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Self-construal priming does not shift self-descriptions, value endorsement, or social obligation judgment: A preregistered replication and extension of Gardner, Gabriel, & Lee (1999)

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This study was preregistered (<https://osf.io/7cx3e>). Data, analysis code, and research materials used in the study are available online (<https://osf.io/ewtm6>).

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1 **Abstract**

2 Humans from different cultures define the self differently, but how cultures influence
3 self-construal—beliefs about the self—remains elusive. Do cultures mold our way of
4 perceiving, thinking, feeling, and acting, much into a habit through cultural practices and
5 daily routines (habit mechanism)? Or do cultures merely modify the accessibility of a
6 certain way of perceiving, thinking, feeling, and acting, just as one’s thoughts constantly
7 change on a daily basis based on the current motive and situation (access mechanism)? A
8 highly influential line of work in cultural priming—self-construal priming—suggests that
9 reading different story primes (reflecting either independent or interdependent thought
10 processes) or circling different types of pronouns in word-search primes (either
11 independent [e.g., I, mine] or interdependent [e.g., we, ours] pronouns) can shift self-
12 descriptions, value endorsement, and social obligation judgment (Gardner, Gabriel, &
13 Lee, 1999). In this preregistered replication and extension study, despite efforts to
14 maximize priming and to identify moderators, we found that self-construal priming,
15 either through story primes or word-search primes, did not change the relative
16 independence or interdependence of one’s self-construal in Chinese participants. Priming
17 was also not modulated by gender, experience living abroad, rice vs. wheat farming
18 legacy, or self-reported earnestness in answering the questions. Thus, the predominant
19 access afforded by cultures is much less malleable than previously assumed, consistent
20 with the habit but not access mechanism of cultural influences. To build a cumulative and
21 reproducible cultural psychology, we call for direct replications of key findings in
22 cultural priming and related literature.

23 **Keywords:** Self-construal priming; cultural priming; culture; self; replication

1 Fundamental in cultural psychology is the observation that humans from different
2 cultures define the self differently (Markus & Kitayama, 1991). People from collectivist
3 cultures such as China and Japan define the self as more interdependent, in which the
4 primary referent concerns the feelings, thoughts, and behaviors of close others or people
5 from the ingroup. In contrast, people from individualist cultures such as America (US)
6 define the self as more independent, in which the primary referent concerns one's own
7 feelings, thoughts, and behaviors. Thus, the answer to the eternal question of "Who am
8 I?" seems intimately related to other individuals in a way that is sculptured by cultures.
9
10 Interdependent and independent self-construal can be thought of as distinct schemas to
11 organize information and attune to the social environment (**Figure 1**; adapted from
12 Markus & Kitayama, 2010). Thus, interaction with others may produce a sense of self
13 either as related to others (small dotted circles that overlap; left) or as distinct from others
14 (small solid circles that do not overlap; right). Such interactions can be guided by cultural
15 expectations to either attune to others' perspectives and expectations (shared "xx"
16 symbols) or develop one's own preferences and goals (non-shared "xx" symbols). As a
17 result, ingroup–outgroup distinction becomes either marked and rigid (large solid circle)
18 or relatively inconspicuous and fluid (large dotted circle).
19

Figure 1. A conceptualization of interdependent vs. independent self-schemas. The interdependent self-schema (left) is characterized by marked boundaries between ingroup and outgroup, and by the self as fundamentally connected and related to close others. The independent self-schema (right) is marked by attenuated boundaries between ingroup and outgroup, and by the self as fundamentally distinct from others.

1

2 This distinction does not mean that the Chinese and Japanese see themselves

3 categorically as interdependent people, or the Americans as independent people. Rather,

4 the difference between people from collectivist and individualist cultures is a matter of

5 degree, not kind. For aspects of both independent and interdependent values and practices

6 are available and recognized in almost every culture. Indeed, according to self-

7 determination theory (Ryan & Deci, 2000), basic psychological needs such as autonomy,

8 competence (aligned with independent self-schema), and relatedness (aligned with

9 interdependent self-schema) are regarded as innate and universal motivations that

10 underlie human behavior. People, then, universally hold elements of interdependence and

1 independence, but they differ in their propensity to think and act in more interdependent
2 or independent manners. Put differently, different cultures cultivate different balances of
3 interdependence and independence.

4 5 **Two Distinct Mechanisms of Cultural Influences**

6 Cross-cultural comparisons have lent support to the idea that cultures can strongly affect
7 how we think, feel, and act, but how cultures exert their influences remains elusive.

8 Against the backdrop of ecological, economic, and historical happenstance, people seem
9 to adopt pervasive views regarding the nature of their social and physical environment
10 and how to organize and conduct themselves accordingly. These ideas find their
11 instantiations in various institutions (e.g., language, education, political system, legal
12 system) and daily practices (e.g., home, school, workplace). Cumulative exposure to
13 these institutions and practices shapes, quietly but surely, how we perceive, think, feel,
14 and act—in short, who we are (Markus & Kitayama, 2010). Thus, cultures can influence
15 how we construe ourselves by affecting how we habitually and routinely perceive, think,
16 feel, and act. In this way, cultural practices permeate our thinking styles and behavioral
17 routines in our daily life. We refer to this view as the *habit* mechanism of cultural
18 influences.

19
20 This habit mechanism can be contrasted with the idea that cultural influence is a matter of
21 accessibility. That is, cultures make a certain way of perceiving, thinking, feeling, and
22 acting more accessible to an individual than another way. According to this view, just as
23 one's thoughts constantly change on a daily basis—from thinking about one's own needs

1 and wants to thinking about others' needs and wants—independent or interdependent
2 self-construal within an individual is fundamentally dynamic, fluid, and malleable. What
3 determines the relative independent or interdependent of an individual is the current
4 motive or situation (Brewer & Gardner, 1996; Gardner et al., 1999; Trafimow, Triandis,
5 & Goto, 1991). We refer to this view as the *access* mechanism of cultural influences.
6
7 This access view is both counterintuitive and attractive at the same time. Counterintuitive
8 because cultural influences are typically thought to be pervasive, resulting in highly
9 automatic and often unconscious thinking styles and behavioral patterns. Indeed, cultural
10 shocks attest to the awakening to one's previously unexamined assumptions and
11 routines—but only when fundamentally different views and behaviors are vividly
12 encountered, often when living abroad. Yet, this access view is attractive because it
13 affords researchers to experimentally examine specific mechanisms of cultural influences
14 on thoughts and behaviors. That is, based on the habit view, one may need to resort to
15 comparing psychology across different cultures. Such cross-cultural comparisons can
16 suffer from confounds arising from group comparisons, such as linguistic biases (Peng,
17 Nisbett, & Wong, 1997), different reference groups (Heine, Lehman, Peng, &
18 Greenholtz, 2002), and different response styles (Schimmack, Oishi, & Diener, 2005).
19 With the access view, one can justifiably manipulate temporal accessibility, by using
20 different cues to make either independent or interdependent self-construal temporally
21 more accessible. And then look at their influences on downstream processes, such as self-
22 descriptions, value endorsement, and social obligation judgment.
23

1 Indeed, this line of thinking finds itself in the field of cultural priming (Oyserman & Lee,
2 2008). In one line of work, cultural meanings and practices are assumed to be embodied
3 through cultural icons, which can be experimentally manipulated. Exposure to collectivist
4 cues such as dragons and the Great Wall primed bicultural Hong Kong (HK) Chinese to
5 behave in a prototypically interdependent manner, whereas exposure to individualist cues
6 such as the Statue of Liberty or Liberty Bell primed them to act in a prototypically
7 independent manner (Hong, Morris, Chiu, & Benet-Martinez, 2000). Whereas priming
8 using cultural icons relies on bicultural individuals, another line of work argues that
9 cultural priming effects can be universal. To the extent that independent and
10 interdependent elements reside in every individual, we are all susceptible to priming from
11 situational cues. To differentiate this line of work from priming using cultural icons, we
12 will refer to this body of cultural priming work as self-construal priming, which is the
13 focus of the current study.

