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The Nature and Effects of Uncertainty Frames in Science Communication

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by

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ABSTRACT

The Nature and Effects of Uncertainty Frames in Science Communication

by

Abel Thomas Gustafson

Uncertainty is native to science and thus also to wholly accurate science communication, However, uncertainties that are inevitable in individual findings of science and in larger processes of science are often not clearly communicated to the public. Instead, many public-facing science communicators purposefully avoid discussing the uncertainties that are attached to the science they communicate – often out of fear of adverse effects of those uncertainty frames. To date, it has remained unclear whether these fears are well-founded because much of the extant literature has produced competing theory and findings.

Much of the uncertainty about uncertainty in the extant literature has been caused by inconsistent and uncoordinated conceptual and operational definitions of uncertainty and uncertainty frames. Therefore, this dissertation develops a new and clarifying conceptual explication of distinct uncertainty types, which enables a more nuanced interpretation of the extant empirical literature. Specifically, that frames of *consensus uncertainty* have been associated with *none* of the reported findings of positive effects of uncertainty frames – only negative effects and nonsignificant effects. Conversely, frames of *technical uncertainty* and *scientific uncertainty* frames have been associated with positive and nonsignificant effects. Frames of d*eficient uncertainty* have not been a focus of the extant empirical literature.

However, the summary observations from this informal meta-analytic review are the product of disparate methods, issue contexts, concepts, and measures – all of which are

confounding factors that render confident meta-analytic conclusions impossible. The literature lacked a controlled experiment that compared the effects of each uncertainty frame type within one consistent methodology.

All of these factors together have created a situation where progress toward the answers to a question with universal importance and tangible applications has been obscured despite many uncoordinated efforts occurring within disciplinary silos. Therefore, this dissertation was an effort to move this field of research forward – providing a rigorous and robust set of findings that inform the relative effects of different types of uncertainty frames in science communication, and the role of motivated reasoning in responses to portrayals of uncertainty in science – all tested in one, large, controlled experimental design.

This dissertation employs an online survey experiment in a between-subjects five (frame type) by three (issue context) factorial design to specify and compare the effects of four distinct uncertainty frame types (*consensus*, *deficient*, *scientific*, and *technical* uncertainties, respectively) and one control condition on attitudinal outcome variables of claim belief, credibility perceptions, and behavioral intentions. These tests are replicated in each of three distinct issue contexts: the effects of climate change, the effects of GMO labeling laws, and the occupational hazards posed by vibrating machinery.

Using an opt-in online sample and quotas that approximated U.S. census levels of education, age, and gender, this dissertation asked participants to read a simulated news article that contained a report of new scientific evidence. These claims of new science findings were the experimental manipulations, and as such used a frame of one (or none) of the uncertainty types. After reading their assigned news article, participants then indicated their belief in the claim, credibility perceptions of the scientists, and intentions to engage in relevant behaviors that indicated support for the claim.

By applying a rigorous sequence of explication and validation in the development of the measures and the measurement model, this dissertation demonstrated clear evidence that the measures have strong convergent and discriminant validity. By first establishing the manipulation check with rigorous testing and revision, and the basic structure of the conceptual model with SEM, this dissertation builds a foundation of confidence in conceptual, theoretical, and methodological validity upon which to base interpretations of the later tests of interaction effects.

These tests of conditional effects found that – in the issue contexts of climate change and vibrating machinery – frames of consensus uncertainty are associated with significantly lower belief in the claim and perceptions of credibility of the scientists, compared to other types of uncertainty frames. Importantly, and interestingly, the other three types of uncertainty frames did not significantly differ from each other in their associations with levels of the attitudinal outcome variables. Also, while the tests of the conceptual model demonstrated strong motivated reasoning effects, these effects do not differ across uncertainty frame types. These findings have important implications for theory, research, and practice, and multiple interpretations of them are discussed at length in the latter portions of this dissertation.

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Scientific knowledge is never complete and is never certain. There are always more questions to be answered and more answers to be questioned. As scientists, we encounter phenomena that defy existing theory, so we propose new theories to make sense of the new corpus of data, and then test those theories through those data. In a great many cases, our most certain understandings will be eventually usurped by a new paradigm. Indeed, two leading epistemological perspectives of science are a) that science can conclusively disprove hypotheses but can only provisionally support hypotheses (Popper, 1959) and b) that science moves in paradigmatic cycles, in which new discoveries spark a sudden rejection of longstanding assumptions and introduce new, additional uncertainties (Kuhn, 1970). In both systems, scientific knowledge at any point in time is an effect of myriad contextual forces, not necessarily a reflection of any absolute Truth. Thus, the first major component of inherent uncertainty in science is the competitive, iterative, unending nature of scientific progress, which necessarily breeds conflicting theories and competing hypotheses. These create inevitable disagreements among experts that are not always transient or easily resolved (Shanteau, 2000).

Second, science often reports individual findings by describing the degree of uncertainty surrounding a parameter. That is, we report our findings in terms of values (e.g., mean, standard deviation) that describe a probability distribution. Even this word we use — "parameters" — is formed from the Greek prefix "alongside, accompanying" (*para-*) and noun "measurement" (*metric*). The Pearsonian statistical revolution developed our understanding that these individual parameters are not the true measurement of a phenomenon (Salsburg, 2001). They are "accompanying," "alongside" descriptions of a probability distribution. Statistical science itself is an exercise in specifying uncertainty with as much precision as possible — highly focused on estimations of variation and error.

In sum, uncertainty is central to both the philosophical, epistemological nature of science, and to the working mechanisms of the scientific study. Consequently, wholly accurate science communication is simply a description of the current state of uncertainty in deliberately (but well-justified) uncertain terms (Carpenter, 1995). When science communication occurs *within* the scientific community (e.g., scholarly publications, conferences, research collaboration), portrayals of uncertainty are part of the shared, native language. Similarly, in public-facing science communication (e.g., news media, edutainment, public campaigns) — which is the chosen context of this dissertation — it is important to honestly convey the uncertainties of science, so that the public is not mislead and so that they can make informed decisions (Campbell, 2011; Stocking, 2010).

However, science communication messages that emphasize uncertainty are neither native to the language of public science communicators nor to the language of their audience: the lay public. Thus, there are likely to be *uses* and *effects* of uncertainty portrayals that are unintended, unanticipated, and/or undesirable. For example, despite the centrality of uncertainty to the nature of science and scientific findings, journalists often misrepresent the uncertainties of science by presenting complex or preliminary information as more certain or unambiguous than it truly is (Brechman, Lee, & Cappella, 2009, Lai, Lane, & Ruttenberg, 2009; Retzbach & Maier, 2015). Jensen (2008) summarizes that journalists can distort science by "removing caveats, relying on too few sources, neglecting context, stressing the results over the process, and presenting science as a quest whose future is assured" (p. 349). This is sometimes done to increase clarity and simplicity to accommodate uninitiated lay audiences (Ebeling, 2008). Also, sometimes it is done out of fear of adverse effects (Stocking, 1999). Specifically, scholars in science and environmental communication argue that portrayals of uncertainty in science communication are likely to invite motivated skepticism and confirmation bias in oppositional audiences, to exacerbate or perpetuate the gaps in knowledge, attitudes, and behavior between scientists and the public, and also to erode the credibility of science and scientists.

However, the body of empirical evidence indicates that the assumption of detrimental effects from portrayals of uncertainty is tenuous at best, as it is supported by a body of inconsistent research findings and contradictory theoretical arguments. In this dissertation, I demonstrate that defensible theoretical arguments across diverse communication research fields have posited *negative* effects of uncertainty portrayals in science communication, while many others have argued in expectation of their *positive* effects. Similarly, I will summarize the numerous empirical findings that support the assumption of negative effects of uncertainty in science communication, as well as the robust evidence that contradicts it.

In sum, despite the inherent, inextricable role of uncertainty in even the most accurate communication of science, science communication research has not yet established the nature and extent of the effects that portrayals of uncertain science have on public responses toward science communication, nor the contextual-, individual-, or message-level factors that moderate this relationship. As Miles and Frewer (2003) summarize, “the literature indicates there are various arguments as to why communicating uncertainty is a positive activity, as well as why uncertainty should not be communicated, although there is little empirical evidence to support either view” (p. 268). Fifteen years later, this assessment is still true.

Given the centrality of uncertainty to science and of uncertainty frames to science communication, the competing theoretical predictions about the effects of uncertainty frames, and the mixed findings of the extant empirical evidence, it is imperative to clarify our understanding of the effects of uncertainty frames in science communication. It is also necessary to inform practical strategies for accurately representing the uncertainties of science in ways that can avoid the negative effects observed in some research and consistently realize the positive effects observed in other research. These are the central aims of this dissertation.

One of the primary causes of scholars' current clouded understanding of the effects of science communication messages that emphasize uncertainty is inconsistency and imprecision in the conceptualization and operationalization of the term, and concept, "uncertainty" itself. Essentially, scholars' explicit and implicit definitions of uncertainty, and our theorizing of its effects, have been characterized — ironically — by enduring and pervasive uncertainty (but not of the accurate or precise kind). One of the ancillary purposes of this dissertation is to clarify, contextualize, reconcile, and simplify the concepts in this literature so as to enable and compare observation of patterns of findings using similar definitions.

Thus, in Chapter 1, I begin by explicating four distinct types of uncertainty that have appeared in the conceptualizations and operationalizations of the literature to date: *deficient uncertainty, technical uncertainty, scientific uncertainty,* and *consensus uncertainty*. Then, in Chapter 2, I summarize the theoretical justifications for competing hypotheses, respectively, about the effects of uncertainty frames in science communication, and then review (through the lens of this new, nuanced typology) the existing — although limited —

empirical research that has found positive, negative, moderated, and null effects of uncertainty-framed science. I conclude this chapter by identifying unanswered questions and offering corresponding research questions.

In Chapter 3, I present methods of data collection and analysis that, together, test the hypotheses and research questions posed in Chapter 2, and in Chapter 4 I report the results of these tests. Chapter 5 reviews this dissertation's overall argument, results, limitations, and implications for research and practice.

Chapter 1: Uncertainty about Uncertainty

1.1 Uncertainty Frames in Science

Generally, uncertainty is "when details of situations are ambiguous, complex, unpredictable, or probabilistic; when information is unavailable or inconsistent; and when people feel insecure in their own state of knowledge or the state of knowledge in general" (Brashers, 2001, p. 478). As mentioned in the introduction, uncertainty is an epistemological fixture that permeates science in particular, and most everything else in the world in general. But it also exists as an individual's perception or belief, and also as a feature or characteristic in communication content.

Regarding the former (uncertainty as a perception, attitude, or belief), communication research usually conceptualizes uncertainty as one of two distinct constructs: internal certainty and external certainty (Corbett & Durfee, 2004; Dixon & Clarke, 2013). An individual's "internal certainty" (IC) is their own belief about the certainty of a particular idea (e.g., "I am uncertain whether my team will win today"). An individual's "external certainty" (EC) is their estimation of someone else's belief about the certainty of a particular idea (e.g., "I think Eric is uncertain whether his team will win today").

Regarding the latter (communicated uncertainty), uncertainty is manifested as descriptive, qualifying information in a message. That is, information can be presented with emphasis on relevant uncertainties. These portrayals of uncertainty vary in style, mirroring the various *causes* of uncertainty. As alluded to above, these causes include, but are not limited to, uncertainties due to measurement error, random variation, unobservable projections or models, out-of-sample generalizations, disagreement among experts, conflicting evidence, a deficit of extant research/data, an expanding problem space, alternative underlying models or theories about the phenomenon, or fundamental unfalsifiability (Broomell & Kane, 2017; Miles & Frewer, 2003; Pidgeon & Beattie, 1997; Zehr, 2000).

To accurately communicate the boundaries of scientific knowledge, scientists often present "findings" or "discoveries" of science as being bounded, qualified, or otherwise limited by one or more of these causes/types of uncertainty, and (with varying fidelity, as discussed above) science media are used to relay these diverse caveats to the public. A large content analysis of mainstream newspaper coverage of climate change over time (Rice, Gustafson, & Hoffman, 2018) found that scientists often include clauses emphasizing uncertainties, controversies, and caveats. These specify distinct types of uncertainty held by a variety of sources about a variety of topics and claims within the broad issue of climate hcnage. In short, the various types and causes of uncertainty manifest themselves in different types of portrayals of uncertainty. I will refer to these as *uncertainty frames*.

1.1.1. Frames of uncertainty. According to Chong and Druckman (2007a), "framing refers to the process by which people develop a particular conceptualization of an issue or reorient their thinking about an issue" (p. 104), the effects of which are visible "when a communication changes a person's attitude toward an object (e.g., climate change) by increasing the weight given to a subset of relevant considerations" (Bolsen, Druckman, & Cook, 2014a, p. 2). From a more tangible, message-focused perspective, the *act of framing* is "to select some aspects of a perceived reality and make them more salient in a communicating text, in such a way as to promote a particular problem definition, causal interpretation, moral evaluation, and/or treatment recommendation for the item described" (Entman, 1993, p. 52).

While early research investigating the effects of gain and loss frames (e.g., Kahneman & Tversky, 1979; 1985) used *equivalence* frames (varying the orientation of equivalent information), much recent research has investigated the effects of *emphasis* frames (emphasizing selected content over other, different, possible content). While there is some friction in the field about the nature and boundaries of the concept of framing (see Cacciatori, Scheufele, & Iyengar, 2016; Scheufele & Iyengar, 2014), scholars agree that a) framing is a core component of human cognition and message processing; and that b) the manner in which information is framed influences communication effects because "... frames are never neutral: they define an issue, identify causes, make moral judgements and shape proposed solutions" (O'Neill, Williams, Kurz, Wiersma, & Boykoff, 2015, p. 380).

Myriad analyses across diverse media, locations, times, issues, and topics agree that science is often discussed through frames of uncertainty (Antilla, 2005; Bailey, Giangola, & Boykoff, 2014; Friedman & Egolf, 2011; Heidmann & Milde, 2013; Kuha, 2009; Boykoff & Boykoff, 2004; Dispensa & Brulle, 2003; Painter & Ashe, 2012; Rice et al., 2018; Zehr, 2000). Uncertainty frames in science communication discourse can arise from diverse causes, including good intentions such as journalistic ethical norms (Bennett, 1996; Boykoff & Boykoff, 2004), malignant motives such as public disinformation campaigns (Jacques, Dunlap, & Freeman, 2008; McCright & Dunlap, 2003; Oreskes & Conway, 2011), and — as discussed in the introduction — even the inherent nature of science itself (Stocking, 1999). These different causes lead to different types of expressions of uncertainty, which are explicated here in Chapter 1 as four distinct uncertainty frames.

1.1.2. Issue contexts of interest. It is helpful to specify which objects of uncertainty frames are the focus of this dissertation (i.e., uncertainty about *what*). Nested within any general science *issue* are many *topics*. For example, the issue of climate change can be separated into distinct topics of its existence, causes, effects, and remedies. A very large number of *claims* (posited facts, ideas, or theories; e.g., "climate change is anthropogenic") could be made regarding any of these topics. Table 1 illustrates these terms, using each of the three sets of issues/topics/claims that will be employed in this dissertation.

Table 1

Levels of Issues, Topics, and Claims

Level	*Example*
Issue A	CLIMATE CHANGE
Topic A1	The effects of climate change
Claim A1.1	Climate change has negative effects on farmers and agriculture workers
Issue B	GMO FOODS
Topic B1	Laws requiring the labeling of all GMO foods
Claim B1.1	GMO labeling laws have negative effects on farmers and agriculture workers
Issue C	OCCUPATIONAL HAZARDS OF FARMING
Topic C1	Extended contact with vibrating machinery
Claim C1.1	Vibrating machinery has negative effects on farmers and agriculture workers

Most framing effects research tests effects in just one issue. In science communication, specifically, climate change dominates the field. A limitation of this paradigm is that it obfuscates whether findings of framing effects are issue-specific. It is, in fact, likely that most findings *are* issue specific, since (as reviewed in Chapter 2) prior beliefs and worldviews are the leading predictors of responses to science communication, and the salience of prior beliefs and worldviews to an issue of science varies across issues. In some issues (e.g., climate change), many individuals have strong prior beliefs and worldviews that are salient to the issue. In other issues (e.g., occupational hazards of farming), most individuals do not have strong existing beliefs and their worldviews are not necessarily relevant to the issue.

In anticipation of issue-specific effects — and in recognition of the importance of prior opinions and worldviews to individuals' responses to science communication — this dissertation tests the effects of uncertainty frames on audience attitudes regarding three specific claims of scientific research. Each claim is related to a distinct issue (Table 1): climate change (CC; using a claim about the harmful effects of climate change on agriculture workers), genetically engineered/modified foods (GMO; using a claim about the harmful effects of GMO labeling laws on agriculture workers), and occupational hazards in farming (VM; using a claim about the harmful effects of contact with vibrating machinery on agriculture workers). The purpose of observing effects across three very different issues is to determine to what extent the observed effects are generalizable across issues (as well as across individual characteristics). That is, these particular three issues are selected because they *differ* in important ways.

The first issue, climate change, is deeply associated with American political views (Roser-Renouf, Maibach, Leiserowitz, & Rosenthal, 2016) as a logical consequent of ideological tenets, and also through popularization and partisan politicizing of the issue, particularly in the United States. Public opinion is deeply divided along party lines.

The second issue, GMO foods, also is popularized and large opposing groups of the population hold strong polarized prior opinions, with one survey reporting that 57% of U.S. adults believe eating GMO foods is "generally unsafe" (Pew Research Center, 2015) and another survey finding that 45% of adults report "absolute moral opposition" to GMO foods (Scott, Inbar, & Rozin, 2016). Only 35% of the American public reports that they fully trust the government to tell them the whole truth about GMO foods (Kennedy & Funk, 2016). Regarding the topic of mandatory GMO labeling, the Pew study reported that half of U.S.

adults say they either sometimes (25%) or always (25%) purposefully look for GMO labels when grocery shopping. When U.S. adults are faced with a forced choice question — "do you support or oppose mandatory GMO labeling" — 84% indicated support (McFadden & Lusk, 2017), and one in six U.S. adults saying they "care a great deal" about it (Pew Research Center, 2016).

However, opinions about GMO foods and GMO labeling are not driven by political ideology (Pew Research Center, 2015). Instead, opposition to GMO foods is (positively) correlated with (lower) education, (female) gender, and (higher) perceptions that genetic engineering is unethical or immoral (Elder, Greene, & Lizotte, 2018; Hassell & Stroud, 2018; Lusk et al., 2004).

For a vast majority of the population, the third issue (occupational hazards in farming) *does not* have strong, pre-existing, polarized attitudes like the first two issues, and also is not connected to any particular ideology. In sum, this assortment of issues will allow for observation of responses to uncertain science in one issue where responses are driven by strong political/partisan prior opinions (CC), in one issue where responses are driven by strong but non-political prior opinions (GMO), and in an issue where there are few, if any, strong prior opinions (VM). This design is a reflection of the integral role of prior opinions and ideological worldviews in responses to science and to uncertainty.

1.1.3. Source context of interest. Often, a *source* is also attached or attributed to a claim ("Dr. Jones believes that climate change is anthropogenic"). Myriad types of actors (e.g., scientists, politicians, celebrities, government agencies, organizations, lobbyists, the public) could be a source of information or opinions about science, and these differences influence the meaning or implications of the frame (Rice et al., 2018). This dissertation exclusively focuses on claims where scientists are the clearly identified source (although relayed via the news media) for three reasons. First, content analyses of uncertainty frames identify scientists as the most frequently named source of information/opinion about scientific research (e.g., Rice et al., 2018). Second, scientists as sources of scientific information/opinion is the core focus of science communication literature. Third, constraints of resources and methodology preclude this dissertation from considering or comparing the effects of uncertainty frames across multiple types of sources. In sum, since it is unwise to conflate different types of sources (Rice et al., 2018), and since it seems most important to (first) develop our understanding of *scientists* as sources of science, relative to other entities, this dissertation investigates only science claims (with and without uncertainty frames) originating from scientists.

1.2 Types of Uncertainty Frames

These uncertainty frames occur in four distinct *types*, following from the cause of the uncertainty. For example, an uncertainty frame depicting a deep controversy in the scientific community over the validity of a key theoretical assumption is a much different *type* of uncertainty frame (certainly in nature, if not in effects) than an uncertainty frame depicting a range of expected values due to inescapable measurement error. The cause of the uncertainty frame drives the very meaning of the claim, and likely also determine what responses to that information are appropriate. The multitude of causes of uncertainty in science mentioned above (as identified by focus groups, scholarly theorizing, and empirical tests) are exhibited in science communication practice and research along four distinct *types* (or families, genres) of uncertainty frames. Following from the contrarian opinion frame types explicated by Rice and colleagues (2018), I will refer to these as *deficient*, *technical, scientific*, and *consensus* uncertainty, respectively (Table 2).

1.2.1. Deficient uncertainty. Discussions of science do not only focus on what is known, but also on what is not yet known — or not yet studied (Stocking & Holstein, 1993). Often, this takes the shape of a frame of what I call *deficient uncertainty*, emphasizing a lack of knowledge that exists because there is a lack of research on this question, or because that thing cannot ever be known, or because the problem space has expanded in a way that has exposed new unknowns (for example, as used by Kuhn, 2000; Rice et al., 2018; Zehr, 2000). Hacking (1975) describes an analogous concept of "epistemic uncertainty" — referring to the uncertainties that stem from gaps in knowledge, which can be reduced by accumulating additional knowledge. This type of uncertainty closely mirrors the colloquial use of "uncertainty," such that high deficient uncertainty would indicate that there is little extant knowledge and much that is unknown or yet unstudied. For example, someone could express deficient uncertainty by saying "Scientists know very little about the complex, interrelated domino effects that climate change will have on ecosystems and their inhabitants, because change of this magnitude has never before been available for observation in the era of modern science." Lower deficient uncertainty would indicate (relatively) greater knowledge and much less that is unknown. Either would be deficient uncertainty (rather than a different type) because they both possess the fundamental component of emphasizing that there is uncertainty *because* of the degree to which the current knowledge falls short of some (more) complete set.

Table 2

Four Uncertainty Types

Uncertainty Type

	Deficient	*Technical*	*Consensus*	*Scientific*
Exemplar phrasing	"Scientists' knowledge in ____ remains limited because most of the research needed to prove or disprove these ideas has not yet been conducted."	"Scientists' best estimate is that the total increase in ___ could be as low as 6% or as high as 24%."	"Scientists remain divided on ____, with each side receiving strong support from leading scientists and research groups."	"Scientists are always striving to develop a better understanding of ___, so scientists fully expect to adjust their opinions about this issue as future research is conducted."
Defining properties	Uncertainty caused by a lack of study, a lack of available evidence, or fundamental "unknowable" nature of the question.	Uncertainty caused by the known properties of precise, quantified estimates; often derived from evidence or study.	Uncertainty caused by disagreement, controversy, or discrepancy across expert opinion or across evidence.	Uncertainty caused by the perpetual potential for future research to reform or reject the current assumptions or body of evidence.

1.2.2. Technical uncertainty. Hacking (1975) also describes "aleatory uncertainty" — an effect of inherent complexity in a problem space, or inherent randomness in a phenomenon. Dieckmann and colleagues (2017) summarize that there is "irreducible uncertainty that is a feature of the world itself" (p. 327). Thus, most scientific claims are limited by measurement error, modeling approximations, statistical assumptions, or inherent imprecision of out-of-sample generalization (Broomell & Kane, 2017). All of these contribute to uncertainty about scientific claims. To account for this, and to promote transparency and accuracy, scientists frequently present findings as estimates couched in error bars or expected ranges of values. For example, a scientist might claim that "global sea level will rise between 7 and 10 centimeters sometime before 2100," which would represent the 95% confidence interval of the distribution of estimations. I will call such portrayals *technical uncertainty*, although varied terminologies and references (usually just "uncertainty") to analogous constructs are frequently employed in myriad epistemological discussions, content analyses, survey research, and experimental tests in the fields of science, environmental, and risk communication (e.g., Budescu, Kuhn, Kramer, & Johnson, 2002;

Cabantous, Hilton, Kunreuther, & Michel-Kerjan, 2011; Dieckmann, Gregory, Peters, & Hartman, 2017; Johnson & Nakayachi, 2017; Johnson & Slovic, 1995; 1998; Morton, Rabinovich, Marshall, & Bretschneider, 2011; Rabinovich & Morton, 2012; Rice et al., 2018; Smithson, 1999). Technical uncertainty can be high (e.g., wide error bars or broad estimated ranges in a prediction) or low (narrow, precise ranges). These diverse content analyses and experiments operationalize technical uncertainty as either a range in an estimate (e.g., 7-10cm), as a probability estimate (e.g., a 65% chance), or as an estimated mean with a confidence interval (a political poll estimating a candidate's support at 37% with a 95% confidence interval of +/-3 percentage points). The fundamental, identifying component of technical uncertainty (which differentiates it from other types) is a precise *quantification* of uncertainty that is derived from research and data, rather than due to a lack of them (Rice et al., 2018).

1.2.3. Scientific uncertainty. Statements of scientific uncertainty position uncertainty about a claim as a function and feature of the process or nature of good scientific research. While such statements often increase uncertainty, they can also increase accuracy — and thus are utilized widely in scholarly science communication (Hyland, 1996). For example, high scientific uncertainty has been operationalized as an explanation of the barriers to generalizability due to methodological limitations or the difficulty of the problem, or the qualification that substantial further research must be conducted to corroborate a preliminary claim (e.g., termed "more uncertainty" in Broomell & Kane, 2017; "evidentiary balance" in Clarke, Dixon, Holton, & McKeever, 2015; "hedging" in Jensen, 2008; the "high uncertainty" condition in Jensen et al., 2017). I will refer to this as *scientific uncertainty*. Like the other uncertainty types, scientific uncertainty exists on a continuum. Low scientific uncertainty has been operationalized as emphasizing confidence in a research finding due to the existence of substantial supporting evidence, but still stipulating that future, forthcoming evidence could modify this body of knowledge (e.g., termed "individual uncertainty" in Binder, Hillback, & Brossard, 2016; the "context" condition in Corbett & Durfee, 2004; the "low uncertainty" condition in Jensen et al., 2017; "scientific uncertainty" in Nakayachi, Johnson, & Koketsu, 2018). As such, scientific uncertainty is similar to deficient uncertainty in that they both highlight that a claim is bounded by limited knowledge, but — like technical uncertainty — scientific uncertainty is distinct from deficient uncertainty by emphasizing a) the knowledge that has been acquired, and b) that uncertainty is a feature and function of the rigorous processes of science, rather than an undesirable state that must be rectified.

Scientific uncertainty and technical uncertainty can be distinguished by their level of objectivity, quantifiability, and precision. Technical uncertainty is expressed as being an objective quantity. Scientific uncertainty is often expressed as being a principle or perspective that should be used to interpret a claim. For example, I could say that there is uncertainty about my height because someone estimated that I am between 6'4" and 6'6" (technical uncertainty). Or I could say that there is uncertainty about my height because *although* I measured myself and found myself to be 6'5", our understanding of human height is still evolving and measuring height with precision is not a simple task, so additional measurements by independent parties — and additional measurements over time — will likely change our current best estimations of my height (scientific uncertainty). Of course, I could also state that there is uncertainty about my height because my height has not yet been measured (deficient uncertainty).

1.2.4. Consensus uncertainty. The (un)certainty of any particular claim can also be described in terms of the collective discord/accord that exists about it — specifically, the degree of (dis)agreement among, or between, scientists or other relevant stakeholders (Aklin & Urpelainen 2014; Binder, Hillback, & Brossard, 2016; Broomell & Kane, 2017; Cobb & Elder, 1983; Dieckmann et al., 2017). Colloquially, this is referred to as disagreement or controversy. For example, much of the early research on the framing of climate change news investigated the frequency with which journalists emphasized the discord between opposing stakeholders (e.g., Boykoff, 2008; Boykoff & Boykoff, 2004). Similar operational definitions of uncertainty — portrayals of two or more entities with competing opinions about a claim — have been presented with other names (e.g., "conflict ambiguity" in Cabantous et al., 2011; Smithson, 1999; or "conflicting information" in Carpenter et al., 2016) across diverse research in risk, science, and environmental communication. For example, Rice and colleagues (2018) explain two forms of consensus uncertainty for their content analysis — disagreement and controversy — noting that both denote mutually opposed opinions, but have some differences in meaning, such as their longevity and amount of shared agreement.

Consensus uncertainty has widely been applied in definitions representing high uncertainty (disagreement or controversy; e.g., Antilla, 2005; Binder, Hillback, & Brossard, 2016; Brossard, Shanahan, & McComas, 2004; Corbett & Durfee, 2004; Dispensa & Brulle, 2003; Jensen & Hurley, 2012; McComas & Shanahan, 1999; Nan & Daily, 2015; Rabinovich & Morton, 2012; Stocking, 1999; Zhao et al., 2016) and also low uncertainty (strong consensus or collective agreement; e.g., Aklin & Urpelainen 2014; Bolsen & Druckman, 2016; Ding et al., 2011; Kahan, 2017; Koehler, 2016; Lewandowsky, Gignac, & Vaughan, 2013; van der Linden et al., 2017, 2018).

To round out the analogy given above, there is some uncertainty about how tall I am because my brother (who is, I am certain, 6'4") and I have long been earnestly making competing claims. This is consensus uncertainty.

1.2.5. Summary of uncertainty frames. In sum, there is a robust record of the conceptualization and operationalization of uncertainty in science occurring in four types: deficient uncertainty (describing a lack of knowledge), technical uncertainty (quantifying the error around some acquired knowledge), scientific uncertainty (situating acquired knowledge within the nature and process of science itself), or consensus uncertainty (disagreement or controversy between stakeholders about a claim).

Importantly, these distinct types of uncertainty *have not* been treated as distinct. That is, it is common for studies that test the effects of one type of uncertainty frame to justify their hypotheses using prior literature that employed an entirely different conceptualization. Still, the extant conceptualizations and operationalizations of "uncertainty in science communication" exhibited in the extant literature — and reviewed in the next section — can be clearly placed into one of these four types that I have proposed.

Therefore, as I review this literature, I will translate the assorted uses of uncertainty into the four types given above, with brief descriptions of their fit. In Section 1.4, Table 4 and Table 5 also organize the extant experimental tests of uncertainty frame effects into categories of these four uncertainty frame types (regardless of the diverse names that the corresponding authors gave to their operationalizations). This translation and restructuring will contribute a new level of nuance and consistency, which will highlight patterns that can help explain the competing findings regarding the effects of uncertainty frames that exist across diverse literature, as well as highlight patterns of empirical findings that emerge across and within uncertainty types.

It is important to note that while these types of uncertainty are distinct concepts, they can be used together, or can even be applied to each other. That is, these uncertainty types can simultaneously function as uncertainty about a claim and as the claim itself. For example, there could be high consensus uncertainty about an uncertainty claim, such that some entities say there is much deficient uncertainty about some topic, and others say there is little. Similarly, there could be technical uncertainty about the quantified estimate of that consensus (e.g., the statement "97% of scientists agree" itself has error bars). A statement could even be in any one of the eight combinations of high/low consensus about their high/low technical uncertainty about their high/low deficient uncertainty about a claim.

Some definitions for other, related concepts have also been assembled using these pieces in this fashion. For example, Bolsen and Druckman (2015) define *politicization* as the phenomenon where "an actor emphasizes the inherent uncertainty of science to cast doubt on the existence of scientific consensus" (p. 746). Translated to the vocabulary of this dissertation and its four uncertainty types, this is a source using the existence of scientific (and/or technical) uncertainty about a claim as evidence of consensus uncertainty about that claim (see also Stocking & Holstein, 2009). Interestingly, this rhetorical tactic for attacking scientific consensus is a fallacy *precisely because* it makes a false equivalence of scientific uncertainty and consensus uncertainty (i.e., an assumption that the existence of one implies the existence of the other, or, more deceptively, that one *is* the other). This further supports the need for developing these distinctions.

Finally, it is important to clarify that (un)certainty frames vary on a continuum of high uncertainty to low uncertainty (with a message emphasizing absolute certainty being at one end of the continuum; Chinn, Lane, & Hart, 2018). In this dissertation, references to high or low uncertainty should not be confused with the intensity, vividness, or explicitness with which that frame, or level of (un)certainty, is communicated. A valuable future direction for other framing research might be to develop measures of intensity (from weak to strong), because current content analyses and experimental manipulations of frames almost exclusively treat them as binary (either present or absent). But this additional distinction is not considered in this dissertation.

1.3. Public Understanding of Uncertainty in Science

In 2003, Frewer and colleagues assessed meta-cognitions — *scientists'* perceptions of the public's understanding of uncertainty. They found that scientists largely assume that the public is incapable of correctly understanding or interpreting uncertainty in science, and that scientists see this as a justification for avoiding uncertainty frames in public communication.

However, they also investigated how the public *actually* perceives various uncertainties in science. Miles and Frewer (2003) probed a focus group to establish the emergent dimensions of uncertainty about the science of food safety. In a subsequent study, they summarized these emergent dimensions in a measure of public perceptions of the possible causes of uncertainty in food safety risk (Frewer et al., 2002). The causes of uncertainty represented in their items (Table 3) clearly include the present dissertation's uncertainty types of *deficient, consensus,* and *scientific* uncertainty. While technical uncertainty is absent in this one study, the review above demonstrates that it is well-represented in other communication research.

Importantly, Frewer and colleagues also investigated the public's opinion of the "acceptability" of different causes (types) of uncertainty, regarding food safety risks. Quite in contrast to the assumptions of scientists, their data also indicated that the public *expects*, even desires, uncertainty information. Their results indicate that the causes that I would call *scientific* uncertainty were, by far, rated the most acceptable by the public, with the causes that map onto *consensus* uncertainty well below the midpoint of the scale, and the causes representing *deficient* uncertainty as the least acceptable of the three. Scientific and consensus uncertainty were also perceived as the most likely causes of uncertainty about food safety risks. While some types of uncertainties are more acceptable than others, respondents also reported a strong desire to be notified of all relevant uncertainty information immediately when a potential health risk arises.

Table 3

Uncertainty Statements (from Frewer et al., 2002)

Scale item from Frewer et al., 2002	*Uncertainty Type, Translated*
The government lacks definite knowledge about the topic.	Deficient
It is not possible for scientists to have all the answers.	Deficient
The government's statement is based on conflicting information.	Consensus
The information provided is the best available at present, but things may change in the future.	Scientific
The government is unsure about the extent of the problem.	Deficient
Scientists disagree with each other on the subject.	Consensus
The government is unsure whether there is a problem or not.	Deficient
More scientific work needs to be done on the topic.	Deficient

Note: Two items from this measure were omitted because they were specific to the study and do not reference general dimensions of uncertainty about science ("The government is withholding information from the public" and "There really is a major food safety problem").

These results are important to this dissertation, because they indicate that the different uncertainty types are in fact *not* unimportant or indistinguishable to the public, and are also *not* perceived with equal favorability. Uncertainty in science (and perceptual distinctions of its types) is not isolated to scientific discourse and/or the epistemological beliefs of the scientific community. Rather, the public reports an *expectation* for uncertainty in science communication and is especially accepting of uncertainty that stems from the inherent complexity of scientific inquiry. These points cannot be understated, because they provide a springboard for the current investigation.

However, it is important to keep in mind that public perceptions of, preference for, and responses to, uncertainty in science can vary significantly across issues (e.g., Broomell & Kane, 2017; Dieckmann et al., 2017; Jensen & Hurley, 2012) — and also (or especially) can vary across individual characteristics such as issue-relevant prior beliefs, values, or ideologies and worldviews (Borah, 2011; Chang, 2015; Chong & Druckman, 2007; Dieckmann et al., 2017; Frewer, Howard, & Shepherd, 1998; Nan & Daily, 2015). While this point will be a focus of Chapter 2, a brief example is the research by Dieckmann and colleagues (2017) that found that segments of the public that are low in cognitive ability and/or are high in self-reported knowledge believe that consensus uncertainty is primarily caused by researcher bias or deficient knowledge. Only those who score high in cognitive ability are prone to attribute consensus uncertainty to the nature of science or inherent complexity in the problem space.

Much correlational data from surveys indicate that perceptions of uncertainty in science are related to attitudes about the scientific findings themselves. For example, individuals' perceptions of the technical (im)precision of a scientific field is a significant predictor of their ratings of the overall quality of the science, support for funding, and perceived social benefit (Broomell & Kane, 2017). For climate change in particular, perceptions of consensus uncertainty are negatively related to support for climate policy and taking action to combat it (Ding, Maibach, Zhao, Roser-Renouf, & Leiserowitz, 2011).

1.4 Summary

In sum, Chapter 1 has demonstrated how the concept of uncertainty can be, and has been, used in (at least) four distinct emphasis frames, each using a particular *type* of uncertainty to describe or qualify a claim. Chapter 1 argued that this is especially germane to the nature and goals of science and scientists, because of the centrality of uncertainty to the scientific method and of uncertain discourse to accurate science communication. Some research indicates that the public may be (somewhat) cognizant of the presence of uncertainty in science, and may also have different opinions about different types of uncertainty. But while survey research has made it clear that there is a correlation between perceptions of uncertainty surrounding a given claim and attitudes toward that claim, specifying the *effects* of these uncertainty frames in science is another matter entirely — and is a primary goal of this dissertation in particular. Thus, in the next chapter, I provide an in-depth summary and reconciliation of the myriad, diverse, seemingly inconsistent findings of the experimental literature on the effects of uncertainty-framed science messages, and also discuss the important role of motivated processing in responses to uncertainty in science.

Chapter 2: The Effects of Uncertainty Frames: Competing Theory and Evidence

Currently, there is some theory and evidence indicating that uncertainty frames (undifferentiated) in science communication generally have negative effects on source credibility, claim-specific beliefs, trust in science, and behavioral intentions. However, there is also some theory and evidence indicating generally positive effects of uncertainty frames (undifferentiated). Here in Chapter 2, I survey the experimental literature that has tested the effects of uncertainty frames on individuals' attitudes, beliefs, and behavioral intentions. Throughout this summary, when referring to the reported effects of uncertainty, I specify which one of the four uncertainty frame types were used in the operationalization according to my definitions (i.e., how the researchers presented "uncertainty" in their experimental manipulation) (to the extent that it was reasonably interpretable as such). At the end of Chapter 2, after observing the rift between the two general perspectives, I reorganize this literature by separating the reviewed results into four categories corresponding with the four uncertainty frame types. This restructuring, presented in Tables 4 and 5, results in a clearer pattern of effects. That is, when the collective findings are grouped together as undifferentiated "uncertainty frames," the group's findings are inconsistent — containing positive, negative, and null effects. But when the results of the extant experimental tests are separated into the four distinct types of uncertainty frames, the findings are more consistent within each group, and there are tentative differences between groups. However, these are only tentative and subjective review observations, not the results of a statistical meta-analysis. This, then, is a natural impetus for the controlled test of the relative effects of the respective uncertainty frame types that is presented in Chapter 3.

2.1 Arguments for Expecting Negative Effects

There is diverse support for the expectation that — relative to frames of certainty — uncertainty frames will instigate, perpetuate, or exacerbate more negative attitudes toward science, scientists, and specific claims of science. Generally, states of uncertainty can often be unpleasant (Camerer & Weber, 1992), such that people seek to regulate the uncertainty they experience to maintain a desired level (Afifi & Weiner, 2004). Uncertain information is also inherently more difficult to understand than certain information, and ambiguity in general can cause confusion (Tversky & Shafir, 1992; van Dijk & Zeelenberg, 2003). People are often ambiguity-averse (Ellsberg, 1961) and ambiguous information can spark negative reactions toward both the source and the claim (Han, Moser, & Klein, 2007). Uncertainty frames can also cause people to justify continuing in their current state rather than responding to information with adaptive action (Budescu, Rapoport, & Suleiman, 1990). Specific to science communication, people often expect experts to be precise and confident (Shanteau, 1987), and non-scientists may not understand the language or purpose of uncertainty in science (Roth, Morgan, Fischhoff, Lave, & Bostrom, 1990). For these reasons, early science communication scholars often recommended that science communicators eliminate uncertain discourse and use, instead, more powerful, factive language (see review in Zehr, 1999).

There is also some clear experimental evidence of negative effects of uncertainty-framed science communication. When asked directly, people report being displeased by the (deficient) uncertainty caused by inadequate research (Frewer et al., 2002), and survey evidence indicates that perceptions of discord among scientists (consensus uncertainty) are negatively associated with support for climate change policies (Ding et al., 2011). Evidence also suggests that when people perceive uncertainty about environmental conservation threats, they tend to act according to self-interest, instead of engaging in collective action for the greater good (Hine & Gifford, 1996). Nagler (2014) found that individuals who report higher exposure to consensus uncertainty about nutrition (e.g., benefits/risks of wine) also report higher confusion, negative reactions toward nutrition science and scientists, and lower intentions toward the recommended dietary behaviors. Further evidence indicates that portrayals of consensus uncertainty about vaccine safety can lower behavioral intentions toward vaccination (Meszaros et al., 1996). Similarly, Dixon and Clarke (2013) report that portrayals of consensus uncertainty cause individuals to have greater doubts about vaccine safety and to perceive greater discord among scientists. These findings are echoed by Chang (2015), who found that contradictory findings published in health news produce lower favorability ratings of health research, lower perceived credibility of the research, greater uncertainty about health research, and lower behavioral intentions toward healthy behavior.

Regarding the effects of *minimizing uncertainty*, the gateway belief model (van der Linden et al., 2017) posits that messages emphasizing high consensus (that is, low uncertainty) will de-bias beliefs about consensus and successively change other attitudes and beliefs about climate science, and can also inoculate against misinformation. Similarly, Bolsen and Druckman (2015) report robust experimental evidence indicating that support for new technologies (fracking and carbon nanotubes) can be strengthened through the inclusion of specific inoculation against (before) or correction to (after) specious arguments that inherent scientific uncertainty implies consensus uncertainty. These perspectives rest upon the advantages of emphasizing certainty instead of uncertainty. Similar hypotheses have been repeatedly supported by empirical evidence (e.g., Lewandowsky et al., 2013; van der Linden et al., 2018). A full list of the relevant empirical literature is presented in Tables 4 and 5.

