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nature

LESS IS MORE

Why subtractive changes require more thought

News & views

Psychology

Adding to our problems

Tom Meyvis & Heeyoung Yoon

A series of problem-solving experiments reveal that people are more likely to consider solutions that add features than solutions that remove them, even when removing features is more efficient. **See p.258**

Consider the Lego structure depicted in Figure 1, in which a figurine is placed under a roof supported by a single pillar at one corner. How would you change this structure so that you could put a masonry brick on top of it without crushing the figurine, bearing in mind that each block added costs 10 cents? If you are like most participants in a study reported on page 258 by Adams *et al.*[1], you would add pillars to better support the roof. But a simpler (and cheaper) solution would be to remove the existing pillar, and let the roof simply rest on the base. Across a series of similar experiments, the authors observe that people consistently consider changes that add components over those that subtract them — a tendency that has broad implications for everyday decision-making.

For example, Adams and colleagues analysed archival data and observed that, when an incoming university president requested suggestions for changes that would allow the university to better serve its students and community, only 11% of the responses involved removing an existing regulation, practice or programme. Similarly, when the authors asked study participants to make a 10 × 10 grid of green and white boxes symmetrical, participants often added green boxes to the emptier half of the grid rather than removing them from the fuller half, even when doing the latter would have been more efficient.

Adams *et al.* demonstrated that the reason their participants offered so few subtractive solutions is not because they didn't recognize the value of those solutions, but because they failed to consider them. Indeed, when instructions explicitly mentioned the possibility of subtractive solutions, or when participants had more opportunity to think or practise, the likelihood of offering subtractive solutions increased. It thus seems that people are prone to apply a 'what can we add here?' heuristic (a default strategy to simplify and speed up decision-making). This heuristic can be overcome by exerting extra cognitive effort to consider other, less-intuitive solutions.

Whereas the authors focused on participants' failure to even consider subtractive solutions, we propose that the bias towards additive solutions might be further compounded by the fact that subtractive solutions are also less likely to be appreciated. People might expect to receive less credit for subtractive solutions than for additive ones. A proposal to get rid of something might feel less creative than would coming up with something new to add, and it could also have negative social or political consequences — suggesting that an academic department be disbanded might not be appreciated by those who work in it, for instance. Moreover, people could assume that existing features are there for a reason, and so looking for additions would be more effective. Finally, sunk-cost bias (a tendency to continue an endeavour once an investment in money, effort or time has been made) and waste aversion could lead people to shy away from removing existing features[2], particularly if those features took effort to create in the first place.

These perceived disadvantages of subtractive solutions might encourage people to routinely seek out additive ones. This is consistent with Adams and colleagues' suggestion that frequent previous exposure to additive solutions has made them more cognitively accessible, and thus more likely to be considered. However, in addition, we posit that previous experience could lead people to assume that they are actually expected to add rather than subtract. As a result, the study's participants might be generalizing from past experiences and instinctively assume that they should

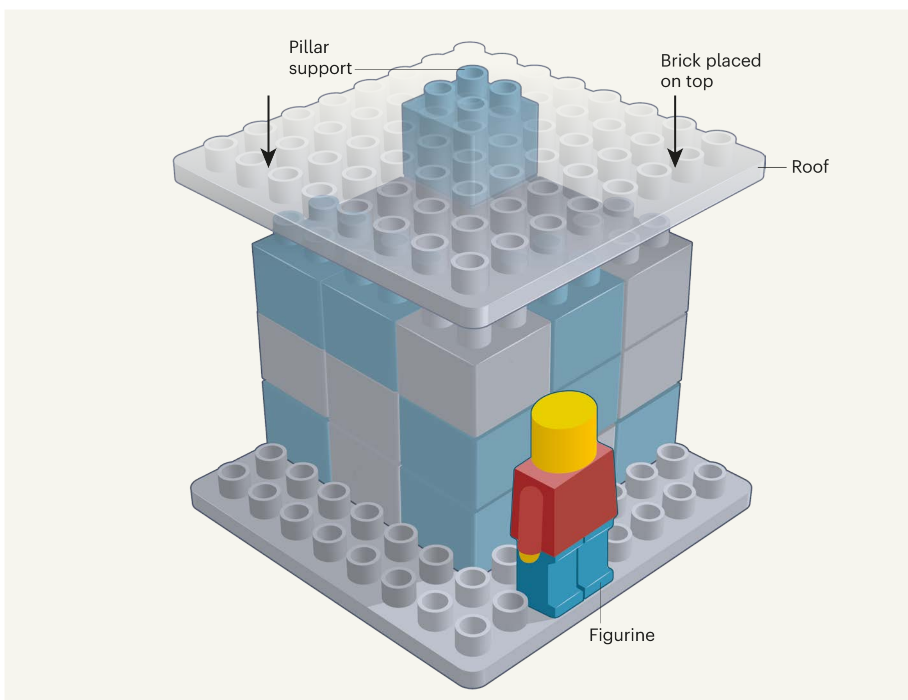

Figure 1 | Improving the stability of a Lego structure. In this structure, a roof is supported by a pillar at one corner of a building. When a brick is placed on top, the roof will collapse onto the figurine. Adams *et al.*[1] asked study participants to stabilize the structure so that it would support the brick above the figurine, and analysed the ways in which participants solved the problem. (Figure adapted from Extended Data Figure 2 of ref. 1.)

add features, only revisiting this assumption after further reflection or explicit prompting. Similarly, members of a university community might implicitly assume that the incoming president wants them to formulate new initiatives, not criticize existing ones.

What are the implications of Adams and colleagues' findings? There are many real-world consequences of failing to consider that situations can often be improved by removing rather than adding. For instance, when people feel dissatisfied with the decor of their home, they might address the situation by going on a spending spree and acquiring more furniture — even if it would be equally effective to get rid of a cluttering coffee table. Such a tendency might be particularly pronounced for resource-deprived consumers, who tend to be particularly focused on acquiring material goods[3]. This not only harms those consumers' financial situations, but also increases the strain on our environment. On a grander scale, the favouring of additive solutions by individual decision-makers might contribute to problematic societal phenomena, such as the increasing expansion of formal organizations[4] and the near-universal, but environmentally unsustainable, quest for economic growth[5].

Adams and colleagues' work points to a way of avoiding these pitfalls in the future — policymakers and organizational leaders could explicitly solicit and value proposals that reduce rather than add. For instance, the university president could specify that recommendations to remove committees or policies are both expected and appreciated. In addition, both individuals and institutions could take self-control measures to guard against the default tendency to add. Consumers could minimize their storage space to restrain their purchases, and organizations could specify sunset clauses that trigger the automatic shutdown of initiatives that fail to meet specific goals.

Of note, it is unlikely that a bias towards addition will always apply. In some situations, it should arguably be easier to generate subtractive changes, because those do not require imagining something that isn't already there. Indeed, when people imagine how a situation could have turned out differently, they are more likely to do so by undoing an action they've taken rather than by adding an action they failed to take[6]. Going forwards, it would be worth exploring when our readiness to imagine removing events extends to imagining removing features, thereby helping us to solve problems through subtraction.

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Quantum information

Quantum computer based on shuttling trapped ions

Winfried K. Hensinger

A microchip-based quantum computer has been built incorporating an architecture in which calculations are carried out by shuttling atomic ions. The device exhibits excellent performance and potential for scaling up. **See p.209**

Quantum computing based on trapped atomic ions has already proved itself to be a leading hardware platform for quantum information processing. Indeed, trapped ions have been used to realize quantum gates — the basic building blocks of a quantum computer — that have the smallest quantum-computation errors of any hardware platform[1,2]. The approach also stands out because it could allow practical machines to be built that do not require cooling to ultra-low (millikelvin) temperatures. However, there have been few comprehensive demonstrations of quantum-computing architectures capable of being scaled up to thousands of quantum bits (qubits). On page 209, Pino *et al.*[3] report the construction and operation of a prototype microchip-based, trapped-ion quantum computer that incorporates a promising architecture based on ion shuttling.

The concept of quantum computing relies on the strange phenomena of quantum physics, the counter-intuitive predictions of which Albert Einstein referred to as spooky. Quantum computers promise to perform calculations in hours or even minutes that might take millions of years to run on the fastest conventional supercomputer. Full-scale quantum computers containing millions of qubits would have transformative uses in nearly every industry, from simulating chemical reactions and helping to develop pharmaceuticals to disruptive

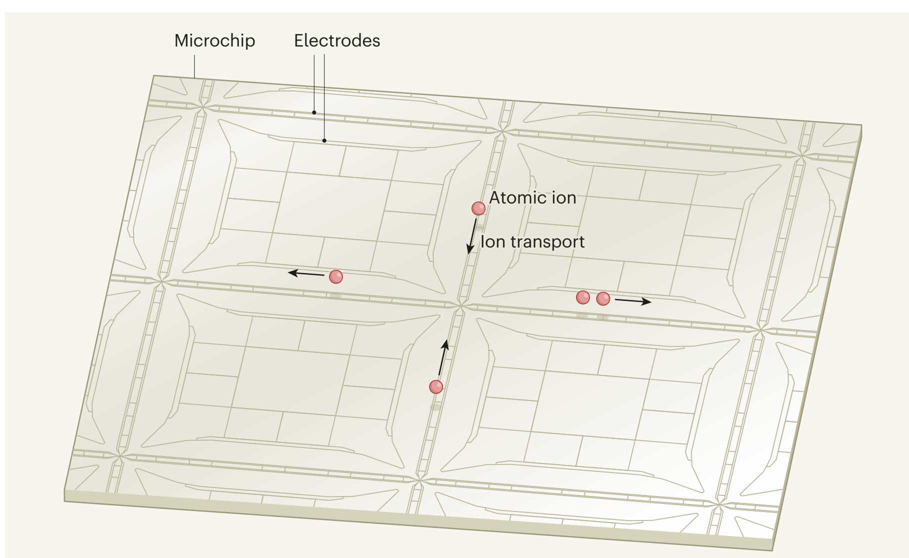

Figure 1 | Quantum-computing architecture based on ion shuttling. In a computing platform known as the quantum charge-coupled device (CCD) architecture, atomic ions hover above the surface of a microchip. These ions are transported along tracks by changing the voltages applied to electrodes (grey lines not in tracks) located on the chip's surface. Quantum computations consist of a sequence of such ion-transport operations interleaved with other operations called quantum gates (not shown). Pino *et al.*[3] built a quantum computer according to this quantum-CCD design.

