Social Identity and Preferences*

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Abstract

In two laboratory experiments, we find that norms associated with one's social identity affect time and risk preferences. When we make ethnic identity 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 is relatively more common. Our ethnic and racial identity results are broadly consistent with U.S. demographic patterns in economic behaviors and outcomes.

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I. Introduction

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

Some social scientists have argued that these demographic differences result from differences in preferences induced by norms that are a part of these groups' social identity (Sowell, 1975, 1981, 2005; Murray, 1984; Chiswick, 1983; Barke, Jenkins-Smith, and Slovic, 1997). For example, Sowell (1975) 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."

Unfortunately, the social identity hypothesis is difficult to test empirically. Social identity is confounded with many other factors such as socioeconomic status and peer pressure (Austen-Smith and Fryer, 2005; Fryer and Torelli, 2005). Exogenous variation in social identity that permits definitive causal inference is rare in field data.

Social psychology offers a methodology for introducing such exogenous variation. "Self-categorization theory," a long-standing idea in psychology (e.g. James, 1890; Turner 1985), posits that people have multiple social identities—race, ethnicity, gender, occupation, etc.—each

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

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

associated with a set of behavioral norms. Naturally occurring environmental cues (or "primes") can temporarily make certain identities more salient, causing the person's behavior to tilt more toward the norm associated with the salient identity. Section II proposes a theoretical framework, inspired by Akerlof and Kranton (2000), for understanding identity and priming effects.

If the self-categorization theory is valid, then one can identify the effect of a particular identity on preferences by exogenously varying the salience of that identity and seeing how people's preferences change. We perform such a manipulation in the laboratory by asking subjects questions about their family background or questions about living with individuals of different races or genders. Control subjects were instead asked neutral questions unrelated to identity. We then elicit subjects' time and risk preferences using standard incentive-compatible mechanisms.

Section III describes our first experiment, in which we make Asian ethnic identity salient to Asian-American subjects by asking them about their family background. Because a patience norm is commonly believed to be associated with Asian identity (Kasindorf, 1982; Abboud and Kim, 2005), we expected that making ethnicity salient to Asian-American subjects would cause them to behave more patiently. Indeed, we find that these primed Asian-American subjects make more patient choices than Asian-American control subjects, requiring a much lower interest rate for delaying receipt of payment. The ethnicity prime does not affect Asians' risk aversion. Asking about family background also has no effect on white subjects' preferences. Our first experiment's findings suggest that identity effects on discount rates play a role in the high financial and educational investment rates found among Asian-Americans.

In our second experiment, described in Section IV, we make racial identity salient to white and black subjects by asking questions about living with individuals of different races. The experiment tests whether identity effects might help explain why whites accumulate more capital and invest more in stocks than non-immigrant blacks. High risk aversion will reduce stock investment. Either high discount rates or high risk aversion will reduce long-run capital accumulation through their impact on savings rates or allocations to assets that command a positive risk premium, respectively.

Perhaps surprisingly, we do not find significant identity-related discount rate differences between 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 over money 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 non-immigrant blacks' capital accumulation and stock market participation.

We also make gender identity salient to a group of our subjects in the second experiment by asking questions about living in single-sex or co-ed dormitory floors. Although we find no mean effect from priming gender identity, we find significant interaction effects on risk preferences. Priming gender increases risk aversion among men who believe that cautious stereotypes about men are relatively more common. Priming gender decreases risk aversion in women who believe that reckless stereotypes about women are relatively more common. These effects reverse for those who hold opposite beliefs about the stereotypes. Analogously, we find that gender-primed subjects conform to the risk-aversion norm they believe children of their gender are told they should adhere to. Gender-primed women also conform to the patience norm they believe girls are told they should adhere to. This evidence shows that identity-related norms need not be homogeneous within an identity group to have a behavioral effect, and these norms need not even be seen as admirable (e.g. recklessness).

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

We interpret our results in light of the psychology literature on identity salience, according to which priming identity causes individuals to behave in conformance with socially prescribed preferences. However, a potential alternative interpretation comes from the psychology literature on "stereotype threat," which argues that priming identity can cause members of disadvantaged groups to become anxious, disrupting cognitive processing and impairing performance on standardized tests (e.g., Steele and Aronson, 1995; Shih, Pittinsky, and Ambady, 1999; Hoff and Pandey, 2006). Conversely, "stereotype lift" increases cognitive

performance of a group when negative stereotypes about *other* groups are made salient (Walton and Cohen, 2003). A necessary condition for stereotype threat and lift effects is that subjects perceive the task to be diagnostic of ability (Croizet and Claire, 1998; Aronson, Quinn, and Spencer, 1998; Kray, Thompson, and Galinsky, 2001). Because we present our preference elicitations as "a matter of personal preference, " we did not expect stereotype threat or lift to occur. Indeed, we do not find evidence of stereotype threat or lift effects. In our second experiment, subjects answered five SAT-style math questions after their preferences were elicited. Primed subjects did not perform differently than unprimed subjects.

Our work contributes to a growing economics literature on how social identity affects economic behavior. Most of this work to date has been theoretical (Akerlof and Kranton, 2000, 2002, 2005; Bénabou and Tirole, 2006). We argue that identity salience experiments can be an important methodology for testing these ideas. Section V concludes the paper, and an Appendix generalizes the analysis from Section II.

II. A Theoretical Framework

In this section, 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 identity reveals the marginal effect of increasing the strength of affiliation with that identity.

Let x be some decision variable, such as how much to delay gratification or how much to take risks. An individual holds some identity I, such as black racial identity or female gender identity, with strength $s > 0$. Let x_N denote the optimal choice of x without identity considerations, and let x_I denote the norm associated with identity I—that is, the choice of x prescribed by I. The individual chooses x to maximize

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

where $w(s)$ is the weight placed on identity I in the person's decision. We assume that $w(0) = 0$ and $w' > 0$. Deviating from the norm prescribed for one's identity causes disutility that is increasing in s, the strength of one's affiliation with that identity. For simplicity, we analyze the case of a single identity I, but it would be straightforward to add terms to the utility function reflecting other identities the individual holds.

We suppose that s has a steady-state value $\bar{s}$ but can be temporarily perturbed away from $\bar{s}$ by naturally occurring environmental identity primes ε; for example, s might follow an AR(1) process, $s = (1-\phi)s_{-1} + \phi\bar{s} + \varepsilon$.