Table 4

Experimental Evidence of the Effects of Consensus Uncertainty

Authors	*Topic*	*Sample*	*Findings*	*Conditional Effects*
Consensus Uncertainty (High)				
Aklin & Urpelainen, 2014	Env. Policy	3331(n)	A	Fully crossed (+/-) along prior trust in scientists
Binder et al., 2016	Nanotechnology	243(s)	+R, ~~IC~~	+R only in individuals with high deference to science
Chang, 2013	Nutrition		-A, -BI	
Chang, 2015	Health Research		-A, -BI, -Cr, -IC	
Clarke et al., 2015	Vaccines	197(s)	-IC, -EC	
Corbett & Durfee, 2004	Climate	209(s)	~~IC, EC~~	
Dieckman et al., 2017	Assorted Science	421(n)	-Cr	Only in Low Edu and/or High (self-report) Knowledge
Dixon & Clarke, 2013	Vaccines	327(s)	-IC, -EC	Mediated by perceived divide in expert opinion
Dixon et al., 2015	Vaccines	371(s)	-A	-A effect is greater in individuals with prior opposition
Jensen & Hurley, 2010	Toxic Waste, Wolves	242(s)	Cr	+Cr in Wolf Conservation topic, -Cr in Toxic Waste Risk
Johnson, 2017	Assorted New Science	2300(n)	-EC, ~~A, Cr~~	
Koehler, 2016	Economics	275(n)	-EC	
Kuhn, 2000	Public Health Risks	177(s)	~~A~~, R	Prior environmental attitudes moderate effect on perceived risk
Nan & Daily, 2015	Vaccines	338(n)	A	Fully crossed (+/-) along prior support for vaccines
Rabinovich & Morton, 2012	Climate	214(s)	BI	Fully crossed (+/-) along prior belief about role of uncertainty
Retzbach & Maier, 2015	Nanotechnology	945(n)	~~A, Cr~~	
Consensus Uncertainty (Low)				
Bolsen & Druckman, 2017	Fracking, C-nanotubes	2484(n)	+A, +R	
Lewandowsky et al., 2013	Climate	120(c)	+EC	The treatment erases the -SC beliefs of free market supporters
van der Linden et al. 2018	Climate	6301(n)	+A, +BI	
van der Linden et al. 2017	Climate	1104(n)	+A, +BI	Opponents increased more than supporters (ceiling effect)

Note: In the "Sample" column, n = nationally representative sample, s = student convenience sample, c = local adult convenience sample. In the "Findings" and "Conditional Effects" columns, "+" denotes positive effect and "-" denotes negative effect, and ~~strike~~ denotes nonsignificant hypothesized effects. A = supportive opinions about, or agreement with, the specific claim/finding; Cr = perceived credibility of the science/scientists; BI = behavioral intentions, IC = internal certainty, or conversely, personal doubt, confusion, or ambivalence regarding the claim; EC = external certainty; perceived scientist/expert certainty or consensus, R = perceived risk.

Table 5

Experimental Evidence of the Effects of Deficient, Technical, and Scientific Uncertainty

Authors	*Topic*	*Sample*	*Findings*	*Conditional Effects*
Deficient Uncertainty				
Kuhn, 2000	Public Health Risks	177(s)	~~A~~, ~~R~~	
Technical Uncertainty				
Dieckmann et al., 2017	Climate, Guns, AgPrice	216-421(n)	other	Worldview predicts interpretations of a probability distribution
Han et al., 2011	Cancer Risk	250 (c)	+R	
Johnson & Slovic, 1995	Toxic Waste	180-290(s)	+R, Cr	Cr = (+)Trust, but (-)Expertise
Kuhn, 2000	Public Health Risks	180(s)	~~A~~, ~~R~~	
Morton et al., 2011	Climate	88-120(s)	BI	In highest uncertainty condition, BI effects are fully crossed (+/-) on gain/loss framing.
Scientific Uncertainty				
Binder et al., 2016	Nanotechnology	243(s)	-R, ~~IC~~	-R, but only in individuals with high deference to science
Broomell & Kane, 2017	Psychology Research	292-301(n)	Cr	-Cr for Republicans, ~~Cr~~ for Democrats
Clarke et al., 2015	Vaccines	197(s)	-IC, -EC	
Corbett & Durfee, 2004	Climate	209(s)	+IC, +EC	Opponents increased more than supporters (ceiling effect)
Frewer et al., 1998	GMOs	240 (c)	+A	Stronger +A effect in those with prior opposition (ceiling effect)
Jensen, 2008	Cancer Research		+Cr	+Cr = (+)Trust, No effect on Expertise
Jensen et al., 2011	Cancer Research	(s)	+A, ~~A, CR~~	(-)fatalism, ~~backlash, skepticism, trust~~
Jensen et al., 2016	Cancer Research	(c)	~~A~~	~~fatalism, backlash, info overload~~
Nakayachi et al., 2018	Earthquake Risks	750(c)	+Cr, ~~BI, R~~	+Cr = (+)honesty, (+)openness, (+)trust, but ~~competence~~

Note: In the "Sample" column, n = nationally representative sample, s = student convenience sample, c = local adult convenience sample.
In the "Findings" and "Conditional Effects" columns, "+" denotes positive effect and "-" denotes negative effect, and ~~strike~~ denotes nonsignificant hypothesized effects. A = supportive opinions about, or agreement with, the specific claim/finding; Cr = perceived credibility of the science/scientists; BI = behavioral intentions; IC = internal certainty, or conversely, personal uncertainty, doubt, or confusion regarding the claim; EC = external certainty, perceived scientist/expert certainty or consensus, or — conversely — perceived doubt or disagreement among scientists/experts; R = perceived risk.

2.2 Arguments for Expecting Positive Effects

There is also diverse theorizing and empirical support for the expectation that — relative to frames of certainty — uncertainty frames will have *positive* effects on attitudes toward science, scientists, and claims of science. The earliest and most programmatic body of literature on the nature and effects of uncertainty portrayals is in the field of risk communication. An early and persisting understanding in this literature was that portrayals of technical uncertainty about scientific claims of threat imminence and threat severity would enhance trust in the source and even increase behavioral responses toward risk mitigation (Habicht, 1992; Johnson & Slovic, 1995; McGregor et al., 1994; Slovic et al., 1984). As such, numerous scholars have encouraged scientists to more openly admit uncertainties when communicating to the public (Campbell, 2011; Leshner, 2003; Parascandola, 2000; Stocking, 2010). The most common justification in this perspective is that the inclusion of uncertainty information increases credibility and accuracy perceptions because it communicates more honesty and transparency than statements of absolute certainty (Frewer et al., 2002; Johnson & Slovic, 1995).

Further, the cultural cognition thesis argues that messages emphasizing strong consensus (low uncertainty) likely result in boomerang effects because those who strongly oppose that position will rationalize the consensus as evidence that scientists are corrupted and colluding, rather than as evidence that the claim is supported by rigorous body of consistent findings (Kahan, Jenkins-Smith, Braman, 2011). Bolsen and Druckman (2017) similarly report that consensus messages can have a boomerang effect in oppositional audiences that possess high knowledge. Such boomerang effects in oppositional audiences can even be considered a rational response if we consider that skeptical audiences *expect* messages about consensus, but simply distrust the source (Cook & Lewandowsky, 2016). This in itself is not direct evidence of positive effects of uncertainty, *per se*, but it does present a rationale for *not assuming that* more certainty is always good and/or always best when communicating science, especially in contexts with oppositional audiences.

There is diverse experimental support for this perspective. For example, Jenson and colleagues report experimental evidence that journalists and scientists are both perceived as more trustworthy when they "hedge" reports of scientific findings with caveats or limitations that describe scientific uncertainty (2008; 2011). Similarly, Clarke et al. (2015) found that — relative to consensus uncertainty frames — frames of (low) scientific uncertainty (hedging plus a weight of evidence statement) directly increased beliefs in scientists' certainty about the vaccine-autism controversy and decreased beliefs of scientific discord, which in turn were directly and positively related to personal certainty. Frewer and colleagues (1998) found that people with initial negative attitudes toward GMOs were more accepting of proposed GMO applications if the information contained admission of scientific uncertainty, although individuals with initially positive attitudes were not influenced by admissions of uncertainty.

Further evidence indicates that when participants are presented with high technical uncertainty about risk outcomes (e.g., a 20% likelihood), they trust scientists significantly more when the risk information also contains a frame of scientific uncertainty explaining the process and limitations of the estimate (Nakayachi et al., 2018). However, the frame of scientific uncertainty did not result in different source perceptions at more certain risk likelihood levels (e.g., 90% likelihood). This is intuitive. If there is zero technical uncertainty, then a statement explaining reasons for uncertainty doesn't seem appropriate. If there is a great deal of technical uncertainty, then an explanation is appreciated.

2.3 Findings of No Effect

In addition, some experimental tests have observed no effect of uncertainty manipulations. For example, Bord and O'Connor (1992) found that individuals' level of concern for risk was unchanged by portrayals of technical uncertainty about threat level. Further, a longitudinal experiment testing the effect of uncertainty framing on participants' beliefs and trust in science found that uncertainty frames had no positive or negative effect on beliefs or trust (Retzbach & Maier, 2015). However, it could be that the topic (nanotechnology) and sample (German adults) do not involve as strong a sense of controversy as those used by other relevant research (e.g., climate change and American adults). Further, the operationalization of "uncertain science" was a condition in which benefits *and* harms of nanotechnology were mentioned. This is not uncertainty, necessarily. That is, such a message could mean that scientists are certain and in agreement that there will be both benefits and harms. In short, a "pros and cons" portrayal does not necessarily equate to (consensus, or any other) uncertainty. Thus, this evidence does not reduce the likelihood that uncertainty portrayals have significant effects in more contentious issues, in more opinion-polarized samples, and in more explicit portrayals of uncertainty.

Finally, Kuhn (2000) found no difference in perceived risk or pro-environmental attitudes across four different uncertainty type message manipulations (which correspond to technical, consensus, deficient, and no uncertainty, respectively). This non-effect was expected, and the data supported the hypothesis of motivated reasoning in responses to uncertainty frames. However, this experiment had 177 participants across 4 conditions, which makes detection of small message effects unlikely.

2.4. Moderators of the Effect of Uncertainty Frames

The extant literature strongly suggests that responses to uncertainty in science are largely dependent on combinations of the issue context, individuals' prior beliefs about the particular claim, individuals' beliefs about science and scientists, and/or their broader ideology and worldview. Many of these findings can be explained via confirmation-biased motivated reasoning. This section discusses these, and other, kinds of moderators.

2.4.1. Motivated reasoning. It is widely understood that individuals' selection and processing of information often follows patterns of motivated reasoning and confirmation bias (e.g., Bolsen, Druckman, & Cook, 2014b; Nickerson, 1998; Taber & Lodge, 2006), such that people tend to attend to, interpret, and respond to information in ways that are most congruent with their existing beliefs, values, or behavior. Diverging interpretations by different people of the same information is a fundamental effect of motivated reasoning, and is ubiquitous in human information processing.

This phenomenon has significant and troubling implications for the effects of science communication campaigns. A growing cadre of science communication scholars have argued that relevant ideologies and prior issue beliefs moderate the relationship between diverse science communication messages and their effects on attitudes, credibility perceptions, and behavioral intentions. These conditional effects result in unintended outcomes such as a reinforcement of prior beliefs, polarization, boomerang effects, and cumulative advantage patterns over time (e.g., Corner, Whitmarsh, & Xenias, 2012; Gustafson & Rice, 2016; Hart & Nisbet, 2012; Hoffman, 2011; McCright & Dunlap, 2011; Pielke, 2007; Peters & Slovic, 1996; Sarewitz, 2004). Frewer and colleagues (1998) identify prior attitudes as a main driver of attitudinal responses, in general, summarizing that "people with negative initial attitudes receiving persuasive information from a distrusted source might become more negative… whilst those with positive attitudes receiving information from a trusted source might become more positive" (p. 17).

Motivated reasoning is germane to this discussion of the effects of uncertainty because there are likely to be characteristics of individuals (and their attitudes, beliefs, or values) that drive interpretations of, and responses to, uncertainty portrayals. Importantly, uncertainty-framed information may be especially fertile ground for motivated reasoning because its ambiguity, controversy, doubt, or imprecision *may inherently allow for* diverse — even discrepant — interpretations across individuals and groups (Chang, 2011, 2012, 2015). Indeed, some misinformation campaigns have been successful in persuading large portions of the public that the *scientific uncertainty* about climate change should be interpreted as reason to doubt the scientific *consensus certainty* or the legitimacy of the evidence (Jacques, Dunlap, & Freeman, 2008; McCright & Dunlap, 2003; Oreskes & Conway, 2010). Still, it is important to note that motivated reasoning is not just associated with unintended, undesired effects. For example, those with preexisting beliefs or values favorable to the message or source will exhibit a *positive* bias in their interpretations and responses of uncertain (and other) information (Frewer et al., 1998).

To date, very few studies have investigated the role of motivated reasoning in the effects of uncertainty frames. The most direct test (Dieckmann et al., 2017) found that —for both climate change and gun control — individuals are likely to apply the most worldview-consistent interpretation of technical uncertainty in scientific evidence. For example, participants in the climate change conditions were told that scientists expect sea level to rise between 1cm and 5cm, and then were asked whether a) the values close to 1cm are more likely, b) all values between 1cm and 5cm are equally likely c) the values close to 5cm are more likely, or d) the values in the middle are more likely than the values on the ends. Of course, the correct answer was "D" — a normal distribution. However, they found that prior support for climate science corresponded with a greater likelihood to choose either "C" (higher values) or "D" (normal distribution), and prior opposition to climate science was associated with a greater likelihood to choose either "A" (lower values) or "B" (uniform distribution).

On an encouraging note, when the correct interpretation and explanation of the range (a normal distribution) were provided to participants, this significantly reduced the motivated reasoning effect for all worldviews — a clear inoculation effect. However, this dependent variable (interpretations of a probability distribution) is of limited utility to most science communication campaigns aimed at increasing public beliefs, attitudes, behaviors, and trust in science. Further, of course, this has only been tested in response to technical uncertainty. It *may* be that some uncertainty types mitigate motivated reasoning while others catalyze it.

Investigating precisely this question, Kuhn's (2000) experiment tentatively indicated differential motivated reasoning effects across different types of uncertainty frames. In particular, the results indicated that the discrepancy in risk perceptions between pro-environmental and anti-environmental individuals was *greatest* when consensus uncertainty was portrayed, compared to the discrepancies associated with portrayals of deficient uncertainty or of technical uncertainty. That is, people were most prone to react to risk information *in accordance with their pre-existing opinions* when the risk information contained consensus uncertainty. This makes sense, because while deficient, technical, and scientific uncertainty frames could conceivably allow leeway for motivated reasoning due to ambiguous or inconclusive evidence, consensus uncertainty is the only uncertainty type that extends to *also* explicitly *providing (some) support for* both competing sides.

These preliminary studies provide a tantalizing preview into this valuable research question. At this point, the role of motivated reasoning in responses to uncertainty frames is still uncertain (deficient). Thus, further research is needed to identify the role of motivated reasoning in the effects of diverse uncertainty frames on widely applicable dependent variables like basic belief certainty about a claim and perceptions of source credibility.

2.4.2. Prior beliefs and ideologies. Consistent with the motivated reasoning perspective, several studies have found that the effect of uncertainty frames on attitudes and behavioral intentions is contingent on prior issue opinions or ideological worldview. For example, Nan and Daily (2015) found that portrayals of high consensus uncertainty regarding vaccine safety resulted in *more supportive* attitudes for individuals with a supportive prior issue position, but *less supportive* attitudes for individuals with opposition. Similar research found that statements of low scientific uncertainty about vaccine safety can negate the effect of a high consensus uncertainty frame, albeit only for those who had prior support for vaccine safety (Dixon et al., 2015).

Broomell and Kane (2017) found that scientific uncertainty frames about psychology research (as a field) produced negative credibility ratings from Republicans, but had no effect on credibility ratings from Democrats. Although it was not the focus of study, Kuhn's (2000) small experiment found that individuals' level of pre-existing environmental concern was positively related to the amount of perceived risk regarding an assortment of uncertainty-framed environmental threats. This finding held true in the technical uncertainty condition (numerical range of expected values), in the consensus uncertainty condition (where the uncertainty is attributed to competing expert biases), and in the zero-uncertainty condition (a specific, exact value).

Often, political ideology is an important determinant of individuals' opinions on science issues. It is salient to this dissertation, so here I briefly evaluate some common ways to measure it. While some researchers (e.g., Broomell & Kane, 2017) measure political ideology by simply asking participants to place themselves into discrete categories (e.g., Democrat or Republican), this approach has two important limitations. First, the use of two or three discrete categories is a reduction in information, so a continuous measure of political ideology would be much more robust. Second, even a continuous measure of political ideology — if it is explicit in its intent (e.g., "how would you rate your political beliefs on a scale from conservative to liberal?") — is not necessarily ideal, because, for example, "somewhat conservative" or "somewhat liberal" mean different things to different people. In strongly conservative regions or cultures, "somewhat liberal" beliefs might equate to the same beliefs held by a person from a strongly liberal region or culture who reports that they are "somewhat conservative." For these reasons, many scholars have moved away from categorical and/or direct measures of political ideology — favoring instead measures that target the ideology roots that underlie political opinion (e.g., Kahan et al., 2011). Leading among these is a measure of worldview that consists of two dimensions — one that assesses a continuum of hierarchical to egalitarian (HE) values, and one that assesses a continuum of individualist to collectivist (InCo) values (e.g., Bolsen & Druckman, 2011; Dieckmann et al., 2017; Kahan et al., 2011). Each of these dimensions is a very strong predictor of political party affiliation and of opinion on political issues. This dissertation will follow in this trend, such that all references to worldview/ideology are in reference to this two-factor construct composed of HE and InCo. In sum, it is clear that individuals' differences in responses to science communication (and to uncertainty in particular) are often driven by the differences in each individual's ideological worldview — particularly in politically charged issues (e.g., climate change).

2.4.3. Trust in science and scientists. Several studies have reported evidence indicating that an individual's deference to, and trust in, science is a significant predictor of their responses to science communication in general (Aklin & Urpelainen, 2014; Anderson, Scheufele, Brossard, & Corley, 2012; Ho, Brossard, & Scheufele, 2008; Lee & Scheufele, 2006). Specific to the effects of uncertainty, Aklin and Urpelainen (2014) manipulated the degree of expert consensus (e.g., 60%, 80%, 98%) that was portrayed about environmental policy, and found that increases in consensus (more agreement, certainty) resulted in stronger policy support *only for* people who already reported high pre-existing trust in scientists. For those who reported low trust in scientists, those portrayals of higher consensus (more agreement, certainty) had a boomerang effect — *decreasing* the policy support — possibly because it confirmed expectations of systemic collusion. Similarly, Binder and colleagues (2016) found that portrayals of scientific uncertainty lead to lower perceived risk of nanotechnology than did portrayals of consensus uncertainty, but only in individuals with high deference to science.

Of course, the influence of pre-existing trust in (or deference to) the source of the message is not limited to situations where scientists (specifically) are the source. Rather, these data simply evidence the human tendency toward confirmatory interpretations of uncertainty based on prior beliefs — more evidence of motivated reasoning in the effects of uncertainty frames.

2.4.4. Understanding of uncertainty in science. Intuitively, individuals' opinions about the role or purpose of uncertainty in science seem to influence their responses to portrayals of uncertainty. For example, one study found that participants who view science as an ongoing debate that will always have inherent uncertainty report relatively *higher* behavioral intentions after viewing messages with technical uncertainty about climate change effects (Rabinovich & Morton, 2012). On the other hand, those who viewed science as a process that additively uncovers absolute truths reported relatively *lower* behavioral intentions after viewing messages with technical uncertainty about climate change effects. Similarly, Johnson and Slovic (1995) found that individuals' belief that ranges around an estimate are an expected, natural characteristic of good science communication was positively related to their understanding of the information, certainty in the claim, and perceptions of scientific validity — and was negatively correlated with beliefs that the uncertainty indicated incompetent scientists. The authors also conducted a focus group, in which some participants reported that they expect scientific findings to have uncertainties, so portrayals of technical uncertainty are welcome and signal greater honesty, but lower competence.

2.4.5. Issue context. Another experiment (Jensen & Hurley, 2012) found that portrayals of contradicting scientific reports (high consensus uncertainty) had effects on perceptions of source credibility that were conditional on the science issue that was presented. Consensus uncertainty about dioxin in sewage sludge *diminished* source credibility, while consensus uncertainty about the reintroduction of gray wolves to populated areas *increased* credibility. The authors invoke the theory of motivated information management (Afifi & Weiner, 2004) to argue that responses to scientific discord or ambiguity are determined by an individual's preferred level of uncertainty on that issue — such that participants felt that uncertainty about toxic dioxin sludge was much less acceptable than uncertainty about the conservation of gray wolves.

2.5. Re-organizing the Literature

As summarized above, the research is currently divided regarding whether uncertainty-framed science communication has — in general — negative, positive, or no effects, as there are theoretical *and* empirical justifications for each perspective. The body of extant findings primarily indicates that in some situations (combinations of issues, audience characteristics, or uncertainty types), uncertainty frames in science communication can produce negative effects, while in other situations they can produce positive effects, and null effects in yet others. Each perspective must reconcile supporting, opposing, and null findings.

Due to the wide range of issues, methodologies, measures, and conceptualizations of the extant research, one of the most intuitive reasons for the mixed findings may be that the discrepant perspectives (negative, positive, null) arise from tests of different uncertainty types. Here, I demonstrate that a re-organization of the extant literature by uncertainty type may lead to better insights regarding their effects. Table 4 and Table 5 (above) present a summary of the results from controlled experiments that have tested the effects of uncertainty-framed science messages. The following sections summarize the general trends of results within each set of studies reorganized by the four types of uncertainty.

2.5.1. Effects of consensus uncertainty. The prominent position taken by science communication researchers is that portrayals of disagreement and controversy are detrimental because they instigate, facilitate, or perpetuate public skepticism of science. As such, *consensus* uncertainty has attracted the lion's share of the content analyses that investigate frames in climate change news (e.g., Antilla, 2005; Boykoff & Boykoff, 2004; Brossard, Shanahan, & McComas, 2004; Dispensa & Brulle, 2003; Zhao, Rolfe-Redding, & Kotcher, 2016). The experimental evidence strongly indicates that portrayals of high consensus uncertainty (controversy, disagreement, or balance) will likely have exclusively negative effects on beliefs about the claim, source credibility perceptions, personal certainty, perceived certainty of the scientists, and behavioral intentions (Table 4). There is no theoretical or evidential support to suggest that consensus uncertainty frames have positive effects.

The gateway belief model — which claims that portrayals of strong consensus (low uncertainty) will have positive effects (van der Linden et al., 2015) — is contested by the cultural cognition hypothesis (Kahan et al., 2011), which argues that portrayals of consensus will be discounted by oppositional audiences via motivated reasoning, causing polarization (Bolsen & Druckman, 2017). However, the empirical literature in this vein has been largely commandeered by the van der Linden camp, and their robust findings strongly support their central hypothesis: relative to high consensus uncertainty and relative to control conditions, portrayals of strong consensus (low uncertainty) result in more supportive attitudes, higher perceived risk, greater perceived scientific consensus, and stronger behavioral intentions (Table 4). Together, this body of empirical work suggests that consensus uncertainty — portrayals of controversy, disagreement, or discord within the scientific community — is *negatively* associated with many of the key desired attitudinal and behavioral outcomes of public science communication.

2.5.2. Effects of technical uncertainty. The evidence suggests that communicating some *technical* uncertainty has been associated with positive effects (e.g., higher trustworthiness and behavioral intentions) (Johnson & Slovic, 1995; MacGregor et al., 1994; Morton et al., 2011; Slovic et al., 1984). Most of the risk communication research that found positive effects exclusively used portrayals of technical uncertainty. Still, though, negative effects have been observed on competence perceptions (Johnson & Slovic, 1995). Morton et al. (2011) found that gain-framed technical uncertainty increased behavioral intentions by increasing efficacy perceptions, while loss-framed technical uncertainty decreased behavioral intentions by decreasing efficacy perceptions (Morton et al., 2011).

While this body of research indicates that technical uncertainty does not have the same undeniable negative effects of consensus uncertainty — and may even have positive effects — we must be cautious in generalizing these findings. Most of the risk communication research that has theorized about — and/or empirically tested — the effects of (technical) uncertainty has investigated apolitical science issues such as earthquake risk, cancer risk, or toxic waste (Table 5; McGregor et al., 1994; Slovic et al., 1984). Because uncertainty likely facilitates motivated reasoning, and because motivated reasoning effects are likely stronger in issues on which individuals have strong prior beliefs or salient values, it would not be surprising for technical (or any other) uncertainty to spark more distrust, uncertainty, or rejection than a portrayal of certainty amongst individuals with strong prior opposition to the issue/claim — an unlikely situation for earthquakes or cancer, though possibly less for toxic waste. Thus, it is important to extend these tests to climate change, GMOs, and other controversial science issues.

2.5.3. Effects of scientific uncertainty. *Scientific* uncertainty has largely resulted in positive responses, with some null findings — but never negative effects (Table 5). It should be no surprise that portraying low scientific uncertainty (e.g., stipulating that future research could, potentially, make small adjustments to the current wealth of supporting evidence) has positive effects (Corbett & Durfee, 2004). However, the evidence also indicates that *highlighting strong* scientific uncertainty can have positive effects as well. When Rabinovich and Morton (2012) gave participants a detailed explanation of the inherent role of uncertainty in science, participants responded more positively to subsequent portrayals of uncertainty. Explaining that there is — and why there is — significant scientific uncertainty that surrounds a claim can result in a better understanding of scientific uncertainty which in turn makes uncertain science more acceptable, trustworthy, and normal (Rabinovich & Morton, 2012). Similarly, Jensen and colleagues (2008, 2011) found that statements emphasizing scientific uncertainty ("hedging") increased trust in cancer researchers and reduced fatalistic beliefs about cancer (i.e., that cancer is unavoidable, and any/everything can cause it).

While the summary in Table 5 indicates that scientific uncertainty is sometimes also associated with effects that are no different than the control condition, this is itself a very informative finding because it indicates that (quite unlike consensus uncertainty) scientific uncertainty is never associated with undesirable effects (e.g., decreased trust, lower attitudinal support, lower behavioral intentions). Further, it appears that positive effects can be gained both from emphasizing scientific uncertainties *and* from assuring audiences of minimal scientific uncertainty — likely because they both encourage audiences to consider the broader context or process of science, which fosters confidence in the rigor of the system that produced the claim.

2.5.4. Effects of deficient uncertainty. The effects of portrayals of scientists (or sources) holding *deficient* uncertainty have not been studied with anywhere near the same frequency of consensus, technical, and scientific uncertainty. However, content analyses indicate that frames of deficient uncertainty are not absent in news reporting on prominent science issues such as climate change (Rice et al., 2018; Zehr, 2000). The lone experiment (Kuhn, 2000) found that when a probabilistic risk estimate is portrayed as being caused by deficient uncertainty, risk perceptions are no different than the control group (no uncertainty), and also no different than when the probabilistic risk estimate is portrayed as being caused by consensus uncertainty. In a survey, people reported being more displeased when scientists have deficient uncertainty than when they have uncertainty of other types (Frewer et al., 2003). Clearly, more research is needed to test the effects of frames of deficient uncertainty in both controversial and non-controversial science issues.

2.6. Reconciling the Evidence on the Effects of Uncertainty Frame Types

If Tables 4 and 5 did not (in general) separate the findings by uncertainty type, it would paint a very self-contradictory portrait of the effects of uncertainty-framed science — as it is filled with both positive and negative effects on (often) the same outcome variables. However, separating by uncertainty type reconciles some of this inconsistency. Thus, the most important takeaway from this reorganization of the literature is: the "competing" perspectives about expecting positive and negative effects of uncertainty frames (in general) are not necessarily in competition after all.

That is, the above categorization of extant empirical literature indicates that different types of uncertainty frames can be seen as associated with different trends of effects (although still with some inconsistency) on a variety of attitudinal variables (Tables 4 and 5).

Generally, *consensus uncertainty* associates with the negative, undesirable effects such as lower internal and external certainty, distrust, and lower behavioral intention. In contrast, *technical and (especially) scientific uncertainty* have been found to associate with positive effects such as stronger beliefs, increased credibility perceptions, and higher behavioral intentions. While several studies have found that technical and scientific uncertainty have no effect on the outcome variables of interest, these are very valuable findings because they are in stark contrast to the largely negative effects of consensus uncertainty.

One potential explanation for these differences across types is that consensus uncertainty emphasizes that the state of the evidence does not yet constitute an answer, *not even* one that is tentative, preliminary, or roughly estimated. On the other hand, scientific and technical uncertainty emphasize that the science *has produced* an answer, albeit one that might be qualified with some well-understood, clearly specified room for approximation error or future further revision.

While there is little experimental evidence regarding the effects of *deficient uncertainty*, it resembles consensus uncertainty in that it communicates that the state of the evidence does not yet constitute an answer. Although we cannot be confident at this point that portrayals of deficient uncertainty produce similar negative effects as consensus uncertainty, some exploratory research has shown that people report being more displeased by scientists' deficient uncertainty than by other types (Frewer et al., 2002).

It is important to emphasize that this review does not constitute conclusive evidence that variations in uncertainty frame types are a significant cause of the disparate findings. This is, in part, because the extant findings have been produced across myriad methodologies, issues, topics, sources, measures, etc. While Table 4 and Table 5 are informative, there are *far from* enough observations in each "cell" of the combinations of types, issues, and dependent variables to inform confident meta-analytic conclusions about the relative effects of each particular uncertainty type. Further, the studies were each recategorized into the four types of uncertainty based on interpretation of the information provided in the publications; but that does not guarantee that each recategorized study belonged solely or even necessarily to that category. Thus, we must be wary of assigning causality to variations in the uncertainty type. There are many methodological or contextual factors that could also be responsible for causing the observed differences across studies, such as different samples, methodologies, measures, and issues. A meta-analysis of a much larger number of studies involving uncertainty and scientific communication would be necessary to make such fine-grained determinations. Unfortunately, a sufficiently large body of literature does not yet exist. Therefore, one of the most valuable contributions of this dissertation will be a controlled test that facilitates valid, robust comparisons of individuals' responses to each of these four uncertainty types within one study.

2.7. Comparisons of Relative Effects of Uncertainty Frames

To date, exactly five studies have undertaken a direct empirical comparison of the effects of two or more uncertainty frame types in a controlled experiment (instead of, say, the comparison of one frame type to a control, or variations in the degree of uncertainty within one frame type). First, Corbett and Durfee (2004) report that individuals' personal certainty about climate science finding in response to a *consensus* uncertainty frame was not significantly different than the control condition. However, it is important to note that the "context" condition (a portrayal of low *scientific* uncertainty — or, stated conversely, higher certainty) resulted in significantly more internal certainty than either the *consensus* uncertainty condition or the *control* condition. Thus, this finding indicates *a significant difference between* uncertainty types in their effects on internal certainty. However, this conclusion is limited by the small, non-representative student sample. Further, the authors controlled for political ideology (muting its effect on the dependent variables), rather than testing for an interaction effect. Thus, for example, it may be that responses to consensus uncertainty were polarized by ideology, which is a very important question. In addition, this experiment compared a condition of very low uncertainty of one type (scientific) against a condition of high uncertainty of a different type (consensus), thus disabling any conclusions about whether the difference is due to the type itself.

Second, Binder, Hillback, and Brossard (2016) found that news articles with portrayals of *consensus* and of *scientific* uncertainty, respectively, about the potential risks of emerging science (nanotechnology) *only* produced significantly different levels of perceived risk in individuals who scored high in deference to science. That is, those (and only those) individuals who reported strong trust and respect for science responded to consensus and scientific uncertainty differently, reporting higher perceived risk after the consensus uncertainty portrayal. However, these results are surrounded by a great deal of uncertainty (of all types). First, the experiment was conducted using a very small student sample (some cells had 9, 11, or 16 observations). This disables the detection of small (or even moderate) effects, such as those that are most commonly observed in message/framing manipulations.

Third, Kuhn (2000) used a similarly small student sample (n=177) and found that when *technical* uncertainty (a range of risk estimates) is portrayed as being caused by expert disagreement (*consensus* uncertainty), then individuals' responses to the message are more likely to follow in line with pre-existing beliefs and values, as compared to individuals' responses to messages that had *technical uncertainty alone*. However, there was no main effect of variation in uncertainty portrayal type (which resembled technical, consensus, deficient, and no-uncertainty, respectively) on perceived risk. Sampling limitations aside, this is an important finding because it suggests that the effect of prior beliefs on post-message attitudes is a stronger effect than message variations, and that motivated reasoning is especially pronounced in response to *consensus uncertainty* portrayals. Still, as with the first two studies in this section, this piece of evidence leaves much unanswered about how other attitudinal variables prioritized in the science communication literature (e.g., credibility, behavioral intentions) would be affected in response to uncertainty frames in a polarized, contentious topic and/or in a nationally representative sample.

Fourth, Rabinovich and Morton (2012) compared conditions of *technical* and *consensus uncertainty* frames, respectively, observing the effects of a message about the effects of climate change on behavioral intentions in a small (n=100) student sample. There was no main effect of either uncertainty condition on behavioral intentions, and no difference in effects between the uncertainty manipulations. But (as mentioned previously), beliefs in the role of uncertainty in science was a significant moderator of both messages' effect on behavioral intentions such that individuals who reported believing that the purpose of science is to find absolute truths reported lower behavioral intentions than those who believe science is a perpetual debate. This study did not investigate the role of ideology/worldview, despite using climate change as the issue context in the experiment.

Fifth, and finally, Clarke and colleagues (2015) investigated the effect of two different uncertainty types on individuals' internal certainty (their personal certainty regarding claim) and external certainty (their perception of how certain the scientists are) about the vaccine-autism controversy. In one condition, they portrayed low *scientific uncertainty* by highlighting the substantial weight of evidence indicating no vaccine-autism link. In another condition, they portrayed high *consensus uncertainty* by highlighting both supporting and opposing opinions from multiple "experts," indicating discord amongst scientists about the claim. Not surprisingly, the low scientific uncertainty condition resulted in higher IC and EC than the high consensus uncertainty condition. However, one of the limitations of this study is that it is comparing *high* uncertainty in one uncertainty type (consensus) with *low* uncertainty (scientific) in the other type. Thus, the observed difference may just be because of higher/lower uncertainty, rather than informing our knowledge about the relative effects of different uncertainty frame types. One of the important questions that is unanswered here is whether *high scientific uncertainty* (explaining that a finding is preliminary and needs replication, or stating the inherent limitations of the research) has any different effect than portraying a high degree of discord or controversy among experts (consensus) — or, for that matter, whether it is different than portraying large uncharted gaps in knowledge (*deficient*), or a wide range of imprecision in an estimate (*technical*).

In sum, the extant literature has left a significant gap in our knowledge of the specific effects of these uncertainty types, despite a) their frequent appearance in content analyses of actual public science communication, *and* b) the theoretical consensus of science communication scholars about their potent role in influencing public interpretations and responses to science. While the above overview indicates that consensus uncertainty has especially negative effects and that technical and scientific uncertainty sometimes have positive effects — which are observations that resonate with intuitive and theoretical expectations — such observations are far from being definitive, comprehensive, or even strictly objective.

The few attempts at *comparing* the relative effects of different uncertainty types in experimental settings each have methodological shortcomings that dampen confidence in the generalizability of their findings. Further, each of these few studies compares some — but not all — of the four uncertainty types. It is clear that a study comparing the effects of all uncertainty types within one robust, controlled, consistent methodology is needed to finally generate useful, defensible data that inform the important question of the effects of uncertainty frames on outcome variables that are relevant to most science communication contexts.

2.8. Outcome Variables of Interest

Several different positive and negative outcomes have been investigated in the research summarized above. In order clarify the context of interest of this dissertation, it is important to describe each of these outcome variables in more detail. Of these, this dissertation will test effects on internal certainty, external certainty, credibility, risk perceptions, and behavioral intentions.

Naturally, the different research fields that have investigated the nature and effects of uncertainty have often been interested in different outcome variables. For example, the risk communication literature often associates portrayals of technical uncertainty in a risk estimate with, unsurprisingly, individuals' *perceived risk* (e.g., Binder et al., 2016; Han et al., 2011). Science communication research (e.g., Dixon & Clarke, 2013) often investigates the effects of consensus uncertainty on *internal certainty* (IC; an individual's personal opinion of the certainty of a claim) and *external certainty* (EC; an individual's estimation of how certain the experts are). Research specific to environmental communication is often concerned with — beyond pro-environmental attitudes alone — *behavioral intentions* toward, say, a sustainable lifestyle or specific action (e.g., Morton et al., 2011). The selection of these individual pairings between fields of study and outcome variables of interest has largely been quite logical. However, the variations and inconsistencies across studies in methods and measures have disabled the potential for any meta-analytic conclusions about the relative effects of uncertainty frames on these different outcome variables, respectively.

Table 4 and Table 5 demonstrate six recurring dependent variables. These are *supportive attitudes/opinions toward the claim, source credibility, internal certainty, external certainty, perceived source credibility, perceived risk,* and *behavioral intentions.* Of these, "supportive attitudes" have the greatest variation in their operationalization — naturally, since there are many distinct attitudes that one could hold in support/opposition to a claim, many claims within a topic, many topics within an issue, and many issues represented across the different studies. In fact, the remaining five variables all incorporate or infer a type of belief support for the claim (especially internal certainty, and also risk perceptions *if* the message was about a risk-related topic). Due to the overlapping, or nested, nature of these constructs I will only cover these latter five specific constructs in this dissertation and will leave out generalized attitude support in the remaining review, discussion, and analyses. Below, for each of these five variables, I mention tentative thoughts regarding differential effects of uncertainty types that are suggested by the extant (usually exploratory) research.

2.8.1. Credibility. *Credibility* refers to an individual's perception of the trustworthiness/honesty and the expertise/competence of the scientist(s) advancing the claim or research finding. These two distinct dimensions of credibility are well-established in a longstanding body of literature (e.g, Hovland, Janis, & Kelly, 1953). For the current study, the distinction of these two subdimensions is important, because — as summarized above (Table 5) — research indicates that uncertainty frames may have differential effects on each dimension (Johnson & Slovic, 1995; Nakayachi et al., 2018). Consistent with this preliminary evidence and theorizing, we could imagine that deficient or consensus uncertainty might have negative effects on perceived expertise, while technical or scientific uncertainty might have positive effects on trustworthiness. As detailed in the methods sections, this dissertation recognizes this bipartite theoretical nature of this construct by first investigating the appropriate factor structure of a credibility scale, and also modifying the measurement model to allow correlated errors between two items within the trustworthiness subdimension.

2.8.2. Internal certainty. *Internal certainty* refers to an individual's opinion of the degree to which a claim or research finding is (un)certain (e.g., Binder et al., 2016; Chang, 2015). For example, studies have operationalized this with Likert-style responses to statements such as "Please indicate how certain YOU are that _*claim*_" (e.g., Corbett & Durfee, 2004; Dixon & Clarke, 2013), or "I feel certain about the level of environmental risk posed by nanotechnology" (Binder et al., 2013). While the literature indicates that internal certainty is negatively affected by consensus uncertainty frames (Clarke et al., 2015; Dixon & Clarke, 2013), there is either no evidence or mixed findings regarding the other uncertainty types. It is difficult to say, at this point, how or if internal certainty would be differentially affected by the different uncertainty types. Thus, this is a central question of this dissertation.

2.8.3. External certainty. *External certainty* refers to a 2nd-order opinion — an opinion about what someone else's opinion is. Specifically, an individual's perception of the degree of certainty that scientists hold about a claim or research finding (e.g., Dixon & Clarke, 2013; Lewandowsky et al., 2013). This has been defined in two slightly different forms, one referring to an individual's estimation of the internal certainty of an individual scientist or scientist group (e.g., Dixon & Clarke, 2013), and the other referring to an individual's estimation of scientists' consensus (e.g., Johnson, 2017; Koehler, 2016; Lewandwosky et al., 2013). For this dissertation, the measure of external certainty will encompass both dimensions by using a measure that targets each of the four uncertainty types.

2.8.4. Behavioral intentions. *Behavioral intentions* refer to an individual's self-reported intentions to engage in a behavior that is demonstrates support for — or positive attitudes regarding — a claim. It is important to measure this construct not only because it is of frequent interest in the extant literature (e.g., Chang, 2015; Rabinobich & Morton, 2012), but also because behavior change is often an intended outcome of science communication campaigns and behavioral intentions are largely considered the best (though admittedly still weak) predictor of actual behavior (Fishbein & Ajzen, 2011).

The extant literature (based on the reorganization in Table 5) suggest that consensus uncertainty has a negative effect on behavioral intentions (Chang, 2013; 2015; van der Linden, 2017; 2018), while technical and scientific uncertainty have no direct effect (Morton et al., 2011; Nakayachi et al., 2018). This dissertation uses a measure of behavioral intentions that explores a wide range of behaviors (e.g., sharing information with others, donating money to a cause, voting for legislation) that are all applicable to diverse science issues — with the goal of determining which of these behaviors group together to represent an attitude of general willingness to take real-life action as a positive response to scientific evidence.

2.8.5. Perceived risk. *Perceived risk* refers to an individual's estimation of the likelihood, salience, and/or severity of a threat (e.g., Binder et al., 2016; Bolsen & Druckman, 2017; Han et al., 2011). Of course, this is more relevant in some messages about some issues/topics than others. While not applicable to all uncertainty-framed science, it included in this study because is important that this study is informative and applicable to the risk communication literature, due to the prominence of that field (and risk-related issues/topics) in the extant research on the effects of uncertainty frames. Some prior research indicates that technical uncertainty can boost risk perceptions because it increases the variance in possible outcomes, thus making unlikely threats seem more possible (Han et al., 2011; Johnson & Slovic, 1995). In contrast, strong consensus (low uncertainty) is associated with higher perceived risk (which was consistent with the message; Bolsen & Druckman, 2017) than a control condition, so we might infer that — unlike technical uncertainty — high consensus uncertainty would be associated with lower risk perceptions than a control condition. Thus, it may be that technical and consensus uncertainty affect risk perceptions differently.

2.8.6. Manipulation check variables. All the above five attitudinal variables will be measured as dependent variables in the survey experiment proposed in Chapter 3. However, intuitively, the effects of *portrayals of* scientists' own uncertainty (uncertainty frames) on *perceptions of* scientists' own uncertainty (i.e., *external certainty*) will be used as a manipulation check. As with many experiments, the stimulus manipulations of this dissertation were pretested, pilot tested, and repeatedly revised to strategically affect the manipulation check items in a desired direction and degree in the main study. Thus, it would be tautological at best (and disingenuous at worst) to, then, propose and test hypotheses and research questions about the effect of the manipulation on that manipulation check construct. Therefore, the hypotheses and research questions of this dissertation focus only on the four 1^{st}-order attitudes of internal certainty, risk, credibility, and behavioral intentions, and not on external certainty.

2.9. A Conceptual Model of the Effects of Uncertainty Frames

The language of this dissertation, thus far, has reflected the extant literature's focus on testing the effect of a message manipulation that uses one or more uncertainty frames. This dominant perspective views the message variations as the principal causal force (or at least the most interesting one), with individual variables such as motivated reasoning, ideology, general worldview, science attitudes, and credibility perceptions positioned as moderators of that main effect.