14 15 **Self-construal Priming**

16 Three seminal papers form the foundation of self-construal priming. In the first study
17 (Trafimow et al., 1991), North American and Chinese participants were asked to think for
18 two minutes of what made them different from their family and friends (*independent*
19 condition; termed private self-prime in the original paper) or what they have in common
20 with their family and friends (*interdependent* condition; termed collective self-prime).
21 They then were given five minutes to complete a self-construal task called the Twenty
22 Statements Test (TST; Kuhn & McPartland, 1954), to complete 20 sentences that begin
23 with “I am”. The authors found that, for both North Americans and Chinese, *independent*

1 priming led to more independent (private) self-descriptions (e.g., “I am intelligent”),
2 whereas *interdependent* priming led to more interdependent (collective) self-descriptions
3 (e.g., “I am a Roman Catholic”). In a separate experiment, priming was implemented
4 through reading a story about a dilemma in which a general deliberated over choosing a
5 warrior to send to the king. In the *interdependent* condition, the general considered
6 benefits to his family, and decided on a member of his own family; in the *independent*
7 condition, the general considered benefits to himself, and decided on a person who was
8 the best for the job. Similar priming results were found.

9
10 Focusing on the self-description task (TST), a second study (Brewer & Gardner, 1996)
11 employed a different form of priming. Participants read a paragraph about a trip to the
12 city and circled all the pronouns. In the *interdependent* condition, all the pronouns in the
13 essay were “we” or “us”; in the two *control* conditions, the pronouns were “they” or
14 “them” (one version) or “it” (another version). The authors found that, compared with
15 control, *interdependent* priming led to more interdependent self-descriptions (termed
16 interpersonal and collective in the original paper; e.g., “I am happily married” or “I am a
17 Black woman”).

18
19 Building on these two preliminary studies, both published in the *Journal of Personality*
20 *and Social Psychology*, a third paper provided comprehensive evidence for self-construal
21 priming (Gardner et al., 1999). First, it extended the word-search method (Brewer &
22 Gardner, 1996) to include not only interdependent prime (e.g., pronouns of “we”, “us”)
23 but also independent prime (e.g., “I”, “me”). Second, it used both word-search primes

1 and story primes (general's dilemma; Trafimow et al., 1991). Third, it measured self-
2 construal not only based on the TST, but also by using a value inventory (Schwartz,
3 1992) and a social judgment task—a vignette about a person name Lisa, who refused to
4 give her friend Amy directions to an art store because she was busy reading an exciting
5 book (Miller, Bersoff, & Harwood, 1990). The authors found that, in Study 1, under both
6 priming methods, European-Americans primed with interdependent (vs. independent)
7 self-construal displayed shifts toward more interdependent self-descriptions, which also
8 mediated shifts toward more collectivist social values and judgments. Similarly, in Study
9 2, American and HK students primed with self-construal inconsistent with their
10 predominant cultural worldview (vs. neutral primes) shifted their value endorsements in
11 the direction of the primed self-construal. But priming with culture-consistent self-
12 construal (vs. neutral primes) did not result in a shift in value endorsements, presumably
13 because of smaller shifts in self-construal.

14
15 These results of self-construal priming are both surprising and important. As discussed
16 above, they are surprising because they challenge the more traditional habit view of
17 cultural influences and provide empirical support for the access view. They are important
18 not only in this theoretical perspective, but also from a methodological perspective.
19 Indeed, in the words of Markus and Kitayama (2010), cultural priming has been “highly
20 instrumental in advancing our understanding about a proximate mechanism by which
21 culturally specific behaviors may be induced”.

1 The impact of self-construal priming is far-reaching, not only on social and cultural
2 psychology, but also on other disciplines, including cognitive psychology (e.g.,
3 Grossmann & Jowhari, 2018; Lin & Han, 2009), neuroscience (e.g., Lin, Lin, & Han,
4 2008; Sui & Han, 2007), and diverse business and applied settings such as consumer
5 behavior (e.g., Mandel, 2003), creativity (Brown & Baer, 2015), and leadership
6 (Schaubroeck, Lam, & Cha, 2007). Such influence is evident in the number and valance
7 of the citations they received, with each of the three papers amassing over 1,000 citations,
8 which are also overwhelmingly positive (**Table 1**). That is, unlike social priming in
9 general, which has been challenged in recent research (e.g., Caruso, Shapira, & Landy,
10 2017; Harris, Coburn, Rohrer, & Pashler, 2013; Payne, Brown-Iannuzzi, & Loersch,
11 2016), studies of cultural priming are generally uncontested. Indeed, cultural priming
12 features prominently in highly influential recent textbooks (e.g., Heine, 2016) and review
13 articles (e.g., Han & Northoff, 2008; Han et al., 2013). For example, self-construal
14 priming effects on neural activities have been a major source of evidence for the view
15 that “cultural background can influence the neural activity that underlies both high- and
16 low-level cognitive functions” (Han & Northoff, 2008), and for the field of cultural
17 neuroscience in general (Han et al., 2013).

18

Table 1. Total citations and citation valance of the three foundational papers in self-construal priming

Paper	Total citations	Valance of the recent 30 citations		
		Positive (%)	Negative (%)	Unclassifiable (%)
Trafimow et al., 1991	1,610	93.3	0	6.7

Brewer & Gardner, 1996	4,752	93.3	0	6.7
Gardner et al. 1999	1,354	86.7	0	13.3

Note: Total citations and recent 30 citations were based on Google Scholar as of September 30, 2021. Positive citations cite the reference to support a claim regarding the phenomenon of interest; negative citations cite the reference but cast doubt on the phenomenon of interest; unclassifiable citations cite the reference for purposes that are not related to the phenomenon itself (e.g., regarding measures used) or for unclear reasons (e.g., irrelevant to the reference or incorrect attributions). Two raters coded the 90 citations separately, with disagreements (8 out of 90) resolved through discussion. Citations and coding are available online (<https://osf.io/ewtm6>, file name “Citation analysis.xlsx”)

1

2 **How Robust is Self-construal Priming?**

3 However, before we ask, “What does cultural priming mean?” (Markus & Kitayama,
4 2010), it is perhaps prudent to first ask, “How robust is cultural priming?” In other words,
5 if an effect is not robust or even replicable to begin with, then all theorizing seems to lose
6 its basis. Although meta-analysis (Oyserman & Lee, 2008) is valuable in aggregating
7 evidence, it suffers from publication bias and selective reporting present in the primary
8 literature even when methods to correct for bias are applied (Kvarven, Stromland, &
9 Johannesson, 2020). For example, in a recent comparison between meta-analyses and
10 preregistered multiple-laboratory replications, effect sizes are on average almost three
11 times as large as in meta-analyses than in preregistered replications (Kvarven et al.,

2020). Therefore, direct replications are essential to produce reliable knowledge and to build a cumulative science.

To evaluate the robustness of self-construal priming, we focus on the three seminal papers described above. Specifically, we attempted to directly replicate and extend the work by Gardner and colleagues (1999) because, as detailed above, their work incorporated and extended the two preliminary studies (Brewer & Gardner, 1996; Trafimow et al., 1991) to provide comprehensive evidence for self-construal priming (see also **Table 2**). Moreover, with a sample size of 25 per group—Study 1, 90 (four groups); Study 2, 75 (three groups) and 82 (three groups)—it had a statistical power of 80% to detect an effect size of Cohen’s $d = 0.81$ ($\alpha = 0.05$; two-tailed). This is a large effect both by itself and in comparison with typical effects sizes in social psychology. For example, the average effect size of social psychology from meta-analyses is $d = 0.43$ (Richard, Bond, & Stokes-Zoota, 2003). And if we apply the estimated three times overestimation of meta-analytic effect sizes over preregistered replication effect sizes (Kvarven et al., 2020), then the average effect size of social psychology becomes $d = 0.14$. Put simply, the sample sizes in Gardner and colleagues (1999) were too small to detect typical social psychological effects. With low-powered studies, study effects can appear to be ostensibly large because of false positives. Meta-analytical thinking may *increase* the prevalence of false positives (Nelson, Simmons, & Simonsohn, 2018).