Article

People systematically overlook subtractive changes

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Improving objects, ideas or situations—whether a designer seeks to advance technology, a writer seeks to strengthen an argument or a manager seeks to encourage desired behaviour—requires a mental search for possible changes[1–3]. We investigated whether people are as likely to consider changes that subtract components from an object, idea or situation as they are to consider changes that add new components. People typically consider a limited number of promising ideas in order to manage the cognitive burden of searching through all possible ideas, but this can lead them to accept adequate solutions without considering potentially superior alternatives[4–10]. Here we show that people systematically default to searching for additive transformations, and consequently overlook subtractive transformations. Across eight experiments, participants were less likely to identify advantageous subtractive changes when the task did not (versus did) cue them to consider subtraction, when they had only one opportunity (versus several) to recognize the shortcomings of an additive search strategy or when they were under a higher (versus lower) cognitive load. Defaulting to searches for additive changes may be one reason that people struggle to mitigate overburdened schedules[11], institutional red tape[12] and damaging effects on the planet[13,14].

Transforming an object, idea or situation in a novel way begins as an act of imagination, a process of searching the environment and one's store of knowledge for possible changes[1–3]. The cognitive science of problem-solving describes iterative processes of imagining and evaluating actions and outcomes to determine whether they would produce an improved state. The essential elements of these processes are mental models of the original state, of possible transformations, and of action categories that can produce the transformations. We conceptualize subtraction and addition as action categories that remove components from or add components to the original, respectively. When a transformed state has fewer components than the original (for example, a revision with fewer words or a process with fewer obstacles), we describe it as a subtractive transformation; when a transformed state has more components than the original, we describe it as an additive transformation. These action categories are conceptually distinct from welfare changes[15], motivational orientations[16] and counterfactual reasoning structures[17]. Furthermore, these action categories are not goals in themselves. People might subtract or add to fulfil any number of underlying interests.

Our research explored whether people tend to search for subtractive changes less readily than they search for additive changes. It was inspired by the apparent need for subtractive counsel across fields. Reminders such as 'less is more'[18], 'omit needless words'[19] and 'remove barriers'[20,21] seem to presume that people who are searching for transformations will otherwise overlook or undervalue subtraction as a way to improve objects, ideas or situations.

Given this anecdotal rationale, we began our investigation with observations of people changing objects, ideas and situations ($k = 8$, $n = 1{,}585$) (Extended Data Table 1, Supplementary Information sections 2.1–2.8). For example, in one controlled observation we asked participants to change a series of digital grid patterns to be symmetrical (study S1, described in Supplementary Information section 2.1). Participants could toggle the colour of any box by clicking on it. It took the same amount of effort to subtract marks from the side that had a greater number of coloured boxes as to add marks to the side with fewer coloured boxes. However, of the 91 participants who favoured one of these two approaches, only 18 (20%) favoured subtraction (this 20% differs significantly from the 50% that would be expected if additive and subtractive transformations were equally common (two-sided binomial distribution probability, $P < 0.001$)). In another observation, we examined archival data from a solicitation for improvement ideas by the incoming president of a university (study S2, described in Supplementary Information section 2.2). Of the 651 responses that coders categorized as additive or subtractive, only 70 (11%) were subtractive (this 11% significantly differs from 50% (two-sided binomial distribution probability, $P < 0.001$)).

We found similarly low rates of subtraction among participants who were prompted to transform block structures (12%, 2% and 5% in studies S3, S4 and S5, respectively), essays (17% and 32% in studies S6 and S7) and itineraries (28% in study S8) (all of these differed significantly from 50% (two-sided binomial distribution probability, $P < 0.001$)) (Supplementary Information sections 2.3–2.8). Rates of subtraction

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Table 1 | Rates of subtractive changes by condition for experiments 1 to 8

Experiment[a]	Rates of subtraction by condition		Test statistic	P value (two-tailed)
	Control	Subtraction cue		
1	41% (40/98)	61% (60/99)	$\chi^2 = 7.72$	0.005
2, 3	28% (47/166)	43% (63/146)	$z = 2.73$	0.006
4 (improve)	21% (17/80)	48% (44/91)	$\chi^2 = 13.63$	<0.001
4 (worsen)	28% (26/92)	50% (53/106)	$\chi^2 = 9.71$	0.002
	Control	Repeated search		
5	49% (74/152)	63% (93/147)	$\chi^2 = 5.87$	0.015
	Higher cognitive load	Lower cognitive load		
6 to 8	2.45/4 ($n = 572$)	2.76/4 ($n = 581$)	$z = 2.97$	0.003

Subtraction cues, repeated search opportunities and a lower cognitive load increased subtraction rates. Subtraction rate refers to the percentage of participants who produced a subtractive transformation for experiments 1 and 5; who listed at least one subtractive idea for experiments 2 to 4; and to the average number of trials on which people subtracted for experiments 6 to 8. The total number of participants in each condition is the denominator listed in experiments 1 to 5, and *n* for experiments 6 to 8.

[a]Stimuli for experiments were as follows: changes to a block structure (experiment 1); ideas to make a design better (experiments 2, 3, 4 (improve)); ideas to make a design worse (experiment 4 (worsen)); abstract grid task (experiments 5–8).

were lower than rates of addition, except when we introduced superfluous or anomalous components (for example, grilled cheese with chocolate) in studies S9 and S10 (Extended Data Fig. 1, Extended Data Table 2, Supplementary Information sections 2.9, 2.10). Although we did not randomly sample from transformation problems, we observed this tendency across a range of goals (including 'improve', 'enable' and 'arrange'), across stimuli that varied in familiarity and importance, for originals created by the research team and originals created by other participants, and in well-defined and ill-defined settings[3].

Low rates of subtraction raise two broad possibilities about the thought processes that produce them: people might generate both kinds of ideas and then disproportionately choose additive ones, or they might overlook subtractive ideas altogether. Although both phenomena probably contributed to the observed behavioural outcomes, we focused our subsequent research on potential differences at the idea-generation phase because this phase necessarily precedes explicit choice. We investigated whether people default to an additive search strategy, making them less likely to consider subtraction in the first place.

Heuristic memory searches can help people to efficiently access the right information at the right time, but—as with any mental shortcut—heuristic memory searches can be overapplied, leading people to accept adequate solutions before considering potentially superior alternatives[4–10]. There are cognitive, cultural and socioecological reasons to suspect that people might privilege additive over subtractive changes. First, additive changes may be incrementally easier to process. Any component that can be subtracted must first be understood as part of the artefact before it can be considered as 'not' part of the artefact[22]. Second, over time, additive changes may come to be viewed more positively than subtractive changes. Numerical concepts of 'more' and 'higher' may map to evaluative concepts of 'positive' and 'better'[23]; tangible contributions are culturally valued[24]; and acquiring and displaying resources is fitness-enhancing[25,26]. Third, people might be reluctant to subtract because of attentional and evaluative processes that favour the status quo[27,28]. Finally, these processes operate in an environment that may, probabilistically, offer more good opportunities to add than to subtract: originals require building before honing; the number of components that can be subtracted is always bound by what exists; and in designed environments, one may infrequently encounter artefacts from which the designers have not already subtracted the obviously negative components.

The more frequently that individuals use an additive search strategy with perceived success, the more cognitively accessible this strategy would become for them[6,10,29,30]. Across many domains of judgment, people rely on quick and easy mental shortcuts—especially when high cognitive demands preclude the pursuit of more tailored approaches and in the absence of information that cues alternative strategies[4–10]. Thus, if additive search is a common default, people should be more likely to rely on it when they are cognitively loaded and—conversely—they should be less likely to rely on it when task experience or task information cues them to use another strategy.