Despite this being the normative structure, most scholars agree that prior beliefs and ideologies, not message characteristics, are the best predictors of responses to persuasive messages, especially when those messages are regarding claims in value-laden, politically charged, or identity-salient issues (e.g., Kahan et al., 2012). Even in the empirical research on the effects of uncertainty frames, several studies have found fully-crossed interactions (Tables 4 and 5; e.g., Aklin & Urpelainen, 2014; Nan & Daily, 2015) — suggesting that individuals' prior issue position directly or indirectly affects whether their responses are positive or negative, while the uncertainty frame manipulation determines either the emergence or the extent of those effects. While it is true that, mathematically, an interaction effect can be viewed from either perspective, our choices in modeling these relationships are not inconsequential and should be justified by theory and extant findings. Rather, presenting these as moderator effects instead of as interaction effects emphasizes which is the modeled direct effect and which is the modeled moderating effect.

For example, Dieckmann and colleagues (2017) decided to break from the norm and, instead, approach their investigation of motivated reasoning in interpretations of uncertainty from this latter perspective. Their findings confirm that individuals' position on hierarchical/egalitarian and individualist/collectivist worldview scales (which underlie many political beliefs) predict existing opinions about the issue (in this case, climate change), which in turn predicts responses to uncertain science. Similarly, Kuhn (2000), in studying responses to environmental risk, hypothesized that uncertainty information manipulations will *not* result in overall mean differences in risk perceptions. Rather, Kuhn argued "a pro-environmental attitude will correlate positively with perceived risk, but the strength of the relation will vary according to the presence and type of uncertainty information" (p. 43). This hypothesis was supported in a small student sample.

Importantly, when describing and displaying the conceptual and structural models of the constructs relevant to this dissertation, I will follow this non-normative perspective, such that the uncertainty frame type is a variable that *moderates* the main effect of an individuals' prior issue beliefs on their response to information about new scientific research. As such, in this study, the "motivated reasoning effect" is the linear relationship between one's general prior opinions about an issue and their subsequent attitudinal responses to a new, specific piece of information about a science claim relevant to that issue. However, still, some of the analyses do test mean differences in outcome variables across conditions of uncertainty frame types (which implicitly takes the normative perspective).

Figure 1 displays a full theoretical model that depicts the relationships of the major constructs that have emerged thus far in the literature review, using the perspective of Dieckmann et al. and Kuhn (uncertainty frames as a moderator of the effect of prior opinion on attitudinal responses). Needless to say, this model is complex. The reader should, at this point, be warned that this dissertation *will not* attempt to test all of these relationships. The boundaries of the present investigation will be discussed shortly.

The following is a brief synthesis of the overall meaning of Figure 1. The relationship between worldview and general issue position is dependent on, of course, which science issue/topic it is (A). Regarding the three topics investigated in this dissertation, we can expect that worldview predicts individuals' pre-existing general issue position on climate change, but not on labeling of GMO foods (Pew Research Center, 2015, 2016) or on the occupational hazards (mechanical vibrations) of farming. The latter two issues do differ significantly from each other — specifically, in the strength and variance of public opinion — but are both distinct from climate change by being apolitical.

Individuals' general prior issue position predicts their responses to a message/claim of science (B), which is motivated reasoning. It is reasonable to expect that this relationship will be stronger in issues where pre-existing issue positions are strong and entrenched (e.g., climate change and GMO foods), compared to a more "neutral" issue where people do not have strong, entrenched pre-existing issue positions (occupational hazards of farming).

Any effect of the presence or type of uncertainty frame used in the message would moderate (C) the motivated reasoning effect (B). The literature review suggests that individuals' deference to science also predicts responses to messages/claims of science (D), but that the uncertainty frame likely would moderate this effect as well (E). Finally, regarding positivist understanding of science (F, G), the degree to which an uncertainty frame has a moderating effect is dependent on the degree to which the individual believes uncertainty is an inherent, integral component of the scientific process.

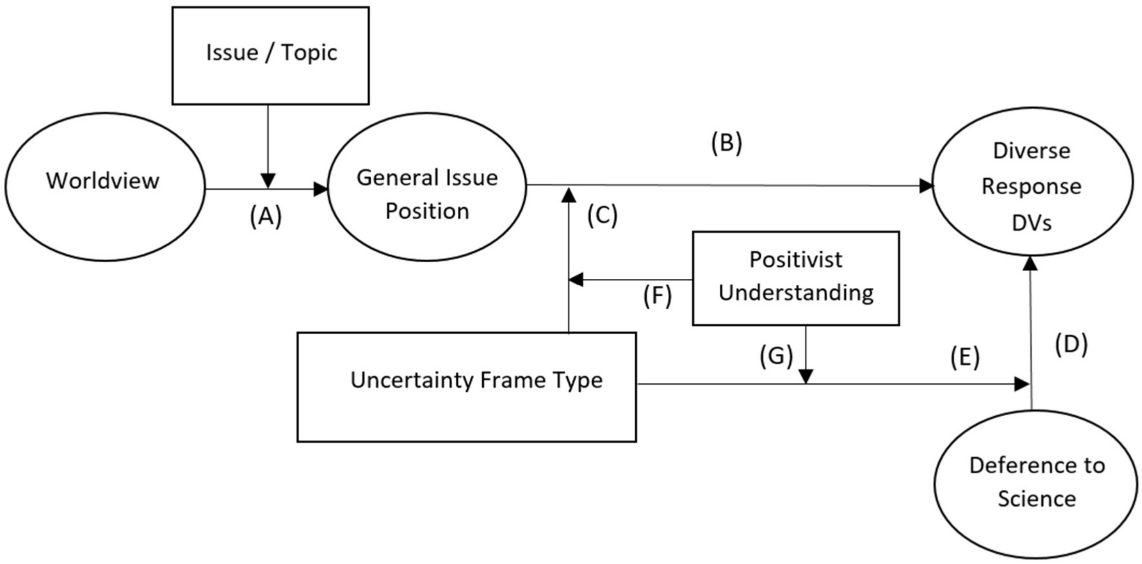


Figure 1. Full theoretical model implied by the literature review.

Despite its complexity, this model — like most other theoretical models in our discipline — is in fact massively and deceptively *over-simplified*. For example, it is intuitive that deference to science would likely also moderate the effect of worldview on general issue position (A), as would positivist understanding of science. In truth, attitudinal responses and communication in general just are not amenable to two-dimensional, directional path models. However, it is helpful to remember that such models are not intended to be exhaustive descriptions of how the brain works, but rather function as a visual aid to help us understand theory and contextualize empirical findings. Thus, the caption of Figure 1, which reads "implied by the literature review" is quite purposeful. These relationships are displayed because they are suggested by the literature review in Chapters 1 and 2, with the open admission that this model — and all others like it —should only be used as a visual aid and not as a mechanistic schematic of human thought. When writing a dissertation about the uncertainty attached to science, it is imperative to be open about such uncertainties attached to communication science.

Further, the time and space constraints of one dissertation do not allow a comprehensive test of this entire model. As a reminder, the fundamental question facing the extant literature is whether and how uncertainty frames affect responses to messages of science, and whether different types do so differently. Therefore, this dissertation will address this question and break new ground by specifying the relative effects of various uncertainty frame types, and the role of motivated reasoning, in a controlled experiment. Portions of the model that are not necessary for these purposes will not be tested in this dissertation.

For example, in the full theoretical model, significant conceptual and analytic complexity is introduced with the inclusion of positivist understanding of science as a moderator (F, G) of interaction effects. Therefore, for this dissertation, this construct will be used only as a control variable. Similarly, the role of deference to science as a predictor of the outcome variables — and its interaction with uncertainty frame type — is implied by extant theory and evidence, but quantifying the effects of deference to science is not the primary aim of this dissertation. As such, it will also be used as a control variable. The scope of this dissertation is displayed in Figure 2. Figure 2 also displays the hypotheses discussed in the next section.

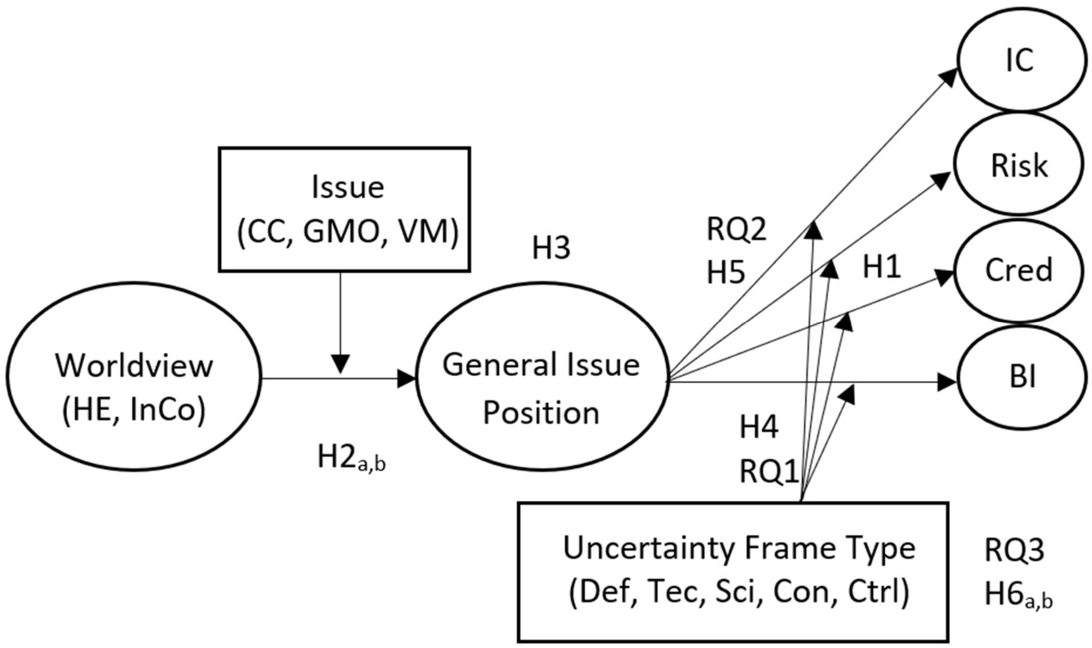


Figure 2. Conceptual model for this dissertation, with the hypothesized relationships and research questions presented in Chapter 2.

Note. IC=internal certainty; Risk=perceived risk; Cred=credibility; BI=behavioral intentions. HE=hierarchical/egalitarian; InCo=individualist/collectivist; CC = climate change; GMO = genetically modified foods; VM = vibrating machinery.
Def=Deficient; Tec=Technical; Sci=Scientific; Con=Consensus; Ctrl=Control (no uncertainty frame).

2.10. Summary, Hypotheses, and Research Questions

2.10.1. Prior opinions as predictors of responses to science. This review has illustrated that prior issue beliefs predicts responses to science (via motivated reasoning), and in some science issues, those prior issue beliefs are predicted by broader worldviews. Further, the literature suggests that different types of uncertainty frames may result in significantly different responses to science messages, that uncertainty frames in general are fertile ground for motivated reasoning, and that some uncertainty frame types may facilitate more motivated reasoning than others. Thus, in this section, I first offer hypotheses that specify the effect of prior beliefs (and sometimes worldview) on attitudinal responses to new science findings (via motivated reasoning). I then pose research questions about how these responses might differ across variations across the four uncertainty frame types (that is, how the motivated reasoning relationship between prior beliefs and attitudinal responses is moderated by uncertainty frame type). Specifically, consistent with the motivated reasoning literature, I expect that:

H1: Individuals' prior general issue position will predict attitudinal responses to a new claim of scientific research, such that support (opposition) in general prior issue positions will be positively (negatively) associated with the attitudinal response variables of internal certainty, risk perceptions, credibility, and behavioral intentions.

As briefly previewed in Chapter 1.1, this dissertation tests these, and other, relationships separately in three distinct issues: one socially contentious issue (climate change) that is divided by political partisanship, one socially contentious issue (GMO foods) that is not divided on party lines, and one issue (occupational hazards of farming) that is — to the best of our knowledge — *neither* widely contended *nor* is divided on party lines.

In the context of climate change specifically, we can expect worldview to be correlated with attitudinal responses to messages about climate change, as is well documented in prior research. But a more immediate cause of these responses is the existing issue beliefs that an individual already holds (H1). This is because in a politicized issue like climate change, prior opinions about the specific issue likely follow from individuals' worldview. But a persons' prior issue opinions are more closely and directly linked to attitudinal responses to information about that issue than their general values of, say, egalitarianism are — and may even be a literal consequent of those broad worldviews. Dieckmann and colleagues (2017) advance this argument and find that, indeed, prior issue position mediated the effect of worldview on attitudinal responses to science messages. Thus, we can expect also expect that, specific to climate change:

$H2_a$: Individuals' prior general issue position about climate change will mediate the effect of worldview on attitudinal responses of internal certainty, risk perceptions, credibility, and behavioral intentions.

and

$H2_b$: In the case of climate change, more egalitarian and collectivist worldviews will be associated with more supportive prior issue positions, relative to more hierarchical and individualist worldviews.

It is not clear, though, whether the mediation predicted in $H2_a$ can be expected to be full mediation or partial mediation. That is, while we can expect prior issue position about climate change in general to explain a significant amount of the effect of worldview on the attitudinal responses, it may or may not explain all the effect. Still, though, because of motivated reasoning and the partisan politicization of climate change (specifically), we can expect that:

$H2_c$: In the case of climate change, any effect of worldview on attitudinal responses not explained by prior issue position will be such that more egalitarian and collectivist worldviews are associated with more supportive attitudinal responses, relative to more hierarchical and individualist worldviews.

Unlike climate change, the issues of GMO foods and occupational hazards of farming (the dangers of vibrating machinery) are not associated with partisan liberal or conservative politics. Therefore, because the worldview dimensions of hierarchical/egalitarian (HE) and individualist/collectivist (InCo) are proxies for political ideology and party affiliation (in the American two-party system), we can expect that:

H3: The effect of worldview dimensions on prior issue position is dependent on the issue, such that the relationship between the worldview dimensions (HE and InCo) and prior issue position will be stronger in a partisan-politicized topic compared to science topics that are not attached to partisan politics.

2.10.2. Relative effects of uncertainty types. It is clear that a) uncertainty is inherent in accurate science communication, b) uncertainty is often framed in four distinct types, c) there are discrepant perspectives and evidence about the effects and ideal uses of uncertainty frames, and d) little regard has been given to distinguishing the nature and effects of these disparate frames that reside under the generalized monolith of uncertainty. Thus, it is incumbent upon researchers to investigate whether, when, and/or why different uncertainty types correspond with increases or decreases in attitudinal support for a science claim, and how they interact with other variables that been identified as predictors of responses to uncertainty (e.g., prior issue position, trust in science). This must be done within the controls of a large experiment, where differences in effects of all uncertainty types can be observed across a constant methodology (issue, sample, dependent measures, etc.). However, there have been very few scholarly inquiries to inform such questions.

While this dissertation structures prior issue position as the main effect on attitudinal responses, it is still important to use this opportunity to first observe any mean differences in attitudinal responses across the uncertainty frame types — in part because the question has been a consistent focus of the extant literature.

Of the four uncertainty frame types, the type with the most (and most clear) experimental evidence supporting causal effects is consensus uncertainty. Table 5 indicates significant support for expecting that, relative to a control condition of no uncertainty frame, portrayals of consensus uncertainty will result in less supportive attitudinal responses (e.g., lower certainty, lower perceived credibility). This is also supported by the theoretical interpretation that consensus uncertainty portrays an absence of an identifiable answer or verdict; in fact, it does not even convey a tentative or vague answer. Due to these arguments and the extant experimental evidence, it can be expected that:

$H4_a$: Overall (controlling for relevant individual prior attitudes and demographics), a claim of scientific research containing a consensus uncertainty frame will correspond with lower internal certainty, perceived risk, credibility, and behavioral intentions, compared to claims portrayed without any uncertainty frame.

While it is still unclear from the review of Chapter 2 whether scientific and technical uncertainty frames have positive effects on attitudinal responses that are significantly different than the control, it seems clear that they do not have significant negative effects. Therefore, it is reasonable to expect that responses to consensus uncertainty will be different than responses to scientific or technical uncertainty, such that:

$H4_b$: Overall, (controlling for relevant individual prior attitudes and demographics) a claim of scientific research containing a consensus uncertainty frame will correspond with lower internal certainty, perceived risk, credibility, and behavioral intentions compared to claims containing a technical or scientific uncertainty frame.

As reviewed above, deficient uncertainty has not been studied often in experimental contexts, but it is quite similar to consensus uncertainty in that it communicates that there is currently no clear answer, not even a tentative or vague one. Further, exploratory focus group data suggests that the public views deficient uncertainty as the *least preferred type* of uncertainty for experts to have (Miles & Frewer, 2003). As such, it would not be surprising to observe less supportive attitudes in response to claims containing a deficient uncertainty frame, compared to a claim containing scientific uncertainty, technical uncertainty or no uncertainty. However, the current theorizing and scant empirical evidence does not justify a hypothesis.

Similarly, experimental evidence and theory suggests that technical and scientific uncertainty frames can be associated with responses of heightened credibility perceptions, and confidence in the findings. This would fit the explanation that technical and scientific uncertainty frames communicate that scientists have produced an answer that is trustworthy, and the uncertainty frame offers more specificity to that answer. However, due to mixed and minimal evidence, a hypothesis is not justified at this time. Thus, I also ask:

RQ1: Overall (controlling for relevant individual prior attitudes, behaviors, and demographics), how do individuals' responses to uncertain science (by way of internal certainty, perceived risk, credibility, and behavioral intentions) compare across claims containing different types of uncertainty frames (four types and no-uncertainty), within each of the three issues?

Kuhn (2000) found that portrayals of consensus uncertainty cause individuals' environmental risk perceptions to be most polarized — and aligned more closely with their pre-existing environmental ideology. This motivated reasoning effect was strongest in the consensus uncertainty and the no-uncertainty condition, and weakest in the deficient uncertainty and technical uncertainty condition. As summarized above, a reasonable theoretical argument to explain this finding is that consensus uncertainty (unlike the other uncertainty types) actually provides *support for dissenting opinions* (in the form of dissenting expert opinion), rather than only providing room for doubt. Thus, I expect that

H5: Attitudinal responses (internal certainty, perceived risk, credibility, and behavioral intentions) to a claim of science will be more strongly predicted by prior issue position when the claim is portrayed with a frame of consensus uncertainty, than when it is portrayed with any of the other (deficient, technical, or scientific) uncertainty frame types.

The potential differential motivated reasoning effects of other frame types are less clear. Tables 4 and 5 indicate that there are very few instances where one particular combination of uncertainty type, issue, and dependent variable has been tested more than once. Therefore, due to the theoretical support for competing perspectives on the effects of general uncertainty, and due to scant empirical evidence about the relationship between each uncertainty type and each dependent variable, it is not prudent to offer confident hypotheses about how the other three uncertainty types compare with each other (or the control) in their respective moderating effects on the relationships between prior opinion and the outcome variables. Thus, for deficient, technical, and scientific uncertainty frame types, I pose the following research questions regarding the relationship between the uncertainty types, prior opinion, and the three attitudinal outcome variables.

As discussed in the review above, the specific type of uncertainty frame (or lack thereof) that is used to portray this claim may result in different responses to the message, thereby moderating the relationship between prior issue position and attitudinal responses. To assess this interaction (or, stated differently, to compare the motivated reasoning effect across uncertainty types), I ask:

RQ2: How do the relationships between prior issue position and the attitudinal responses (internal certainty, perceived risk, credibility, and behavioral intentions) compare across the four different frame types (and the no uncertainty frame)?

It is also important to determine whether uncertainty frames, overall, do in fact facilitate or catalyze motivated reasoning relative to the absence of uncertainty frames. While we cannot yet make confident predictions about comparisons between each frame type regarding this effect, we can expect in general — due to the heightened ambiguity of uncertainty frames — that:

H6: The motivated reasoning effects presented in H1 will be stronger in science news that contains (any) one of the four uncertainty frames, compared to science news without (i.e., science news with no uncertainty frame).

An investigation of these questions will clarify the relative (potentially different) effects of the four uncertainty frames in science communication — and their interplay with individuals' prior general issue positions — across a consistent and robust methodology.

Chapter 3: Methods

The hypotheses and research questions presented in Chapters 1 and 2 were investigated through an online survey experiment. Here in Chapter 3, I outline the design, sampling, measures, and analyses involved in this study. Each of the measures of the outcomes variables was assessed in a pilot test in order to check the reliability and dimensionality of the measures (reported in Section 3.4), and to guide any necessary revisions. Further, the experimental manipulations (described in Section 3.5) were pilot-tested to check for appropriate strength and validity (results reported in Chapter 4).

3.1. Design

3.1.1. Conditions. In a between-subjects three (issue) by five (frame type) factorial experimental design, participants (n=2247) responded to demographic and attitudinal self-report items and also read a simulated news article that reported on scientists' summary of a new scientific finding. This news article was the experimental manipulation, and as such it was manipulated to vary across three issues/claims (the effect of climate change on farmers, the effect of GMO labeling laws on farmers, and the effect of exposure to vibrating machinery on farmers) and five types of uncertainty portrayals (deficient uncertainty frame, technical uncertainty frame, scientific uncertainty frame, consensus uncertainty frame, and a control condition with no uncertainty content). All pre-test measures (demographic and attitudinal predictor variables) and post-test measures (responses to the stimulus message) were administered in each of the conditions.

Table 6

Conditions by Uncertainty Type and Issue

Frame Type	Deficient	Consensus	Technical	Scientific	Control

Issue/Claim	CC	GMO	VM	CC	GMO	VM	CC	GMO	VM	CC	GMO	VM	CC	GMO	VM
Condition #	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15

Note: CC=climate change effects; GMO=GMO labeling effects; VM=vibrating machinery effects.

As previewed in Chapter 1, the purpose of using three separate issues is test the hypotheses and research questions in diverse contexts, since many effects may be issue-specific. Due to this dissertation's focus on motivated reasoning, this experiment uses three issues that might vary with respect to what (and how) prior opinions and ideologies influence responses to claims of science. Specifically, the issues include a science issue in which much of public has strong prior opinions driven by political ideology (climate change; Kahan et al., 2011), a science issue in which much of the public has strong prior opinions that are *not* driven by politics (GMO foods; Hassell & Stroud, 2018; Pew, 2015), and a science issue in which most of the public does *not* have strong prior opinions and is also *not* driven by politics (occupational hazards of farming — vibrating machinery in particular). Overall, from a motivated reasoning standpoint, this set of three issues is quite heterogeneous. The relationships between worldview and prior issue position on these issues is also hypothesized in $H2_{a,b}$ and is reported in Chapter 4.

In addition to the strategic ways in which they differ, these three issues were also chosen because of how they are alike. That is, each of the three claims that were presented in the news articles has a strong level of actual scientific support in the real world, and each of the three claims was a scientific finding that suggested tangible risk to farmers and agriculture workers. In the climate change conditions, the claim was of the negative effects of climate change on farmers and agriculture workers. In the GMO conditions, the claim was of the negative effects of GMO labeling laws on farmers and agriculture workers. In the farming occupational hazards conditions, the claim was of the negative effects of vibrating machinery on farmers and agriculture workers. Regarding the latter topic, while scientists are confident (and alarmed) that extended contact with vibrating machinery (tractors, power tools, etc.) is extremely damaging to musculoskeletal health (e.g., Langer, Ebbesen, & Kordestani, 2015; Lings & Leboeuf-Yde, 2000), there is no indication that significant portions of the general public have strong (or any) pre-existing opinions about this issue, and the opinions that do exists are likely not politically motivated. Even occupational health and safety professionals are largely unaware of the large body of evidence regarding this occupational hazard, as one survey found that 70% of a sample of almost 3,000 occupational safety professionals had a "less than basic" understanding of the health risks of long-term exposure to whole-body vibration (Paschold & Sergeev, 2009).

The use of four different uncertainty frame types, as well as a control condition, enables tests that inform $H4_{a,b}$, H5, H6, RQ1, and RQ2. These tests make comparisons of the levels of post-stimulus attitudinal responses across the uncertainty type conditions, respectively, within each of the three issues.

3.1.2. Procedure. In each of the 15 conditions, participants first were informed of the general nature and purpose of the study, and were given the opportunity to indicate their consent to participate. They also were reminded that they could cease participation at any time and still receive their full compensation. After consenting to participate, participants were reminded that their careful attention to each individual item on each question was very important, and then were asked to respond to a set of pre-stimulus demographic and attitudinal questions. Some of these were used to build a quota-based sample that approximated current U.S. proportions (age, education, gender, political party affiliation), some of these were used as control variables and predictor variables in various analyses (education, gender, political party affiliation, general issue position, hierarchical/egalitarian worldview, individualist/collectivist worldview, deference to science, and frequency of news media consumption), and some of these were used as distractor items (ethnicity, family size) to obfuscate the true focus of the study and dilute any priming effect of the measures that were most salient to the later measures.

Then, the main stimulus message — a news article (which varied by the three issues and the five framing manipulations) — was displayed. The stimulus is described in detail in Section 3.6 and exemplar stimuli from each of the three issues are available in Appendix A. After participants read the news article, they responded to a series of questions that measured the outcome variables. The full list and operational details of these variables is given in Section 3.5. After completing these measures, participants were directed to a debriefing page that explained the true nature of the study, provided contact information for the principal investigator, and thanked them for their participation.

3.2. Sample

3.2.1. Sample sourcing. Participants were recruited using Qualtrics (www.qualtrics.com/online-sample), a 3rd-party survey recruitment, software, and management service that offers a "Panels" product that assembles custom-ordered samples for survey research by selecting the participants recruited by a large assortment of other, traditional market research panels to fill the particular requirements of any desired proportions of demographics (e.g., age, education) or attitudinal characteristics (e.g., political opinion). The participant recruitment is usually done via ads on social media, traditional websites, and via email — usually offering incentives such as gift cards or money in exchange for participation. Opt-in online panels are frequent in social science research and participants in them perform similar to nationally-representative probability samples and much better than student convenience samples (Bartneck, Duenser, Moltchanova, Zawieska, 2015; Hauser & Scwartz, 2015; Kees, Berry, Burton, & Sheehan, 2017; Leeper & Mullinix, 2015). Also, Qualtrics claims that the participants they recruit are more naïve to academic survey research and experimental manipulations, compared to the "professional survey-takers" that flood the worker pools of platforms like Amazon's Mechanical Turk (Mturk). Further, as described in Section 3.2.3, Qualtrics enables many different methods of ensuring data quality that are either not feasible or not economical in data collection platforms like Mturk tasks, random-digit dialing, and paper-and-pencil surveys. For these reasons, many other scholars have used Qualtrics panels to conduct survey research, including survey experiments — like the present one — that specifically tested the effects of message variations on attitudinal responses in the contexts of climate change (Feldman & Hart, 2016) and GMO foods (Yue, Zhao, & Kuzma, 2015).

Because Qualtrics Panels procures participants from a variety of other market research panels — each with their own terms and agreements for participation — the compensation and incentives for participation vary within the sample. That is, some participants were paid slightly more or less than others. The project managers at Qualtrics estimated that the value of the compensation for each of the participants in the final sample ranged in value between $1.50 and $5.00.

3.2.2. Sample size required by PROCESS and SEM. The literature suggests that framing manipulations often exhibit small effects (e.g., $\eta^2 < .059$, as defined by Cohen, 1988). Because I will be using Hayes' PROCESS (see Chapter 3.7; Hayes, 2017) to test interaction effects, the required sample size can be estimated with G*Power's recommendation for linear multiple regression. Detecting effects as small as r^2=.02 with two predictor variables with power (1-ß) at .80 requires 485 observations per test. The PROCESS tests using the smallest portions of the sample (H5, RQ2) will include the participants from five unique conditions (one of each uncertainty frame type), which equates to a minimum of 97 observations per condition (a total *n* of 1455). To err on the side of caution — anticipating the possibility of missing data, speeding, insufficient attention, and/or η^2<.02 — the full sample (after filtering and cleaning) consisted of about 150 participants per condition (target n=2250, resulting in a final total *n* of 2247 valid cases for analysis in the main study; or about 750 observations in each PROCESS model test), as well as a prior, additional 622 participants for the 15-condition pilot test of the scales and manipulations (Section 3.4).

The SEM portions of the analyses use 37 indicators to comprise the latent factors and observed variables. A general best-practice convention is to have at least 10 observations (in this case, participants) per observed indicator in the model – which sets the minimum sample size for testing the SEM model at 370 participants. The sample size mentioned above (target n=2250; 150 per condition) allowed for the test of SEM model fit with the fewest participants (containing only participants who were in a control condition) to have 443 participants. All other SEM models were tested using subsamples varying from 740 to 2247 participants.

3.2.3. Sample demographics. Both the pilot test and the main study used quotas set to match educational attainment levels of the 2010 census, an even split in gender, and an even split in political party affiliation (to ensure varied worldviews). The specific resulting proportions of several demographic characteristics within the main study sample are displayed in Table 7.

Table 7

Demographics for Main Study Sample

Age	*18-25*	*26-35*	*36-45*	*46-55*	*56-65*	*66-75*		*75+*
%	6.5	13.7	12.1	16.0	27.2	20.0		4.5
Ethnicity	*AmInd*	*Asian*	*Black/AA*	*Hisp/Lat*	*Haw*	*Wh/Eur*		
%	1.6	3.3	8.7	5.1	0.1	83		
Education	*<H.S.*	*H.S.*	*Some U*	*AA/AS*	*BA/BS*	*Ma./Dr.*		
%	2.4	25.5	22.4	11.7	22.8	15.2		
*Field**	*HardSci*	*SocSci*	*Human*	*Bus*	*Comp*	*Arts*	*Voc*	*Other*
%	9.7	15.0	7.3	15.4	7.1	5.4	5.5	
Gender	*Female*	*Male*	*Other*	*No Answer*				
%	49.8	49.8	0.4	0.0				
Political	*Con*	*Lib*						
%	50	50						

Note: AmInd= American Indian; Black/AA= Black or African American; Hisp/Lat=Hispanic or Latina/o; Haw=Hawaiian or Pacific Islander; Wh/Eur=White, European, Middle East; <H.S.= less than high school diploma; H.S. = high school diploma; Some U = some college, no degree; AA/AS = associate's degree; BA/BS = bachelor's degree; Ma./Dr. = Master's or Doctorate; HardSci=traditional sciences; SocSci= social sciences; Human=humanities; Bus=applied business; Comp=computational and informational sciences; Arts=fine arts; Voc=technical/vocational training; Con=conservative; Lib=liberal.

3.2.3. Design features ensuring data quality. In the main study, several steps were taken to ensure that only high-quality data (i.e., participants who gave thoughtful, honest responses and attention to the stimulus) entered the sample. Naturally, those who did not agree to the consent form (n=354) did not participate in the study. Further, immediately prior to the main stimulus message (a simulated newspaper article), participants were asked to indicate whether they agreed to read the news article in full. Those who did not agree (n=154) were automatically eliminated from the sample.

For further data quality, after advancing past the stimulus, participants were required to respond to two comprehension check questions that asked about a) the topic of the stimulus news article, and b) the directionality of the effect found by the study that was reported in the stimulus news article. Qualtrics filtered out 800 participants that failed this test by answering at least one of these questions wrong. It is worth noting that these questions were not just "attention checks" (e.g., "Please select answer choice "B""), but required an understanding of the stimulus news article. Also, the survey program did not allow participants to navigate back to the news article when answering the comprehension checks.

The stimulus (described in detail in Section 3.6; exemplars presented in Appendix A) was presented on a page by itself, and the Qualtrics' software automatically required participants to spend at least 15 seconds viewing that page. The software also tracked the time spent viewing the stimulus, which is a metric that was then used for post-hoc data cleaning (Section 3.3). Further, Qualtrics also timed participants' total elapsed survey participation time, and filtered out participants passed the attention checks but completed the study too quickly ("speeding"). The cutoff for minimum total survey time was set by the industry standard method — which is finding the median elapsed time (13.1 minutes) of the first 10% of the sample, and then filtering out the participants (from both the first 10% and all future participants) who completed the survey in less than 1/3 of that median time (4.3 minutes). This step eliminated 82 participants. Lastly, Qualtrics assessed the response patterns of each remaining participant and deleted 34 participants who exhibited "straight-lining" — where item responses follow a repetitive pattern (e.g., all 7s or all 4s).

In sum, Qualtrics filtered out 1070 participants who did the following: consented to participate in the study *and* would have filled one of the demographic quotas (education, political affiliation, gender, etc.), *but then* failed to pass one of the screening processes and quality filters that were implemented either during or after their participation. These 1070 participants were each replaced until the demographic quotas were filled with valid observations that passed all the screening processes and quality checks. Thus, the participants who comprised the Qualtrics sample (n=2435) that was delivered to the principal investigator for analysis each satisfied *all* of the requisite characteristics set by Qualtrics: stated a promise to read the news article in full, spent at least 15 seconds viewing the news article, correctly responded to each of two comprehension checks, had a total elapsed survey time greater than 1/3 of the median, *and* did not exhibit a straight-lining response pattern.

3.3 Data Cleaning

Section 3.2.3 explained Qualtrics design features that were employed before and during data collection to ensure data quality, including a promise to read the whole news article, two comprehension checks (requiring 100% accuracy on both), a speeding check (enforcing minimum total completion time of 260 seconds), and an automatic algorithm-based "straight-lining" detection. In total, 2,435 participants completed the study, filled the demographic quotas, and passed all of these *a priori* Qualtrics filters.

This dataset was then cleaned via several additional quality checks. First, I manually checked the entire dataset for patterns of straight-lining in all or part of each participant's responses that may have been missed by the automatic Qualtrics filter. As a result, 23 (about 1%) of the participants were removed from the sample for exhibiting straight-lining patterns in their responses, leaving 2,412 responses.

Next, I removed outliers in total study completion time and in time spent viewing the stimulus. This is important because outliers on the low end of these two variables likely indicate lower attention to the items and/or the stimulus, and outliers on the high end likely indicate that the participant completed the study in more than one sitting and/or with distractions. Either type of outlier would likely contaminate the study results.

Of the 2,412 remaining cases, the total time of study completion was right-skewed due to a few high outliers (mean=1,306 sec.; median=1,020; SD=1,353; min=268; max=18,813), with 22 participants taking less than 6 minutes (360 sec.) to complete and 74 taking more than 1 hour (3600 sec.) to complete. In order to guide estimations of a) a reasonable total time of study completion and b) a reasonable total time spent viewing the stimulus — specifically amongst respondents who are carefully attending to each survey item and the stimuli — an informal pretest of the study procedure was conducted among a convenience sample of 35 non-scientist acquaintances of the principal investigator. For this informal pretest, participants were evenly distributed between the five GMO labeling conditions. The mean total elapsed time of study completion was 14.4 minutes (864 sec.), with the two shortest completion times being 8.2 minutes (492 sec.) and 8.7 minutes (522 sec.), and the two longest completion times being 19.8 minutes (1188 sec.) and 22.3 minutes (2,338 sec).

It seems clear from the informal pretest and the distribution of the main study sample that completion times over 1 hour (3600 sec.) likely indicate interrupted participation. Thus, these 74 participants were removed. Also, the 22 participants who completed the study in less than 360 seconds (6 minutes) were removed.

Of the remaining 2,316 participants, the time spent viewing the stimulus was also right-skewed due to a few high outliers (mean=128 sec.; median=103; SD=120; min=16; max=2191), with 54 participants viewing the article for less than 20 seconds (<5 seconds more than the required minimum) and 15 participants viewing the article for between 600 seconds (10 min.) and 2200 seconds (36 minutes). In the informal pretest, the mean time spent viewing the stimulus (reading the news article) was 143 seconds (2.4 min.), with the two shortest being 32 and 46 seconds and the two longest being 303 and 343 seconds.

It seems clear from the informal pretest and the sample distribution that stimulus viewing times less than 20 seconds and greater than 10 minutes are not reasonable. To increase data quality, these 69 participants were removed from the sample, leaving 2,247.

It is possible that some participants could "speed" through the questions but take a long time viewing the stimulus, resulting in a total study completion time of >360 seconds. To check for this, each participant's total time spent answering questions was computed by subtracting their stimulus viewing time from their total study completion time. Of the remaining 2,247 cases, none had a total time spent answering questions that was less than 300 seconds (5 minutes). None were deleted for this cause.

These data cleaning steps left 2,247 participants in the sample, for which the total completion times ranged from 363 to 3,586 seconds (mean = 1,130 seconds; median = 1,014; SD = 526) and the stimulus viewing times ranged from 20 to 598 seconds (mean = 124 seconds; median = 106; SD = 83). These 2247 participants are the sample used for all analyses in the main study.

3.4. Pilot Test

Before the main study, a pilot test was conducted to assess the stimulus manipulations, and to verify the reliability of each of the multi-item scales. Another purpose was to determine via exploratory factor analysis (EFA) whether the items in each measure group into single factors (or, instead, each measure is better explained by multiple factors) that are distinct from the other measures/factors (discriminant validity).

For this pilot test, 622 participants, also obtained from Qualtrics, were randomly assigned to one of the 15 conditions, and completed study procedures that were identical to the main study (described in Section 3.1), with a few minor differences. First, the pilot test did *not* require participants to promise to read the entire news article, did *not* eliminate participants who exhibited straight-lining response patterns, and did *not* eliminate participants based on study completion time, and *did include* participants who viewed the stimulus for 15-20 seconds. These modifications were implemented in the main study at the suggestion of a research adviser to increase data quality. Second, the pilot test contained a pre-stimulus four-item Likert-type measure of positivist understanding of science — due to its relevance to the present topic and its observed effects in prior literature — with the intent of using it as a control variable in the analyses. This measure was not administered as a pre-stimulus independent variable in the main study because it did not demonstrate adequate reliability (Cronbach's α=.59), was not correlated with the outcome variables, and came under suspicion of priming participants to think about the inherent role of uncertainty in science. Instead, it was included at the very end of the main study, and is not used in this dissertation's analyses. All other differences between the pilot test and the main study were adjustments made to the measures and are discussed in detail in Section 3.5.

The full results of the pilot test are interspersed in Section 3.5, Chapter 4, and Appendix B. Specifically, the results of the analyses indicating the reliability and factor structure of each scale are reported in Table 8, Table 12, and are also explained individually in greater detail in Section 3.5 (Measures) alongside the description of each respective measure. The results of the manipulation check are presented in Chapter 4.

3.5. Measures

These sections describe the content and structure of the measures used in this study. Each measure was identical across all conditions, with the exception of prior issue position (explained below), and the measures are presented in the order that they appeared in the survey. For some measures, the results of the pilot test prompted small changes to some scale items. All of these instances are discussed here, alongside the description of those measures. Appendix B contains the full list of individual items in each measure, as well as the means and standard deviations for each scale and individual items, and also the factor loadings of each item as indicators of their respective latent variables.

The multi-item scales in this study were used for two purposes. The first purpose is to create mean scales that were used as individual variables in linear regression analyses (see Section 3.7 for a full description of the analyses). Therefore, in the next section (3.5.1) — to justify creating mean scales — the description of the format of each multi-item scale is accompanied by a mention of its reliability and basic evidence for the dimensionality of that scale. The dimensionality of each scale will be informed by a preliminary set of EFAs performed *within each scale separately*. I will refer to these as "*local EFAs*."

The second purpose of the multi-item scales is to determine whether the items load into groups where each item is a strong indicator of the latent variable (convergent validity), and whether the items are *only* indicators of one latent variable (discriminant validity). This is the exploration and confirmation of the measurement model. To do this, I first perform an exploratory factor analysis (EFA) on a random split-half of the sample and then a confirmatory factor analysis (CFA) on the remaining half. This EFA, unlike the local EFA, uses all items of the measures of all variables that are included in the structural model hypothesized in H1 and H3. I will refer to this as the "*global EFA*." The latent variables indicated by this sequence of analyses will —together — be used in testing the basic structural model via SEM.

Therefore, in Section 3.5.2, I report the methods and results of the global EFA, which explores a measurement model including all of the items of (only the) variables used to test hypotheses and research questions regarding the basic structural model (i.e., variables used in the SEM). For example, the deference to science scale is examined for its own reliability and dimensionality in Section 3.5.1, using a local EFA, in order to justify the creation of a mean scale. But deference to science is *not* used in the tests of the structural model informing H1 and H3, so its scale items are *not* included in the global EFA used to specify that larger measurement model. Similarly, local EFA results for the ECgen and EU type scales (individually) are presented in Section 3.5.1, but because they are *not* used to inform the hypotheses about the structural model (or any hypotheses, for that matter), they are *not* included in the global EFA of the measurement model in Section 3.5.2.

Descriptions of the measure of family size (a filler or distractor measure) and of positivist understanding of science (not administered in the main study) are withheld for space considerations.

3.5.1 Measure format, structure, and reliability. Of the measures described in this section, several are multi-item scales that are intended to measure one construct. The reliability (Cronbach's α) of the full (all items) version of each of these multi-item scales — for both the pilot test and the main study — is presented in Table 8. Also, Table 8 presents basic evidence of the degree to which each full, unmodified scale can reasonably be considered to be unidimensional. As a result of these analyses, some scales were slightly modified. The corresponding values of the modified versions are given in Table 12.

Evidence of dimensionality was provided using Mplus (version 7.11; Muthen & Muthen, 2013) to perform individual local EFAs with 100% of the pilot test sample and, separately, a random 50% split of the main study sample (Table 8). Each local EFA includes all of one scale's items and employs maximum likelihood (ML) estimation with oblique (geomin) rotation, since emerging factors would likely be correlated. Inferences about dimensionality were guided by Kaiser's eigenvalue criteria, Catell's scree plot, and parallel analysis (i.e., eigenvalue Monte Carlo analysis with 50 iterations). Other fit criteria applicable to EFAs with ML estimation – like chi-square test of model fit, root-mean-square error of approximation (RMSEA), comparative fit index (CFI), and standardized root-mean-square residual (SRMR) – are not provided in Mplus for two-factor solutions emerging from four or fewer indicators. To remind the reader, Section 3.5.2 will provide in-depth validation (using RMSEA, CFI, and SRMR) of a combined measurement model by reporting the results of a global EFA performed with all items used to measure the latent variables involved in the structural model, followed by a CFA reported in Section 4.1.

