Reference: Ranney, M.A., Shonman, M., Fricke, K., Lamprey, L. N., & Kumar, P. & (2019). Information that boosts normative global warming acceptance without polarization: Toward J. S. Mill's political ethology of national character. In D. A. Wilkenfeld & R. Samuels (Eds.), *Advances in experimental philosophy of science* (pp. 61-96). (In Bloomsbury's Advances in Experimental Philosophy series.) New York: Bloomsbury.

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Information That Boosts Normative Global Warming Acceptance without Polarization: Toward J. S. Mill's Political Ethology of National Character

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Introduction

The history of psychology (e.g., Sahakian, 1968) is marbled with debates, of varying scales, about the relative degrees to which top-down or bottom-up processes dominate particular mental phenomena and performances. In this chapter, we consider this theory-driven/data-driven dialectic with respect to (1) four new, pertinent, experiments in which factual material increases people's acceptance that global climate change is occurring/concerning, and (2) conceptually adjacent philosophical considerations related to rationalism and national identity. The experiments collectively demonstrate (occasionally replicating) five brief interventions that boost people's acceptance of (e.g., anthropogenic) global warming (GW)—without yielding polarization. Experiment 1 demonstrates GW acceptance gains across ten mini-interventions contrasting graphs of Earth's temperature rise and equities' valuations rise. Experiment 2 replicates and extends earlier work showing GW acceptance changes among participants receiving feedback for a handful of their estimates regarding climate statistics. Experiment 3 exhibits a GW acceptance gain among US participants receiving feedback for nine of their estimates regarding "supra-nationalism" statistics that suppress American (over-)nationalism. Experiment 4's large contrastive study compares Experiment 2's representative stimuli's effect with (a) the effect-sizes of five videos and three texts that differentially explain global warming's mechanism, as well as (b) non-mechanistic controls; Experiment 4 additionally serves to further replicate some (textual and statistical) findings from prior research and bolster Experiment 3's findings. The interventions' success further justifies John Stuart Mill's and others' perspective that humans often function as empiricists. (Many of the interventions are—fully or partially—available at www.HowGlobalWarmingWorks.org, our multi-language website for directly enhancing all nations' public "climate change cognition.")

The reasoning researcher James F. Voss once told the first author that (paraphrasing) "psychology's history suggests that, any time a cognitive scholar considers explanatorily excluding one of two major contrastive yin-yang possibilities—such as either top-down or bottom-up processing—s/he is likely to ultimately be seen as mistaken." This chapter's experiments' findings, we believe, simultaneously (a) provide some insight into Americans' often-peculiar thinking with respect to global warming, and (b) cohere, in this realm, with Voss's synergistic generalization (in this incarnation: that individuals can change their minds due to either, or both, evidence-based and culture-based cognition—although we empirically focus mostly on the former herein). We will present our four new experiments about GW cognition in the spirit of J. S. Mill's proposed "political ethology"—a science of national character. (Domains attendant to GW cognition include, among others we have identified, the psychology of evolution, religion, nationalism, affiliation, and conservatism; e.g., Ranney and Thanukos, 2011.) We start here by considering Mill's musings about how nations might be differentiated—in relief to the much more recent Reinforced Theistic Manifest Destiny theory (RTMD; e.g., Ranney, 2012).

Some critical ways in which nations might differ: RTMD as a Millian Theory

International polls are increasingly salient and frequently illuminating, such as recent polling about how nations' populaces view Vladimir Putin (sometimes in comparison with Donald Trump; Vice/Pew, 2017). John Stuart Mill (1843/2006) may not have imagined such samplings when considering "a State of Society" (p. 911) or "the character of the human race" more broadly (p. 914), but his philosophy entertained both individual and sociological mental analyses in articulating his senses of the differential characters of nations—while highlighting "psychological and ethological laws" on the development of "states of the human mind and of human society" (p. 914). Mill envisioned a science that would "ascertain the requisites of stable political union" (p. 920), a stability that he ascribed to England, among some other nations (but certainly not all[1]). One of Mill's essential conditions of a stable political society is a sense of "nationality" (which he contrasts to its more negative senses)—a citizenry's "feeling of common interest" that provides "cohesion among the members of the same community or state" (p. 923). The experiments presented below are part of a scientific research program that (with Ranney and Clark, 2016, etc.), in various ways, probes aspects of US national character (Ranney, 2012; Ranney and Thanukos, 2011)—thus implementing the kind of science Mill seemed to advocate.

Recognizing political ethology, Mill called for a science of national character. In modern terms, he seemed to effectively posit the existence of certain local cognitive minima in the energy-space of national identities—in other words, that there are essentially discrete, stable configurations of societal positions about important issues. Rather as behaviorists noted that not all stimuli and potential consequences are equally naturally associable (e.g., Garcia and Koelling, 1966), on a molar level, the associationist (Sahakian, 1968) Mill argued: "When states of society, and the causes which produce them, are spoken of as a subject of science, it is implied that there exists a natural correlation among these different elements; that not every variety of combination of these general social facts is possible, but only certain combinations; that, in short, there exist Uniformities of Coexistence between the states of the various social phenomena" (Mill, 1843/2006, p. 912).

Earlier in this decade, the RTMD theory was induced (Ranney, 2012; Ranney and Thanukos, 2011) to explain some of the kinds of inter-"element" correlations that Mill might consider, were he researching today. RTMD may best be considered quasi-causal in that, rather as Mill might have noted of nineteenth-century England, some RTMD relationships approximate a logically competitive character (e.g., at an extreme today: if biblical creation is thought to be literally true, that person sees evolution as much less plausible because they compete). In contrast, other relationships approximate historical concept-affiliations (e.g., fossil-fuel-poor countries, *ceteris paribus*, generally have fewer nationalist-energy reasons to deny climate change [setting aside those countries' development-inhibition fears]). Figure 4.1 illustrates RTMD (which is more extensively explicated elsewhere, especially in Ranney, 2012), which embodies relatively stable national relationships among six main constructs—both noticed and predicted—including acceptance/affinity regarding (a) an afterlife, (b) a deity (/deities), (c) creationism, (d) nationalism, (e) evolution, and (f) global warming (GW; e.g., the focal aspects of climate change—that Earth is generally warming, largely anthropogenically, and therefore warrants considerable concern). For Americans, and presumably for *any* nation's people, one's acceptance of (a)–(d) are predicted to correlate with each other (e.g., caricaturing the religious

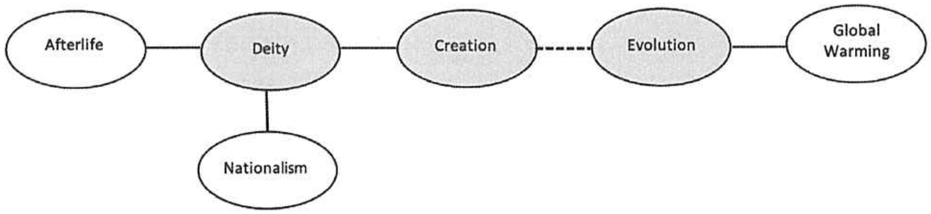

Figure 4.1 Reinforced Theistic Manifest Destiny (RTMD) theory (Ranney et al., 2012a), which extended a more "received view" perspective (i.e., the three shaded ovals) regarding some classically controversial socio-scientific constructs. A geopolitical theory, RTMD posits coherent or conflicting notions with, respectively, solid or dashed conceptual links. Especially for a given individual (and plausibly for any given nation), RTMD predicts that the degree to which the four leftmost constructs are accepted, the less the rightmost two constructs will be accepted, and vice versa. Correlational and causal data to date have overwhelmingly borne this out.

right: "God and country," "joining God in heaven," "God created organisms as they are," etc.), as are (e)–(f) (e.g., caricaturing the less religious left: "evolution has, through humans, caused global warming"). However, each construct of {a–d} should negatively correlate (i.e., anticorrelate) with each construct of {e–f}, a prediction that is causally demonstrated in Experiments 3 and 4 (e.g., d-versus-f: "solving global warming involves *inter*national agreements and less über-nationalism"). Figure 4.1 highlights the three central (theistically impacted) constructs from RTMD's original explication—deity, creation, and (creation's strongly anticorrelated associate) evolution—from which much of nations' conceptual tensions may stem. (See also Figure 4.1's caption.)

With other colleagues from Berkeley's Reasoning Group, we have assessed these six constructs' interrelationships using various US participant populations, and when sample sizes permit, the fifteen correlations among (a)–(f) always appear in the predicted directions, usually with statistical significance (e.g., Ranney et al., 2012a). Indeed, we have never found a significant US correlation opposite in direction to that which RTMD theory predicts (although for some rather small-sample studies focused on "boutique" hypotheses, we occasionally find *non*significant correlations in contrapredicted directions).