Experiments

As in the observational studies, we presented participants with an original object, idea or situation and asked them to change it in some way. Unlike the observational studies, we designed all of the experimental tasks so that participants would have good reason to report or produce subtractive transformations if they thought of them. This design feature inflated baseline rates of subtraction but allowed us to infer that people who did not pursue subtraction probably did not consider it. Differing rates of subtractive transformation across conditions could therefore be attributed to idea generation rather than to choice. To minimize transient influences that might cue a specific search strategy, we used neutral prompts (for example, 'change' or 'improve') rather than connotative additive or subtractive prompts (for example, 'bolster' or 'hone', respectively); and we avoided original artefacts that would appear to participants in our samples to have obvious superfluities (for example, a bacon, lettuce, tomato and peanut butter sandwich) or omissions (for example, a lettuce and tomato sandwich). Within these specifications, each experiment compared conditions in which people's mental searches should depend relatively more versus less on a heuristic search strategy: that is, when a search-expansion cue is absent versus present; when the task provides fewer versus more opportunities to recognize the value of an alternative strategy; or when the participant is under higher versus lower cognitive load. We predicted that subtraction rates would be higher in the latter conditions (see Methods for descriptions of our approach to determining sample sizes, randomization, analysis and reporting for all experiments).

Experiments 1, 2 and 3 manipulated the presence of subtraction cues in the problem-solving environment. Explicitly priming a specific, relevant action category should offset the influence of a heuristic that prioritizes a different category[6,27,29,30]. In a task inspired by sustainable resource challenges, experiment 1 presented participants ($n = 197$) ('Experiment 1' in the Methods) with an implicit choice between using costly new resources or working with what was already there. We offered participants a bonus of one dollar if they could stabilize a Lego structure such that it could hold a masonry brick above a figurine. The original could not hold the brick because its platform was supported in one corner, in a manner similar to a one-legged table (Extended Data Fig. 2). Participants could add new supports (at a cost) or they could remove the existing support (for free), which allowed the platform to sit flush on the layer below. The subtractive transformation therefore maximized their bonus. In the control condition, instructions explicitly mentioned addition ('each piece that you add costs ten cents') but not subtraction. In the subtraction-cue condition, instructions mentioned both ('each piece that you add costs ten cents but removing pieces is free'). Instructions in both conditions stated 'you may alter the structure however you want'. Results from a follow-up study (S11) corroborated the assumption that most participants who thought of subtraction would have recognized its value (Supplementary Information section 2.11.) In the control condition of experiment 1, 41% of participants produced the subtractive transformation; by contrast, in the subtraction-cue condition 61% of participants produced this transformation ($\chi^2 = 7.72$,

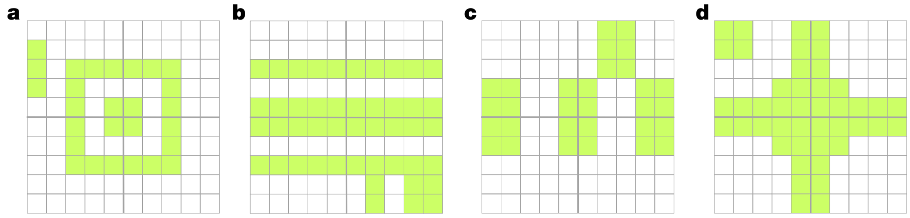

Fig. 1 | Grid task from experiments 5 to 8. a–d, Participants could achieve symmetry by adding to the three empty quadrants (with more clicks) or by subtracting from the marked quadrant (with fewer clicks). In experiment 5, **d** was the critical trial and participants in the repeated search condition completed **a**–**c** as practice. In experiments 6 to 8, all four patterns (**a**–**d**) were critical.

$P = 0.005$, $\varphi = 0.20$) (see Supplementary Information section 1.1 for information regarding preregistration and analysis) (Table 1). In the absence of a subtraction cue, a significantly greater number of participants overlooked the advantageous subtractive transformation.

In experiment 2 and experiment 3 ($n = 147$ and 165, respectively) (described in 'Experiment 2' and 'Experiment 3' in the Methods), we showed participants an illustration of a miniature golf hole (Extended Data Fig. 3), solicited their improvement ideas and coded whether each was additive (for example, 'add a windmill'), subtractive (for example, 'remove the sand trap') or neither (for example, 'reverse the direction'). We designed this stimulus to provide an engaging, novel task for participants[10]. Whereas experiment 1 solicited only a single transformation, experiments 2 and 3 asked participants to list 'all of the different ways that [they] might be able to improve' the original. This minimizes the potential role of evaluation at the choice stage, and offers a view of relative accessibility[27,31]. The no-cue instructions did not mention either addition or subtraction, whereas the cue instructions reminded participants that they could 'add or subtract'. We did not find evidence that the cue affected the likelihood that participants would submit a list with at least one additive idea (odds ratio = 0.92, $z = -0.24$, $P = 0.810$), but we did find evidence that the cue increased the likelihood that participants would submit a list with at least one subtractive idea (odds ratio = 1.93, $z = 2.73$, $P = 0.006$) (Table 1, Extended Data Fig. 4; additional information is provided in Supplementary Information sections 1.2, 1.3 and an alternative analysis is described in Supplementary Information section 1.5). The subtractive part of the cue seems to have brought new options to mind.

Experiment 4 ($n = 369$) (described in 'Experiment 4' in the Methods) investigated whether people overlook subtraction across different transformation goals. We used the task from experiments 2 and 3, crossing the cue manipulation with a goal manipulation that assigned participants to 'improve' the original or to 'make it worse'. The cue increased the percentage of participants who generated at least one subtractive idea within the improvement conditions (no-cue = 21%, cue = 48%, $\chi^2 = 13.63$, $P < 0.001$) and the make-it-worse conditions (no-cue = 28%, cue = 50%, $\chi^2 = 9.71$, $P = 0.002$) (Table 1, Extended Data Fig. 4; the regression details and analysis of additive ideas are given in Supplementary Information section 1.4). These results imply that subtraction neglect is not proximally attributable to norms about how to contribute to improvement efforts or to linguistic associations between 'more' and 'better'.

In contrast to experiments 1 to 4 (which directly manipulated task-specific information), experiments 5 to 8 manipulated how favourable task conditions were for allowing participants to get beyond a heuristic search and potentially activate subtractive ideas on their own. In experiment 5 ($n = 299$) (described in 'Experiment 5' in the Methods), we presented participants with a digital 10 × 10 grid of white and green boxes (a novel stimulus in which the individual components have no inherent value). Participants could click on any box to toggle its colour. Their goal was to make the grid symmetrical from left-to-right and top-to-bottom using the fewest number of clicks. Figure 1 illustrates that the original grid had extraneous filled boxes in one quadrant. Participants could achieve symmetry by adding to the three empty quadrants or by subtracting from the marked quadrant. Unlike the task in study S1, this task had an objectively correct transformation but participants could only recognize the subtractive transformation as correct if they thought of it. In the control condition, participants proceeded immediately to the critical trial (Fig. 1d). In the repeated search condition, participants first completed practice trials on three similar grids (Fig. 1a–c) but received no external feedback on their responses. The repetition merely gave them more opportunities to recognize the shortcomings of an additive approach. Increasing the cumulative probability of an incidental discovery of subtraction during the practice trials should increase the likelihood of a participant using a subtractive search in the critical trial. As predicted, 49% of participants produced the subtractive solution in the control condition, whereas 63% produced it in the repeated search condition ($\chi^2 = 5.87$, $P = 0.015$, $\varphi = 0.15$) (Table 1, Supplementary Information section 1.6). Experiment 5 showed that participants were more likely to produce a superior subtractive transformation when they had more opportunities to recognize the task-specific shortcomings of an additive search strategy.

In experiments 6 to 8 ($n = 1{,}153$) (described in 'Experiment 6' and 'Experiments 7 and 8' in the Methods), we examined whether participants would be less likely to produce a subtractive transformation when they were under cognitive load (a state that is known to increase reliance on cognitive shortcuts[4,22,32,33]). In an adapted version of experiment 5, participants completed four critical trials with no practice trials (Fig. 1a–d). To induce a higher cognitive load, we used a concurrent head-movement task[33] in experiment 6 and a concurrent digit-search task[22,32] in experiments 7 and 8. Meta-analysis of the three experiments indicates that participants failed to identify the subtractive transformation for more puzzles in the higher- versus lower-load condition (Hedge's $g = 0.18$, $z = 2.97$, $P = 0.003$) (Table 1; details and a one-trial version are given in Supplementary Information sections 1.7–1.10). When participants had more attentional resources available, they were more likely to identify a superior subtractive transformation.

Discussion

This Article introduces a basic conceptual distinction between pursuing changes to the physical, intellectual and social world through subtraction or through addition. This distinction links what might otherwise be treated as disparate phenomena in design and problem-solving (changing objects); reasoning, learning and communication (changing ideas); and coordination, decision-making and motivation (changing situations).

Empirically, our research identifies conditions under which people are more or less likely to overlook subtraction. Our experiments showed that the identification of advantageous subtractive changes depends on the presence of cues that prompt subtractive search (experiments 1 to 4), on the number of opportunities one has to recognize the shortcomings of an additive default (experiment 5) and on the situational availability of cognitive resources (experiments 6 to 8).

It is likely that heuristic search does not account for all variation in subtractive and additive transformations. For instance, some transformation tasks may not require the general memory search associated with problem-solving. Dangerous, disgusting or dissonant components would probably cue a specific goal (for example, 'remove the roaches from the apartment') with a clear end state and limited set of means. Task-specific features might also cue a tailored memory search (for example, instructions from an editor to 'shorten'). Furthermore, whenever subtractive and additive ideas do arise, potential biases in choice become another source of variance.