Table 8

Reliability and Basic Structure of Full Unedited Multi-Item Scales

	Main Study					*Pilot Test*				
	α	*ev1*	*ev2*	*f1var*	*f2var*	*α*	*ev1*	*ev2*	*f1var*	*f2var*
Prior(cc)	.90	3.60	0.66	73%	12%	.87	3.35	0.79	67%	16%
Prior(gmo)	.88	3.47	0.60	69%	12%	.82	2.93	0.89	58%	18%
Prior(vm)*	.74	2.61	0.84	49%	18%	.53	1.79	1.11	36%	22%
Def2Sci	.76	2.36	0.65	59%	16%	.70	1.98	0.94		
IndiColl	.78	2.98	1.04	48%	17%	.79	2.92	1.11	49%	18%
HierEgal	.90	4.00	0.76	66%	13%	.87	3.68	1.00	61%	17%
ICgen	.84	2.29	0.48	77%	16%	.80	2.16	0.56	72%	19%
Risk	.85	3.44	1.09	57%	18%	.83	3.30	0.96	55%	16%
Credible	.93	5.33	0.80	67%	10%	.92	5.17	0.78	64%	10%
BehaveIn	.76	2.58	1.14	51%	22%	.78	2.68	1.00	54%	20%

Note: α= Cronbach's alpha; ev1= initial eigenvalue for one factor; ev2= initial eigenvalue for second factor; f1var= variance explained by one factor; f2var= additional variance explained by second factor; Prior= prior issue position; Def2Sci= deference to science; IndiColl= individualist/collectivist; HierEgal= hierarchical/egalitarian; ICgen= general internal certainty; Risk= six-item perceived risk scale; BehaveIn= five-item behavioral intentions scale; Credibility= perceived credibility; Full list of all items in each scale is available in Appendix B; *= scale items changed between pilot test and main study.

Demographics. Participants provided their age by choosing one option (with one option for 18-25, response options in five-year increments from 25 to 75, one option for 76+, and one option for Prefer Not to Answer), indicated their ethnicity by choosing one or more options (with response options of "American Indian, First Nation, or Alaska Native," "Asian or Indian subcontinent," "Black or African American," "Hispanic/Latino," "Native Hawaiian or Pacific Islander," White/European/Middle Eastern," or Prefer Not to Answer), and indicated their gender by choosing one option (Male, Female, Other/Non-conforming, or Prefer Not to Answer).

Political affiliation. For sampling purposes, participants were asked "How would you describe your own personal political opinions?" with response options of "Conservative," "Moderate," "Liberal," and "Choose not to answer." Responses on this question were not used for the study's analyses, but rather were used by Qualtrics to assemble a sample that was 50% self-reported conservative and 50% self-reported liberal.

Education. Participants' general educational attainment was assessed by asking them to indicate their highest level of completed coursework, with response options of "Less than a high school diploma," "High school diploma or equivalent (e.g., GED)," "Some college, no degree," "Associates degree (e.g., AA, AS)," "Bachelor's degree (e.g., BA, BS)," and "Advanced degree (e.g., MA, MS, MBA, JD, PhD)." For the purpose of identifying participants who had science-specific higher education, all participants who reported an education level of "Some college, no degree" or higher were given an additional question that asked them to indicate which category — from a list of categories with examples — best fit their major or focus of study (e.g., traditional sciences, social sciences, humanities, applied business, informational sciences, fine arts, technical/vocational training). Educational attainment was used as a control variable in many analyses in this study, and was also used by Qualtrics to assemble a sample that approximated U.S. census proportions of educational attainment.

Relevant behaviors. Since the stimulus in each condition was a newspaper article and was about risks to farmers and agriculture workers, it is important to assess two participants' *news media consumption*, and *association with farming*. To assess news media consumption, participants were asked to indicate how often they "read a news article (either in print or online articles)" and how often they "watch video from the news (either on TV or online)." Response options for each item were "Never," "About once per year," "About once per month," "About once per week," "About once per day," "More than 5 per day," and "More than 15 per day." These two scale items did not demonstrate strong reliability (α=.64), and thus they are treated as separate variables rather than as two items in a scale of news exposure. These variables are used as covariates in the analyses.

Then, participants were asked to indicate whether they or a close family member "has ever worked in the agriculture/livestock industry, for example, as a farmer, rancher laborer, etc." Response options were "No, neither myself nor a close family member," "I myself have not, but a close family member has," "I myself have, but no other close family members have," and "Both myself and close family member(s) have."

Also, because personal exposure to vibrating machinery might affect responses to the stimulus in the occupational hazards of farming (vibrating machinery) conditions, participants reported their frequency of *tool use*. Specifically, how often they use power tools (e.g., power drill, chainsaw) or operate heavy machinery (e.g., riding lawn mower, tractor, snow blower, backhoe, plow). Response options were "Never," "Around 1 time per year," “Around 5 times per year,” “Around 10 times per year,” “Around 1 time per week,” and “Almost every day.” It was expected that these measures of tool use and of association with farming would be used as covariates, but they did not correlate well (no $r > .08$) with any of the outcome variables in any condition. Therefore, they are not used in any analyses.

Prior issue position. Naturally, participants were given a measure of prior issue position that corresponded with the issue condition they are in. That is, the measure of prior issue position was different (although very similar in structure and style) between the three issues, but was identical within each issue’s five frame conditions.

In the five *climate change* conditions, prior issue position was assessed with a five-item measure adapted from Dieckmann et al. (2017). This measure asked participants to respond to statements such as “Humans are the main cause of climate change” and “The climate change we see today is part of a natural cycle of warming and cooling” (reverse coded) on 7-point Likert-type scales with anchors of “Strongly Disagree” and “Strongly Agree.” The scale demonstrated strong reliability in the pilot test (α=.87) and main study (α=.90) and is best described as comprising one factor (Table 8). Therefore, participants’ responses on the five items were combined to create a mean scale, with higher values indicating greater support for the ideas that climate change exists and is primarily caused by human activity.

Similarly, for all five *GMO foods labeling* conditions, participants’ general issue position was assessed with a five-item measure adapted from Frewer and colleagues (1997; 1998). This measure asked participants to respond to statements such as “GMO foods are beneficial to society” and “It is morally wrong to be changing nature with genetic engineering” (reverse coded) on 7-point Likert-type scales with anchors of “Strongly

Disagree" and "Strongly Agree." This scale demonstrated good reliability in both the pilot test (α=.82) and main study (α=.88) and the items comprised one factor (Table 8). Therefore, participants' responses on these five items were combined to create a mean scale, with higher values indicating greater support for the production and consumption of GMO foods.

For all five *occupational hazards of farming* (vibrating machinery) conditions, participants' prior issue position about the hazards or healthiness of farming and agriculture work was assessed with a five-item measure styled to resemble its counterpart measures in the climate change and GMO conditions. In the pilot test, this measure asked participants to respond to statements such as "Farmers and agriculture workers could get hurt easily" and "A career in farming or agriculture work is healthy" (reverse coded) on 7-point Likert-type scales with anchors of "Strongly Disagree" and "Strongly Agree." As indicated in Table 8, this measure was not reliable in the pilot test (α=.53) and the scale items comprised more than one factor.

Upon closer examination of the items, it was apparent that they referenced disparate issues (such as the general harmfulness of manual labor and the safety of machinery, which are distinct ideas), which increases the likelihood that individuals will score some items quite differently than others — decreasing reliability. To focus the measure more clearly and consistently on opinions about the occupational hazards/health of farming and agriculture work, a wholly revised scale was employed in the main study. For this revised scale (five items), participants responded to three statements about perceptions of careers in farming and agriculture ("a career in farming or agriculture work *is dangerous*," "…*is safe*," and "…*is healthy*") and two statements about perceptions of the safety of farmers and workers ("farmers and agriculture workers *could get hurt easily*" and "…*should fear for their* *health*"). Responses were given on 7-point Likert-style scales with anchors of "Strongly Disagree" and "Strongly Agree."

This revised measure tested in the main study achieved adequate reliability (α=.74) and the scale items comprised one factor (Table 8). Therefore, participants' mean responses on the items of the revised measure were combined to create a mean scale, with higher values indicating greater support for the idea that farming and agriculture work is a physically dangerous, generally unhealthy career.

Deference to science. Deference to science was assessed with the four-item measure developed by Binder et al. (2016). While they report a Cronbach's α of only .66, they also argue that all other studies that have attempted to measure this construct (with other items) reported similar reliability (ranging from .65 to .69). This measure asks participants to respond to statements such as "Public opinion is more important than scientists' opinions when making decisions about scientific research" (reverse coded) on a 7-point Likert-type scale with anchors of "Strongly Disagree" and "Strongly Agree.

This scale demonstrated adequate reliability in its original four-item form (pilot test α=.70; main study α=.76) and comprised one factor (Table 8). An individual's mean score on these four items was used to construct a mean scale indicating deference to science.

Individualist-collectivist worldview. As briefly described in Section 2.4.2, ideological worldview is sometimes operationalized as an individuals' political party affiliation (e.g., a categorical measure of Republican / Democrat / Independent) or political ideology (e.g., a continuous scale ranging from conservative to liberal). However, many science communication scholars have preferred to measure ideological worldview on two separate (although correlated) dimensions: hierarchical-egalitarian and individualist-collectivist (e.g., Dieckmann et al., 2017; Kahan et al., 2011). One of the advantages of this approach is that it is subtle. That is, it avoids the potential limitation of influencing politicization in individuals' interpretations or responses by priming political partisanship. Another advantage is that it is a significantly more nuanced (two continuous dimensions) and valid (measuring the underlying values that characterize worldview) measure than a simple categorical measure of party affiliation. For these reasons, ideology/worldview was assessed with the short form of the cultural cognition measure (Bolsen & Druckman, 2011; Kahan et al., 2011) which contains six items measuring egalitarian worldview attitudes and six items measuring collectivist worldview attitudes.

Sample items from the collectivist worldview scale are "The government needs to make laws that keep people from hurting themselves" and "The government should stop telling people how to live their lives" (reverse coded). Responses to these statements were given on 7-point scales with anchors of "Strongly Disagree" and "Strongly Agree". This six-item scale (InCo) exhibited good reliability (pilot test α=.79; main study α=.78).

However, the local EFA indicated that the InCo factor structure was ambiguous, with the scree plot and parallel analysis indicating only one factor (Figure 3), but the eigenvalue of the second factor being greater than 1 (Table 8). Further, when the factor structure was probed as part of the full global EFA, the inclusion of the sixth scale item ("The government should put limits on the choices individuals can make…") destabilized the collectivist worldview factor — and the item cross-loaded with the behavioral intentions factor. When this item is removed, it *not only* results in clear unidimensionality within the scale — resolving the ambiguity (second factor eigenvalue = 0.89; Figure 3; Table 12) — *but also* results in a unique factor (indicating discriminant validity) in the measurement model

(Section 3.4.2). Therefore, the five-item variant of this scale (pilot test α=.76; main study α=.76) was used for all analyses. Participants' mean scores on the five items used to create mean scales indicating collectivist worldview and egalitarian worldview.

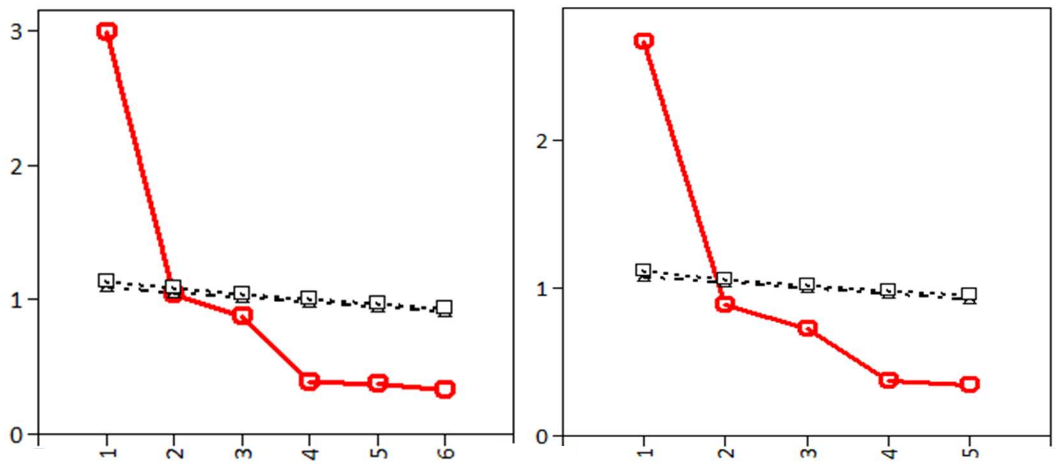


Figure 3. Scree plots and parallel analyses of the six-item (left) and five-item (right) variants of the collectivist worldview scale.

Hierarchical-egalitarian worldview. Sample items from the egalitarian worldview scale are "Our society would be better off if the distribution of wealth was more equal" and "We have gone too far in pushing equal rights in this country" (reverse coded). Responses to these statements were given on 7-point scales with anchors of "Strongly Disagree" and "Strongly Agree". This six-item scale exhibited good reliability (pilot test α=.87; main study α=.90) and the local EFA indicated that they comprised one factor (Table 8).

However, this scale became a perfect illustration of the point made earlier in this section about the importance of distinguishing between dimensionality *within* a scale and dimensionality as determined by a comprehensive measurement model that includes measures of other constructs and their factors. Each item in any scale/factor varies on how much it could be belonging to more than one factor. So, while the scale might be reliable and unidimensional (and thus well-suited for creating a mean scale), one or more of its items might also be strongly related *enough* to another construct/factor to justify exclusion in the interests of establishing a valid measurement model.

In this case, specifically, the initial global EFA (Section 3.5.2) exploring the full measurement model demonstrated that the inclusion of the first item ("We have gone too far in pushing equal rights in this country"; reverse coded) caused cross-loading on multiple factors. However, excluding this item results in a cohesive and unique factor in the measurement model (Section 3.4.2). In sum, the full set of six items does not comprise a factor that is unique from the rest of the items and factors in the measurement model, but the set of five items does. Therefore, the five-item variant of this scale (pilot test α=.84; main study α=.88) was used for all analyses. Participants' mean scores on the five items used to create mean scales indicating collectivist worldview and egalitarian worldview.

External certainty. General external certainty has most commonly been measured with one-item self-report measures (e.g., "how certain are scientists that…") (e.g., Corbett & Durfee, 2004; Dixon & Clarke, 2013; Johnson, 2017; Koehler, 2016). However, since the external certainty measure is used as a manipulation check, a measure of general external uncertainty would be inadequate because it would not determine whether participants were sensitive to the *variations in distinct types* of uncertainty that were portrayed across the manipulations. Thus, to see if participants reported differences across conditions in the *types* of uncertainty that they believed experts had, —a measure of external uncertainty *types* was administered. This measure contained four items, each of which corresponded to one of the four types of uncertainty (deficient, technical, consensus, scientific; Table 9). I will refer to this group of four items as the "EUtype" measure.

These items are *not* intended to be combined as a multi-item measure, but rather each of the individual items is used for the respective manipulation check. In fact, low reliability would be evidence of participants making desired *conceptual and perceptual distinctions* between the distinct types of uncertainty represented in the items. Indeed, when treated as a single four-item measure (which it is not), the EUtype items exhibit low reliability (α=.63), and the manipulation check (Section 4.3) demonstrates clearly that participants are sensitive to the variations in uncertainty types across the four EUtype items.

The pilot test contained a slightly different wording for the technical uncertainty item ("…findings of this research are precise and exact."). After finding no effect (relative to the control) of the technical uncertainty manipulation on that technical uncertainty EUtype item in the pilot test manipulation check, the wording was changed — for the main study (Table 9) — to more specifically reference the type of verbiage found in the technical uncertainty stimuli ("…findings of this research are rough estimates that could vary by a large margin.")

Table 9

Items in External and Internal (Un)Certainty Measures

External Uncertainty Types (EUtype)	
Deficient	These scientists think that there is still a lot that they don't know about this subject.
Technical	These scientists think that the findings of this research are rough estimates that could vary by a large margin.
Consensus	These scientists think that they often disagree with each other or have controversy about this subject.
Scientific	These scientists think that their findings and opinions about this topic will significantly change as future research progresses.
Internal General Certainty (ICgen)	
	I myself am very certain that _*claim*_.
	I myself tend to be skeptical of the idea that _*claim*_.
	I myself think there is very strong evidence for believing that __*claim*__.

Note. Responses were given on 7-point Likert-type scales (1=Strongly Disagree, 7=Strongly Agree).

Internal certainty. General internal certainty has most commonly been measured with self-report Likert-style measures that often contain only one item (e.g., "I am certain that...") (e.g., Binder et al., 2016; Chang, 2015; Clarke et al., 2015; Corbett & Durfee, 2004; Dixon &

Clarke, 2013). The measure employed in this study was similar, but expanded on the typical one-item measures by including three different phrasings of internal certainty, with one reverse-coded item. Table 9 displays all three items in this measure. I will refer to this measure of general internal certainty as "ICgen." Participants were asked to indicate their level of agreement with each statement on 7-point Likert-type scales (1=Strongly Disagree, 7=Strongly Agree). The three-item ICgen measure demonstrated good reliability (pilot test α=.80; main study α=.84) and comprised one factor (Table 8). Therefore, each participant's scores on these three items was used to calculate a mean scale indicating general internal certainty about the claim presented in the stimulus news article.

Alternative (un)certainty measures. The preceding measures targeted distinct *types* of external certainty (EUtype) and *general* internal certainty (ICgen). Naturally, it is interesting to also measure *types* of internal certainty (IUtype) and *general* external certainty (ECgen). Therefore, these alternative measures were also administered in the pilot test and in the main study, but they are not used in any of the analyses.

The IUtype measure mirrored the EUtype measure (Table 9), with the only differences being references to one's own opinion ("I myself think that…") instead of scientists' opinions ("These scientists think that…"). Like the EUtype items, the IUtype items are *not* intended to represent a single construct — and thus are *not* used to create a mean scale for use in any analyses. Unlike the EUtype items, the IUtype items are not ideal for a manipulation check, because they do not represent perceptions of what was portrayed in the manipulations (the uncertainties of scientists). Rather, they represent personal opinions that may or may not reflect what was portrayed in the manipulations. For these reasons, and for space constraints, IUtype is not considered as an outcome variable in this dissertation.

The ECgen measure mirrored the ICgen measure (Table 9), with the only differences being that each item referred to the opinion of scientists ("It seems to me that these scientists think that…") instead of to one's own opinion ("I myself think…"). This three-item measure was *not* included in any analyses for three reasons. First, ECgen is very close in concept to ECtype — which is the manipulation check variable, and should be not confounded as being a hypothesized outcome variable. Second, the ECgen scale (unlike ICgen) demonstrated poor reliability (pilot test α=.61; main study α=.66). Third, when included in the global EFA that tests the measurement model, the ECgen items did not load together — indicating that the ECgen items are not distinct from other factors that emerge from the items of other scales. For these reasons, it is not considered as an outcome variable of interest in this dissertation.

Perceived risk. Perceived risk regarding the threat presented by the finding/claim of the study reported in the stimuli was assessed with a six-item measure in which participants responded to statements such as "I think *(issue)* poses serious dangers to agriculture workers" on a 7-point Likert-type scale with anchors of "Strongly Disagree" and "Strongly Agree." Three of these items referred to the severity of the threat posed to farmers and agriculture workers — which was the focus of the claim. The other three items referred to the severity of the threat posed to the study participant personally.

For each set of three, the items are adapted from the risk measure used by Binder and colleagues (2016; α=.79), which assesses threat severity and concern. Bolsen and Druckman (2015; no alpha reported) use a similar measure. In sum, the six-item scale represented both perceived risk to others and to self. One of the purposes of these analyses is to determine whether these two sets should be treated separately and which should be used in this study.

These six items demonstrated strong reliability (pilot test α=.83; main study α=.85) and the preliminary evidence for dimensionality (local EFAs; Table 8) indicated that a single-factor interpretation is reasonable, due to the scree plot and parallel analysis (Figure 4). However, with an eigenvalue of 1.09 for the second factor, it is reasonable to entertain the possibility of a two-factor structure. Not surprisingly, the loadings for two factors demonstrated that the three "risk to others" items loaded clearly on to the first factor (Table 10), while the three "risk to self" items comprised the second factor, with the "first risk to self" item cross-loading on the first factor.

Table 10

Risk Scale (6 Items) Factor Loadings for Local EFA

	Risk to Others	Risk to Self
1. "…poses serious danger to agricultural workers."	**.890**	
2. "…farmers and workers should be worried…"	**.897**	
3. "…farmers and workers will be…unaffected." (r)	**.580**	
4. "…poses serious danger to me and my loved ones."	.282	**.612**
5. "…people like myself do not need to be worried…" (r)		**.650**
6. "…will affect my life or lifestyle…"		**.901**

Note. Factor loadings > .40 are in boldface; Loadings < .15 are suppressed; (r)=reverse coded; maximum likelihood estimation and geomin (oblique) rotation.

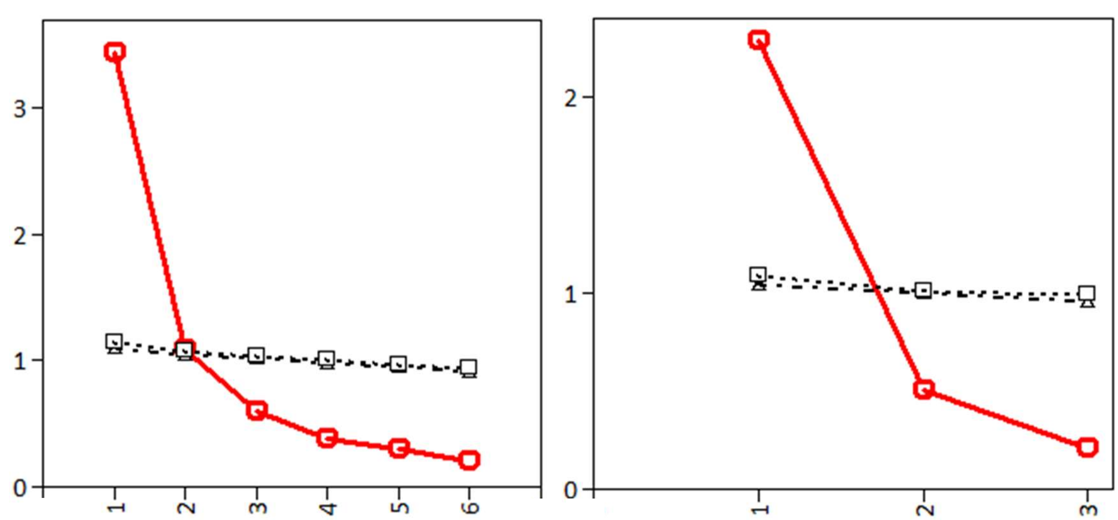


Figure 4. Scree plots and parallel analyses of the six-item (left) and three-item (right) variants of the perceived risk scale.

While these test results give some justification for treating the two sets of three items as distinct, the strongest argument is a conceptual one. Intuitively, the claims in each stimulus described risk that is far-removed from most people (i.e., risk to farmers and agriculture workers). Thus, the items targeting perceived risk to self are significantly less relevant than the items targeting perceived risk to farmers and agriculture workers. For these reasons, only the three "risk to others" items were used in the analyses, including for the creation of a mean scale indicating perceived risk (pilot test α=.80; main study α=.84).

Perceived credibility. Perceived credibility of the scientists who produced the claim/finding was assessed using a measure constructed from items from foundational (e.g., Berlo et al., 1969; McCroskey, 1966) and contemporary (e.g., Jensen & Hurley, 2010) credibility scales. Using seven-point semantic differential scales, participants responded to the question "Based on the article, how would you describe the scientists who produced the research?" with four response items measuring expertise (competent / incompetent; knowledgeable / ignorant; skilled / unskilled; intelligent / unintelligent) and four response items measuring trustworthiness (trustworthy / untrustworthy; honest / dishonest; biased / unbiased; withholding information / telling the whole truth). The full eight-item credibility scale demonstrated high reliability (pilot test α=.92; main study α=.93) and constituted one factor (Table 8). An individual's mean score on these items was used to construct a mean scale indicating perceived credibility of the scientists quoted in the news article.

Behavioral intentions. The measure of behavioral intentions was not designed to be a measure of intentions to perform a specific behavior. Rather, the goal was to measure an attitude of general willingness to engage in behaviors that indicate supportive attitudes toward the claim. Therefore, each of the five items on the behavioral intentions measure referenced a different behavior. The first two items asked participants “In the future, if you saw another newspaper article about this topic, how likely would you be to read it?” and “…how likely would you be to share it with others?”. Responses were given on 7-point Likert-type scales with anchors of “Extremely Unlikely” and “Extremely Likely.” The third item presented the participants with a hypothetical scenario of being given the option to donate part of their compensation for this survey to non-profit groups raising money to provide financial assistance to the farmers and workers who are being affected by _*issue*_. Then the item asked “how much of it do you think you would give?” Responses were provided on a 7-point scale with response options of 0%, 17%, 33%, 50%, 67%, 83%, and 100%.

The fourth and fifth items presented participants with hypothetical scenarios where the area in which they live was considering legislation that would generate funds to provide financial assistance to the farmers and workers that the research study claimed have been affected by _*issue*_. The fourth item described proposed legislation that was a consumer tax on agricultural products, which would fund the assistance to farmers. The fifth item described proposed legislation that was a tax break that would be given to farmers directly. These two variants were included so that this measure would not unevenly favor liberal or conservative worldviews. For each of these two items, participants were asked how likely they would be to vote Yes in favor of this legislation. Responses to these two items were given on 7-point Likert-type scales with anchors of “Certainly Not (0%)” and “Certainly Yes (100%”).

Overall, the five items demonstrated adequate reliability (pilot test α=.78; main study α=.76), but the local EFA did not produce convincing evidence of unidimensionality (main study second eigenvalue = 1.12; Table 8). This, of course, is not surprising for an exploratory measure that is explicitly designed to encompass a suite of different behaviors. It was expected prior to the analyses that this group of items would be pared down to a cohesive set that represents one construct. The factor loadings demonstrated that, for a two-factor solution, the first two items loaded together, and the last three items loaded together. Either (or both) of these sets would be reasonable to use as measures of behavioral intentions. However, the first set only had two items, and Mplus requires a minimum of three indicators for identifying a factor. Therefore, the second set — the third (donations), fourth (legislation), and fifth (legislation) items from the behavioral intentions measure — is the set that is best suited for assessing behavioral intentions in this study. These three items demonstrate adequate reliability (pilot test α=.71; main study α=.73) and comprised one factor (Figure 5; Table 11). Therefore, each participant's mean score on these three items was used to create a mean scale indicating intention to engage in behaviors that demonstrate support for the claim.

Table 11

Behavioral Intentions Scale (All 5 Items) Loadings from Local EFA 2-Factor Solution

	Risk to Others	Risk to Self
1. Read a similar news article in the future	**0.735**	
2. Share a similar news article with others in the future	**0.984**	
3. Donate a portion of survey payment to help the farmers		**0.434**
4. Vote in favor of a tax that funds assistance to the farmers		**0.942**
5. Vote in favor of a tax break given to the farmers		**0.679**

Note. Factor loadings > .40 are in boldface; Loadings < .15 are suppressed; (r)=reverse coded; maximum likelihood estimation and geomin (oblique) rotation.

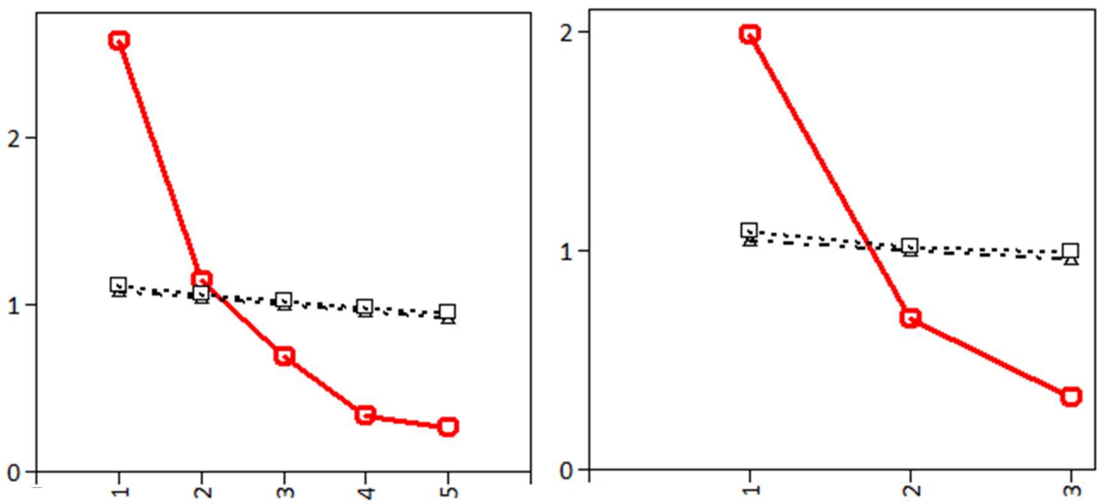


Figure 5. Scree plots and parallel analyses for the five-item (left) and three-item (right) variants of the behavioral intentions scale.

Table 12

Reliability, Descriptives, and Eigenvalues of Modified Multi-Item Scales

		Main Study					Pilot Test				
	Final Items	*α*	*M*	*SD*	*ev1*	*ev2*	*α*	*M*	*SD*	*ev1*	*ev2*
IndiColl	1, 2 ,3, 4, 5, ~~6~~	.76	3.43	1.20	2.67	0.89	.76	3.38	1.23	2.56	0.98
HierEgal	~~1,~~ 2, 3, 4, 5, 6	.88	4.58	1.69	3.40	0.68	.84	4.47	1.61	3.11	0.91
Risk	1, 2, 3, ~~4, 5, 6~~	.84	4.69	1.43	2.29	0.50	.80	4.69	1.38	2.14	0.65
BehaveIn	~~1, 2,~~ 3, 4, 5	.73	3.55	1.44	1.98	0.69	.71	3.62	1.45	1.93	0.76

*Note: "*Final Items" lists the number of items in each scale and depicts which items were excluded from the final scale in ~~strikethrough~~. α= Cronbach's alpha; ev1= initial eigenvalue for one factor; ev2= initial eigenvalue for second factor; f1var= variance explained by one factor; f2var= additional variance explained by second factor; IndiColl= individualist/collectivist; HierEgal= hierarchical/egalitarian; Risk= perceived risk scale; BehaveIn= behavioral intentions scale; Full list of all items in each scale is available in Appendix B.

3.4.2. Global EFA. The hypotheses imply a structural model in which the two worldview dimensions predict prior issue position, which in turn predicts the four outcome variables (internal certainty, perceived risk, credibility, and behavioral intentions) (Figure 2). Section 3.7 describes how and why SEM will be used to test some of the hypotheses and research questions. However, prior to testing the structural model, the measurement model must be validated via EFA and CFA. It is important to note that although individual scales (and their local EFAs) largely appear to demonstrate reliability and single-factor structures (convergent validity; Section 3.5.1), this does not mean that those factors are also necessarily distinct from each other (discriminant validity) or would be unidimensional when analyzed simultaneously.

So, to investigate the factor structure and discriminant validity of the items indicating the latent variables in the structural model, a global EFA using ML estimation was performed using the same random selection of 50% of the main study sample that was used for the individual local EFAs (Section 3.5.1). Differing from the individual local EFAs, the initial global EFA included all (34) items in the collectivism worldview (six items), egalitarian worldview (six items), prior issue position (five items), internal certainty (three items), risk (three items), credibility (eight items), and behavioral intentions (three items) scales. Because emerging factors would likely be correlated, oblique (geomin) rotation was used.

For EFAs with ML estimation, there are several methods of determining the appropriate number of factors comprising a scale, such as Kaiser's eigenvalue criteria, Catell's scree plot, parallel analysis, RMSEA, CFI, SRMR, as well as the size and alignment of factor loadings (with a minimum threshold of .30; Brown, 2006). Kaiser's eigenvalues and Catell's scree plot techniques can gave preliminary insight, but are arguably the least precise methods. When there is uncertainty in interpreting the eigenvalues and the scree plot, parallel analysis can add more confident suggestions. Chi-square tests that are significant (for solution with j factors) recommend a rejection of the null hypothesis that there is no difference between the observed data and the j-factor model. However, with a large sample such as this one, this test is almost always significant simply as an artifact of sample size (Fabriger et al., 1999). Therefore, to choose the appropriate number of factors, I supplemented the information provided by the eigenvalues, scree plot, and parallel analysis with that of the RMSEA, CFI, and SRMR values. The RMSEA index indicates good fit if the upper bound of the 90% confidence interval is <.08 (Hu & Bentler, 1998), the CFI indicates good fit if the value is >.95 (Hu & Bentler, 1998), and the SRMR index indicates good fit if the value is <.08 (Hu & Bentler, 1999).

The first iteration of global EFA testing included all items from all measures, and the fit indices (Table 13) indicate that the first solution to achieve good fit on two of the three fit indices was the six-factor solution. As discussed in Section 3.5.1, the preliminary local EFAs revealed that the collectivism worldview scale had an ambiguous structure, which was further evidenced in this first iteration of the full global EFA. The removal of the sixth item resolved cross-loadings between the two worldview dimensions and the behavioral intentions scale. All three of these scales have items that target individuals' preferences for the roles and responsibilities of government and the individual in ensuring a better society. Therefore, it is not surprising that items on these three scales might load on to more than one factor. The sixth collectivism item may be responsible for much of this overlap — sitting on the fence between factors — because its removal enables the five remaining collectivism items to group together as one unique factor.

Table 13

Model Fit Indices by Global EFAs and Factor Solutions

Item Excluded	*RMSEA*	*RMSEA 90% CI*	*CFI*	*SRMR*
None				
1 Factor	.149	[.147, .149]	.45	.128
2 Factors	.131	[.129, .133]	.60	.101
3 Factors	.107	[.104, .109]	.75	.070
4 Factors	.091	[.089, .094]	.83	.056
5 Factors	.079	[.076, .082]	.88	.036
6 Factors	**.070**	**[.068, .073]**	**.91**	**.031**
7 Factors	.063	[.060, .066]	.94	.024
InCo #6				
1 Factor	.151	[.149, .153]	.46	.128
2 Factors	.132	[.129, .134]	.61	.100

3 Factors	.105	[.103, .108]	.77	.067
4 Factors	.088	[.086, .091]	.85	.050
5 Factors	.078	[.076, .081]	.89	.035
6 Factors	**.069**	**[.066, .071]**	**.92**	**.029**
7 Factors	.060	[.058, .063]	.94	.022
InCo #6, HE #1				
1 Factor	.151	[.149, .153]	.47	.125
2 Factors	.130	[.127, .132]	.64	.107
3 Factors	.107	[.104, .109]	.77	.067
4 Factors	.089	[.086, .091]	.85	.049
5 Factors	.078	[.076, .081]	.89	.036
6 Factors	**.072**	**[.069, .074]**	**.92**	**.029**
7 Factors	.062	[.059, .065]	.94	.022

Note. Chi-square test of model fit is not reported because all χ^2 values were significant at p<.001 in all factor solutions of all model variants due to sample size. RMSEA = root-mean-square error of approximation; CI = confidence interval; SRMR = standardized root-mean-square residual.

The second iteration of a global EFA included all 33 items except that sixth collectivism item. The fit indices suggested that the first strong solution — achieving good fit on two of three indices — was again the six-factor solution (Table 13). Similar to the first iteration, one of the worldview items (the first item on the egalitarian scale: "We have gone too far in pushing equal rights in this country", reverse coded) caused some of the egalitarian items to comprise two factors that cross-loaded with each other and with the behavioral intentions scale. Removing this item resolved all cross-loading, leaving only unique factors.

These unique factors are exhibited in the third iteration of a global EFA, which included all 32 remaining items. The fit indices again suggested the six-factor solution (Table 13). All emerging factors were unique — with no cross-loadings above 0.2 (Table 14). This clear and robust six-factor solution was, of course, unexpected because the global EFAs included measures of seven constructs.

Table 14

Final Measurement Model EFA Item Factor Loadings

	Prior Issue Position	Collectivist Worldview	Egalitarian Worldview	Perceived Credibility	Belief in Claim	Behavioral Intentions
Prior1	**0.847**					
Prior2	**0.839**					
Prior3	**0.578**					

Prior4	**0.759**					
Prior5	**0.658**					
InCo1		**0.665**				
InCo2		**0.455**				
InCo3		**0.623**	0.157			
InCo4		**0.816**				
InCo5		**0.434**				
HE2			**0.828**			
HE3			**0.910**			
HE4			**0.715**			
HE5			**0.662**			
HE6		0.171	**0.552**			
ICgen1					**0.830**	
ICgen2					**0.612**	
ICgen3					**0.887**	
Cred1				**0.717**		
Cred2				**0.810**		
Cred3				**0.723**		
Cred4				**0.818**		
Cred5				**0.875**		
Cred6				**0.916**		
Cred7				**0.494**		
Cred8				**0.710**		
BI3						**0.484**
BI4						**0.880**
BI5						**0.647**
Risk1					**0.778**	
Risk2					**0.789**	
Risk3					**0.613**	

Note. Geomin (oblique) rotated factor loadings. Loadings > .40 are in boldface; Loadings < .15 are suppressed; (r)=reverse coded.

The two measures that group together to comprise a single and unique factor are internal certainty and perceived risk. Considering that the claim presented in the stimuli was precisely that farmers and agriculture workers were being harmed by either climate change, GMO labeling laws, or vibrating machinery, this finding is not surprising at all. In fact, it should have been expected. Internal certainty about a claim of risk to farmers is very close to the same thing as perceived risk to farmers.

There is, however, a very slight difference between the two constructs. The items on the internal certainty scale represent threat *likelihood*, and items on the perceived risk (to farmers) add threat *severity*. Both of these are often included as dimensions of risk perception in science communication research and beyond (e.g., Han et al., 2011). In sum, the nature of the claim (risk to farmers and workers) resulted in a close association between internal certainty about the claim and perceived risk to farmers and workers. When treated as one six-item scale, it exhibited strong reliability (α=.91). Thus, for all the analyses in this dissertation, internal certainty and perceived risk was treated as one construct: *claim belief*. Therefore, *claim belief* is an outcome variable in the model, alongside credibility and behavioral intentions, and will be referred to as such for the remainder of this dissertation. In addition to being a latent variable as identified by the global EFA, *claim belief* was also operationalized by creating a mean scale from the six items.

This type of finding is one of the central reasons why global EFAs are important. Even in cases where strong theory and prior empirical research support a particular factor structure, a measurement evaluation is always necessary to capture slight variations. One question that remained open before these analyses were conducted was whether the two worldview dimensions would emerge as distinct factors (they did). Similarly, it was uncertain whether the two often-theorized dimensions of credibility — expertise and trustworthiness — would emerge as distinct factors (they did not). The results of the global EFA were subsequently tested using a CFA performed on the remaining random 50% split of the main study data. The CFA results are reported in Section 4.1.

3.6. Stimuli

3.6.1. Main message. The main stimulus message consisted of a simulated news article reporting the results of scientific research — a claim of science — in one topic of one issue (Table 1). The language of the news article was held constant across all 15 conditions, except for references to the claim (which of course varied across the three issues) and the clauses that comprised the uncertainty frame manipulation (which of course varied across the five uncertainty frame type conditions). The content and form of the uncertainty frame manipulations, as well as the visual structure and discursive style of the news article itself, were adapted from actual news articles published in *The New York Times*, *The Washington Post*, and *The Wall Street Journal* between 2009 and 2015 (as defined in Chapter 1; as exemplified in Table 2; as coded by Rice et al., 2018). However, the publication was not specified, so as to not trigger confounding preconceptions or attitudes regarding any particular publication. The article directly quoted the scientists who produced the research and identified them as such. All 15 conditions referenced the names of the same fictional scientists. The length of the 15 news articles — including the text of the headline, sub-headline, and break-out box — ranged from 275 words in the climate change control (no uncertainty) condition to 411 words in the vibrating machinery scientific uncertainty condition (M=357.07; SD=40.54). Three exemplar stimuli news articles (conditions: climate change consensus uncertainty, GMO labeling technical uncertainty, and vibrating machinery scientific uncertainty) are available in Appendix A.

3.6.2. Uncertainty type manipulation. The manipulation of uncertainty frame types took the form of several statements attributed to scientists within the article that specified a particular type of uncertainty (at the article level) that qualified both the finding itself and therefore its risk implications for farmers and agriculture workers. These uncertainty statements resemble the operationalizations of substantial prior literature (reviewed in Chapter 1) and each satisfied the key operational components of *high* uncertainty in each uncertainty type (Chapter 1.2; Table 2) *and* the key points of differentiation (Chapter 1.2; Table 2). Four of these uncertainty statements appeared throughout each article, in order to emphasize the uncertainty frame in multiple ways in multiple locations. The first appeared in the sub-heading, the second appeared in the middle of the body text, the third used a break-out box to repeat the second uncertainty statement, and the fourth appeared at the conclusion of the article. Table 15 provides the specific wording of the clauses used to construct each article-level uncertainty frame in each condition. These manipulations were pilot tested (Section 3.4) to verify their effect on perceptions of the corresponding EUtype item.

Table 15

The Four Uncertainty Statements Positioned in Each News Article

	Uncertainty Type			
Position	Deficient	Technical	Consensus	Scientific
Sub-heading	New evidence says the effects of __ are a threat to millions of agriculture workers, although much is still unknown.	New evidence says the effects of __ are a threat to millions of agriculture workers, __ somewhere between 5% and 22%.	New evidence says the effects of __ are a threat to millions of agriculture workers, although some experts disagree.	New evidence says the effects of __ are a threat to millions of agriculture workers, although future research may change this.
Body text, 3rd paragraph	The impact of __ appears to include damage to the __ of working-class farmers and laborers, although much remains unknown and more research is still needed.	The impact of __ appears to include damage to the __ of working-class farmers and laborers, with estimated decreases in __ varying between 5% and 22%.	The impact of __ appears to include damage to the __ of working-class farmers and laborers, although this is in contrast to the research of some other scientists.	The impact of __ appears to include damage to the __ of working-class farmers and laborers, although — like with all science — we expect further research to clarify, or even change, these preliminary findings.
Break-out box	Same as 3rd par.	Same as 3rd par.	Same as 3rd par.	Same as 3rd par.
Conclusion	When considering the findings reported in this study, it is important to note that scientists' understanding of the effects of __ on agriculture workers remains limited because, so far, very little research has been conducted on this issue.	When considering the findings reported in this study, it is important to note that the effect of __ on agriculture workers can vary widely, and that researchers use their data to form an estimated range of possible amounts.	When considering the findings reported in this study, it is important to note that there is continued controversy in the scientific community about the effects of __ on agriculture workers, with some scientists contending that __ is not causing the observed pattern of __ for farmers and laborers.	When considering the findings reported in this study, it is important to note that the effects of __ on agriculture workers is a highly complex process that requires repeated study before any strong conclusions. Therefore, scientists fully expect that future research could cause their current understanding of this issue to change as more data become available.

3.6.3. Manipulation check. One of the purposes of the pilot test was to include a manipulation check — indicating whether the different stimuli across conditions were perceived by participants as being, in fact, different. The best variable to use as a manipulation check is the individual items in the EUtype measure, because they assess participants' perceptions of scientists' opinions. These 2nd order opinions reflect whether the different types of uncertainty portrayed by scientists in the stimulus articles are noticed by participants. For example, if — in the consensus uncertainty condition — the mean score on the consensus uncertainty item in the EUtype measure is significantly lower than the mean score on the consensus uncertainty item in the control condition, then this indicates that the consensus uncertainty manipulation affected perceptions of scientists' consensus uncertainty to a significant degree.

