




**Advancing the Psychology of Social Class with
Large-Scale Replications in Four Countries**

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Registered Report:

Stage 1 in-principle acceptance at *Nature Human Behaviour*

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Abstract

As the topic of inequalities has gained attention in the past decade, social scientists have developed theoretical frameworks to understand how social class shapes the way individuals think, feel, and behave. These frameworks suggest that lower-class contexts nurture psychological and behavioral tendencies oriented toward others and the environment (interdependence, contextualism), whereas higher-class contexts nurture tendencies oriented toward the self (independence, solipsism). However, empirical research on social class faces obstacles that limit the generalizability of extant findings (small sample sizes, nondiverse convenience samples, measurement flexibility). We propose a large-scale ($N = 36,000$) and theoretically comprehensive replication of 43 key effects (from 22 studies) of social class on the self, relationships, emotions, cognition, social behavior, and decision-making. The project will also contribute to refining theoretical models by testing the predictive strength of widely (and flexibly) used measures of social class and individual, social, and structural moderators of the effects of social class.

Keywords: Social class; socioeconomic status; self; relationships; emotion; cognition; social behavior, decision-making; replication; social class indicators.

As the topic of socioeconomic inequality has gained considerable political momentum in the last decade, psychologists and behavioral scientists have finally turned their attention to the study of social class, developing various theoretical frameworks to account for the role of social class in shaping the way individuals think, feel, and behave¹⁻⁵. These theoretical frameworks describe social class as a relative social position defined by unequal access to economic, cultural, social, and/or symbolic resources (e.g., income, diplomas, self-perceived rank). Lacking or possessing such resources defines the type of social context to which individuals are exposed: Lower social class contexts are often characterized by higher levels of constraints and uncertainty (e.g., employment insecurity, scarce resources, social class prejudice), whereas higher social class contexts are characterized by more freedom and volition (e.g., employment security, abundant resources, social class privilege). Repeated experiences in these different social contexts frame individuals' psychological tendencies⁶⁻⁹. Because individuals from lower social classes live in more unstable and high-constraint contexts, they tend to be more oriented toward others and their environment (manifested in being vigilant to external influences and potential threats, seeing oneself as connected to others, and using community as a resource). In contrast, because individuals from higher social classes live in more stable and low-constraint contexts, they tend to be more oriented toward the self (manifested in being focused on internal states and potential rewards, seeing oneself as unique, and pursuing independence and self-sufficiency).

This central premise has generated a large number of studies documenting the effects of social class¹⁰. A literature review led us to identify 43 effects that illustrate the pervasiveness of the psychological impact of social class on (1) the self, (2) relationships, (3) emotion, (4) cognition, (5) social behavior, and (6) decision-making (Table 1). First, regarding the self, the recurring hardships experienced by individuals from lower social classes manifest in lower sense of control, perceived agency, and self-esteem than are

observed in individuals from higher social classes^{11,12} (see also, Refs. 13–15). Because of the greater constraints and instability of their life contexts, individuals from lower social classes tend to construe their self as interdependent (emphasis on connection to others), whereas individuals from higher social classes construe their self as independent (emphasis on distinctiveness from others)^{16,17}. Second, regarding relationships, individuals from lower social classes tend to develop small, close-knit, and homogeneous social networks, which they rely on when facing adversity; in contrast individuals from higher social classes have more dispersed and diverse networks and prioritize the use of personal economic resources when facing adversity^{16,18} (for work on social class and intergroup relations, see Ref. 19). Third, regarding cognition, differences in terms of other/environment- vs. self-oriented psychological tendencies entail different cognitive styles and patterns of sense-making: Individuals from lower social classes tend to process information as a global whole, anticipate more change in life trajectories (e.g., a higher likelihood of going from a millionaire to being bankrupt), and produce more contextual explanations of events (e.g., failure/success are seen as beyond an individual's control), whereas individuals from higher social classes process information in parts, anticipate more stability in life trajectories, and produce more dispositional explanations of events^{11,16}. Fourth, with respect to emotion, other/environment- vs. self-oriented psychological tendencies further imply that individuals from lower social classes are more prone to other-oriented emotions such as compassion or guilt, whereas individuals from higher social classes are more prone to self-oriented emotions such as contentment or pride²⁰ (for additional research, see Refs. 11,21). Fifth, when it comes to social behavior, these tendencies ultimately affect unethical and prosocial behaviors. For instance, individuals from lower social classes are more likely to lie when it benefits others (lying for a friend), whereas individuals from higher social classes are more likely to lie when it benefits them (lying in one's own interest)²². Individuals from lower social classes also act

more prosocially in private settings, whereas individuals from higher social classes act more prosocially in public settings (when their behavior could reflect positively on the self)²³ (see also Ref. 24). Finally, concerning decision-making, due to cumulative exposure to deprivation (sometimes since childhood), individuals from lower social classes tend to show greater risk aversion, increased preference for immediate rewards (rather than delayed gratification), and higher cognitive load when facing difficult financial decisions than individuals from higher social classes^{25,26} (for work on social class and moral decision-making, see Ref. 27).

Together, the 43 effects from the 22 studies referenced in the above paragraph embody a new line of research seeking to understand systematic social class differences that had, until recently, been mostly interpreted as random interindividual differences²⁸. This body of research is highly influential. Although the articles reporting these effects have been published over the past decade, they have already garnered large numbers of citations (the median field-weighted citation impact was $FWCI = 3.4$, meaning that the articles got 240% more citations than expected based on the average number of citations of similar papers in the discipline over a three-year window²⁹). Moreover, most of these effects appear in studies published in prestigious, highly-visible general or specialized journals such as *Science*, *PNAS*, or the *Journal of Personality and Social Psychology* and some of the findings have been used to inform policymakers (e.g., see Ref. 30).

However, this body of research is not immune to some of the core challenges faced by the social and behavioral sciences that have led to the so-called “replication crisis”^{31–33}: (i) small sample sizes, (ii) nondiverse convenience samples, and (iii) measurement flexibility. First, the median sample size of the studies is relatively small ($M = 215$ participants), with a mean statistical power between .42 and .70 (to detect small [corresponding to $r = .10$] and medium [$r = .20$] psychological effects, respectively³⁴). Second, the vast majority of the

studies (86%) use samples that are not distributed similarly to the underlying population in terms of key demographics or nonrepresentative samples, one-third use undergraduates, and most participants (89%) are from the United States. Third and finally, the measure of social class varies greatly³⁵ between studies without—in most cases—any justification provided: The measure is based on participant's level of education ($\approx 1/4$ of the studies), income ($\approx 1/4$ of the studies), and/or subjective social class ($\approx 1/2$ of the studies). Each of these three challenges increases the probability of type I errors and threatens generalizability^{36–38}. In fact, about half of the critical p -values from the 22 studies are above .01, when the majority of the p -values should be below .01 if all effects are true (p -values between .01 and .05 are indicative of lower odds of replication, as the distribution of p -values is right-skewed for both adequately and insufficiently powered studies)^{39,40}.

