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Climate Science, Character, and the "Hard-Won" Consensus

ABSTRACT: What makes a consensus among scientists credible and convincing? This paper introduces the notion of a "hard-won" consensus and uses examples from recent debates over climate change science to show that this heuristic standard for evaluating the quality of a consensus is widely shared. The extent to which a consensus is "hard won" can be understood to depend on the personal qualities of the participating experts; the article demonstrates the continuing utility of the norms of modern science introduced by Robert K. Merton by showing that individuals on both sides of the climate science debate rely intuitively on Mertonian ideas—interpreted in terms of character—to frame their arguments.

INTRODUCTION

The late Michael Crichton, science fiction writer and climate contrarian, once remarked: "Whenever you hear the consensus of scientists agrees on something or other, reach for your wallet, because you're being had. . . . In science consensus is irrelevant. What is relevant is reproducible results" (Crichton 2003). Reproducibility of results and other methodological criteria are indeed the proper basis for scientific judgments. But Crichton is wrong to say that consensus is irrelevant. Consensus among scientists serves to certify facts for the lay public.[1] Those on the periphery of the scientific enterprise (i.e., policy makers and the public), who don't have the time or the expertise or the equipment to check results for themselves, necessarily rely on the testimony of those at the center. When those at the center are more or less in agreement on a subject, that consensus carries considerable weight.

Such a consensus is not, however—as Crichton might rejoin—a guarantee of accuracy.

This paper explores a range of arguments about the reliability and unreliability of scientific consensus. It does this in two ways: first, in the

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abstract, and second, with reference to a live public debate, that surrounding climate science. The example of Crichton's remark should not lull the reader into assuming that the polarized debate over climate science is framed in simplistically polarized arguments. Consider that Lawrence Solomon (2010) eloquently argues for the authority of scientific experts in a defense of climate skepticism—and that Joseph Romm (2008) advances arguments almost identical to Crichton's in a rebuttal of climate skepticism. This paper is a study of arguments, and one of its goals is to demonstrate that even as they square off in vehement disagreement over scientific theories and facts, those who participate in the climate debates share a common stock of flexible rhetorical strategies and a common stock of ideas about the proper working of science. Specific ideas about what makes a consensus "hard won," and therefore likely to be reliable, are widely shared on both sides.

As we explore arguments about the authority of scientists and the reliability of scientific consensus, we make reference to relevant recent academic literature on the philosophy and sociology of expertise. In addition, we draw on the older literature on "Mertonian" scientific norms; another goal of this paper is to show the continuing relevance of this line of study. We find that the notion of a hard-won consensus includes an intuitive reliance on Mertonian norms, understood in terms of character—essentially, judging the quality of the science by judging the quality of the scientist.

The paper concludes with some reflections on the role of character judgments in ordinary discourse as well as in science.

A HEURISTIC APPROACH TO ASSESSING THE RELIABILITY OF SCIENTIFIC CONSENSUS

The Possibility of Error

What do we mean when we say that consensus among scientists in a discipline is not a guarantee of accuracy? From a Kuhnian perspective, of course, one might say that today's consensus may be viewed as false by the standards of some future era. But putting aside Kuhnian relativism (or limiting the scope of our inquiry to so-called normal science) and positing an objective truth that scientists could discover—or an objective falsehood they could refute—it is conceivable that members of the scientific community could be so grossly in error that they fail to discover the truth, or that they mistakenly certify the falsehood. At least three grounds could be offered to explain such gross communal error:

1. *Bias*. Individuals with life histories are inevitably subject to biases, and these may cloud judgment. Conceivably, there may be biases that would apply to a whole class of experts, either by virtue of their expert status (e.g., a bias about the relative importance of their specialty vis-à-vis other specialties) or by virtue of other shared traits (e.g., political or ideological leanings).
2. *Interest*. Individuals with a stake in the outcome of a debate, either material (e.g., allegiance to sponsors, expectation of financial rewards) or personal (emotional investment in a favored hypothesis) may delude themselves with spurious arguments (in fact a form of bias), or suppress inconvenient arguments or conclusions, or even attempt to deceive with spurious arguments and evidence. Conceivably, many or all members of an expert community could share common or overlapping interests.
3. *Groupthink*. Individuals may feel pressure to agree with peers to reduce interpersonal conflict, pressure either self-applied or socially applied. In such a position, an individual might moderate or mediate his or her views in order to come into accord with others. Or, faced with a widely accepted plausible theory, the individual might accept it at face value and fail to explore alternative theories.