A separate analysis of covariance (ANCOVA) for each EUtype item — controlling for education, prior opinion, deference to science, individualist worldview, hierarchical worldview, and news media consumption for each test — was used to observe differences in mean items scores across conditions. SPSS does not allow for post-hoc tests with ANCOVAs, but the marginal means and upper/lower bound (95% confidence interval) are an adequate stand-in. The results of these analyses, for both the pilot test and the main study are reported in Section 4.3.

3.7. Analyses for Hypotheses and Research Questions

Overall, the literature review and subsequent hypotheses indicate the conceptual model displayed in Figure 2. This model is one where the effects of $X_{1,2}$ (two worldview dimensions) on Y_{1-3} (DVs) are (at least partially) mediated by M (prior opinion), and whatever direct effects X_{1-2} have on Y_{1-3} are moderated by W_{1-3} (Issue). W_{1-3} also moderates the effect of $X_{1,2}$ on M. $Z_{1\text{-}5}$ (frame type) moderates the effects of M on $Y_{1\text{-}3}$. Throughout this section, I will refer to these variables with this notation, for consistency and simplicity (Figure 6).

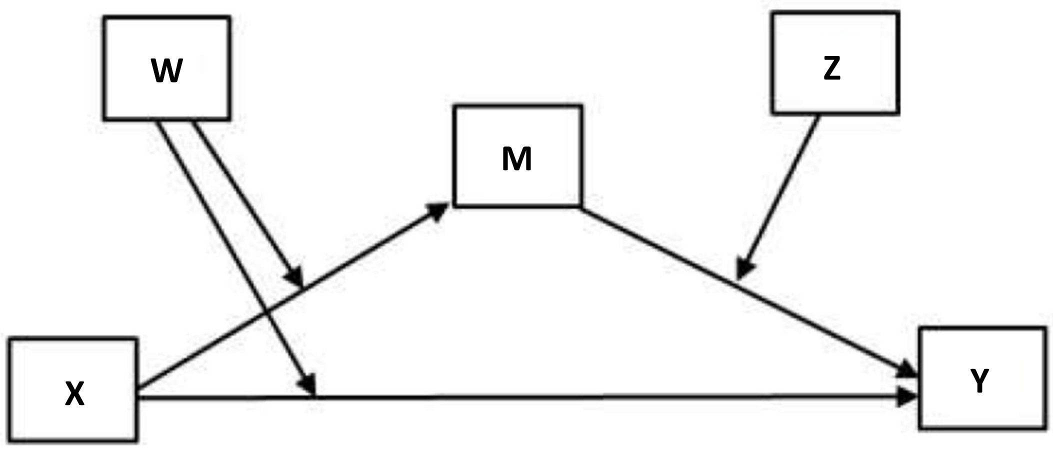


Figure 6. Basic hypothesized structural model.

Note: Figure adapted from "Model 22" in Hayes, 2017.

In many cases, the most intuitive and elegant method of testing a moderated mediation model is via structural equation modeling (SEM). However, a feature of this dissertation's hypothesized model (Figure 2) that adds complexity and difficulty is the fact that both of the moderators are multi-categorical (three levels of issue and five levels of uncertainty frame type — which together constitute the experimental conditions). Still, many other researchers have tested model differences in model path coefficients across the levels of multi-categorical moderators in SEM, so this in itself is not an insurmountable roadblock.

The greatest difficulty for testing the fit of this model all at once is introduced by the fact that these interaction effects involve two worldview dimensions (X_1 and X_2) and three distinct attitudinal responses ($Y_{1\text{-}3}$). *Simultaneously* estimating interaction effects of multi-categorical moderator W (three levels) on the relationships between $X_{1,2}$ and M, as well as multi-categorical moderator Z (five levels) on the relationships between M and each of $Y_{1\text{-}3}$, and comparing those interaction effects between models (conditions), is *not* feasible as a method for hypothesis testing. This is in part due to the intercorrelation of variables, shared error, and insufficient power. Therefore, it was necessary to sacrifice some elegance and parsimony in the interests of achieving the specificity needed to inform the hypotheses and research questions. Specifically, a piecemeal approach was adopted – with some hypotheses being tested with SEM in Mplus and others with Hayes' PROCESS in SSPS. SEM is used whenever possible, due to the numerous advantages of SEM over the ordinary least squares (OLS) regression approach used in PROCESS, such as the ability of SEM to model error terms and specify causal effects (Bollen & Pearl, 2013). As such, the analytic strategy consisted of the following steps. The results are presented in Chapter 4.

3.7.1. Step 1: Test the fit of the basic model. First, after observing the results of the global EFA and subsequent CFA, I used structural regression with maximum likelihood estimation in Mplus to test the overall model fit without the moderating variables W (issue) and Z (frame type) — that is, the proposed theoretical structure of the two worldview dimensions as exogenous variables, prior issue position as the lone mediating variable, and (now, three) attitudinal response outcome variables in all conditions combined. Moderator W (issue) is not relevant in this test because it only involves one issue (i.e., one level of W), and moderator Z (frame type) is not of interest to this hypothesis of the overall test of basic model structure. This test informed H1, which represents one piece of the overall model – positing that, overall, prior issue position predicts the attitudinal response variables which the factor analyses determined to be best described as claim belief, perceived credibility, and behavioral intentions.

A very important consideration is that this overall model *is expected to not fit* (equally, anyways) in all issues, as implied by the moderation effect of the "Issue" variable.

Specifically, the hypothesized relationship between the two worldview dimensions and prior issue position are only expected to be (although not "expected only to be") significant in the climate change conditions. This is why the $H2_a$ prediction that individuals' prior issue position will (at least partially) mediate the relationship between worldview and the attitudinal response variables *is limited to* climate change conditions. To test this proposed mediation path in climate change conditions specifically, a CFA was first conducted in climate change conditions only in order to verify that the measurement model validated by the CFA across all conditions was also valid in the climate change conditions alone (Section 4.1). Next, an SEM identical to that used to test H1 was used to test the fit of the data from the climate change conditions to the overall basic model of $X_{1,2} \rightarrow M \rightarrow Y_{1\text{-}3}$. This test of mediation informed $H2_a$.

$H2_b$ predicted that, in climate change conditions, each X_i has a significant positive effect on M (controlling for the effect of the other X_i and of education, age, deference to science, and news media consumption). $H2_c$ predicted that, in the climate change conditions, any observed effects of X_1 and X_2 on $Y_{1\text{-}3}$ not explained by M (that is, any direct effects) will be positive (also accounting for those covariates).

The predictions of $H2_b$ and $H2_c$ are limited to the climate change conditions, and thus involve only one level of moderator W (issue). Thus, no moderation effect of W is considered for these tests. These predictions also do not involve the $M \rightarrow Y_{1\text{-}3}$ relationships, and thus do not (neither theoretically nor statistically) involve the levels of moderator Z (frame type). Therefore, hypotheses $H2_{b,c}$ are making predictions about a set of exactly eight direct paths (two combinations of $X_{1,2}$ to M; and all six possible combinations of $X_{1,2}$ to $Y_{1\text{-}3}$) within the climate change conditions. All of these paths are estimated in the structural regression model that tests $H2_a$, and the path coefficients displayed in the results of the SEM will already account for covariates mentioned above. In sum, $H2_{b,c}$ can be informed by the SEM model when it is fit to the data of only the climate change conditions.

3.7.2. Step 2: Test the interactions. H3, H4, H5, H6, RQ1, and RQ2 each explore interaction effects in the model (Figure 2). Here, I will progress through each sequentially, explaining the analyses that informed each research question or hypothesis.

Tests of H3. H3 predicted that the effects of the worldview dimensions (X_1, X_2) on prior issue position (M) are dependent on the issue ($W_{1\text{-}3}$) – that is, X_i*$W_{1\text{-}3}$$\rightarrow$M. Specifically, that the relationship between X_i and M is stronger in climate change issue conditions (W_1), compared to either GMO issue conditions (W_2) or vibrating machinery (W_3) issue conditions. To test this hypothesis, it is not necessary to involve $Y_{1\text{-}3}$ and $Z_{1\text{-}5}$, as their values and variations do not affect the variables and relationships in H3 (neither theoretically nor in terms of the study design, because all of the measures of variables in H3 were administered before the stimulus manipulations $Z_{1\text{-}5}$ and before $Y_{1\text{-}3}$). So H3 is a question of simple moderation (X_i*$W_{1\text{-}3}$$\rightarrow$M), not a question of the moderated mediation that characterizes the full theoretical model (Figure 6). This can be tested in PROCESS.

The presence of multiple independent variables ($X_{1,2}$) adds a small amount of complexity for analyses done in PROCESS. That is, Hayes (2017) explains that PROCESS can specify the effect of each of *k* independent variables, but it must be done by running *k* tests, with each test taking turns identifying a different independent variable as being the independent variable of interest and listing the rest as covariates. Thus, the presence of two independent variables ($X_{1,2}$) requires running two tests of X_i*$W_i$$\rightarrow$M: one that structures collectivist worldview (X_1) as the independent variable and egalitarian worldview (X_2) as a covariate, and a second test that does the converse.

H3 is *not* necessarily interested in comparing the $X_i \rightarrow M$ relationship between W_2 and W_3. Therefore, the tests of H3 need only to make two pair-wise comparisons of the $X_i \rightarrow M$ relationship: first, between W_1 (climate change) and W_2 (GMO labeling) and, second, between W_1 (climate change) and W_3 (vibrating machinery). To test this hypothesis, two separate tests (one for each X_i) of multi-categorical moderation (three levels of W) were performed in Hayes' PROCESS (Model 1) in SPSS, controlling for education, deference to science, age, news media consumption, and the other X_i. Each test identifies the model's R^2 change from the inclusion of the X_i*W interaction term and the significance of that interaction. These tests all used standardized versions of the variables (M=0; SD=1), resulting in standardized regression coefficients (β). All tests employed a heteroskedasticity-consistent standard error estimator (HC3 in PROCESS; Long & Ervin, 2000) which is recommended by Hayes and Cai (2007) because large samples can cause even slight heteroskedasticity to affect the outcomes of hypothesis tests.

While these are informative, Robinson and colleagues (2013) demonstrate that when testing the moderating effect of a categorical W on the linear relationship between X and Y, comparisons of simple slopes are superior to tests of the significance of the interaction term. Therefore, it is important that each PROCESS test also produces simple slopes for $X_i \rightarrow Y$ for each level of W, accompanied by 95% confidence intervals for the β coefficient. These coefficients of these simple slopes can also be compared pair-wise with z-tests, which then provides a precise *p*-value for the difference in β. These latter methods were used to inform H3.

However, there has been some confusion and controversy regarding the specific formula used to compare regression coefficients. Paternoster and colleagues (1998) demonstrate that the denominator of the often-used formula for the z-statistic

$$z = \frac{(b_1 - b_2)}{\hat{\sigma}_{b1-b2}}$$

is negatively biased (inflating the probability of rejecting the null). They demonstrate that the appropriate way to de-bias the test statistic is to instead use

$$z = \frac{(b_1 - b_2)}{\sqrt{SEb_1^2 + SEb_2^2}}$$

Paternoster and colleagues (1998) find that the latter formula for the estimated standard error of the difference between coefficients is likely to make the most difference (have the largest debiasing effect) in the final test statistic when there is a large disparity in group sample sizes. While this is decidedly *not* the case in the test of H3 (which makes comparisons between groups of almost exactly a 1:1 ratio in size), this *is* a legitimate concern in the test of H5 (which makes comparisons between groups with a 4:1 ratio in size). Therefore, to ensure a rigorous test that avoids Type 1 error, I will use the latter formula – as recommended by Paternoster et al (1998).

Other points of discussion on this issue have been the dangers of comparing standardized coefficients that originated from subpopulations with significantly unequal variances for an exogenous predictor, because standardized coefficients are a function of the variance of the predictor(s) (Buchner, 2014). For example, if the effect of yearly medical expenses on perceived quality of healthcare was compared between subpopulations of age, the massive inequality in variance in medical expenses between age groups would bias the standardized coefficient. However, due to random assignment in the current study, and because the variables of interest are measured on 1-7 scales, there is no reason to expect that the variance of the variables included in any z-tests in this dissertation to vary significantly across comparison groups.

Tests of RQ1 and H4. While the theoretical model implies that prior issue position is the key driver of the outcome variables, it is still interesting and informative to observe simple mean differences after controlling for these diverse attitudinal and demographic predictors. Specifically, RQ1 and $H4_{a,b}$ explore how attitudinal outcome variables ($Y_{1\text{-}3}$) vary across uncertainty frame types, when controlling for demographic variables such as education and age, behavior patterns such as news media consumption, and prior attitudes such as worldview, prior issue position, and deference to science. This differs from the perspective of RQ2 and H5, which explore how the relationship between prior issue position (M) and Y_i vary across uncertainty frame type conditions.

To inform RQ1 and $H4_{a,b}$, a one-way MANCOVA tested mean differences in each of the attitudinal outcome variables across the five uncertainty frame type conditions, while controlling for the covariates mentioned above. This test was repeated separately for each of the three issues.

Tests of RQ2 and H5. RQ2 and H5 each explore how the effects of M (prior issue position) on $Y_{1\text{-}3}$ differ across levels of Z (uncertainty frame type). As mentioned above, the full model (Figure 6) includes two multi-categorical moderators ($W_{1\text{-}3}$ and $Z_{1\text{-}5}$) that each moderate multiple paths. Therefore, it is not feasible to simultaneously estimate all of these effects in SEM. However, these relationships can be tested via PROCESS with a piecemeal approach. This is where the analysis reached into the lowest levels of parsimony in order to arrive at specificity.

Overall, testing H5 and RQ2 involved running a separate test of a moderated mediation model (Model 14 in Hayes' PROCESS; Figure 7) for each of the three outcome variables in each of the three issues – a total of nine model tests. In these models, the moderator Z is multi-categorical (five levels of frame type). The Figure 7 model differs from the model displayed in Figure 6 by excluding the moderator W (issue). Considering different levels of W (issue) is not applicable because each test occurs within only one issue.

These tests did not estimate the effect of *both* X_1 and X_2 on M, but rather set one as the modeled exogenous variable X and the other as a covariate so as to control for its effect on Y_i. This decision was made because the indirect and direct effects of $X_{1,2}$ on $Y_{1\text{-}3}$ through M were estimated (better) by the SEM analyses of the overall basic model. The purpose of including $X_{1,2}$ is simply to account for their effects on Y_i. These tests each also controlled for education, gender, age, deference to science, news media consumption, and the other two outcome variables not modeled as Y in the test (e.g., tests of $M^*Z_i \rightarrow Y_1$ controlled for Y_2 and Y_3). All tests employed the HC3 heteroskedasticity-consistent standard error estimator in PROCESS, and all used standardized versions of the variables (M=0; SD=1), resulting in standardized regression coefficients (β).

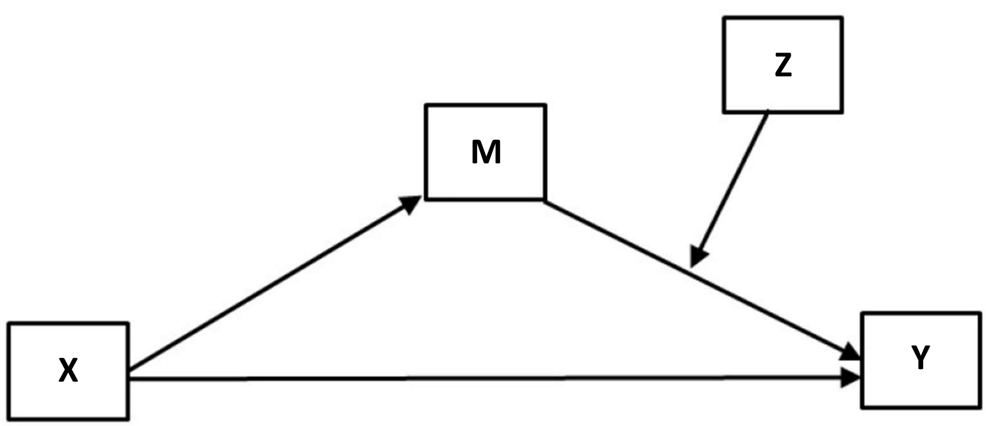


Figure 7. Moderated mediation.

Note. Figure adapted from Hayes' PROCESS (Model 14).

Similar to the test of H3, the levels of Z (uncertainty frame type) were labeled with indicator coding ("dummy coding"), enabling the estimations of multi-categorical moderation effects in PROCESS. As with H3, these tests produce two options for observing interaction effects. First, the significance of the interaction term in the overall model (M* Y_i). Second, the simple slopes (β) of M→ Y_i in each specific frame type condition – any two of which can be compared with a z-test (Paternoster et al., 1998; Robinson et al., 2013).

If the overall interaction term of M*Z is significant, this is evidence that the effect of M (prior issue position) on Y_i (an outcome variable) differs between at least two of the five levels of Z (uncertainty frame type) included in that test. If this is the case, then individual z-tests can be used to make post-hoc pair-wise comparisons to identify which combination(s) of frame types was responsible for creating the significant overall interaction term. If a z-test is significant, this indicates a significant difference between those two specific frame type conditions in their respective β of M→ Y_i. For both, a non-significant result indicates that the effect of M on Y_i did not vary across the levels of Z that were included in that test. Overall, these tests informed H5 and RQ2.

There are some limitations to this piecemeal method. First, it does not simultaneously consider all the outcome variables. Thus, it does not account for shared variance among the outcome variables, and thus each test is somewhat inefficient. However, controlling for the two outcome variables not included in each test mitigates this concern. Also, to the degree that the CFA and SEM find that the outcome variables are independent constructs, we can have *some* confidence in taking each individual test at face value (i.e., that there is little shared variance among the outcome variables). However, it is important to reiterate that *while* this approach *does* facilitate, for each outcome variable, a test of whether each uncertainty frame has a different effect than the control condition (and to compare uncertainty frames against each other), it *does not* determine whether the three outcome variables are differentially affected by a given uncertainty frame.

Tests of H6. H1 describes motivated reasoning effects, such that prior issue position predicts attitudinal responses to the science claim. H6 posits that these motivated reasoning effects will be stronger in conditions with an(y) uncertainty frame than in conditions without (i.e., control conditions). Because the motivated reasoning effect was calculated as a standardized regression coefficient (the effects of M on $Y_{1\text{-}3}$), H6 was tested with a z-test comparing standardized coefficients from a model that included only participants who received an uncertainty frame with the standardized coefficients from a model consisting only of participants in control conditions (who did not receive an uncertainty frame). This z-test used the formula for the standard error of the difference recommended by Paternoster et al. (1998).

Table 16 presents a summary of each hypothesis and research question alongside their corresponding tests.

Table 16

Hypotheses, Research Questions, and Their Corresponding Analytical Approaches

	Hypotheses and Research Questions	Analytical Approaches
H1	Prior issue position predicts the outcome variables.	SEM
$H2_{a,b,c}$	In climate change conditions, worldview predicts prior issue position, and prior issue position mediates the effect of worldview on the outcome variables.	SEM
H3	The relationship between worldview and prior issue position is dependent on the issue.	PROCESS, z-tests
RQ2 $H4_{a,b}$	How the means of the outcome variables compare across the five uncertainty frame conditions, when controlling for relevant attitudinal priors.	MANCOVA
H5	The effect of prior issue position on the outcome variables is strongest in consensus uncertainty, relative to the other uncertainty frame types.	PROCESS, z-tests
RQ2	Interaction effect of uncertainty frame type condition on relationship between prior issue position and the outcome variables.	PROCESS, z-tests

H6	The motivated reasoning effect (H1) will be stronger in conditions with an uncertainty frame than in conditions without.	SEM, z-test

Chapter 4: Results

4.1 Confirmatory Factor Analyses

The remaining 1,106 cases of the random 50% split of the main study sample were used to confirm the measurement model identified by the global EFAs described in Section 3.5.2. To do this, a confirmatory factor analysis (CFA) was conducted in Mplus with ML estimation. The fit indices used to determine appropriate fit were chi-square test of model fit, RMSEA, CFI, and SRMR. Like with the EFAs, the chi-square test is expected to be significant due to the large sample size. The fit indices indicated good fit, with only the CFI falling outside the recommended bounds (Table 17).

Table 17

Values of Fit Indices from CFAs Across All Topics

Modification	$\Delta\chi^2$	χ^2	*df*	*p*	*RMSEA*	*RMSEA 90% CI*	CFI	SRMR
None	-	2783.72	449	.000	.069	[.066, .071]	.89	.047
C5 - C6	187.91	2595.81	448	.000	.066	[.063, .068]	.90	.047
InCo1- InCo4	178.42	2417.39	447	.000	.063	[.061, .066]	.91	.049
HE2 - HE3	293.28	2124.11	446	.000	.058	[.056, .061]	.92	.049

Note. Modification= the modification to the base model, indicating allowed correlated errors between items. $\Delta\chi^2$=change in chi-square test statistic from previous model; χ^2 = chi-square test of model fit; *df* = degrees of freedom; RMSEA = root-mean-square error of approximation; CI = confidence interval; SRMR = standardized root-mean-square residual.

Next, modification indices, associated estimated parameter changes, and standardized residuals were inspected for any estimations of substantial improvement in model fit that also aligned with theoretical explanations or expectations. Three suggested modifications stood out. In particular, three of the scales (credibility, collectivism worldview, and egalitarian worldview) contained pairs of items that were near-synonyms with each other. Specifically, the fifth (C5; Trustworthy-Untrustworthy) and sixth (C6; Honest-Dishonest) credibility items were one such pair. The first (InCo1; "The government interferes far too much in our everyday lives") and fourth (InCo4; "The government should stop telling people how to live their lives”) collectivism items were another pair. The second (HE2; “Our society would be better off if the distribution of wealth was more equal”) and third (HE3; “We need to dramatically reduce inequalities between the rich and the poor…”) egalitarian items were another pair. Structural features like this can produce a measurement effect such that values for these items vary for reasons other than their common membership in the “credibility,” “collectivism,” or “egalitarian” factor, respectively.

Therefore, new CFAs were conducted — allowing correlated errors between each pair mentioned (C5 with C6; InCo1 with InCo4; HE2 with HE3). These modifications were made iteratively, testing the model for fit and re-checking the modification indices after each test. After each iteration, the largest of the remaining modification indices was the next of the pairs listed above. Each modification significantly improved the overall model fit (Table 17), and the final model demonstrated good fit. The loadings of all parameters, factor correlations, and their significance are reported in Appendix C.

Because H2 makes predictions about the climate change conditions specifically, and because SEM is used to test H2, an additional CFA for just the climate change conditions was performed with the same methods and fit indices as the full CFA. This CFA used the 372 climate change cases contained in the second 50% random split of the main study sample (so, all of the climate change cases that were in the sample for the full CFA). As reported in Table 18, the unmodified measurement model demonstrated adequate fit, and the modification indices recommended – each in turn – the same modifications made to the CFA that used all topics. After these modifications, the fit indices demonstrated that the measurement model was a good fit for data from the climate change conditions.

Table 18

Values of Fit Indices from CFAs in Climate Change Conditions

Modification	$\Delta\chi^2$	χ^2	*df*	*p*	*RMSEA*	*RMSEA 90% CI*	CFI	SRMR
None	-	1241.39	449	.000	.069	[.064, .073]	.91	.049
C5 - C6	56.20	1185.19	448	.000	.067	[.062, .071]	.92	.050
InCo1- InCo4	74.71	1110.48	447	.000	.063	[.059, .068]	.93	.054
HE2 - HE3	45.64	1064.84	446	.000	.061	[.056, .066]	.93	.049

Note. Modification= the modification to the base model, indicating allowed correlated errors between items. $\Delta\chi^2$=change in chi-square test statistic from previous model; χ^2 = chi-square test of model fit; *df* = degrees of freedom; RMSEA = root-mean-square error of approximation; CI = confidence interval; SRMR = standardized root-mean-square residual.

4.2 Variable Descriptives

4.2.1. Means and correlations. All means and standard deviations of each measure's mean scale and those of each measure's individual items are provided in Appendix B, along with skewness and kurtosis statistics for each mean scale. Notably, participants reported more supportive prior opinions in the climate change condition (Table 19) compared to the GMO labeling or vibrating machinery conditions. As expected, prior opinions about occupational hazards of farming (i.e., the vibrating machinery conditions) varied the least of all issues (Table 18), such that the standard deviations of prior opinions about climate change and GMO labeling were each more than 50% higher. This indicates that extreme high or low prior opinions regarding the issue of occupational hazards of farming (i.e., the vibrating machinery conditions) are very infrequent relative to the issues of climate change and GMOs. This is important because it is hypothesized that strong prior opinions (whether positive or negative) are what drive the motivated reasoning effects.

Across issues, ratings of credibility were also significantly above the midpoint of the seven-point scale (Table 19). Worldview correlates with prior opinion on climate change (as expected), but does not have a strong relationship with prior opinion on GMOs or vibrating machinery (as expected).

Table 19

Means, Standard Deviations, and Correlations of Multi-Item Mean Scales

Scale	*M*	*SD*	1	2	3	4	5	6	7	8	9
1. Prior (cc)	5.08	1.52	-	-	-	.45*	.52*	.74*	.85*	.51*	.61*
2. Prior (gmo)	3.80	1.49	-	-	-	.42*	.12*	-.01	.27*	.04	.30*
3. Prior (vm)	3.96	0.94	-	-	-	.03	.02	.10*	.31*	.06*	.09*
4. Def2	4.57	1.36				-	.25*	.35*	.24*	.35*	.37*
5. InCo	3.35	1.16					-	.48*	.27*	.31*	.27*
6. HE	4.60	1.70						-	.37*	.39*	.37*
7. Belief	4.78	1.49							-	.45*	.53*
8. BI	5.41	1.25								-	.35*
9. Cred	5.29	1.27									-

Note: *=p<.001; all scale items and item means listed Appendix B; n = 2,247.
M=scale mean. SD=standard deviation; Prior=prior issue position; Def2=deference to science; Posi=positivist understanding of science; InCo=individualist/collectivist; HE=hierarchical/egalitarian; ECgen=general external certainty; ICgen=general internal certainty; Cred=perceived credibility.

4.2.2. Normality. Every mean scale — the variables constructed from multi-item attitudinal measures — rejected the null hypothesis of the Shapiro-Wilk test (p<.001), indicating a deviation from a normal distribution. This is expected, as large samples make minor deviations significant — and there is no reason to expect that self-reported opinions about something are necessarily normally distributed. Although the tests used in this dissertation assume normally-distributed variables, the parametric tests and the large sample used in this dissertation are fairly robust against detrimental effects of non-normality (Rasch & Guiard, 2004; Tabachnick & Fidell, 1996).

Still, it is worth specifying which (and to what degree) variables are not normally distributed. A common convention is that skewness values above |1| are considered highly skewed, and values between |0.5| and |1| can be considered moderately skewed. By these standards, prior issue position about climate change, deference to science, and credibility perceptions are moderately left-skewed. Perhaps the best way to reason about normality is to observe the distribution(s) (Figure 8).

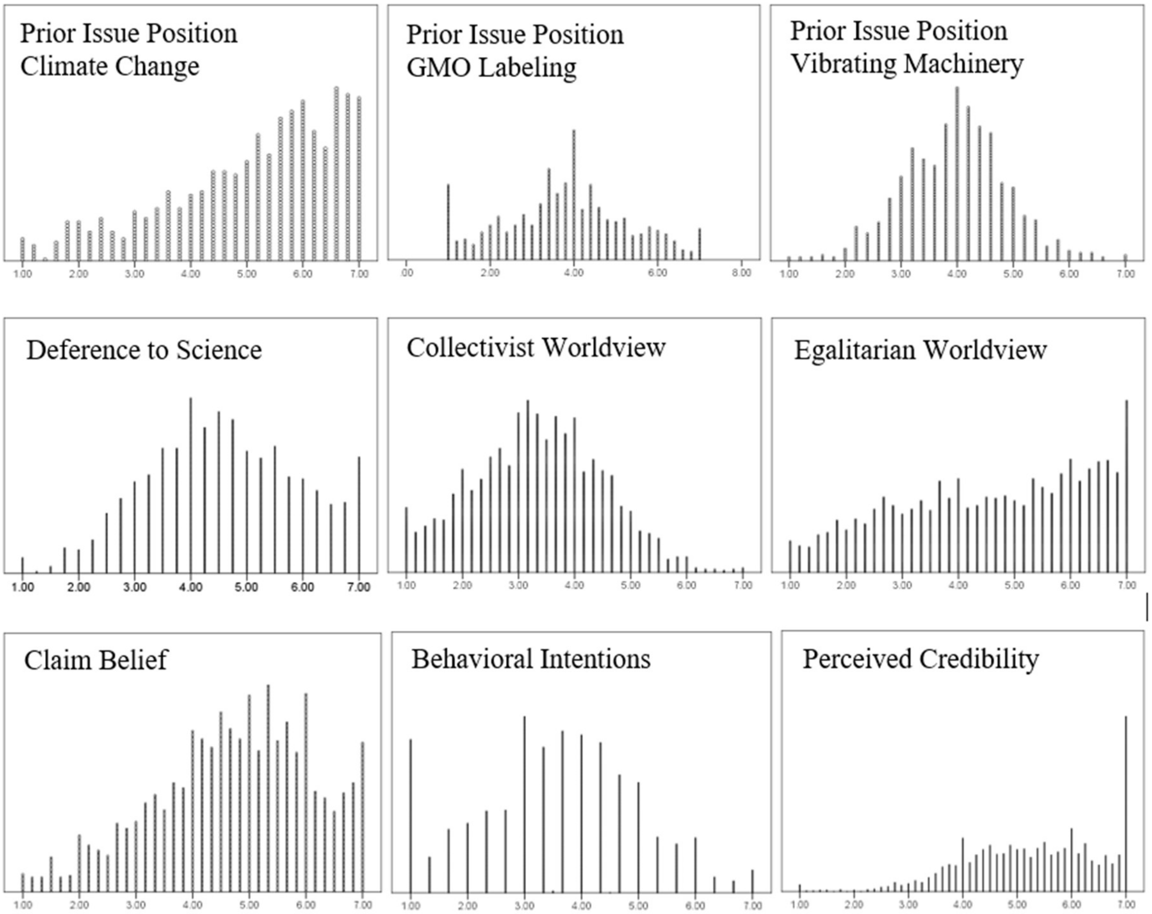


Figure 8. Distributions of each mean scale used as variables in the analyses.

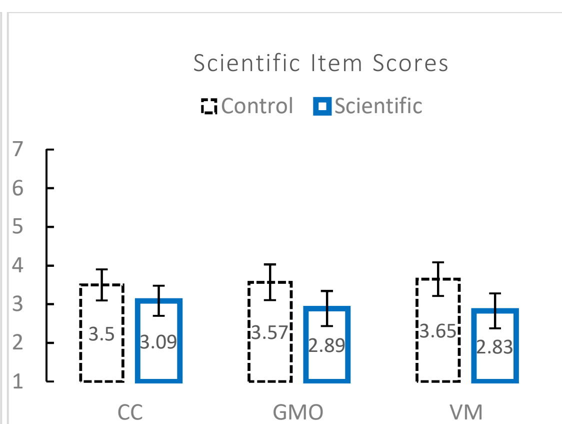
4.2.3. Transformations. To reduce multicollinearity in tests of interactions and to increase the interpretability of intercepts and results, each mean scale was mean-centered before conducting the analyses reported in the remaining sections. Some tests used fully-standardized scales (M=0; SD=1), and are noted as such in the future sections.

4.3 Manipulation Checks

4.3.1. Pilot test manipulation check. A series of ANCOVAs in the pilot test suggested that, overall, participants' ratings of the distinct types of uncertainty held by the scientists in each news article were somewhat sensitive to the manipulations of the articles' portrayals of those distinct types of uncertainty. Because post-hoc tests for ANCOVAs are unavailable in SPSS, the marginal means and 95% confidence intervals are used for indication of significant mean differences.

Figure 9 portrays four cluster bar graphs — each showing the mean scores on one of the items of the EUtype measure (e.g., perceptions of scientists' level of deficient uncertainty). Each graph contains 3 pairs of bars, which are the results shown for each separate issue (e.g., climate change). The pairs within each issue compare the EUtype items scores between the control condition and the treatment condition (that is, the treatment condition that corresponds with that item's uncertainty type). The scores are oriented such that higher values indicate more certainty, less uncertainty.

For example, for climate change, scores on the consensus item (bottom left graph) are significantly lower in the consensus uncertainty conditions than in the control conditions (as shown by the error bars indicating the 95% confidence interval). This comparison approaches significance in the other issues, likely restricted by the low pilot test cell size (ranging from n=37 to n=45). Because lower scores represent more uncertainty, this is tentative indication that when participants are exposed to portrayals of scientists having consensus uncertainty, they report slightly higher perceptions of scientists having consensus uncertainty.



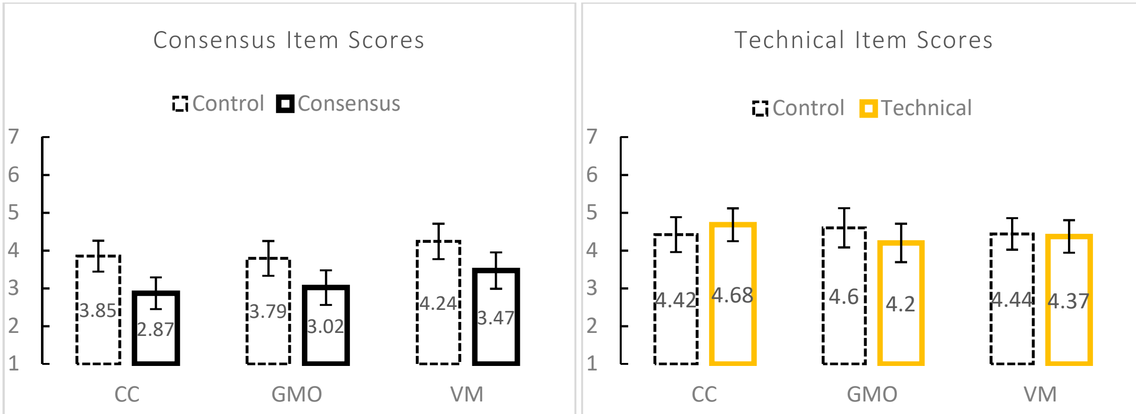


Figure 9. Pilot test manipulation check results with 95% confidence interval.

Note: Each box represents one item (uncertainty type). The bars show, for each issue, the estimated marginal means for EUtype items (higher scores indicate more certainty) in control conditions and the treatment conditions that match the uncertainty type referenced by that EUtype item. Values are calculated with ANCOVAs controlling on prior issue position, deference to science, collectivist worldview, egalitarian worldview, education, and news media consumption.

Similarly, for deficient and scientific uncertainty — across issues — the item means exhibit a trend in the expected direction with consistent, but non-significant, differences between the treatment condition and the control condition. The vibrating machinery conditions were most sensitive to the manipulation.

In the pilot test, despite the repeated emphasis of technical uncertainty in the conditions with technical uncertainty-framed articles, perceptions of scientists' technical uncertainty were clearly no different in those conditions compared to the control conditions. As mentioned in Section 3.5.3., after observing no difference, the technical uncertainty EUtype item was modified — for use in the main study — to more specifically reflect the technical uncertainty manipulation.

To strengthen all of the manipulations and/or to ensure quality attention to the stimulus in the sample, the main study included the filtering question asking participants whether they promised to read the entire article. Also, in the main study, participants who completed the study and/or stimulus too slowly or too quickly were eliminated — thereby reducing poor responses in the sample. Also, in the main study, a clause was added to the instructions that prefaced the main study stimulus that reminded participants that their careful attention was important to the study.

4.3.2. Main study manipulation check. In much more convincing fashion than the pilot test, the manipulation check in the main study demonstrated quite clearly that participants were sensitive to each frame type manipulation in each issue. A series of ANCOVAs in the main study indicated that reported perceptions of the types of uncertainty held by the scientists in each news article were significantly affected by portrayals of those same distinct types of uncertainty. In all analyses, Levene's test of equality of error variances was non-significant, indicating no evidence to reject the null hypothesis that error variances in the respective EUtype items are equal across groups.

Figure 10 displays the results of the main study manipulation check, where (as in Figure 9) higher scores on each EUtype item indicate more certainty, and lower scores indicate more uncertainty. For example, for climate change, scores on the consensus item (bottom left graph) are significantly lower in the consensus uncertainty conditions than in the control conditions (as shown by the error bars indicating the 95% confidence interval).

Figure 10 compares treatment conditions to control conditions. While not displayed in Figure 10, each of the mean EUtype item scores in the corresponding treatment condition were almost always *also* significantly different than the mean score of that EUtype item in the *other* treatment conditions. For example, for climate change, scores on the consensus uncertainty EUtype item were significantly lower (more uncertainty) in the consensus uncertainty conditions than in *any* of the other conditions. That is, the manipulations were not only distinguished from the control condition, but were also uniquely distinguished from the other uncertainty types. The only exception is that scores on the technical uncertainty EUtype item in the technical uncertainty conditions did not (quite) differ significantly from that item's scores in the deficient and scientific uncertainty conditions.

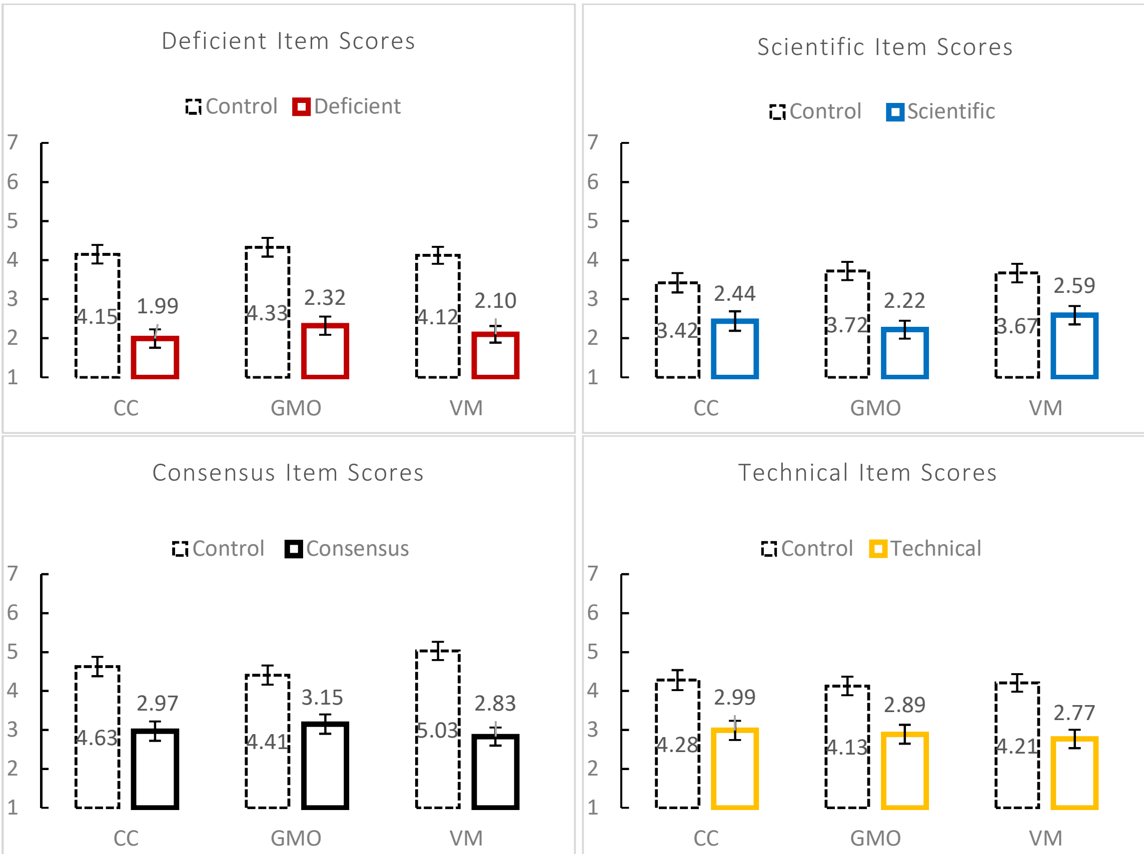


Figure 10. Main study manipulation check results with 95% confidence interval.

Note: Each box represents one item (uncertainty type). The bars show, for each issue, the estimated marginal means for EUtype items (higher scores indicate more certainty) in control conditions and the treatment conditions that match the uncertainty type referenced by that EUtype item. Values are calculated with ANCOVAs controlling on prior issue position, deference to science, collectivist worldview, egalitarian worldview, education, and news media consumption.

As displayed in Figure 10, all differences in item scores between control conditions and treatment conditions are significant — for each issue. This is largely a function of the large number of observations per condition. However, the absolute differences in marginal means are much greater than those observed in the pilot test. Further, Table 20 displays all ANCOVA model statistics, which describe the degree to which EUtype item scores (within a specific Issue) can be explained by variations in (any) stimulus frame condition. Table 20 shows that the effects of variations in frame condition (on the individual EUtype items) in the main study are moderate to large (i.e., η^2 values from .05 to .21).

Table 20

ANCOVA Model Statistics: Effect of Frame Condition on EUtype Items

EUtype Item (ISSUE)	*Main Study*				*Pilot Test*			
	df	*F*	*p*	η^2	*df*	*F*	*p*	η^2
Deficient								
(CC)	[4, 731]	46.27	.000	.20	[4, 225]	3.03	.018	.05
(GMO)	[4, 737]	47.49	.000	.21	[4, 179]	1.56	.187	.03
(VM)	[4, 743]	49.70	.000	.21	[4, 182]	3.84	.005	.08
Consensus								
(CC)	[4, 731]	16.72	.000	.15	[4, 225]	5.00	.001	.08
(GMO)	[4, 737]	15.43	.000	.07	[4, 179]	1.72	.148	.04
(VM)	[4, 743]	50.17	.000	.21	[4, 182]	2.02	.094	.04
Scientific								
(CC)	[4, 731]	25.07	.000	.06	[4, 225]	1.52	.196	.03
(GMO)	[4, 737]	27.25	.000	.13	[4, 179]	2.08	.085	.04
(VM)	[4, 743]	10.59	.000	.05	[4, 182]	2.17	.074	.05
Technical								
(CC)	[4, 731]	17.56	.000	.09	[4, 225]	0.60	.667	.10
(GMO)	[4, 737]	16.52	.000	.08	[4, 179]	0.80	.524	.02
(VM)	[4, 743]	23.07	.000	.11	[4, 182]	0.05	.996	.001

Note. df=degrees of freedom; η^2=partial eta squared;
Model statistics describe the degree to which variations in an EUtype item (within a specific issue group) can be explained by variations in (any) stimulus frame condition. Controlling for education, prior issue position (an issue-specific covariate value), egalitarian worldview, individualist worldview, deference to science, and media consumption.