To address these three challenges, we carry out a theoretically comprehensive replication of the 22 studies (Refs. 11–27) described above using (i) samples that yield high power (ii) samples that are either distributed similarly to the national population in terms of key demographics (U.S., France, India) or representative of the national population (Switzerland), and (iii) relying on a preregistered method and analytical strategy. Specifically, we will replicate 17 correlational and five experimental studies selected on the basis of criteria related to theoretical relevance, domain coverage, and feasibility (for detailed information about the study selection process, see *Methods, Design*). Table 1 presents the list of the hypothesized effects derived from the studies to be replicated (see also Table 2), and Table S1 lists all the deviations from the original studies (including lab vs. online setting, even if—based on the results from Many Labs 2⁴¹—we see no reason to expect this deviation to influence the effects). The detailed list of the preregistered hypotheses, materials, and analytical strategies can be found in Supplementary Information and on the OSF page for the project (https://osf.io/3tjzs/?view_only=85a8b0203d464becb727f2cf2490c9a4).

We will recruit a total of $N = 36,000$ participants from four countries, namely, three samples that are matched to the underlying population in terms of key demographic features (later referred to as “quota-based samples”) of $N = 9,000$ U.S. residents, $N = 9,000$ French residents, and $N = 9,000$ Indian residents and one random representative sample of $N = 9,000$ Swiss residents. The countries were selected based on both practical and cultural reasons (for detailed information about the selection of countries, see *Methods, Sampling Plan*). Importantly, four national samples will provide more cultural diversity than is usually found in extant research but will not be sufficient to estimate the worldwide generalizability of findings.

Participants will take a 20-minute online questionnaire that will include a random subset of the outcome variables used in the original correlational studies and—at most—one experiment. The questionnaire will also include nine commonly used measures of social class: education, income, occupation, subjective SES (i.e., socioeconomic status), childhood subjective SES, childhood SES, social class self-categorization, financial scarcity, and sense of power (for detailed information, see *Methods, Design*). For each country, the statistical power to detect a small-sized effect of social class on a given outcome variable will be above .97 (for detailed information, see *Methods, Sampling Plan*).

The primary purpose of our work is to test the 43 effects from the 22 studies using the same analytical strategy used in the original studies (confirmatory preregistered analysis). To ensure the quality and fairness of the replication project, the hypotheses, method, and analytical strategy were extracted from the original publications and compiled in a preregistration document; this document was then submitted to the original authors for review and amended according to their feedback. A replication will be considered successful if we observe an effect in the same direction as in the original study with an α of .05 (for detailed information, see *Methods, Analysis Plan*). With $4 \times 9,000$ participants, even practically

insignificant effects can be statistically significant⁴². However, large sample sizes allow one to obtain reliable estimates of effect size, and we will use these estimates (along with their confidence intervals) to further describe each replicated effect.

The secondary purpose of our work is to systematically compare the predictive strength of the nine measures of social class when testing the 43 effects (exploratory preregistered analysis). Although social class measures are often used in a flexible way in extant research, theory suggests that these measures should not always be considered interchangeable. Social class contexts involve complex combinations of different types of objective and self-perceived economic, cultural, and symbolic resources. These dimensions are related but can be independent (e.g., income and education are positively correlated, though one can have a high income level and a low education level). They are associated with different experiences (e.g., economic abundance or familiarity with higher social class practices) and could shape different psychological patterns⁴. Investigating whether and how the different dimensions of social class contexts foster different psychological and behavioral tendencies could facilitate the refinement of theoretical predictions and the development of new insights.

The tertiary purpose of our work is to test three relevant potential moderators of the main effects to be replicated (confirmatory preregistered analysis). First, we will test an individual moderator: social class identification. Because social class identification is defined as the degree to which an individual assigns subjective value to their social class⁴³, it should logically increase the strength of the effects of social class. Second, we will consider a social moderator: system justification beliefs. However, our prediction is tentative. On the one hand, because believing that society is fair implies that individuals from lower social classes deserve their social status (threatening their ego), system justification beliefs could increase the strength of most effects of social class (on the self, relationships, emotions, and

behaviors)¹⁵. On the other hand, system justification beliefs can serve a palliative function for disadvantaged groups (preserving their ego), which could make this prediction inaccurate^{44,45}.

Third, we will consider a structural moderator: local income inequality. Because a higher level of income inequality increases the salience of social class stratification⁴⁶, it should increase the strength of the effects of social class. Investigating these moderators should help refine the theoretical framework of the psychology of social class by specifying the boundary conditions of these effects.

Table 1. List of the 43 effects of social class taken from the 22 studies to be replicated. For each study, we report (1) sample size, (2) statistical power estimates to detect small ($r = .10$) and medium ($r = .20$) effects (because interaction effects are usually smaller than main effects⁴⁷, our small/medium effect thresholds were halved for interactions; for the script, see the OSF page of the project), (3) whether the sample was a quota-based or random sample, (4) the main measure of social class, and (5) the critical p-value(s).

Study ID	Journal, year, and #study number	Effects	Sample N	Power	Quota or Random	Main measure	Ps	
S1/2 ¹¹	<i>JPSP</i> , 2009, #1-2	Social class \pm → Sense of control ($\times 2$)	103/86	.15, .53	No (US)	Subjective	< .05	
The self (14 effects)	S3 ¹²	<i>JPSP</i> , 2018, #1	Social class \pm → Agency/Persistence in goal striving/Self-esteem/Omnibus self-orientation measure	2832	1.0, 1.0	✓ (US/JP)	Subjective Education	< .001 < .001
	S4 ¹³	<i>PLoS ONE</i> , 2019	Social class \pm → Narcissism	400	.52, .98	No (CN)	Subjective	< .001
	S5/6 ¹⁴	<i>PSPB</i> , 2014, #1a/b	Social class \pm → Entitlement ($\times 2$, one “marginal”)	195/105	.17, .80	No (US)	Subjective	.021
	S7 ¹⁵	<i>JSP</i> , 2020, #1	Social class \times High (vs. low) system-justification \pm → Entitlement	669	.74, .96	No (CN)	Occupation	< .001
	S8a ¹⁶	<i>PNAS</i> , 2010	Social class \rightarrow Interdependent self-construal	235	.33, .87	✓ (US)	Education	n.s.
	S9 ¹⁷	<i>JPSP</i> , 2007, #4b	Social class \pm → Negative reactions to reduced individuation ($\times 3$, one n.s.)	801	.81, 1.0	No (US)	Education	< .05
	Relationships (7)	S8b ¹⁶	<i>PNAS</i> , 2010	Social class \rightarrow Individuals in one’s inner circle	235	.33, .87	✓ (US)	Education
			Social class \rightarrow Social support received				Education	n.s.
S10 ¹⁸		<i>JPSP</i> , 2012, #1	Social class \times Chaos (vs. stability) \rightarrow Communal orientation	76	.14, .23	No (US)	Income	.01
S11 ¹⁸		<i>JPSP</i> , 2012, #4	Social class \times Chaos (vs. stability) \pm → Obsession with money	134	.21, .38	No (US)	Subjective	< .02
			Education bias (i.e., highly educated people rated higher)					< .001
S12 ¹⁹	<i>JESP</i> , 2018, #2	Social class \pm → Education bias	448	.56, .99	No (US)	Education	< .001	
		Education bias = Ethnic/national bias					n/a	

Table 1 (continued).