These three possible explanations for erroneous expert consensus range from the banal and mundane (e.g., every person is subject to mild biases and has a personal interest in his or her work) to the egregious. In the literature on expertise, Alvin Goldman (2006, pp. 24–31) includes consideration of interest, bias, and groupthink ("conditional independence") in a general theoretical discussion of the reliability of experts; their applicability under conditions of expert consensus can be taken as a sort of limiting case for Goldman's observations. Heather Douglas (2008, pp. 11–12) also provides a useful discussion of bias in expert judgment.

The scientific enterprise has some built-in practices that reduce or minimize these possible sources of error. Peer review, for example, is meant to smooth out individual biases. Disclosure of financial interests serves to alert readers to possible biases resulting from such interest. But beneath the level of institutional practices, there are other factors that might counteract such sources of error. In particular, there are norms and values that, according to a school of thought initiated by sociologist Robert K. Merton in the mid-twentieth century, underpin the modern scientific enterprise.

The Mertonian Tradition

Merton introduced the norms in a 1942 essay entitled "The Normative Structure of Science" (1973 [1942]).[2] Other social scientists who made use

of and built on Merton's normative framework in the decades that followed (including Bernard Barber [1952], Warren Hagstrom [1965], and Norman W. Storer [1966]) varied the list somewhat, but canonically there are four norms: universalism, communalism ("communism" in Merton's original formulation), disinterestedness, and organized skepticism. Universalism requires that contributions to scientific discourse be judged on their merit, without regard for the contributor's nationality, race, religion, sex, class, and so forth. Communalism makes imperative the free and open communication of methods and results. Disinterestedness requires practitioners to disclaim or forgo any egoistic or material stake in the outcome of their research. Organized skepticism (sometimes simply called "skepticism") refers to the right and duty of the scientist (in Merton's formulation, the scientific community at large) to question any and all dogma.

Naturally, the norms represent an ideal that is not attained in practice. Nevertheless, according to Merton's school of thought, the norms are institutionalized in ways that influence scientists' behavior, making scientific communities particularly well suited to the collaborative production and certification of reliable knowledge.

In the 1970s, scholarly interest in the norms went into eclipse. Practitioners of the emerging sociology of scientific knowledge (SSK) tradition emphatically rejected the Mertonian legacy, perceiving it as naive or paternalistic or as denying the validity of the sort of inquiries they wished to pursue (though some from the SSK tradition considered the conflict to have been largely unnecessary at the time [Collins 1982] and "over-publicized" in retrospect [Shapin 1995, p. 297]). Among traditional sociologists of science, in the meantime, interest shifted away from norms and toward other aspects of Merton's research program, as part of a general turn away from humanistic inquiry and toward quantitative research methods (Ben-David 1991).[3]

Today there is renewed interest in the Mertonian norms in several disciplines. The field of scientometrics, which has always acknowledged a debt to Merton, has started to reclaim the language of norms (see the 2004 special issue of *Scientometrics*). Those interested in professional ethics in science have found the norms a useful framework for empirical study (Anderson, Martinson, and de Vries 2007; Antes, Brown, Murphy, et al. 2007) and for prescriptive programs (Kalleberg 2009; Meyer and Sandøe 2010). Historians such as Toby Huff (2003, 2007) and myself have made preliminary forays into the investigation of the historical origin and development of the modern scientific ethos. Perhaps more than anyone else, science scholar John Ziman has provided continuity in this

story, having made use of the Mertonian framework in published writings from the 1960s through the early years of the twenty-first century; His magisterial *Real Science* (Ziman 2000) offers a useful modern restatement of the norms.