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

$$x^*(s) = \frac{x_N + w(s)x_I}{1 + w(s)}, \tag{2}$$

which is a weighted average of the optimal choice without identity considerations and the identity norm. This condition has several important implications. First, the higher the steady-state strength $\bar{s}$ of the identity, the closer x^* is to x_I. Second, an identity prime $\varepsilon > 0$ (whether naturally occurring or experimentally induced) also causes x^* to move closer to x_I. Thus, the behavioral effect of priming identity I reveals the marginal behavioral effect of increasing the steady-state strength $\bar{s}$ of identity I. This is why priming manipulations are a useful experimental procedure for studying identity effects. Third, the derivative

$$\frac{dx^*}{ds} = \frac{w'(s)(x_I - x_N)}{(1 + w(s))^2}, \tag{3}$$

depends on the sign of $x_I - x_N$. 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_N, directional results from priming experiments with college students will generalize as long as $x_I - x_N$ has the same sign on average for both groups.

Is the priming effect weaker or stronger for individuals who feel a stronger (steady-state) identity affiliation? Many psychologists have expressed an intuition that priming should have a stronger effect on those who identify more strongly with their identity. For example, LeBouef and Shafir (2006, p.18) hypothesize that "the elicitation of a given social identity will lead to preference assimilation for those high, but not low, in identification with that identity." In our framework, for an individual with $x_N < x_I$, this hypothesis of increasing sensitivity to priming corresponds to the condition $\frac{d}{ds}\left(\frac{dx^*}{ds}\right) > 0$. Our formal framework implies two interesting and perhaps surprising conclusions about the interaction between priming and strength of identity affiliation.

First, in general it is ambiguous whether the priming effect is stronger or weaker for individuals with a stronger identity affiliation.

Proposition 1: Suppose $x_N < x_I$. Then $\frac{d^2x^*}{ds^2} > 0$ if and only if $\frac{w''(s)(1+w(s))}{(w'(s))^2} > 2$.

Proof. Straighforward calculation.

Depending on the shape of $w(\bullet)$ and the level of s, d^2x^*/ds^2 could take either sign. For that reason, even though we report interaction effects between priming effects and identity strength, we do not emphasize those empirical results.

Second, for $w(\bullet)$ functions in a broad class, d^2x^*/ds^2 has an unambiguous sign for sufficiently high levels of identity strength. The priming effect will be *weaker* for individuals with a stronger identity affiliation, which is the opposite of what is often assumed.

Proposition 2: Suppose $x_N < x_I$. If $w(s)$ is an algebraic function[3] that is strictly increasing, then there exists an $\hat{s}$ such that $\frac{d^2x^*}{ds^2} < 0$ for all $s > \hat{s}$.

Proof. See the Appendix, where we prove this result for a generalization of utility function (1).

To understand this result intuitively, recall that a person's action x increases monotonically toward the identity norm x_I as s approaches infinity. Hence even if $x^*(\bullet)$ is a convex function of s for small s, it must become concave for large enough s, or else $x^*(s)$ would exceed x_I, which is impossible.

Finally, because it appears to be empirically relevant for the effect of priming race on blacks' time preference, we note that the effects of priming can be subtle if there is population heterogeneity. Some American-born blacks have a high-patience norm x_I^H, while others have a

[3] Loosely speaking, an algebraic function is constructed from a finite number of additions, subtractions, multiplications, divisions, and exponentiations. Functions that are not algebraic are transcendental (like the sine function). The assumption that $w(s)$ is an algebraic function rules out the possibility that $x^*(s)$ becomes convex infinitely often over increasingly small intervals of s.

low-patience norm $x_I^L < x_I^H$. However, baseline (unprimed) behavior for these two groups is even more extreme, implying that $x_N^H > x_I^H$ and $x_N^L < x_I^L$. In cases like this one, the population average effect of priming could go either direction, but priming unambiguously causes convergence in behavior toward x_I.

III. Experiment 1: Asian-American Ethnic Identity

Norms for patient behavior seem to be linked to many Asian ethnic identities. American stereotypes about East Asian patience and industriousness date back to at least the 19th century (Twain, 1872)[4] and persist to this day (e.g., Kasindorf, 1982; Abboud and Kim, 2005). Although there are differences between Asian cultures, Hofstede and Bond (1988) argue that most are high in "Confucian Dynamism," which emphasizes a "future-oriented mentality." Therefore, we hypothesized that making Asian ethnic identity salient to Asian-Americans makes them more patient. If true, then identity effects on preferences could help explain high Asian-American savings rates and educational investment.

We use the method developed by Shih, Pittinsky, and Ambady (1999) to prime Asian ethnic identity. We then elicit time and risk preferences from primed and unprimed subjects using an incentive-compatible mechanism. If Asian ethnic identity plays a causal role in determining preferences, then we expect to see that the priming manipulation causes Asians to behave more patiently. We apply the same prime to white subjects to check that any Asian priming effect is working specifically through the increased salience of Asian ethnic identity, rather than through some other channel. We did not expect to find priming effects on the white participants because the prime is designed to activate *Asian* ethnic identity (and American whites tend to have weak ethnic affinities in any case).

A. Participants

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

[4] 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."

remainder were of Asian Indian descent.[5] All of our Asian identity results continue to hold if we drop Asian Indians from the sample.

We recruited participants by putting up posters in the Harvard psychology building, e-mailing students who reported being members of undergraduate Asian-American clubs on facebook.com, and e-mailing Harvard dormitory lists. There were a small number of subjects who walked into experimental sessions upon observing that they were about to start. At no point did we specify in our recruiting materials that we were looking for white and Asian students.

B. Procedure

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

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

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

[5] Specifically, there were 41 Chinese, 7 Indians, 7 Koreans, 5 Taiwanese, 2 Japanese, 1 Filipino, 1 Thai, 1 Vietnamese, and 6 unspecified Asians.

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

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

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

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

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

Although our approach to measuring time preferences is standard (Frederick, Loewenstein, and O'Donoghue, 2002), it has been argued that choices over the timing of monetary rewards should not measure time preference, since people can (in principle) borrow or lend money at the market interest rate regardless of how they discount future utility (Fuchs, 1982). However, in experiments like ours, most participants discount future rewards at a much higher rate than the market interest rate (Frederick, Loewenstein, and O'Donoghue 2002), perhaps because they are liquidity-constrained or do not realize that money is fungible. In either case, questions involving monetary rewards do measure discounting over utility. Consistent with this interpretation, time preference measured in a manner similar to ours predicts variation in discounting-related behaviors such as drug addiction (e.g., Kirby, Petry, and Bickel, 1999; Kirby and Petry, 2004), cigarette smoking (Fuchs, 1982; Bickel, Odum, and Madden, 1999), excessive gambling (Petry and Casarella, 1999), use of commitment savings devices (Ashraf, Karlan, and Yin, 2004), borrowing on installment accounts and credit cards (Meier and Sprenger, 2006), and rapid exhaustion of food stamps (Shapiro, 2005). (See also Loewenstein, Read, and Baumeister, 2003.)[6]

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

Existing evidence suggests that risk preferences measured through choice tasks are related to real-world risk behaviors. Risk aversion measures derived from real-stakes experimental choices are highly correlated with measures from hypothetical choices (Dohmen et al., 2005), which in turn predict risky behaviors such as smoking, drinking, failing to hold insurance, holding stocks rather than Treasury bills, being self-employed, switching jobs, and moving residences (Barsky et al., 1997; Guiso and Paiella, 2001; Dohmen et al., 2005; Sahm, 2007).