	ID	Journal, year, and #study number	Hypothesized effects	Sample		Quota or random	Main Measure	Ps
				N	1 - β			
Cognition (7)	S8c ¹⁶	<i>PNAS</i> , 2010	Social class \rightarrow Thematic/holistic thinking style	235	.33, .87	✓ (US)	Education	.08
	S8d ¹⁶	<i>PNAS</i> , 2010	Social class \rightarrow Anticipation of change	235	.33, .87	✓ (US)	Education	< .01
	S13 ¹¹	<i>JPSP</i> , 2009, #3	Social class \rightarrow Contextual explanations (\times 2) Social class \pm Sense of control \rightarrow contextual explanations (\times 3)	444	.56, .99	No (US)	Subjective	< .05 < .05
Emotion (6)	S14 ²⁰	<i>Emotion</i> , 2018	Social class \rightarrow Other-oriented positive emotions (\times 2) Social class \pm Self-oriented positive emotions (\times 2)	1519	.97, 1.0	✓ (US)	Income	\leq .002 \leq .033
	E1 ²¹	<i>Frontiers</i> , 2014, #2	Social class \times Sharing (in)equality \rightarrow Self-conscious negative emotions	103	.17, .30	No (US)	Composite	< .05
	S15 ¹¹	<i>JPSP</i> , 2009, #4	Social class \rightarrow Influence of contextual emotional information	125	.20, .61	No (US)	Subjective	< .05
Behavior (3)	E2 ²²	<i>JPSP</i> , 2015, #2	Social class \times Self- (vs. other-) benefits \pm Unethical behaviors	81	.15, .25	No (EUR)	Income	< .001
	E3 ²³	<i>SPPS</i> , 2016, #3	Social class \times Private (vs. public) context \rightarrow Prosocial behavior	363	.48, .77	No (US)	Other	.001
	S16 ²⁴	<i>PNAS</i> , 2012, #5	Social class \pm Unethical behaviors	108	.18, .55	No (US)	Subjective	< .02
Decision-making (6)			Social class \pm Preferring immediate rewards					.009
	E4 ²⁵	<i>JESP</i> , 2018, #4	Social class \rightarrow Preferring risky choices Social class \times Mortality (vs. control) \emptyset Preferring immediate rewards Social class \times Mortality (vs. control) \emptyset Preferring risky choices	1293	.95, 1.0	No (US)	Other	< .001 n.s. n.s.
	E5 ²⁶	<i>Science</i> , 2013, #1	Social class \times Hard (vs. easy) financial problems \pm Performance	101	.17, .30	No (US)	Income	.03
	S17 ²⁷	<i>JPSP</i> , 2013, #1	Social class \pm Utilitarian moral decision making	277	.38, .92	No (US)	Other	< .001

Notes: S* and E* indicate a correlational or experimental study, respectively; \pm means “positive effect,” \rightarrow “negative effect,” \emptyset “null effect,” and = “equivalence effect”; n/a means “not applicable” (the effect was not tested in the original study) and n.s. “nonsignificant.”

Methods

Ethics information

The project was approved by the Research Ethics Board of the university of the corresponding author (C_SSP_032020_00004). Participants will give their informed consent at the beginning of the study. Participants recruited by panel providers (U.S., French, and Indian samples) will receive various types of compensation (e.g., gift cards), whereas participants directly recruited by a local team (Swiss sample) will not be compensated.

Design

Selection of the studies. The studies were selected based on a review conducted by the three authors. In June 2019, we used the reference lists of two recent reviews about the psychology of social class^{10,48} and further searched for empirical studies on a scientific publication search engine with three guiding criteria: the studies had to be theoretically relevant (testing a central claim of the various frameworks of the psychology of social class), cover a large variety of domains (i.e., self, cognition, emotion, relationships, decision-making and behavior), and be feasible online. This led to an initial selection of 21 studies. In December 2019, after securing funding for the project, we updated the list by searching for all new empirical studies that cited these 21 studies. To further ensure that we had not missed any important studies, when asking the authors of the original studies to review the protocol, we asked them to list findings they deemed important to replicate. This led us to remove one study and add two others.

Questionnaire. We will invite participants to complete an online questionnaire. Drawing on former large-scale replication initiatives^{41,49}, we will randomize the blocks of our questionnaire so that (i) the total duration will not exceed 20 minutes and (ii) participants will not take part in more than one experiment. In addition to completing the nine measures of social class (≈ 2 min.), participants will be randomly assigned to complete 2/3 of the short

tasks or scales (\approx 12-13 min.) and 1/6 of the long tasks or experiments (\approx 3-4 min.). Two translation service companies will translate the questionnaire from English to French (for the French and Swiss samples), Hindi (for the Indian sample), German, and Italian (for the Swiss sample).

Predictor variables. Participants will first complete the nine measures of social class. The measures that will not be used to build the quotas will be counterbalanced (placed at the beginning of the questionnaire for half of the participants and at the end for the other half). For each hypothesis, we will determine the need to control for the order variable in preliminary analysis (see *Preregistration*).

Education will be measured using various categories for the highest level of education and combined into three groups representing the national population-based tertiles: (i) high school graduate or less vs. two-year college degree vs. four-year college degree or higher (U.S. sample)⁵⁰, (ii) less than high school vs. high-school graduate or two-year college degree vs. three-year college degree or higher (French sample)⁵¹, (iii) less than high school vs. high school graduate vs. some college or higher (Indian sample)⁵², and (iv) less than high school vs. high school and non-university diploma vs. bachelor's degree or higher (Swiss sample)⁵³. For students, we will use the highest level of education completed by either of their guardians.

Income will be measured using bands corresponding to the national population-based household income deciles⁵⁴⁻⁵⁷. An open-ended question will then ask participants to specify the exact amount of their household income. In the analysis, we will use the response to the open-ended question or—if this response is missing—the midpoint of the household income bands (see *Preregistration*). To adjust for household size, we will use the OECD square-root scale to compute equivalized income (i.e., dividing household income by the square root of household size)⁵⁸.

Occupation will be measured using Hoffmeyer-Zlotnik's scheme⁵⁹. After reporting their current employment status, self-employed participants will indicate the size of their farm/business, whereas employed participants will indicate the level of autonomy/complexity of their job. Participants will then be assigned to one of five occupational class categories (from 1 = *unskilled, semiskilled, manual workers* to 5 = *far-reaching leadership tasks and decision-making powers*). For retirees and students, we will determine the former occupational class category or the category of the main earner in the family, respectively.

Subjective SES will be measured using the MacArthur Scale¹⁸. Participants will be presented with a 10-rung ladder representing "where people stand in [their] local community." They will be asked to indicate where they think they stand on the ladder (from 1 = *bottom* to 10 = *top*).

Childhood subjective SES will be measured using an adapted version of the MacArthur Scale⁶⁰. This time, participants will be asked to indicate where they and their family stood on the ladder when they were 5 to 10 years old (from 1 = *bottom* to 10 = *top*).

Childhood SES will be measured using a three-item scale (e.g., "I grew up in a relatively wealthy neighborhood"; from 1 = *strongly disagree* to 7 = *strongly agree*)³.

Social class self-categorization will be measured by asking participants to report their social class (from 1 = *lower class* to 5 = *upper class*)⁶¹.

Financial scarcity will be measured by asking participants about the balance of their income and expenses (from 1 = *saves a lot of money* to 5 = *gets into debt*; for evidence of the convergent validity of the instrument, see Ref. 62,63).

Sense of power will be measured using a three-item scale (e.g., "I think I have a great deal of power"; from 1 = *strongly disagree* to 7 = *strongly agree*)²².

Outcome variables: self-reported scales or short tasks. Unless otherwise specified, response scales range from 1 = *strongly disagree* (or *not at all*) to 7 = *strongly agree* (or

completely). Items belonging to the same scale will be presented in a randomized order.