Our findings, which are based on samples of participants in the USA, raise questions about cultural generalizability. Our preliminary study with university students from Germany and Japan suggests that the additive search strategy extends beyond the USA (study S12; details are provided in Extended Data Table 3, Supplementary Information section 2.12). However, additional research is needed to understand culture as a candidate moderator, including the potential contributions of industrialization, resource availability, aesthetic preferences and social norms. Future research that explains variability in subtraction neglect might help to pinpoint its social, cognitive and developmental origins, and—further—to suggest ways to reduce its harmful consequences.

As with many heuristics, it is possible that defaulting to a search for additive ideas often serves its users well[4,5,7]. However, the tendency to overlook subtraction may be implicated in a variety of costly modern trends, including overburdened minds and schedules[11], increasing red tape in institutions[12] and humanity's encroachment on the safe operating conditions for life on Earth[13,14]. If people default to adequate additive transformations—without considering comparable (and sometimes superior) subtractive alternatives—they may be missing opportunities to make their lives more fulfilling, their institutions more effective and their planet more liveable.

Online content

Any methods, additional references, Nature Research reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/s41586-021-03380-y.

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Methods

We report all close replications and meta-analyses conducted across similar experiments. We report all conditions, measures and data exclusions. Supplementary Information section 1 and the Reporting Summary provide detailed dropout information for all studies. Research protocols were approved by the Institutional Review Board at the University of Virginia. Participants consented to participate in all studies. The universities at which we collected data are not named to protect participant anonymity and confidentiality. No deception was used.

Sample size determination and randomization

Sample sizes for each study were determined before data collection (that is, no data were collected for any study after analysis began). For studies that were the first of their design, we used informal rules-of-thumb to determine a target sample size. For subsequent studies that used a similar design, we used observed effect sizes as an informal guide. The exceptions to this approach were experiment 1 (which was preregistered on the basis of a power calculation (https://osf.io/rkqvw/)), study S2 (an archival study) and study S12 (for which we were limited by the number of volunteers). Except for experiment 1, no power analysis was performed. For all experiments, we aimed to recruit participants from separate samples using appropriate identifiers (for example, participant identification number, Amazon Mechanical Turk identification number or IP address) to avoid duplicated responses. In experiment 1, we used a randomizer to make a predetermined randomization schedule; in experiments 2 to 8, we used the randomization feature present in the survey software of Qualtrics.

Data analysis and reporting

Data analysis was conducted in R (v.4.0.2) and SPSS (v.27). All reported *P* values are two-sided. We calculated effect sizes using either Cohen's *d* (using the effsize package in R[34]), a phi correlation coefficient (using the psych package in R[35]), or Cramer's *V* (using jamovi[36] for R) as appropriate. Reported *t*-tests do not assume equal variances and are therefore reported as Welch's *t*-tests, with corrected degrees of freedom[37].

Experimental samples and procedures

Experiment 1. We designed the task for experiment 1 so that participants would earn more money if they pursued a subtractive transformation, such that overlooking subtraction was financially costly to participants (see https://osf.io/rkqvw/ for preregistration of the hypothesis and analysis plan, and Supplementary Information section 1.1 for discussion of a deviation from this plan). We recruited 203 individuals from those passing by a table in a highly trafficked area of a large public university in the USA. Six sessions were excluded for procedural or random-assignment protocol violations, yielding a final sample of $n = 197$ participants. To simplify the study procedure, we did not administer any surveys to participants or collect demographic information. Participants earned candy and the chance of a bonus of up to $1.

Participants were randomly assigned to one of two conditions: control ($n = 98$) or subtraction cue ($n = 99$). Participants saw the Lego structure (Extended Data Fig. 2) and learned from the experimenter (blind to hypothesis) that their task was to change the Lego structure so that it could hold a masonry brick over the head of the figurine without collapsing. The experimenter also demonstrated to the participant how—without any modifications to the Lego structure—the masonry brick would cause a collapse of the structure. The original structure had one Lego brick serving as a pillar that held up the platform for the masonry brick, with three other pillars missing (similar to a one-legged table). Participants could transform the Lego structure and satisfy the goal of the task by adding supports (for example, using Lego bricks to provide the three 'missing' legs or adding an entire layer of Lego bricks). Alternatively, participants could transform the Lego structure by removing the corner Lego brick and allowing the platform to rest stably on the solid layer of Lego bricks below.

The experimenter said to all participants 'You will earn $1 if you successfully complete this task. Each piece that you add costs ten cents'. For those participants randomly assigned to the subtraction-cue condition, the experimenter added 'but removing pieces is free and costs nothing'. In both conditions, the experimenter then affirmed 'You may alter the structure however you want, and you have as much time as you want. Please let me know when you are done'. Participants then received a written copy of the instructions that the experimenter had delivered (corresponding to their condition), and the experimenter left them alone in a semi-private area to complete the task. When the participant finished, the experimenter recorded the number of Lego bricks that were added, removed and moved, and took a photograph of the transformed structure for record-keeping.

We designed study S11 as a follow-up experiment to assess participants' interpretation of the experiment 1 instructions and—by implication—the plausibility of a choice-based alternative explanation (Supplementary Information section 2.11).

Experiment 2. In the task for experiment 2, participants generated ideas for improving a miniature golf course hole. In this task, participants sequentially listed all of their change ideas (rather than producing only a single transformation). This allowed observation of not only how many people have subtractive ideas, but also the order in which additive and subtractive ideas emerge[27,31]. We recruited on Amazon Mechanical Turk and received 147 valid responses (83 men, 63 women and 1 unspecified; $M_{age} = 34.7$, s.d. = 10.1 (Supplementary Information section 1.2)). Participants received $0.60 for participating.

Participants were randomly assigned to one of two conditions: no cue ($n = 79$) or cue ($n = 68$). All participants read a vignette asking them to imagine themselves as the assistant manager of a miniature golf course. The vignette showed an image of a miniature golf hole (Extended Data Fig. 3) and asked participants to 'make a list of all the different ways that you might be able to improve the hole without spending a ton of money'.

Advising against 'spending a ton of money' was one way that the vignette subtly encouraged all participants to consider subtractive actions. Moreover, we designed the original hole to include opportunities for subtractive transformations. For example, participants who wanted to transform the hole to be more challenging could remove the corner bumper that players might use to carom around the corner; and participants who wanted to transform the hole to be easier could remove the sand trap.

By random assignment, the vignette included a cue to participants in the appropriate condition: 'Keep in mind that you could potentially add things to the hole as well as take them away'. Participants in both conditions then advanced to a screen with space in the survey interface for up to 24 ideas. At the top of this response-elicitation screen, all participants again saw the prompt to improve the hole and—for those in the cue condition—a second cue that 'you could add or subtract from the current design'. If the cue to 'add or subtract' increased either category of idea, it would suggest that the cue brought new ideas to mind. Because the cue suggests both adding and subtracting, it mitigates any demand effects, in which participants respond according to what they perceive to be the experimenters' desire.

Following the task, all participants provided demographic information and responded to a manipulation check that asked them to think back to the instructions they read and to indicate whether they had been explicitly instructed that they could add and also subtract, with three response options: 'yes', 'no' and 'don't recall'.

Two research assistants, who were blind to condition and hypothesis, independently coded the responses of the participants as one of the following: additive (that is, changes the hole by adding elements or resources), subtractive (that is, changes the hole by removing elements or resources), change (that is, alters some element of the hole without adding or removing anything) or an invalid response (that is, empty or incoherent answer). The coders achieved a high reliability (overall Cohen's $\kappa = 0.68$), and disagreements were resolved through discussion.

Experiment 3. Experiment 3 was a close replication of experiment 2. We recruited on Amazon Mechanical Turk and received 165 valid responses (after excluding 13 participants who failed an attention check (Supplementary Information section 1.3); 90 men, 74 women, 1 unspecified; $M_{age} = 35.7$, s.d. = 9.44). Participants received \$0.60 for participating.

Participants were randomly assigned to one of two conditions: no cue ($n = 87$) or cue ($n = 78$). The procedure was similar to experiment 2, but in experiment 3 instructions asked participants to imagine that they were friends with 'Mr. Popo', the owner of another miniature golf course and that 'For as many summers as you can remember, you and Popo have quietly traded favors. One of you will sneak onto the other's course late at night and change around one of the holes to make it better. The goal of the favor is to make the hole better, but without completely renovating the hole in a way that would attract the other owner's attention'.

As in experiment 2, instructions were identical for the no cue and cue conditions, except that participants in the cue condition also saw a first cue above the hole design ('Keep in mind that you can add things to the hole or take them away') as well as a second cue that was displayed above the response boxes (and this time appeared in red-coloured font) and stated 'Remember that you can add or subtract from the current design'. Next, we administered an attention check. We asked participants to write the word 'pirate' as part of instructions embedded within a larger block of text that ostensibly instructed participants to describe the changes they made.

Three research assistants, who were blind to condition and hypothesis, coded the responses of participants using the same categories as in experiment 2. We used Krippendorff's α in this study to accommodate more than two coders rating categorical responses ($\alpha = 0.86$). They resolved disagreements through discussion.