These findings demonstrate that variations in uncertainty frame types were noticed by participants and clearly affect their perceptions of the specific types of uncertainty that the scientists held. As such, these findings provide crucial context when interpreting the results of the tests of the hypotheses and research questions, and instill confidence in the validity and rigor of the findings.

4.4 Basic Model Structure

4.4.1. Structural regression model. Overall – that is, aggregated across all frame types and issues – prior issue position was hypothesized to predict claim belief, credibility, and behavioral intentions (H1) as evidence of motivated reasoning in response to scientific evidence. Having found the measurement model to be well-fitting to the data – and in order to inform H1 – the relationships between latent variables of collectivist worldview, egalitarian worldview, prior issue position, claim belief, credibility, and behavioral intentions were then tested in a structural regression model (Figure 11) with ML estimation using 100% of the main study data (n=2247) and 1,000 bootstrapped samples. Overall, the model achieved adequate fit (Table 21) such that the RMSEA (<.08), CFI (>.95), and SRMR (<.08) all either approach or exceed the "good fit" criteria (Hu & Bentler, 1999).

Table 21

Values of Fit Indices from SEM Variants

Issues	χ^2	*df*	*p*	*RMSEA*	*RMSEA 90% CI*	CFI	SRMR
All Issues Combined	4741.64	586	.000	.056	[.055, .058]	.91	.07
Climate Change Only	2289.35	586	.000	.063	[.060, .065]	.91	.08

Note. $\Delta\chi^2$=change in chi-square test statistic from previous model; χ^2 = chi-square test of model fit; *df* = degrees of freedom; RMSEA = root-mean-square error of approximation; CI = confidence interval; SRMR = standardized root-mean-square residual.

Figure 11 displays selected paths of interest – suppressing many direct and indirect effects that are not relevant to H1 or are not meaningful when issues are combined. A figure displaying all direct and indirect paths with their standardized coefficients is available in Appendix D, and the individual item loadings and error terms are displayed in Appendix C.

As predicted by H1, the results indicate that – aggregated across three diverse science issues and controlling for a plethora of other potential effects – individuals' prior opinion toward a general science issue predicts their attitudinal responses to reports of new scientific evidence. This is a clear motivated reasoning effect and appears to be strongest for claim beliefs and weakest for behavioral intentions. H1 is supported by these results.

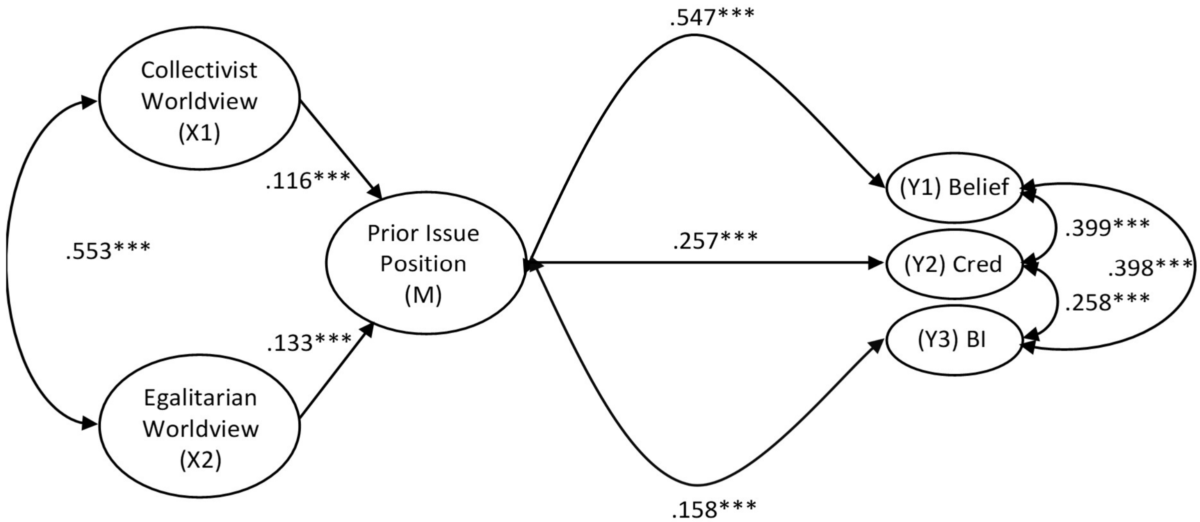


Figure 11. SEM path model of all conditions combined.

Note: Standardized path coefficients; ***= p<.001; hidden covariates in the model are education, age, deference to science, and news media consumption. A full model displaying all direct and indirect paths, their standardized coefficients, factor loadings, and error terms is available in Appendix D.

It is interesting to note that while collectivist (X_1) and egalitarian (X_2) worldview dimensions relate strongly to each other, Figure 11 displays a weak effect on prior issue position (M). Accordingly, the standardized *indirect* effects (not displayed; see Appendix D) of X_1 and X_2 on $Y_{1\text{-}3}$, respectively, range from a minimum of .018 to a maximum of .073. These findings have a simple explanation. Namely, this analysis was conducted using the full main study dataset, which includes all three issues (Appendix D displays a separate full model for each separate issue). Only in the climate change issue (Figure 14) was it hypothesized that the worldview dimensions would predict prior issue position. In the GMO (Figure 12) and vibrating machinery (Figure 13) conditions, respectively, these relationships are much different from each other and from the climate change conditions.

One of the purposes of selecting these three particular science issues was that they differed in the nature and strength of the link between worldview and prior issue position, and in the strength of prior issue position itself (which determines the motivated reasoning effect). As reported in Section 4.2.1, extreme high and low prior issue positions are more likely in the climate change and GMO labeling conditions, and less likely in the vibrating machinery conditions. In theory, strong prior opinions are what produce motivated reasoning effects. Figure 14 shows that, in climate change conditions, the motivated reasoning effects of prior issue position on each of the outcome variables are strong and significant. Figure 12 shows that, in GMO labeling conditions, the motivated reasoning effects on claim beliefs and credibility perceptions are strong and significant, but the effect of prior issue position on behavioral intentions is not significant. Figure 13 shows that, in vibrating machinery conditions, the motivated reasoning effect on claim beliefs is significant, but the effects of prior issue position on credibility perceptions and behavioral intentions are not. These findings are consistent with the assumptions that were made in choosing this assortment of issues for analysis in this dissertation.

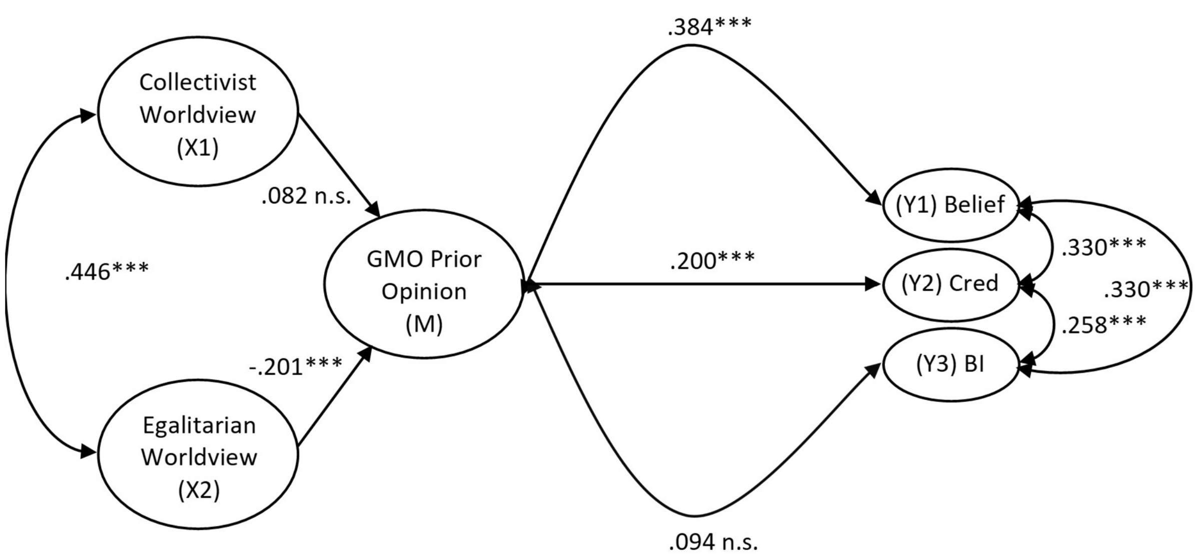


Figure 12. SEM path model of GMO labeling conditions only.

Note: Standardized path coefficients; ***= p<.001; hidden covariates in the model are education, age, deference to science, and news media consumption. A full model displaying all direct and indirect paths, their standardized coefficients, factor loadings, and error terms is available in Appendix D.

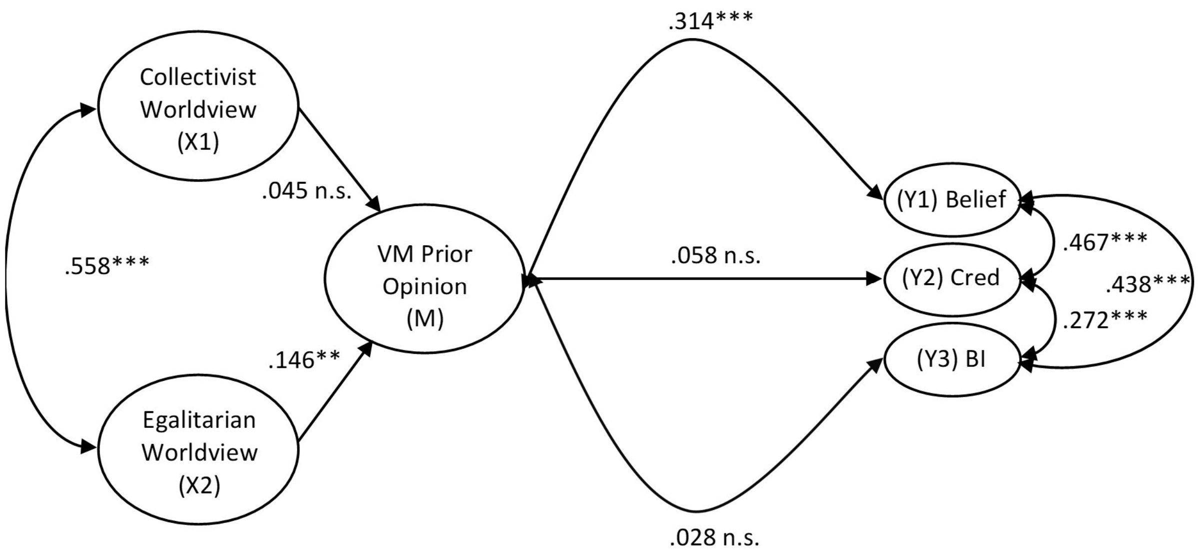


Figure 13. SEM path model of vibrating machinery conditions only.

Note: Standardized path coefficients; ***= p<.001; hidden covariates in the model are education, age, deference to science, and news media consumption. A full model displaying all direct and indirect paths, their standardized coefficients, factor loadings, and error terms is available in Appendix D.

$H2_{a,b,c}$ focus on the climate change conditions, specifically (Figure 14). In particular, $H2_a$ predicts that in the climate change conditions, prior issue position (M) will (at least partially) mediate the effect of the worldview dimensions ($X_{1,2}$) on the outcome variables (Y_{1-3}). The SEM testing this climate-change-specific hypothesis used the same methods and measures as the SEM testing H1 in the full dataset. As reported in Table 20, the model demonstrated adequate fit. Figure 14 displays all direct and indirect paths, the standardized coefficients of the direct paths, and the indirect effects. A full model complete with factor loadings and error terms is available in Appendix D.

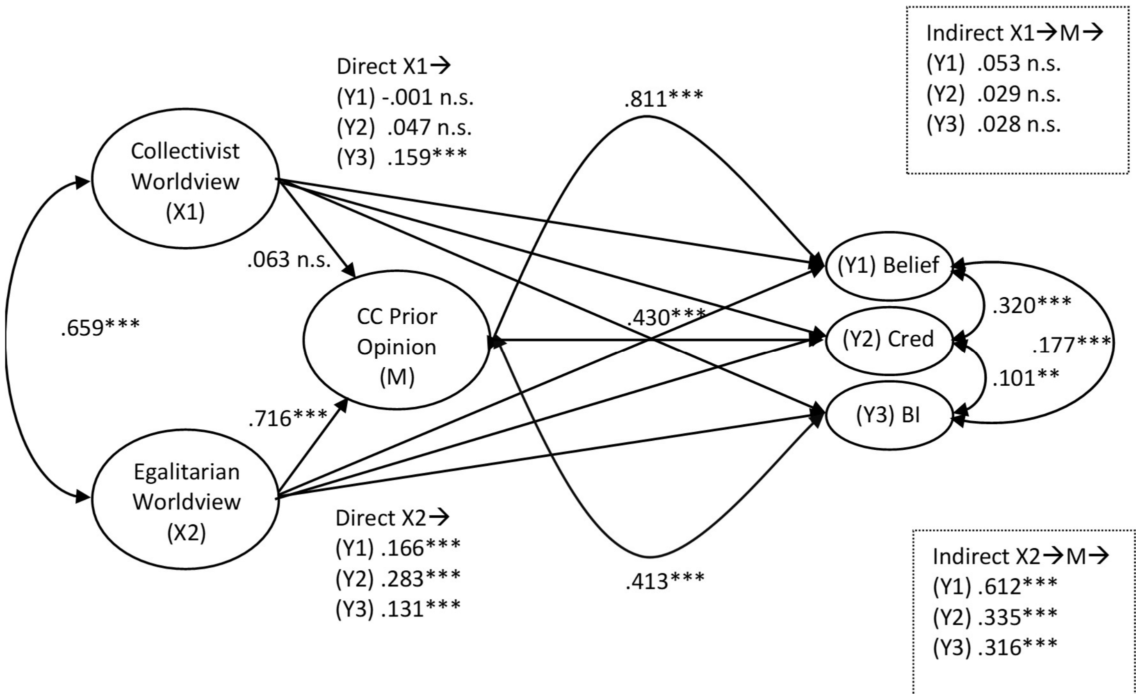


Figure 14. SEM path model of climate change conditions.

Note: Standardized path coefficients; ***= $p<.001$; **=$p<.01$; hidden covariates in the model are education, age, deference to science, and news media consumption. Break-out boxes identify the specific indirect effects of X_i on Y_i by way of M; A full model displaying all direct and indirect paths, their standardized coefficients, factor loadings, and error terms is available in Appendix D.

As predicted by $H2_b$, the results indicate that in the climate change conditions, the effect of hierarchical-egalitarian worldview (X_2) on climate change prior issue position (M) is significant and positive. As predicted by $H2_a$, prior issue position on climate change accounts for a significant portion of the effect of hierarchical-egalitarian worldview (X_2) on each of the attitudinal outcome variables. The standardized coefficients of the indirect effects are listed in the break-out boxes on the right side of Figure 14. These indirect effects represent the effect of X_i on Y_i that is explained by M.

However, contrary to $H2_a$, prior opinion did *not* mediate the effect of individualist-collectivist worldview (X_1) on $Y_{1\text{-}3}$. This is because, contrary to $H2_b$, there was no effect of collectivist worldview on prior issue position – as well as almost no direct effect of collectivist worldview on $Y_{1\text{-}3}$. It is clear from these results that hierarchical-egalitarian worldview plays the dominant role in explaining prior general opinions about the broad issue of climate change. It is also very interesting to note the strong motivated reasoning effect within the context of climate change, with M being a strong predictor of Y_{1-3} – most notably of claim beliefs (β=.811). That is, an individual's prior general opinion on climate change affects their subsequent claim beliefs, scientist credibility ratings, and behavioral intentions measured as responses to a new scientific finding about the effects of climate change.

$H2_c$ predicted that any significant residual direct effects of X_1 and X_2 on Y_{1-3} would be positive in direction, not negative. This hypothesis is supported by the results, as the residual direct effects that exist – those of egalitarian worldview on each Y_{1-3}, and of collectivist worldview on behavioral intentions – are indeed positive in direction.

4.5 Conditional Effects

4.5.1. H3: Moderating effect of issue. H3 predicted that the relationship between X (worldview) on M (prior issue position) is moderated by W (issue), such that the effect of X on M would be stronger in climate change conditions compared to either GMO labeling conditions or vibrating machinery conditions. Table 22 reports the results of two separate tests (one for each X_i) of multi-categorical moderation performed in PROCESS (Model 1) using SPSS. The simple slopes for $X_i \rightarrow M$ for each level of W are displayed in Table 22, accompanied by 95% confidence intervals for the standardized β coefficient. As recommended by Robinson et al. (2013), these differences in β between levels of W were also compared using z-tests – using the formula recommended by Paternoster et al. (1998) – resulting in a z-statistic and *p*-value for each pair-wise comparison (Table 22).

Table 22

The Moderating Effect of Issue

	Model (X_i*W→M)	X_i*W Interaction

	R^2	F	$df1$	$df2$	p	ΔR^2	F	p
X_1 (collectivist)	.33	165.61	11	2235	.000	.04	74.77	.000
X_2 (egalitarian)	.40	244.78	11	2235	.000	.11	203.47	.000

	Simple Slopes						*z-test of difference from $X_i \rightarrow M$ in CC*				
Collectivist	β	t	SE	P	$LLCI$	$ULCI$	β_{diff}	z	$SEdiff$	p	n
$X_1 \rightarrow$M in CC	.384	12.02	.032	.000	.322	.447	-	-	-	-	-
$X_1 \rightarrow$M in GMO	.025	0.61	.041	.541	-.055	.105	.435	6.91	.052	.000	1492
$X_1 \rightarrow$M in VM	-.109	-3.42	.0315	.001	-.170	-.046	.596	10.98	.045	.000	1498
Egalitarian											
$X_2 \rightarrow$M in CC	.629	20.55	.031	.000	.569	.689	-	-	-	-	-
$X_2 \rightarrow$M in GMO	-.112	-2.80	.040	.005	-.190	-.034	.741	14.71	.050	.000	1492
$X_2 \rightarrow$M in VM	-.052	-1.82	.029	.070	-.108	.004	.629	16.27	.036	.000	1498

Note. All analyses included covariates of education, deference to science, age, news media consumption, and the other X_i; CC = climate change conditions; GMO = GMO labeling conditions; VM = vibrating machinery conditions; R^2 = variance in prior issue position explained by the model; df = degrees of freedom; ΔR^2 = change in R^2 from the interaction term; *LLCI* = lower limit of 95% confidence interval; *ULCI* = upper limit of 95% confidence interval; β_{diff} = difference in β from the β of $X_i \rightarrow$M in CC; n = combined samples size of compared groups; z = z-statistic from z-test comparing standardized β coefficients; *SEdiff* = denominator of z-test formula as prescribed by Paternoster et al. (1998).

Table 22 demonstrates that the respective effects of collectivist worldview and egalitarian worldview on prior issue position differ by issue. Specifically, the z-test comparisons of standardized coefficients indicate a significant difference between climate change conditions and GMO conditions, and also a significant difference between climate change conditions and vibrating machinery conditions. That is, both worldview dimensions have a stronger effect on prior issue position about climate change than about either GMOs or occupational hazards of farming (vibrating machinery). Thus, H3 is supported.

4.5.2. $H4_{a,b}$ and RQ1: Means of outcome variables across conditions. H4 and RQ1 explore the ways in which individuals' reported claim belief, credibility perceptions, and behavioral intentions vary (or are invariant) across uncertainty frame type conditions. Specifically, $H4_a$ predicts that $Y_{1\text{-}3}$ will each be lower in conditions with a consensus uncertainty frame than in control conditions. $H4_b$ predicts that $Y_{1\text{-}3}$ will each be lower in conditions with a consensus uncertainty frame than in conditions with technical or scientific uncertainty frames. For the remaining uncertainty frame types, RQ1 asks how $Y_{1\text{-}3}$ compare across uncertainty frame conditions. One way to inform H4 and RQ1 is to combine the cases of an uncertainty frame from all three issues and compare it to the control condition cases from all three issues. This would evidence whether – irrespective of issue – Y_{1-3} differed between uncertainty frame conditions. Another way is to separate the analyses by issue, so that issue-specific effects might become apparent. That is, it is reasonable that, for example, Y_i could differ significantly between two frame conditions in the context of climate change, but not differ significantly between those same two conditions in the context of GMO labeling or vibrating machinery. The former method – which combines all three – would mask that important and interesting interaction effect that occurs only within climate change. However, the findings of the latter method might not be generalizable to other issues. Therefore, this dissertation employed both approaches.

To inform $H4_{a,b}$, Figure 15 displays the results of MANCOVAs with LSD post-hoc pair-wise comparisons that show how the marginal means (with 95% confidence intervals) of Y_{1-3} in the consensus uncertainty condition(s) compare to the marginal means of Y_{1-3} in each of the other frame type conditions. Table 23 reports the test statistics of the interaction term. This analysis is repeated four times – once with all issues combined (top left) and once in each of the three issues separately. In Figure 15, the y-axis reflects non-standardized units on the 7-point scale used to measure each outcome variable, and the point where y=0.00 represents the Y_i value in each other frame type condition(s), respectively. Thus, confidence intervals that do not reach the x-axis (y=0.00) indicate a significant difference ($p<.05$) in the value of Y_i between the consensus uncertainty condition(s) and the other uncertainty frame type condition(s) that is referenced. Confidence intervals that cross the x-axis (y=0.00) indicate a non-significant difference ($p\geq.05$). For example, within climate change conditions (top right), claim belief is significantly lower in consensus uncertainty conditions than in technical uncertainty conditions (yellow box) and also significantly lower than in scientific uncertainty conditions (blue box).

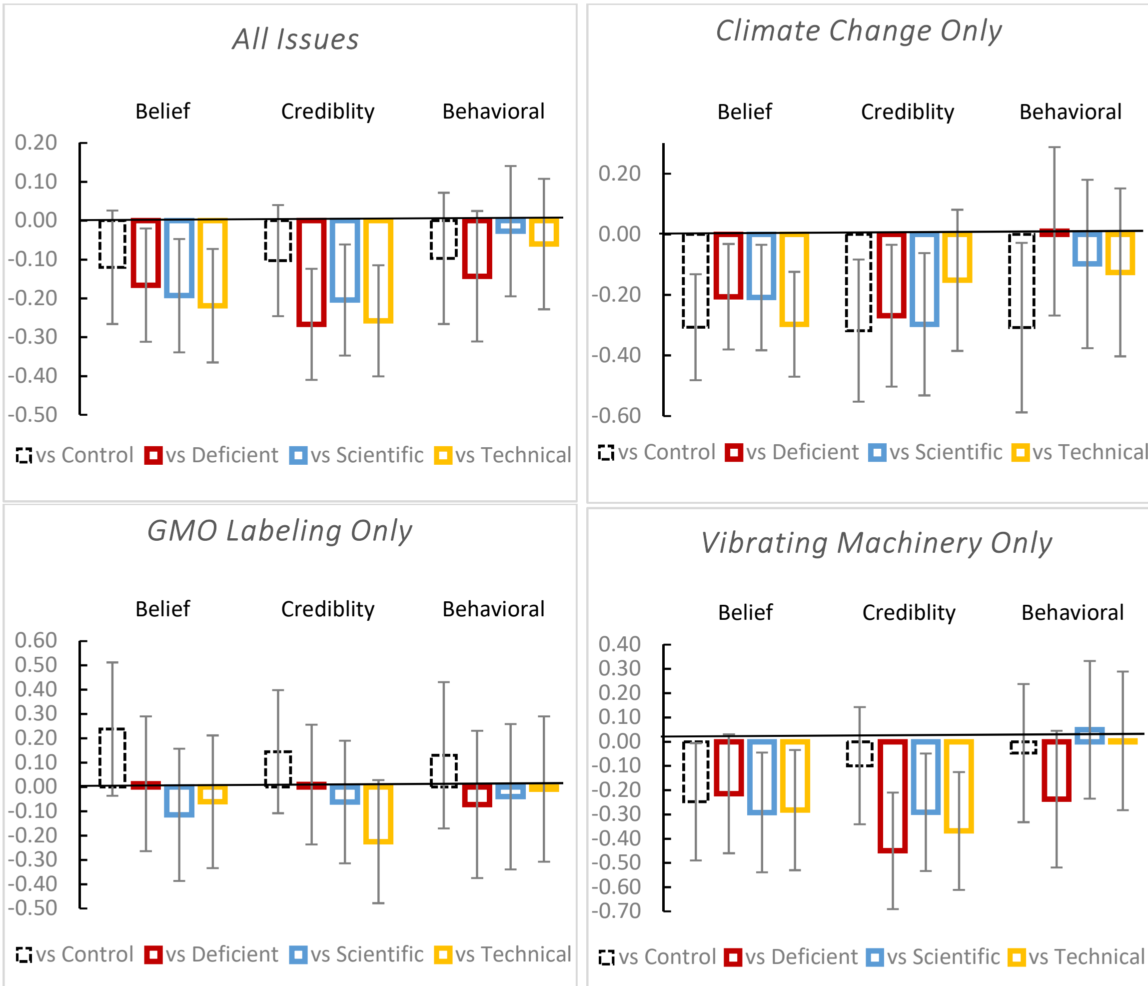


Figure 15. Comparing the consensus uncertainty marginal means of DVs to those in each other frame type.

Note. Values displayed are differences between the estimated marginal mean of that DV in the consensus uncertainty condition(s) and the estimated marginal mean of that DV in another frame type condition(s). Values are marginal means (and 95% confidence intervals) in non-standardized units, reflecting points on the 7-point mean scales measuring each attitudinal outcome variable. All values produced by MANCOVAs controlling for education, deference to science, prior issue position, collectivist worldview, egalitarian worldview, age, and media consumption.

Figure 15 indicates partial support for $H4_{a,b}$, displaying a pattern where claim belief and credibility perceptions are slightly, but significantly, lower in consensus uncertainty conditions than the other frame types – for all issues except GMO labeling. It should be noted, though, that these statistically significant differences have very small effect sizes

(Table 23). In the GMO labeling issue, specifically, none of the outcome variables significantly differed between consensus uncertainty frame conditions and the other frame conditions. Also, there were no instances of behavioral intentions differing between consensus uncertainty frame conditions and other frame conditions.

Table 23

Significance of Interaction Term in MANCOVAs

Issues	*DV*	*df*	*df error*	*F*	*p*	η^2
All Issues	Belief	4	2234	2.69	**.030**	.01
	Cred	4	2234	4.86	**.001**	.01
	BI	4	2234	0.88	.478	.00
Climate Change	Belief	4	730	3.87	**.004**	.02
	Cred	4	730	2.44	**.045**	.01
	BI	4	730	1.64	.162	.01
GMO Labeling	Belief	4	736	1.90	.108	.01
	Cred	4	736	2.20	.067	.01
	BI	4	736	0.52	.724	.00
Vibrating Machinery	Belief	4	742	1.82	.123	.01
	Cred	4	742	4.65	**.001**	.02
	BI	4	742	1.20	.309	.01

Note. Values report the MANCOVA test results that specify the interaction term. Tests controlling for education, deference to science, prior issue position, collectivist worldview, egalitarian worldview, age, and media consumption. η^2=partial eta-squared; Belief=claim belief; Cred=credibility; BI=behavioral intentions; All=all issues combined; CC=climate change; GMO=gmo labeling; VM=vibrating machinery.

Informing RQ1, Figure 16 displays the marginal means of each DV in each of the five framing conditions, with 95% confidence intervals. Like Figure 15, Figure 16 first reports these mean differences that emerge with all issues combined (top left), and then within each issue separately. The values of any Y_i displayed in Figure 16 are centered to the full-sample mean of that Y_i, which is the x-axis (y=0.00).

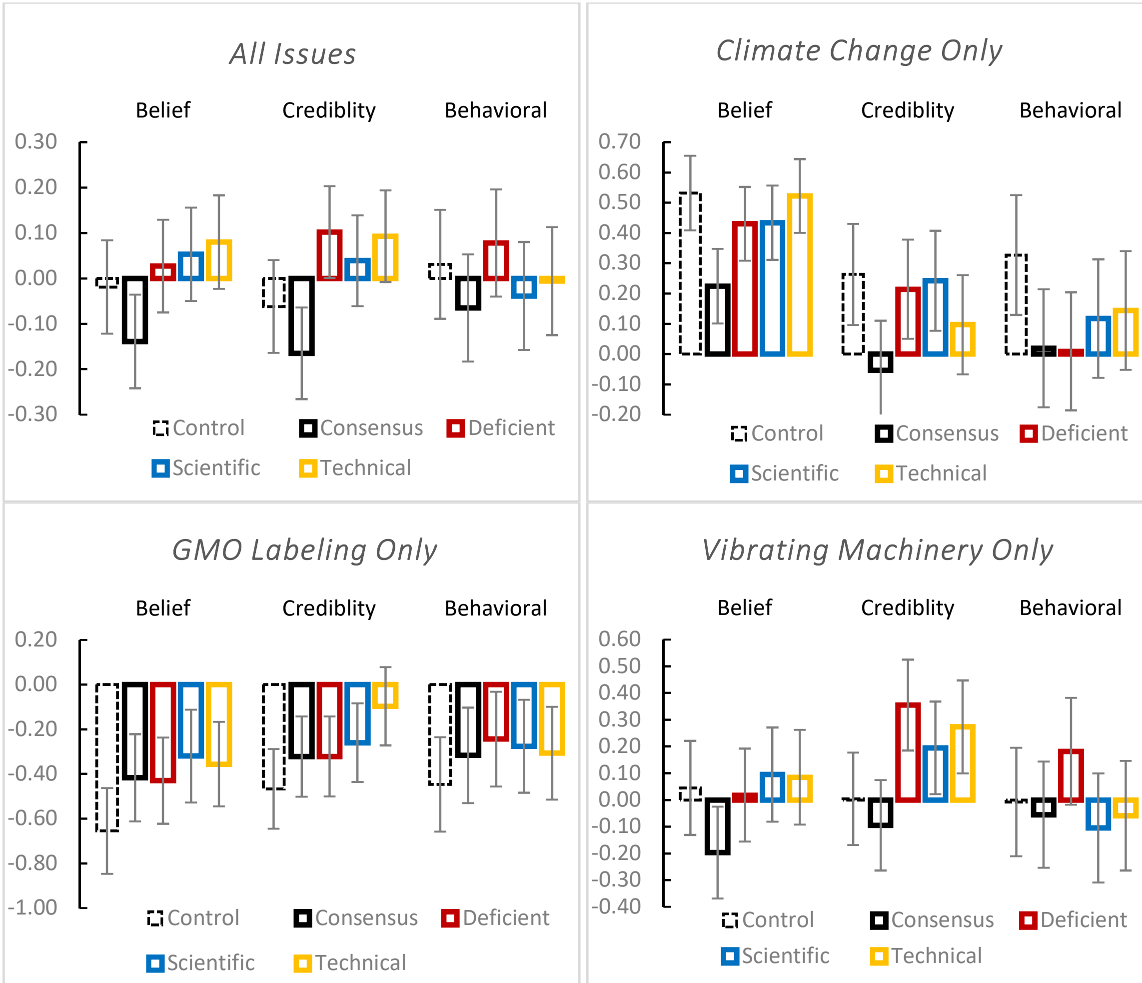


Figure 16. Estimated marginal means and confidence intervals of DVs across frame types and issues.

Note: Values are estimated marginal means (and 95% confidence intervals) in non-standardized units, reflecting points on the 7-point mean scales measuring each attitudinal outcome variable. All values produced by MANCOVAs controlling for education, deference to science, prior issue position, collectivist worldview, egalitarian worldview, age, and media consumption.

Similar to the findings reported in Figure 15 regarding H4, Figure 16 and Table 23 demonstrate that within each issue (and thus also across issues) participants' reported behavioral intentions do not differ significantly across frame conditions. However, for both claim beliefs and perceive credibility, there are significant differences between some conditions in some issues (Table 23). The only pattern with consistency is that of lower claim beliefs and lower credibility perceptions in consensus uncertainty conditions compared to all other conditions (specifically in climate change and vibrating machinery conditions) – that is, what was reported above regarding H4. To answer RQ1, it is adequate to summarize that these data do not exhibit convincing evidence of differences in claim belief and/or credibility perceptions between any pairs of conditions that do not include consensus uncertainty.

When separated by topic, there are small but significant differences between the consensus uncertainty condition and the control condition in levels of both claim belief and credibility perceptions, such that these attitudes *are significantly lower* in the consensus uncertainty condition (Figure 15; top left). No such differences were evidenced in the GMO or vibrating machinery conditions (Table 23).

These findings indicate partial support for H4, such that in the context of climate change – the most polarized and partisan of these three issues – consensus uncertainty frames significantly and negatively affect claim beliefs and credibility perceptions. However, this interpretation of these findings must be considered tentative. This is because, over the span of many tests of mean differences with a "significance" threshold of p=.05, it is probable that there will be a few significant results that are simply due to chance. Aside from replication tests in independent samples, two ways to separate the proverbial wheat from the chaff – and therefore to inform H4 and RQ1 – are to look for evidence of a *cohesive pattern* of findings that emerges across the many tests and, second, to apply relevant theoretical reasoning to the individual significant finding(s) and/or to any emergent pattern.

The results of these tests *do* demonstrate a pattern where exactly one uncertainty frame type is likely to be associated with reported levels of claim belief and credibility perceptions that are slightly different than those associated with the rest of the frame types. Specifically, consensus uncertainty tends to be associated with slightly lower claim belief and lower credibility. Interestingly, this effect was decidedly absent in the GMO labeling conditions. Further discussion and interpretations of these – and other – findings are offered in Chapter 5.

4.5.3. H5 and RQ2: Interaction between prior issue position and frame type. H5 predicted that the motivated reasoning effect – that is, the relationship between prior issue position (M) and the attitudinal outcome variables ($Y_{1\text{-}3}$) – would be strongest in the consensus uncertainty conditions. RQ1 asked how the M→$Y_{1\text{-}3}$ relationships in the other frame type conditions compare with each other.

As described in Section 3.7.2, H5 and RQ2 were informed by using PROCESS to fit the data to Model 14 (Figure 7), such that the effect of X_2 (with X_1 as a covariate) on each Y_i is mediated by M (prior issue position) and the M→Y_i relationship is moderated by levels of Z (frame type). These tests were conducted within each issue separately – that is, three tests of the effects on $Y_{1\text{-}3}$ in climate change conditions, three tests of the effects in GMO labeling conditions, and three tests of the effects in vibrating machinery conditions. In each test, uncertainty frame type was included as a multi-categorical variable that moderates the relationship between M and Y_i. The PROCESS output provided the significance of the overall interaction terms (M*Y_i), which are reported in Table 24. When this omnibus test is significant, it indicates that at least one of the pair-wise comparisons between the five levels of frame type evidences a significant difference. Nonsignificant test statistics indicate that the M→Y_i relationship is not significantly different in any pair-wise combination of the five levels of frame type in that issue for that Y.

Importantly, these PROCESS tests *also* provided simple slopes that specify the beta coefficient for each M→Y_i relationship. Like with H3, these coefficients are standardized because the mean scales were standardized for these analyses (M=0; SD=1). Pair-wise comparisons of these standardized coefficients with z-tests (Paternoster et al., 1998) inform H5 and RQ2 by identifying how the effect of M on Y_i compares across individual combinations of frame types and issues.

Table 24

Overall Interaction Effects of $M*Z_{1-5} \rightarrow Y_i$

	Model ($X_2 \rightarrow M*Z \rightarrow Y_i$)					β range	M*Z Interaction		
	R^2	F	$df1$	$df2$	p	$\lvert\beta_{max}-\beta_{min}\rvert$	ΔR^2	F	p
CC									
Belief	.78	142.79	18	724	**.000**	.039	.00	0.21	.934
Credibility	.51	42.30	18	724	**.000**	.315	.01	2.88	**.022**
Behavioral	.34	25.60	18	724	**.000**	.099	.00	0.42	.793
GMO									
Belief	.25	12.56	18	730	**.000**	.120	.00	0.34	.848
Credibility	.29	17.32	18	730	**.000**	.147	.00	0.60	.661
Behavioral	.25	15.08	18	730	**.000**	.215	.01	1.27	.280
VM									
Belief	.45	33.85	18	736	**.000**	.068	.00	0.25	.909
Credibility	.37	28.17	18	736	**.000**	.107	.00	0.40	.809
Behavioral	.35	27.60	18	736	**.000**	.129	.00	0.69	.599

Note. Each row indicates a separate model test that includes 5 levels of uncertainty frame condition as a multi-categorical moderator of $M \rightarrow Y_i$. Each test used egalitarian worldview as the exogenous predictor (X), prior issue position as the mediator (M), and controlled for collectivist worldview, education, age, deference to science, news media consumption, and the two Y variables not tested in the model.
CC = climate change conditions; GMO = GMO labeling conditions; VM = vibrating machinery conditions; R^2 = variance in Y_i explained by the model; df = degrees of freedom; β_{max}-β_{min} = absolute value of the difference between the largest and smallest β values found in the five $M \rightarrow Y_i$ paths (one per uncertainty frame condition) included in that row; ΔR^2 = change in R^2 from the interaction term.

Table 24 clearly displays that these data do *not* indicate interaction effects between prior issue position and frame type when predicting claim belief, credibility perceptions, or behavioral intentions. That is, the motivated reasoning effects found in the test of H1 do *not* differ across uncertainty frame types. For greater specificity, Appendix E reports the results of t-tests comparing the β coefficient of $M \rightarrow Y_i$ in every possible pair-wise combination of the five levels of Z (frame type), for each Y and within each issue. Only two of the 90 possible comparisons are significantly different, which could be explained simply by chance. Overall – as a result of this robust and rigorous test with a large representative sample of participants who exhibited awareness of, and sensitivity to, the frame type manipulations –

we can confidently say that these data indicate that the M→Y_i effect is *not* different across uncertainty frame conditions.

These results must be interpreted in the context of the findings of the manipulation check and with those from H1, H4, and RQ1. In sum, although individuals tended to recognize and correctly identify the existence of distinct types of scientists' uncertainties in the reports of scientific evidence, and even though prior issue position predicts the attitudinal outcome variables, it seems clear that these uncertainty portrayals do *not* have a significant effect on how closely attitudinal responses fall in line with prior opinion. However, this does not mean that the uncertainty types do not have different effects on attitudinal responses. Specifically, the findings of the tests of H4 and RQ1 indicate that portrayals of consensus uncertainty (in climate change and vibrating machinery) sometimes *do* result in lower claim belief and credibility. Thus, the failure to reject the null hypothesis of H5 means that the regression slopes of M→Y are parallel across the different levels of Z (frame type), with consensus uncertainty having a different intercept than the other frame types (for belief and credibility in climate change and vibrating machinery). For example, the degree to which consensus uncertainty decreases attitudinal support is equal across the prior issue position spectrum. This has both discouraging and encouraging implications for science communication practice. These implications, and others, are discussed in Chapter 5.

4.5.4. H6: Control conditions compared to all uncertainty conditions. H6 predicted that the relationships between prior issue position (M) and the attitudinal outcome variables ($Y_{1\text{-}3}$) – that is, the motivated reasoning effect – would be stronger in conditions where individuals were exposed to any uncertainty frame, compared to conditions where individuals were not. To test this hypothesis, first, two SEM models were estimated – one with data from all of the uncertainty frame conditions (all types and issues), and one with data from all of the control conditions (all issues). Both demonstrated good fit to the data (Table 25). Then, z-tests were used to compare the standardized β coefficients of each M→Y_i in each model, using the formula recommended by Paternoster et al. (1998). The results of the tests are reported in Table 26.

Table 25

Fit Indices of Uncertainty-Only SEM and Control-Only SEM

Issues	χ^2	*df*	*p*	*RMSEA*	*RMSEA 90% CI*	CFI	SRMR
All Uncertainty Conditions	3863.03	586	.000	.056	[.054, .057]	.91	.065
All Control Conditions	1599.18	586	.000	.062	[.059, .066]	.89	.077

Note. $\Delta\chi^2$=change in chi-square test statistic from previous model; χ^2 = chi-square test of model fit; *df* = degrees of freedom; RMSEA = root-mean-square error of approximation; CI = confidence interval; SRMR = standardized root-mean-square residual.

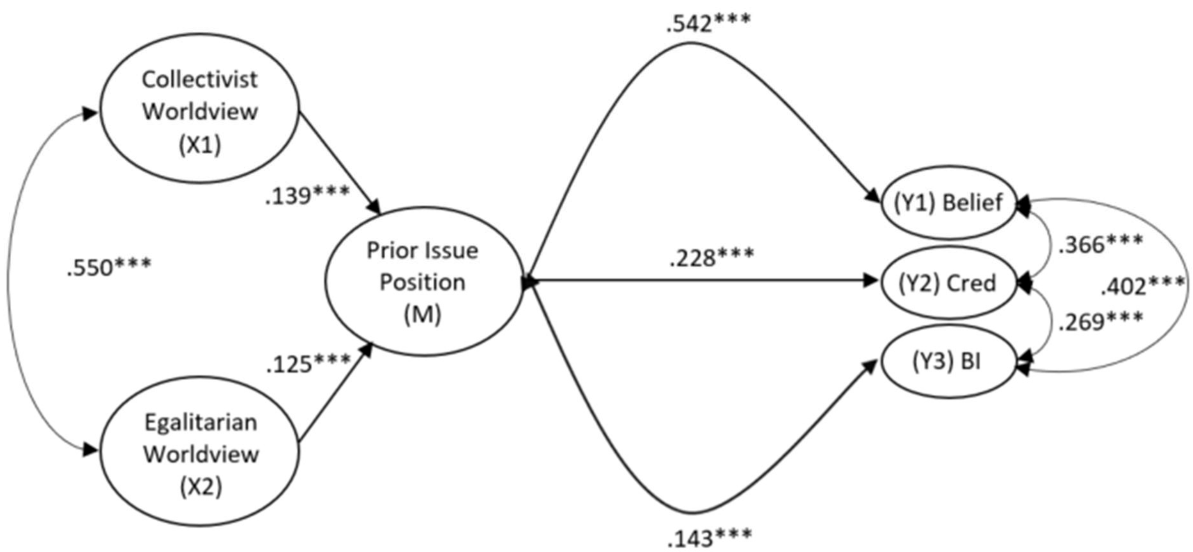


Figure 17. SEM Path Model of All Uncertainty Conditions Combined

Note: Standardized path coefficients; ***= *p*<.001; hidden covariates in the model are education, age, deference to science, and news media consumption. A full model displaying all direct and indirect paths and their standardized coefficients is available in Appendix D.

