The Causal Effect of Cultural Identity on Cooperation

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Abstract

The impact of culture on non-kin cooperation has been singled out as critical for economic activity. However, causal evidence of culture's influence on cooperation remains scant. In this paper we provide such evidence, focusing on two key components of culture: preferences and beliefs. Adopting the view that culture is one aspect of an individual's multi-faceted self-concept (identity) we run experiments with first- and second-generation Chinese immigrants at a large US public university. By exogenously varying the salience of participants' American or Chinese cultural identities we reveal the combined causal effect of culture on cooperation through both preferences and beliefs. In a second nearly identical experiment we shut down culture's effect on beliefs, revealing culture's effect through preferences alone. Comparing behavior across experiments, our results suggest that culture influences cooperation through both preferences and beliefs and that culture's effect through beliefs is as important as its effect through preferences.

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1 Introduction

An emerging consensus in economics on a concrete definition of culture as beliefs and values shared among people groups has led to rapid growth in the economics literature on culture (Guiso, Sapienza and Zingales, 2006; Bisin and Verdier, 2008; Tabellini, 2010*a*; Fernández, 2011).[1] While the question of *whether* culture matters for economic outcomes is seemingly settled (Fernández and Fogli, 2006; Tabellini, 2010*b*; Alesina, Giuliano and Nunn, 2013; Algan and Cahuc, 2013; Alesina and Giuliano, 2015; Lowes et al., 2017), the channels through which culture affects behavior and economic outcomes is an important open question.

In this paper we study experimentally the two channels suggested by the definition above – beliefs and values – and provide evidence on their relative importance in culture's causal impact on non-kin cooperation. We focus on stranger cooperation because it is a question of perennial interest across multiple disciplines which economists have argued facilitates the well functioning of economies and societies (Knack and Keefer, 1997; Algan and Cahuc, 2013; Alesina and Giuliano, 2015).

Conceptually, we conjecture that culture may affect behavior directly through preferences by, for example, instilling cooperation as a virtue (Tabellini, 2008), or indirectly, by coloring beliefs about how others will behave and what others will expect. Beliefs may be colored in various ways. For instance, culture may transmit beliefs about how particular groups or individuals will behave.[2] Another way culture may color beliefs is by influencing which contextual cues receive attention, possibly affecting how unfamiliar others are categorized and, by extension, beliefs about those others. Many situations feature some contextual cues that, if attended to, could lead individuals to perceive strangers as similar to themselves (human, common fate) and some cues that could lead to a perception of strangers as being different (gender, ethnicity, role). Research in cultural psychology

[1] For example, Guiso, Sapienza and Zingales (2006, p. 23) define culture as "those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation." Similarly, Bisin and Verdier (2008) refer to culture as "preferences, beliefs, and norms that govern human behavior" while Fernández (2011, p.482) provides a working definition of culture as a "distribution of social preferences and beliefs." Tabellini (2010*a*) measures culture by aggregating "specific indicators of values and beliefs."

[2] The literature on trust may be illustrative, as parents and other authorities often teach children about "stranger danger," which can be thought of as culturally transmitted (pessimistic) beliefs about the intentions, traits and behavior of a particular category of people, strangers (e.g., Butler et al., 2016; Butler, Giuliano and Guiso, 2015, 2016).

suggests that Easterners perceive relationships more readily than Westerners and, consequently, tend to categorize objects based on relationships (monkey, banana) rather than properties (monkey, panda) (Ji, Zhang and Nisbett, 2004). Contextual cues related to relationships may therefore receive more attention from Easterners than from Westerners. Similarly, social relationships, with a particular focus on delineating the in-group, occupy a central role in collectivist cultures but not in individualist cultures, which in contrast prioritize self over social groups (Hwang, 1987; Triandis, 1995).[3] Moreover, across cultures, once categorization occurs intergroup differences tend to be subconsciously exaggerated (Haslam and Ellemers, 2005; Butler, 2018), perhaps amplifying these indirect beliefs channels.

To provide a reasonable chance of success at inducing substantial variation in both cultural beliefs and preferences, we focus on two well studied cultures thought to be quite different in both respects, the US and China. These two cultures have served as central examples in the psychological literature on Eastern vs. Western cultures (Nisbett et al., 2001; Nisbett, 2010) as well as in the expansive transdisciplinary literature on collectivism vs. individualism (Greif, 1994; Triandis, 1995, 2001; Oyserman, Coon and Kemmelmeier, 2002; Hofstede, Hofstede and Minkov, 2010; Gorodnichenko and Roland, 2011; Talhelm et al., 2014; Heine, 2015; Buggle, 2017; Gorodnichenko and Roland, 2017; Hajikhameneh and Kimbrough, 2018).[4]

Using participants with ties to both of these cultures, i.e., first- and second-generation Chinese immigrants studying at a large public university in the US, we examine cooperation in a laboratory Prisoners' Dilemma (PD) game. We depart from much of the economics literature on culture and adopt the view that culture is one aspect of an individual's multifaceted self-concept, or identity (Akerlof and Kranton, 2000, 2002, 2005, 2010).[5] In particular, we assume that behavior at any moment is primar-

[3]For example, Triandis (1995, p. 6) asserts that, in contrast to individualism, collectivism entails an "emphasis on the views, needs and goals of the in-group rather than the self."

[4]On the 0 to 100 Hofstede scale of individualism, with 100 indicating a fully individualist culture and 0 indicating a fully collectivist culture, China scores 20 while the US scores 91 (Hofstede, Hofstede and Minkov, 2010). Some scholars suggest contemporary collectivism stems from "collaborative" irrigation practices in pre-industrial agriculture (Talhelm et al., 2014; Buggle, 2017), and that collectivism in China can be traced back to the "extreme cooperation and coordination" needed to produce public projects that protected Chinese villages thousands of years ago (Triandis, 1995, p. 33). Others have argued that collectivism enhances "coordination" and the stability of trading relationships (Gorodnichenko and Roland, 2011, 2017; Hajikhameneh and Kimbrough, 2018).

[5]Building on early work by Tajfel et al. (1971), Akerlof and Kranton formalized identity as a preference for belonging to a particular social category. In their formulation, individ-

ily influenced by the most accessible, activated or context-appropriate (cultural) identity (Higgins, 1996; Peng and Knowles, 2003; Akerlof and Kranton, 2010; Kranton et al., 2016; Kranton and Sanders, 2017). We induce exogenous variation in the influence of those cultures by experimentally varying contextual factors which have been shown to activate specific cultural identities among multi-cultural participants (Shih, Pittinsky and Ambady, 1999; LeBoeuf, Shafir and Bayuk, 2010; Benjamin et al., 2010).[6] This technique ("priming") has been widely used in cultural psychology (for an overview, see Matsumoto, 2001; Nisbett, 2010) as well in several highly regarded studies in the identity economics literature (e.g., Benjamin et al., 2010; Chen et al., 2014; Cohn, Fehr and Maréchal, 2014; Cohn, Maréchal and Noll, 2015), but is largely absent from economists' study of culture.

We conduct two experiments featuring a one-shot anonymous PD, a setting that mimics the type of non-kin arms-length cooperation that economists have singled out as particularly important for economic activity. Across treatments we prime either a Chinese cultural identity or a US cultural identity while, within each session, each participant receives the same prime. In our first experiment, participants play the PD immediately after the identity prime. Our second experiment is identical to the first except that between the culture prime and the PD participants receive concrete information about cooperation rates among similar participants in a nearly identical experiment, our first experiment.

Because our first experiment leaves the beliefs-formation process totally unrestricted, cooperation there can be thought of as incorporating the combined effects of culture on preferences and culture on beliefs. By providing relevant and unbiased information about others' behavior in our second experiment, we aim to substantially weaken or completely shut down culture's effects on beliefs. Heuristically, in our second experiment beliefs should be influenced, if not completely pinned down, by the concrete information we provide. To the extent that we succeed in shutting down culture's effect on beliefs, behavior in the second experiment should primarily reflect the direct effect of culture on preferences. Comparing cooperation rates in our first experiment to cooperation rates in our second experiment

uals derive utility from living up to the prescriptions of their chosen social category. As relevant social categories may vary with context, incorporating identity utility provides "...a theory of decision making where social context matters" (Akerlof and Kranton, 2010, p. 6).