The self. *Sense of control* (S1/2) will be measured using the 12-item Personal Mastery and Perceived Constraints Scale (e.g., “What happens in my life is often beyond my control”)⁶⁴. *Agency* (S3) will be measured using the Agency Scale, which asks participants the extent to which five traits describe them (e.g., “self-confident”)⁶⁵. *Persistence in goal striving* (S3) will be measured using the 5-item Persistence in Goal Striving Scale (e.g., “When I encounter problems, I don’t give up until I solve them”)⁶⁶. *Self-esteem* (S3) will be measured using the 4-item Rosenberg Self-Esteem Short Scale (e.g., “I take a positive attitude toward myself”)⁶⁷. *Narcissism* (S4) will be measured using the 6-item Narcissistic Admiration and Rivalry Questionnaire Short Scale (e.g., “Being a very special person gives me a lot of strength”)⁶⁸. *Entitlement* (S5/6, S7) will be measured using the 9-item Psychological Entitlement Scale (e.g., “I honestly feel I’m just more deserving than others”)⁶⁹. *Inter/independent self-construal* (S8a) will be measured using an adaptation of Singelis’ Self-Construal Scale, which asks participants the extent to which 10 independent (e.g., “I always express my opinions clearly”) and 10 interdependent (e.g., “I am concerned about what people think of me”) statements describe them (from 1 = *does not describe me at all* to 5 = *describes me very much*)^{16,70}. *Negative reactions to reduced individuation* (S9) will be measured using three questions that participants respond to by expressing whether they feel good or bad after imagining a friend purchasing the same car as them¹⁷.

Relationships. *Chaos (vs. stability)*, *Communal orientation* (S10) and *Obsession with money* (S11) will be measured as follows: First, participants will choose between a chaotic (with ups and downs) and a stable (steadily increasing) graph to represent the expected trajectory of their future economic well-being; second, they will complete the 13-item Communal Orientation Scale (e.g., “I often go out of my way to help another person”; from 1 = *extremely uncharacteristic of me* to 7 = *extremely characteristic of me*)⁷¹ and the 5-item

Obsession subscale of the Money Beliefs and Behavior Scale (e.g., “I feel that money is the only thing that I can really count on”)⁷². The scales will be presented in a random order.

Education and ethnic/national biases (S12) will be measured with thermometer ratings (from 0 = *very cold* to 100 = *very warm*) of seven groups (i.e., more-educated vs. less-educated people as well as five ethnic groups – four outgroups and one ingroup, adapted to the cultural context)¹⁹.

Cognition. *Thematic/holistic and taxonomic/analytical thinking style* (S8c) will be measured using the triad task, which includes 12 lists of three objects (e.g., cow, grass, chicken), each involving one thematic pair (cow/grass) and one taxonomic pair (cow/chicken); participants have to indicate which two objects are more closely related¹⁶.

Contextual explanations (S13) will be measured using ratings of eight events (e.g., “being laid off at work”) on a 7-point bipolar scale ranging from 1 = *individual [is] primarily responsible* to 7 = *outside forces [are] primarily responsible*¹¹. *Anticipation of change* (S8d) will be measured with the estimated likelihood (in %) that contradictory events happen in eight different situations (e.g., “Two kids are fighting at kindergarten. How likely is it that they will become lovers some day?”)¹⁶.

Emotion. *Other- and self-oriented positive emotions* (S14) will be measured using the 12-item Dispositional Positive Emotions Scale assessing feelings of contentment (e.g., “I am generally a contented person”), pride (e.g., “I take great pride in my achievements”), compassion (e.g., “I am a very compassionate person”), and love (e.g., “I grow to love people who are kind to me”)²⁰. *Influence of contextual emotional information* (S15) will be measured using 12 pictures with a central character showing an emotion (anger, happiness, or sadness) and four background characters showing either the same emotion (three pictures) or a different emotion (nine pictures). For each picture, participants will rate the anger, happiness, and sadness of the center character using scales ranging from 1 = *not at all* to 10 = *very*

*much*¹¹.

Social behavior. *Unethical behaviors* (S16) will be measured as follows. Participants will be asked to take the role of an employer negotiating a salary with a job candidate seeking a long-term position. Participants will be told that the job will be eliminated after six months, but they have strong incentives to fill the position. Afterwards, they will report as a percentage how likely they would be to hide the truth about the position from the candidate²⁴.

Decision-making. *Utilitarian moral decision-making* (S17) will be measured using the so-called footbridge dilemma. After reading a scenario about a trolley heading down the tracks toward five workmen, participants will have to indicate whether it is appropriate or not to push a stranger onto the tracks to stop the trolley from killing the five workmen²⁷.

Outcome variables: experiments or longer task. After completing 2/3 of the self-report scales or short tasks, participants will be randomly assigned to one of the five experiments or the social network task.

Relationships: Social network task (S8b). Participants will be presented with three concentric circles centered on a small circle labeled “YOU.” First, we will assess the proportion of *Individuals in one’s inner circle*. Participants will be asked to report the initials of (i) the people to whom they are very close in the inner circle, (ii) the people who are not so close but still important in the middle circle, and (iii) the people who are important enough in the outer circle. Second, we will assess the *Social support received*: Participants will report whether each of their network members shows more social support or annoyance¹⁶.

Emotion: Sharing (in)equality and self-conscious negative emotions (E1). First, participants will self-rate nine baseline emotions (i.e., *Self-conscious negative emotions* [embarrassment, fear, guilt, and worry] and five other negative emotions; from 1 = *not at all* to 8 = *very much*)²¹. Second, participants will play the recipient in the Dictator Game, with a bogus other allocating 10 raffle tickets between themselves and the participant. They will be

randomly assigned to one of two between-participants conditions: (i) in the near-equal sharing condition, the bogus other will share 4 out of 10 tickets with the participant; (ii) in the low sharing condition, the bogus other will share 1 ticket. Third, participants will again complete the emotion measures.

Social behavior: Self- (vs. other-) benefits and unethical behaviors (E2).

Participants will be randomly assigned to one of two between-participants conditions: (i) in the self-benefit condition, they will be asked the extent to which they would engage in eight *Unethical behaviors* for their own benefit (from 1 = *very unlikely* to 7 = *very likely*); (ii) in the other-benefit condition, they will be asked the extent to which they would engage in the same eight unethical behaviors for the benefit of another person (using the same scale)²².

Social behavior: Private (vs. public) context and prosocial behavior (E3).

Participants will be asked to play the dictator in the Dictator Game twice and allocate 10 raffle tickets between themselves and a bogus other. Participants will be assigned to the two within-participants conditions in a counterbalanced order: (i) in the private condition, they will be instructed that their gift will be anonymous; (ii) in the public condition, they will be instructed that their gift will be accompanied by identifying information (i.e., name and city of residence). *Prosocial behavior* will be measured using the number of raffle tickets allocated to the bogus other²³.

Decision-making: Mortality (vs. control) and preference for immediate rewards and risky choices (E4). Participants will be randomly assigned to one of two between-participants conditions: (i) in the mortality condition, they will read a text presenting the world as dangerous, and (ii) in the control condition, they will read a text about how to choose a rain jacket²⁵. *Preference for immediate rewards* will be measured using seven hypothetical choices involving an increasing monetary reward for delayed options (e.g., “Do you want to get \$100 tomorrow or \$110 90 days from now?” [with \$10 increments]), and *Preference for*

risky choices will be measured using seven hypothetical choices involving an increasing monetary reward for less risky options (e.g., “Do you want a 50% chance of getting \$800 OR [to] get \$100 for sure?” [with \$100 increments]). The order of presentation of the measures will be counterbalanced.