People are empiricists (and rationalists)

RTMD theory has inspired many GW-focused experiments (e.g., Ranney and Clark, 2016; Ranney, Munnich, and Lamprey, 2016), and four more are explicated herein. Beyond more specific hypotheses, each experiment assesses whether people are empiricists—in the sense that they will change their views to better cohere with presented scientific (e.g., statistical or empirically induced mechanistic) information. We show below, as previously, that they do. These findings hardly negate rationalistic (e.g., theory-conserving) processes, and the findings hardly suggest that people are merely bottom-up thinkers—because top-down, hypothesis-driven rumination is assumed to *also* be fundamental to human cognition (e.g., Ranney, 1987; cf. Sahakian, 1968, p. 2, regarding Anaxagoras, Democritus, and reason vs. the senses).[2]

Many, like we, believe psychologists had *already* proven that humans are empiricists in that they change beliefs in the face of empirical evidence (e.g., van der Linden et al., 2017). However, the occasional researcher claims that cultural biases eliminate the possibility of changing people's minds with scientific information (e.g., Kahan et al., 2012). This position, which Kahan and his colleagues ascribe to people, is so extreme that no noteworthy philosopher appears to have held it[3]—and it might be seen as an untenably extreme (e.g., fully top-down) form of some rationalists' *superiority of reason thesis*[4] (Markie, 2017), which would also conflict with the data priority principle of explanatory coherence theory (Ranney and Schank, 1998; Ranney and Thagard, 1988; Thagard, 1989, etc.). Kahan and Carpenter (2017) continued this overly rationalist attribution to people, inaccurately[5] asserting an "immobility of public opinion" on GW, even as they noted that "decision scientists have been furiously" trying to improve "public engagement with this threat" (p. 309). Ranney and Clark (2016) extensively disconfirmed this (essentially anti-empiricist) "stasis" view through controlled experiments, historical counterexamples, and a literature review of crucial research (see, e.g., Ranney & Clark's pp. 50–55 and 65–67) showing that even Kahan et al.'s own (2015) data disconfirmed the stasis view (also see Ranney, Munnich, and Lamprey, 2016). More recently, van der Linden et al. (2017) have more briefly elaborated upon Ranney and Clark's (2016) exposition that culture versus information is a false dichotomy, calling "culture versus cognition" a "false dilemma."

Common sense and Mill's work both share our predilection toward empirical information as crucial in determining beliefs. At a gut-phenomenal level, people are strikingly changed by even a single observation (also see Ranney, Munnich, and Lamprey, 2016). Examples include (a) watching a would-be recommendation letter-writer fatally flattened by a road roller—or (b) returning home unexpectedly to observe a supposedly monogamous partner engaged in infidelity. One could reject such bottom-up evidence through some *extreme* reason-superiority rationalist interpretation, but that rarely happens. In another contrastive example, (c) people ending their vacations to find their house burned/shaken/blown/washed to its foundation don't just deny it, as they see that their structure is gone (and denial might warrant psychiatric hospitalization). The datum, in the moment, changes one forever. We are empiricists, even if we are also—perhaps equally or more at times—top-down rationalists with *non*extreme intransigence and/or *non*extreme affections regarding extant beliefs. (Naturally, a single rational inference also, at times, transforms what seems empirically settled, e.g., regarding explanatory coherence: Ranney and Thagard, 1988; Ranney and Schank, 1998.) The empiricist Mill (1843/2006) highlighted new knowledge's crucial role in changing cultures/societies (just as we have highlighted mind-changing information: Ranney and Clark, 2016; Ranney, Munnich, and Lamprey, 2016). Although celebrating the "progress of knowledge" and lamenting that "the changes in the opinions" of people are slow, Mill noted that every "considerable advance in material civilization has been preceded by an advance in knowledge" (p. 927).

Global climate change denial

Early in *this* century, many characterize the most frustratingly slow opinion change to be Americans'—and many of their representatives'—reluctance to quickly inhibit global climate change. Mill induced overall societal progress, yet saw nations *nonmonotonically* improving—seemingly anticipating *local* optimization notions (cf. Ranney and Thagard, 1988; Thagard, 1989, etc.): "the general tendency is, and will continue to be, saving occasional and temporary exceptions, one of improvement; a tendency towards a better and happier state" (1843/2006, p. 913, etc.). Our materials and our website, HowGlobalWarmingWorks.org (Ranney and Lamprey, 2013–present), discussed below, use scientific information to foster such improvements regarding climate change beliefs—hopefully even "flipping" beliefs. Our efforts are meant to better engage Mill's "social science," of which he said (p. 912) the "fundamental problem . . . is to find the laws according to which any state of society produces the state which succeeds it and takes its place." In modest respects, RTMD represents seeking such laws (e.g., in positing and confirming an inverse nationalism–GW relationship).

Every nation signed the Paris climate change accord, yet America announced plans to leave it. US acceptance of the following propositions lag behind its peer nations on an alarm-spectrum of beliefs, including that GW is (a) occurring, (b) partially anthropogenic, (c) largely anthropogenic,

(d) imminently concerning, and (e) demanding fast action. So, although RTMD ought to apply to virtually all nations, we focus our instruction on participants from the United States, where urgent need seems greatest, especially given that America presently represents the largest economic, military, and (arguably) political force in the world. Therefore, this chapter's four new experiments each further assesses the hypothesis that—and the degree to which—empirical information can increase Americans' GW acceptance. Extending our prior efforts, Experiment 1 assesses a new dimension of potentially persuasive empirical information: contrastive graphs. Experiment 2 both extends and replicates a prior finding (from Ranney and Clark, 2016, whose seven experiments mostly focused on the utility of explaining GW's mechanism) regarding the effectiveness of *statistics* to increase GW acceptance—with improved stimuli and a larger sample. Experiment 3 assesses a novel RTMD prediction (related to Mill's interest in studying national character): that reducing one's super-nationalism with supra-nationalist statistics will increase the person's GW acceptance. Experiment 4 represents a twenty-one-condition mega-experiment that contrasts the usefulness of many interventions to consider their relative "bang per buck" in increasing such acceptance. In essence, Experiments 1 and 2 address the stasis—or "science impervious"—view again (re: empiricism), Experiment 3 engages RTMD theory, and Experiment 4 does both.

Experiment 1: Boosting GW acceptance with graphs and averaging

Building on prior research showing that statistical and mechanistic information (separately) increase GW acceptance (Ranney and Clark, 2016; Ranney, Munnich, and Lamprey, 2016), we hypothesized that visual graphs representing observed temporal data trends could similarly effectively and durably increase such acceptance. Utilizing graphs of Earth's average surface temperature and the Dow Jones Industrial Average (adjusted for inflation: "DJIA-a"), Experiment 1 participants responded to queries encouraging them to reflectively reason and critically process the graphs' information.

This experiment (Chang, 2015; cf. Ranney, Chang, and Lamprey, 2016) was inspired by Lewandowsky (2011), who asked participants to extrapolate from a temperature graph or an identical pseudo-stock graph; overall, Lewandowski's participants extrapolated rises in both global temperature and (purported) stock prices for future years, establishing that graphs depicting temporal temperature trends could increase GW acceptance. Experiment 1 replicated and elaborated Lewandowsky's finding by (a) soliciting broader responses about provided graphs, (b) depicting *actual* stock data (i.e., the DJIA-a), and (c) assessing the longevity of participants' changes with a delayed ("Phase 2") experimental session.

Method

Participants

Participants were 712 workers recruited from Amazon's Mechanical Turk (MTurk). Forty-nine participants were excluded after the experiment's initial phase, and 429 of the retained 663 participants both chose to complete, and satisfactorily completed, the experiment's second phase—a delayed posttest that was administered after 6–9 (M = 8.6) days. Participants were excluded if they (a) were not US citizens located in the United States, (b) took excessive or too little time relative to their peers, (c) failed too many attention checks, and (d) generated *overly* incoherent/self-contradictory data, as determined through index scores. An example of (d) would be a participant fully accepting a deity's existence on one item, yet fully rejecting any deities' existence on a later item (which suggests rushed, "random," or inattentive responding).

Design, procedure, and materials

All the experiments' materials were presented using Qualtrics. Experiment 1's participants responded, using a 1–9 Likert scale[6], to the same set of eight GW items during three testing times: twice in Phase 1—during pretesting and immediate posttesting—and once in Phase 2's delayed posttest (9 days hence). Table 4.1 presents the items, which were similar to GW items from Ranney and Clark (2016) and so on. Following pretesting, participants learned about an unbiased alien-robot, "Bex," who helped guide them through one of ten randomly assigned conditions and helped them understand the data and the utility of averaging when analyzing graphical data over long periods. (Chang, 2015, explicated the conditions, which were specifically selected from 48 [3 × 2 × 2 × 2 × 2 factorial] possibilities to test particular hypotheses.)