In addition to statistical power, several important questions were not addressed by Gardner and colleagues (1999). First, both story prime and word search prime were

employed and shown to be equally effective in European-American participants (Study 1), but in the Chinese participants, only the story prime was used (Study 2). Consequently, the relative effectiveness of the two priming methods in Chinese participants could not be determined. Second, priming was shown to be effective in changing self-descriptions, value judgment, and social judgment in European-American participants (Study 1), but because only the value judgment task was used in the Chinese participants (Study 2), it is unclear whether priming also modifies self-descriptions and social judgment in Chinese participants. Finally, it was unclear whether and how potential self-construal priming effects might be modulated by gender, experience living abroad, subcultures (specifically, rice vs. wheat cultures in China), and self-reported earnestness in participation.

The Present Study

The present study evaluated the robustness of self-construal priming by directly replicating and extending the work of Gardner and colleagues (1999), a topic that we have previously published in (Lin & Han, 2009; Lin et al., 2008). Our study provided a comprehensive replication of their Studies 1 and 2. Specifically, their Study 1 used two priming methods (story and word-search), each with two conditions (interdependent and independent), to measure changes in self-descriptions, values, and social judgments in European-Americans. Their Study 2 used only one priming method (story primes) with three conditions (interdependent, independent, and control) to measure changes in values in European-Americans and HK Chinese. Here, our study used two priming methods (story and word-search), each with three conditions (interdependent, independent, and

control), to measure changes in self-descriptions, values, and social judgments in Chinese participants.

This design allows us to address the relative effectiveness of the two priming methods in Chinese participants, and evaluate priming effects not only in value judgment but also in self-descriptions and social judgment, neither of which was possible in the original study. In addition, we made a number of notable improvements over the original study, from design to analysis. First, we adopted both the original 56-item value inventory used by Gardner and colleagues (1999) and an updated 58-item value inventory (Wang, 2014) by adding three new items. This addition allowed us to examine the sensitivity of the priming effect to different measurements of values. Second, we investigated potential moderators, including gender, experience living abroad (Xi, Mamat, Luo, & Wu, 2018), rice vs. wheat culture (Talhelm & English, 2020), and self-reported earnestness in study participation. Third, we coded self-descriptions using both coarse scales (independent vs. interdependent; as in the original study) and fine scales (private-personal orientation, social orientation, relational interdependent, and collective interdependent) to better reflect different aspects of self-construal (Brewer & Chen, 2007; Cross, Hardin, & Gercek-Swing, 2011; Kashima et al., 1995). Fourth, we used multiple analytic methods, including traditional frequentist statistics, Bayesian statistics, and the small-telescopes approach (Simonsohn, 2015). **Table 2** summarizes the comparisons of the current study with the three foundational papers in self-construal priming.

1 To minimize concerns about “null hacking”—the pursuit of null results by exploiting
2 flexibility in experimental design and data analysis by replicators (Bryan, Yeager, &
3 O'Brien, 2019)—our study was preregistered before data collection (<https://osf.io/7cx3e>)
4 using the Replication Recipe (Brandt et al., 2014). Indeed, as authors who have published
5 in self-construal priming (Lin & Han, 2009; Lin et al., 2008) and would like to build on
6 this work to examine the developmental trajectories of cultural priming, we have genuine
7 incentives for the effect to replicate (in a rigorous manner). Sample size and stopping rule
8 were determined beforehand ($n = 468$; 78 per group). We also made efforts to minimize
9 bias. To minimize participant bias, priming and test were set up as two separate
10 experiments in two different rooms. To minimize experimenter bias, task instructions
11 were provided in writing, and verbal instructions were written out beforehand for the
12 experimenters to follow; data coding and exclusion were conducted without knowing the
13 priming condition of the participants.
14

Table 2. Comparisons of the current study with the three foundational studies in self-construal priming

Design and analysis	Trafimow et al. (1991)	Brewer & Gardner (1996)	Gardner et al. (1999)	Current study
Priming				
<i>Method</i>	Self vs. family/friend comparison; story	Word-search	Story; word-search	Story; word-search
<i>Interdependent</i>	Yes	Yes	Yes	Yes

<i>Independent</i>	Yes	No	Yes	Yes
<i>Control</i>	No	Active control	Negative control for word-search only	Active control for both
Outcomes				
<i>Self-descriptions</i>	Yes	Yes	Yes	Yes
<i>Value endorsement</i>	No	No	Yes (56-item version)	Yes (56-item and 58-item versions)
<i>Social judgement</i>	No	No	Yes	Yes
Moderators				
<i>Gender</i>	No	No	No	Yes
<i>Cross-cultural experience</i>	No	No	No	Yes
<i>Rice vs. wheat culture</i>	No	No	No	Yes
<i>Earnestness in participation</i>	No	No	No	Yes
Others				
<i>Participant background</i>	American participants with a mix of Chinese and North American background	American participants (cultural background not mentioned)	A mix of European Americans and Hong Kong Chinese	Chinese participants (some with experience living aboard)

<i>Blinding</i>	No	Participant	No	Participant and
		blinding		analysis blinding
<i>Analysis</i>	Frequentist	Frequentist	Frequentist	Frequentist;
				Bayesian; small-
				telescopes

Note: Minor differences not noted in the table are explained in the text, including task requirements and constraints, as well as coding of self-descriptions in TST.

1

2 **Method**

3 **Transparency and openness.** This study was preregistered (<https://osf.io/7cx3e>), with
4 detailed explanations regarding key elements such as the rationale, effect size, sample
5 size, data exclusion criteria, and analysis plan. We report how sample sizes were
6 determined, all data exclusions, all manipulations, and all measures in the study.
7 Data were analyzed using R, MATLAB, and SPSS. Data, analysis code, and research
8 materials used in the study are available online (<https://osf.io/ewtm6>).

9

10 **Pilot study.** A pilot run was conducted at the Chinese University of Hong Kong,
11 Shenzhen (CUHKSZ; $n = 19$) and Shenzhen Institute of Information Technology (SIIT; n
12 $= 12$) to determine the time needed for each task and to make necessary adjustments to
13 ensure clear instructions and proper blinding. It also helped to familiarize the
14 experimenters with the procedure.

15

16 **Participants.** Participants were recruited from CUHKSZ and SIIT, both located in
17 Shenzhen, a city neighboring HK. They were compensated with either course credits or

1 cash and equivalents. The study was approved by the local Institutional Review Board.

2 All data (including pilot data) were collected between November 2020 and June 2021.

3
4 The sample size was predetermined based on the small-telescopes approach (Simonsohn,
5 2015). That is, if the true effect is zero, a replication needs 2.5 times as many
6 observations as the original study to have about 80% power to reject the effect size that
7 would give the original study 33% power. The original study (Gardner et al., 1999)
8 included 90 students in 4 groups (Study 1), 75 in 3 groups, and 82 in 3 groups (Study 2),
9 but a detailed breakdown was not given. The highest average would be 27.3 (82/3) per
10 group. With a liberal estimation of 30 per group in the original study and the need to
11 counterbalance the order of three tests in the current study (i.e., multiple of 6), we
12 decided on a sample size of 78 per group. Since there were six groups in total (2 priming
13 methods $\times$ 3 priming types; see below), the required total sample size was 468.