Much ink has been spilled about the ontology of the norms. I do not attempt here to defend the moderately strong claim (the classical one) that the norms are real and exert influences that help make science productive and accurate, still less the very strong claim (a parody of the classical view that has sometimes served as a straw man) that the norms somehow guarantee the integrity of scientists and the accuracy of scientific pronouncements. Still, I believe the weak claim I uphold in this essay—that Merton's norms approximate how scientists and laymen alike think about how science is supposed to work—takes us much of the way to Merton's moderately strong thesis by supporting the idea that the norms are shared and have moral force, that is, that they are indeed real.

The norms can be viewed from several angles. They can be interpreted as prescriptions or they can be viewed descriptively as objects of study. When viewed descriptively, they could be located in *institutional* arrangements and practices that influence and constrain scientists, like blind peer review and disclosure of financial interest, or they could be sought in the *character* of scientists who have internalized those institutional norms—or both. Here we are concerned with character, but we approach the topic obliquely. Our object of study is not the actual moral character of scientists but the prescriptive ideal of scientists' moral character in the popular mind. This popular prescriptive expectation could perhaps itself be seen as one "institutional" factor that influences the constitution and behavior of the scientific community in the classical Mertonian view.

Of the four norms, two in particular—disinterestedness and skepticism—are of primary interest in this paper, for their connection to the idea of the "hard-won" consensus

The Hard-Won Consensus

We may define a hard-won consensus as one that emerges only after vigorous debate and a thorough examination of the range of alternative explanations. It is one in which centrifugal tendencies are strong, and the experts are drawn into agreement only reluctantly and after careful consideration. Again, the fact that a consensus is hard won is not a guarantee that it is correct. But it provides some assurance that the scientific community has done its due diligence.

The next question is: how does one *recognize* that a consensus is hard-won and not merely an artifact of groupthink, or interest, or bias? One strategy is to evaluate the quality of the dialogue among experts, to look for evidence that vigorous debate took place and that multiple hypotheses were entertained before consensus was achieved. This could be done by a layperson, but only at cost and with difficulty.[4] It would require the layperson to monitor the emerging technical literature and/or wade through extensive archives. And there are no clear criteria for deciding whether a debate has been robust enough, whether the hypotheses debated have been imaginative enough.

Another strategy is to evaluate not the quality of the dialogue but the quality of the participants. We are adept at evaluating people (or so we think—at the very least we are accustomed to it—more on this below), and it does not take long to form an opinion of a person's character, which makes this a feasible shorthand strategy, more feasible than monitoring and evaluating the technical discussions. It makes the problem of evaluating expert consensus tractable: judge the experts, and you have a way to draw heuristic conclusions about the confidence that should be placed in their pronouncements.

Philosophers may cringe at the lack of rigor in such a chain of reasoning, and one may point in particular to our unreliability in judging character. I do not contest the lack of rigor, but I think we should entertain this line of reasoning for two reasons. First, it is well documented that scientists evaluate *each other* based on personal qualities,[5] and history of science scholarship shows that since the earliest days of the modern era scientists have been at pains to develop personae—for the benefit of both the public and their fellow practitioners—to demonstrate that they possess traits considered important or necessary for participation or excellence in the profession (Shapin 1994; see also Shapin 2008). Second, lack of formal rigor notwithstanding, I wish to show that *this is how people actually think about* issues of controversy and consensus. These are terms in which debates about the reliability of consensus is played out, debates that include both scientists and laypersons, both defenders and critics of a given scientific orthodoxy.

What are the criteria for evaluating persons? Here we invoke two of the Mertonian norms. First, we expect scientists to be radically individualistic and skeptical. This trait motivates them to turn a critical eye on each others' work and collectively to leave no stone unturned when thinking about hypotheses, counterarguments, and perspectives on a problem.