[6] Some economists are troubled by the fact that subjects in experiments such as ours require extremely high interest rates to delay payment receipt. For example, a subject choosing to receive $3 today rather than $3.05 in one week is borrowing at an annualized interest rate of 136%. Although it is difficult to believe that such impatience is normatively justified, the real-world payday loan market typically features annualized interest rates of 400% (Morse, 2006).

Compensation scheme. Before the participant answered any of the preference elicitation questions, the experimenter explained that after the experiment, the participant would randomly select one of the time or risk preference choices to determine his or her payout by drawing a number out of a bag.[7] The bag contained slips of paper numbered 1 to 64, one for each preference elicitation question. If a risk preference question was selected, and if the participant had chosen the gamble in that question, then the participant would randomly draw a number out of a different bag, which contained numbers between 1 and 100. If the drawn number was less than or equal to the *Y*% probability of winning, the participant won $8.[8]

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.[9]

C. Econometric methodology

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

Our dependent variable for the risk preference task is the minimum expected return premium that the subject requires to accept the gamble over the certain payout. For example, for a subject who chose to gamble for $8 rather than accepting the sure $4 if and only if the

[7] 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).
[8] The printed instructions on the risk elicitation sheet mistakenly stated that gambles would be resolved by drawing from a bag of red and blue marbles, which had been the original intention.
[9] To secure the promise to pay at the end of a loan term, payday lending companies typically use postdated checks collected from borrowers at the time of loan origination (Potter, 2002). Although a check-issuer's bank bears no legal liability if it pays a postdated check early (provided the check-writer did not notify the bank of the check in advance; see U.C.C. §4-401), many banks will not allow account holders to deposit post-dated checks. Although we did not keep track of check deposit dates in Experiment 1, we found in Experiment 2's Temple sample that almost all subjects deposited their checks after the check date. (Because of how we ensured anonymity, a similar analysis in Experiment 2's Michigan sample was impossible.) All but one participant deposited his or her check into a bank checking account.

probability of winning is at least 58%, the dependent variable value is $\pi = (8 \times 0.58 - 4)/4 = 0.16$.[10]

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

The normality assumption implies that the dependent variable sometimes takes on negative values. This negativity is not a problem in the risk preference regressions, since we do observe some risk-seeking behavior in our data. We therefore use π as the dependent variable in the risk preference regressions. However, our prior belief is that negative interest rates are perverse and likely to be due to elicitation errors. Therefore, we impose lognormality on the interest rate variable by making log(r) the dependent variable in the interval regression, thus ruling out negative interest rates. In the interest rate regression tables that follow, if the coefficients imply that a certain set of explanatory variable values are associated with a mean log(r) of $\hat{\mu}$, then the median r is $\exp(\hat{\mu})$. Because of outliers, we will focus on median interest rates in our analysis.[11]

We observe four r (interval) values for each participant, since we elicited four sets of intertemporal preferences. In the time preference results that follow, we report results that pool the four r values together, adding explanatory dummy variables to indicate 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.

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[10] Only two subjects did not have a threshold such that they chose the earlier payment if and only if the interest rate was below that threshold. These two subjects also did not have a risk premium threshold such that they chose the certain payoff if and only if the risk premium was below that threshold. Our results are unaffected by excluding these two subjects. In our main analysis, we use the interval corresponding to the lowest interest rates and lowest risk premia at which the subject behaved impatiently or risk-aversely, respectively.

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

D. Results

Table 1 displays coefficients from a regression of participants' required log interest rate and risk premium on experimental condition and trade-off type. Column 1 shows the main result from Experiment 1: the interest rate required by Asians to defer payment falls dramatically when Asian ethnic identity is made salient. For example, for trade-offs between $4 now and money one week from now, the median required interest rate falls from 8.8% to 2.1%. Running separate regressions for each intertemporal choice type (immediate payment amount × 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.[12] Columns 2 and 4 show, in analogous regressions for white subjects, that whites' choices are not affected by the prime.[13] These results are consistent with the prime causing Asians' time preference to move in the direction of high patience prescribed by the Asian identity norm.

IV. Experiment 2: Black-Racial Identity and Gender Identity

Experiment 1 focused on Asian ethnic identities. We ran a second experiment to explore how preferences are related to black-racial identity and gender identity. Our hypothesis—that identity norms drive some of the demographic differences in economic outcomes—suggested that American-born black identity would increase discount rates and/or risk aversion, immigrant black identity would have the opposite effects, and that gender identity would decrease risk aversion in males relative to females.

Experiment 2 also expanded on the earlier experiment by measuring larger-stakes (in addition to small-stakes) risk preferences and by asking a host of questions that would enable us to test potential mechanisms underlying the identity salience effects. In addition, we introduced

[12] We do not place much importance on the fact that control Asians are more risk averse and slightly more impatient than control whites. Differential selection effects arising from the way we recruited subjects and subjects' decisions to participate confound these comparisons. Only within-race comparisons between treatment and control groups are reliable for inferring identity effects.

[13] It would be interesting to examine whether primed Asians who have been in the U.S. for one or fewer generations, who report speaking only an Asian language at home, or who list an Asian language first when asked what languages they know demand especially low interest rates. However, we are hampered by our not having collected these affiliation strength data for the control group, preventing us from controlling for baseline preference differences associated with differing affiliation strength.

variation in the delay between the salience manipulation and the preference elicitation, which allows us to investigate the impulse response function of an identity salience shock. To our knowledge, we are the first to provide evidence on the duration of identity salience effects.

A. Participants

We recruited 280 Temple University students by handing out flyers on campus and providing a $1 referral fee to participants for each friend they got to sign up for the experiment. [14] We recruited 231 University of Michigan students by handing out flyers, putting up posters, and emailing student groups likely to have many black members. In order to avoid pre-priming participants with their racial identity, 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 conducted 19 fifty-minute experimental sessions in Temple classrooms on March 18, 25, and 26, 2006. The smallest session had 6 participants, and the largest had 29. We also conducted 28 sessions at the University of Michigan between November 30, 2006, and April 10, 2007. There were 2 participants in the smallest session and 28 in the largest. Our results from the Temple and Michigan samples are directionally similar, so we pool them in all analyses.