Participants rated the degree to which they agreed with each statement, selecting a number from 1 (Extremely Disagree) to 9 (Extremely Agree).

In each condition, participants received graphs concerning the Earth's surface temperature and the DJIA-a. The Earth's air and surface temperature data,

Table 4.1 Global warming attitude items on pre- and posttest. (Participants rated the degree to which they agreed with each statement, selecting a number from 1 [Extremely Disagree] to 9 [Extremely Agree].)

Item Text	Response Range
Human activities are largely responsible for the climate change (global warming) that is going on now.	1–9
Global warmings or climate changes, whether historical or happening now, are only parts of a natural cycle.	1–9
If people burned all the remaining oil and coal on Earth, the Earth wouldn't be any warmer than it is today.	1–9
I am confident that human-caused global warming is taking place.	1–9
I am concerned about the effects of human-caused global warming.	1–9
I would be willing to vote for a politician who believes human-caused global warming doesn't occur.	1–9
Global warming (or climate change) isn't a significant threat to life on Earth.	1–9
The Earth isn't any warmer than it was 200 years ago.	1–9

spanning 1880–2014, were from NASA. The DJIA-a data reflected US financial stock/equity values during 1885–2014 (Williamson, 2015). Because *annually* averaged scatter-plots increase the difficulty of honing in on temperature and DJIA-a trends, beyond-annual averaged graphs were provided that used two forms of averages: moving and span. With these averages, we created DJIA-a and temperature graphs that showed 4, 8, 16, and 64-year-averaged trends (pp. 150–151 of Ranney, Munnich, and Lamprey, 2016, displays six of these graphs, and 16-year span-averaged graphs are displayed at howglobalwarming works.org/2graphsab.html). Participants were randomly assigned to one of the ten conditions, which differed in the number and types of graphs shown, and so on. The ten interventions were brief—averaging 6 minutes, with the longest taking 7 minutes. (Chang, 2015, offers much greater detail.)

During the interventions, each participant was asked to complete/answer sets of interactive exercises/questions designed to facilitate numerical reasoning and engagement with the DJIA-a and temperature graphs. Four graphical analysis techniques were utilized. First, participants analyzed whether the averaged stock and/or temperature graphs were increasing, decreasing, or neither. Second, participants differentiated between side-by-side 16-year-averaged DJIA-a and temperature graphs (i.e., to guess/decide which graph was which). Third, participants extrapolated five future data points on both DJIA-a and temperature graphs (in 5-year increments, up to 2035). Finally, participants were told that some participants had received switched graphs (i.e., graphs stated as DJIA-a graphs were temperature graphs and vice versa), and participants were asked whether they believed theirs were switched.

Following the intervention, participants completed an immediate posttest that reassessed GW attitudes and elicited demographic data. Nonexcluded participants were invited to complete a (shorter) second phase 6–9 days after Phase 1. Phase 2's delayed posttest was similar to Phase 1's immediate posttest, in that it again reassessed GW attitudes (to assess the immediate posttest gains' durability), but participants were *not* reintroduced to Bex, the logic of averaging, or why span- and moving-average graphs are useful. Following Phase 2's central portion of the delayed posttest (e.g., focusing on GW beliefs), participants were asked to both (a) differentiate between 16-year-averaged DJIA-a and temperature graphs, and (b) extrapolate DJIA-a and temperature up to 2035.

Results

Participants demonstrated durable, robust, and statistically significant GW acceptance gains for *each* of the ten interventions at *both* immediate and delayed posttests (p values from .0015 to 2.95×10^{-14}). Regarding gains possible (i.e., from pretest acceptance to extreme, "9," acceptance), combining all ten individually significant conditions, the mean gain was roughly 23.4 percent of what was possible on the immediate posttest (.655 points; $t(523) = -20.33$; $p < 2 \times 10^{-16}$) and 20.7 percent on the delayed posttest (.579 points; $t(381) = -16.19$; $p = 2.2 \times 10^{-16}$), demonstrating participants' high retention of the gained acceptance even 9 days postintervention. In addition, 98 percent of participants believed the 16-year span-average temperature graph to be increasing, and the other 2 percent did not believe it decreased. Virtually all participants also predicted that Earth's temperature (and the DJIA-a) would continue to rise through 2035. Furthermore, no polarization was observed; even the most conservative participants (on *both* economic and social subscales) exhibited GW acceptance gains—as did liberals.

Rather as did Garcia de Osuna, Ranney, and Nelson (2004, which regarded abortion-reasoning), we analyzed "flips" and "semi-flips"—here, individuals changing relative to the 5.0 midpoint of a scale. As is typical with our GW intervention experiments, many more participants' average GW scores (a) flipped from below-5.0 to above-5.0 than vice versa ($p = .004$) or (b) semiflipped upward (from 5.0 to above-5.0 or from below-5.0 to 5.0) than vice versa ($p = .006$). Even more strikingly, because considerably fewer participants were below-5.0 on the pretest than above-5.0 on the pretest, it was *12.45 times* more likely (regarding conditional probabilities) that a 5.0-or-lower participant would flip or semi-flip toward greater GW acceptance than that a 5.0-or-higher participant would flip or semi-flip toward lesser GW acceptance.

Discussion

The results show that the graphs' plotted and averaged scientific data (e.g., Earth's surface temperature, and the DJIA-a, over time) help people visualize and sort through noisy data, making trends clear. Our graphs generally become more interpretable when each averaged datum subsumed longer temporal periods (i.e., 4- vs. 8- vs. 16- vs. 64-year-averaged trends). The 64-year moving-average graphs are particularly compelling because their near-monotonicity makes it difficult to deny that the DJIA-a—and, more importantly, Earth's temperature—have been rising since the 1880s.

Our graphs spawn scientific climate change inferences in several ways. First, many people infer the rising temperature trend rather directly after merely viewing the annual (and more aggregated) temperatures since 1880. Second, even if one cannot honestly infer/"see" the temperature rise (for instance, in the 16-year-averaged graphs), if one (a) believes that the DJIA has been increasing, yet (b) cannot confidently differentiate between the DJIA-a and temperature graphs (or if one cannot tell whether the graphs were switched), then one should (c) infer that both equity values and mean temperature are increasing. Additionally, asking participants to extrapolate future annual means encourages participants to analyze past data trends and incorporate them coherently into their knowledge of climate change trends—yielding positively sloping extrapolations.

Experiment 1's findings again (as per Ranney and Clark, 2016; also see Ranney, Munnich, and Lamprey, 2016; and van der Linden et al., 2017) *disconfirm* two suggestions: (a) that conveying germane scientific climate information polarizes conservatives and (b) that effecting conceptual changes through new knowledge is virtually hopeless. The disconfirmed "stasis theory" (i.e., the [b] just mentioned; e.g., see Kahan et al., 2012) underestimates people's abilities to counter purported inclinations toward extreme reason-superiority (i.e., über-rationalist, top-down thinking) and incorporate new, germane, information—such as the temperature data. Experiment 1 replicated prior stasis theory disconfirmations, adding to a growing body of experiments that have successfully used short knowledge-based interventions to change participants' GW acceptance (n.b. Ranney and Clark, 2016, pp. 54–55, provides a partial review; also see Lombardi, Sinatra, and Nussbaum, 2013; Otto and Kaiser, 2014).

No matter which condition participants received, 100 percent of this experiment's ten interventions fostered a significant GW acceptance increase. Strikingly, the observed gains were retained with virtually no decay after 9 days' delay. Given these "ten for ten" conditions' successes in yielding (and overwhelmingly maintaining) GW acceptance gains, this form of intervention seems a useful addition to prior successful instruction (e.g., mechanistic explanations and statistical evidence; Ranney and Clark, 2016, etc.) that have helped convince people that GW is occurring, anthropogenic, and worthy of concern. Each of these ways also reinforces a notion of "human as empiricist" and not just "human as extreme rationalist"—in keeping with Mill's (e.g., 1843/2006) philosophy.

Experiment 2: Changing GW acceptance with representative (and misleading) statistics

Experiment 1 established that brief interventions involving averaged graphs can durably increase one's GW acceptance. Prior studies (Ranney & Clark's Experiments 2–5) found similar durability using mechanistic GW explanations. In Experiment 2, we (Ng, 2015; Ranney, Munnich, and Lamprey, 2016) sought to (a) extend and replicate Ranney and Clark's (2016) Experiment 6, which showed that numerical feedback following participants' estimates of a series of *representative* statistical GW-related quantities also increased GW acceptance, and (b) replicate Ranney and Clark's (2016) Experiment 7 finding that *misleading-albeit-accurate* statistics can induce doubt regarding GW's reality.