Decision-making: Hard (vs. easy) financial problems and cognitive performance

(E5). Participants will be randomly assigned to one of two between-participants conditions: (i) in the hard financial problems condition, they will have to think about how to solve four financial problems involving large amounts of money, and (ii) in the easy financial problems condition, they will have to think about how to solve similar problems involving smaller amounts of money (the amounts of money will be adapted to the standard of living in the national population)²⁶. Then, participants will be instructed to keep thinking about the financial problems while performing three Raven’s Standard Progressive matrices presented in a random order. *Cognitive performance* will be measured using the number of matrices correctly solved. We will pretest Raven's Matrices (focusing on matrices of average difficulty according to the manual⁷³) in a pilot study and select three matrices correctly solved by ≈50% of individuals.

Control variables. Control variables similar to those used in the original studies will be included at the end of the questionnaire. In the U.S. sample, *Political orientation* will be measured using a scale ranging from 1 = *very liberal* to 7 = *very conservative* (in the French, Indian, and Swiss samples, the labels will be adapted). In all samples, *Religiosity* will be measured using a scale ranging from 1 = *not at all religious* to 7 = *very religious*. In the U.S. sample, *Ethnicity* will be measured using the U.S. Census Bureau’s categories (e.g., White, African American) plus Latinx (in the Indian sample, the labels will be adapted). In the French and Swiss samples, the ethnicity question will not be displayed because (1) in France, ethnicity-based statistics are prohibited except under very specific circumstances, and (2) in

Switzerland, it is also a very sensitive question. Instead, participants will be asked about their nationality to differentiate citizens and non-citizen residents.

Moderators. *Social class identification* will be measured using a single-item scale asking participants to rate the importance of their social class in describing them⁶¹. System justification beliefs will be measured using the four-item System Justification Scale (e.g., “In general, I find society to be fair”)⁷⁴. Local income inequality indicators will be extracted from public economic data and merged with the survey data; we will use the ACS-based ZIP code-level Gini coefficients for the U.S. sample⁷⁵, the INSEE-based municipality-level Gini coefficients for the French sample⁷⁶, the NSSO-based district-level Gini coefficient from the Indian sample⁷⁷, and the SFC-based municipality-level Gini coefficients for the Swiss sample⁵⁴.

Attention checks. In line with the current recommendations about quality checks⁷⁸, we will use two easy attention checks, one at the beginning of the questionnaire and another at the end (e.g., “This is an attention check; please select ‘somewhat agree’”). For the studies involving a manipulation, no manipulation check is used because (i) the original studies included a successful manipulation check, and (ii) manipulation checks do not provide information relevant to construct validity except in pilot studies⁷⁹.

Sampling Plan

Selection of the countries. Our total sample will comprise participants residing in four countries: the U.S., France, India, and Switzerland. The selection of the countries was based on both practical and cultural reasons. Practical reasons involved—among other things—cost, feasibility, and the authors’ familiarity with the national context. For instance, including the U.S. allowed for replications with the same population used in 89% of the selected studies, whereas including Switzerland allowed us to draw a random probability sample at a low cost. Cultural reasons involved ensuring a degree of diversity in terms of

cultural attitudes toward power inequality and hierarchy (e.g., power distance index⁸⁰) or WEIRDness (Western educated industrialized rich democracies⁸¹). For instance, France is one of the Western European countries that is the most culturally distant from the U.S., with a long tradition of class protests and the endorsement of an overall socialist agenda⁸², whereas India is a LMIC (lower middle income country⁸³) and the only LMIC for which we could recruit a large quota-based sample.

Data collection. Our total sample will comprise three quota-based samples (U.S., French, and Indian samples) and one random representative sample (Swiss sample).

U.S., French, and Indian samples. We entered into a contract with Qualtrics (a market research company), which will deliver three quota-based samples of 9,000 U.S., 9,000 French, and 9,000 Indian adult residents (above 18). To achieve distribution of the samples that matches the underlying populations, Qualtrics will use quota sampling with five quotas: (1) income, (2) education, (3) gender, (4) age, and (5) region (in the case of India, Qualtrics can only guarantee three quotas, namely, income, gender, and age). Specifically, each sample will be built gradually so that the sample distribution will match the official national distribution for each quota (e.g., the U.S. sample income deciles will be similar to the official national income deciles⁵⁵). The main advantage of quota sampling is that it ensures that each stratum of the population is equally represented in the sample, but its main limitation is that the sample is nonprobability (i.e., self-selected into the panel and hence not representative of the underlying population).

Swiss sample. We will build a representative sample of 9,000 Swiss residents. To achieve representativeness, we will use random sampling: (1) the Swiss Federal Statistical Office drew a random sample (stratified by canton) of $\approx 50,000$ Swiss addresses; (2) a public institution (the DAL) will print, fold, and assemble 50,000 letters of invitation to participate in our study; and (3) our university will send these 50,000 letters. We expect a response rate

between $1/5$ and $1/6$ (the response rate for a Swiss probability-based web survey without monetary incentive⁸⁴, if the final sample size is $N \geq 7,734$ (response rate ≥ 15.45), the statistical power to detect a small-sized hypothesized effect on a given outcome will be .95, and we will stop data collection; however, if the final sample size is $N < 7,734$ (response rate < 15.45), the statistical power will be below .95, and we will ask Qualtrics to complete the sample (to reach $N = 7,734$) using the same quota sampling approach used in the U.S. and French samples). The main advantage of random sampling is that each individual in the population has the same probability of being invited to participate in the study, but its main limitation is that individuals from specific subgroups (e.g., people with lower income) may have a lower response rate (nonresponse bias). In the context of our research, we believe that the advantage of random sampling compensates for the limitation of quota sampling and vice versa.

Power analysis. We aimed to reach a power of .95+ for each individual hypothesis. For each country, with $9,000 \times 2/3 = 6,000$ participants per self-reported scale or short task, we will have a power of .99999999712 to detect one small individual effect ($f^2 = .01$). For each country, with $9,000 \times 1/6 = 1,500$ participants per experiment or long task, we will have a power of .97312 to detect one small effect ($f^2 = .01$). The power achieved for each *hypothesis* (i.e., power estimates raised to the power of the number of effects associated with each hypothesis)—which is always higher than .97—can be found in Table 2.

Exclusion criteria. For the U.S., French, and Indian samples, Qualtrics will remove and replace participants who provided low-quality responses (e.g., participants failing at least one of the two attention checks, speeders [participants who complete the questionnaire in less than half the median time], and straight-liners [participants always providing the same responses to the Likert scales]). For the Swiss sample, we will use the same exclusion criteria. For the experiments using lengthy experimental inductions (i.e., E4 [mortality] and

E5 [financial problems]), we will test the hypotheses with and without participants who read the induction in less than half the median time (we will not conduct the analysis excluding participants if the power falls below .95; given we will oversample by $\approx 15\%$, this should seldom occur).

Missing data. For each particular model, we will use listwise deletion to handle missing data (thus, the sample size will vary from one analysis to another).

Analysis Plan

Reliability criteria for item inclusion in the scales. We will calculate Cronbach's alpha to test the reliability of the multi-item scales. If the Cronbach's alpha is below .60, we will remove problematic item(s). If the Cronbach's alpha remains below .60, we will proceed with the analysis, but we will add a note to indicate that the scale's reliability is unsatisfactory.