14
15 To take into account potential unusable data (such as loss of data and exclusion of invalid
16 data), we aimed to collect at least 10% more than 468 participants and to keep collecting
17 data until each group had valid data from 78 participants for final analysis. This resulted
18 in the over-collection of data from 15 participants that were not used. Also excluded from
19 coding analysis were 32 participants from the pilot study (see “pilot study” above), 17
20 participants due to operating errors (assigning wrong IDs to participants, 9; value
21 questionnaire website failure, 5; accidentally exiting questionnaire, 3), one participant for
22 participating twice (discarding the second set of data), and one participant who provided
23 the same responses to the value inventory. Final data included for coding analysis were

from 517 students, 321 at CUHKSZ ($M_{age} = 19.45$, $SD = 1.88$; 75.1% female, 24.9% male) and 196 at SIIT ($M_{age} = 19.59$, $SD = 1.17$; 69.9% female, 30.1% male). During coding, 49 participants were further excluded for providing invalid answers to TST (see “quality check and exclusion” below), making the final sample 468 ($M_{age} = 19.48$, $SD = 1.67$, 74.6% female, 25.4% male), 78 for each of the six conditions as planned. Based on sensitivity power analysis, this sample size has 80% power to detect an effect size of Cohen’s $d = 0.45$ ($\alpha = 0.05$).

Procedure. To minimize experimenter bias, task instructions were provided in writing. Study purpose, procedure, and debriefing were explained verbally. To make them as consistent as possible among participants, verbal instructions were written out beforehand for the experimenters to follow. Experimenters were also available for any questions before leaving the participant alone to finish each task.

After informed consent, demographic information was collected, including gender, age, ethnicity, the province where the participant grew up, and experience of living aboard (duration and country). The study was then divided into two parts, priming and test. To minimize response bias, participants were told that there were two separate experiments: the first one was to test their reading ability and whether they could finish the following tests carefully; the second one was to measure their values and self-understanding. To enhance the credibility of the cover story, the two parts took part in two different rooms. At the end of the study, they were asked two questions. First, how serious they were in answering the questions to the two experiments (“seriously”, “relatively seriously”, or

1 “glanced over”). Second, whether they saw a connection between the two experiments
2 (and if yes, what the connection was; Brewer & Gardner, 1996). When they indicated a
3 connection, experimenters also verbally asked when they suspected this relationship
4 (during vs. after the study), because the very questioning itself might cause the
5 participant to suspect a relationship. On average, the total study took about 25 minutes.

6

7 Priming was implemented as a paper-and-pencil reading task. There were six between-
8 subjects conditions: two priming methods (word-search task and story task) crossed with
9 three priming types (interdependent, independent, and control). To make it consistent
10 across conditions, based on the pilot study, the soft time limit for each priming task was
11 set to be eight minutes, after which the experimenter would check on the participant. In
12 the word-search task (Brewer & Gardner, 1996), participants read a short essay about a
13 trip to the countryside or about a tourist attraction (adopted from Lin & Han, 2009; Sui &
14 Han, 2007). In the *interdependent* condition, all the pronouns in the essay were “wǒ mén”
15 (“we” or “us”); in the *independent* condition, the pronouns in the essay were changed to
16 “wǒ” (“I” or “me”); in the *control* condition, the essay was about Huizhou West Lake
17 with no pronouns. To maximize priming effect, participants were instructed to read the
18 essay twice and carefully. The task was to circle all the pronouns in the essay or, for the
19 control condition, the word “hú” (“lake”). The target word appeared 17 times for each
20 condition.

21

22 In the story prime task (Trafimow et al., 1991), participants read a story about a dilemma
23 in which a general deliberated over choosing a warrior to send to the king. In the

1 *interdependent* condition, the general considered benefits to his family (“jiā zú”), and
2 decided on a member of his own family; in the *independent* condition, the general
3 considered benefits to himself, and decided on a person who was the best for the job. The
4 *control* condition, not included in the original study (Trafimow et al., 1991) but adopted
5 from elsewhere (Rice, Clayton, Trafimow, Keller, & Hughes, 2009), told a story of the
6 chosen warrior being a talented general. After reading, participants were asked to answer
7 whether they admired the general or not. To maximize potential priming effect, as in Rice
8 et al. (2009), they were further asked to come up with five other ways that the general
9 could do to benefit his family (*interdependent* condition), or himself (*independent*
10 condition), or to describe five daily routines in the participant’s life (*control* condition).

11

12 After the priming task, participants took part in three different tests, with the order
13 counterbalanced between participants. These included a values inventory called the
14 Schwartz Value Survey (SVS; Schwartz, 1992; Triandis, Mccusker, & Hui, 1990), a
15 social judgment task (Miller et al., 1990), and a self-construal task called the Twenty
16 Statements Test (TST; Kuhn & McPartland, 1954), as described below.

17

18 The SVS used the 56-item inventory as originally used in Gardner et al. (1999), plus
19 three items new to the current recommended Chinese version of the 58-item SVS (which
20 dropped the item "detachment [from worldly concerns]"; Wang, 2014), totaling 59 items.
21 The three new items were: “privacy (the right to have a private sphere)”; “self-indulgent
22 (doing pleasant things)”; and “observing social norms (to maintain face)”. For each value,

1 participants were asked to evaluate its personal importance as a guiding principle in life,
2 from -1 (“opposite to my value”) and 0 (“not important”) to 7 (“of supreme importance”).
3

4 The social judgment task presented a story about a person named Lì Lì who refused her
5 friend’s request for directions to an art store, as she was too busy reading a thrilling book.

6 There were three questions at the end: (1) “*desirability*”: how desirable Lì Lì’s actions
7 were, using a 7-point scale, from 1 (“very undesirable”) to 7 (“very desirable”); (2)
8 “*obligation*”: whether Lì was obligated to help her friend or that her action was simply a
9 personal choice; and (3) “*punishment*”: whether Lì Lì’s action should be punished or not.
10

11 The TST asked participants to come up with 20 self-descriptions starting with “I ...”
12 under 12 minutes. Because of grammar differences between English and Chinese, the
13 starting statement was “I ...” instead of “I am ...” (Ross, Xun, & Wilson, 2002).
14

15 **Quality check and exclusion.** All the participants who finished the study reported
16 answering the questions in the study “relatively seriously” ($n = 69$) or “seriously” ($n =$
17 399). No participant reported having only “glanced over” when doing the tasks. Also,
18 most participants (92.7%) correctly identified the exact number of target words in the
19 word-search prime task (17; incorrect responses were: 10 answers with 16; two answers
20 each with 15, 19, 10; and one answer with 24).
21

22 According to the exclusion criteria in the preregistration, participants were excluded if
23 they failed to finish the studies ($n = 17$, as noted above) or provided the same responses

to all the items in SVS ($n = 1$ from SIIT). For TST, transient answers (e.g., “I am hungry now”, “I am going to eat soon”; about 1.8%) were excluded as in the original study. During coding, some other inappropriate answers in TST were noticed that should be excluded, including answering with questions instead of statements (e.g., “I like to exercise or not”, “I am happy or not”) or with other inappropriate answers (e.g., “I fly”). If more than half of the answers in TST were inappropriate, participants were excluded ($n = 49$; 14 from CUHKSZ, 35 from SIIT); otherwise, only the invalid answers (1.8%) were excluded, and the valid answers from these participants (98.2%) were analyzed. Importantly, to avoid any possible experimenter bias, coding and exclusion were conducted without knowing the priming condition of each participant.

Participant blinding. Of the 468 participants, 329 of them indicated a connection between the two studies when explicitly asked—168 of them reported having the thought of the connection during the studies, 161 after completing the studies. Indeed, as one participant commented: “*I don’t think two things that have no connections will appear in scientific experiments*”. But most of the suspected connections reported were superficial. Typical responses include that 1) both studies were about the understanding of the self; 2) both studies were about values (or choices, understanding of the world, responsibility, peace, human nature, a person’s connection with the society, etc.); 3) the first study made the participant relaxed and better prepared for the second study; and 4) the first study tested reading ability or the ability to focus, so that the participant could participate in the second study.