[6]More generally, contextual cues ("primes") can temporarily boost the salience of a specific social category and thereby induce behavior that is prescribed by this social category (Turner, 1985; Benjamin et al., 2010).

therefore provides novel causal evidence on the relative strength of culture's two defining channels, preferences and beliefs. This is the primary contribution of our study. Its importance lies in the notion that correcting miscalibrated beliefs may be more easily accomplished, with more straightforward policy measures, than altering culturally transmitted values.

A priori it is not obvious what to expect. While Eastern/collectivist cultures are commonly characterized by interactions within a tight radius where cooperation is unconditional and bad behavior can be punished, which suggests we should expect more cooperation from Chinese-primed participants, it is also the case that cooperation outside the circle is unusual in these cultures perhaps because one cannot expect preferential treatment from strangers, which suggests the opposite. Several previous studies do document that individuals from collectivist cultures tend to be more cooperative when interacting with community members, even in anonymous settings like ours (Domino, 1992; Triandis, 1995; LeBoeuf, Shafir and Bayuk, 2010; Zhou, Alysandratos and Naef, 2018). Moreover, recent representative survey evidence reveals that behaviors related to cooperation like positive reciprocity, altruism, and trust are consistently more pronounced in China than in the USA (Falk et al., 2018). On the other hand, it is Western/individualist cultures that feature frequent interactions with strangers, less emphasis on *in-group* cooperation and more loosely knit relationships (Yamagishi, Cook and Watabe, 1998; Alesina and Giuliano, 2015; Gächter, Schulz and Thoeni, 2017; Enke, 2018), which suggests we should expect more cooperation from US-primed participants. Supporting this prediction, cross-cultural research highlights that greater market integration, generally more characteristic of individualism than collectivism, is associated with more cooperative and fair behavior in anonymous transactions (Henrich et al., 2001, 2005, 2010). To more explicitly lay out how preferences and beliefs interact in affecting cooperation in our setting, we provide a simple framework building on insights from Identity Economics (Akerlof and Kranton, 2010).

As a preview of our results, in our first experiment we find that priming a US cultural identity substantially *increases* cooperation with anonymous strangers in the PD. US-primed participants are about 25 percentage points more likely to cooperate than their counterparts in our Chinese-prime treatment, who cooperate only about 40% of the time. In our second experiment, which ideally reflects culture's impact on preferences only, we find the exact opposite: US-primed participants cooperate only 41% of the time, while Chinese-primed participants are substantially and significantly more likely to cooperate (59% of them do). The comparison between cooperation in the

first experiment and cooperation in the second experiment suggests that culture's impact on cooperation operates as strongly, if not more strongly, through the beliefs channel as through the preferences channel. Overall, the patterns are consistent with the idea that Chinese culture is associated with cooperative preferences but pessimistic beliefs about strangers and that the opposite is true for US culture.

Several robustness exercises support this view, two of which are worth mentioning here. One primary concern is that unobserved heterogeneity explains the differences in behavior between our first and second experiments. One way we address this concern is by pooling observations from both experiments, controlling for relevant demographics, including a measure of whether the Chinese region to which participants have ties is considered collectivist or individualist, and re-estimating our main econometric specifications. The results paint the same picture as our main analysis. Another way we address this concern is to compare cooperation rates in our first and second experiments among participants who plausibly hold similar beliefs. As an example, we compare the cooperation rates of second-experiment US-prime participants receiving optimistic information about previous cooperation rates to cooperation rates of all first-experiment US-identity prime participants. We find no significant differences in cooperation. This comparison together with the causal evidence of how (induced) beliefs affect cooperation further illustrate the strong impact of beliefs that we find in our first experiment.

Our paper makes several contributions. We provide, to the best of our knowledge, the first causal evidence on the relative importance of cultural transmission of beliefs versus preferences in determining stranger cooperation.[7] Our second contribution is methodological. The by-now standard "epidemiological approach" in cultural economics exploits exogenous variation in inherited beliefs and values with respect to second-generation immigrants' experiences in their countries of residence to provide causal evidence of culture on outcomes (e.g., Fernández and Fogli, 2006; Giuliano, 2007; Fernández and Fogli, 2009; Algan and Cahuc, 2010; Luttmer and Singhal, 2011). We depart from this approach by providing complementary causal evidence that sidesteps concerns about endogeneity due to, e.g.,

[7] Bigoni et al. (2017) provide intriguing correlational evidence. In a repeated public goods game experiment, they find that students from the north and south of Italy exhibit similar preferences for conditional cooperation. In a second experiment, they find that students from both the north and from the south believe northern Italians will contribute more in a similar game. They argue that their findings suggest differences in (conditional) cooperation between the north and south of Italy are explained by differences in beliefs.

omitted variable bias (see Fernández (2008, 2011) and Alesina and Giuliano (2015) for a more thorough discussion of these concerns). This allows us to examine the causal impact of cultural identity on cooperation precisely enough to plausibly identify its effects through separate channels, beliefs and preferences, which leads to our third contribution. We provide an explanation for the seemingly contradictory findings in the transdisciplinary literature on cooperation which sometimes finds that Eastern/collectivist cultures cooperate more and sometimes finds the opposite. Because the separate effects of culture on beliefs and preferences may be substantial and opposite, culture's ultimate effect on behavior requires understanding both channels.

We contribute to the economic literature on identity (Akerlof and Kranton, 2000, 2002, 2005, 2010) by introducing consideration of identity's influence on social perception. We build a framework permitting a straightforward way of relating own-values and beliefs about others' values to stranger cooperation. This is important because, while the identity economics literature has focused on how the in-group vs. out-group distinction affects *preferences* (see *inter alia* Akerlof and Kranton, 2000, 2010; Chen and Li, 2009; Hargreaves-Heap and Zizzo, 2009; Butler, 2014; Kranton et al., 2016; Kranton and Sanders, 2017), the effect of categorization on beliefs about others has eluded serious attention. This oversight is not characteristic of the social psychological literature on identity where there is a long history of asking how categorization affects social perception (see the discussion in Butler, 2018).

Finally, we formally connect the largely separate literatures on identity and culture in economics.[8] Surprisingly, given the intuitive connection between identity's category-specific prescriptions and the beliefs and values central to economists' definition of culture, to date these bodies of economic research remain largely separate (one prominent exception is Besley and Persson (2018)). Connecting these two literatures may benefit both. In particular, the role of cultural identity in coloring beliefs about others features prominently in the culture literature but is absent from the identity economics literature.[9]

[8]The link between culture and identity theory *has* been made by some cultural psychologists, who typically view the in-group vs. out-group distinction as a particularly important aspect of collectivism Triandis (e.g., 1995).

[9]Besley and Persson (2018, p. 4) assert "...the most common approach among economists who study [corporate] cultures focuses on beliefs." At the same time, Akerlof and Kranton's model of identity (Akerlof and Kranton, 2000, 2002, 2005, 2010) takes place in a complete and perfect information setting with no role for beliefs formation, while Bénabou and Tirole

The remainder of the paper proceeds as follows. In the next section, we lay out a framework illustrating how both preferences and beliefs may contribute to cooperation in the PD. We continue by describing in detail our first experiment and presenting its results. Next, we detail our second experiment and results. In the final section we summarize our findings across both experiments and provide concluding remarks. In a Supplementary Appendix we report several robustness exercises.

2 Framework

Here, we present a framework to explain the impact of cultural identity on cooperation in the PD. The framework provides a natural way to incorporate own values and beliefs about others' values into an identity utility model, inspired by Akerlof and Kranton (2005) and building upon Bicchieri (2006).[10] In particular, it allows us to characterize when preferences or a combination of preferences and beliefs matter and thus provides novel insights into how these channels contribute to culture's causal effect on cooperation.