Following the Numerically Driven Inference paradigm (NDI; e.g., Ranney et al., 2001; Ranney et al., 2008; Ranney, Munnich, and Lamprey, 2016), which includes inducing learning by providing feedback regarding the veracity of participants' estimates, this experiment also provided the true value of each statistic as feedback. We hypothesized—replicating Ranney and Clark's (2016) Experiments 6 and 7—that feedback on the representative statistics would increase GW acceptance, while providing the *misleading* statistics would *decrease* GW acceptance (e.g., Clark, Ranney, and Felipe, 2013). Extending those two experiments, we sought to assess such changes' durabilities about 9 days later (as in Experiment 1). The null hypothesis is that, perhaps due to efforts by those denying climate change, postintervention effects might be labile (see Ranney, 2008, and Ranney, Munnich, and Lamprey, 2016, on durability); our hope was that, for the representative statistics, increased GW acceptance would be maintained upon delayed posttesting.

Enhancements to Ranney and Clark's (2016) methods included updated statistical feedback values and the expansion of the representative statistics from seven to nine items. Methodology was also improved, with (a) a new example statistic for participants to practice the estimation procedure, (b) instructions better highlighting the feedback statistics as accurate, and (c) added exclusion criteria, such as checks to determine whether participants' answers were self-consistent. This study also incorporated Carol Dweck's "fixed versus growth" mindset concept. Dweck (2006) defines a fixed mindset as regarding "basic qualities, like their intelligence or talents" as "simply fixed traits," and a growth mindset as viewing one's "most basic abilities" as able to "be developed through dedication and hard work." Because climate change challenges people to modify their thinking and behavior, a secondary hypothesis was that the Representative intervention might shift participants, possibly durably, more toward a growth mindset.

Method

Participants

Participants were 282 MTurk workers. Following exclusions, 257 remained (129 receiving representative items and 128 receiving misleading items), and 176 of those (68.48 percent) completed a delayed posttest occurring 6–9 ($M = 8.59$) days later.

Design, procedure, and materials

Of four (2 × 2) experimental conditions, two received nine representative statistics (exhibited in Ranney, Munnich, and Lamprey, 2016, pp. 158–159) and two received eight misleading statistics (described in Ranney and Clark, 2016). The "Sandwich Representative" and "Sandwich Misleading" groups received both a pretest and immediate posttest; the pretest was omitted for the "Open Representative" and "Open Misleading" groups to assess/counter any effects of perceived experimenter demand. (The Sandwich/Open analogy maps a test to a bread slice; a "sandwich" procedural phase has pretest *and* posttest "slices," whereas the no-pretest procedure here represents an "open" sandwich.) For misleading conditions, Phase 1 (i.e., the pre-delay phase) ended with a debriefing on the facts behind GW, which was meant to reverse any induced beliefs denying climate change.

Participants' attitudes and mindsets were assessed during the pretest (for the Sandwich groups), immediate posttest, and delayed posttest. The RTMD surveys used the nine-point scale described above, while the mindset survey used its six-point scale.

For each statistical quantity/item, participants were prompted to (a) estimate its value, (b) state the maximum and minimum values they would find surprising were the true number to fall outside of that range (a "nonsurprise interval"; Ranney, Munnich, and Lamprey, 2016), and (c) rate their confidence regarding their estimate. After receiving the true value, participants were also shown their original estimate and prompted to rate their surprise level on a nine-point scale. Finally, participants were queried regarding embarrassment(s) at any divergence between their estimate and the true value.

Results and discussion

Supported predictions re: Changed global warming beliefs

Ranney and Clark's (2016) central results were both replicated and extended, in terms of effect-durability. The Representative intervention replicably produced an increase in GW acceptance, while the Misleading intervention led to a decrease. Both shifts were statistically significant and numerically greater than those from Ranney and Clark's (2016): the increase was 22 percent of the possible gain (+.62: 6.79 vs. 6.17 on the nine-point scale; $t(64) = 8.069$, $p < .0001$) and larger than Ranney and Clark's 0.4-point increase (in their [representative] Experiment 6), while the *decrease* was 13 percent of the possible *loss* (−.74: 6.02 vs. 6.76; $t(59) = -6.461$, $p < .0001$)—and a larger loss compared to Ranney and Clark's 0.3-point reduction (in their [misleading] Experiment 7). Explanations for the larger upward-and-downward shifts include (a) improved methodology, (b) larger samples yielding more reliable effect-sizes, and (c) a different, less homogeneous, participant population for the Misleading sample (compared to Ranney & Clark's Experiment 7 Berkeley undergraduates). The representative statistics were compelling: for *every* rare Sandwich Representative participant who decreased GW acceptance, *eight* Sandwich Representative participants *increased* their GW acceptance.

No significant decay in the heightened GW acceptance occurred between the immediate and the delayed posttest scores for both of the Representative groups, showing that the shift produced by the Representative statistics remained stable over 9 days. Indeed, the Representative statistics had still reduced the "room to improve" regarding GW beliefs by 19 percent even after nine days (also $p < .0001$)—barely less than the 20 percent reduction noted on the *immediate* posttest over one week earlier. (As hoped, and consistent with the effectiveness of receiving representative scientific GW information, significant *increases* in acceptance over the retention period were observed for *misleading* participants following the immediate posttest—due to the debriefing provided.)

In more complex data analyses using mixed-effects models, GW acceptance changes were even further explained with participants' (a) surprise ratings upon receiving the statistics' true feedback values, and (b) data from eleven demographic items (gender, age, party, religion, education, income, etc.). Space constraints prohibit elaboration, but we have noted surprise's crucial role (e.g., that greater surprise yields greater belief change; Ranney, Munnich, and Lamprey, 2016; Munnich and Ranney, 2019), and many have noted demographic variables affecting GW acceptance (e.g., McCright and Dunlap, 2011).

Partially supported predictions re: Mindset changes

Comparing pretest and immediate posttest scores, a statistically significant gain in growth mindset was found for the Sandwich Representative group, with a mean increase from 23.05 to 24.18 (out of 48; $t(64) = -3.034$, $p < .005$). However, by the delayed posttest, mindset nonsignificantly differed from pretest levels. Thus, the predicted increase in growth mindsets due to the representative statistics were only observed in the sandwich (and not the open) configuration, suggesting both (a) a metacognitive element (perhaps including experimenter demand), and (b) that the mindset change was not durable. (As noted above regarding the GW acceptance data, a more complex data analysis also showed that utilizing one's feedback-surprise rating and demographic information improved our models' explanatory power regarding mindset change.)

Once again, no polarization is observed

Polarization was absent in participants' changed GW views. Far from exhibiting a backfire effect, conservative participants receiving representative statistics showed mean GW gains at *each* conservative level (i.e., separately for those self-rating conservatism as 6, 7, 8, or 9). Likewise, liberal participants receiving the *misleading* statistics showed mean GW *losses* at *each* liberal level (i.e., separately for those self-rating as 1, 2, 3, or 4 on the 9-point conservatism scale). Indeed, both results are the same for participants for two independent self-ratings regarding economic *and* social conservatism—effectively representing an internal replication.

These findings again *refute* (1) claims that polarization occurs when people receive (at least highly germane) scientific information and (2) that such information is inert in conveying GW's reality to the public. Our interventions "floated all boats" in the directions intended, whether the information direction was representative or misleading.

Interim conclusion

Experiments 1 and 2 further demonstrated how germane, compelling, climate change information directly affects GW acceptance—in the spirit of Ranney and Clark's (2016) experiments and Mill's (e.g., 1843/2006) empiricism. In contrast, Experiment 3 breaks new empirical ground—assessing an *indirect* way to change GW attitudes.

Experiment 3: Changing GW beliefs with supra-nationalistic (and super-nationalistic) statistics

As noted above, Ranney (2012; Ranney and Thanukos, 2011, etc.) developed the RTMD theory to explain the low proportion of Americans who accept GW, relative to peer (e.g., Organisation for Economic Co-operation and Development [OECD]) nations. When the theory was conceived almost a decade ago, it was assumed that Figure 4.1's three more peripheral constructs—the acceptance of an afterlife, of high nationalism, and (their negative associate) of GW—would exhibit more modest correlations (in absolute value) among those three, compared to the remaining (12) interconstruct correlations. Early surveys suggested this to be the case, as the initial correlation between nationalism and GW acceptance across two US samples was about −.2 (Ranney, 2012).[7] Since then, though, the negative correlation between nationalism and GW acceptance seems larger in absolute value: for instance, in Experiment 1 described above, the correlation was −.43, nationalism's highest correlate between it and other RTMD constructs (and even stronger than the nationalism-deity relationship, etc., despite Figure 4.1's configuration)—which was also true in Experiment 2. GW's connection to nationalism now seems to rival the (inverse) strength of GW's connection to evolution acceptance (which was +.44 in Experiment 1), even though it was analogical aspects/relationships noticed between evolution and GW that produced RTMD theory (Ranney, 2012; Ranney and Thanukos, 2011).