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.

The principal experimenter for the Temple sessions was a male of black, Mexican, and white descent. He was assisted by a white male and an Asian male. The Michigan sessions were

[14] We initially ran the experiment at Temple University because it has one of the largest black student populations (approximately 20% of the 34,000 students) in the United States outside of the historically black colleges. Running the experiment at an historically black college would have precluded our recruiting white subjects from the same population, and we were concerned that students at historically black colleges may be so saturated with their racial identity that a priming manipulation would have no additional effect. We ran additional sessions at Michigan in order to credibly identify interaction effects when we analyze determinants of racial and gender identity norms and also in order to measure childhood messages.

conducted by various experimenters of white, black, Hispanic, and Asian descent and both genders.[15]

After the questionnaire booklet was distributed to each participant, the principal experimenter guided session participants through the questionnaire together by reading instructions aloud before each section. The questionnaire was divided into sections (with the neutral labels "Section 1," "Section 2," and so on). The first section contained the identity-salience manipulation or control. The next three sections were a time preference elicitation (which took 5 minutes for instructions and responses), a risk preference elicitation (5 minutes), and a six-question version of the Spielberger State-Trait Anxiety Inventory (Marteau and Bekker, 1992) (1.5 minutes). These three sections' order varied across sessions. The penultimate section was a six-question math quiz with SAT-like questions, The questionnaire's final section asked a variety of questions about personal and family background, as well as questions unrelated to the study in order to mask its purpose.[16] Each of the time and risk preference measures was incentive-compatible, as explained below. We also paid subjects 10 cents for each math question they answered correctly. Participants were paid for their choices, plus a $1 show-up fee, by check immediately upon completing the experiment. In order to avoid contaminating future subjects, participants' debriefing form did not reveal that our study was about racial and gender identities.[17]

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?

[15] Although we have little power to test directly for experimenter race effects, the fact that the Temple and Michigan results are directionally similar when analyzed separately suggests that these effects were not an important factor for our results.

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

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

(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[18] 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* =

[18] Shih, Pittinsky, and Ambady (1999) do not ask the subjects' gender in their gender prime. In addition, we slightly rephrased question (d) to remove some potential ambiguity in the analogous question used by Shih et al.

$10.10, $10.25, $10.50, $10.75, $11.00, $11.25, $11.50, $12.00, $12.50, $13, $14, and $15. The second set of 12 questions was the same as the first set, except that option (A) occurred "one week from now," and option (B) occurred "two weeks from now." These questions were presented with the delayed reward *X* in ascending order.[19]

The section's instructions gave two sample questions and explained that later during the experiment, a participant would roll a 24-sided die to determine which question would count for payment in that session. All payments would be made by check, and if on the chosen question the subject had selected the delayed payment, he would receive that delayed payment as a post-dated check. The experimenter told participants that post-dated checks can be cashed any time on or after the check's date.[20] The final two sentences of the section's instructions made clear that the questions were not intended to evaluate performance: "It's important to keep in mind that there are no right or wrong answers here. Which choice you make is a matter of personal preference." (We used this same wording again in the instructions for both risk preference sections.)

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

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

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

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

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

The larger-stakes risk section choices were analogous, except that the monetary amounts were multiplied by 100. For example, the first question gave a choice between "(A) I get $100 for sure," and "(B) If the six-sided die comes up 1, 2, or 3, I get $160. If the six-sided die comes up 4, 5, or 6, I get nothing." The section's instructions explained that we would pay a participant for a randomly selected question in the section *if* the participant could correctly guess in sequence two roulette wheel spin outcomes which would take place later in the session.[21] Participants submitted written predictions before answering this section's questions. (No one correctly predicted both spins.) The instructions presented a sample question and told the participants that any money earned in this section would be paid by an immediately cashable check.

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

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

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

Math quiz. We gave participants eight minutes to answer six questions similar to those found on the SAT Math exam. The instructions told participants that unlike the previous preference

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

questions, these math questions did have right answers. For each question they answered correctly, 10 cents would be added to the check that they could cash immediately.

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

In this section, we asked about the credibility of our payment promises. The first question asked, "Throughout this experiment, you made choices that involved various amounts of money. We said that your responses would affect how much you get paid, but you may not have believed us. Did you believe that your responses would affect how much you get paid?" To be conservative, we drop from our analysis the 105 participants who did not circle the answer, "I believed that my responses would matter, exactly as the questionnaire said."

The second question asked, "Think back to when you were answering questions about getting a certain amount of money today versus getting some different amount of money in a week. Did you believe that you would actually get paid in a week if you chose to take the money in a week?" For our analysis of time preference, we drop an additional 114 participants who did not circle the answer, "I believed that I would get paid in a week if I chose to take the money in a week, as the questionnaire said."

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

Finally, we asked a series of questions about participants' beliefs 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.

C. Econometric Methodology

As in Experiment 1, our dependent variables are $\log(r)$ (the log of the interest rate that induced subjects to choose the later payment) and π (the risk premium that induced subjects to choose the gamble), and we use interval regressions for our estimations.[22] We observe two r

[22] Respectively, 6% and 17% of participants did not answer the intertemporal questions and the risk preference questions in such a way that there was one price above which they always chose one way and below which they always chose the opposite. For these participants, we used the lowest interest rate or risk premium that induced them

intervals and two π intervals for each participant. In the regressions reported below, we pool the two r values or the two π values and add dummy independent variables that indicate in which choice situation (now versus one week, one week versus two weeks, small gamble, large gamble) the r or π was observed. In addition, we control for the school at which the subjects were recruited, as well as an interaction between the school and choice situation. Standard errors are clustered by individual. For the race-salience analysis, we drop participants who were not non-Hispanic white or black.

D. Main Results

Table 2 shows summary statistics for the pooled Temple and Michigan sample. Sociological research indicates that blacks whose families have long-standing U.S. roots grow up with a very different cultural heritage than blacks whose families have recently immigrated to the U.S. (e.g., Waters, 1994). Immigrant blacks are over-represented among the black economic elite and share an identity that emphasizes their status as immigrants (Sowell, 1975; Rimer and Arenson, 2004). Therefore, we separately analyze blacks who were born abroad or who have at least one parent who was born abroad.