Skepticism may counteract groupthink. Second, we also expect scientists to be professionally humble and disinterested: to turn their skepticism inward and examine their biases, to set aside ego and material interests, and generally to strive for objectivity and be willing to follow the evidence wherever it leads. This serves two distinct purposes that are relevant to our discussion. One is that it helps make possible consensus among skeptics. That is, it reduces individual attachment to first impressions and pet hypotheses, enabling collective scientific opinion to shift and coalesce around the best hypotheses as evidence accumulates. The other is that it also counteracts, at least to an extent, errors of interest and bias, helping to prevent scientists from making avoidable errors of judgment.

In other words, among the norms that govern our expectations for the behavior and character of scientists, the norms of skepticism and disinterestedness can help to counteract groupthink, interest, and bias and facilitate the emergence of consensus that—because it is born of a rigorous and frank examination of problems, peers, and self—may be considered "hard won."

Before moving on to the empirical portion of this study, I wish to point out that the norms have deep historical roots. Merton gave illustrative examples going back all the way to the early years of the Royal Society of London. My own historical research (publication pending) identifies them in the writings of philosophical reformers a generation earlier. In the theologically charged atmosphere of the seventeenth century, the norms were articulated by some using the language of Christian virtue. The virtue of humility, it was thought, would predispose natural philosophers to critically examine their own beliefs and assumptions. (In the modern literature [e.g., Ziman (2000)], "humility" is still sometimes used as a synonym for disinterestedness.) The virtue of individual conscience would predispose natural philosophers to apply their own private judgment to scientific questions rather than aligning themselves with one or another party in a controversy. The historical roots of the norms are significant because the old, common vocabulary of virtue is more accessible even today than the technical jargon used by sociologists. In public debate about scientific controversies in the twenty-first century, invocations of skepticism and disinterestedness may appear alongside more colloquial expressions like independence and humility—and their opposites in the vocabulary of virtue, namely, partisanship and arrogance.

A SURVEY OF THE CLIMATE SCIENCE DEBATE

We turn now to a contemporary issue, the public controversy over climate science, to survey arguments that are made about the credibility of scientific consensus by scientists themselves, members of the public, and commentators.

This brief survey examines representative arguments made both in defense of the consensus and in opposition to it. The survey does *not* touch on "first-order" arguments about climate science, that is, arguments about the merits of particular scientific data and models and hypotheses. It also does not dwell on arguments about whether consensus has been achieved. We take as a starting point—as do many of those whose opinions are cited here, on both sides of the debate—that there is general agreement among scientists in climate-related disciplines that the weight of evidence indicates that human activity is altering the climate. The consensus is not unanimous, but it is an effective consensus, solid enough to inform policy (which is what evidently rouses to activism many of those opposed to it).[6]

Most of the arguments made can be located on one of several planes introduced above: they involve the role and privileges of expertise, sources of error in expert judgments, and the character of scientific experts. The "rhetorical space" in which the debate takes place can be diagrammed as shown on the facing page.

Now we proceed to survey the arguments that populate this schema, going roughly clockwise from the lower left. Some of the arguments cross boundaries, especially the boundary between row (2) and row (3). It turns out that it is very difficult to talk about bias, interest, or groupthink without talking about character flaws as well.

Before we begin, we must address a question of terminology that already invites controversy, namely the very basic question of who is a "skeptic." That label is conventionally used to describe those who oppose the consensus view. Such individuals, who generally bristle at the label "denier," claim that the professional scientific community is being inadequately skeptical and so the consensus that has been achieved is suspect. Mainstream scientists, for their part, insist that being skeptical is part of their job description and that so-called skeptics are actually dogmatic or partisan or just aren't knowledgeable enough to pass judgment on the science . For convenience, we retain conventional terminology and refer to those who oppose the consensus view as "skeptics."