Confirmatory analysis and expected outcomes for the primary analysis (i.e., replications). For each country and each effect, we will use the same analytical approach used in the original study (when possible). The full list of hypotheses, power estimates, and planned analyses can be found in Table 2 and the provisional preregistration. All deviations from the original studies are listed in Table S1.

Exploratory analysis and expected outcomes for the secondary analysis (i.e., comparison of the measures of social class). For each effect, we will repeat the planned analysis (described in Table 2) without control variables (for reasons of comparability) and pooling country-specific samples (for reasons of brevity and intelligibility). Specifically, we will run nine seemingly unrelated regression models with each of the nine measures of social class (see *Methods, Design*) and country-based dummies (i.e., country fixed effects⁸⁵). This analytical approach will enable us to discard all between-cluster variations, eliminating all potential between-country confounders. This will produce unbiased estimates of the pooled

within-country effects of each social class indicator⁸⁶. Then, we will perform a series of postestimation Wald tests comparing the standardized coefficient estimates associated with the nine measures of social class. We have not formulated hypotheses regarding specific differences between the nine measures in terms of predictive strength.

Confirmatory analysis and expected outcomes for the tertiary analysis (i.e., moderation effects). For each effect and each measure of social class, we will again repeat the primary analysis without control variables and using the pooled sample. This time, we will additionally include one of the three potential moderators: social class identification, system justification beliefs, and local income inequality (in 3 [moderators] \times 9 [measures of social class] separate models). To maintain a high level of statistical power, we will focus on potential moderations of original main effects (i.e., avoiding testing second-order interactions). In each case, we will mean-center predictors and enter the first-order interaction term between social class and the moderator. The interaction term should reveal a larger social class effect when social class identification, system justification, or local income inequality is high (the prediction regarding system justification beliefs is more tentative). For local income inequality, we will first estimate the design effect (which quantifies the degree to which a multilevel sample differs from a simple random sample) and—if the design effect is above 1.5—we will use multilevel modeling (with country fixed effects)⁸⁷. In any case, because we are interested in comparing participants within a given country rather than within ZIP codes or municipalities, we will use grand-mean centering⁸⁸. The full list of hypotheses, power estimates, and planned analyses can be found in Table 3.

Correction for multiple tests. For the primary goal (replications) and tertiary goals (moderations), the different confirmatory tests will not be considered as belonging to the same family of tests⁸⁹. Thus, we will use the conventional alpha level, that is, $\alpha = .05$ (e.g., as in Ref. 49). For the secondary goal, exploratory tests pertaining to a similar outcome but

using different measures of social class will be considered to belong to the same family of tests. Thus, we will use the sequential Bonferroni procedure as a correction for multiple tests. For each outcome, we will use an adjusted alpha level of $\alpha_{\text{adj}} = .05/9 = .0056$ for the measure of social class with the smallest p -value, $\alpha_{\text{adj}} = .05/8 = .0063$ for the measure with the second smallest p -value, $\alpha_{\text{adj}} = .05/7 = .0071$ for the measure with the third smallest p -value, and so on⁸⁹.

Null results. In the primary part, when observing a null hypothesized effect, we will determine if the effect is absent by using equivalence testing⁹⁰. Equivalence testing enables one to reverse the null and alternative hypotheses, so that the burden of proof rests in proving equivalence⁹¹. Specifically, we will use $f = |0.05|$ as the smallest effect size of interest (which corresponds to $r = |0.05|$ or $d = |0.10|$, and arguably pertains to a trivial effect^{34,92,93}). For each occurrence, we will compare the hypothesized effect size to $f = -0.05$ (our lower equivalence bound) and $f = +0.05$ (our upper equivalence bound) using one-sided postestimation Wald tests⁹⁴. If both tests are significant, the effect will be interpreted as equivalent to zero; otherwise, the result will be deemed to be inconclusive. In the secondary and tertiary part, we will interpret a null effect as inconclusive.

Data transformation. We do not anticipate data transformation. If the homoscedasticity assumption is violated, we will use robust estimation of SE.

Data availability.

A supplementary information file will link to the original dataset and to the material.

Code availability.

A link to the codes used to conduct the power analyses in Stage 1 is currently available in the supplementary information file. After Stage 2, this file will also link to the codes used to analyze the full data in Stage 2.

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Acknowledgements

This study was supported by a Swiss National Science Foundation Spark grant awarded to the authors (#CRSK-1_190412). This publication was also supported by the Swiss National Center of Competence in Research LIVES—Overcoming vulnerability: Life course perspectives (NCCR LIVES). The funders had no role in study design, data collection and analysis, decision to publish or preparation of the manuscript. We warmly thank Dorsa Amir, Wei Cai, Stéphane Côté, David Dubois, Yong-Yu Guo, Igor Grossmann, Michael Kraus, Toon Kuppens, Yuri Miyamoto, Jinkyung Na, Paul Piff, Eldar Shafir, and Nicole M. Stephens for reviewing the materials and preregistration document, and for providing additional details from the original studies when needed. We thank Fabrizio Butera, Luisa Meglio Vallat, Nathanaëlle Minard, Marianna Schismenou, for their institutional and administrative support. We thank the Swiss Federal Statistical Office and, in particular, Cédric Grandchamp, for their help in drawing the Swiss random sample.

Author contributions

The authors equally contributed to the conceptualization of the replication studies, to the acquisition of the financial support for the project and wrote the registered report.

Competing interests

The authors declare no competing interests.

Table 2. Design Table: Primary Purpose

Question	Hypothesis	N (power)	Analysis plan	Interpretation
S1/2. Sense of control	The higher the social class, the higher the sense of control (H1)	4 × 6,000 (.9999+)	Sense of control will be regressed on subjective SES with and without the original covariates (Ed, I, P)	I2
S3. Agency, persistence in goal striving, self-esteem	The higher the social class, the higher the agency (H2), persistence in goal striving (H3), and self-esteem (H4).	4 × 6,000 (.9999+)	Agency (H1), persistence in goal striving (H2), and self-esteem (H3) will be regressed in separate models on (i) subjective SES or (ii) education, each time with the original covariates (G, A). [†]	I2
S4. Narcissism	The higher the social class, the higher the narcissism (H5).	4 × 6,000 (.9999+)	Narcissism will be regressed on subjective SES, with the original covariates (G, A).	I1
S5/6. Entitlement	The higher the social class, the higher the entitlement (H6).	4 × 6,000 (.9999+)	Psychological entitlement will be regressed on subjective SES, with the original covariates (G, Et).	I1
S7. System justification and entitlement	When system justification beliefs (SJB) are high, the higher the social class, the higher the entitlement; when SJB are low, this relation is attenuated (H7).	4 × 6,000 (.9999+)	Entitlement will be regressed on occupation, SJB, and their interaction.	I1
S8a. Interdependent self-construal	The higher the social class, the less interdependent the self-construal (H8).	4 × 6,000 (.9999+)	The difference between the mean ratings given to interdependent and independent statements will be regressed on education.	I1
S9. Negative reactions to reduced individuation	When presented with a hypothetical scenario of a friend purchasing the same car, the higher the social class, the more negative the response (H9).	4 × 6,000 (.9999 ³ +))	The responses to three close-ended questions measuring feeling (0 = “bad” and 1 = “good”) will be regressed (in separate logistic models) on education, with and without the original covariate (Et).	I2
S8b. Individuals in one’s inner circle and social support received	The higher the social class, the lower the proportion of individuals in one’s network belonging in the inner circle (vs. the middle and outer circle) (H10) and the lower the proportion of social support received (vs. social annoyances experienced) (H11).	4 × 1,500 (.9731)	The proportion of members in one’s inner circle compared to one’s middle and outer circle will be regressed on education (H10); the proportion of members showing more social support relative to annoyances will be regressed on education (H11).	I2
S10. Chaos and communal orientation	Among participants expecting future chaos, the higher the social class, the lower the communal orientation; for participants expecting stability, this relation is attenuated (H12).	4 × 6,000 (.9999+)	Communal orientation will be regressed on income, expectation of chaos (-0.5 = “Stable” and +0.5 = “Chaotic”), and their interaction, with the original covariates (Et, Chaos × Et).	I1
S11. Chaos and obsession for money	Among participants expecting future chaos, the higher the social class, the higher the obsession for money; for participants expecting stability, this relation is attenuated (H13).	4 × 6,000 (.9999+)	Obsession for money will be regressed on subjective SES, expectation of chaos (-0.5 = “Stable” and +0.5 = “Chaotic”), and their interaction, with the original covariates (Et and Chaos × Et).	I1