Because the identity-salience manipulations were randomly assigned, there should not be systematic differences between participants across experimental conditions. Table 2 shows that participants generally appear similar across conditions, once we control for university attended.[23] The exception is men in the gender-salience condition, who have lower SAT Math scores and are less likely to believe our payment promises than men in the control condition. We have confirmed that our male gender priming results are not affected by adding SAT Math score as a control.

Table 3 presents the baseline results for whites, native blacks, immigrant blacks, men, and women. The main result of Experiment 2 is that making racial identity salient to native blacks reduces their willingness to take financial risks, raising their required risk premium by 19 percentage points. In contrast, immigrant blacks seem to become *less* risk averse when their

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to choose deferred or risky payments. Our results are qualitatively unchanged if we exclude these participants from our regressions instead.

[23] We control for university because the proportion of Michigan students in each experimental group is not equal. We administered treatments in different proportions at Michigan and Temple due to the desire to prioritize data collection for the race-salience study. We have also dropped from our sample four native blacks in the race-salience group who were over 22 years old. Our priming results are unchanged if we include these four subjects.

racial identity is made salient, although the 11 percentage point risk premium drop is not statistically different from zero. We also find no significant white identity risk aversion effect.[24] The native black priming effect on risk aversion is statistically different from the white and immigrant black priming effects (both p-values < 0.01). These results are consistent with the hypothesis that identity-induced preferences cause native blacks to be more reluctant to invest in high-return risky assets (thus reducing their capital accumulation relative to immigrant blacks and whites).

Because we varied the order of the time preference elicitation, risk preference elicitation, and anxiety scale sections across experimental sessions, we can gain some insight into how quickly priming effects decayed. Keeping in mind that the standard errors on 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 12, 23, and 16 percentage points, respectively, at 0, 5, and 7 minutes after the prime (the times the risk preference elicitations began). Therefore, even subtle identity salience manipulations appear to have effects that last at least 7 minutes.

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 their own gender stereotypes).[25] Priming identity 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 identities. 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.

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

[25] In our data, priming gender does cause *white* men to become more significantly less risk-averse (not shown in Table 3). We do not emphasize this finding because Table 3 suggests that, if anything, the gender prime affects women's average risk-aversion more than men's when we do not restrict the analysis to whites.

E. Within-Group Heterogeneity in Identity Norms and Strength

The theory in Section II predicts that if beliefs about the identity norm differ within an identity group, then priming identity will have different effects on different individuals. In this subsection, we measure beliefs about channels that are sometimes thought to affect identity prescriptions. We then see if variation in these beliefs predicts variation in the priming effect. We also examine how priming interacts with the strength of identity affiliation.

Conformance to perceived stereotypes. It is sometimes asserted that stereotypes about Asian math ability or black athletic ability push members of those races towards math or sports. If societal stereotypes affect identity norms, then the effect of priming an aspect of an individual's identity should depend on what that individual believes about stereotypes related to that identity.

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 $p < 0.01$).

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

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

Taken as a whole, the results suggest that racial stereotypes do not affect race identity norms. Columns 1 and 3 of Table 4's Panel A show that the interaction of the patient stereotype belief index with the race prime has only a small and insignificant effect on the required interest rate for whites and immigrant blacks. The same columns in Panel B show that the interaction between the risk-averse stereotype belief index and the prime also has no effect on the risk premium.

Interestingly, for native blacks, even though there is no significant interaction between the risk-averse stereotype belief index and the prime (Table 4, Panel B, column 2), there is a positive interaction between beliefs about patient black stereotype prevalence and the race prime on the required interest rate. In other words, native blacks who believe that patient black stereotypes are common become relatively *less* patient when primed than native blacks who believe such stereotypes are uncommon. This result initially appears contrary to the notion that identity salience causes people to conform to that identity's norms. But recall from Section II that the effects of priming can be subtle when there is population heterogeneity. In this case, note that unprimed native blacks with a high patience stereotype belief index are much more patient than unprimed native blacks with a low patience stereotype belief index. This is a case where the identity norms are more extreme than baseline behavior, $x_N^H > x_I^H$ and $x_N^L < x_I^L$. When primed, high patience stereotype belief native blacks become relatively less patient, whereas low patience stereotype belief native blacks become relatively more patient; their choices converge toward the middle.

Unlike for race identity, stereotype conformance appears to play an important role for gender identity, 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 4's Panel B). The opposite effect occurs for those who believe risk-averse stereotypes about their gender are relatively less common. The size of this interaction effect is large: a one standard deviation increase in the risk-averse stereotype index is associated with a 16.1 percentage point increase in the gender prime's risk premium effect among men and a 12.4 percentage point increase among women.[26] The interaction is not

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

statistically significant for women, but this is due to noise introduced by aggregating the stereotype beliefs into one index. Separately analyzing the components of the risk-averse stereotype index (not shown), we find that these effects are driven by beliefs about the "cautious" stereotype for men and the "reckless" stereotype for women (both significant at the 5% level).

This interaction effect between priming and gender risk stereotypes decays over time, apparently more quickly than the main effect of priming on blacks' risk aversion. Examining the size of the "cautious" standardized stereotype interaction for men and "reckless" standardized stereotype interaction for women,[27] we find that the coefficient goes from 21.2 to 10.0 to 5.3 percentage points for men and from 20.2 to 20.2 to -0.1 percentage points for women as 0, 5, or 7 minutes intervened between the end of the gender prime and the start of the risk preference elicitation.[28] (Not shown in tables.) The difference between the interaction effects when 0 versus 7 minutes separated the prime and the elicitation is significant at the 5% level for both men and women.

Conformance to normative childhood messages. Societal prescriptions for identities can come in the form not only of stereotypes, but also in the form of explicit normative messages. Michigan subjects answered 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** children receive messages that they should behave in the following ways?" Subjects responded on a six-point scale from "extremely rarely" to "extremely often." The messages subjects rated were the same as the stereotypes we asked about: generous, lazy, frugal, impatient, studious, cautious, artistic, patient, and reckless. We also asked about black children, male children, and female children.

As for the stereotype prevalence beliefs, we construct a patient childhood norm index pertaining to race (or gender) by adding the participant's numerical rating of "patient" and "frugal," subtracting the "impatient" rating, and standardizing the resulting variable to have mean zero and unit variance within the race or gender group. We create an analogous index for

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

risk-averse stereotypes by subtracting the participant's rating of "reckless" from the rating of "cautious" and standardizing.

Table 5 displays the results of interacting these childhood norm indices with the identity salience dummy. We omit immigrant blacks from the table because there were not enough of them who passed our payment belief filters in the Michigan sample to obtain numerical convergence in the maximum likelihood estimates.[29] Although our sample sizes for this analysis are much smaller, the results are similar to those obtained in the stereotype prevalence regressions. Native blacks who believe that black children commonly hear messages to be patient are more patient at baseline than native blacks who believe the opposite. When primed, these two groups' required log interest rates converge.