	C	D
C	S,S	W, B
D	B,W	T, T

Figure 1: Prisoners' Dilemma

Consider the one-shot Prisoners' Dilemma game (PD) in Figure 1, where the letters B, S, T, W refer to the Best, Second Best, Third Best and Worst material payoffs, respectively. To explain cooperation in the PD requires assuming that material payoffs are not the only component of utility, as otherwise C(ooperation) is strictly dominated by D(efect). Therefore, we assume that overall utility is composed of both economic utility, stemming from material incentives, and identity utility associated with living up to one's ideals. Ideal traits and behaviors may obviously be internalized at the individual level, but may also be prescribed by one's social categories, or

(2003, 2006*a*,*b*) model identity as beliefs about oneself and leave outside the model how these beliefs might color beliefs about others.

[10]Note that Social Identity Theory, beginning with Tajfel et al. (1971), gives scope for beliefs to matter as it is a theory of group-contingent preferences, where uncertainty about what groups others belong to should translate into uncertainty about the utility consequences of particular actions.

identities. For simplicity, we assume non-economic utility is completely determined by living up to the prescriptions of an individual's (social) identity. We further assume that all identities prescribe cooperation irrespective of others' identities. This allows us to simplify our notation and denote ideal action by $a^I = C$.

We write overall utility as follows, where $x_i(a_i, a_j)$ is Player i's material payoff from playing a_i against the co-player's action a_j and $\gamma_i \geq 0$ is an idiosyncratic parameter capturing the weight given to identity concerns.

$$U_i(a_i, a_j) = x_i(a_i, a_j) - \gamma_i f(a_i, a^I)$$

For concreteness, let f be a simple indicator function taking the value one when an individual deviates from the prescription a^I, and zero otherwise. In the transformed payoff matrix resulting from incorporating identity utility, cooperation is an optimal response to co-player cooperation whenever:

$$U_i(C, C) = S \geq B - \gamma_i = U_i(D, C)$$

In this case, the loss of identity utility outweighs the potential monetary gain from defecting on a cooperating co-player, which reduces to a threshold condition: $\gamma_i \geq B - S$

Similarly, when $\gamma_i \geq T - W$ cooperation becomes an optimal response to co-player defection:

$$U_i(C, D) = W \geq T - \gamma_i = U_i(D, D)$$

If both of these thresholds are simultaneously satisfied, which would be the case if, e.g., $T - W \geq B - S$, then C is a dominant strategy and strictly dominant if strict inequalities hold in the threshold conditions.

All together, we can break the identity-concern parameter space into three intervals:

$$\begin{aligned}
\gamma_{low} &= [0, \min\{B - S, T - W\}) \\
\gamma_{med} &= [\min\{B - S, T - W\}, \max\{B - S, T - W\}] \\
\gamma_{high} &= (\max\{B - S, T - W\}, \infty)
\end{aligned}$$

For simplicity, abusing notation slightly we denote by $0 \leq b_i(\gamma_{low}) \leq 1$ Player i's belief that Player j's identity-concern parameter lies on the interval γ_{low}. We define $b_i(\gamma_{med})$ and $b_i(\gamma_{high})$ analogously. These intervals

immediately illustrate how culture may exert distinct effects on preferences (through, e.g., γ_i) and beliefs about others' types ($b_i(\gamma_{k\in\{high,med,low\}})$), i.e., social perception. On the first interval, $\gamma_i \in \gamma_{low}$, Player i cares little about identity so that defect is a strictly dominant strategy. Conversely, when Player i cares quite a lot about identity, $\gamma_i \in \gamma_{high}$, cooperate is a strictly dominant strategy.

Notice that beliefs about a co-player's identity-concern parameter are irrelevant whenever a player has a dominant strategy. However, beliefs come into play for an intermediate range of identity-concern, $\gamma_i \in \gamma_{med}$, where either cooperation or defection is a possible outcome. Suppose that beliefs are correct. If Player i is certain that Player j's identity-concern parameter is low, $b_i(\gamma_{low}) = 1$, then Player i knows defection is Player j's dominant strategy to which Player i optimally responds by defecting — mutual defection is the unique equilibrium outcome in this case. If, on the other hand, $b_i(\gamma_{high}) = 1$, then Player i knows that cooperate is Player j's dominant strategy; Player i's optimal response is cooperation, implying that mutual cooperation is the unique equilibrium. For $b_i(\gamma_{med}) = 1$, either mutual cooperation or mutual defection is an equilibrium, as the transformed payoff matrix essentially becomes a coordination game.[11] The benefit of our Identity utility approach is that it permits a relatively simple Bayesian game approach to understanding the relationship between beliefs and cooperation.[12]

Summing up, our framework demonstrates that culture's influence through

[11]Compare this with the useful framework presented by Bicchieri (2006), summarized in Figure S1. In her view, (psychic) utility costs from violating others' "expectations" (i.e., player's social and moral norms (Schram and Charness, 2015)) may turn the PD into a coordination game. If there is uncertainty about what others expect, and expectations are summarized as a player's type, the overall situation can be represented by a Bayesian game. In the more complex psychological game case (Geanakoplos, Pearce and Stacchetti, 1989; Battigalli and Dufwenberg, 2007; Bernheim, 1994) where "expectations" are taken to mean, e.g., the row player's (second-order) beliefs about the column player's (first-order) beliefs about the row player's behavior, a Bayesian game representation may still be *empirically* valid (see the discussion in Butler et al. (2016)).

[12]Of course, there are many alternative models relating beliefs about others to behavior. For instance, if we assume a preference for conformity (Bernheim, 1994), then if individuals believe others will cooperate — for whatever reason — they may be more likely to cooperate as well. Guilt or disappointment aversion together with the assumption that others (mathematically) expect cooperation, delivers a similar result (Geanakoplos, Pearce and Stacchetti, 1989; Battigalli and Dufwenberg, 2007; Charness and Dufwenberg, 2006). We remain agnostic on the precise model relating beliefs to behavior, but note that many models imply a positive relationship between beliefs about the likelihood others will cooperate (or will expect cooperation) and an individual's propensity to cooperate.

beliefs can have a significant impact on behavior, potentially even stronger than its effect through preference. For example, a culture that succeeds in transmitting moderately strong cooperative preferences ($\gamma_i \in \gamma_{med}$) but fails in transmitting similarly strong beliefs that strangers will share this value ($b_i(\gamma_{low}) = 1$), as might be the case in Eastern/collectivist cultures, may fail to generate *any* cooperation. On the other hand, a similar *preference* for cooperation may generate quite a lot of cooperation in a culture that succeeds in transmitting a strong belief that others will cooperate ($b_i(\gamma_{high}) = 1$), which some evidence suggests is characteristic of Western/individualist cultures.

This has two implications for our setting. Firstly, in the first experiment where culture can affect behavior through preferences *and* beliefs, we expect to observe less cooperation from Chinese-primed participants than from US-primed participants. Secondly, shutting down culture's effects on beliefs and thus revealing culture's influence through preferences alone, as we attempt to accomplish in our second experiment, should lead to a reversal of this pattern: higher cooperation under the Chinese prime than under the US prime.

3 Design and Procedures: Experiment 1

Our first experiment consisted primarily of two phases: an identity priming phase and a game-playing phase. In the identity priming phase participants answer a short questionnaire, which begins with filler questions about demographics and student life. The last three items on the questionnaire constitute our priming instrument, which follows closely LeBoeuf, Shafir and Bayuk (2010) and vary across treatments (see the Supplementary Appendix for the questionnaire). In particular, we take the two US-prime and Chinese-prime questions mentioned verbatim from their study. Additionally, we add a free-form response question about cultural experiences to our priming instrument closely modeled on the priming instrument in Peng and Knowles (2003). About 40 percent of participants were randomly assigned questions making their Chinese identity temporarily salient (e.g. "what is your favorite Chinese holiday?"), while another 40 percent received questions making their US identity salient (e.g. "what is your favorite US holiday?"). The remaining 20 percent served as a control group and answered questions unrelated to their Chinese or US identity (e.g. "What is your favorite place on campus?").