Experiment 4. Experiment 4 tested whether people overlook subtraction across different types of transformation goal (specifically when trying to make something better versus worse). The procedure in experiment 4 was similar to experiments 2 and 3, in which participants modified a miniature golf hole. Participants were randomly assigned to read a cue stating that they could add or subtract. Experiment 4 additionally manipulated whether the goal was to make the hole better or worse, producing the following design: 2 (cue: present versus absent) × 2 (goal: make better versus make worse). We recruited on Amazon Mechanical Turk and received 369 valid responses (after excluding 27 participants who failed an attention check (Supplementary Information section 1.4); 181 men, 183 women, 5 unspecified; $M_{age} = 37.00$, s.d. = 11.73). Participants received \$0.60 for participating.

Participants were randomly assigned to one of four conditions: make-better and no-cue ($n = 80$), make-better and cue ($n = 91$), make-worse and no-cue ($n = 92$) or make-worse and cue ($n = 106$). As in experiment 3, participants read a scenario in which they run a miniature golf course and Mr. Popo runs one in the neighbouring town. In the make-better conditions, Mr. Popo was described as a close friend and participants learned that, for many summers, they and Popo had quietly traded favours. Their goal was to make the hole better without completely renovating the hole in a way that would attract the attention of the owner. Participants in the make-worse condition received the same instructions, except that Mr. Popo was described as a friendly rival and participants learned that, for many summers, they had quietly traded good-natured pranks. Their goal was to make the hole worse without completely ruining the hole in a way that would attract the attention of the owner. We included the detail about not drawing too much attention to the change to pre-empt extreme response (especially in the make-worse condition) that would fundamentally alter the hole. Crossed with this goal manipulation, half of the participants were randomly assigned to also read a cue at the bottom of the instructions ('Keep in mind that you can add things to the hole or take them away'), whereas the other half did not see this cue. On the next page of the task, all participants saw the diagram (the same as experiments 2 and 3) (Extended Data Fig. 3), with either the question 'What ideas do you have for how to improve Hole #6?' or the question 'What ideas do you have for how to make Hole #6 worse?'. Participants in the cue conditions additionally saw (in red) the statement 'Remember that you can add or subtract from the current design'. Participants could write up to 24 ideas. All participants received the same attention check used in experiment 3.

Three independent coders, who were blind to condition and hypothesis, coded responses using the same categories as in the previous experiments (overall reliability, Krippendorff's $\alpha = 0.87$), with disagreements being resolved through discussion.

Experiment 5. In experiment 5, we instructed participants to transform a digital grid pattern. We designed each original pattern so that it would be incorrectly transformed if participants thought only of additive actions but could be correctly transformed if participants thought of subtractive actions. We recruited on Amazon Mechanical Turk and received 299 valid responses (170 men, 126 women, 3 unspecified, $M_{age} = 34.99$, s.d. = 9.27 (Supplementary Information section 1.6)). Participants received \$0.60 for participating.

Participants were randomly assigned to one of two conditions: control ($n = 152$) or repeated search ($n = 147$). Participants learned that they would work on a set of patterns, and read 'First, please take a moment to familiarize yourself with the workspace. Click below to see how you can change colors on the grid'. On this orientation screen, participants could interact with a 10 × 10 grid that functioned in the same way as the grid(s) in the subsequent trial(s). Half of the blocks for the grid on the orientation screen were already green (the top left and bottom right quadrants), and the other blocks were white (top right and bottom left quadrants). Participants could click any one of the squares to see how it would change the colour.

Participants then learned that their focal task was to change a forthcoming pattern 'so that it is perfectly symmetrical *from left to right*, and *from top to bottom*'. Participants also read 'Obviously, there are many technically correct solutions to this puzzle. However, your challenge is to make it symmetrical using the fewest possible mouse clicks'. The phrase 'using the fewest possible mouse clicks' was displayed in bold font. We designed each grid so that making it symmetrical with an additive transformation would require more clicks than would making it symmetrical with a subtractive transformation. Participants could therefore not achieve the correct response without thinking of subtractive actions.

Participants in the control condition then proceeded straight to the main task, whereas participants in the repeated search condition learned that they would first 'run through 3 practice trials' before completing the main task. In the repeated search condition, participants advanced through three grids, each labelled as 'Practice Trial' 1, 2 or 3 (Fig. 1a–c). Critically, participants received no feedback about their performance. In both conditions, the critical trial (the dependent variable) was labelled 'Main Task' (Fig. 1d).

Experiment 6. In experiment 6, we instructed participants to transform the same series of digital grid patterns used in experiment 5. We recruited on Amazon Mechanical Turk and received 299 valid responses (170 men, 128 women, 1 unspecified; $M_{age} = 35.03$, s.d. = 9.68 (Supplementary Information section 1.7)). Participants received \$0.60 for participating.

Participants were randomly assigned to one of two conditions: lower cognitive load ($n = 151$) or higher cognitive load ($n = 148$). After participants learned the basic rules and aim of the task (which were the same as experiment 5), we introduced the cognitive load manipulation.

Participants randomly assigned to the lower-load condition were instructed to complete the grid tasks while sitting naturally. These instructions included an illustration of a forward-facing head. Participants in the higher-load condition were instructed to complete the grid tasks while moving their head around in a circle, focusing on their chin. These instructions included an illustration of a forward-facing head with arrows to indicate circular movements[33].

Experiments 7 and 8. Experiments 7 and 8 provided a conceptual replication of experiment 6, using a university-based participant pool and a different concurrent-task manipulation[22,32]. Experiment 7 took place in a laboratory setting. We recruited 286 undergraduates enrolled in introductory psychology courses (88 men, 197 women, 1 unspecified; $M_{age} = 18.82$, s.d. = 1.61; no exclusions were necessary (Supplementary Information section 1.8)).

Participants were randomly assigned to one of two conditions: lower load ($n = 140$) or higher load ($n = 146$). All participants first interacted with the grid workspace and read instructions for the task. Then, all participants learned 'During this task, numbers will be scrolling by'. In the higher-load condition, participants read 'As you are working on making the grid symmetrical, keep an eye on the number stream. Every time you see a '5' enter the screen, press the 'f' key on your keyboard. The computer will record the accuracy of your responses'. In the lower-load condition, participants read 'As you are working on making the grid symmetrical, you can just ignore these'. The digit streams contained approximately 20% '5' digits and scrolled at a rate of approximately 4 digits per second, with up to approximately 36 digits on the screen at any given time.

Participants then viewed the four grids in Fig. 1, one at a time in randomized order. Each grid was arranged such that one quadrant of the grid contained extra marks. As in experiment 6, participants could achieve symmetry by adding corresponding marks to the other three quadrants or by removing the extra marks, with the latter approach requiring fewer clicks. Following the task, participants responded to the manipulation check question 'How difficult was this task?' on a scale from 1 = not at all difficult to 7 = extremely difficult.

Experiment 8 directly replicated experiment 7 using a larger sample and online administration. Procedures were identical to experiment 7, except that participants completed the study online instead of in person. We recruited 600 undergraduate students from a large public university in the USA to respond to an online study for partial course credit. We received 568 valid responses (after excluding 17 duplicates, 12 for not responding to all four grid tasks, and 3 for making no changes to the grids (Supplementary Information section 1.9); 200 men, 367 women, 1 unspecified, $M_{age} = 18.84$, s.d. = 1.67). As in experiment 7, participants were randomly assigned to either a lower-load condition ($n = 290$) or a higher-load condition ($n = 278$).

Reporting summary
Further information on research design is available in the Nature Research Reporting Summary linked to this paper.

Data availability
All data and materials are available on the Open Science Framework at https://osf.io/7v6r2/. The coded responses from study S2 are posted per our agreement with the organization that supplied the data. Open-ended responses are available from the authors upon reasonable request and with permission of the organization.

Code availability
All R code used to produce analyses for all studies is available on the Open Science Framework at https://osf.io/7v6r2/.

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Author contributions As all authors contributed equally to this work, they are listed alphabetically in the front matter. G.S.A. and L.E.K. conceived the initial question; G.S.A., B.A.C., A.H.H. and L.E.K. developed the hypothesis, designed studies and oversaw data collection for all studies; G.S.A. and A.H.H. analysed data; B.A.C. wrote the first draft with input from G.S.A., A.H.H. and L.E.K. A.H.H. and G.S.A. wrote the Supplementary Information with input from B.A.C. and L.E.K.