Men and women who believe children of their gender are frequently given messages to be risk averse become relatively more risk averse when primed. The point estimates of the interactions are large: a one standard deviation increase in the risk-averse childhood norm index is associated with a 21.2 percentage point increase in the gender prime's risk premium effect among men and a 15.2 percentage point increase among women. Due to the small sample, the interaction is not statistically significant when each gender is analyzed separately, but pooling the genders causes the interaction to be significant at the 5% level. Examining the components of the risk-averse childhood norm index, we again find that cautious norms provide most of the explanatory power for men and reckless norms for women.

The one result that does not have an analog in the stereotype prevalence belief analysis is the interaction between women's required log interest rate and patient childhood messages. We find that women who believe girls are frequently told to behave patiently become significantly more patient in response to the gender prime than women who believe the opposite.

Conformance to traditional gender roles. To see if subjects' attitudes towards traditional gender roles influenced the gender-salience effect, we asked subjects to indicate their agreement (on a six-point scale from "strongly disagree" to "strongly agree") with four statements about traditional gender roles:

(a) The man should always pay for the first date between a man and a woman.

[29] Only seven immigrant blacks at Michigan believed their choices mattered, and only five also believed our delayed payment promises.

(b) A pre-school child is likely to suffer if his/her mother works outside the home.

(c) Men shouldn't cry.

(d) Ultimately, the husband is responsible for making sure the family is financially secure.

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

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

Identity strength. Recall from Section II that it is theoretically ambiguous whether a given identity-salience effect will be stronger or weaker for individuals who affiliate more strongly with the primed identity. Nonetheless, we report these interaction effects here for the priming main effects that we found.

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

(a) My racial identity is an important part of my self-image.

(b) My racial identity is an important reflection of who I am.

(c) My racial identity has very little to do with how I feel about myself.

(d) My racial identity is unimportant to my sense of what kind of person I am.

For gender identity, the questions were analogous, but we substituted "being a woman/man" for "my racial identity" in the statement text. These questions are taken from the "private collective self-esteem subscale" (Luhtanen and Crocker, 1992), a standard psychological instrument for measuring identity affiliation. We assign a value from 1 to 6 to the responses to each statement, where 6 corresponds to the response indicating the highest degree of identification. We sum the race (or gender) responses and standardize this race (or gender) identification variable to be mean zero and variance one within each regression we run. Responses to these questions are not

generally thought to be affected by momentary primes (Luhtanen and Crocker, 1992), and our evidence is consistent with that assumption (results not shown).

We do not find evidence of an interaction with identity strength for either the priming effect making native blacks more risk averse, or the priming effect in which women who believe that women are stereotypically more cautious become more risk averse (in both cases, point estimates are near zero; results not shown). However, for men, stronger gender identification greatly attenuates the interaction effect whereby men who believe that men are stereotypically more cautious become more risk averse ($p = 0.013$; results not shown).

F. Alternative Explanations

In this subsection, we consider alternative explanations that might explain why our priming manipulation caused changes in time and risk preferences for reasons different than identity salience.

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

A possible explanation for our results is that the identity primes induced stereotype lift among Asians in Experiment 1, 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 (e.g., 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 were explicit in telling 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 and lift or emotional changes, we examine how the treatment affected performance on five SAT Math-like questions administered after the elicitations and responses to a shortened version of the Spielberger State-Trait Anxiety Index (a standard psychometric measure of anxiety).[30] The primes had no effect on math quiz performance for whites, blacks, and women. 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. That effect remains after controlling for anxiety, SAT math score, and math quiz score (the coefficient is 0.197, $p < 0.01$). (None of these results are displayed 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. We believe this is unlikely because participants were unaware that the first section of the questionnaire (which contained the race-identity prime, the gender-identity 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?" 90% 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 instead who circled yes, most made innocuous guesses about the purpose of the experiment (like

[30] 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.

"to see whether or not we were risk takers with money"), and no one made a guess related to race or gender.

V. Conclusion

Our findings suggest that social identity matters for fundamental economic preferences. We found that priming Asian-American subjects' ethnic identity causes them to exhibit more patient preferences. Priming racial identity among black subjects did not affect time preference, but it increased risk aversion among those who had longstanding roots in the U.S. and may have decreased risk aversion among those who had at least one parent born abroad. Priming gender identity appears to cause both men and women to behave more closely in accordance with risk stereotypes they hold about their own gender. Overall, our results are broadly supportive of the view that preferences are influenced by internal identity affiliations, social norms and stereotypes, and environmental identity primes.

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

Appendix

In this appendix, we generalize the model and results from Section II. Let $w(s):[0,\infty]\to[0,\infty]$ be a smooth, strictly increasing function with $w(0) = 0$, and let $L_N, L_I : \mathbb{R} \to \mathbb{R}$ be smooth, strictly concave functions each having a maximum at 0. Also let $x_N < x_I$ be constants. For each value of s, the function

$$u(x) = L_N(x - x_N) + w(s)L_I(x - x_I)$$

is again strictly concave, hence it has a unique global maximum x^*.

Proposition 3: Consider x^* as a function of s.

1. $x_N < x^*(s) < x_I$ for all $s > 0$.
2. $x^*(s)$ is strictly increasing in s.
3. If w, L_N, L_I are all smooth algebraic functions, then there exists $\hat{s}$ such that $x^*(s)$ is a concave function of s for all $s > \hat{s}$.

Proof:

1. Treat u as a function of x and s. When $x \le x_N$, $\partial u/\partial x = L_N'(x - x_N) + w(s)L_I'(x - x_I) > 0$, so x cannot be the maximum; analogously, when $x \ge x_I$, we have $\partial u/\partial x < 0$. This establishes $x_N < x^*(s) < x_I$.
2. The implicit function theorem gives

$$\frac{dx^*}{ds} = -\frac{w'(s)L_I'(x^* - x_I)}{L_N''(x^* - x_N) + w(s)L_I''(x^* - x_I)}.$$

The denominator is ≤ 0 by the concavity assumption. Since $x^* < x_I$, we have $L_I'(x^* - x_I) \ge 0$. Thus the numerator is positive, giving $dx^*/ds \ge 0$. To check that x^* is *strictly* increasing in s, just notice that it is not possible to have $L_N'(x - x_N) + w(s)L_I'(x - x_I) = 0$ for the same x but two different values of s: otherwise the L_N' and L_I' terms would both have to be zero, which cannot occur.