Consistent with Mill's (1843/2006) interest in a contrastive study of nations, RTMD theory's central gist proposes that Americans commonly implicitly believe the United States to be the country most reinforced (for over a century) for believing that God is on its side—due to economic, military, and other successes. This high nationalism has likely been modulated by (a) often false assertions implying that the United States ranks #1 in virtually every desirable category (e.g., Congress members calling America's health care system Earth's best; cf. Davis et al., 2014) and (b) nonrandom associations among nationalism, environmental concern, and US political parties' platform-planks (e.g., the 2008 election's connection between the "Country First" slogan and the fossil-fuel-friendly "Drill, Baby, Drill" slogan at one party's political convention). More recently, the US political rhetoric even connects free-market economic positions with climate change (e.g., Lewandowsky, Gignac, and Vaughan 2013) and nationalism, such that those preferring pro-environment regulations are sometimes deemed "job-killers" and/or "anti-American."

RTMD was initially proposed based upon a set of incomplete, extant findings in the (often sociological) literature. Our laboratory's first formal surveys confirmed the directions of the predicted fifteen correlations among American participants. Experiment 3 (Luong, 2015; Teicheira, 2015) was designed to assess RTMD as a *causal* model, moving beyond correlative-explanatory accounts. This required that we manipulate a relatively benign construct to determine its effect on other constructs—particularly GW acceptance, given its societal importance. (Our laboratory already *directly* manipulated GW acceptance—as in Experiments 1 and 2, and in Ranney and Clark, 2016, etc.) Thus, this experiment manipulated nationalism level, predicting that *GW* acceptance would be indirectly changed; that is, we (partially) tested RTMD's causality by examining whether altering Americans' degrees of nationalism would drive GW acceptance changes.

It seems worth noting that our hypothesis that manipulating people's nationalism levels would change their GW acceptance is nontrivially novel, and prior to Experiment 3 (and Experiment 4), various colleagues were skeptical that the connection would prove causal—or that we could alter levels of nationalism at all. Given the past success of the NDI paradigm, though, we further hypothesized that we could bi-manipulate nationalism levels by respectively providing participants with statistics that were either pro-nationalist (e.g., on dimensions in which the United States's ranking is flattering) or *supra*-nationalist (i.e., intended to lessen super-nationalism—e.g., using dimensions in which the United States's ranking is unflattering).

Method

The experimental paradigm resembled Experiment 2's in that compelling statistics were offered to manipulate beliefs—in this case, regarding nationalistic beliefs. However, the two studies' specific methods diverged markedly in the paradigm's implementation.

Participants

Participants were US-resident MTurk workers, and 227 (35–61 per various conditions) completed the full study following exclusions.

Design, procedure, and materials

Participants were assigned to one of five groups. Groups A, B, and C received a pretest; after 12 days, they completed the study's remainder. Groups D and E received no pretest or pretreatment questionnaire, in part to assess and (if needed) account for experimenter-demand possibilities. Group A, the control group, received no treatment. Groups B and D received the pro-nationalism (+Nat) treatment, while groups C and E received the supra-nationalism (−Nat) treatment. Thus, B and C were "Sandwich" groups, and D and E were "Open" groups; Group A's mere pretest-posttest "empty sandwich" incarnation was to control for any scientific/political events occurring during the testing epoch. All groups received a posttest, and Table 4.2 illustrates the overall design.

The pretest and posttest each included a thirty-three-item survey assessing participants' attitudes regarding the six RTMD constructs. Responses used the preceding experiments' 1–9 scale. Both testings included "attention check" items, and many items, as before, were reverse-coded. Identical demographics questionnaires followed both testings.

The intervention included nine statistics chosen either to enhance pro-US nationalism (reinforcing perceptions of the United States as the world's "greatest" nation) or to reduce nationalism (employing data showing the US ranking below

Table 4.2 Experiment 3's abstracted design

		Pretest and Posttest Groups			No-Pretest Groups	
		Group A	Group B	Group C	Group D	Group E
Time 1	Test	Pretest	Pretest	Pretest		
Time 2	Statistics Provided	none (control)	+Nat	−Nat	+Nat	−Nat
	Test	Posttest	Posttest	Posttest	Posttest	Posttest

various developed nations). As in Experiment 2, participants estimated each quantity and later rated their level of surprise upon receiving each feedback value (but on a 1–5 scale, as in Munnich, Ranney, and Song, 2007). The pro-nationalist statistics favorably compared the United States to all the world's nations, whereas the supra-nationalist statistics generally compared the United States to forty-two "peer nations" (roughly aligned to the "First World")—yielding measures ranking the United States below many countries. Statistical phrasings were intended to be as apolitical as practical/possible, and the nine supra-nationalist statistics appear in Ranney, Munnich, and Lamprey (2016, p. 166, e.g., involving Americans' weight, debt, homicides, teen pregnancies, etc.).

Results and discussion

Primary analyses

The experiment successfully demonstrated that feedback on participants' statistical estimates altered their nationalism levels. Group C's supra-nationalist treatment significantly decreased nationalism by 10 percent of the possible drop (5.14 to 4.73, $t(34) = -3.3127$, $p = .0022$) and significantly increased GW acceptance by 10 percent of the possible rise (6.54 to 6.79, $t(34) = 3.441$, $p = .0015$). The pro-nationalist treatment for Group B produced a marginally significant nationalism increase (+.27 points, $p = .0649$)—and an imputed analysis involving Group D indicated a significant nationalism increase (+.39 points, $p = .0257$)—but predicted GW decreases were not statistically significant. The greater effectiveness of the supra-nationalist treatment may be attributed to the comparative familiarity of pro-nationalist arguments among many Americans (e.g., the United States having the largest GDP), as discussed more below. Two-way repeated analyses of variance (ANOVAs) confirmed, as predicted, a significant nationalism decrease and a significant GW acceptance increase in Group C, as well as a relative nationalism decrease in Group B (i.e., a significant Group × Test-time interaction was observed for nationalism: $F(1,69) = 13.04$; $p = .006$).[8]

As in prior studies, polarization was *not* observed. Conservative Group C participants—who received supra-nationalist statistics—increased their GW acceptance, as did C's liberal participants. Thus, the intervention boosting liberals' climate change beliefs did *not* have the opposite effect for conservatives.

Matching prior studies, the interconstruct correlations supported RTMD's hypothesized associations. *All* thirty correlations (fifteen pretest and fifteen posttest) were statistically significant in the directions RTMD predicts. The highest correlations (all $p < .00005$), in absolute-value terms, were Creation-Evolution (pretest: −.875; posttest: −.849), Deity-Creation (pretest: .861; posttest: .879), and Afterlife-Deity (pretest: .888; posttest: .839). Two correlations weakened significantly from pretest to posttest, in absolute terms: GW-Creation (−.603 to −.445, $p = .024$)—which may encourage communicators wishing to decouple climate change from religion or even highlight the latter's stewardship message—and Deity-Afterlife (.888 to .839, $p = .045$). Confirming similar findings that motivated this experiment, the nationalism–GW correlation was robustly about −.35 for both testings.

Secondary analyses

Posttest (Groups A–E) data further analyzed with fixed-effects models demonstrated that gender, age, political party, religion, and conservatism variables, when added to condition-type (i.e., pro-nationalist-, or supra-nationalist-, or no-intervention) as predictors, explained the greatest variance-percentage in both nationalism (22.2 percent) and GW acceptance (36.1 percent). The models indicated that women tended to be significantly less nationalistic than men, Libertarians were significantly less nationalistic than Democrats, and Independents were generally intermediate between Democrats and Libertarians in nationalism; all else being equal, the models indicated that Libertarians and Conservatives accepted GW less strongly than, respectively, Democrats and Liberals.

Groups receiving both a pretest and posttest (Groups A, B, and C) were yet further analyzed with mixed-effects models to assess the influence of Test (testing time) on nationalism and GW attitudes. The model employing Test, Group, the Test*Group interaction, and the demographics best explained nationalism and GW acceptance, with the *Test* predictor (a random effect in the model) representing time between pretest and posttest. Among the models including participants' *surprise-ratings* (thus excluding no-treatment control Group A participants, who could not experience surprise), the full model with three two-way interactions (namely, *Demographics + Test + Group + Surprise + Test*Group + Test*Surprise + Group*Surprise*) fit best for nationalism and GW acceptance.