1 A few participants hinted at the connection of the two sessions through self-construal,
2 commenting that 1) circling “I”/“me” in the first part helped the participant focus on the
3 self or the participant’s own true thoughts (three participants), or that 2) emphasizing
4 “we”/“us” in the first part made the participant more sensitive to the environment (one
5 participant). Interestingly, one participant commented that “*the short story in the first*
6 *study involved things such as calculation, planning, and strategizing in human relation,*
7 *which turned me off, and it might indirectly affect my answers to who I am, in which I*
8 *emphasized that I value freedom*”. Thus, although 70.3% of participants reported a
9 connection when explicitly asked, the reported connections were generally superficial.

10
11 **Data analysis.** Information regarding ethnicity and province was used to analyze rice vs.
12 wheat farming legacy. In accordance with previous studies (e.g., Talhelm et al., 2014),
13 non-Han Chinese (i.e., ethnic minorities) and participants from major herding provinces
14 (Xinjiang, Inner Mongolia, and Tibet) as well as from Qinghai Province (for which data
15 were not available) were excluded ($n = 22$) when analyzing rice vs. wheat cultural
16 modulation. Data regarding the included provinces—area of rice paddies, area of total
17 sown farmland, GDP per capita, and disease prevalence—were retrieved from the earliest
18 available China Statistical Yearbook on the National Bureau of Statistics of the PRC web
19 site (<http://www.stats.gov.cn/tjsj/ndsj/>; 1996 yearbook for rice paddies and farmland,
20 1999 yearbook for GDP) and China Public Health Statistical Yearbook on the National
21 Health Commission of the PRC web site
22 (http://www.nhc.gov.cn/zwgk/tjnj1/ejlist_3.shtml, disease prevalence from the 2001
23 yearbook). The prevalence of rice farming for each province was measured as the

1 percentage of cultivated land (sown farmland) devoted to rice paddies. Hierarchical linear
2 regression was used to test the increase in the variance accounted for when rice/wheat
3 farming was added as a predictor (*R-square* change) after gender, GDP per capita, and
4 disease prevalence were entered as predictors first.

5

6 Responses to SVS were coded in the endorsement of *individualist* vs. *collectivist* goals
7 and values. Based on the original 56-item inventory, five items (i.e., freedom,
8 independence, choosing one's own goals, living an exciting life, and living a varied life)
9 were strongly endorsed in individualist cultures such as the US, five other items (i.e.,
10 belongingness, friendship, family safety, national security, and respect for elders) in
11 collectivist cultures such as China. Rating scores were tallied and averaged for the five
12 individualist values, which were then averaged again to arrive at the average *individualist*
13 value score. The same procedure was carried out for collectivist values. If self-construal
14 priming affected values, then a shift in the endorsement of individualist vs. collectivist
15 value scores should be observed in different priming conditions.

16

17 Importantly, with the inclusion of the updated 58-item inventory, based on the user
18 manual, these items can be organized into ten sets of values: achievement, hedonism,
19 power, self-direction, stimulation, benevolence, conformity, tradition, security, and
20 universalism. Of the ten values, achievement, hedonism, power, self-direction, and
21 stimulation represent *individualist* values, whereas benevolence, conformity, and
22 tradition represent *collectivist* values. Unlike the 56-item inventory, to control for
23 response bias, each participant's ratings to the 58 items were normalized by subtracting

1 the mean response score of that participant. The rest of the analysis was the same as
2 before. The effects of self-construal priming were examined based on the endorsement of
3 these individualist and collectivist value scores.

4
5 Responses to the social judgment task were coded in the perceived *obligation* to help, as
6 well as the perceived *desirability* and endorsement of *punishment* for violating this
7 obligation. Members of collectivist cultures were more likely to perceive an obligation to
8 help than members of individualist cultures. If self-construal priming affected the
9 salience of interdependent vs. independent self-construal, then a shift in the sensitivity to
10 social obligations should be observed in different priming conditions.

11
12 Responses to the TST were rated as either *independent* (describing personal attributes) or
13 *interdependent* (describing a role in an important relationship or membership in a social
14 group) as in Gardner et al. (1999). Transient or other improper answers were excluded, as
15 explained previously. Independent responses were further divided into *private-personal*
16 orientation (“I like to eat Jiangsu and Zhejiang cuisine”) and *social* orientation (“I like to
17 help others”), as in some previous studies (e.g., Tanti, Stukas, Halloran, & Foddy, 2008).
18 Interdependent responses were divided into *relational* interdependent responses (i.e.,
19 describing a role in a specific, close, or important relationship; “I am my parents’ child”)
20 and *collective* interdependent responses (describing membership in a social group; “I am
21 a student of CUHKSZ”), as in some previous studies (e.g., Brewer & Gardner, 1996; Liu,
22 Liang, Feng, & Zhou, 2019).

1 Because of the subjective nature of the rating, two raters conducted the rating
2 independently (except for three participants, for whom only one rater did the rating
3 because the other rater knew their experimental condition). Inter-rater reliability was
4 calculated based on rater agreement after taking chance agreement and prevalence (i.e.,
5 much higher proportion of one category of ratings over another) into consideration,
6 which is known as the prevalence-adjusted, bias-adjusted kappa (PABAK; Byrt, Bishop,
7 & Carlin, 1993). Any disagreements between the two raters were resolved through
8 discussion. Importantly, to avoid experimenter bias, coding was conducted without
9 knowing the associated priming condition. If self-construal priming affected the salience
10 of interdependent vs. independent self-construal, then a shift in the proportion of
11 independent vs. interdependent self-descriptions should be observed in different priming
12 conditions.

13
14 **Bayesian analytics.** To quantify the strength of the evidence supporting the null
15 hypothesis (e.g., there was no main effect or interaction) over the alternative hypothesis
16 (e.g., there was a main effect or an interaction), Bayesian statistics—Bayes Factor BF_{01}
17 and BF_{excl} —were calculated with the default priors using JASP (version 0.13.1, JASP
18 Team, 2021). The default prior was a multivariate Cauchy prior (fixed effects scale
19 factor, $r = 0.5$; random effects scale factor, $r = 1$) for ANOVAs, and a Cauchy prior
20 centered on zero (width, $r = 0.707$) for t-tests. The Bayes Factor for an ANOVA
21 interaction effect was calculated as the change from prior to posterior inclusion odds,
22 denoted as BF_{incl} . To determine the evidence base for the lack of an interaction effect, the
23 reverse was used, termed BF_{excl} .

1

2 A higher BF_{01} (or BF_{excl}) value indicates more favorable evidence for the null hypothesis.3 In general, $BF_{01} = 1$ is ambiguous evidence; BF_{01} in between 1 and 3, anecdotal evidence4 for the null hypothesis; $BF_{01} \geq 3$, moderate (substantial) evidence; $BF_{01} \geq 10$, strong5 evidence; $BF_{01} \geq 30$, very strong evidence; and $BF_{01} \geq 100$, extreme evidence

6 (Schonbrodt & Wagenmakers, 2018).

7

8 **Results**9 **Self-construal priming effects on self-descriptions, values, and social judgment.**10 **Table 3** shows the descriptive results of how self-construal priming—independent,

11 interdependent, and control primes—affected self-descriptions, values, and social

12 judgment. A quick glance reveals that not only were the differences between

13 independence and interdependence primes numerically small, but several contrasts went

14 into opposite directions to theoretical predictions.

15

Table 3. Summary of how self-construal priming affected self-descriptions, values, and social judgment

Priming method	Self-descriptions (TST)		Values (SVS)		Social judgment		
	Independent	Interdependent	Individualist	Collectivist	Desirability	Obligation	Punishment
Word-search							
<i>Independent</i>	83.6%	14.4%	4.72	5.33	2.82	71.8%	92.3%
<i>Interdependent</i>	83.4%	14.6%	4.77	5.46	2.76	65.4%	96.2%
<i>Control</i>	83.1%	15.4%	4.51	5.38	2.76	65.4%	92.3%
Story							
<i>Independent</i>	79.3%	18.6%	4.64	5.62	2.65	66.7%	92.3%
<i>Interdependent</i>	79.7%	18.0%	4.73	5.75	2.85	62.8%	96.2%

<i>Control</i>	82.0%	16.8%	4.52	5.39	2.64	73.1%	97.4%
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Note. Value ratings were based on a scale of -1 and 0 to 7, averaged across five items in each category (56-item inventory); desirability in social judgment was based on a scale of 1 to 7; others in percentages.