Question	Hypothesis	N (power)	Analysis plan	Interpretation
S12. Education bias and ethnic/national bias	The higher-educated group is evaluated more positively than the lower-educated group (Education Bias) (H14). The higher the social class, the higher the education bias (H15). The size of education bias is not smaller than the size of the ethnic/national bias (ethnic/national ingroup evaluated more positively than outgroups) (H16).	4 × 6,000 (.9999+)	A paired sample <i>t</i> -test will be performed on the thermometer ratings of the higher vs. lower-educated group (H14). The difference between the thermometer ratings of the two groups will be regressed on education (H15). An equivalence test will be performed on the difference between the education and ethnic/national bias (using the average of the four ethnic-outgroup ratings) (H16).	I2 + I3
S8c. Thematic/holistic thinking style	The higher the social class, the less thematic/holistic the thinking style (H17).	4 × 6,000 (.9999+)	The proportion of response pertaining to a thematic categorization will be regressed on education.	I1
S8d. Anticipation of change	The higher the social class, the lower the predicted likelihood of change in life trajectories (H18).	4 × 6,000 (.9999+)	The mean percentage of change prediction will be regressed on education.	I1
S13. Sense of control and contextual explanation	The higher the social class, the lower the orientation toward contextual explanations (H19). The total effect of social class on orientation toward contextual explanations is mediated by a decrease in sense of control (H20).	4 × 6,000 (.99992 ² +))	Contextual explanations will be regressed on subjective SES, with and without the original covariates (Ed, I, P) (H19). A mediation analysis with bootstrap percentile CIs will be performed with sense of control as the mediator (same covariates) (H20).	I2
S14. Other-/self-oriented positive emotions	The higher the social class, the lower the other-oriented positive emotions (compassion, love; H21-H22) and the higher the self-oriented positive emotions (contentment, pride; H23-H24).	4 × 6,000 (.9999+ (.9999 ² +))	Compassion, love, contentment, and pride will be individually regressed on income (in separate models), controlling for the other three emotions, with and without the original covariates (G, A, Et, R, P).	I2
E1. Sharing (in)equality & self-conscious negative emotions	When participants receive less resources (from a bogus other), the higher the social class, the lower the self-conscious negative emotions (but not anger or other negative emotions); when they receive equal resources, this relation is attenuated (H25).	4 × 1,500 (.9731)	The difference between the baseline and post-induction self-conscious negative emotions will be regressed on the condition (-0.5 = “low sharing” and +0.5 = “nearly equal”), the original social class measure (education <i>or</i> income), and their interaction.	I2
S15. Influence of contextual emotional information	The higher the social class, the less the rating of a target individual’s emotion is influenced by the emotions of individuals in the background (H26).	4 × 6,000 (.9999+)	The index of contextual influence on emotion ratings will be regressed on subjective SES, with the original covariates (Et, I, Ed).	I1
E2. Self- vs. other-benefits & unethical behaviors	When an unethical behavior benefits the self, the higher the social class, the higher the likelihood of behaving unethically; when the same behavior benefits others, the higher the social class, the lower the likelihood of behaving unethically (H27)	4 × 1,500 (.9731)	Propensity to engage in unethical behavior will be regressed on the beneficiary (-0.5 = “self-” and +0.5 = “other-beneficial”), income, education, income × beneficiary, and education × beneficiary interactions, with and without the original covariates (G, A).	I1

Question	Hypothesis	N (power)	Analysis plan	Interpretation
E3. Public vs. private context & pro-social behavior	When the context is private, the higher the social class, the less pro-social the behavior; when the context is public, the higher the social class, the more pro-social the behavior (H28).	4 × 1,500 (.9731)	The between-condition difference of donated tickets (“public” vs. “private”) will be regressed on each social class measure used in the original (education or income).	I2
S16. Unethical behaviors	The higher the social class, the higher the chance to behave in an unethical way (i.e., to lie; H29)	4 × 6,000 (.9999+)	Percentage of chance one would hide the truth will be regressed on subjective SES, with the original covariates (G, Et, A, R, P).	I1
E4. Mortality vs. control, preference for immediate rewards and risky choices	The lower the childhood SES, the greater the preference for immediate rewards (H30), and the lower the preference for risky choices (H31). These effects should not be different between a mortality and a control prime condition (i.e., no interaction effect; H32-H33).	4 × 6,000 (.9731)	Preference for immediate rewards (H30, H32) will be regressed on childhood SES, the condition (+0.5 = “dangerous world” and -0.5 = “control”), and their interaction, with and without current subjective SES as a covariate. Preference for the risky options will be regressed on the same regressors (H31, H33).	I2
E5. Easy vs. hard financial problems & cognitive performance	When participants think about hard financial problems, the higher the social class, the higher the cognitive performance; when they think about easy financial problems, this relation is attenuated (H34).	4 × 1,500 (.9731)	Performance on three Raven’s matrices will be regressed on the type of financial problems (-0.5 = “easy” and +0.5 = “hard”), income, and their interaction.	I1
S17. Utilitarian moral decision	The higher the social class, the higher the odds of choosing the utilitarian option in the footbridge dilemma (H35).	4 × 6,000 (.9999+)	The odds of pushing the stranger onto the track will be regressed (logistically) on perceived resources availability, with and without the original covariates used in the original study (G, A, Et, R, P).	I1

Notes: Each analysis will be repeated for each country; hypothesis-specific power estimates are given for a given country (for the script, see the OSF page of the project); for multi-items scale, the mean score will be used; when testing an interaction, the predictor will be mean-centered; Education will be contrast-coded: The planned contrast will compare the lowest educational group (coded “-1”), to the highest educational group (coded “1”). The middle educational group will be coded “0” (the weight of the orthogonal contrast will be “-1/3,” “2/3,” “-1/3,” respectively); I1 (for studies involving one test per hypothesis) = “If the effect is significant, the null hypothesis will be rejected, and the result of the study will be interpreted as replicated”; I2 (for studies involving several tests per hypothesis) = “If only some effects are significant, the null hypothesis will be partially rejected, and the results will be interpreted as partially replicated”; I3 (for studies involving a null finding) = “If the effect is significant, the null hypothesis will be rejected; if the effect is nonsignificant, the result of the study will not be interpreted as a failure to replicate”; I4 (for the study involving an equivalence test) = “The smallest effect size of interest will be defined as corresponding to a Cohen’s d of 0.2. If the hypothesis difference between bias is smaller than the lower equivalence bound ($d = -0.2$), the difference will be deemed equivalent, and the result of the original study will be interpreted as replicated”; G = Gender, A = Age, I = income, Ed = education, Et = Ethnicity (only in the U.S. sample), P = political orientation, R = religion. †We will repeat the analysis using the self-orientation composite index (comprising agency, persistence in goal striving, and self-esteem) used by the authors. In the case of a null hypothesized effect, we will use equivalence testing, comparing the effect size to the smallest negative ($f = -0.05$) and positive ($f = 0.05$) effect size of interest using one-sided postestimation Wald tests.