Significant pretest-to-posttest belief changes among RTMD constructs other than nationalism and GW could result from nationalism/GW changes impacting participants' sense of personal control—and the acceptance of awe-related experiences relating to afterlife and creation. One's control-sense could decline as one increasingly accepts GW (e.g., ruminating about how GW will affect humanity, one's self, or one's family)—producing fear, and threat/danger feelings

(Keltner and Haidt, 2003). Rutjens, van der Pligt, and Harreveld (2010) found that in losing a sense of personal control, one's acceptance of God-related roles increases to restore conceptual order. Therefore, boosting GW acceptance (by reducing nationalism) could decrease Group C's control-perception, increasing religious beliefs. However, Group B's increased nationalism (albeit a smaller effect) could raise participants' sense of control, perhaps inhibiting religious beliefs (given less need to cling to awe-related experiences), and enhancing proximal religion-conflicting scientific notions like evolution. Supporting evidence for this perspective is that (a) Group C's supra-nationalist treatment increased Afterlife acceptance (4.9→5.3, $t(34) = -2.682$, $p = .01$), whereas (b) Group B's pro-nationalist treatment increased Evolution acceptance (6.65→6.98, $t(35) = -3.004$, $p = .005$) and decreased Creation acceptance (4.46→4.09, $t(35) = 2.439$, $p = .02$). These results suggest that increasing GW acceptance with a more balanced view of a nation's strengths/shortcomings may increase a population's acceptance of applied-metaphysics beliefs involving religion, whereas focusing on national strengths may reduce creation beliefs' attractiveness. Accordingly, rather than heightened GW acceptance shaking a society's religious foundations, it might *enhance* them.

Interim conclusions

The results of the pro-nationalism and supra-nationalism interventions showed that estimated statistics followed by true-value feedback successfully changed nationalism levels. The supra-nationalist intervention's significant effects—reducing nationalism and thus boosting GW acceptance—impacted participants' nationalism more than did the pro-nationalist intervention, which did not reduce GW acceptance. This asymmetry may arise from Americans commonly hearing about US excellence, perhaps desensitizing many to additional pro-nationalistic evidence. Americans less frequently hear unflattering rankings versus peer nations, potentially explaining the supra-nationalist intervention's (e.g., Group C's) apparently greater impacts.

All thirty correlations among the six constructs over two testings were significant in the RTMD-predicted directions: GW and Evolution acceptance positively correlated, yet each negatively correlated with Nationalism, Creation, Deity and Afterlife acceptance. (The latter four constructs were positively correlated among themselves.) Again, the fifteen RTMD-predicted correlations persisted even after RTMD's nationalism construct was manipulated.

Experiment 3's considerations regarding the character of US society, among the constructs examined, seem generally coherent with the national belief-change considerations Mill (1843/2006) pondered. Decreasing one's nationalism attitudes (as with Condition C) represents a sixth way our laboratory has found to increase GW acceptance. To begin a new program of comparing the interventional efficacy among such interventions (in a *Consumer Reports* spirit), Experiment 4 sought to contrast three such ways—often in varying "dosages."

Experiment 4: Increasing GW acceptance with scientific statistics, texts, and videos

This larger, ambitious experiment simultaneously compared the effectiveness of dosages and types of brief interventions—and relevant control conditions. It also further demonstrated that quick climate instruction of various types durably increases Americans' acceptance that GW is occurring and that humankind contributes to GW. Participants received one of eleven main conditions across four types: (a) three text and (b) five video interventions that explain GW's mechanism[9] to varying degrees, (c) a replication of Experiment 2's representative-statistics' effects, and (d) two control conditions. Experiment 3's findings inspired a secondary hypothesis that increasing GW acceptance *causes* a nationalism *decrease*.

Analyses of pretests, immediate posttests, and delayed posttests roughly 9 days later showed that overall GW knowledge and acceptance had increased, with the longer-duration interventions generally resulting in larger and more durable gains. As noted below, even on the delayed posttest, every condition except the nonintervention control group showed statistically significant increases in mechanistic knowledge (cf. Thacker and Sinatra, 2019)—including the statistics condition and a unique control condition containing a "vacuous" video bereft of mechanistic explanation. Among the mechanistic interventions, those leading to greater knowledge increases generally led to greater acceptance increases. The results *disconfirm* the "science-impervious" stasis theory implicitly proposed by Kahan et al. (2012)—even beyond the disconfirmations demonstrated in Experiments 1–3 above, in Ranney and Clark's (2016) experiments, and in Ranney, Munnich, and Lamprey (2016). Further, analyses of demographic and other data yet again revealed no evidence of polarization.

Method

Experiment 4's paradigm generally resembled Experiment 2's: information was presented to change GW beliefs. The two studies' particular methods differed markedly, however, in implementation.

Participants

After 24 percent of 1447 original MTurk workers were excluded due to attention- and coherence-checks, time cut-offs, audiovisual issues, noncompletion, or several other criteria, 1103 participants' data were analyzed. Roughly half the participants' party affiliations were Republican, Libertarian, Independent, or Other—the other half representing Democratic, Green, or None.

Design, procedure, and materials

Ten interventions (including a control video)—and an eleventh, no-intervention control condition—were designed. Eight interventions included GW mechanistic instruction (varying in length and modality), one included Experiment 2's representative statistical GW information, and one was a vacuous control video offering neither mechanism nor statistics. Three mechanistic interventions were text-based: 35, 400, and 596-word written explanations. Five mechanistic interventions were videos under 1, 2, 3, 4, and 5 minutes in duration (the latter using the 596-word text as its script). Other than the control video, the interventions are at www.HowGlobalWarmingWorks.org (Ranney and Lamprey, 2013–present).

Ranney, Clark, Reinholz, and Cohen (2012b) first published the 400-word GW mechanistic explanation and its 35-word summary. Ranney et al. (2012a) and Ranney and Clark (2016) showed that the 400 words both dramatically increased participants' knowledge (consistent with Thacker and Sinatra's [2019] results) and increased anthropogenic GW *acceptance*. The 35-word text is as follows: "Earth transforms sunlight's visible light energy into infrared light energy, which leaves Earth slowly because it is absorbed by greenhouse gases. When people produce greenhouse gases, energy leaves Earth even more slowly—raising Earth's temperature."

The 596- and 400-word explanations differ (see HowGlobalWarmingWorks.org) in several ways, including added conversational elements designed to (1) inhibit self-charitable ("knew it all along") hindsight bias, (2) evoke GW's scientific consensus, (3) assure participants that *many* cannot explain GW's mechanism, and (4) motivate participants (e.g., "how can we make informed decisions without understanding the issues we're debating?"). All videos employed (a) measured, well-enunciated, narration, (b) key visual text/labels, (c) simple diagrams/animations tightly mirroring the audio, and (d) minimal peripheral design, to reduce cognitive load and keep viewers' attention on crucial aspects. Four shorter video versions were created by *nonmonotonically* reducing the 5-minute video (e.g., the 1-minute version includes phrases that are not in the 2-minute version) to those ranging from approximately 1–4 minutes.

A sixth video (Control 1) devoid of climate-scientific information was created as the first control condition by deleting mechanistic explanations and climatic evidence from the longest video. However, it included the four (1–4) elements listed above, and several other sentences from that video. The other control condition (Control 2) provided no intervention. The nine-statistics intervention was essentially the same as in Experiment 2's Representative conditions, effectively serving as a replication for that experiment—and for Ranney and Clark's (2016) Experiment 6.

Participants completed a pretest before randomly receiving one of the nine experimental interventions (text, video, statistics), Control 1, or Control 2. Half the participants (other than those in Control 2) completed an immediate posttest, yielding twenty-one total conditions. Most (68.9 percent) participants completed a delayed posttest 6–9 days later.

Each testing included GW items (generally randomized in order), including those assessing knowledge of, acceptance of, and concern about GW—as well as attitudes regarding RTMD constructs. The immediate and delayed posttests further included demographic items and items about the intervention.

During testings, participants provided written explanations regarding three aspects of GW's mechanism. The three responses per test were coded and scored together as a "response set" by two condition-blind and testing-time-blind humans using coding and scoring rubrics (Ranney and Clark, 2016, provides these, and intercoder reliabilities were comparable). A given participant's tests were all scored by the same coders, so apparent knowledge changes could not be attributed to intercoder differences.

Results and discussion

The results clearly supported a primary hypothesis—that learning scientific GW information would durably increase participants' acceptance (i.e., decrease their denial) of GW and humankind's contribution to GW. Participants exposed to short mechanistic/physical-chemical climate change instruction, or the nonmechanistic nine highly germane statistical facts, generally showed a GW acceptance increase; those participants receiving one of our nine noncontrol interventions exhibited a mean acceptance gain of 10.57 percent of the room to increase on the immediate posttest ($t(486) = 7.78$, $p < 2.1 \times 10^{-15}$), and a similarly robust 7.47 percent gain even nine days later on the delayed posttest ($t(684) = 6.73$, $p < 2.5 \times 10^{-11}$). Even the nine-statistics (i.e., "nonmechanistic") intervention, on its own, replicated Ranney and Clark (2016's Experiment 6) *and* Experiment 2's Representative effects—yielding 15.06 percent and 10.97 percent denial reductions on the immediate and delayed posttests (respectively: $t(60) = 3.0$, $p < .005$; $t(89) = 3.35$, $p < .001$).