1

2 To analyze these results with formal statistical models, we first examine how different
3 types of self-construal priming affected self-descriptions. The percentage of agreement
4 between raters in coding independent and interdependent descriptions was 92.3% (88.8%
5 for finer categorization). Interrater reliability was excellent, KABAK = 0.85 (0.78 for
6 finer categorization). Consistent with previous observations, most of the self-descriptions
7 were related to personal attributes (81.8% independent self-descriptions compared with
8 16.3% interdependent ones).

9

10 Importantly, as **Table 3** shows, prime types did not affect the proportion of *independent*
11 self-description ($F(2, 462) = 0.283, p = .754, \eta_p^2 = 0.001; BF_{01} = 32.17$), and it was not
12 modulated by priming methods (word-search vs. story priming; $F(2, 462) = 0.574, p$
13 $= .588, \eta_p^2 = 0.002; BF_{excl} = 213.09$). The same pattern emerged for *interdependent* self-
14 description (main effect of priming types: $F(2, 462) = 0.033, p = .968, \eta_p^2 < 0.001; BF_{01} =$
15 40.62 ; interaction with priming methods: $F(2, 462) = 0.471, p = .625, \eta_p^2 = 0.002; BF_{excl}$
16 $= 240.21$). Testing only independent and interdependent primes without the control prime
17 revealed the same results. Prime types again did not affect the proportion of *independent*
18 self-description ($F(1, 308) = 0.003, p = .958, \eta_p^2 < 0.001; BF_{01} = 8.01$), which was also
19 not modulated by priming methods ($F(1, 308) = 0.024, p = .875, \eta_p^2 < 0.001; BF_{excl} =$

20.44). The same pattern emerged for *interdependent* self-description (main effect of priming types: $F(1, 308) = 0.021, p = .885, \eta_p^2 < 0.001; BF_{01} = 7.94$; interaction with priming methods: $F(1, 308) = 0.079, p = .778, \eta_p^2 < 0.001; BF_{excl} = 18.60$). Taken together, neither independent nor dependent priming types change the nature of self-descriptions.

As an exploratory analysis, we further examine private-personal orientation, social orientation, relational-interdependence, and collective-interdependence separately. The results remain the same (all $ps \geq .128$). An unexpected significant interaction was found between priming methods and prime types in relational-interdependence self-descriptions ($F(1, 308) = 4.272, p = .040, \eta_p^2 = 0.014$). However, $BF_{excl} = 5.95$, which provides moderate evidence for a lack of an interaction effect. Indeed, simple main effects showed non-significant effects: interdependent (vs. independent) prime slightly increased relational-interdependence self-descriptions when using story primes (9.3% vs. 7.3%, $p = .150$), and slightly decreased the same self-descriptions when using word-search primes (8.9% vs. 10.9%, $p = .140$).

We then look at how self-construal priming modulated the endorsement of individualist and collectivist values. Consistent with the overall collectivist culture of China, collectivist goals and values were endorsed more than individualist ones (5.49 vs. 4.65, $t(467) = 13.67, p < .001, d = 0.63$). Importantly, as **Table 3** shows, prime types did not affect the relative endorsement of individualist and collectivist values, as reflected by the lack of their interaction ($F(2, 462) = 0.130, p = .879, \eta_p^2 = 0.001; BF_{excl} = 84.89$), which

was not modulated by priming methods ($F(2, 462) = 0.928, p = .396, \eta_p^2 = 0.004; BF_{excl} = 30995.02$). Testing only independent and interdependent primes without the control prime did not change the results. Prime types again did not affect the endorsement of individualist and collectivist values ($F(1, 308) = 0.140, p = .708, \eta_p^2 < 0.001; BF_{excl} = 19.77$), which was also not modulated by priming methods ($F(1, 308) = 0.014, p = .905, \eta_p^2 < 0.001; BF_{excl} = 428.52$). The same pattern was observed when individualist and collectivist values were derived from the updated 58-item inventory instead of the original 56-item inventory (all $ps \geq .126$).

As an exploratory analysis—not conducted in the original study (Gardner et al., 1999)—we further compare priming effects on individualist and collectivist values separately. We found similarly that prime types did not affect the endorsement of *individualist* values (main effect: $F(2, 462) = 1.914, p = .149, \eta_p^2 = 0.008; BF_{01} = 6.90$; interaction with priming methods: $F(2, 462) = 0.074, p = .929, \eta_p^2 < 0.001; BF_{excl} = 426.57$) or *collectivist* values (main effect of priming types: $F(2, 462) = 1.554, p = .212, \eta_p^2 = 0.007; BF_{01} = 9.83$; interaction with priming methods: $F(2, 462) = 0.857, p = .425, \eta_p^2 = 0.004; BF_{excl} = 79.49$). Testing only independent and interdependent primes without the control prime did not change the results. Prime types again did not affect the endorsement of *individualist* values ($F(1, 308) = 0.362, p = .548, \eta_p^2 = 0.001; BF_{01} = 6.74$), which was also not modulated by priming methods ($F(1, 308) = 0.040, p = .841, \eta_p^2 < 0.001; BF_{excl} = 97.89$). The same pattern emerged for *collectivist* values (main effect of priming types: $F(1, 308) = 1.156, p = .283, \eta_p^2 = 0.004; BF_{01} = 4.65$; interaction with priming methods: $F(1, 308) = 0.003, p = .958, \eta_p^2 < 0.001; BF_{excl} = 12.85$). The same pattern was observed

when individualist and collectivist values were derived from the updated 58-item inventory instead of the original 56-item inventory (all $ps \geq .081$). Thus, regardless of priming methods, neither independent nor dependent priming types change the endorsement of individualist and collectivist goals and values.

Next we evaluate how self-construal priming modulated the perceived obligation to help in the social judgment task. Prime types did not affect the perceived *obligation* to help (main effect of priming types: $F(2, 462) = 0.619, p = .539, \eta_p^2 = 0.003; BF_{01} = 23.38$; interaction with priming methods: $F(2, 462) = 0.813, p = .444, \eta_p^2 = 0.004; BF_{excl} = 690.62$), the perceived *desirability* of the act that violated this obligation (main effect of priming types: $F(2, 462) = 0.268, p = .765, \eta_p^2 = 0.001; BF_{01} = 32.53$; interaction with priming methods: $F(2, 462) = 0.459, p = .632, \eta_p^2 = 0.002; BF_{excl} = 1102.54$), or the endorsement of *punishment* for violating this obligation (main effect of priming types: $F(2, 462) = 1.136, p = .322, \eta_p^2 = 0.005; BF_{01} = 14.41$; interaction with priming methods: $F(2, 462) = 0.649, p = .523, \eta_p^2 = 0.003; BF_{excl} = 378.76$).

Comparing independent and interdependent primes directly, we found similarly that priming did not affect perceived *obligation* (main effect of priming types: $F(1, 308) = 0.916, p = .339, \eta_p^2 = 0.003; BF_{01} = 5.17$; interaction with priming methods: $F(1, 308) = 0.057, p = .811, \eta_p^2 < 0.001; BF_{excl} = 67.38$), *desirability* (main effect of priming types: $F(1, 308) = 0.196, p = .658, \eta_p^2 = 0.001; BF_{01} = 7.30$; interaction with priming methods: $F(1, 308) = 0.784, p = .377, \eta_p^2 = 0.003; BF_{excl} = 74.87$), or *punishment* (main effect of priming types: $F(1, 308) = 2.110, p = .147, \eta_p^2 = 0.007; BF_{01} = 2.91$; interaction with

priming methods: $F(1, 308) < 0.001$, $p = 1.000$, $\eta_p^2 < 0.001$; $BF_{excl} = 50.13$). These results therefore indicate that, regardless of priming methods, neither independent nor dependent priming types change the perceived obligation to help a friend in need.