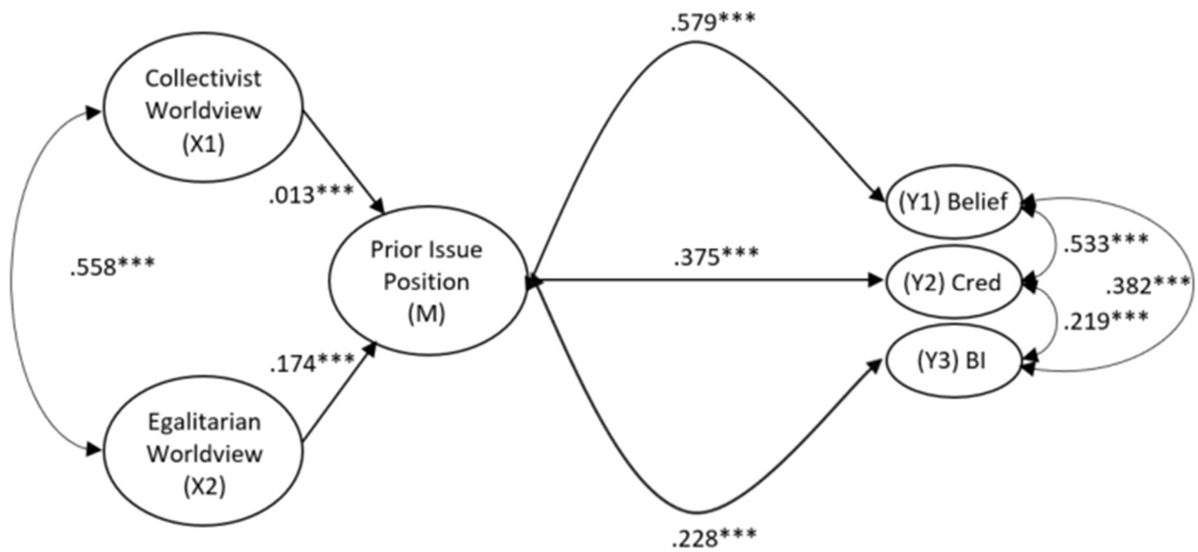


Figure 18. SEM Path Model of All Control Conditions Combined

Note: Standardized path coefficients; ***= $p<.001$; hidden covariates in the model are education, age, deference to science, and news media consumption. A full model displaying all direct and indirect paths and their standardized coefficients is available in Appendix D.

Table 26

Test of the Moderating Effect of Any Uncertainty Frames vs Control

	β	*SE*	*p*	*n*		
Claim Belief (Y_1)						
Uncertainty Conditions	.542	.021	.000	1804		
Control Conditions	.579	.041	.000	443	*z*	*p*
Difference	-.037	.046	-	2247	-0.80	.212
Credibility Perceptions (Y_2)						
Uncertainty Conditions	.228	.025	.000	1804		
Control Conditions	.375	.048	.000	443	*z*	*p*
Difference	-.147	.054	-	2247	-2.72	**.003**
Behavioral Intentions (Y_3)						
Uncertainty Conditions	.143	.026	.000	1804		
Control Conditions	.228	.057	.000	443	*z*	*p*
Difference	-.085	.063	-	2247	-1.36	.087

Note: β = standardized path coefficient indicating effect of prior issue position (M) on Y_i in the SEM estimated from the data of the specified group of experimental conditions; *SE* = standard error of β; *z* = test statistic from z-test of the difference between the βs of the $M \rightarrow Y_i$ paths in the two models.

Table 26 indicates that the motivated reasoning effect ($M \rightarrow Y_i$) in uncertainty frame conditions is *not* stronger than in control conditions. Specifically, there is no significant difference between uncertainty frame conditions and control conditions in terms of the effect of prior issue position on claim belief or on behavioral intentions. There *is* a small difference in their respective effects on credibility perceptions. However, this difference is in the opposite direction of the hypothesis, such that the relationship between prior issue position and credibility perceptions is slightly stronger in the control conditions. Thus, these data and test results do not support H6. This is not surprising, given the results of the tests of H5 and RQ2, which already presented very convincing evidence that $M \rightarrow Y_i$ does not differ in response to different uncertainty frames.

Table 27

Hypotheses, Research Questions, and The Results of their Respective Tests

	Hypotheses and Research Questions	Results
H1	Prior issue position predicts the outcome variables.	Supported (partial)
$H2_{a,b,c}$	In climate change conditions, worldview predicts prior issue position, and prior issue position mediates the effect of worldview on the outcome variables.	Supported (partial)
H3	The relationship between worldview and prior issue position is dependent on the issue.	Supported
RQ1 $H4_{a,b}$	How the means of the outcome variables compare across the five uncertainty frame conditions, when controlling for relevant attitudinal priors.	Supported (partial)
H5	The effect of prior issue position on the outcome variables is strongest in consensus uncertainty, relative to the other uncertainty frame types.	Not Supported
RQ2	Interaction effect of uncertainty frame type condition on relationship between prior issue position and the outcome variables.	None Significant
H6	The motivated reasoning effect (H1) will be stronger in conditions with an uncertainty frame than in conditions without.	Not Supported

4.6 Summary

Together, the results of these many tests accomplished four goals. First, to validate the design and implementation of this study – which is evidenced by the manipulation check results and the reliability and validity of the measures. Second, to test the fit of the data to a theoretical model linking worldview, prior issue position, and attitudinal responses to uncertainty-framed science. Third, to explore whether (and to what degree) attitudinal responses are affected differently by different types of uncertainty frame portrayals. Finally, fourth, to determine whether the motivated reasoning effect (i.e., the effect of prior issue position on the attitudinal outcome variables) differs across uncertainty frame types.

The strength of the first two of these steps provided a foundation that secured confidence in the findings associated with the third and fourth goals. While H5 and H6 were not supported, these non-significant results produced by this rigorous test hold important, practical implications for science communication research and practice. These, and other, implications are discussed at length in Chapter 5, along with detailed explanations and interpretations of the findings of this dissertation.

Chapter 5: Discussion, Limitations, and Future Directions

5.1 The Context and Rationale

Uncertainty is native to science and to wholly accurate science communication (Popper, 1959; Shanteau, 2000; Stocking, 2010). However, the uncertainties that are inevitable in individual findings of science and in larger processes of science are often not clearly communicated to the public. Instead, many public-facing science communicators purposefully avoid discussing the uncertainties that are attached to the science they communicate – often out of fear of adverse effects of those uncertainty frames (Ebeling, 2008; Retzbach & Maier, 2015; Stocking, 1999). To date, it has remained unclear whether these fears are well-founded. Even the social scientists who study public understanding of science and science communication often lack a clear or cohesive understanding of what the effects of uncertainty frames are. Ironically, this is in part due to consensus, deficient, scientific, and technical uncertainties.

At first glance, it appears that the uncertainty about uncertainty has been caused by contradictory empirical findings – with some studies finding that uncertainty frames are associated with greater attitudinal support for a claim, other studies finding an association with less attitudinal support, and still others finding no effect (Tables 4-5). However, this dissertation's conceptual explication of distinct uncertainty types enabled a more nuanced interpretation of the extant empirical literature. Specifically, that frames of *consensus uncertainty* have been associated with *none* of the reported findings of positive effects of uncertainty frames – only negative effects and nonsignificant effects. Conversely, *technical uncertainty* and *scientific uncertainty* frames have been associated with positive and nonsignificant effects. *Deficient uncertainty* has not been a focus of the extant empirical literature.

The explication of the nature of these four types of uncertainty frames, and the consequent revised organization of the literature, which reveals more consistent results within each type of uncertainty, are unique and valuable contributions of this dissertation. Still, though, Tables 4 and 5 did not answer the core question of the (different) effects of (different) uncertainty frames in science communication. This is because the collection of findings reported in Tables 4 and 5 are the product of disparate methods, issue contexts, concepts, and measures – all of which are confounding factors that render meta-analytic conclusions impossible. The literature lacked a controlled experiment that compared the effects of each uncertainty frame type within one consistent methodology.

All of these factors together created a moment in science communication research where progress toward the answers to a question with universal importance and tangible applications was obfuscated despite many uncoordinated efforts occurring within disciplinary silos. Therefore, this dissertation was an effort to move this field of research forward – providing a rigorous and robust set of findings that inform the relative effects of different types of uncertainty frames in science communication and explore how these effects might differ across individual and contextual variables.

5.2 The Priority of Method

Much of this dissertation is exploratory. That is, it breaks new ground with the explication of new constructs and asks research questions about new relationships involving those new constructs. The few hypotheses were based on existing general theory and a small amount of related empirical research. Therefore, it was apparent early on in this process that many of the key findings of interest to this dissertation would not be able to be compared to substantial precedent set by prior research. Because of this, it was of utmost importance to construct a methodological approach that was a) derived from prior theory and research enough to provide clear tests of justified hypotheses, b) robust enough to enable full confidence that any effects that do exist would very likely be detected, and c) rigorous enough that it would be very unlikely that spurious or confounding explanations would be the cause of any observed effects or non-effects. This is reflected in the extensive and meticulous care given to designing and validating the methodological components and procedures of this dissertation, as well as the extensive and meticulous care given to describing those efforts to readers of this manuscript.

By testing the effects of uncertainty in each of three distinct issues (that each differed in nature and degree of polarization in public opinion), this dissertation enabled preliminary evidence as to whether each of the (non)significant findings might be an artifact of one unique issue context or – instead – whether they might be generalizable to multiple, diverse issue contexts. By structuring each issue's claim to refer to risk to farmers, the potential for confounding influences across issues is reduced. By using a large national sample that approximated census levels of education, age, and gender – and contained equal proportions of self-identified liberals and conservatives – this dissertation instills confidence that the results are not unique to a niche, non-representative subgroup of American adults.

By filtering the sample to only include those who agreed to read the whole news article, viewed the news article for a reasonable amount of time, passed two comprehension checks, completed the survey in reasonable time, and exhibited no evidence of straight-lining response patterns, this dissertation maximized the likelihood that all responses (and, thus, the results of the analyses) indicate genuine personal opinions given in conjunction with careful consideration of the stimulus. By applying a rigorous sequence of reliability analysis, exploratory factor analysis, and confirmatory factor analysis in the development of the measures and the measurement model, this dissertation demonstrates clear evidence that the multi-item scale measures have strong convergent and discriminant validity. By first establishing the manipulation check and the basic model structure, this dissertation builds a foundation of confidence in conceptual, theoretical, and methodological validity upon which to base interpretations of the later tests of interaction effects.

5.3 The Results

5.3.1. The manipulation check. A manipulation check can be the cornerstone of the interpretations of the results of the later hypothesis tests. That is, the purpose of the manipulation check is to effectively eliminate or mitigate the possibility that the (non)significance of the test results is due to a manipulation or treatment that is too weak, too strong, too blatant, or too subtle. For this study, when one has confidence in the manipulation check, this enables confidence that the observed test results are in fact the effect of the manipulation, and not, instead, the result of a manipulation that went unnoticed (perceptually) or was misinterpreted (conceptually).

In this dissertation, the manipulations were news portrayals of scientists having different types of uncertainties about new research. Naturally, the manipulation check was measures of participants' perceptions of the different types of uncertainties that scientists have about that new research. In short – as reported in Section 4.3.2 – the results of the manipulation check demonstrated that participants demonstrated awareness and understanding of the manipulations. This is evidenced by the fact that in each uncertainty frame condition (e.g., consensus uncertainty condition, deficient uncertainty condition) participants' ratings of the corresponding item on the external uncertainty type (EUtype) scale (e.g., consensus uncertainty item, deficient uncertainty item) was significantly different from the ratings of that item in the control condition (Figure 10). To summarize, people reported believing that scientists had significantly more _*type*_ uncertainty when they read an article that said scientists indeed had _*type*_ uncertainty, compared to when they read an article that didn't mention uncertainty at all, *and also compared to* when they read an article that said scientists had some other uncertainty type.

One natural follow-up consideration is whether the reported EUtype item scores were also different from the scores of the same item in *other uncertainty frame conditions*. For example, it is clear that perceptions of scientists' consensus uncertainty were greater in the consensus uncertainty condition than in the control condition. But were perceptions of consensus uncertainty in the consensus uncertainty condition *also* greater than perceptions of consensus uncertainty in the technical, scientific, and deficient uncertainty conditions? The answer is "yes" – this was true within every issue and overall across issues for consensus, deficient, and scientific uncertainty – such that the differences between their item scores in their corresponding frame type condition and their item scores in any and all other frame type conditions were all significant. Like with the other uncertainty type items/conditions, the scores of the technical uncertainty item *were most extreme* (most reported technical uncertainty) in the technical uncertainty condition, as expected, and these levels were significantly different than the control and consensus conditions. However, participants also reported moderate levels of external (scientists') technical uncertainty in the deficient and scientific uncertainty frame conditions, and these estimated marginal means were *not quite* significantly different from those in the technical uncertainty condition. That is, the scores on the technical uncertainty item in the technical uncertainty condition did not (quite) differ significantly from the technical uncertainty item scores in the deficient and scientific uncertainty conditions. This is understandable, since the technical uncertainty item ("…these findings are rough estimates that could vary by a large margin…") is also often salient to situations of deficient uncertainty and scientific uncertainty.

Overall, this indicates that all uncertainty frame type manipulations are noticed and correctly understood as representing the uncertainties held by scientists. In addition, participants perceived consensus, deficient, and scientific uncertainty in scientists as each being distinct from portrayals of all other types and from portrayals of no uncertainty. Participants perceived technical uncertainty as being distinct from no uncertainty, which approached significance. In sum, these findings indicate that participants were indeed taking notice of the existence of uncertainty frames portrayed in the stimuli and even were correctly parsing the semantic differences between individual uncertainty types.

5.3.2. The structural model. One of the central purposes of testing the structure of the conceptual model with SEM was to specify the motivated reasoning effects (H1) within and across issues while simultaneously accounting for all the other relevant variables and their respective errors. Across all three issues combined (Figure 11), this hypothesis was supported – with stronger effects of prior issue position on claim belief and credibility than on behavioral intentions (which reflects most research on attitude-behavior relationships). When the SEM is performed within each issue separately, interesting but expected differences emerge between issues (Figures 12-14).

First, overall, the motivated reasoning effect is clearly stronger for climate change than for the other two issues. Specifically, all paths between prior issue position and the outcome variables are β>.400, and the effect on claim belief is β=.811. This means that in climate change conditions, every 1 SD increase in prior general opinion about the broad issue of climate change resulted in a 0.8 SD increase in beliefs in the likelihood and severity of the risk to farmers that was presented as a new scientific finding in the news article, and a 0.4 SD increase in both perceived credibility of the scientists and intentions to engage in behaviors that would help mitigate the consequences of this threat. This model controls for education, general deference to science, and worldview – among other covariates – leaving motivated reasoning as the primary explanation for these strong effects.

The strength of motivated reasoning in response to science regarding climate change is not surprising, as prior empirical research cited above has demonstrated that many people have strong positive and negative opinions about climate change (generally), that in turn cause motivated reasoning in response to (specific) climate change information. What is more interesting is the observed difference in the motivated reasoning effects between issues. Specifically, these effects are much smaller in the models of GMO labeling and vibrating machinery opinion. In these two issue contexts, the motivated reasoning effect of prior issue position on behavioral intentions is not significant. The effect on credibility is significant in the GMO labeling conditions, but is less than half of the size (β=.200) of the effect on credibility in climate change (β=.430).

There are a few possible inferences that could be drawn from these findings. One is that regardless of issue and the distribution of prior general issue positions, there is a significant motivated reasoning effect of prior issue position on (at least these three) specific claim beliefs. This interpretation is reasonable – given that specific beliefs about a claim have more conceptual similarity to general beliefs about the broader issue than, say, intentions to engage in specific behaviors. However, this interpretation should be considered in conjunction with the caveat that this relationship could be partly an artifact of survey designs where responses to measures of general prior issue position can prime similar responses to the subsequent measures of agreement with a scientific claim about that issue.

A second possible inference from these findings is that the climate change issue has some characteristic that causes stronger motivated reasoning effects. It is understandable that the vibrating machinery model would show smaller or non-significant motivated reasoning effects, given that most people have weak or non-existent prior opinions on this issue and thus their tendencies to believe the claim, perceive the scientists as credible, and be willing to help would be not significantly influenced by those weak or non-existent prior opinions. A more difficult puzzle is that the GMO labeling model displays much weaker motivated reasoning effects than the climate change model, even though they exhibit similar variance in prior issue position. One explanation is that the partisan nature of the climate change issue makes it more value-laden and identity-salient, while prior opinions about the issue of GMOs are (among other differences) less related to identity, culture, worldview, and values and are more of a cerebral, calculated opinion. If there is indeed a significant difference in the roots and nature of prior opinions about these two general issues, this could impact the degree to which individuals are open to processing new evidence independent from their prior stance on the issue.

Another potential explanation for the strong association between prior issue position and the attitudinal outcome variables in climate change is the skewed distribution of climate change prior opinions in the sample (Figure 8). Like the U.S. population at large, there are quite a few more people with strongly or partially supportive opinions toward the scientific consensus on climate change than there are people with strongly or partially oppositional opinions. This is not the case for prior opinions on the general issues of GMOs and occupational hazards of farming (i.e., vibrating machinery conditions) (Figure 8), which include a greater proportion of moderate opinions. If it is the case that strong positive and negative opinions are more likely to be associated with motivated reasoning, then it is likely that the issue with the most extreme (rather than moderate) opinions (i.e., climate change) has the strongest motivated reasoning effects.

A third possible inference is that the items selected to comprise the behavioral intentions factor do not have a strong causal link with prior issue position. With the exception of the motivated reasoning effect in climate change, behavioral intentions are not affected by prior issue position. Further, as will be discussed in Section 5.3.4, the mean scale of behavioral intentions was not differentially affected by uncertainty frame manipulations. Thus, while the behavioral intentions items demonstrated convergent and discriminant validity, it is possible that they either a) lack construct validity or b) are measuring types of behaviors that are simply not affected by prior issue position and/or frame type manipulations. Of course, any explanation would have to also reconcile the significant effect found in climate change. One reasonable explanation is that these items comprising the behavioral intentions factor (donating, voting for a tax, voting for a tax break) are seen as logical consequents of general issue support in climate change, but not in GMO labeling or vibrating machinery. This could be because real-world instances of these kinds of behaviors are much more common and familiar in the context of climate change than in GMOs and vibrating machinery. That is, in climate change, *these particular* behavioral intentions are an extension of supportive attitudes toward the issue/claim, while in GMOs and vibrating machinery, *these particular* behavioral intentions are more closely associated with individuals' broader attitudes toward helping other people, regardless of the issue. This is likely, because Appendix E shows that both the GMO labeling model and the vibrating machinery model show significant *residual direct effects* of collectivist worldview on behavioral intentions (GMO β=.126; VM β=.145) and of egalitarian worldview on behavioral intentions (GMO β=.259; VM β=.468). But in these two models, both worldview dimensions *have no indirect effects* on behavioral intentions that are explained by prior issue position.

The assorted SEM models (Figures 12-14; Appendix E) reveal further interesting insights about how the effects of each worldview dimension vary across issues. In sum, the collectivist dimension of worldview does not predict prior issue position in any issue. In each issue, it has a small residual direct effect on behavioral intentions, but no direct effect on claim belief or credibility, and (again) no indirect effect through prior issue position. However, the egalitarian dimension of worldview *does* – in each issue – have a significant effect on prior issue position, to varying degrees and directions.

Interestingly, in every instance where an indirect effect of egalitarian worldview (X_2) on an attitudinal outcome variable (e.g., behavioral intentions) via prior issue position was nonsignificant (all of such instances occurred in the GMO and vibrating machinery models), there was instead a significant direct effect of egalitarian worldview on that outcome variable (Appendix E). Together, these findings indicate that the egalitarian worldview was much more influential than collectivist worldview in terms of their effects on prior attitudes and attitudinal responses to the stimuli. This is *not* easily attributed to inadequate construct validity in the measure of collectivist worldview, because the collectivist worldview measure has been frequently validated and employed successfully in prior research – and in this study, the relationship between collectivist and egalitarian worldview dimensions is robust as expected (ranging from β=.446 to β=.659). Also, an independent samples t-test comparing the mean levels of the individualist-collectivist mean scale scores between self-identified conservatives (N=1124; M=2.93) and self-identified liberals (N=1123; M=3.76) indicates a significant difference in the expected direction, $t(2245)=18.03$, $p=.000$, such that liberals are more collectivist than conservatives. It is not clear at this point why collectivist worldview did not play a more important role in the model. It may be that collectivist worldview just happened to *not* be relevant to prior general opinions on these three particular issues and *also not* relevant to beliefs, credibility perceptions, and behavioral intentions regarding these particular claims, but is still relevant to prior issue positions, beliefs, credibility, and behavioral intentions in other science issues and claims. Future research should explore the degree to which these nonsignificant associations can extrapolate to other contexts, to other measures of these variables, and to other variables entirely.

Also, it is interesting to note that although egalitarian worldview had a *negative* relationship with prior issue position about GMOs (indicating that people with more egalitarian worldviews were less in agreement with the scientific consensus on GMOs), there were still small but significant *positive* direct relationships between egalitarian worldview and perceived credibility of the scientists and between egalitarian worldview and behavioral intentions. This means that even though high-egalitarians tended to think *less* positively of GMOs than low-egalitarians do, they still tended to think *more* positively of the scientists doing GMO research than low-egalitarians did. The same was true for behavioral intentions.

Strong egalitarian worldview may have been associated with *lower* support for the use of GMOs in general, but it was associated with *greater* intentions to enact behaviors to support those negatively affected by GMO labeling laws.

5.3.3. The indirect and interaction effects in the climate change model. $H2_a$ was supported, in that prior issue position on climate change mediates the relationship between egalitarian worldview and each of the three attitudinal outcome variables. This was partial mediation, as there were smaller but significant and positive residual direct effects ($H2_c$) of egalitarian worldview on each of the outcome variables. $H2_a$ was only partially supported because this mediation effect was not found with collectivist worldview. This is because, in partial support for $H2_a$, only egalitarian worldview had a positive effect on prior issue position on climate change, while collectivist worldview had no significant effect.

The test of H3 added more confidence in the overall structural model, and provided a specific test of the differences between the issues of climate change, GMO labeling, and vibrating machinery in terms of the relationships between the worldview dimensions and prior issue position. Specifically, the findings indicated that the relationships between worldview dimensions and prior issue position were significantly stronger in climate change conditions than in GMO or vibrating machinery conditions.

Overall, these findings validate the core pieces of the conceptual model. Also, they indicate the importance of measuring and modeling prior issue position when predicting attitudinal response variables in the context of climate change, instead of just relying on worldview as a proxy for prior opinion, and *especially instead of* relying on a combined measure of collectivist and egalitarian worldview as a proxy for prior opinion. That is, certainly in the context of GMOs and occupational hazards of farming – but also in the context of climate change – the motivated reasoning effects that are of interest to this dissertation are likely better modeled as the influence of prior general issue position, rather than as the influence of ideological worldview.

Having established clear evidence for the validity of the manipulations and of the underlying conceptual model, we can now discuss the conditional effects involving variations in uncertainty frame types.

5.3.4. The comparisons of DV means across frame types. $H4_{a,b}$ predicted that frames of consensus uncertainty would be associated with lower belief certainty, perceived credibility, and behavioral intentions compared to control conditions, to technical uncertainty conditions, and to scientific uncertainty conditions. Figure 15 shows how the estimated marginal means of each variable in the consensus uncertainty condition(s) compare to the estimated marginal means of the same variable in a different frame type conditions. Overall, for all issues combined, and for climate change and vibrating machinery separately, claim belief and credibility perceptions are significantly lower in the consensus uncertainty conditions than in technical, scientific, and deficient uncertainty conditions (providing partial support for $H4_b$). For climate change and vibrating machinery, separately, claim belief is significantly lower in the consensus uncertainty conditions than in the control conditions and for climate change only, credibility is significantly lower (providing tentative partial support for $H4_a$).

One explanation for this is precisely the rationale given for the hypothesis and given by the prior research that has found negative effects of consensus uncertainty. That is, that portrayals of consensus uncertainty have especially negative effects because they introduce the possibility of expert support for both sides, thereby legitimizing and giving credibility to positions of dissent. One other explanation for this observed effect is that it is, in fact, scientific, technical, and deficient uncertainty frames that are exhibiting positive effects, which is why consensus uncertainty's effects appear relatively negative. That is, the potential positive effects on the part of technical, scientific, and deficient uncertainty frames (e.g., increased attitudes of trust and perceptions of transparency, as reviewed in Chapter 2) may be what is causing the observed difference between consensus uncertainty frames and the others. Not, *per se*, the "negative effects" of consensus uncertainty.

One way of determining which (or both) explanation is justified is by investigating the mean levels of these variables in the control conditions. But this seems to vary by issue. That is, in the climate change condition (Table 16), the mean levels of belief and credibility in each frame type indicate that the control condition was just as high (or higher) than any of the uncertainty types. But in the vibrating machinery condition (Table 16), the mean levels of belief and credibility in each frame type indicate that the significant differences between consensus uncertainty and the three other uncertainty frames (deficient, scientific, technical) is because consensus uncertainty tends to be slightly lower than the control condition, and deficient, scientific, and technical tend to be slightly higher than the control condition. These differences are, however, too small to offer confident conclusions.

There are two inferences that we can have confidence about. The first is that in two very different types of issues (in terms of prior opinions), consensus uncertainty frames – in many situations – seem to be associated with the lowest claim beliefs and credibility perceptions. This echoes the existing findings of the negative effects of consensus uncertainty frames (Table 4), and goes beyond them by comparing them to the other uncertainty frames. This is important because, for science communication practice, it is not entirely helpful to *only* know that consensus uncertainty frames have detrimental effects when compared to a control condition of no uncertainty. That is, it is not particularly helpful because science communicators often *do not have the option of* communicating with no uncertainty. In the real world, a "control condition" message to the public can be misleading and therefore harmful in its own way. This makes the results of these comparisons between consensus uncertainty and the other three uncertainty frame types ($H4_b$) especially valuable for public-facing science communicators. It gives science communicators reassurance that not all uncertainty is certain to have negative effects.

The second confident inference is that these data indicate *no* significant differences in the respective effects of scientific, technical, and deficient uncertainty on attitudinal outcome variables of claim belief, credibility perceptions, and behavioral intentions. These three types do have effects that are decidedly more positive than consensus uncertainty and decidedly not different from each other (for any variable in any issue). Also, as a reminder, distinctions between these uncertainty types were clearly made in the manipulation check, such that participants reported beliefs of highest external uncertainty of a particular type precisely when they stimulus contained that type (although, in fairness, this is least true of technical uncertainty). So the finding that attitudinal outcome variables did not vary across conditions of scientific, technical, and deficient uncertainty is *not likely* because participants failed to see a difference between any of these three types of frames. Rather, it is more likely that these three different frames just do not have different effects (on these attitudes, in this study, on these issues, among this sample, etc.).

The most surprising of these is deficient uncertainty. Prior to the hypotheses, I offered a tentative statement that said, in effect, although there is not enough theoretical or empirical evidence to officially hypothesize, that deficient uncertainty will have negative effects akin to consensus uncertainty, I would not at all be surprised if it did. Miles and Frewer (2003) found that individuals in a focus group identified deficient uncertainty as the least desirable type for scientists to have. In a strong rejection of these tentative suspicions, the effects of deficient uncertainty did not differ from those of the types of uncertainty frames that have occasionally been associated with positive effects (scientific and technical). Deficient uncertainty communicates science's current state of ignorance and shortcoming in knowledge. This could be considered a negative thing that reflects poorly on the capability and achievements of science and scientists. However, this could also be considered a positive thing that reflects well on the honesty, transparency, and even ambitions of science and scientists (quite similar to scientific uncertainty). I had not considered this latter interpretation of deficient uncertainty before seeing the results. Further, to reconcile these findings with those of Miles and Frewer (2003), it could be that people list deficient uncertainty as being maximally undesirable but in fact do not react negatively to portrayals of it. Of course, this has especially valuable implications for public-facing science communicators. If it is true that portrayals of deficient uncertainty do *not* result in negative effects on beliefs and credibility, then they should not be hesitant to openly present preliminary findings and new breakthroughs as still having some uncertainties.

There are two truly puzzling findings of these tests. First, behavioral intentions were no different across any frame type conditions within any issue or combinations of issues. That is, these manipulations of uncertainty frame did not appear to affect behavioral intentions, and this lack of effect was constant across issues. However, effects on beliefs and credibility perceptions did emerge. One potential explanation that is well-supported by theory is that changing behaviors is more difficult than (and thus less likely than) changing beliefs (e.g., McGuire, 2012). Another potential explanation that is supported by the findings of the SEM models is that the behavioral intentions measure used in this study was somehow semantically and conceptually disconnected from either the stimulus itself or from the concept it was designed to measure. The answer to this is impossible to determine at this point, so it is important for future research to determine if other measures of behavioral intentions in other contexts are similarly unaffected by variations in uncertainty frame types.

The second puzzling finding is that consensus uncertainty (or any of the frame types) did not have an effect on any outcome variables in the GMO conditions like it did in the climate change and vibrating machinery conditions. Excepting random chance, one potential explanation is that prior opinions about GMO foods were much less supportive of the scientific consensus than were prior opinions about climate change or vibrating machinery. This could affect attitudinal responses in the consensus uncertainty condition (in particular) such that people who were more opposed to GMO foods (and thus supportive of GMO labeling) would be more amenable to a report that repeatedly mentioned that scientists admit they have consensus uncertainty. So, given that the GMO issue had the lowest mean prior opinion score and the largest number of strong opponents, positive responses to consensus uncertainty by this segment of the prior opinion distribution could raise the mean attitudinal response scores in the consensus uncertainty condition of GMO foods. There is (at least) one problem with this explanation, though. If this were the case to a significant degree, then there would be a significant interaction of $M*Z \rightarrow Y_i$ in the GMO conditions involving consensus uncertainty and claim beliefs, and consensus uncertainty and credibility. There are not (Appendix F).

Another possible explanation for this puzzling finding is that individuals expect (or are at least accepting of) more consensus uncertainty about GMO foods than about an issue like climate change where consensus is highly publicized, or an unfamiliar science issue like vibrating machinery where there is no reason for most people to assume anything except scientists being in agreement. The issue of GMO foods is widely known to be controversial and polarized, but the scientific consensus is *not* widely publicized. Therefore, portrayals of disagreement among scientists (i.e., high consensus uncertainty) are expected, rather than a violation of a normative state. This implies that "acceptable" levels of uncertainty vary across issues. This is also argued by Jensen and Hurley (2012) who invoke the theory of motivated information management (Afifi & Weiner, 2004) to explain why uncertainty frame effects vary by issue. This explanation raises some widely-applicable questions that should be addressed by future research: How do expected, desired, and tolerated *levels* and *types* of uncertainty vary across science issues? How do these expectations, desires, and tolerations of *levels* and *types* moderate the effects of uncertainty frames? This field is ripe for growth and will produce actionable recommendations for science communicators.

5.3.5. The interactions of motivated reasoning with frame types. The tests of H5, H6, and RQ2 indicated that the motivated reasoning effects of prior issue position on each attitudinal outcome variable (i.e., $M \rightarrow Y_i$) are *not* meaningfully different between any pairs or combinations of frame types in any issue. This is a very interesting finding, and has several important implications and applications. Also, as demonstrated in detail above, it is important to emphasize that these nonsignificant test results are likely *not* due to participants' inattention in the study generally, inattention to the stimulus or manipulations specifically, or misunderstanding or conflation of those subtle frame type manipulations. This study took many steps to ensure confidence in whatever results the tests produced.

First, researchers and theorists should consider the following. A natural initial critique of the observed lack of differences across frame types is to reference the multiple studies that have, individually and separately, found different effects for different frame types, respectively (Tables 4-5). However, one must keep in mind that one of the reasons for finding non-significant differences in a controlled experiment that only varied frame type (within each issue) is precisely because *it was a controlled experiment that only varied frame type.* For example, imagine if Study A tests the effects of consensus uncertainty about an issue/claim in which consensus uncertainty among scientists (and/or portrayals of it) is not normal (e.g., climate change), while Study B tests the effects of technical uncertainty about a different issue/claim in which technical uncertainty by scientists (and/or portrayals of it) *is* normal (e.g., earthquake risk probability). It would not be surprising for the former to have more negative effects and the latter to have more positive effects. This is, in fact, what most of the literature has done to date. However, it is fallacious to assume that these two findings together indicate that it is the difference in these two frame types that is primarily (or even at all) responsible for the observed different effects. That is, despite findings of vastly different effects found in different studies, it may be the case that when all other things are held equal, variations in uncertainty frame type have no differences in effect. It is important to remember that this is precisely why this dissertation's large controlled experiment was necessary and continues to be valuable.

With that said, the lack of significant differences between frame type conditions in the strength of the motivated reasoning relationship indicates several implications for theory and research and several applications for science communication practice, as well as some tempering caveats and considerations. Overall, in response to H5 and RQ2, these data indicate that it is not the case that consensus uncertainty or any other uncertainty type significantly increases or decreases the likelihood for individuals to exercise motivated reasoning in their attitudinal responses to new claims of science. Importantly, this is consistent in all three topics – indicating that the large variations in the nature, strength, and distribution of prior opinions across issues did not change the interaction effects of frame type with prior issue position on the outcome variables to a significant degree.

These findings are consistent with the large body of evidence in the framing literature (e.g., O'Keefe & Jensen, 2007; 2009) that has indicated that framing effects are typically small, are context-specific, and play a very minor role in comparison to the effects of prior beliefs and identity associations. In this study, the results indicate strong, reliable, and predictable motivated reasoning effects that are not significantly different across variations in whether or which uncertainty frames were used. The focus of this dissertation on the nature and effects of uncertainty frames should not be interpreted as a perspective that message manipulations are the most influential forces (or even most important research agenda items) in science communication. Rather, it furthers the trend in framing research that is indicating that frame variations have small effects that emerge in very particular combinations of contexts and individual characteristics.

Still, these findings have simultaneous negative and positive implications for science communication practice. The glass-half-full perspective is that portrayals of uncertainty do not exacerbate motivated reasoning and, thus, do not exacerbate opinion polarization. If these findings are generalizable, science communicators can present a great many of the interesting and necessary uncertainties of science without fear of creating further distrust and opposition in those who already hold oppositional opinions. Opinion divides will not increase (nor decrease) from uncertainty portrayals.

The glass-half-empty perspective is that the observed negative effects of consensus uncertainty (H4 and RQ1) are *not unique to those with oppositional prior opinion*. That is, if we think of the relationship between prior issue position and each outcome variable ($M \rightarrow Y_i$) as a slope (which it is), the effect of consensus uncertainty portrayals (relative to other uncertainty types or no uncertainty) is not to change the slope (i.e., differential effects), but instead to decrease the levels of that outcome variable (Y_i) in everyone equally. In consensus uncertainty conditions, the slope of the motivated reasoning relationship ($M \rightarrow Y_i$) is parallel to, but has a lower intercept than, the other uncertainty frame conditions. This indicates that those with supportive prior attitudes are similarly negatively affected by consensus uncertainty portrayals (about climate change and vibrating machinery, in this study, using these measures, in this sample, etc.).

One should not, though, interpret this evidence as demonstrating that uncertainty frames never exacerbate motivated reasoning in the real world. Despite the manipulation check demonstrating that participants were able to notice and distinguish between the frame type manipulations, it may be the case that much stronger, weaker, more repeated, more ecologically valid, or otherwise different implementations of uncertainty frames are necessary to trigger those interaction effects referenced in H5 and RQ2. It is likely that framing effects are most potent over time, and that the effects of different uncertainty types are most potent when internalized into individuals' subconscious schema and not when engineered as a few short sentences. As mentioned above, there may also be large differences between issues in the degree to which variations in uncertainty frame types affect the motivated reasoning effect. Preliminary evidence also indicates that differing *amounts* of portrayed (consensus) uncertainty have differential effects on attitudes, particularly for those with high trust in science (Chinn et al., 2018). These are all valuable questions for future research. The theoretical rationale of this dissertation should not be rejected on the basis of one study's results. Rather, a position of scientific, deficient, and technical uncertainty should be adopted regarding these preliminary findings of no effect. But, still, future research can build off of this foundation of conceptual explication, rigorous design, robust power, and preliminary findings.

5.4 The Limitations

With every study, there are aspects of theory, design, execution, and analysis that each (and together) limit the interpretability, reliability, and generalizability of the results. With a study that is both exploratory and complex – such as this one – there are many such limitations. In this section, I summarize many of them – although I am sure that there are others that have eluded my attention.

First and foremost, it is not the case that there are necessarily four types of uncertainty (frames), and if there happen to be, it may not be these four. The typology presented here is a valid one, but it is just one of many potential valid categorizations of the broader concepts of uncertainty and uncertainty portrayals. It may be that under a different (whether more or less nuanced) categorization, the results of this study would have been different. For example, consensus uncertainty could be subdivided into "competing opinions by entities" and "conflicting evidence/findings." It may be the case that portrayals of disagreement between experts have different effects than portrayals of mixed evidence in the extant body of research. Or, it could be that it is efficient and appropriate to combine technical, scientific, and deficient uncertainty into one higher-order dimension (e.g., "known uncertainties") and to treat all kinds of consensus uncertainties as the other dimension (e.g., "competitive uncertainties"). This typology would reflect one of the core explanations given in this dissertation for the observed pattern of effects (i.e., that consensus uncertainty is unique in that it provides evidence for multiple competing sides). As such, it would not necessarily be more/less incorrect than the four-piece taxonomy, and the question of the whether it would be more/less useful remains open. In general, it is important for communication scholars to not settle for the definitions and taxonomies that have been invented by others. These are among the most unscientific fixtures in our discipline, and they should be treated with levels of skepticism that they deserve.

Second, the operationalizations of each uncertainty frame in the stimuli are just one set of countless possible variants of valid operationalizations, and are not necessarily the most optimal set (by any standard). In fact, it is not difficult to identify elements of the manipulations that could have confounding effects, including many ways in which the operationalizations of the four uncertainty frame types may be inequivalent in ways other than just the *type* of uncertainty. For example, the technical uncertainty conditions do not use the word "although" to introduce the uncertainty clause in the article sub-heading, while all three of the remaining uncertainty type conditions do. Also, there is no good metric to assess whether the *degree* of uncertainty portrayed and/or *strength/vividness* of the portrayal were equivalent across frame types. Further, future research is needed to investigate whether the position of uncertainty clauses in a sentence, and/or in the article overall, determines effects. An additional consideration is that consensus uncertainty was operationalized by portraying the scientists as admitting that other scientists disagree. This may produce much different responses than if the article directly reported the competing opinion from the disagreeing scientist(s) separately. That is, one person admitting that there is competing evidence might even have *positive* effects on credibility by demonstrating willingness to give full disclosure (similar to deficient, scientific, and technical uncertainty). If it is the case that consensus uncertainty has an especially strong effect on attitudinal responses (relative to other uncertainty types or to no uncertainty), it is very important that future research determine how variants of consensus uncertainty differ in their effects.

Third – for all its careful attention to design – this experiment does not test the effects of uncertainty frames in an ecologically valid environment. As with many scientific studies, participants did not encounter the article in a natural way, were aware that the stimulus and measures were part of a study, and some may have even been aware that these types of studies are experiments that manipulate characteristics of the stimulus. Of course, many safeguards were put in place to mitigate these concerns and to ensure the quality of the responses. But these safeguards are themselves a potential limitation, as they ensured that the data consisted of responses from people who paid close attention to the entire article, understood it, and carefully considered their opinions of it. This is not representative of a great deal of information consumption in the real world. Rather, people skim headlines, scroll past content, and rarely engage in concerted self-reflection about their opinions about one piece of content. The elaboration likelihood model (ELM; Petty & Cacioppo, 1986) suggests that there is good reason to expect that highly motivated, highly focused individuals often respond to messages differently than those who are processing with low effort and low attention, particularly when the message components of interest are small nuanced variations in phrasing. This is not to say with certainty that different effects would have emerged if participants were less attentive, as some studies have found that elaboration accentuates framing effects and other have found that elaboration mutes framing effects (Rothman & Updegraff, 2010). Rather, the reader should simply make a note that this sample may have processed the messages with higher elaboration than occurs in the real world, due to the artificial nature of the study and due to the "quality" filters. Future research could even investigate differences between results with low- and high-attention (or, elaboration) participants.

Fourth, the results of this study are fully subject to the criticisms of all studies of one-shot message effects. Specifically, attitudes rarely change after one message and when they do the change is likely small and/or transient. Longitudinal studies and/or studies using a diversified mix of media sources and platforms are difficult and expensive, but they are often much more informative and much more important than this dissertation.

Fifth, this dissertation does not focus on comparing relationships between issues – in part because the issues are likely inequivalent in innumerable inherent and irreconcilable ways. For example, the issues likely differ in terms of public understanding of the science, public awareness and understanding of the issue, scientific consensus, threat severity, threat salience, and the degree to which a particular degree of scientists' uncertainty is perceived as acceptable. While these differences can be an advantage when a consistent pattern of (non)significant findings emerge across all three disparate issues, it also introduces a significant confound when interpreting results that emerge in one or two issue(s) but not the other(s).

Sixth, the complexities of this experimental design precluded simultaneous testing of all pieces of the conceptual model, which resulted in numerous tests in a piece-meal analytic approach. Conducting numerous tests can reduce confidence in the findings, as the chances of Type 1 error increases rapidly. It is important that future studies test and re-test pieces of model of relationships between constructs that was proposed here.

Seventh, also regarding the conceptual model, most traditional theoretical persuasion models structure behavioral intentions as an antecedent and consequent of attitudes (Fishbein & Ajzen, 2011; McGuire, 2012). In the conceptual model proposed by this dissertation, behavioral intentions is placed at the same level as beliefs and perceived credibility. It may be more theoretically and conceptually accurate to treat beliefs and credibility as mediators that in turn influence behavioral intentions. The possibility of this relationship is further evidenced by the results of the models presented above, which indicated small or nonsignificant direct and indirect effects of prior issue position on behavioral intentions, but strong associations between credibility and beliefs with behavioral intentions. Future analyses could explore this.

Eighth, the nature of the sample recruitment and survey administration process makes it impossible to control the environment(s) in which this study was performed. Within the sample, there is likely great variation in the motives for participation and the general study experience. More importantly, this might include systematic variation such that certain demographic or attitudinal segments of the sample were motivated to opt-in and/or experienced the study in a consistently different manner than other segments of the sample. Such differences could cause systematic differences in response patterns between segments. One way to further explore how the differences in study experience affect the results is to use measures of study completion time and of stimulus viewing time as covariates, or even as modeled moderators.