SCHEMA OF ARGUMENTS ABOUT CONSENSUS IN CLIMATE SCIENCE

	A. In defense of consensus	B. In opposition to consensus
1. Arguments about the role and privileges of expertise	• Invocation of climate scientists' expert authority	• Invocation of the authority of experts in neighboring disciplines • Deconstruction of the authority of experts
2. Arguments about sources of error in expert judgments (bias, interest, groupthink)	• Explanations for the failure of the recalcitrant minority of scientists to embrace the consensus view	• Explanations of the consensus as an artifact
3. Arguments about the character of experts	• Defense of the character of mainstream scientists • Attacks on the character of the recalcitrant minority of scientists	• Attacks on the character of mainstream scientists • Defense of the character of the recalcitrant minority of scientists

A Survey of the Arguments

We start with arguments made by defenders of the consensus view, seeking to explain why some credentialed scientists reject it (2A and 3A). Naomi Oreskes (2011) addresses the recalcitrance of some credentialed experts by distinguishing the "maverick" from the "mule." The "maverick" is a scientist who thinks outside the box when a research question is new and there is no consensus. Sometimes the maverick turns out to be at the leading edge of the community's thinking as evidence accumulates, and sometimes the maverick is left behind. The "mule" is someone who clings to a favorite hypothesis that is contradicted by the weight of evidence and refuses to acknowledge that he or she has been left behind. This stubborn unwillingness to abandon a favored hypothesis is presented as a character flaw: excessive pride that makes an individual's opinions rigid and blocks consensus formation. Not hesitating to name names, Oreskes suggests that MIT's climate contrarian Richard Lindzen belongs in this category.

Being a mule is perhaps not the gravest of sins, since consensus need not be unanimous to be effective. Arguably there is always room in science for a handful of mavericks who are willing to place really long bets. More serious is the accusation of partisanship—referring to scientists who are in the pay of interested parties like the fossil fuel lobby or who otherwise (e.g., for ideological reasons) toe a party line. In *Merchants of Doubt,* Oreskes and coauthor Erik Conway document the cases of several individuals who appear to have been co-opted in this way in the climate science debates. Some of those individuals, the authors report, like physicists Frederick Seitz and S. Fred Singer, have a long history of acting as "scientific gadfly," taking ideologically motivated and in some cases financially rewarded positions on controversial issues from secondhand smoke to ozone depletion to acid rain—and, in each case, maintaining the contrarian position even as specialist colleagues in the relevant disciplines reach an effective consensus (Oreskes and Conway 2010). A 2008 review of environmental skeptics' writings and networks and funding concluded that "environmental skeptics are not, as they portray themselves, independent and objective analysts. Rather, they are predominantly agents of conservative think tanks, and their success in promoting skepticism about environmental problems stems from their affiliation with these politically powerful institutions" (Jacques, Dunlap, and Freeman 2008, p. 351).

In the media wars over climate science, the argument from authority (1A) is frequently heard: since so many experts agree that human activity is likely changing the climate, it is reasonable for the public to accept that

proposition and prudent for policy makers to act on it. The argument from authority can be reinforced quantitatively. One study (Oreskes 2004) found that that 75 percent of refereed journal articles on global climate change argued for or assumed that humans are influencing the climate and none argued against it or indicated doubt. (The "neutral" 25 percent were articles on methods or paleoclimate.) Another study (Anderegg, Prall, Harold, et al. 2010) found that "97–98% of the climate researchers most actively publishing in the field" are in agreement about anthropogenic climate change (ACC) and that "the relative climate expertise and scientific prominence of the researchers unconvinced of ACC are substantially below that of the convinced researchers."