Additional analyses revealed that the lack of a priming effect was not modulated by participants' gender (male vs. female; interactions with prime types, all $ps \geq .055$), experience living aboard (yes vs. no; all $ps \geq .104$), province growing up (rice vs. wheat farming region; all $ps \geq .067$), or self-reported earnestness in answering the questions (seriously vs. relatively seriously; all $ps \geq .060$).

Two unexpected interaction effects were observed, both involving priming methods. One significant interaction concerned priming methods, prime types, and experience living aboard in judging the desirability of the act to not help ($F(2, 456) = 5.098$, $p = .006$, $\eta_p^2 = 0.022$). But Bayesian evidence indicates a lack of interaction ($BF_{excl} = 7246.38$). Indeed, simple main effects show non-significant effects: interdependent (vs. independent) prime numerically increased perceived desirability when using story primes (3.8 vs. 2.1, $p = .069$), and numerically decreased perceived desirability when using word-search primes (2.4 vs. 3.4, $p = .397$), a pattern that was restricted to those with experience living aboard (total $n = 37$) and absent in those with no experience living aboard (total $n = 431$).

The other interaction concerned priming methods, prime types, and self-reported earnestness in the endorsement of collectivist values from the 56-item inventory ($F(2, 456) = 4.697$, $p = .010$, $\eta_p^2 = 0.020$). Again, Bayesian evidence indicates a lack of

1 interaction ($BF_{excl} = 200$). The only significant simple main effects were that
2 interdependent (vs. control) prime increased the endorsement of collectivist values when
3 using story primes for those answering the questions “relatively seriously” (5.88 vs. 4.51,
4 $p = .032$, total $n = 69$), and when using word-search primes for those answering the
5 questions “seriously” (5.95 vs. 5.38, $p = .011$, total $n = 399$). Considering the exploratory
6 nature of these analyses, the issue of multiple comparisons (two interactions out of many
7 tests), and the small effects, we deemed these interaction effects uninformative. In any
8 case, dependence on priming methods, even if genuine, is also inconsistent with
9 predictions of self-construal priming.

10
11 Together, these results show that self-construal priming, either through story primes or
12 word-search primes, did not change the relative independence or interdependence of
13 one’s self-construal, with moderate to strong and very strong evidence from Bayesian
14 statistics. The lack of a priming effect was in spite of multiple testing, including on
15 independent and interdependent self-descriptions (coarse and fine categorizations),
16 endorsement of individualist and collectivist values (original and updated inventories), as
17 well as perceived obligations to help a friend and the desirability and punishment for
18 violating such obligations. Because these results are so consistent in showing a lack of
19 self-construal priming effects, with strong support from Bayesian evidence, we decide
20 not to pursue the plan in the preregistration of conducting an online version of the study
21 (which was to use only independent and interdependent story primes and value inventory
22 as the only outcome measure).

1 These results can also be interpreted in relation to the original reports, by using the small-
2 telescopes approach. Specifically, what statistical powers would the original study have
3 to detect the effect sizes observed here? To match the original and current studies, the
4 effect sizes were based on the main effect of the two priming types (independent vs.
5 interdependent). With the sample size of 90 (Experiment 1) in the original study, it would
6 yield 5.1% power to detect the TST effect size (F test), 5.0% for the SVS effect size (F
7 test), and in the social judgment task, 5.6% for desirability (F test), 8.1% for obligation
8 (Chi-squared test), and 12.2% for punishment (chi-squared test). These powers are all
9 well below the cutoff of 33% power to assess detectability (Simonsohn, 2015). That is,
10 the effect sizes of self-construal priming are so close to zero that the original study would
11 have been unable to meaningfully study such small effects.

12 13 **Additional exploratory analyses: rice vs. wheat farming and self-construal**

14 The current study focuses on self-construal priming. The collection of demographic
15 information allows us to reveal that self-construal priming was not modulated by
16 historical rice versus wheat agriculture. Previously, it has been argued that a history of
17 farming rice (vs. wheat) makes people and cultures more interdependent (Talhelm et al.,
18 2014) and social norms tighter (Talhelm & English, 2020). At the same time, issues such
19 as sample bias, measurement error, and model misspecification have been raised (Ruan,
20 Xie, & Zhang, 2015).

21
22 Using the current dataset, we further explore how rice and wheat farming might affect
23 self-construal. Specifically, how were the proportions of rice paddies in each province

1 related to the self-construal of participants in these provinces? Self-construal was
 2 measured by the proportion of interdependent/independent self-descriptions in TST, the
 3 endorsement of collectivist/individualist values in SVS, and the desirability, obligation,
 4 and punishment judgment in the social judgment task. Hierarchical linear regression
 5 revealed that, after controlling for gender, disease prevalence, and GDP, the extent of rice
 6 farming was positively related to the endorsement of collectivist values in SVS
 7 (standardized $b = 0.110$, $p = .031$ for the 56-item inventory; standardized $b = 0.153$, p
 8 $= .002$ for the 58-item inventory) and negatively related to the endorsement of
 9 individualist values (only for the 58-item inventory, standardized $b = -0.133$, $p = .009$).
 10 However, rice farming did not affect independent or interdependent self-descriptions in
 11 TST, endorsement of individualist values for the 56-item inventory, or any of the three
 12 desirability, obligation, and punishment measures in the social judgment task (all ps
 13 $\geq .088$).
 14
 15 Because participants at SIIT were primarily from Guangdong (a rice region in the south),
 16 and because testing the rice theory requires sampling from a large number of rice and
 17 wheat provinces, our results could be biased because of this oversampling in one
 18 province. To test this possibility, we excluded participants from Guangdong ($n = 232$)
 19 and ran the analysis with the rest of the eligible participants ($n = 221$). We found that rice
 20 farming no longer affected value endorsement, consistent with the lack of modulation in
 21 self-descriptions and social obligation judgments (all $ps \geq .114$). These patterns—
 22 significant modulation of rice/wheat farming legacy on value endorsements only when

1 data from Guangdong were included—remain when the GDP data were from 1996 or
2 from 2020.

4 **Discussion**

5 In a trio of papers that paved the foundation of cultural priming, it was argued that
6 independent versus interdependent self-construal within an individual is fluid and
7 malleable enough to be affected by situational cues, especially those inconsistent with the
8 predominant self-construal. In particular, in a seminar paper, Gardner and colleagues
9 (1999) incorporated and extended two preliminary studies (Brewer & Gardner, 1996;
10 Trafimow et al., 1991) to provide comprehensive evidence for self-construal priming.
11 They showed that both story primes and word-search primes were effective in shifting
12 European-Americans toward displaying more interdependent self-descriptions and
13 endorsing more collectivist social values and judgments, when primed with
14 interdependent (vs. independent) self-construal. Similarly, story primes were effective in
15 nudging European-Americans and HK Chinese to endorse values not predominant in
16 their cultural worldview, when primed with self-construal inconsistent with their
17 predominant cultural worldview (vs. neutral primes).

18
19 Both because of its profound influence in and out of psychology—indeed, personally it
20 attracted author ZL to conduct his first two psychology studies using self-construal
21 priming before graduate school (Lin & Han, 2009; Lin et al., 2008)—and because of its
22 theoretical and methodological innovations, here we directly replicated and extended the
23 classic study by Gardner and colleagues (1999). In this preregistered study that was

1 substantially better powered and that minimized participant and experimenter bias, we
2 showed that self-construal priming, either through story primes or word-search primes,
3 did not change the relative independence or interdependence of one's self-construal in
4 Chinese participants. The lack of any priming effect was in spite of multiple testing, as
5 measured by both coarse and fine self-descriptions, endorsement of values in both
6 original and updated inventories, as well as perceived obligations to help a friend and the
7 desirability and punishment for violating such obligations. Priming was also not
8 modulated by participants' gender, experience living abroad, province growing up (rice
9 vs. wheat farming region), or self-reported earnestness in answering the questions.
10 Indeed, Bayesian statistics showed moderate to strong and very strong evidence for the
11 null effect.

