the dependent variable. *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. 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.

Panel A: Log interest rate					
	Whites	Native blacks	Immigrant blacks	Men	Women
Social Category Salient	-0.6937*	-0.6591	-0.4494	-0.3915	-0.2069
	(0.3224)	(0.3678)	(0.3327)	(0.4144)	(0.4316)
Social Category Salient × Patient Stereotype	0.0641	0.6329	-0.0758	0.2647	-0.0535
	(0.3437)	(0.3426)	(0.3332)	(0.3718)	(0.3787)
Patient Stereotype	-0.0499	-0.4552**	0.0573	-0.0952	0.2059
	(0.2469)	(0.1715)	(0.2507)	(0.2991)	(0.2604)
1 Week vs. 2 Weeks	-0.1964	-0.0969	0.0963	-0.4495	0.1798
	(0.1471)	(0.2216)	(0.3599)	(0.2331)	(0.2267)
UMich	-0.3468	-0.0157	-0.0105	-0.7377	0.0667
	(0.3088)	(0.5387)	(0.6298)	(0.4699)	(0.4118)
1 Week vs. 2 Weeks × UMich	0.0280	0.1984	0.9008	0.2999	-0.4346
	(0.2189)	(0.2723)	(0.5968)	(0.2928)	(0.2865)
Constant	-2.3968**	-1.6802**	-2.4105**	-1.8432**	-2.9935**
	(0.2335)	(0.2587)	(0.3587)	(0.3131)	(0.3636)
$\hat{\sigma}$	1.7806	1.3636	1.1063	1.5297	1.6953
	(0.1300)	(0.2145)	(0.1548)	(0.1831)	(0.1736)
N	270	78	52	124	128

Panel B: Risk premium					
	Whites	Native blacks	Immigrant blacks	Men	Women
Social Category Salient	-0.0615	0.2702**	-0.1284	-0.0889	-0.1145
	(0.0513)	(0.0886)	(0.0957)	(0.0726)	(0.0691)
Social Category Salient × Risk-Averse Stereotype	-0.0037	-0.1185	-0.0316	0.1660*	0.0762
	(0.0550)	(0.0962)	(0.1296)	(0.0811)	(0.0863)
Risk-Averse Stereotype	0.0221	0.0300	0.0482	-0.0577	0.0263
	(0.0349)	(0.0585)	(0.1144)	(0.0555)	(0.0683)
Larger Stakes	0.3103**	0.0341	0.1087	0.2998**	0.0942
	(0.0490)	(0.0990)	(0.0746)	(0.0657)	(0.0662)
UMich	-0.0045	0.0867	0.0755	0.0074	-0.1851*
	(0.0495)	(0.1441)	(0.1015)	(0.0830)	(0.0747)
Larger Stakes × UMich	-0.0304	0.2323	0.2394	-0.1186	0.1467
	(0.0678)	(0.1830)	(0.2489)	(0.0857)	(0.1114)
Constant	0.2087**	0.0234	0.1066	0.2114**	0.3077**
	(0.0410)	(0.0862)	(0.0918)	(0.0514)	(0.0591)
$\hat{\sigma}$	0.3987	0.4358	0.3526	0.3839	0.3914
	(0.0195)	(0.0415)	(0.0516)	(0.0264)	(0.0292)
N	372	102	76	172	172

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