Longer interventions tended to yield larger acceptance increases. The highest GW acceptance gains generally resulted from one of the three longest interventions: the 5-minute video, the 596-word text, or the nine statistics. The largest gains from pretest to *immediate* posttest resulted from the 5-minute video (20.7 percent: $t(49) = 4.58$, $p < 1.6 \times 10^{-5}$) and the nine statistics (15.1 percent: $t(60) = 3.0$, $p < .005$)—followed by the 596-word text (+11.2 percent, p < .01, which virtually tied with the 4-minute [+11.5 percent, p < .005] video). The three longest-per-mode interventions accounted for the three highest gains between pretest and *delayed* posttest, and all remained statistically significant. The 596-word text's gain was numerically the greatest after the 9 days (a robust +13.6 percent, p = .001)—its high durability perhaps reflecting the greater vividness of individuals' own mental imagery. The shortest intervention (or lowest "dose"), the 35-word text, unsurprisingly resulted in some of the smallest gains, and its result was not statistically significant on the delayed posttest, although even the 35 words yielded a marginally significant gain on the immediate posttest. In sum, we observed remarkably robust effects: For each intervention that statistically significantly decreased denial upon immediate posttesting, not one had statistically significantly *less* denial-reduction upon *delayed* posttesting. Further, the 35 words was the *only* of the nine interventions that did not obtain at least a marginally significant gain in GW acceptance after 9 days (including our original 400 words [p = .025], replicating many prior experiments' findings, e.g., Ranney and Clark, 2016).

Regarding enhancing people's GW beliefs, Experiment 4's dosage trend illustrates the importance of maintaining people's attention for longer periods (at least up to 5 minutes or 596 words; further research will determine how much more still longer interventions—e.g., Ranney and Clark's, 2016, Experiment 5—enhance effectiveness). Whether in antibiotics or cognition, more complete interventions usually outperform alternatives. While the 35-word text is better than no intervention (and useful when rehearsed and/or reflected upon), it is suboptimal for delivering practically significant GW instruction—unless its brevity facilitates providing it to a larger segment of humanity or with repetitiveness (e.g., through many 10-second Super Bowl or World Cup commercials). (Ranney, in Ranney, Munnich, and Lamprey, 2016, p. 139, wrote a thirteen-word haiku/sentence that highly concentrates GW's mechanistic description.) However, the 596-word text, the 5-minute video (Ranney et al., 2013), and the nine statistics are superior and worth the investments when possible.

The mechanistic interventions also led to large and statistically significant knowledge increases—replicating all prior experiments that employed that scientific explanation (e.g., Ranney and Clark, 2016's Experiments 2–5, and Thacker and Sinatra's recent 2019 study). The longer, more detailed explanations that tended to change GW mechanistic knowledge scores the most also tended to yield larger changes in GW acceptance. Knowledge reliably predicted acceptance at all three test times (using fixed-effects models; $p < .01$), which again (Ranney and Clark, 2016) disconfirms claims (e.g., Kahan et al., 2012) of no such relationship. The correlation between knowledge-score gains and acceptance gains was hardly perfect—perhaps because merely *recognizing* the scientific reasoning/evidence undergirding GW can enhance one's acceptance. That is, participants may recall *following* the GW explanation, *understanding* it, and *believing* it, without necessarily retaining (all) its details. This result seems satisfactory, rather like accepting "regression to the mean" without necessarily re-proving it for each use.

Replicating this chapter's prior experiments—and Ranney and Clark (2016)—Experiment 4 found no polarization. Indeed, conservatives increased their GW acceptance *more* than liberals; for instance, acceptance gains at the *delayed* posttest were significantly *positively* correlated with both conservatism measures (economic: $p < .03$; social: $p < .04$), and conservatives' gains averaged 70 percent larger than liberals' (across economic and social measures). Furthermore, at each conservatism self-rating level (from 1 to 9, including "9," extreme conservatism), participants evidenced acceptance gains. We believe such results stem from our interventions avoiding (sometimes counterbalancing) polemics or "quasi-propaganda" (even if/when the information is true) that are not rare in social psychology "vignettes"—stimuli that could possibly spawn polarization, such as language evoking inferences that experimenters may have political agendas. We focus on science and statistics, but others' interventions may offer hints of bias.

The aforementioned secondary hypothesis was also obtained, showing that nationalism and GW acceptance are *bidirectionally* causal (building on Experiment 3's result that decreasing super-nationalism increases GW acceptance). Combining all of Experiment 4's noncontrol conditions, nationalism significantly decreased (−.09 points; $p < .006$) on the immediate posttest, with the nine-statistics condition being a large contributor, even in isolation (−.26, $p < .04$). This GW-*supra*nationalism effect was largely temporally labile; few conditions showed significant nationalism decreases upon delayed posttesting, with the curious exception of the 35-word condition: −.26, $p = .004$. (A related, and predicted, "learning reinforcement" finding is that intervention conditions utilizing an immediate posttest yielded roughly double the GW increase on the *delayed* posttest than those without an immediate posttest [.24 vs. .12; $p < .001$]—so a *seemingly* diluted effect on nationalism-reduction 9 days later was expected.)

Our findings yet again disconfirm the science-impervious stasis theory that some ascribe to large swathes of Americans (implicitly or not, e.g., Kahan et al., 2012), which asserts that scientific knowledge interventions cannot increase GW acceptance, and that climate science instruction is futile or potentially counterproductive. As RTMD theory explicates, though, there are obviously potent factors (e.g., economic considerations, religious narratives, and political agendas) inhibiting GW acceptance that offer *some* resistance to scientific explanations/information (Ranney, 2012; Ranney and Thanukos, 2011). As aforementioned, such influences upon national (here, American) character is part of the research program Mill (1843/2006) seemed to call for. Fortunately for Mill's sense of progress, in Experiment 4, the receipt of scientific information was, yet again, neither insignificant nor polarizing. The findings of Experiment 4, Ranney et al. (2012a), and Ranney and Clark (2016) show that scientific interventions describing the mechanism of—or (as also in Experiment 2) representative statistical evidence regarding—GW can significantly increase climate change acceptance. These findings further *disconfirm* the stasis/science-impervious theory, highlighting the value of understanding effective interventions' characteristics.

General discussion

This chapter's four experiments, combined with Ranney and Clark's (2016) experiments, represent ten incarnations from our laboratory alone that disconfirm the stasis view that "people won't change when provided scientific information." (We have at least three new experiments that disconfirm similarly—yielding 13 or more in total, without even counting recent studies with foreign collaborators, e.g., Arnold et al., 2014.) Experiments 1–4 add to a growing set of studies demonstrating a relationship between representative scientific knowledge and GW acceptance (e.g., Shi et al., 2016)—even for the half of Experiment 2's GW stimuli that were *misleading*-yet-veridical—and even though Experiment 3's "scientific" intervention regarded America's *international status*. Furthermore, as found in our past experiments, Experiments 1–4 achieved these significant GW-denial reductions without polarizing (even economic) conservatives from liberals. Experiment 4 even demonstrated that Experiment 3's (supra-)nationalism→GW causality is *bi*directional, with increased GW acceptance (perhaps more modestly) causing decreased nationalism (i.e., exhibiting the GW→supra-nationalism direction too).

Recently—and cohering with our experiments' data (e.g., Experiment 4; Ranney, Munnich, and Lamprey, 2016's data, etc.)—Urban and Havranek (2017) independently confirmed that our 5-minute GW-mechanism video increases objective (and subjective) mechanistic knowledge. (Finding a preinstruction mechanistic GW knowledge dearth, as we always do, they also found mis-calibrated impressions of individuals' understandings of GW's mechanism: those in the highest percentiles were least likely to realize that they understood it more than their peers.)

Regarding empiricists like J. S. Mill, our collective results further support a fundamental empirical notion that, even for a relatively divisive topic, merely providing short types of informative interventions (from a growing handful of such interventions) independently increases GW acceptance.[10] When one includes our laboratory's most recent finding (which space constraints prohibit us from exhibiting here) that information regarding sea-level rise also increases GW acceptance (Velautham, Ranney, and Brow, 2019), we have now demonstrated six such productive brief (e.g., roughly 5 minutes or less) information types: (1) contrastive empirical graphs, (2) empirical statistics about GW's effects (and consensus), (3) supra-nationalist empirical statistics, (4) sea-level rise information, (5) mechanistic texts, and (6) mechanistic videos. (Most of these interventions, in full or part, are available at our website, HowGlobalWarmingWorks.org; e.g., Ranney, Chang, and Lamprey, 2016.) Our laboratory is currently piloting more such interventions, including (a) why the public can/should generally trust climate scientists, (b) the flimsiness of claims that climate change is a hoax, and (c) how beneficial, both economically and in terms of health, it is for humans to switch to sustainable fuels. We note that our indirect intervention—employing supra-nationalist statistics—seems more modestly effective in increasing GW acceptance than our more direct interventions. (Similarly, we found that boosting participants' GW acceptance relatively modestly decreased their super-nationalism.)