12
13 Categorically, these results fail to replicate Gardner and colleagues (1999), based on three
14 predefined criteria of successful replication in our preregistration: a priming effect that is
15 detectably different from zero in the same direction as in the original report ("not
16 significantly different from zero"); the detectability of the priming effects based on the
17 small-telescopes approach ("not detectable"); Bayesian evidence in favor of the effect
18 sizes reported in the original studies as compared with null effects ("Bayesian evidence in
19 favor of the null effects").

20
21 Thus, despite our efforts to maximize potential priming effects (e.g., for word-search,
22 participants were instructed to read the essay twice and carefully; for story primes, they
23 were asked to come up with five other ways that the general could do to benefit his

1 family or himself, as in Rice et al., 2009), we found no priming effects. The discrepancies
2 between the original and replication results are unlikely to be due to a lack of knowledge
3 or expertise—we have published previously using the same priming procedure (Lin &
4 Han, 2009; Lin et al., 2008).

6 **Theoretical Implications**

7 Unlike priming using cultural icons, which relies on bicultural individuals, self-construal
8 priming is regarded as universal. It is this universality—in addition to the theoretical
9 position of cultural influence as temporary access—that makes self-construal priming
10 particularly attractive. Indeed, Gardner and colleagues (1999) observed self-construal
11 priming effects in both monocultural European-American and bicultural HK Chinese
12 students. Here, in a substantially better-powered study, we found strong evidence for no
13 self-construal priming in Chinese students. In some ways, our participants from
14 CUHKSZ—where teaching is conducted in English—were more similar to bicultural HK
15 Chinese, compared with our more traditional participants from SIIT. But whether
16 participants are bicultural or monocultural, the key tenet of self-construal priming is that
17 primes inconsistent with a person’s predominant cultural worldview result in a shift of
18 self-construal. Our results are evidence inconsistent with this prediction.

19
20 Unlike Gardner and colleagues (1999), where participants in the control condition were
21 “given no prime”, here we used active control, by matching the task across conditions
22 (e.g., searching for a specified word instead of pronounces, or reading a similar story but
23 without the dilemma, as in Lin & Han, 2009; Rice et al., 2009; Sui & Han, 2007). But the

1 results of Gardner and colleagues (1999) could not be attributed to whatever way their
2 control condition was designed, because the reported priming effect was also obtained
3 when comparing independent and interdependent primes (Study 1). Yet, no priming
4 effects were observed in the replication when comparisons were restricted to independent
5 and interdependent primes.

6
7 Whereas Gardner and colleagues (1999) presented materials in English for both
8 American and HK Chinese students, here we presented materials in Chinese for our
9 Chinese students. One concern of their study is that the use of English in bilingual HK
10 Chinese could influence their responses. Apparently, this concern did not sabotage their
11 study: cultural differences between American and HK Chinese students were still
12 apparent in their no-prime control conditions (Study 2). Our choice of using the Chinese
13 language to present materials parallels the choice by Gardner and colleagues (1999) to
14 use the English language for their American counterparts. Although our Chinese
15 participants at CUHKSZ could be considered proficient in English or even bilingual,
16 participants at SIIT (a vocational school in which teaching is in Chinese) were not. Even
17 if they all were, the use of the Chinese language would be the preferred choice so as to
18 better align with their predominant cultural worldview.

19
20 As with the differences in location and participants between the original and replication
21 studies, the difference in time (20 years apart) could not have contributed to the different
22 results. That is, although it is conceivable that the mainland Chinese today are perhaps
23 more independent than the Chinese 20 years ago, changes in relative independence or
24 interdependence ultimately should not matter. The central position of the access

1 perspective is that, because independent and interdependent elements reside in every
2 individual, we are universally susceptible to priming from situational cues. Indeed, as
3 reported by Gardner and colleagues (1999), self-construal priming effects were evident in
4 both monocultural and bicultural individuals.

5

6 Our results therefore support the *habit* perspective of cultural influences but not the
7 *access* perspective. They indicate that cultural influences appear to be pervasive and
8 rigid, by affecting how we habitually and routinely perceive, think, feel, and act. Just as
9 old habits die hard, cultural influences could not be easily shaken off, or nudged through
10 temporal cues, at least not for monocultural individuals using story primes and word-
11 search primes. Even if cultures make a particular way of perceiving, thinking, feeling,
12 and acting more accessible to an individual than another way, the relative access afforded
13 by cultures appears to be much more rigid and less malleable than previously assumed
14 (Brewer & Gardner, 1996; Gardner et al., 1999; Trafimow et al., 1991).

15

16 We have found that the lack of self-construal priming effect was not modulated by
17 experience living aboard. Still, it is possible that perhaps bicultural individuals might be
18 more susceptible to priming than monocultural individuals, as suggested by cultural
19 priming studies that used cultural icons to prime bicultural individuals (Hong et al.,
20 2000). Our results speak directly only to one of the two main types of cultural priming—
21 namely, self-construal priming. For bicultural individuals who internalize two different
22 cultural frames, it is conceivable that they may switch between these frames in response
23 to situational cues—so-called frame switching (Hong et al., 2000). Given the important

1 implications of cultural priming, it would be valuable to also directly replicate cultural
2 priming studies that use cultural icons to enable frame switching in bicultural individuals.

3
4 Our results might also have implications for the so-called rice theory of cultural
5 differences. The rice theory argues that a history of farming rice (vs. wheat) makes
6 people and cultures more interdependent (Talhelm et al., 2014) and social norms tighter
7 (Talhelm & English, 2020). Interestingly, the three primary measures of the self in the
8 original report (Talhelm et al., 2014) were not on independent vs. interdependent self-
9 construal, but on related concepts, including holistic thought (to pair two out of three
10 items), implicit individualism (to draw circles of friends and the self), and
11 loyalty/nepotism (to decide whether to reward or punish an honest friend/stranger and a
12 dishonest friend/stranger). Our measures of independent/interdependent self-construal—
13 self-descriptions, value judgment, and social obligation judgment—allowed us to
14 examine how rice culture (vs. wheat culture) affected self-construal. The results suggest
15 that rice or wheat cultures appeared to affect the endorsement of collectivist values, but
16 not other measures such as self-descriptions or social obligation judgment. Moreover, the
17 rice/wheat farming modulation on value endorsement vanished after excluding
18 participants from Guangdong. These results can be interpreted to have partially supported
19 the original observations (Talhelm et al., 2014), but they are also consistent with critiques
20 of the rice theory (e.g., Ruan et al., 2015), particularly considering the lack of
21 modulations in self-descriptions in the classic TST. Therefore, it would be of great
22 interest and importance to understand how rice (vs. wheat) cultures influence different
23 measures of self-construal—self-descriptions, value judgment, social obligation

judgment, holistic thought, implicit individualism, and loyalty/nepotism. To this end, future direct replication and extension of the study by Talhelm et al. (2014), by considering comprehensive measures of independent/interdependent self-construal, would be highly valuable.

Conclusion

We show that, despite efforts to maximize priming and to identify moderators, self-construal priming, either through story primes or word-search primes, did not change the relative independence or interdependence of the self-construal in Chinese participants. These results undermine a foundation of cultural priming—self-construal priming—that has had a profound influence on psychology, neuroscience, and many applied fields. Our findings suggest that the predominant access afforded by cultures is much more rigid and less malleable than previously assumed. In turn, they support the habit mechanism of cultural influences but not the access mechanism. To build a cumulative and reproducible cultural psychology, we call for direct replications and extensions of related findings, including cultural priming and beyond (e.g., the rice theory).

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