Table 3. Design Table: Tertiary Purpose)

Question	Hypothesis	N (power)	Analysis plan	Interpretation
The moderator increases the strength of the effect of social class:				
S1/2 ...on sense of control	The higher the social class, the higher the sense of control. This effect should be stronger when the moderator is high rather than low (H1').	24,000 (.9999+)	Sense of control will be regressed on each nine measures of social class, the moderator , and their interaction in nine separate models (for this and the subsequent model).	TI1
S3 ...on agency	The higher the social class, the higher the agency. This effect should be stronger when the moderator is high rather than low (H2').	24,000 (.9999+)	Agency will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S3 ...on persistence in goal striving	The higher the social class, the higher the persistence in goal striving. This effect should be stronger when the moderator is high rather than low (H3').	24,000 (.9999+)	Persistence in goal striving will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S3 ...on self-esteem	The higher the social class, the higher the self-esteem. This effect should be stronger when the moderator is high rather than low (H4').	24,000 (.9999+)	Self-esteem will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S4 ...on narcissism	The higher the social class, the higher narcissism. This effect should be stronger when the moderator is high rather than low (H5').	24,000 (.9999+)	Narcissism will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S5/6 ...on entitlement	The higher the social class, the higher the entitlement. This effect should be stronger when the moderator is high rather than low (H6').	24,000 (.9999+)	Entitlement will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S8a ...on interdependent self-construal	The higher the social class, the less interdependent the self-construal. This effect should be stronger when the moderator is high rather than low (H8').	24,000 (.9999+)	The difference between the mean ratings given to interdependent and independent statements will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S9 ...on negative reactions to reduced individuation	When presented with a hypothetical scenario of a friend purchasing the same car, the higher the social class, the more negative the response. This effect should be stronger when the moderator is high rather than low (H9').	24,000 (.9999+)	The responses to three close-ended questions measuring feeling (0 = "bad" and 1 = "good") will be regressed (in separate logistic models) on each nine measures of social class, the moderator , and their interaction.	TI1

Question	Hypothesis	N (power)	Analysis plan	Interpretation
The moderator increases the strength of the effect of social class				
S8b ...on individuals in one's inner circle	The higher the social class, the lower the proportion of individuals in one's network belonging in the inner circle (vs. the middle and outer circle). This effect should be stronger when the moderator is high rather than low (H10').	6,000 (.97239+)	The proportion of members in one's inner circle compared to one's middle and outer circle will be regressed on education, the moderator , and their interaction	TI1
S8b ...on social support received	The higher the social class, the lower the proportion of social support received (vs. social annoyances experienced). These effects should be stronger when the moderator is high rather than low (H11').	6,000 (.97239+)	The proportion of members showing more social support relative to annoyances will be regressed on each nine measures of social class, the moderator , and their interaction	TI1
S12 ...on education bias	The higher the social class, the higher the education bias. This effect should be stronger when the moderator is high rather than low (H12').	24,000 (.9999+)	The difference between the thermometer ratings of the higher vs. lower-educated group will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S8c ...on thematic/holistic thinking style	The higher the social class, the less thematic/holistic the thinking style. This effect should be stronger when the moderator is high rather than low (H13').	24,000 (.9999+)	The proportion of response pertaining to a thematic categorization will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S8d ...on anticipation of change	The higher the social class, the lower the predicted likelihood of change in life trajectories. This effect should be stronger when the moderator is high rather than low (H14').	24,000 (.9999+)	The mean percentage of change prediction will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S13 ...on contextual explanation	The higher the social class, the lower the orientation toward contextual explanations. This effect should be stronger when the moderator is high rather than low (H15').	24,000 (.9999+)	Contextual explanations will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S14 ...on other-oriented positive emotions	The higher the social class, the lower the tendency to experience other-oriented positive emotions (compassion, love). This effect should be stronger when the moderator is high rather than low (H16'-17').	24,000 (.9999+)	Compassion and love will be individually regressed on each nine measures of social class, the moderator , and their interaction, controlling for the other three emotions.	TI1

Question	Hypothesis	N (power)	Analysis plan	Interpretation	
The moderator					
increases the strength of the effect of social class					
S14	...on self-oriented positive emotions	The higher the social class, the higher the tendency to experience self-oriented positive emotions (contentment, pride). This effect should be stronger when the moderator is high rather than low (H18'-19').	24,000 (.9999+)	Contentment and pride will be individually regressed on each nine measures of social class, the moderator , and their interaction, controlling for the other three emotions.	TI1
S15	...on influence of contextual emotional information	The higher the social class, the less the rating of a target individual's emotion is influenced by the emotions of individuals in the background. This effect should be stronger when the moderator is high rather than low (H20').	24,000 (.9999+)	The index of contextual influence on emotion ratings will be regressed on each nine measures of social class, the moderator , and their interaction.	TI1
S16	...on unethical behaviors	The higher the social class, the higher the chance to behave in an unethical way (i.e., to lie). This effect should be stronger when the moderator is high rather than low (H21').	24,000 (.9999+)	Percentage of chance one would hide the truth will be regressed on each nine measures of social class, the moderator , and the interaction.	TI1
E4	...on preference for immediate rewards	The lower the social class, the greater the preference for immediate rewards. This effect should be stronger when the moderator is high rather than low (H22').	6,000 (.97239+)	Preference for immediate rewards will be regressed on each nine measures of social class, the moderator , and the interaction.	TI1
E4	...on risky choices	The lower the social class, the lower the preference for risky choices. This effect should be stronger when the moderator is high rather than low (H23').	6,000 (.97239+)	Preference for the risky options will be regressed on each nine measures of social class, the moderator , and the interaction.	TI1
S17	...on utilitarian moral decision	The higher the social class, the higher the odds of choosing the utilitarian option in the footbridge dilemma. This effect should be stronger when the moderator is high rather than low (H24').	24,000 (.9999+)	The odds of pushing the stranger onto the track will be regressed (logistically) on each nine measures of social class, the moderator , and the interaction.	TI1

Notes: “**The moderator**” stands for “social class identification,” “system justification beliefs,” or “local income inequality”; each analysis will be performed on the pooled country-specific samples, with country-based dummies (i.e., fixed-effects regression) and without control variables (unless otherwise noted); power estimates to detect a small-sized attenuation interaction ($f^2 = .0025$) are given for the pooled sample (for the script, see the OSF page of the project); for multi-items scale, the mean score will be used; the predictor and the moderator will be mean-centered; education will be contrast-coded: The planned contrast will compare the lowest educational group (coded “-1”), to the highest educational group (coded “1”). The middle educational group will be coded “0” (the weight of the orthogonal contrast will be “-1/3,” “2/3,” “-1/3,” respectively); TI1 = Moderation will be supported by a significant interaction and simple effects analyses indicating that the relationship between social class and the outcome is stronger when the moderator is high rather than low.