The game-playing phase was identical across all three treatments and

consisted of several experimental games and individual decisions. Immediately after the identity prime, we randomly assigned participants to anonymous pairs to play the one-shot PD, depicted in Figure 1, with payoff parameters (indicating dollar amounts) $B = 12$, $S = 11$, $T = 5$, and $W = 0$. Note that the symmetry of the PD permits use of the direct response method, making it particularly appropriate when investigating decisions potentially involving a substantial non-deliberative component, such as cooperation. We familiarized students with, and quizzed students on, the bi-matrix notation used to describe the PD at the very beginning of each session – well before the priming instrument. This allowed us to implement the PD immediately following the identity prime, when the effects were most likely to be detectable.

After the PD, participants completed several other tasks and a post-experiment survey. As these other tasks are not the focus of our study, we analyze them here primarily in the context of robustness exercises (see the Supplementary Appendix for details). Before beginning the experiment, participants were instructed that there would be several "decision situations" during the experiment and that each separate decision situation would be clearly labeled, but were given no other information about what these decision situations would entail. Importantly, they were instructed that only one of these situations would be randomly chosen by the computer to determine their entire earnings for the experiment. This design feature ensures each decision situation is incentive compatible and mitigates against hedging across decisions.

The experiment was implemented using z-Tree (Fischbacher, 2007) in the Xlab at the University of California, Berkeley. Because our hypotheses rely on the assumption that participants identify with both a Chinese and US cultural identity, we aimed to recruit only students who originated from a Chinese country (China, Taiwan, Hong Kong, and Singapore). Due to restrictions of the recruiting software and limited data availability, screening was done by common Chinese last names and the list of eligible students was then hand selected from the entire subject pool. In total, we recruited 124 participants. However, the ex-ante screening on nationality was not perfect and, consequently, we exclude ex-post from our analysis any participant who did not have at least one parent born in a Chinese country, yielding a total of 102 participants. Note that excluding participants ex-post neither adversely affects sample sizes in the three treatments (Fisher's exact test, $p = 0.65$) nor the balance of observable characteristics of participants (see the Supplementary Appendix).

The sessions took place from the fall of 2014 through the spring semester

of 2015. All participants in a session participated in the same treatment: 39 participants in the US-prime treatment, 42 in the Chinese-prime treatment and 21 who received the Neutral prime.[13] Sessions lasted about 45 minutes and participants earned $16 on average.

4 Results: Experiment 1

4.1 Main result

We are primarily interested in how cultural identity affects cooperation as measured by behavior in the PD. Overall, we find higher cooperation rates in the US-prime treatments (66.6%) than in the Chinese-prime treatments (42.9%), which is similar to cooperation in the Neutral-prime treatment (42.9%). The difference in cooperation rates between the US-prime and Chinese-prime treatments is statistically significant at conventional levels ($p = 0.04$; two-tailed permutation test). The effect size is also substantial according to commonly-used guidelines (Hedges' g = 0.447).[14] Cooperation rates are summarized graphically in Figure 2.

It is possible that other variables confound this result. For example, demographics may not be exactly balanced across treatments so that what appears to be a priming effect may simply reflect, e.g., a gender difference in cooperation. As a first pass at addressing this concern, in the Supplementary Appendix we report multiple balance checks showing that an important, albeit limited, set of demographics such as age, gender and college major does not vary across our treatments.

As a second pass, we estimate econometric models in which we explicitly control for participants' observable characteristics. To account for arbitrary within-session correlations in behavior stemming from, e.g., differences in demographic variables we did not collect or traits we cannot observe, we cluster standard errors by session. We insert a full set of indicator variables for our three treatments which allows us to answer the important auxiliary question of whether a US identity increases coopera-

[13] An exception to this rule is that in the Chinese-prime treatment we inserted a filtering question before the priming survey asking where the participants' family originated from. Those who indicated a non-Chinese country subsequently received the Neutral-prime survey to not confuse them with Chinese-specific follow-up questions from the Chinese-prime survey (e.g., "what is your favorite Chinese holiday?"). These participants are among the excluded participants.

[14] For example, the US Department of Education considers an effect size, or Hedges' g, larger than 0.25 to be "substantively important"(Institute of Education Sciences, 2014, p. 23).

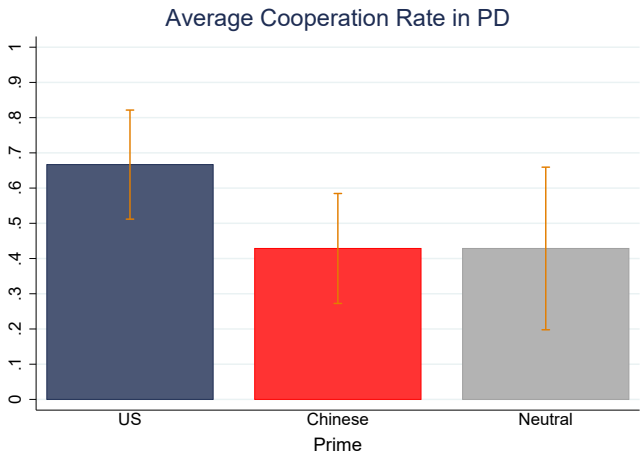

Figure 2: Cooperation Rates in PD, Experiment 1

tion or a Chinese identity reduces cooperation relative to the baseline in which neither of these identities is primed.

We report the marginal effects from a series of probit models in Table 1. The primary explanatory variables common to all columns are a set of dummy variables for our treatments, with the Chinese-prime treatment serving as the excluded category. For comparison with patterns in the raw data, the first column includes no additional controls. The estimated marginal effect of the US prime relative to the Chinese prime, 23.3 percentage points, matches the percentage point difference observed in the raw data and is highly significant ($p = 0.008$). The zero marginal effect associated with the Neutral-prime treatment suggests that the Chinese-identity prime has little effect on cooperation. In the second column, we include individual controls for age, gender, whether the participant reported being an economics major as some have argued that economics students are less pro-social (see, e.g., Frank, Gilovich and Regan, 1996), and the birthplace of participants. This last control takes the form of an indicator for being born in the US, i.e., for being a second-generation immigrant. Controlling for these demographics slightly increases the coefficient estimate of the US prime, which remains highly statistically significant ($p = 0.003$). None of the controls are individually statistically significant, nor are they jointly significant ($\chi^2(4) = 4.14; p = 0.39$), providing reassurance that our findings are not confounded by demographics.

In columns (3) and (4) of Table 1 we aim to control for the possibility

Table 1: Regression: Cooperation rates in the PD – Experiment 1

	Dependent variable: PD cooperation			
	(1)	(2)	(3)	(4)
US prime	0.233***	0.250***	0.290***	0.276***
	(0.088)	(0.085)	(0.109)	(0.085)
Neutral prime	0.000	-0.0113	0.0334	0.0209
	(0.126)	(0.130)	(0.125)	(0.125)
Individual controls	no	yes	yes	yes
Observations	102	102	83	98
Pseudo R^2	0.04	0.06	0.07	0.07

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Each column reports marginal effects estimates from a Probit model. Columns 3 and 4 restrict the data to participants who believed our payment procedure and participants who have not identified the purpose of our study, respectively. Controls include age, gender, economics major, and birthplace of participant (born in US). Robust standard errors clustered by session appear in parentheses.

that our treatment estimate is biased by participants who did not believe our payment procedure (i.e., one randomly determined task) or who identified the purpose of our experiment. Excluding those participants leads to stronger treatment effects in both cases. In the Supplementary Appendix, we report several other robustness tests adding further reassurance.

Summarizing the evidence so far, the data reveal that priming a US identity substantially and significantly increases cooperation relative to the counter-factual of priming a Chinese identity. Comparing behavior to the Neutral-prime treatment suggests that the comparative increase in cooperation associated with a US cultural identity primarily works through the US identity increasing cooperation rather than a Chinese identity decreasing cooperation.