Ninth, there are many aspects of the measures used in this dissertation that introduce known and unknown confounds. Some were significantly modified from the form used in prior studies, and some were constructed from scratch for the purposes of this particular study. For example, the behaviors included in the behavioral intentions measure were selected with minimal justification, and were not a logical consequent of the stimulus itself. This disconnect and minimal development may have been the cause of the nonsignificant effects. As such, one cannot conclude from this dissertation that uncertainty frame variations do not affect behavioral intentions, in general. In addition, these Likert-type self-response measures do not differentiate the reported strength of attitude from its indelibility, confidence, or perceived evidence-basis. This is important because, for example, there may be a difference in motivated reasoning effects between attitudes that are reported as "strong" in their extremity on a scale and, instead, attitudes that are based on a great deal of evidence or experience. Further, many measures could conceivably be combined or subdivided into different variables or factors, which may produce different results. For example, it may be that the interaction effects of uncertainty type is different for the trustworthiness dimension of credibility compared to the expertise dimension. This concern is mitigated by the evidence of unidimensionality of the credibility factor, but this does not preclude the possibility of differential effects. Overall, it is likely that these (or any) self-response items, mean scales, and latent factors are far from the ideal method of assessing these (or any) attitudinal constructs. The limits on construct validity associated with such measures are applicable in most survey experiments but also are especially applicable in this one that uses (but still carefully validates) new and specialized measures.

Relatedly, tenth, the main study did not make full use of the constructs of positivist understanding of science and deference to science, which are both included in the full theoretical model (Figure 1). For positivist understanding of science, the exclusion was because the pilot test found it to have low reliability and small or nonsignificant correlations with the outcome variables, and also because it introduced a risk of priming thoughts about the nature of uncertainty in science. However, it is intuitive that individual differences in opinions about the role of uncertainty in science would moderate attitudinal effects of uncertainty frames, and prior research has supported this (Rabinovich & Morton, 2012). Thus, it is important for future research to improve this measure and more closely investigate this construct and its influence in the effects of uncertainty. Similarly, individuals' trait-level tolerance for uncertainty and context-specific preferences for uncertainty are important to assess in order to specify the degree to which observed effects can be attributed to the message characteristics and which are better-attributed to individual and contextual factors. To preserve some parsimony in this study, the analyses simply used deference to science as a covariate, rather than as a modeled predictor of the outcome variables (a relationship that would be moderated by uncertainty frame). This interaction effect should be explored by future research because an individual's trust in, or deference to science, likely has an important effect on their tolerance for – and/or interpretations of – uncertainty by scientists.

Eleventh, in an exploratory study with large ambitions, a complex design, and many layers of analysis, one of the limitations is that there are a) too many limitations to document, and b) more limitations than are readily apparent. Thus, the reader should interpret the findings of this dissertation in full consideration of any additional limitations that they think are applicable.

5.5 The Future Directions

The conceptual explication, theoretical model, and empirical tests presented in this dissertation form a springboard for many future research opportunities – many of which I hope to undertake. First, given that this particular taxonomy of uncertainty frames is at once innovative, exploratory, and (somewhat) subjective, it is important for future research to explore and expand on whether functional differences underlie the responses to these different uncertainty frame types. That is, although Chapter 1 demonstrates intuitive linguistic and semantic distinctions between the four uncertainty frame types, this does not necessarily imply differences in fundamental cognitive responses to, or interpretations of, these different types. The degree to which, and the manner in which, these different frames spark different schema or emotions is the fundamental reason for expecting there to be (or not be) different effects. Given the nascent state of research into the effects of uncertainty frames, these questions present valuable opportunities for future research.

Also, it may be the case that (say, consensus) uncertainty frames about Claim A in Issue 1 may *not only* have negative effects on attitudes about Claim A in Issue 1, but also about Claim B in Issue 1. A highly relevant practical example is if scientists are portrayed as having consensus uncertainty about the effects of climate change (which would not be inaccurate), this portrayal may also increase perceptions that scientists have consensus uncertainty about *the existence or causes* of climate change (which would be inaccurate). This "uncertainty transfer" might also follow patterns of motivated reasoning. I hope to continue the research on uncertainty frame effects in this direction.

In addition, I hope to explore how variations in dosage (quantity and strength) and source (e.g., sources beyond just scientists) moderate the effects of uncertainty frames. This dissertation only tested the effects of one level of dosage, and that particular level may not have been the level associated with the strongest (or weakest) effects. Similarly, this dissertation only tested the effects of portrayals of uncertainty on the part of scientists. There are many other entities that are often portrayed as having uncertainties about science (Rice et al., 2018), and responses to these uncertainties may be different than responses to the uncertainty by scientists that were manipulated in this dissertation.

Further, it is likely that affective responses are relevant to the effects of uncertainty frames. Specific to science communication in climate change, studies have indicated that the effects of frames are mediated by felt emotions (e.g., Nabi, Gustafson, & Jensen, 2018). That is, framing manipulations have effects on attitudinal outcome variables *to the degree that* they elicit discreet emotions of hope or fear. Applied to the present investigation, it could be that a _*type*_ uncertainty frame has a significant effect on _*outcome variable*_ to the degree that it evokes a particular emotional response. If this was the case, tests of effects of frame type that do not consider emotional response would be (partially) muted. For example, it seems probable that portrayals of consensus uncertainty would be associated especially negative effects on beliefs, credibility, and behavioral intentions in instances where it elicited *positive emotional responses* (i.e., positive affective response to the portrayed discord). Conversely, consensus uncertainty would *not* be associated with negative effects on beliefs, credibility, and behavioral intentions when the affective response is *negative* (e.g., frustration that agreement is not being portrayed). These, of course, are hypothetical musings. And the variance in the model that is explained by measures of affective response might already be mostly captured by the measure of prior issue position. But, still, given the relevance of prior research in science communication that has found that framing effects are mediated by emotional responses, this is a valuable avenue for future research.

Another consideration for future research is differences in effects across levels of the covariates used in the analyses of this dissertation. Specifically, the present study controlled for education, frequency of news media consumption, deference to science, and age – thereby muting their effect. But this does not inform valuable questions about how the modeled relationships and the interaction effects might differ across levels of these variables. For example, the review of relevant theory and empirical evidence (Chapters 1 and 2) implicated education and deference to science as likely moderators of the effects of uncertainty frames. But controlling for their respective effects simply helps specify the other relationships in the model – it does not answer the valuable question of how they might interact with these other relationships. Future research should apply specific focus to these questions.

Also, there is reason to suspect that uncertainty frames could be a strategic tool used to mitigate psychological reactance. That is, it may be that the right portrayals of (say, scientific or deficient) uncertainty could reduce the severity or frequency of instances where oppositional audiences perceive scientists (or others) as being elitist, domineering, and demanding obedience. It may be that individuals – particularly those with prior oppositional attitudes – would respond more positively to behavioral recommendations (and the sources of them) if they were presented with full disclosure of the uncertainties of science. All of these are potentially valuable future opportunities for research, and they all build on the groundwork that was laid by the theorizing and empirical tests of this dissertation.

5.6 Conclusion

Amongst themselves, scientists take great care to specify the uncertainties of science – using tentative verbs and clauses that bound the confidence of their findings and the degree to which those findings can be extrapolated. And even when the uncertainties of a field or finding are not specified, scientists usually assume them anyways. These norms of communication, and of information processing, reflect the fundamental philosophies of science – such that continued uncertainty is one of the primary (and valuable) epistemological characteristics of science and the scientific method.

But public-facing science communication is much different. Not only are the uncertainties of science communicated to the public with sporadic frequency and tenuous fidelity, but the public(s) also likely has different understandings of uncertainty in science than scientists do. As such, the different types of uncertainty frames may have unintended, undesirable, or conditional effects within and across various publics. Still, as science communicators, it is both impractical and fallacious to take the perspective that the solution to the problems of uncertainty communication is to demand *the public* develop a better understanding of uncertainty (a deficit model perspective). Further, it is neither prudent nor feasible to eliminate mentions of uncertainty in science communication. Thus, we must develop robust and generalizable theoretical and practical understandings of the positive and negative effects of specific uncertainty portrayals in specific contexts.

This dissertation is a small effort in this direction, indicating that consensus uncertainty is more detrimental than other uncertainty framing options; that the effects deficient, technical, and scientific uncertainty frame types do not differ significantly from one another; and that motivated reasoning is a more powerful effect than framing manipulations and is (practically) invariant across frame types.

It is easy to say that these findings are specific the contexts and methods of this study, but the truth is that these findings are supremely relevant to *many* contexts. That is, while these analyses should be interpreted in light of their many limitations, these stated limitations and unanswered questions should serve as catalysts for extensions to relevant contexts like risk communication, health communication, and political communication – as well as to other types of research, such as longitudinal designs and/or natural experiments. Given the complexities of human behavior, the development of reliable, generalizable best-practices for diverse uncertainty communicators is not achieved just through tighter study controls but instead through more (and more diverse) studies. As such, the small theoretical and empirical contributions of this dissertation are just a step toward its greater goals – which are to catalyze future studies that progressively and programmatically reduce and specify the existing uncertainties about the effects of uncertainty frames.

Overall, this dissertation clearly unveils more questions than it answers. Or, put differently, it increases more uncertainties than it reduces. Uncertainty is (at least) as much a fixture of this dissertation as it is of science at large. However, in the world of science, the specification of uncertainties is a valuable accomplishment, and is integral to advancement. This dissertation identified a great deal of deficient uncertainty that exists within the extant research about the concept(s) of uncertainty frames. It also highlighted the consensus uncertainty that has surrounded the question of the effects of uncertainty frames on attitudinal responses. Through a robust empirical test, it specified the technical uncertainties which are *the very evidence for* the estimates of the differential effects of uncertainty frames in a structural model of motivated reasoning effects. And, finally, this dissertation explored the many scientific uncertainties attached to these methods and findings – offering suggestions for the many ways that these ideas will (hopefully) be modified by future scientific inquiry. The identification and specification of these uncertainties are what enable a more nuanced understanding of the social effects of public-facing science communication for researchers in the present, and will lead to the development of more confident practical recommendations for science communicators in the future.

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Appendix A: Exemplar Stimuli

Item 1

Example Stimulus: Climate Change Consensus Uncertainty Condition

HOME / NEWS / SCIENCE NEWS

Climate Change is Harming Farmers, One Study Says

New evidence says the effects of climate change are a threat to millions of agriculture workers, although some experts disagree.

April 25, 2018 at 9:00 AM

By Ryan Goei and Amy Hubbard

MINNEAPOLIS (Minn.) – Researchers at the University of Minnesota published a scientific report this week indicating that the livelihood of millions of farmers and agriculture workers around the world may be harmed by the effects of climate change. However, other experts disagree.

Dr. Thomas Mozzetti, a research scientist who led the study, said:

"Our data show that global warming is detrimental to individual workers by significantly changing ecosystems and local weather patterns, making it difficult to grow crops in locations and seasons where they used to thrive."

He continued, "We are finding that this is frequent around the globe. The impact of climate change appears to include damage to the livelihood of working-class farmers and laborers, although this is in contrast to the research of some other scientists. Our study suggests that because of the effects of climate change, many have less opportunities to work, and when they do have work it yields less produce."

"The impact of climate change appears to include damage to the livelihood of working-class farmers and laborers, although this is in contrast to the research of some other scientists."

The study collected data from more than 15 countries, including advanced economies such as the United States and France, as well as developing economies such as Nicaragua and Bangladesh. The findings showed that in areas where the effects of climate change were most severe, the livelihood and security of local farmers and agricultural laborers were most negatively affected.

The study also accounted for the effect of many other variables, including current political unrest, each nation's wealth, and global demand for that region's leading exports.

When considering the findings reported in this study, it is important to note that there is continued controversy in the scientific community about the effects of climate change on agriculture workers' livelihood, with some scientists contending that climate change is not causing the observed pattern of lower earnings by farmers and laborers.

Item 2

Example Stimulus: GMO Labeling Technical Uncertainty Condition

HOME / NEWS / SCIENCE NEWS

Laws Requiring GMO Labels are Harming Farmers, One Study Says

New evidence says these laws are a threat to millions of agriculture workers, decreasing income somewhere between 5% and 22%.

April 25, 2018 at 9:00 AM

By Ryan Goei and Amy Hubbard

MINNEAPOLIS (Minn.) – Recently, many countries and states have passed laws that require food manufacturers to place labels on packaging that notify consumers of any genetically modified (GMO) ingredients. Researchers at the University of Minnesota published a scientific report this week indicating that these laws harm millions of farmers and agriculture workers around the world. However, the degree of this negative effect can vary.

Dr. Thomas Mozzetti, a research scientist who led the study, said:

"Our data show that GMO labeling laws are detrimental to individual workers by decreasing demand for crops that are hearty, pest-resistant, and economical, and increasing demand for crops that are very costly to grow."

He continued, "We are finding that this is frequent around the globe. The impact of these laws appears to include damage to the livelihood of working-class farmers and laborers, with estimated decreases in income varying between 5% and 22%. When GMO labeling laws are in effect, many have less opportunities to work, and when they do have work there is less demand for their produce."

"The impact of these laws appears to include damage to the livelihood of working-class farmers and laborers, with estimated decreases in income varying between 5% and 22%."

The study collected data from more than 15 countries, including advanced economies such as the United States and France, as well as developing economies such as Nicaragua and Bangladesh. The findings showed that in areas that have implemented strict GMO labeling laws, the livelihood and security of local farmers and agricultural laborers were most negatively affected.

The study also accounted for the effect of many other variables, including current political unrest, each nation's wealth, and global demand for that region's leading exports.

When considering the findings reported in this study, it is important to note that the effect of GMO labeling laws on agriculture workers' livelihood can vary widely, and that researchers use their data to form an estimated range of possible amounts.

Item 3

Example Stimulus: Vibrating Machinery Scientific Uncertainty Condition

HOME / NEWS / SCIENCE NEWS

Machinery Vibrations are Harming Farmers, One Study Says

New evidence says that extended contact with vibrating machinery is a health risk for millions of agriculture workers, although future research may change this.

April 25, 2018 at 9:00 AM

By Ryan Goei and Amy Hubbard

MINNEAPOLIS (Minn.) – Researchers at the University of Minnesota published a scientific report this week indicating that millions of farmers and agriculture workers around the world are harmed by extended periods of contact with vibrating machinery, termed "whole body vibration." This includes sitting on tractors and handling large power tools. However, scientists expect that their knowledge in this area will continue to evolve.

Dr. Thomas Mozzetti, a research scientist who led the study, said:

"Our data show that the continuous vibrations of agriculture machines and farm tools are detrimental to individual workers by literally eroding their musculoskeletal health and also increasing chronic pain."

He continued, "We are finding that this is frequent around the globe. The impact of "whole-body vibration" appears to include increases in early onset of arthritis or chronic pain in working-class farmers and laborers, although – like all science – we expect further research to clarify, or even change, these preliminary findings. Our study suggests that because of these over-time effects on their health and physical capabilities, many have less opportunities to work, and when they do work they are unable to be as productive."

"The impact of "whole-body vibration" appears to include increases in early onset of arthritis or chronic pain in working-class farmers and laborers, although – like all science – we expect further research to clarify, or even change, these preliminary findings."

The study collected data from more than 15 countries, including advanced economies such as the United States and France, as well as developing economies such as Nicaragua and Bangladesh. The findings showed that in areas where the careers in farming and agricultural labor were the longest, the musculoskeletal health of local farmers and agricultural laborers was most negatively affected.

The study also accounted for the effect of many other variables, including current political unrest, each nation's wealth, and global demand for that region's leading exports.

When considering the findings reported in this study, it is important to note that the effect of "whole-body vibration" on agriculture workers' health is a highly complex process that requires repeated study before any strong conclusions. Therefore, scientists fully expect that future, continued research could cause their current understanding of this issue to change as more data become available.

Appendix B: Scale Descriptive Statistics, Scale Items, and CFA Factor Loadings

	α	*skew*	*kurt*	*M*	*SD*	*CFA*
Prior Opinion (climate change)	**.90**	**-.77**	**-.25**	**5.08**	**1.52**	
1. Climate change (aka "global warming") is happening				5.56	1.74	.851
2. Humans are the main cause of climate change				5.05	1.87	.797
3. The climate change we see today is part of a natural cycle of warming and cooling (r)				3.82	1.87	.605
4. Climate change is going to have serious negative impacts on our planet				5.54	1.70	.919
5. Climate change is going to have serious negative impacts on our way of life				5.42	1.74	.918
Prior Opinion (GMO foods)	**.88**	**.17**	**-.47**	**3.80**	**1.49**	
1. GMO foods are harmful to our health (r)				3.64	1.76	.893
2. GMO foods are unethical (r)				3.71	1.88	.794
3. GMO foods are beneficial to society				3.88	1.71	.784
4. It is morally wrong to be changing nature with genetic engineering (r)				3.89	1.91	.737
5. Widespread use of GMO food does more good than bad				3.85	1.75	.652
Prior Opinion (vibrating machinery)	**.74**	**.01**	**.43**	**3.96**	**0.94**	
1. A career in farming or agriculture work is dangerous				4.39	1.46	.684
2. A career in farming or agriculture work is safe (r)				3.82	1.30	.593
3. A career in farming or agriculture work is healthy (r)				3.12	1.22	.430
4. Farmers and agriculture workers could get hurt easily				5.06	1.25	.571
5. Farmers and agriculture workers should fear for their health				3.41	1.47	.615
Deference to Science	**.76**	**-.07**	**-.58**	**4.58**	**1.36**	
1. Scientists should listen to the wishes of the public, even if they think citizen are mistaken or do not understand their work (r)				4.07	1.81	-
2. Scientists should do what they themselves think is best, even if they have to persuade the public that it is right				4.52	1.64	-
3. Public opinion is more important than scientists' opinions when making decisions about scientific research (r)				5.04	1.59	-
4. We depend too much on science and not enough on faith (r)				4.67	2.03	-
Individualism/Collectivism	**.78**	**-.08**	**-.30**	**3.43**	**1.20**	
1. The government interferes far too much in our everyday lives (r)				3.09	1.71	.452
2. The government needs to make laws that keep people from hurting themselves				4.32	1.63	.689
3. It's NOT the government's business to try to protect people from themselves (r)				3.84	1.77	.821
4. The government should stop telling people how to live their lives (r)				2.90	1.59	.541
5. The government should do more to advance society's goals, even if that means limiting freedom and choices of individuals				2.99	1.70	.432
6. The government should put limits on the choices individuals can make so they don't get in the way of what's good for society				2.92	1.67	-
Hierarchical/Egalitarian	**.90**	**-.33**	**-.99**	**4.58**	**1.69**	
1. We have gone too far in pushing equal rights in this country (r)				4.65	2.19	-
2. Our society would be better off if the distribution of wealth was more equal				4.79	2.02	.659
3. We need to dramatically reduce inequalities between the rich and the poor, between whites and people of color, and between men and women				4.98	1.97	.725
4. Discrimination against minorities is still a very serious problem in our society				5.28	1.83	.771
5. It seems to me like blacks, women, homosexuals, and other groups don't want equal rights; they want special rights just for them (r)				4.26	2.34	.835
6. Society as a whole has become too soft and sensitive (r)				3.61	2.17	.733
External Uncertainty Types (manipulation check)	**-**	**-**	**-**	**-**	**-**	

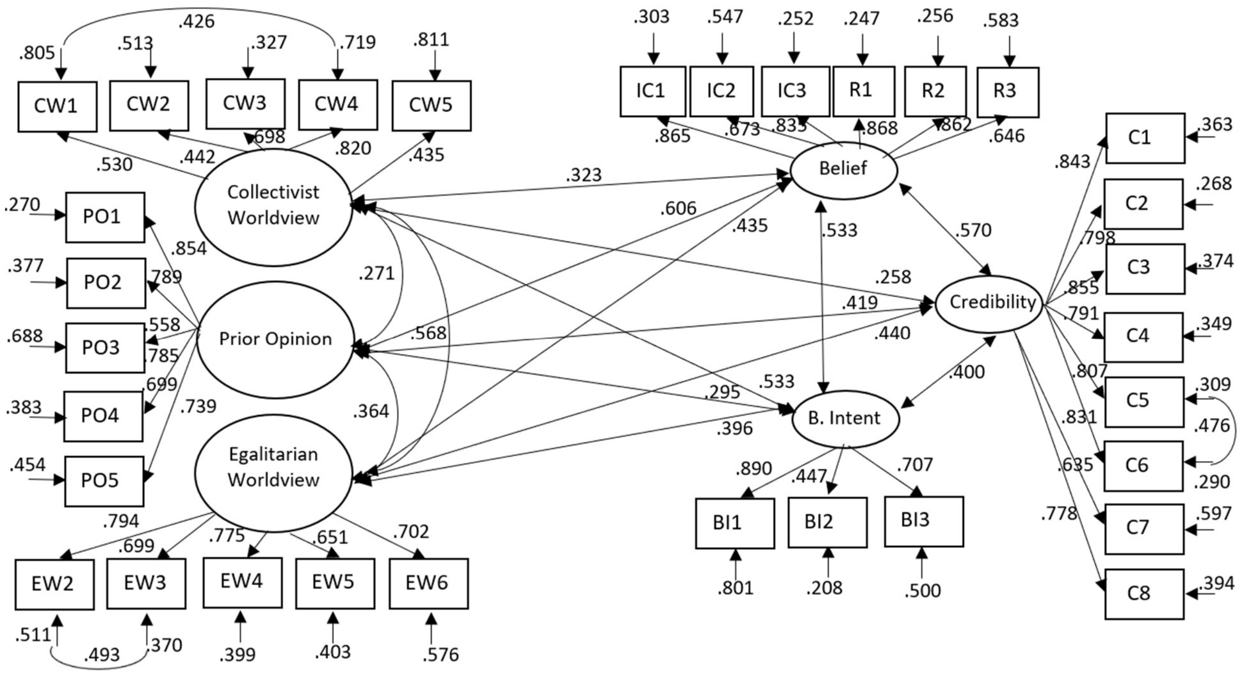
1. These scientists think there is still a lot that they don’t know about this subject				3.23	1.62	-
2. These scientists think that the findings of this research are rough estimates that could vary by a wide margin				3.43	1.58	-
3. These scientists think that they often disagree with each other or have controversy with each other about this subject				4.16	1.68	-
4. These scientists think that their findings and opinions about this topic will significantly change as future research progresses				3.20	1.57	-
Claim Belief	**.91**	**-.42**	**-.39**	**4.73**	**1.39**	
Internal Certainty (general)	**.84**	**-.44**	**-.43**	**4.78**	**1.49**	
1. I myself am very certain that __ is indeed causing negative effects on _				4.84	1.67	.835
2. I myself am skeptical of the idea that __ is indeed causing negative effects on __ (r)				4.54	1.80	.673
3. I myself think there is very strong evidence for believing that __ is indeed causing negative effects on __				4.94	1.64	.865
Risk Perceptions	**.84**	**-.42**	**-.41**	**4.69**	**1.43**	
1. I think __ poses serious dangers to agriculture workers.				4.78	1.69	.868
2. I think farmers and agriculture workers should be worried about ___.				5.00	1.61	.862
3. I think, despite __, farmers and agriculture workers will be able to continue on as usual, remaining mostly unaffected. (r)				4.29	1.62	.645
4. I think __ poses serious dangers to me and my loved ones.				3.40	1.92	-
5. I think people like myself do not need to be worried about __. (r)				4.32	2.00	-
6. I think __ will affect my life or lifestyle.				3.70	1.97	-
Credibility	**.93**	**-.53**	**-.06**	**5.29**	**1.27**	
1. Incompetent … Competent				5.59	1.39	.798
2. Knowledgeable … Ignorant (r)				5.58	1.47	.855
3. Unskilled … Skilled				5.70	1.38	.791
4. Intelligent … Unintelligent (r)				5.72	1.48	.806
5. Trustworthy … Untrustworthy (r)				5.11	1.60	.832
6. Honest … Dishonest (r)				5.31	1.53	.843
7. Biased … Unbiased				4.60	1.87	.635
8. Telling the Whole Truth … Withholding Information (r)				4.74	1.74	.779
Behavioral Intentions	**.73**	**-.00**	**-.50**	**3.55**	**1.44**	
1-2. *“In the future, if you saw another newspaper article about this same topic…”*						
How likely would you be to read it?				5.31	1.71	-
How likely would you be to share it with others?				4.53	1.88	-
3. “*Recently, non-profit organizations have been raising money to provide financial assistance to the farmers, workers, and their families whom the research study claims have been affected by ____.”*						
If you were given the option to donate part of your payment for this survey to this charitable cause, how much of it do you think you would give?				2.25	1.63	.444
4. *“Some countries and states have considered creating a small tax on agricultural products, which is then used to provide financial assistance to the workers that the research study claims have been affected by ___.”*						
Would you vote Yes in favor of creating this tax to assist workers?				3.76	1.92	.897
5. *“Some other countries and states have considered giving a small tax break to the agricultural workers that the research study claims have been affected by____, which would be a way to provide financial assistance to them.”*						
Would you vote Yes for this?				4.64	1.80	.702
General External Certainty (not used)	**.64**	**-1.02**	**1.01**	**5.41**	**1.25**	

1. It seems to me that these scientists are very certain that __ is indeed causing negative effects on farmers' and workers' livelihood/health				5.33	1.51	-
2. It seems to me that these scientists are very skeptical that __ is indeed causing negative effects on __ (r)				5.12	1.62	-
3. It seems to me that these scientists see very strong evidence for believing that __ is indeed causing negative effects on __				5.50	1.38	-
Internal Uncertainty Types (not used)	-	-	-	-	-	
1. I myself think there is still a lot that these scientists don't know about this subject (r)				2.97	1.57	-
2. I myself think that the findings of this research are rough estimates that could vary by a wide margin (r)				3.11	1.56	-
3. I myself think that these scientists often disagree with each other or have controversy with each other about this subject (r)				3.60	1.67	-
4. I myself think that these scientists' findings and opinions about this topic will significantly change as future research progresses (r)				3.16	1.53	-

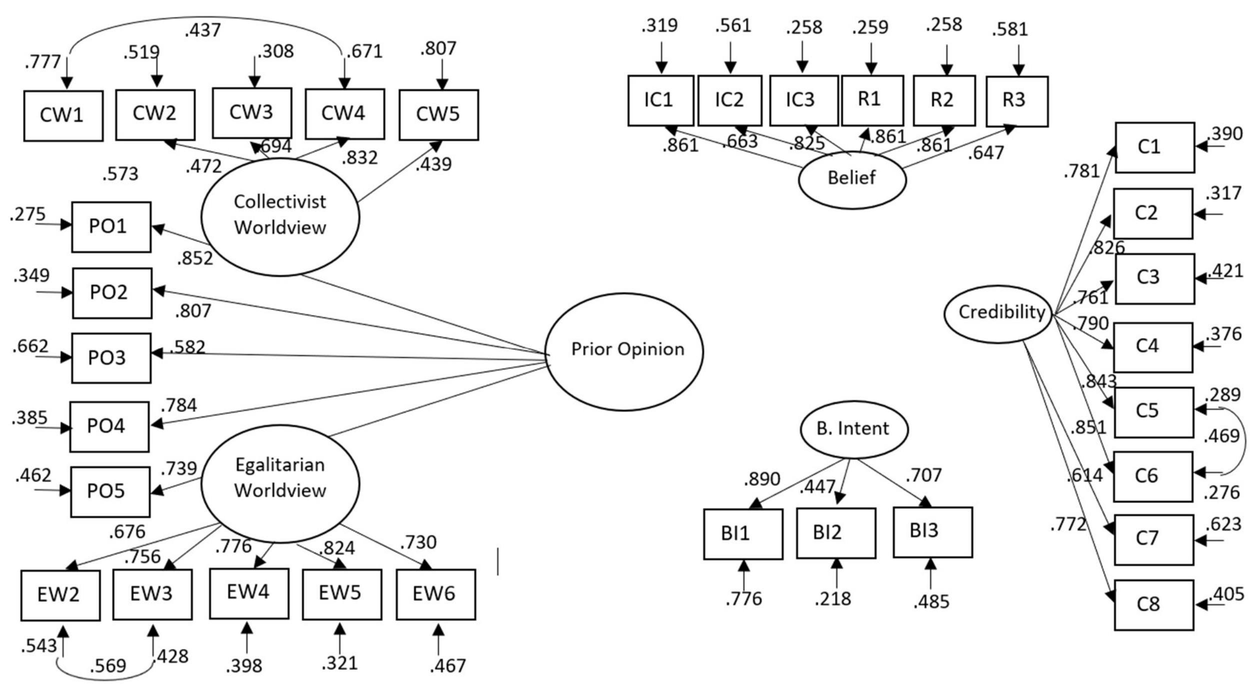
Note: All values calculated across all 15 main study conditions (except Prior Opinion values, which are within each issue); α=Cronbach's alpha for the items used in mean scales; skew=skewness; kurt=kurtosis; M=mean for scale and for items; SD=standard deviation for scale and for individual items; CFA=item loadings as produced by measurement model CFA with oblique rotation. Prior Opinion CFA values are from CFAs performed within each issue. Section 3.4 in the main manuscript justifies the one-factor structures for each scale and the excluded items. (r)=reverse coded item.

Appendix C: CFA and SEM Factor Loadings and Error Terms

CFA Factor Loadings and Parameters

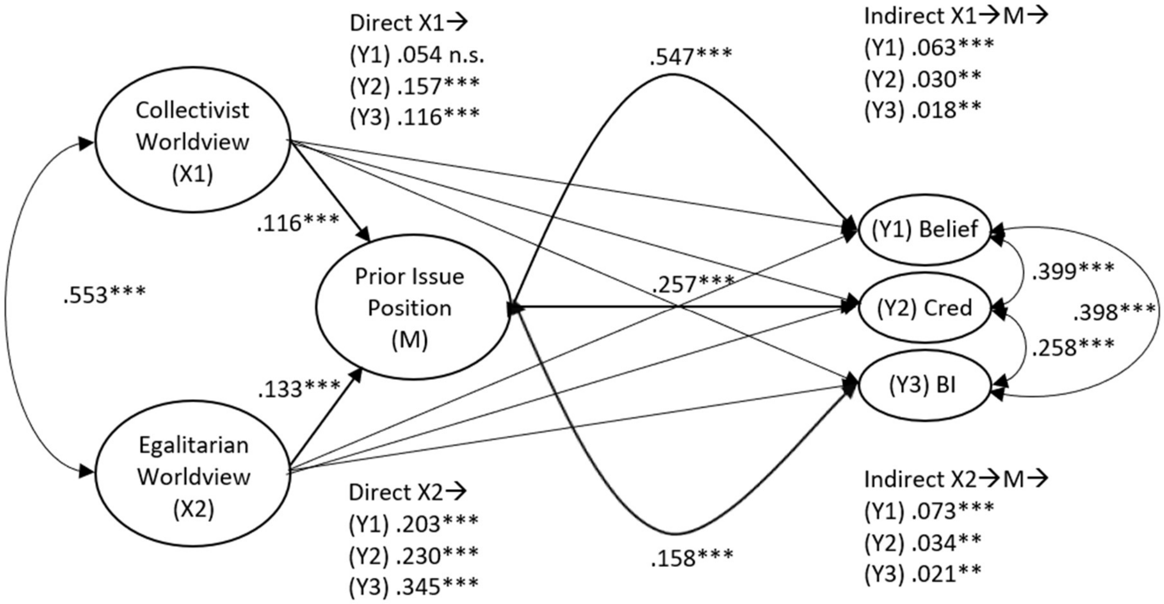


SEM Factor Loadings and Error Terms

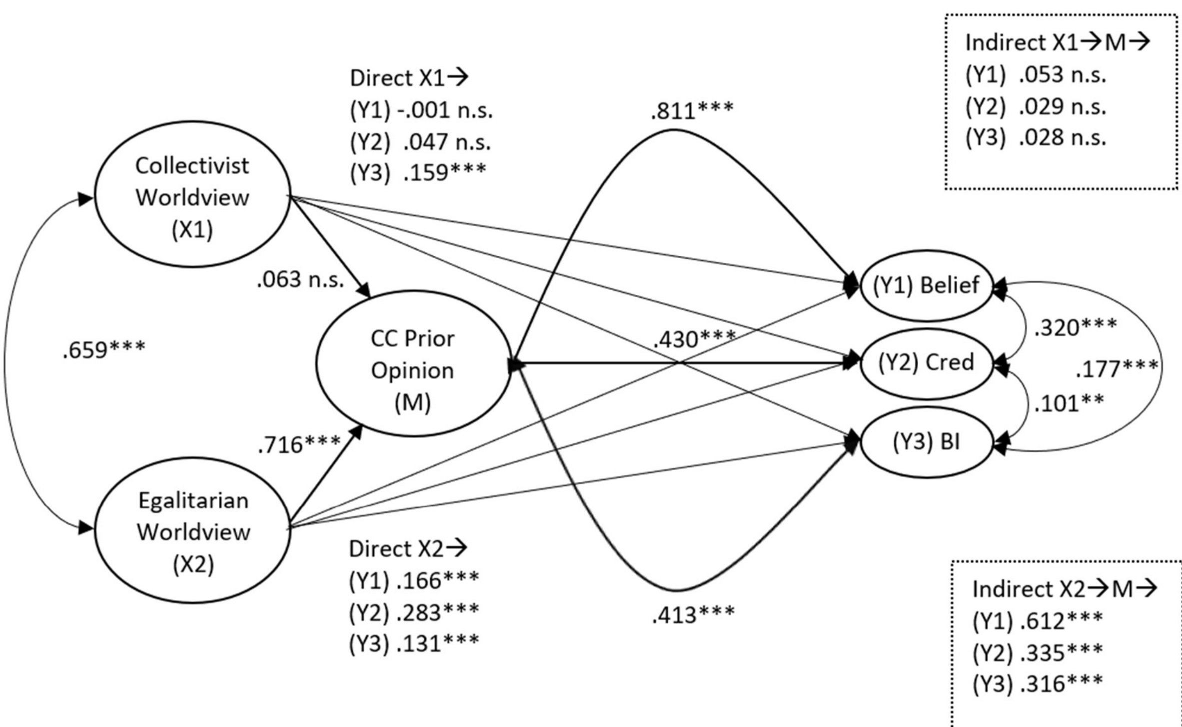


Appendix D: SEM Paths, Coefficients, and Indirect Effects

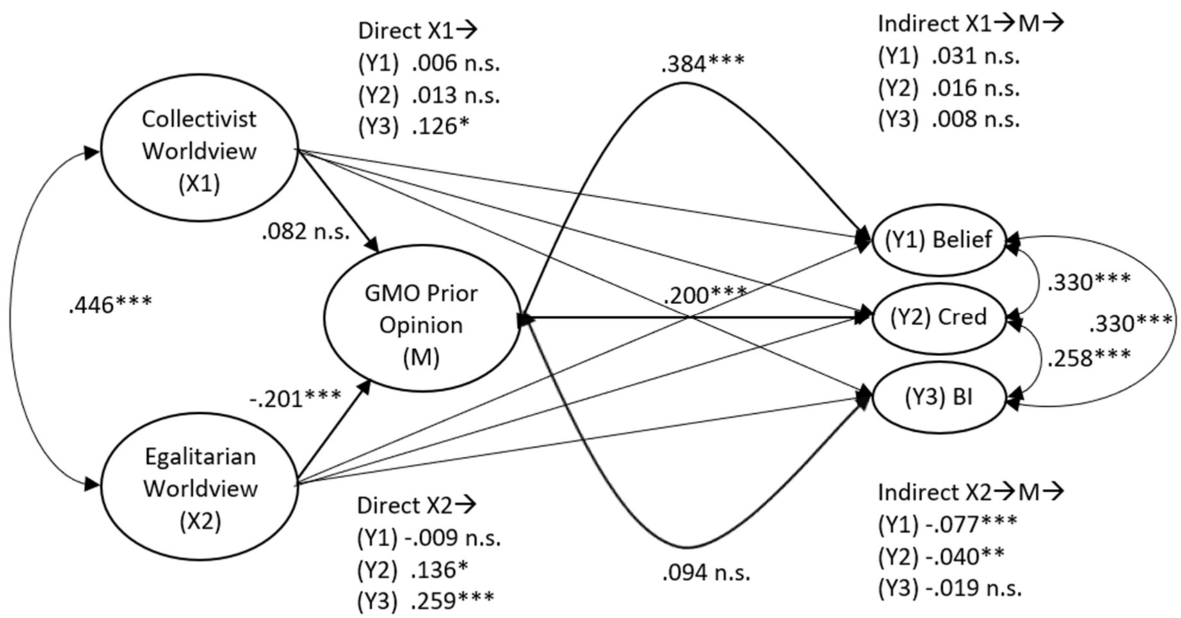
Item 1: SEM Paths, Coefficients, and Indirect Effects in All Issue Combined



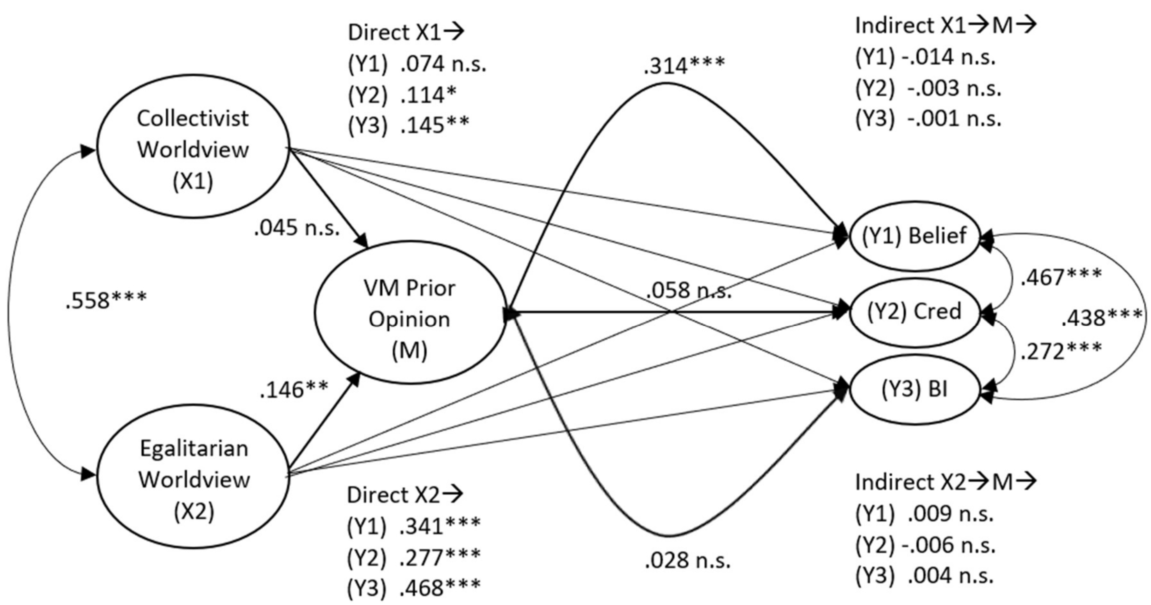
Item 2: SEM Paths, Coefficients, and Indirect Effects in Climate Change Conditions



Item 3: SEM Paths, Coefficients, and Indirect Effects in GMO Labeling Conditions



Item 4: SEM Paths, Coefficients, and Indirect Effects in Vibrating Machinery Conditions



Appendix E: All Pair-Wise Combinations of Z-Tests

Z-statistics of Pair-wise Comparisons of the $M \rightarrow Y_i$ Effect Between Uncertainty Frame Types

$M \rightarrow Y_i$	β	*SE*	1 (Ctrl)	2 (Con)	3 (Def)	4 (Sci)	5 (Tec)
Belief (CC)							
1. Control	.6124	.0484	-	0.05	0.46	0.03	0.47
2. Consensus	.6163	.0564		-	0.48	0.03	0.53
3. Deficient	.5776	.0585			-	0.48	0.02
4. Scientific	.6144	.0485				-	0.50
5. Technical	.5795	.0505					-
Credibility (CC)							
1. Control	.2366	.0825	-	0.72	2.50**	1.39	1.52
2. Consensus	.1387	.1081		-	1.51	0.46	0.62
3. Deficient	-.0783	.0951			-	1.25	1.01
4. Scientific	.0766	.0798				-	0.20
5. Technical	.0529	.0879					-
Behavioral (CC)							
1. Control	.0876	.0963	-	0.18	0.77	0.04	0.34
2. Consensus	.1114	.0863		-	0.61	0.23	0.16
3. Deficient	.1820	.0771			-	0.86	0.43
4. Scientific	.0831	.0851				-	0.39
5. Technical	.1317	.0891					-
$M \rightarrow Y_i$	β	*SE*	1	2	3	4	5
Belief (GMO)							
1. Control	.3418	.0818	-	0.90	0.54	1.03	0.72
2. Consensus	.2452	.0686		-	0.29	0.22	0.07
3. Deficient	.2773	.0870			-	0.46	0.19
4. Scientific	.2216	.0826				-	0.26
5. Technical	.2536	.0901					-
Credibility (GMO)							
1. Control	.0558	.0785	-	1.19	0.10	0.61	0.05
2. Consensus	.2025	.0944		-	1.31	0.64	1.14
3. Deficient	.0452	.0748			-	0.72	0.15
4. Scientific	.1240	.0797				-	0.56
5. Technical	.0616	.0792					-
Behavioral (GMO)							
1. Control	.0781	.0771	-	1.25	1.92*	1.14	0.27
2. Consensus	-.0598	.0792		-	0.68	0.15	0.96
3. Deficient	-.1366	.0806			-	0.84	1.63
4. Scientific	-.0440	.0747				-	0.84
5. Technical	.0477	.0793					-
$M \rightarrow Y_i$	β	*SE*	1	2	3	4	5
Belief (VM)							
1. Control	.2620	.0634	-	0.23	0.47	0.47	0.28
2. Consensus	.2860	.0819		-	0.64	0.64	0.01
3. Deficient	.2193	.0647			-	0.02	0.74
4. Scientific	.2207	.0619				-	0.74
5. Technical	.2873	.0660					-

Credibility (VM)							
1. Control	-.0581	.0801	-	0.39	0.65	0.01	0.04
2. Consensus	-.0093	.0969		-	1.02	0.40	0.39
3. Deficient	-.1167	.0424			-	0.67	0.83
4. Scientific	-.0588	.0756				-	0.05
5. Technical	-.0543	.0624					-
Behavioral (VM)							
1. Control	-.0161	.0830	-	0.74	1.26	0.26	0.04
2. Consensus	-.0968	.0716		-	0.52	0.49	0.78
3. Deficient	-.1455	.0600			-	1.03	1.39
4. Scientific	-.0457	.0763				-	0.25
5. Technical	-.0206	.0672					-

Note: Values are the absolute value of the z-statistic resulting from a z-test comparing the β of M→Y_i in the corresponding row and column frame type conditions; *=*p*<.05; **=*p*<.01.