Arguments about the authority of experts are also heard in the heat of debate: for example, in response to skeptics who would second-guess paleoclimatologists' selection and interpretation of tree ring data.[7] One scientist asks: "Why should [Keith] Briffa—one of the world leaders in this field—have to explain himself to people who are not even specialists in this area, who are in fact amateurs who have set themselves up as judges of professional activity?" (qtd. in Pearce 2010, p. 57). Allowing amateurs, even those who have immersed themselves in the primary literature, to pass judgment on the work of experts would seem to violate what sociologists of expertise Harry Collins and Robert Evans (2007, pp. 60–63) call "downward discrimination" (the idea that those with greater expertise in a topic can be trusted to evaluate statements made by those with less expertise and not vice versa) and indeed to undermine the very notion of expertise itself.

That climate scientists' fear of data being mishandled and misinterpreted by amateurs is sincerely held and not mere public posturing or obfuscation can be seen in the private emails leaked after the Climategate hack. Some scientists were clearly exasperated by what they perceived as elementary mistakes, misconceptions, and misinterpretations of published results made by their hostile critics, and by their interrogators' apparent failure to grasp even the content of their own formal data requests (e.g., Climategate emails 1233245601.txt and 1229468467.txt).[8]

However sincerely held, such arguments from authority are ripe to be deconstructed (1B). Among the amateur skeptical community there is a hearty appetite to master the science of climate change, at least in the spirit of a whodunit. Hence the incessant demands that have been made by amateur skeptics for the data underpinning published studies and even for computer code and intermediate calculations. Those who would

attribute this slavish intent to exactly reproduce the work of the professionals to lack of imagination or analytical ability (e.g., Climategate email 1228258714.txt) miss the point. The amateur critics are not primarily interested in *replicating* results via independent analysis, as a scientific peer would be. Rather, they wish to *audit* the results, to look for flaws, evidence of wrongdoing or incompetence.

The presumption behind an audit is that expertise holds no mystique: experts may apply seasoned judgment, but every instance of such judgment can be isolated and interrogated and should be evaluated by canons of common sense accessible to anyone. Arguments for such "flattening" of expertise have not been neglected in the philosophy of science literature. Douglas (2007) suggests that Julia Annas's (2006) account of the essential unity and communicability of "practical expertise" can be applied to scientific expertise.[9]

Michael Polanyi's (1958, pp. 101–2) observations about the acquisition of tacit knowledge are also apposite here. Polanyi offers the (autobiographical) example of the medical student whose classroom apprenticeship to the study of pulmonary X-rays produces a cognitive transformation, almost a transfiguration. After the groping period of apprenticeship, what once were faint spidery lines on the dark background became a fully interpretable picture of lungs, in enough detail to make diagnoses. Mastering this interpretive scientific technique is akin to mastering a craft. In Collins's (2010) terminology, this sort of interpretive technique may be difficult to convey because it involves *ostensive* knowledge (knowledge that can only be conveyed by pointing), *unrecognized* knowledge (knowledge that the teacher is not consciously aware of), and even *somatic* tacit knowledge (unconscious knowledge or skill that develops in physical interaction with objects).

Interpretation of tree ring data is a discipline that has many of the same craft-like elements. If Polanyi's views of craft-like expertise are correct, then no matter how diligently a tree ring expert seeks to explain the basis of her judgments, she will never completely succeed and thus may never completely satisfy her critics. And no matter how fiercely critics interrogate from a distance, they will not really get the hang of interpreting tree ring data until they enroll in a graduate program and learn the craft directly.

This line of reasoning (which I have not seen fully articulated in the climate debate but is suggested by comments like "the only ones qualified to interpret the actual technical science are the professional scientists themselves" [SurvivalistBoards 2011]) might seem to mystify expertise

in a way that provides experts cover to behave arbitrarily, as well as to undermine any possibility of rational dialogue. This need not be so. Defenders of expertise should readily concede that although expertise might never be *fully* articulable, nevertheless scientists are bound to make their best efforts to lay bare and articulate the bases of their judgments. And climate skeptics could argue that merely being craft-like does not make an interpretive tradition valid. The medical student who studies pulmonary X-rays will want to be sure he is studying with a master who has a record of successful diagnoses. Since dendroclimatology is a relatively young field, and primarily a historical and observational discipline (in which case-control verification of hypotheses can not easily be performed), it might be possible to argue that current interpretive canons should not yet be considered fully validated.