4.2 Evidence on the Beliefs Channel

Before turning to our second experiment, we outline evidence from our first experiment that is consistent with a prominent role for the beliefs channel in culture's effect on cooperation. As our first piece of evidence, we use elicited beliefs. At the end of the experiment, we explicitly asked participants to report what proportion of their fellow PD participants they

thought would cooperate. In Figure 3, we present participants' elicited beliefs side-by-side with actual cooperation rates. Qualitatively, beliefs follow the same pattern as actions: the US prime induced the highest cooperation rates and is associated with the most sanguine beliefs about others' cooperation. The difference between Chinese-prime and US-prime participants' beliefs is not statistically significant, however (Mann-Whitney test, $p = 0.39$).

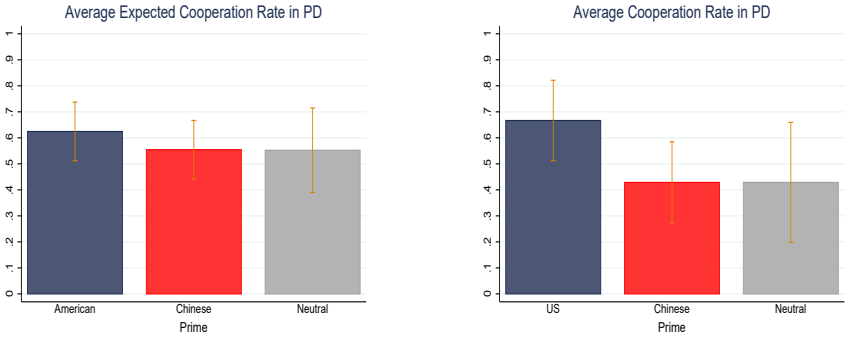

Figure 3: PD Cooperation: beliefs vs behavior

As a second piece of evidence, we examine another question from the post-experiment survey in which we asked participants about the prevalence of common Asian-American stereotypes, such as being trustworthy and cooperative. As would be consistent with the US-prime inducing more sanguine beliefs about co-player cooperation, we observe that participants in the US-prime treatment reported believing that Asian Americans are more trustworthy and cooperative than did their Chinese-prime counterparts (Mann-Whitney test, $p = 0.02$).[15]

When interpreting survey responses, however, we should keep two things in mind. First, elicited beliefs were not remunerated. We did this to keep the beliefs-elicitation procedures as simple as possible.[16] Second, a primary issue with relying on elicited beliefs or beliefs-related questions, such as the stereotype question, is that they may be tainted by behavior. Previous research suggests not only that individuals *do* in fact project their own behaviors onto others when forming beliefs in a process known

[15]Following Kling, Liebman and Katz (2007), we standardize the answers to the two stereotype questions and use the equally-weighted average for our test.

[16]Some evidence suggests that while incentivizing belief elicitation improves consistency with own behavior, non-incentivized beliefs are as predictive of others' behavior as incentivized beliefs (e.g., Trautmann and Kuilen, 2015).

as "false consensus" (Ross, Greene and House, 1977; Butler, Giuliano and Guiso, 2015), but that they *should* (Schelling, 1966, p.150).

As a final bit of evidence, we consider behavior in decision situations where beliefs about others' cooperativeness should be less central: a sequential prisoners' dilemma and dice-rolling tasks permitting lying for financial gain. In the Supplementary Appendix we show that our primes did not affect behavior in these subsequent situations.

All together, the evidence from Experiment 1 suggests that culture affects cooperation through both a preference channel and through a beliefs channel. In fact, the evidence presented so far hints at a prominent role for the beliefs channel. To get a handle on how much of culture's effect on observed behaviors operates through beliefs while avoiding predictable beliefs-tainting confounds, such as false consensus, we conducted a second experiment. In this second experiment, we minimized culture's direct effect on beliefs formation by providing concrete and credible information about the likelihood of others' cooperation.

5 Design and Procedures: Experiment 2

Treatments in our second experiment followed exactly the same procedures as in our first experiment except that immediately before participants made their decisions in the PD we provided them with information about cooperation rates in our previous experiment.[17]

To convey information about past behavior in the PD, participants viewed a screen immediately before submitting their decisions in the PD with the following text (where decisions "option L" and "option T" refer to the cooperative outcome): "We have conducted many experimental sessions here in the X-lab over the last year that have involved exactly *this same game*. Participants in these previous experimental sessions were recruited from *the same pool of people* as participants for today's experiment. To give you an idea of how previous participants in the role of the column [row] player played this game, a random selection of 10 participants reveals that X out of these 10 participants chose 'option L' ['option T']."

To fill in the "X" for each participant, we drew a sequence of random samples of size ten from the Experiment 1 PD data. Within each treatment, each participant received information derived from a different element in

[17] Because recruiting Chinese participants proved difficult in Experiment 1, to economize on the number of participants needed for adequate statistical power, in Experiment 2 we implemented only the Chinese-prime and the US-prime treatments.

this sequence. To plausibly induce similar or, ideally, identical beliefs across our two treatments, we assigned elements of the sequence in parallel to participants across treatments.

To illustrate the process, suppose we drew 100 random samples of size ten from our Experiment 1 PD data which resulted in a sequence $\{X_1, ..., X_{100}\}$ of cooperation rates. The first participant in the Chinese-prime treatment of Experiment 2 would receive the information X_1, the second participant would receive information X_2 and so on. The first participant in the US-prime treatment of Experiment 2 would also receive information X_1, while the second US-prime participant would receive information X_2, etc. We drew our sequence of subsamples before conducting any sessions of our second experiment. Our sequence was sufficiently long to ensure that we would not exhaust its elements.

This process introduces the same information across treatments but different information across individuals, permitting us to estimate the causal impact of culture on *preferences* for cooperation holding (induced) beliefs fixed in both conditions. This is the primary advantage over the perhaps more straightforward option of transmitting the same information to all participants. It also allows us to truthfully instruct participants that we "randomly selected" the information we were providing without running the risk of selecting unlikely information to transmit (e.g., all previous participants cooperated) raising concerns about experimenter demand effect.

We conducted the experiment at the X-lab of the University of California, Berkeley and, again, attempted to recruit only Chinese students. Because we were restricted to the same recruiting process as in Experiment 1 (inviting only participants with common Chinese last names), the recruitment was not perfectly successful. Therefore, we exclude from our analysis any participant who did not have at least one parent born in a Chinese country.[18] In total, we have observations from 132 participants: 68 in the US-prime treatment and 64 in the Chinese-prime treatment. Sessions lasted about 45 minutes and participants earned $16 on average.

[18]One might worry that excluding ex-post participants who were neither first- nor second-generation Chinese may have introduced an imbalance in information transmitted within each treatment. However, this was not the case. On average, Chinese-prime participants were informed that 50% of previous participants cooperated in the PD, while this figure was 48% for US-prime participants, a difference which was far from significant ($\chi^2(6) = 2.59, p = 0.86$).

Table 2: Regression: Cooperation rates in the PD – Experiment 2

	Dependent variable: PD cooperation				
	(1)	(2)	(3)	(4)	(5)
US prime	-0.179***	-0.170***	-0.176***	-0.106	-0.182***
	(0.059)	(0.058)	(0.064)	(0.080)	(0.068)
Information		0.049*	0.047*	0.066**	0.041*
		(0.025)	(0.026)	(0.027)	(0.024)
Individual controls	no	no	yes	yes	yes
Observations	132	132	132	92	118
Pseudo R^2	0.02	0.04	0.06	0.08	0.05

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Each column reports marginal effects estimates from a Probit model. Columns 4 and 5 restrict the data to participants who believed our payment procedure and participants who have not identified the purpose of our study, respectively. Controls include age, gender, economics major, and birthplace of participant (born in US). Robust standard errors clustered by session appear in parentheses.

6 Results: Experiment 2

6.1 Main result

By exogenously introducing information about how previous participants drawn from the same subject pool played the PD, our goal was to greatly ameliorate the direct influence that our primes had on the beliefs-formation process, while leaving intact their direct influence on preferences. This allows us to more clearly reveal the effect of our cultural primes on *preferences* for cooperation.