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Additional information
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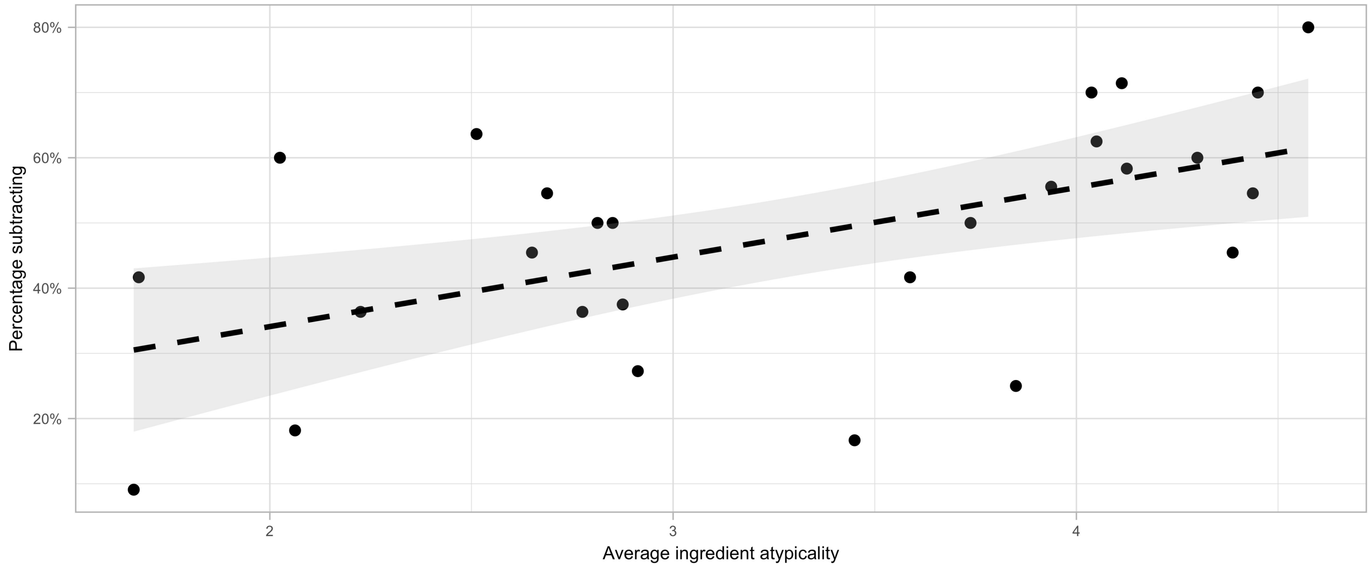
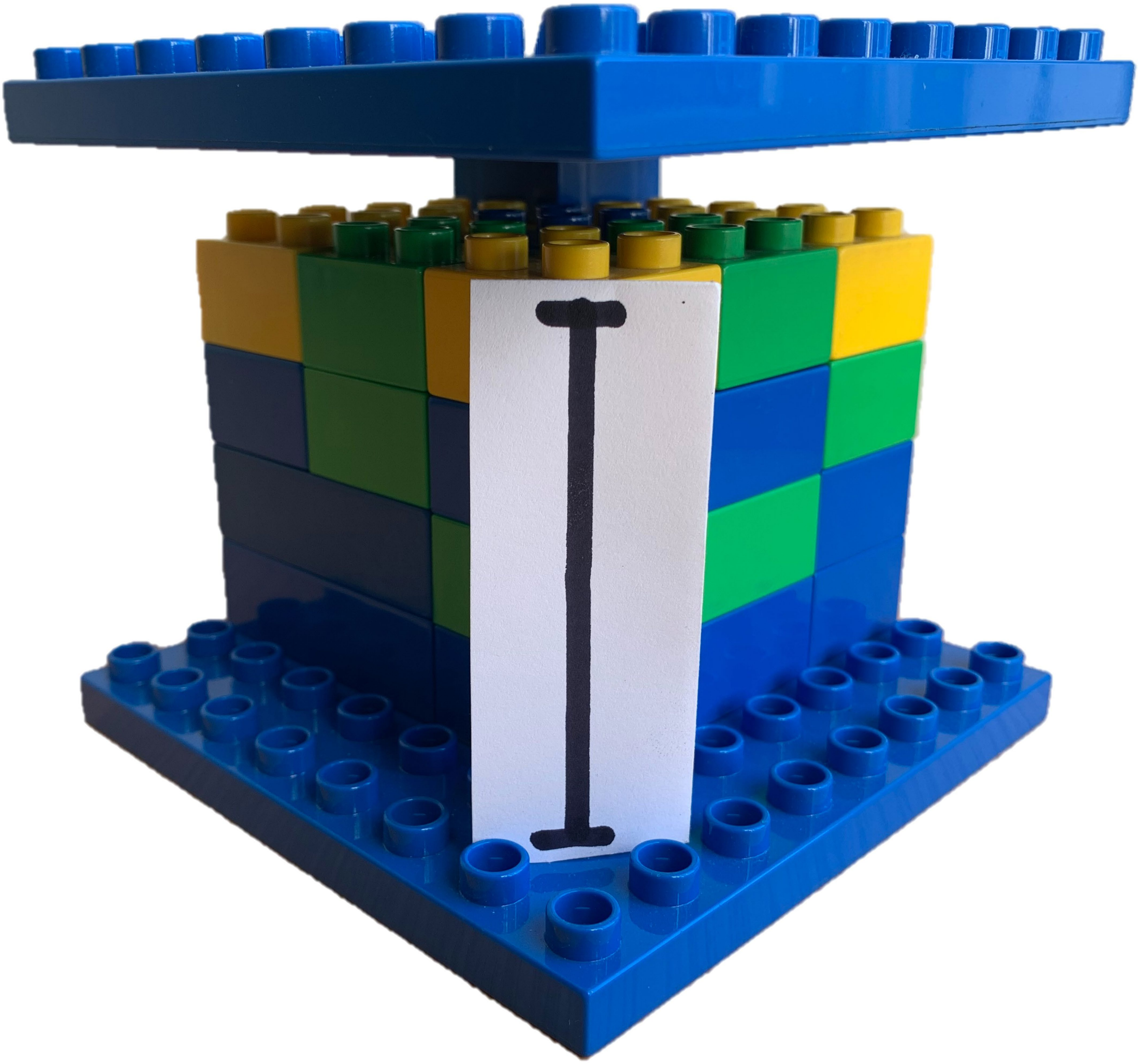
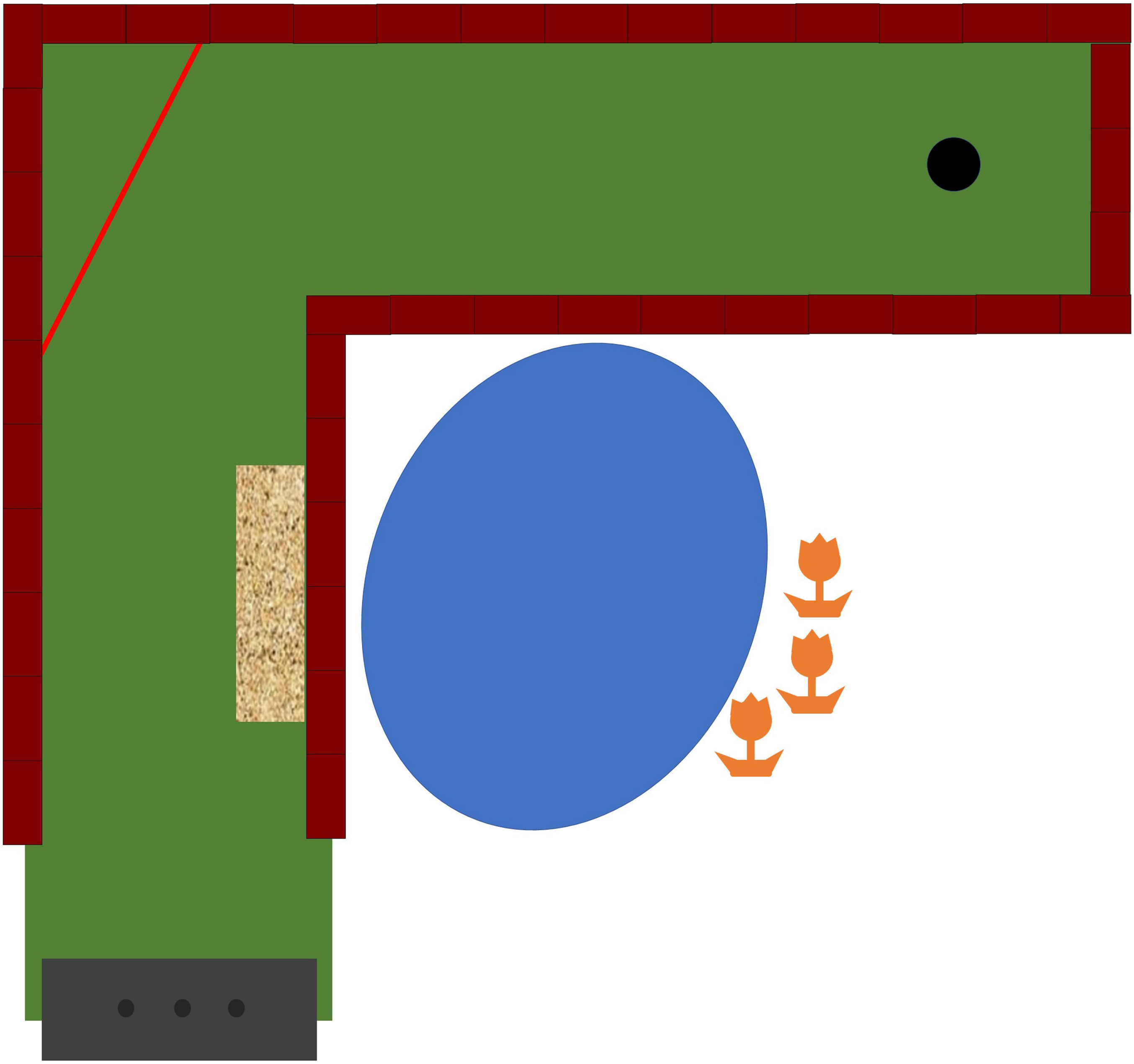