Many ask, "Why do some deny climate change?" We answer by proposing a metaphor—a "table of denial" supported by roughly a dozen reasons or "legs."

Someone mostly denying GW may do so for only a few such reasons. They include ignorance of GW's mechanism[11], effects, or scientific consensus—which our research program addresses with our texts, videos, representative statistics (e.g., Experiments 1, 2, and 4), and (more recently and somewhat indirectly) information on sea-level rise (Velautham, Ranney, and Brow, 2019). Another "table-leg" is represented by super-nationalist thinking—including that Americans can "solve climate if need be"—which we (least directly) address with our supra-nationalist statistics (Experiment 3). Yet another denial-leg seems to be the libel that scientists are untrustworthy or hoaxers, which our current research is addressing (also see Edx.org, 2015). Likewise, our newest intervention counters a "leg" that asserts that adopting sustainable fuels is financially or societally problematic. ~~Some~~ Still other denial reasons involve scientific climate misconceptions that we plan to address. Overall, our "table-destabilizing goal" is to generate short, compelling, interventions that can "knock out" each leg of individuals' climate change denial, such that no one's "GW skepticism remains standing" (assuming that science evidence continues supporting GW acceptance!). HowGlobalWarmingWorks.org, our public-outreach website with its hundreds of thousands of pageviews (Ranney, Munnich, and Lamprey, 2016), represents a central tool in implementing that goal.

Mill's (1843/2006) interest in political ethology—particularly, a science of national character—is one many resonate to. Although few are as learned as Mill was, people are prone to generalize (e.g., "the French are X; the Chinese prefer Y; Brazilians dislike Z; Canadians are polite"). Such stereotypes often overstate national differences, sometimes relying on imprecise heuristics (e.g., availability) or unrepresentative subsamples (e.g., urban vs. rural Chinese/Bangladeshis/Brazilians). Yet Americans *do* diverge from peer nations' residents (e.g., OECD members) on various dimensions (Ranney, 2012), with modest GW acceptance being notably salient and dangerous among anomalous belief-distributions (Ranney and Thanukos, 2011). Regarding climate, humanity cannot afford delays like those preceding the acceptance of heliocentrism, a (largely) spherical Earth, or tobacco-illness connections. The exceptionalist/divergent national character exhibited by a large segment of the US population/legislatures may push Earth toward great extinctions and climate change before wisdom can expediently vanquish ignorance (Ranney, Munnich, and Lamprey, 2016).

Time is fleeting—as Anthropocene extinctions increase. We fervently hope that humans will fully comprehend anthropogenic GW extremely rapidly and collectively act to quickly inhibit climate change as optimally as possible.

The experiments above further demonstrate that accurate, representative[12], scientific information can increase the portion of the populace who accept GW's existence, magnitude, and/or threat-level—without polarization. HowGlobalWarmingWorks.org, our direct-to-the-public site for providing such information, is increasingly "international"—with burgeoning translations[13] of its texts, videos, and so on (e.g., in Mandarin, German, Czech, Spanish, Japanese, among several others); given that Earth's slender gaseous envelope is a transnational resource, we hope nations will communally redouble their efforts to address GW's potentially existential threat. Mill's century was essentially naïve concerning this threat, and largely presumed boundless frontier resources—including a rather rapidly self-cleaning ("winds will come") atmosphere. But Mill (1848/1965) saw (a) boundless growth as a danger to ecological systems, and (b) utility in nongrowing states of capital, wealth, and population. Although he quasi-celebrated diversity in national character (1843/2006), a twenty-first-century Mill would likely sacrifice bits of that diversity and urge countries to immediately reduce international variance on the dimension of sustainability. A mortal enemy of sustainability in *this* millennium is climate change.

Acknowledgments

We especially thank Charles Chang, Tina Luong, Tommy Ng, and Justin Teicheira, as well as Oliver Arnold/Taube, Lloyd Goldwasser, Jan Urban, Elijah Millgram, Florian Kaiser, Leela Velautham, Emily Harrison, Lukas Gierth, Karen Draney, Sophia Rabe-Hesketh, Daniel Wilkenfeld, Richard Samuels, Sean Li, and all of Berkeley's Reasoning Group. This research was partially supported by a bequest from the estate of Professor Barbara White.

Notes

1 Probably not surprisingly for a man of the time, Mill's writing had racist (p. 921) elements.

2 In the present chapter, *empiricist* and *rationalist* are not intended to closely track traditional philosophical usage. Instead, we use them in accord with the meanings that they have often had in psychological research (e.g., Wertheimer, 1987), and which pertain recently to the public reception of science. Roughly put, by *empiricist processes* we mean processes of belief maintenance and revision that are highly

sensitive to the data and causal models of the sort that scientific research is apt to generate. Furthermore, when saying that people are empiricists, we mean that their beliefs about the world are substantially influenced by such processes. In contrast, when speaking of *rationalistic processes*, we have in mind processes of belief maintenance and revision that are substantially *insensitive* to the data and causal models of the sort that scientific research is apt to generate. When saying that people are (not) rationalists, we mean that their beliefs about the world are (not) largely or entirely a product of such insensitive processes. So construed, it should be clear that the empiricist-rationalist distinction is both imprecise and admits of degree. Nevertheless, it is, as we will see, a useful way to organize recent debate regarding the popular reception of science, especially at it pertains to global warming (e.g., Ranney and Clark, 2016, and van der Linden et al., 2017 vs. Kahan et al., 2012). In essence, the distinction relates to the popular but oft-disconfirmed hypothesis, "People just believe what they want to believe." (One can argue that researchers who repeat this easily disconfirmed hypothesis—e.g., in suggesting that culture completely drives climate change beliefs—indirectly give comfort to those seeking to delay global warming mitigation due to personal financial interest.)

3 We thank the editors and Lije Millgram for pointing this out.

4 In philosophy, near-neighbor concepts to this extreme view might include the loyalty one finds in political noncognitivism (Millgram, 2005), religious credence (Van Leeuwen, 2014), and echo chambers (Nguyen, 2018). As the first author has pointed out elsewhere (e.g., Ranney, 2012), "facts on the ground" will likely overwhelm those denying global warming today, just as evidence such as circumnavigation and so on overwhelmed deniers of heliocentrism and/or a spherical earth in the last millennium.

5 It is clearly inaccurate both worldwide (e.g., the Paris accord) and over the long term (e.g., 1960s United States vs. today).

6 Our 1–9 scale coheres with Mill's anticipation of such measures: "beliefs . . . and the degree of assurance with which those beliefs are held" (1843/2006, p. 912).

7 A Canadian pilot study in Ontario even showed a nonsignificant positive correlation of +.08 between nationalism and global warming acceptance—probably due to a contrast effect with how the United States was viewed. This nonnegative correlation that may have already changed due to Canadian tar sand exploitation. Furthermore, Ontario differs considerably from, say, the more conservative and energy-producing Alberta, and *every* US study our laboratory had conducted demonstrates a negative nationalism-GW correlation.

8 Group A, receiving no intervention, showed no significant change over the twelve days between pretest and posttest—indicating that no external/news events altered participants' GW or nationalism views during that period.

9 Ranney and Clark (2016) explicate mechanistic information's utility in "breaking ties" between competing positions. For example, imagine asserting toilets'

existence to a skeptic in a culture that never heard of them. Drawing a bowl, a tank, pipes, and so on, and using metaphors of lakes and creeks, might "break the tie," compelling the skeptic.

10 People are clearly not fully/blindly credulous; another experiment in our laboratory, spearheaded by Leela Velautham, shows that participants were able to distinguish between the misleading and representative global warming statistics (i.e., able to identify and discriminate based on accuracy indicators), and were influenced by the informative statistics in that their global warming acceptance increased.

11 As an example of *mechanistic*, leg-destabilizing, persuasion, the first author recently asked a man who believed that increased volcanic activity was causing global warming, "Why is there increased volcanic activity at this point in earth's history?" His vacuous reply, compounded with the receipt of a science-normative (emissions) explanation, caused the man to doubt his volcanic explanation.

12 In yellow journalism's recent US upsurge, reporters and media consumers alike need greater guidance on how to detect "fake news" by assessing what is representative and what is misleading (e.g., Yarnall and Ranney, 2017).

13 Climate change acceptance is hardly unanimous outside of the United States, and even "extreme acceptors" can use help in understanding GW better to more effectively persuade others.

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Advances in Experimental Philosophy of Science

Edited by
Daniel A. Wilkenfeld and Richard Samuels

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