We start by examining behavior in the PD. The cooperation rate in the US-prime treatment was 41.2%, whereas 59.4% of participants in the Chinese-prime treatment chose to cooperate. In the raw data, this difference is statistically significant at a nearly conventional level ($p = 0.053$; two-tailed permutation test) and, again, substantial (Hedge's g = 0.369). It is also the mirror image of cooperation rates in Experiment 1, potentially reflecting the negligible impact of culturally transmitted beliefs in this setting. Before discussing beliefs directly, we turn to a more rigorous analysis of cooperative behavior in Experiment 2.

Following our analysis for Experiment 1, we report marginal effects from several probit model estimates of PD cooperation in Table 2. In our

most basic specification (column 1) our only explanatory variable is an indicator for the US-prime treatment. The negative and statistically significant coefficient indicates that a US cultural identity *reduces* cooperation substantially. In column 2, we control for the information about previous cooperation the participant received and in column 3, we introduce the same demographic controls as in our analysis of Experiment 1. None of these additional controls have much of an effect on the estimated magnitude, sign, nor statistical significance of the marginal effect of a US identity. The specific information participants received is significantly and positively associated with cooperation, suggesting that information moved beliefs and that both beliefs and preferences play a role in cooperation. As in Experiment 1, none of the demographic controls are significant, neither jointly nor individually. In columns 4 and 5 we restrict our sample. Excluding participants who did not believe that their decisions would affect their payment reduces the magnitude and significance of the US-prime coefficient. In the last column we exclude participants who identified the purpose of our experiment, which yields estimates and statistical significance similar to the first three columns.

In the Supplementary Appendix, we further substantiate our results with the same set of robustness checks we conducted for Experiment 1 (see Table S5). In addition to these robustness checks, in Table S6 we pool the data from the US-prime and Chinese-prime treatments across both of our experiments. We then re-estimate our treatment effects both with and without a full set of common controls (age, gender, collectivism, etc). As before, the controls are neither individually nor jointly significant and including them has little effect on the magnitude or significance of our primary coefficients of interest. In particular, we find no indication that the behavioral differences in our second experiment are driven by differences in historical collectivism associated with wheat versus rice cultivation. In summary, our results appear robust to a wide range of potentially important confounders.

6.2 The Role of Beliefs

We now discuss the role of beliefs in more detail. Using the fact that we randomized the information participants received about previous cooperation, we can look at the causal impact of different information on behavior holding cultural identity fixed. This allows us, in particular, to illustrate the strong impact of (induced) beliefs on behavior.

We start by examining the broad effects of the information provided on cooperation in the raw data. As expected, the provided information

about others' cooperation is positively related to cooperation ($p = 0.041$).[19] Digging a little deeper, we go one step further and divide the provided information into three categories: optimistic information (i.e., that more than 5 in 10 previous participants cooperated), neutral information (exactly 5 in 10 previous participants cooperated), and pessimistic information (fewer than 5 in 10 previous participants cooperated). Using these categories, we find that optimistic information generates more cooperation (0.64) than neutral information (0.48) which, in turn, generates more cooperation than pessimistic information (0.39, Jonckheere-Terpstra Test for ordered alternatives, $p < 0.01$). This analysis highlights that the same prime can result in very different behavior, depending on the (induced) beliefs. This lends additional credibility to the observed relative strength of beliefs in Experiment 1.

Another comparison we conduct gets closer to the fundamental question of whether the information we provided shut down the effect of culturally transmitted beliefs. To do this, we construct subsets of participants who plausibly hold similar beliefs across our two experiments. For example, in Experiment 1 we found that the US-prime increased cooperation, ostensibly partially due to optimistic (culturally transmitted) beliefs. A comparable subset of participants in Experiment 2 might be US-prime participants who received optimistic information, as defined above. Similarly, if the lower cooperation rate among Chinese-prime participants in Experiment 1 is partially due to pessimistic cultural beliefs, then a plausible Experiment 2 comparison group would be Chinese-prime participants who received pessimistic information, as defined above. In Figure S2 of the Supplementary Appendix, we present exactly these comparisons. Comparing Experiment 1 Chinese-prime participants to Experiment 2 Chinese-prime participants who received pessimistic information, we find cooperation rates to be virtually identical (43% vs. 46%, Fisher's exact test, $p = 1$). At the same time, comparing US-prime Experiment 1 participants to US-prime Experiment 2 participants who received optimistic informa-

[19] An interesting ancillary question is the effect of the information provided on the beliefs we elicited in the post-experiment survey. One might expect these elicited beliefs to strongly reflect the information we provided, since participants received no feedback during the experiment about actual behavior. While this was the case for US-prime participants (0.53 vs. 0.48, Wilcoxon signed-rank test, $p = 0.41$), average reported beliefs among participants in the Chinese-prime treatment were substantially higher than the information they received (0.65 vs 0.50, $p = 0.01$). However, because these beliefs were again not incentivized their interpretation is subject to the same caveats about being possibly tainted by participants' behavior mentioned earlier. Perhaps reflecting such concerns, it turns out that reported beliefs in the Chinese-prime treatment were particularly high among PD-cooperators.

tion also reveals a non-significant difference (67% vs. 55%, Fisher's exact test, $p = 0.42$). Overall, these comparisons suggest that *but for* differences in (induced) beliefs, the Chinese prime and US prime have similar effects on behavior across our two experiments.

The framework presented in Section 2 outlines when and how beliefs about others' identity concerns might affect behavior. Consistent with this framework, the findings of Experiment 2 indicate that beliefs play a substantial role in cultures' impact on cooperation. After constraining culture's capacity to affect beliefs, we find that a collectivist/Eastern cultural identity has the effect on the *preference* for cooperation that is commonly assumed, i.e., the enhancement of the attractiveness of cooperative behavior. This contrasts sharply with our findings in Experiment 1, which represent the joint effect of culture's effects on preferences and beliefs, where we find the exact opposite. There, an individualist/Western cultural identity induces more cooperation. Putting these two sets of results together suggests that culture's effect on the beliefs-formation process in situations involving strategic uncertainty is strong enough to overturn its effects on preferences, as outlined in our framework.

7 Concluding Remarks

In this paper, we adopt the view that cultural identity is one aspect of individuals' typically multi-faceted self-concept and investigate how it affects cooperative behavior. We present a simple identity-utility framework, following Akerlof and Kranton (2005) and building upon Bicchieri (2006), that allows us to delineate the impact of two major channels: preferences and beliefs. To reveal the causal impact of particular identities on behavior, we borrow a technique, priming, that has been used repeatedly successfully in the identity economics literature. In an initial experiment using participants with ties to Chinese and US culture, we primed either their Chinese (Eastern/collectivist) identity, their US (Western/individualist) identity or neither and found that the individualist/Western cultural identity induced substantially more cooperation. This is in line with our framework as well as the view that individualist cultures typically transmit more optimistic beliefs about others' willingness to cooperate. Accordingly, this result highlights the importance of beliefs.

To provide evidence on the relative impact of the beliefs channel in particular, we conducted a second experiment in which we attempted to constrain the direct effects of culture on beliefs by providing concrete infor-

mation about previous participants' cooperation rates. Shutting down the beliefs channel, our data suggest that a collectivist/Eastern cultural identity enhanced participants' preference for cooperation. Comparing results across experiments, our results imply that the cooperation patterns in our first experiment largely stem from a relative pessimism about strangers' cooperation rates being induced by Chinese culture relative to US culture. Consequently, overall our results suggest that culture's effect on the beliefs formation process in situations of strategic uncertainty warrants more than the scant research attention it has thus far received.