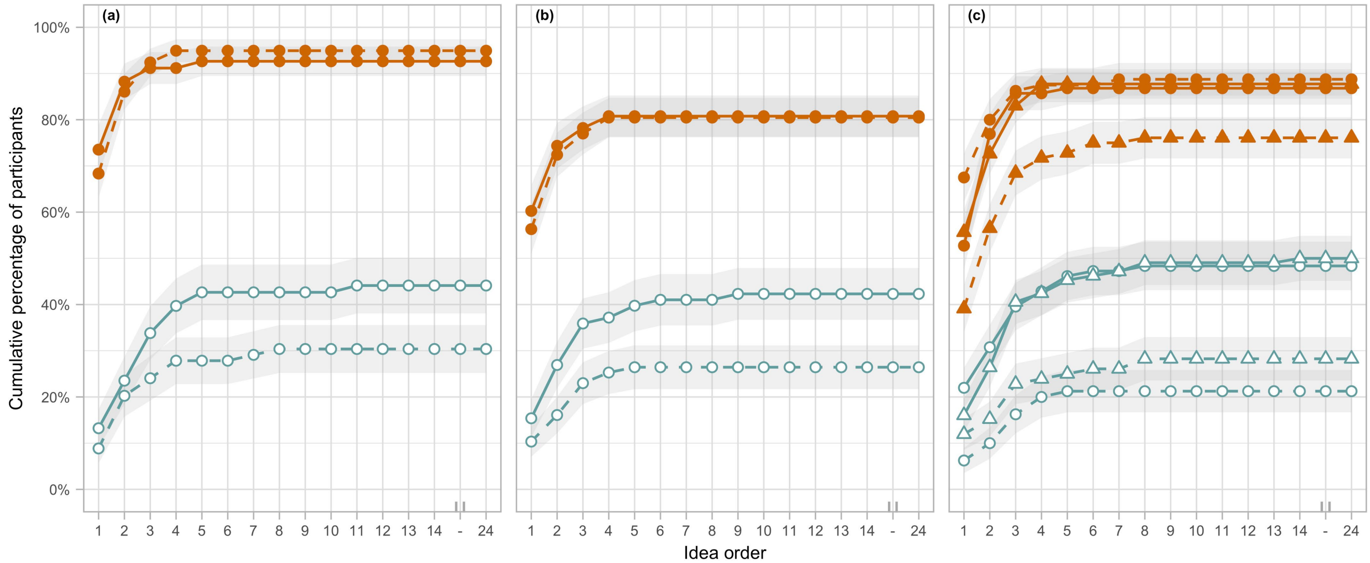
Extended Data Fig. 1 | People are more likely to subtract from recipes with atypical ingredients (study S9). Each dot represents one recipe ($k=27$). Placement on the x axis reflects the atypicality score for each recipe, determined by the mean rating from $n=80$ workers from Amazon Mechanical Turk. Independent samples of $n=7$–12 workers from Amazon Mechanical Turk (total, $n=284$) then had an opportunity to transform one of the randomly assigned recipes. Placement on the y axis reflects the percentage of participants who produced a subtractive transformation of each recipe. The atypicality score of an ingredient positively predicted the percentage of participants who subtracted; $r_{25}=0.54$, $P=0.003$; two-sided test. Error band represents the 95% confidence interval of predicted percentage subtracting.



Extended Data Fig. 2 | Study material for experiment 1. In the stimuli that participants saw, a toy action figure (image removed for reasons of copyright) stood at the height marked on the white paper. Participants could stabilize the top platform of the Lego structure so it could hold a masonry brick above the head of the action figure by adding new supports to reinforce the single corner block or by removing the corner block and letting the platform sit flush on the layer below. They earned $1 for successful completion, but adding Lego bricks cost money. The most profitable solution was to remove the single support. Participants were randomly assigned to instructions that explicitly stated ‘removing pieces is free’ (cue condition) or instructions that did not mention removing pieces (control condition).



Extended Data Fig. 3 | Study material for experiments 2 to 4. Participants reported their ideas for how to change this miniature golf hole. We coded whether each idea was additive (for example, 'add a windmill'), subtractive (for example, 'remove the sand trap') or neither (for example, 'reverse the direction'). Participants were randomly assigned to a no-cue instruction that mentioned neither addition nor subtraction or to a cue condition that reminded participants they could 'add or subtract'. In experiments 2 and 3, participants reported all of the ways that they might improve the original. In experiment 4, participants were randomly assigned to a condition that solicited their improvement ideas or a condition that solicited their ideas for making the hole worse.



Extended Data Fig. 4 | Cumulative percentage of participants who included at least one type of idea by the *i*th idea in their list in experiments 2 to 4. Participants were randomly assigned to one of two conditions in experiments 2 and 3 (no-cue versus cue) ($n = 312$); and one of four conditions in experiment 4 ((no-cue versus cue) × (improve versus make-it-worse)) ($n = 369$). The y axes show cumulative percentage. The x axes show idea order (i). Empty blue shapes represent subtractive ideas and filled orange shapes represent additive ideas. Dotted lines represent no-cue conditions and solid lines represent cue conditions. Circles represent responses to an improve prompt and triangles represent responses to a make-it-worse prompt. **a**, **b**, Across experiments 2 (**a**) and 3 (**b**), we did not find evidence that the cue affected the likelihood of participants listing at least one additive idea (odds ratio = 0.92, $z = -0.24$, $P = 0.810$), but we did find evidence that it increased the likelihood of participants listing at least one subtractive idea (odds ratio = 1.93, $z = 2.73$, $P = 0.006$). **c**, In experiment 4, the cue increased the likelihood of participants listing at least one subtractive idea within the improvement conditions (no-cue = 21%, cue = 48%, $\chi^2 = 13.63$, $P < 0.001$) and the make-it-worse conditions (no-cue = 28%, cue = 50%, $\chi^2 = 9.71$, $P = 0.002$). The error band represents s.e. of proportion.

Extended Data Table 1 | Observed rates of subtractive changes, additive changes and other changes in studies S1 to S8

Study and original stimulus	Observation of Interest	Percent (number) of changes by type			Subtraction % (excluding other) [95%CI]
		Subtraction	Addition	Other	
S1 – Abstract grid task	Participants who subtracted (vs. added) on 4 or more (out of 6) trials	19% (18/94)	78% (73/94)	3% (3/94)	20% [12, 29%]
S2 – Request for ideas from university stakeholders (field observation)	Ideas that suggest removing (vs. adding) elements	8% (70/827)	70% (581/827)	21% (176/827)	11% [8, 13%]
S3 – Block structures	Structures with fewer (vs. more) bricks	12% (5/42)	86% (36/42)	2% (1/42)	12% [4, 26%]
S4 – Block structures created via randomized process	Structures with fewer (vs. more) bricks	2% (1/60)	98% (59/60)	0% (0/60)	2% [0, 9%]
S5 – Block structures created by independent participants	Structures with fewer (vs. more) bricks	5% (1/20)	95% (19/20)	0% (0/20)	5% [0, 25%]
S6 – One's own writing	Summaries with fewer (vs. more) words	16% (32/198)	80% (159/198)	4% (7/198)	17% [12, 23%]
S7 – Others' writing	Summaries with fewer (vs. more) words	31% (61/200)	64% (128/200)	5% (11/200)	32% [26, 39%]
S8 – Pre-filled daytrip itinerary	Itineraries with fewer (vs. more) activities	19% (28/144)	51% (73/144)	30% (43/144)	28% [19, 36%]

Subtractive changes are those that have fewer components than the original; additive changes are those that have more components than the original; the 'other' category refers to all responses that could not be categorized as subtractive or additive. The unit of analysis in study S2 is ideas rather than people. Numbers in parentheses represent the count of participants who subtracted (as the numerator) divided by the total number of participants in the condition (denominator). The 'substraction %' column excludes 'other' responses; it represents the percentage of subtractive changes out of all additive and subtractive changes, and provides the basis for a test against 50%. For all studies, values in the 'subtraction %' column differ significantly from the 50% that would be expected if the two transformations were equally common (two-sided binomial distribution probability, $P < 0.001$). Procedural and analysis details are available in the Supplementary Information sections 2.1–2.8.

Extended Data Table 2 | Changing originals with more and fewer components in study S10

Condition	Average change in ingredients	*SD*	Two-tailed *p*	Effect size *d*
5-ingredients (*N* = 84)	+2.85	2.13	< .001	1.34
10-ingredients (*N* = 89)	+0.45	2.12	0.048	0.21
15-ingredients (*N* = 77)	-1.87	2.53	< .001	-0.74

Mean number of ingredients added to a 5-, 10- or 15-ingredient soup recipe ($n = 250$). Two-sided P values refer to one-sample t-tests of whether the change in ingredients differs significantly from 0 (representing no change).

Extended Data Table 3 | Overlooking subtraction in samples from university students from the USA, Germany and Japan

Group	Made subtractive change	Recognized subtractive change as correct	McNemar's χ^2	Two-tailed p	Odds ratio
USA	82% (81/99)	96% (95/99)	10.56	.001	1.17
Japan/ Germany	74% (93/126)	90% (113/126)	13.79	<.001	1.22

A sample of university students from the USA, and a combined sample of university students from Germany and Japan, both systematically overlooked subtraction. Study S12 showed that—within both samples—participants were significantly more likely to (incorrectly) produce the additive transformation and then (correctly) choose the subtractive transformation in a multiple-choice format than to (correctly) produce the subtractive transformation and then (incorrectly) choose the additive transformation in a multiple-choice format.