3. Note that $x^*(s)$ is an algebraic function of *s*. This holds because we can write out the first-order condition as

$$L_N'(x - x_N) + w(s) L_I'(x - x_I) = 0.$$

Now L_N' and L_I' are algebraic functions and are nonzero (except at zero). It follows that $w(s)$ is an algebraic function of x^*, and so *s* is as well; thus, there is a real polynomial *P* with $P(x^*, s) = 0$. So $x^*(s)$ is an algebraic function of *s*. Standard results from algebraic geometry imply that d^2x^*/ds^2 is also an algebraic function of *s*.

The key fact is that because d^2x^*/ds^2 is an algebraic function, it cannot be equal to zero for arbitrarily large *s* unless it is identically zero (for large *s*). This would imply that $x^*(s)$ is eventually linear, which is impossible since it is increasing and bounded. So once *s* is large enough, $x^*(s)$ is either always concave or always convex. The latter is again impossible for a bounded increasing function. So it must be that $x^*(s)$ is concave, as needed.

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

This table presents interval regressions for Asians and whites in Experiment 1, where the dependent variable is the log interest rate required to defer payment receipt or the risk premium required to accept a gamble. We pool each subject's four intertemporal choices. *Ethnicity Salience* is a dummy for 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. σ is the conditional standard deviation of the dependent variable. Standard errors appear in parentheses below the point estimates. Huber-White standard errors, clustered by subject, are reported for the log interest rate regressions.

	Log interest rate		Risk premium	
	Asians	Whites	Asians	Whites
	(1)	(2)	(3)	(4)
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 × (*1 Week vs. 2 Weeks*)	-0.0584	0.0887		
	(0.1512)	(0.1773)		
Constant	-2.4322***	-2.7841***	0.2060***	0.0887**
	(0.2448)	(0.3110)	(0.0509)	(0.0440)
σ	1.6360	1.6461	0.2918	0.2652
	(0.1352)	(0.1456)	(0.0283)	(0.0250)
N	284	264	71	66

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

Table 2. Summary Statistics for Participants in Experiment 2

This table provides summary statistics for the participants in each experimental condition in Experiment 2. Identity salience refers to the race-salience group in the first three columns and the gender-salience group in the last two columns. The second column drops native blacks over age 22, and the last two columns exclude both native and immigrant blacks. In order to identify differences between the control and treatment groups, we perform an OLS regression of each background variable on a treatment dummy, an indicator for recruitment location, and a constant. The *p*-values for the treatment dummies are provided below the summary statistics. "Believed Choices Mattered" is the percent of subjects who reported believing experimental choices would affect payments. "Also Believed Deferred Payment Promise" is the percent of subjects who also reported believing that deferred payment promises were credible.

		Whites	Native blacks	Immigrant blacks	Men	Women
Mean Age	Identity Salience	19.6	19.9	19.8	20.0	19.8
	Control	20.0	19.3	19.5	20.1	19.6
	p-value	0.289	0.207	0.437	0.747	0.892
Mean SAT I Math Score	Identity Salience	606.7	529.5	534.3	615.3	614.5
	Control	632.9	532.6	551.4	665.6	616.0
	p-value	0.113	0.707	0.450	0.058*	0.995
Mean SAT I Verbal Score	Identity Salience	622.6	583.0	567.1	605.3	606.9
	Control	624.1	523.7	559.5	623.9	624.7
	p-value	0.791	0.112	0.915	0.661	0.288
Proportion with Household Income > $80,000	Identity Salience	61.5%	31.6%	36.0%	55.3%	49.0%
	Control	64.1%	26.9%	36.0%	63.4%	61.4%
	p-value	0.810	0.914	0.962	0.430	0.218
Proportion Who Believed Choices Mattered	Identity Salience	83.9%	86.4%	68.0%	72.3%	76.0%
	Control	84.5%	77.8%	82.1%	85.4%	82.1%
	p-value	0.880	0.466	0.243	0.053*	0.415
Proportion Who Also Believed Deferred Payment Promise	Identity Salience	57.0%	63.6%	48.0%	48.9%	56.0%
	Control	64.3%	51.9%	57.1%	64.6%	61.9%
	p-value	0.272	0.270	0.534	0.074*	0.544
Sample size	Identity Salience	93	44	25	47	50
	Control	129	27	28	82	84

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

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

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

Panel A: Log interest rate

	Whites	Native blacks	Immigrant blacks	Men	Women
Identity Salient	-0.7064**	-0.6361	-0.3800	-0.4408	-0.0349
	(0.3319)	(0.4005)	(0.3454)	(0.3975)	(0.4183)
1 Week vs. 2 Weeks	-0.1979	-0.0929	0.0968	-0.4468*	0.1798
	(0.1480)	(0.2205)	(0.3586)	(0.2300)	(0.2295)
UMich	-0.4258	-0.0059	-0.3111	-0.7181*	-0.2018
	(0.3135)	(0.4950)	(0.5760)	(0.3826)	(0.4183)
1 Week vs. 2 Weeks ×	0.0185	-0.0248	0.9917*	0.4339	-0.5207
UMich	(0.2258)	(0.2876)	(0.5141)	(0.2752)	(0.3256)
Constant	-2.3971***	-1.6869***	-2.4392***	-1.8285***	-3.0932***
	(0.2349)	(0.2920)	(0.3570)	(0.2672)	(0.3626)
σ	1.8021	1.4274	1.0918	1.5004	1.7794
	(0.1343)	(0.2190)	(0.1386)	(0.1669)	(0.1670)
N	262	82	56	148	146

Panel B: Risk premium

	Whites	Native blacks	Immigrant blacks	Men	Women
Identity Salient	-0.0438	0.1978***	-0.1062	-0.0869	-0.1159*
	(0.0519)	(0.0736)	(0.0887)	(0.0727)	(0.0678)
Large Stakes	0.3100***	0.0436	0.1088	0.3027***	0.0936
	(0.0489)	(0.0914)	(0.0741)	(0.0658)	(0.0658)
UMich	-0.0095	0.0892	0.1101	-0.0723	-0.1374**
	(0.0500)	(0.1143)	(0.0868)	(0.0685)	(0.0635)
Large Stakes ×	-0.0022	0.1432	0.0915	-0.0084	0.1690*
UMich	(0.0686)	(0.1420)	(0.2166)	(0.0968)	(0.0946)
Constant	0.2027***	0.0657	0.0919	0.2162***	0.3036***
	(0.0416)	(0.0793)	(0.0832)	(0.0512)	(0.0556)
σ	0.4005	0.3952	0.3513	0.4047	0.3899
	(0.0199)	(0.0375)	(0.0525)	(0.0278)	(0.0263)
N	360	114	80	204	198