Taking our results at face value, one may wonder how they can be reconciled with key findings in the cultural economics literature documenting long-term persistence of cultural traits and behavior. While evidence on the long-term persistence is accumulating, it is important to note that there is a recent debate among prominent scholars on cultural persistence. Fernández (2011, p. 484) argues forcefully that "... a definition of culture that considers [culture] to be slow-moving (see, e.g. Guiso, Sapienza, and Zingales (2006)) is rejected. The speed of cultural change depends on how quickly social beliefs and preferences change over time, which in turn depends on the environment broadly speaking ...." Our results contribute support to both sides of this debate. We demonstrate the importance of both an ostensibly slow-moving component — preferences — as well as a cultural component that may move much more quickly, i.e., beliefs. Indeed, our second experiment suggests that particular cultural beliefs may be readily updated to incorporate new information. Whether and, if so, how quickly, these updated beliefs are subsequently absorbed and transmitted beyond the individual by culture is an important open question.

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Supplementary Appendix

Figures

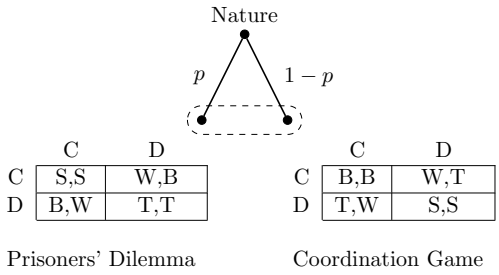

Figure S1: Bayesian Game, adapted from Bicchieri (2006)

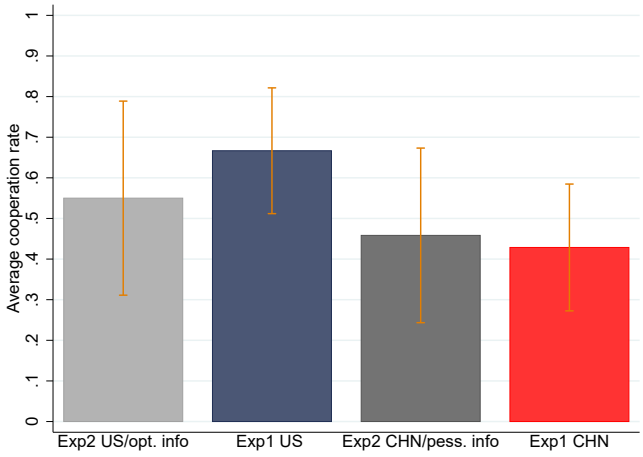

Figure S2: Cooperation Rates across Experiments with same Prime and Beliefs

Priming instrument

1. What is your age?
2. What year in school are you?
3. *[Chinese prime only]* What is your ethnicity?
4. *[Chinese prime only]* From which country did your family originate?
5. What is your major field of study?
6. Do you live on or off campus?
7. How many hours do you spend in a typical week (7 days) on the following activities?
   (a) Attending movies, concerts, sports, or other entertainment events
   (b) Participating in physical exercise, recreational sports, or physically active hobbies
   (c) Socializing with friends
   (d) Spending time with family
8. *[US prime]* Where do you live? *[Chinese prime]* Where were your parents born? *[Neutral prime]* During this academic year, what was the average number of hours per night you slept on weeknights?
9. What is your favorite [*US prime:* American holiday?] [*Chinese prime:* Chinese holiday?] [*Neutral prime:* place on campus?]
10. Everyday, we have various cultural experiences. In the boxes below, please briefly describe up to 5 incidents, activities or encounters on campus that have happened to you that make you feel comfortable [*US prime:* with American culture.[[*Chinese prime* with Chinese culture.] [*Neutral prime:* as a student at UC Berkeley.] It could include people, events, or objects in society or on campus.

Additional Tasks in Experiment 1 and 2

Following the Prisoner's Dilemma (PD), we randomly and anonymously re-paired participants to play a sequential version of the PD (SPD). Because we were particularly interested in participants' decisions as a second mover, we presented the game in its normal form (see Figure S3) and participants played both roles – first as a row player (second mover) and then as a column player (first mover).

	C	D
CC	\$11, \$11	\$0, \$12
DD	\$12, \$0	\$5, \$5
CD	\$11, \$11	\$5, \$5
DC	\$12, \$0	\$0, \$12

Figure S3: Sequential Prisoners' Dilemma

Next, participants were given three dishonesty task adapted from Fischbacher and Foellmi-Heusi (2013). For each of these opportunities, participants rolled a 10-sided die in private and reported the result, which determined their own earnings. Across each of these three opportunities we varied who else the die roll affected, pitting own earnings against: i) nobody; ii) another randomly chosen participant in the same session; iii) a well-known charity. The mapping between earnings consequences and the number on which the die landed was randomized across individuals in order to minimize the influence of unintentional cheating (e.g., confusing a "6" for a "9").

After the dishonesty opportunities, we elicited participants' risk attitudes using a multiple-price list procedure (see Charness, Gneezy and Imas (2013) for a recent overview of risk elicitation methods). The procedure included 18 decisions between a certain payment of \$15 and a binary lottery paying \$30 with probability x and \$0 with probability $(1 - x)$, where x increased from 0.25 in increments of 0.03.

Finally, participants answered a short exit questionnaire including socioeconomic demographics, hypothetical measures of risk and time preferences and measures for cognitive ability. Additionally, we asked participants about their understanding of the experimental tasks, about the reasoning behind their decisions in the experiment as well as about the purpose of the experiment.

Balance Checks

In Table S1 and S2 we present summary statistics of observable characteristics, separately for each priming treatment and along with p-values from non-parametric Fisher's exact tests and ranksum tests. All treatment comparisons have p-values well-above conventional levels of significance ($p > 0.1$), except for the share of students with Taiwanese parents in Experiment 1. We nevertheless control for all variables in our regression analysis.

Table S1: Summary statistics: Experiment 1

Variable		Chinese prime		US prime		p-value
	N	mean	sd	mean	sd	
Male	81	0.36	0.07	0.26	0.07	0.35
Age	81	19.9	0.35	19.8	0.28	0.81
Econ	81	0.14	0.05	0.15	0.06	1.00
Born in US	81	0.52	0.08	0.33	0.08	0.12
Taiwan	81	0.02	0.02	0.15	0.06	0.05
Hongkong	81	0.10	0.05	0.05	0.04	0.68
Wheat cultivation	61	0.18	0.08	0.19	0.07	0.62

Table S2: Summary statistics: Experiment 2

Variable		Chinese prime		US prime		p-value
	N	mean	sd	mean	sd	
Male	132	0.39	0.06	0.32	0.06	0.47
Age	132	20.2	0.25	20.1	0.28	0.51
Econ	132	0.14	0.04	0.24	0.05	0.86
Born in US	132	0.44	0.06	0.41	0.06	0.55
Taiwan	132	0.20	0.05	0.18	0.05	0.83
Hongkong	132	0.05	0.03	0.06	0.03	1.00
Wheat cultivation	91	0.27	0.08	0.41	0.07	0.23

Table S3: Balance check Experiment 1 & 2

	Experiment 1			Experiment 2
	US prime	Chinese prime	Neutral prime	
	(1)	(2)	(3)	(4)
Male	0.51	0.22	0.40	0.46
Age	0.85	0.88	0.99	0.84
Economics major	0.76	0.55	0.30	0.18
Born in US	0.04	0.29	0.24	0.80
Taiwan	0.32	0.08	0.56	0.73
Hongkong	0.76	0.17		0.77

Notes: Columns 1–4 report p-values from a series of probit regressions of the form $y = \alpha + \beta Covariate_i + \epsilon_i$, where $Covariate_i$ is the variable listed in the row and y is a treatment indicator.

In addition, we estimate probit models of the probability of being randomized into one of the three treatments on individual characteristics (age, gender, major, etc). Each regression takes the form $y = \alpha + \beta Covariate_i + \epsilon_i$, where $Covariate_i$ is the an observed individual characteristic and y is a treatment indicator. Table S3 presents the p-values for each covariate and shows that the randomization is balanced along observable characteristics.

Further Evidence on the Belief Channel in Experiment 1

To provide further evidence on the role of beliefs, we examine behavior in tasks where beliefs about others' cooperativeness should be less central. This is the case in a sequential prisoners' dilemma (SPD, see Table S3) and dice-rolling tasks permitting lying for financial gain. If the belief channel plays a prominent role in culture's effect on cooperation in Experiment 1, then we should observe no differences in behavior between treatments in these two tasks.