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☒	☐	A description of all covariates tested
☐	☒	A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons
☐	☒	A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
☐	☒	For null hypothesis testing, the test statistic (e.g. *F*, *t*, *r*) with confidence intervals, effect sizes, degrees of freedom and *P* value noted *Give P values as exact values whenever suitable.*
☒	☐	For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings
☒	☐	For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
☐	☒	Estimates of effect sizes (e.g. Cohen's *d*, Pearson's *r*), indicating how they were calculated

Our web collection on statistics for biologists contains articles on many of the points above.

Software and code

Policy information about availability of computer code

Data collection	Qualtrics survey software
Data analysis	R v. 4.0.2 (including the effsize and psych packages), SPSS v. 27, and jamovi v. 1.2. R code is available at: https://osf.io/7v6r2/

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Research guidelines for submitting code & software for further information.

Data

Policy information about availability of data

All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A list of figures that have associated raw data
- A description of any restrictions on data availability

Data and materials are available at: https://osf.io/7v6r2/
The coded responses from Study S2 are posted per our agreement with the organization that supplied the data. Open-ended responses are available from the authors upon reasonable request and with permission of the organization.

Field-specific reporting

Please select the one below that is the best fit for your research. If you are not sure, read the appropriate sections before making your selection.

☐ Life sciences ☒ Behavioural & social sciences ☐ Ecological, evolutionary & environmental sciences

For a reference copy of the document with all sections, see nature.com/documents/nr-reporting-summary-flat.pdf

Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description

All data are quantitative. As described in the Methods document, Experiments 1-8 use experimental designs. As described in the Supplementary Information, Studies S1 through S10 are observational studies; S11 is an experiment; Study S12 is a quasi-experiment.

Research sample

We recruited participants from a variety of convenience samples (MTurk, University student pools, passersby). Samples were not intended to be representative of any population.

Experiment 1: passersby; no demographic data collected
Experiment 2: MTurk; 63 women, 83 men, 1 unspecified; Mean age = 34.70, SD = 10.10
Experiment 3: MTurk; 74 women, 90 men, 1 unspecified; Mean age = 35.70, SD = 9.44
Experiment 4: MTurk; 183 women, 181 men, 5 unspecified; Mean age = 37.00, SD = 11.73
Experiment 5: MTurk; 126 women, 170 men, 3 unspecified, Mean age = 34.99, SD = 9.27
Experiment 6: MTurk; 128 women, 170 men, 1 unspecified; Mean age = 35.03, SD = 9.68
Experiment 7: U.S.-based university participant pool; 197 women, 88 men, 1 unspecified; Mean age = 18.82, SD = 1.61
Experiment 8: U.S.-based university participant pool; 367 women, 200 men, 1 unspecified; Mean age = 18.84, SD = 1.67
Study S1: MTurk; 58 women, 39 men; Mean age = 34.19, SD = 9.59
Study S2: archival (original responses are from stakeholders at a U.S.-based university)
Study S3-S5: passersby; no demographic data collected
Study S6: U.S.-based university participant pool; 129 women, 68 men; 1 unspecified; Mean age = 21.8, SD = 6.72
Study S7: U.S.-based university participant pool; 140 women, 58 men; 2 unspecified; Mean age = 21.40, SD = 6.04
Study S8: MTurk; 59 women, 85 men; Mean age = 37.09, SD = 12.81
Study S9 Pretest: MTurk; 28 women, 51 men, 1 unspecified; Mean age = 34.71, SD = 10.92
Study S9: MTurk; 126 women, 156 men, 2 unspecified; Mean age = 35.62, SD = 10.79
Study S10: MTurk; 108 women, 134 men; Mean age = 36.28, SD = 11.59
Study S11: U.S.-based university participant pool; 160 women, 49 men, 1 non-binary; Mean age = 18.69, SD = 1.23
Study S12: students in Japan = 48, students in Germany = 75, students in the U.S. = 99; 64 men, 159 women, 2 unspecified; Mean age = 22.61, SD = 7.43.

Sampling strategy

Sample sizes for each study were determined before data collection (i.e., no additional data were collected in any study after analysis began). For studies that were the first of their design, we used informal rules of thumb to determine a target sample size. For subsequent studies that used a similar design, we used observed effect sizes as an informal guide. The exceptions to this approach were Experiment 1 (pre-registered based on a power calculation, see https://osf.io/rkqvw/), Study S2 (an archival study) and S12 (we were limited by the number of volunteers). Except for Experiment 1, no power analyses were performed.

Data collection

Research assistants recruiting passersby and recording data; blind to hypothesis (Experiment 1; Studies S3-S5)
Qualtrics survey administered virtually through MTurk; researchers blind to condition (Experiments 2-6; Studies S1, S8-S10)
Qualtrics survey administered in person to members of university-based participant pools; researcher assistants blind to condition, hypothesis (Experiment 7)
Qualtrics survey administered online to members of university-based participant pools; researchers blind to condition (Experiment 8; Studies S6-S7, S11-12)
Archival data (no data collected by researchers): S2

Timing

Studies were conducted between 2017 and 2020.

Data exclusions

We established before data analysis that we would exclude those who participated in the study more than once (as determined by duplicate IP addresses), participants who failed an attention check, or those for whom study procedures were violated during participation.
These criteria were applied only when relevant to the sample (e.g., duplicate IP addresses only for online studies), and are consistent across similar methods and samples (e.g., all MTurk samples had the same exclusion criteria).
The number of people excluded under each criterion is reported in the methods section of each study and is also listed here (if an experiment or study does not appear under a criterion, no exclusions were made):
Duplicated responses: Experiment 2 (n = 5); Experiment 3 (n = 4); Experiment 4 (n = 3); Experiment 5 (n = 2); Experiment 7 (n = 16); Experiment 8 (n = 3); Study S1 (n = 6); Study S8 (n = 2); Study S9 Pretest (n = 5); Study S9 (n = 14); Study S10 (n = 21), Study S12 (n = 1)
Failed attention check: Experiment 3 (n = 13), Experiment 4 (n = 27); Study S8 (n = 42), Study S9 Pretest (n = 13); Study S9 (n = 1), Study S10 (n = 5)
Violation of study procedure: Experiment 1 (n = 6), S11 (n = 1)

Non-participation

Participants were determined not to have participated if they did not provide consent or if they withdrew during the task and provided no valid responses. These data were only collected for online studies (i.e., not for in-person studies). The number of people

excluded for non-participation is reported in the methods section of each study and is also listed here (if an experiment or study does not appear under a criterion, no exclusions were made):
Did not provide consent: Experiment 2 (n = 96), Experiment 3 (n = 27), Experiment 4 (n = 80), Study S12 (n = 4)
Withdrew during task/no valid responses: Experiment 2 (n = 14), Experiment 3 (n = 127), Experiment 4 (n = 179), Experiment 5 (n = 12), Experiment 6 (we did not keep a record), Experiment 7 (n = 16), Experiment 8 (n = 11), Study S1 (n = 7), Study S6 (n = 70), Study S7 (n = 49), Study S8 (n = 23), Study S9 Pretest (n = 10), Study S9 (n = 36), Study S10 (n = 50), Study S11 (n = 5), Study S12 (n = 9)

Randomization

In Experiment 1, we used a randomizer to make a pre-determined randomization schedule. In Experiments 2—8 and Studies S9—S11, we used the randomization feature present in Qualtrics's survey software.
In Studies S1—S8, there were no experimental conditions.
In Study S12, participants were analyzed in groups based on their geographic location.

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involved in the study	
☒	☐	Antibodies
☒	☐	Eukaryotic cell lines
☒	☐	Palaeontology and archaeology
☒	☐	Animals and other organisms
☐	☒	Human research participants
☒	☐	Clinical data
☒	☐	Dual use research of concern

Methods

n/a	Involved in the study	
☒	☐	ChIP-seq
☒	☐	Flow cytometry
☒	☐	MRI-based neuroimaging

Human research participants

Policy information about studies involving human research participants

Population characteristics

See above.

Recruitment

For studies with passersby, we advertised the study on a whiteboard and offered candy. (For Experiment 1, passersby also received a bonus for successful completion, $1 minus 10 cents per added brick.)
MTurk workers were recruited on the platform and were paid between $0.50 and $1.00 depending on the estimated length of each study.
Participants in Experiments 7-8 and Studies S6-S7 and S11 were recruited from a participant pool and received course credit. For S11, participants also received up to a $1 bonus.
Study S12 participants were university students in Japan and Germany who were emailed information about the study by their instructor and were paid in Amazon gift cards.
Due to informed consent procedures and the use of brief advertisements, people may have chosen to participate based on their knowledge of or interest in the topic. Because participants were randomly assigned to condition, it is highly unlikely self-selection would give rise to the effects described in this research.

Ethics oversight

Research protocols were approved by the University of Virginia's Institutional Review Board for the Social and Behavioral Sciences. Human subjects consented to participate in all studies. No deception was used.

Note that full information on the approval of the study protocol must also be provided in the manuscript.