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

Table 4. Identity-Salience Interaction Effects with Stereotype Prevalence Belief

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

Panel A: Log interest rate					
	Whites	Native blacks	Immigrant blacks	Men	Women
Identity Salient	-0.6991**	-0.5749*	-0.4494	-0.3682	-0.1519
	(0.3334)	(0.3474)	(0.3327)	(0.4130)	(0.4571)
Identity Salient ×	0.0962	0.7477**	-0.0758	0.2339	-0.2951
Patient Stereotype	(0.3525)	(0.3108)	(0.3332)	(0.3738)	(0.4364)
Patient Stereotype	-0.0419	-0.4575***	0.0573	-0.0556	0.1792
	(0.2488)	(0.1604)	(0.2507)	(0.3004)	(0.2647)
1 Week vs. 2 Weeks	-0.1982	-0.0949	0.0963	-0.4504*	0.1799
	(0.1482)	(0.2180)	(0.3599)	(0.2337)	(0.2272)
UMich	-0.4361	0.2410	-0.0105	-0.8419*	-0.0696
	(0.3196)	(0.4920)	(0.6298)	(0.4835)	(0.4428)
1 Week vs. 2 Weeks ×	0.0115	0.1193	0.9008	0.3159	-0.5019
UMich	(0.2291)	(0.2596)	(0.5968)	(0.3008)	(0.3084)
Constant	-2.4022***	-1.7327***	-2.4105***	-1.8453***	-3.0378***
	(0.2362)	(0.2487)	(0.3587)	(0.3133)	(0.3716)
σ	1.8087	1.2905	1.1063	1.5345	1.7356
	(0.1354)	0.2226	(0.1548)	(0.1863)	(0.1798)
N	260	76	52	122	120
Panel B: Risk premium					
	Whites	Native blacks	Immigrant blacks	Men	Women
Identity Salient	-0.0468	0.2378***	-0.1284	-0.0861	-0.1232*
	(0.0526)	(0.0860)	(0.0957)	(0.0727)	(0.0696)
Identity Salient ×	-0.0132	-0.0907	-0.0316	0.1611**	0.1243
Risk-Averse Stereotype	(0.0560)	(0.0942)	(0.1296)	(0.0817)	(0.0882)
Risk-Averse Stereotype	0.0253	-0.0010	0.0482	-0.0522	-0.0023
	(0.0354)	(0.0547)	(0.1144)	(0.0569)	(0.0725)
Large Stakes	0.3104***	0.0333	0.1087	0.3000***	0.0940
	(0.0491)	(0.0982)	(0.0746)	(0.0658)	(0.0661)
UMich	-0.0049	0.1634	0.0755	-0.0082	-0.1675**
	(0.0504)	(0.1387)	(0.1015)	(0.0851)	(0.0765)
Large Stakes ×	-0.0012	0.1505	0.2394	-0.1115	0.1954*
UMich	(0.0692)	(0.1765)	(0.2489)	(0.0870)	(0.1182)
Constant	0.2021***	0.0383	0.1066	0.2110***	0.3104***
	(0.0413)	(0.0848)	(0.0918)	(0.0515)	(0.0596)
σ	0.4006	0.4197	0.3526	0.3853	0.3863
	(0.0198)	(0.0393)	(0.0516)	(0.0270)	(0.0295)
N	358	98	76	170	162

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

Table 5. Identity-Salience Interaction Effects with Childhood Norm Belief

This table presents interval regressions for whites, native blacks, immigrant blacks, men, and women recruited for Experiment 2 at the University of Michigan, where the dependent variable is the log interest rate required to defer payment receipt or the risk premium required to accept a gamble. The last two columns exclude both native and immigrant blacks. We pool each subject's two intertemporal choices together, and each subject's two risk choices together. *Identity Salient* is a dummy for the race-salience treatment in the first two columns or the gender-salience treatment in the last two columns. *Immigrant* is a dummy for if the subject or at least one parent was born abroad. *1 Week vs. 2 Weeks* is a dummy for if the intertemporal choice was between payments deferred for one week versus two weeks. *Large Stakes* is a dummy for if the sure payout in the risky choice was \$100. σ is the conditional standard deviation of the dependent variable. *Patient Norm* is the extent to which the subject believes "patient" and "frugal" norms are common and "impatient" norms are uncommon about his or her race in the first two columns or gender in the last two columns. *Risk-Averse Norm* is the extent to which the subject believes "cautious" norms are common and "reckless" norms are uncommon about his or her race in the first two columns or gender in the last two columns. Huber-White standard errors, clustered by subject, are reported below the point estimates.

Panel A: Log interest rate

	Whites	Native blacks	Men	Women
Identity Salient	-0.5791	-3.6480*	-0.6063	-0.6518
	(0.4893)	(2.1184)	(1.0906)	(0.5357)
Identity Salient ×	0.7216	6.6465**	0.1254	-1.3088**
Patient Norm	(0.4976)	(2.9771)	(0.8899)	(0.6505)
Patient Norm	0.2716	-5.0831*	0.1764	0.9473*
	(0.2995)	(2.8062)	(0.4328)	(0.4987)
1 Week vs. 2 Weeks	-0.1905	0.0631	-0.1405	-0.3628*
	(0.1849)	(0.1778)	(0.2077)	(0.2184)
Constant	-2.9159***	1.4355	-2.6508***	-2.5245***
	(0.3021)		(0.4575)	(0.3048)
σ	1.8373	1.5048	1.6416	1.5298
	(0.1950)	(0.3472)	(0.3623)	(0.2826)
N	126	28	34	48

Panel B: Risk premium

	Whites	Native blacks	Men	Women
Identity Salient	0.0616	0.3953**	-0.2215*	0.0084
	(0.0748)	(0.1755)	(0.1193)	(0.1041)
Identity Salient ×	-0.1058	-0.0665	0.2121	0.1515
Risk-Averse Norm	(0.0719)	(0.1697)	(0.1385)	(0.0967)
Risk-Averse Norm	0.0539*	-0.0301	-0.0814	-0.0837*
	(0.0325)	(0.0431)	(0.1065)	(0.0502)
Large Stakes	0.3090***	0.2124	0.1884***	0.2914***
	(0.0510)	(0.1664)	(0.0618)	(0.1014)
Constant	0.1640***	0.0732	0.2380***	0.0824
	(0.0477)	(0.1564)	(0.0901)	(0.0678)
σ	0.3968	0.5351	0.3294	0.3937
	(0.0291)	(0.0686)	(0.0555)	(0.0519)
N	170	40	48	66

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