In the SPD, second-movers can condition their behavior on their partner's cooperation, such that beliefs are irrelevant for the decision to cooperate or not. Indeed, we find no evidence that second-mover SPD strategies are affected by our prime. In the US-prime treatment about 56% are conditional cooperators (choose cooperate if the first mover cooperates and defect otherwise), while in the Chinese-prime treatment the share is about 52%. Looking at the whole distribution of choices, we find no significant

differences in choices across primes (Fisher's exact test, $p = 0.49$).

Another decision in which beliefs should play little role is in the die-rolling task, as there is no strategic uncertainty involved. Here, we also we find no evidence that our primes affected behavior. For example, reports do not vary across treatments in the die-rolling task where an increased empathy for laboratory-mates might show up as less dishonesty when higher own-gains are pitted against others' earnings (Mann-Whitney test, $p = 0.24$).

Robustness Checks for Experiment 1 and Experiment 2

Here, we present several robustness checks validating our results from both experiments (see Table S4 and Table S5). First, we control for cultural variation within Chinese countries in various ways. In column (1) we control for participants with at least one parent born in Hong Kong or Taiwan. This does not change our conclusions from Experiment 1 and Experiment 2. Going one step further, we restrict the sample to participants with at least one parent born in mainland China (column 2) or with both parents born in mainland China (column 3). Again, we see that the treatment estimates remain significant. Finally, we address the concern that historical crop cultivation practices in mainland China may have led to varying degrees of collectivism. Specifically, we use information on birth cities of parents to identify whether participants have ancestral roots in the regions north of the Yangtze river where wheat cultivation may have shaped a more individualistic culture (Talhelm et al., 2014). Including this information reduces our sample due to missing information on parents' birth city in both cases, but does not change our results (column 4).

Second, we interact our treatment indicator with an indicator whether a participant is born in the US (column 5). We find no differential priming effect on cooperation for participants born in the US. For example, in Experiment 1 first and second-generation participants cooperate more under the US prime, though the gap to the Chinese prime is much smaller for participants born in the US than participants not born in the US. As such, it is consistent with second-generation participants being less conflicted about their two cultural identities and adopting values, social norms, and political behavior of natives over time (Fernández and Fogli, 2009; Luttmer and Singhal, 2011; Blau et al., 2013). Alternatively, we control for the number of years a participant lived in the US, showing no effect in both cases.

Finally, in Table S6 we pool the data from the US-prime and Chinese-prime treatments across both of our experiments. We then re-estimate our

treatment effects both with and without a full set of our standard controls. As before, the controls are neither individually nor jointly significant and including them has little effect on the magnitude or significance of our primary coefficients of interest. In particular, we find no indication that the behavioral differences in our second experiment are driven by differences in historical collectivism associated with wheat versus rice cultivation. In summary, our results appear robust to a wide range of potentially important confounders.

Table S4: Robustness checks: Experiment 1

	Dependent variable: PD cooperation					
Sample:	full	restricted	restricted	restricted	full	full
	(1)	(2)	(3)	(4)	(5)	(6)
US prime	0.294***	0.282***	0.297***	0.260***	0.317***	0.224**
	(0.086)	(0.104)	(0.101)	(0.080)	(0.106)	(0.096)
Neutral prime	0.042	0.033	0.108	0.120	-0.014	-0.015
	(0.125)	(0.149)	(0.169)	(0.192)	(0.131)	(0.132)
Male	0.078	0.0201	-0.009	0.086	0.085	0.090
	(0.12)	(0.129)	(0.123)	(0.141)	(0.122)	(0.129)
Age	0.013	0.002	0.031	0.054*	0.018	0.020
	(0.024)	(0.027)	(0.030)	(0.028)	(0.026)	(0.028)
Economics major	0.105	0.041	-0.012	0.131	0.131	0.115
	(0.118)	(0.133)	(0.136)	(0.145)	(0.112)	(0.120)
Born in US	0.072	0.084	0.143	0.177	0.107	0.107
	(0.114)	(0.107)	(0.112)	(0.123)	(0.155)	(0.222)
Taiwan	-0.217					
	(0.227)					
Hongkong	0.323*					
	(0.171)					
Wheat cultivation				-0.048		
				(0.058)		
US prime x Born in US					-0.165	
					(0.241)	
Years lived in US						-0.005
						(0.013)
Joint sig. controls (p-value)	0.21	0.93	0.73	0.19	0.50	0.66
Observations	102	86	81	60	102	99
Pseudo R^2	0.09	0.05	0.06	0.12	0.06	0.05

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column reports marginal effects estimates from a Probit model. Column 2 excludes participants with at least one parent born in Hong Kong and Taiwan. Columns 3 and 4 restrict the data set to participants whose parents were both born in China. Column 5 and 6 use the full data set of experiment 1. Robust standard errors clustered by session appear in parentheses.

Table S5: Robustness checks: Experiment 2

	Dependent variable: PD cooperation					
Sample:	full	restricted	restricted	restricted	full	full
	(1)	(2)	(3)	(4)	(5)	(6)
US prime	-0.177***	-0.203***	-0.190**	-0.260***	-0.143*	-0.180***
	(0.064)	(0.068)	(0.075)	(0.088)	(0.085)	(0.066)
Information	0.043*	0.060**	0.063**	0.086**	0.044*	0.046*
	(0.026)	(0.026)	(0.031)	(0.034)	(0.024)	(0.024)
Male	-0.116	-0.024	0.038	0.095	-0.120	-0.120
	(0.101)	(0.091)	(0.099)	(0.104)	(0.100)	(0.098)
Age	-0.002	-0.014	-0.015	-0.017	-0.003	-0.001
	(0.023)	(0.021)	(0.022)	(0.017)	(0.024)	(0.024)
Economics major	0.018	0.041	0.063	0.061	0.0144	0.004
	(0.093)	(0.094)	(0.101)	(0.092)	(0.099)	(0.096)
Born in US	0.076	0.032	0.067	-0.051	0.132	0.130
	(0.092)	(0.097)	(0.081)	(0.065)	(0.158)	(0.179)
Taiwan	0.0613					
	(0.101)					
Hongkong	0.065					
	(0.170)					
Wheat cultivation				-0.018		
				(0.0665)		
US prime x Born in US					-0.082	
					(0.183)	
Years lived in US						-0.003
						(0.010)
Joint sig. controls (p-value)	0.69	0.96	0.76	0.45	0.58	0.60
Observations	132	100	94	69	132	132
Pseudo R^2	0.06	0.06	0.07	0.13	0.06	0.06

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column reports marginal effects estimates from a Probit model. Column 2 excludes participants with at least one parent born in Hong Kong and Taiwan. Columns 3 and 4 restrict the data set to participants whose parents were both born in China. Column 5 and 6 use the full data set of experiment 2. Robust standard errors clustered by session appear in parentheses.

Table S6: Robustness checks: Experiment 1 & 2

	Dependent variable: PD cooperation		
	(1)	(2)	(3)
US prime	0.235***	0.248***	0.248***
	(0.090)	(0.085)	(0.087)
Experiment 2	0.161*	0.174**	0.201*
	(0.095)	(0.088)	(0.112)
US prime x Experiment 2	-0.412***	-0.432***	-0.526***
	(0.105)	(0.100)	(0.114)
Male		-0.084	0.039
		(0.074)	(0.081)
Age		0.015	0.018
		(0.019)	(0.017)
Economics major		0.026	0.045
		(0.079)	(0.094)
Born in US		0.071	0.042
		(0.074)	(0.092)
Taiwan		0.016	
		(0.117)	
Hongkong		0.210*	0.114
		(0.121)	(0.143)
Wheat cultivation			-0.012
			(0.099)
Joint sig. controls (p-value)		0.18	0.89
Observations	213	213	130
Pseudo R^2	0.03	0.05	0.07

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Each column reports marginal effects estimates from a Probit model using data from both experiments (except the Neutral-prime treatment in Experiment 1). Robust standard errors clustered by session appear in parentheses.