In addition to deconstructing expertise, another tactic taken by amateur skeptics is to appeal to the authority of experts in a neighboring discipline (1B): most importantly, statistics. With a background in mathematics, amateur skeptic Steven McIntyre, a retired minerals consultant, has made some substantive criticisms of the statistical work of Michael Mann and other paleoclimatologists. The climate experts are vulnerable in this area since (as was noted by one of the independent review panels that was assembled after the Climategate email scandal [Oxburgh, Davies, Emanuel, et al. 2010, p. 3]) the community of professional paleoclimatologists has tended to rely on its own statistical training rather than collaborating with professional statisticians. If we envision "ladders" of expertise, both the professional climatologists and skeptics like McIntyre stand high on the ladder of statistical expertise but not at the top, so when official reviews and investigations have been made (e.g., the Wegman report, the National Research Council [NRC] report, the Muir Russell review, the Lord Oxburgh panel) it has always been necessary to bring in professional statisticians to adjudicate by exercising downward discrimination.[10] The statisticians have not always upheld the judgments made by the climate scientists. The 2006 NRC and Wegman reports, for example, though they differed in their main conclusions, both found fault with the statistical work behind the famous early "hockey stick" chart of historical temperatures produced by Mann and colleagues; the Intergovernmental Panel on Climate Change subsequently stopped using the chart.

This suggests a more general point, which is that climate science is so complex and multidisciplinary that no one individual can possibly possess the expertise to stand in an epistemically privileged position over

the whole domain. Atmospheric chemists have to take on trust, much as educated laypersons do, that the solar physicists know what they are talking about and that a consensus of oceanographers is a meaningful, hard-won consensus. For skeptical author Andrew Montford (2010a, pp. 222–23), who observes that geochemists and glacier scientists who are fully convinced of the overall argument for anthropogenic global warming nevertheless downplay the adequacy of data in their own disciplines, such considerations only magnify a skeptic's doubts. Solomon (2010, p. 46) similarly argues that scientists who have reservations about the evidence in their own area of expertise may have fewer reservations about the big picture. Recognizing this phenomenon as a general feature of expert communities, Collins (1992, p. 145) explains it with the quip that "distance lends enchantment." Collins and Evans elaborate: "Core-scientists are continually exposed, in case of dispute, to the counter-arguments of their fellows and, as a result, are slow to reach complete certainty about any conclusion. In general, it is those in the next ring out . . . —the non-specialists in the scientific community—who, in the short term, reach the greatest certainty about matters scientific" (2002, p. 246; cf. Collins and Evans 2007, pp. 20–21).

Moving on: skeptics also attack the consensus view by advancing arguments about the character, interests, and biases of mainstream scientists (2B and 2C). One tactic is the mirror image of the argument about co-optation by the fossil fuel lobby. It is said, for example, that climate scientists are only "in it for the money," or otherwise have impure motives, and are thus not truly disinterested. One representative skeptical commentator, for example, refers to an alleged "co-opting of climate science by politics, ambition, greed, and what seems to be a hereditary human need for a righteous cause" (Happer 2011). More specifically as regards money, there is speculation that scientists trim their research programs (e.g., Haddad 2011) or even falsify data (e.g., remarks made by Texas governor and U.S. presidential hopeful Rick Perry [Batheja 2011]), in order to continue to receive funding.[11]

Others appeal to the motives and character of mainstream climate scientists in order to defend the consensus view (2A and 3B), portraying these individuals in a light that minimizes the plausibility of error. It is argued that climate scientists are men and women of integrity. For example, one may point to the results of the official investigations launched in the wake of the Climategate hack. These investigations found several points to criticize but no evidence of fraud and no reason to doubt the "rigour and

honesty" of the climate scientists (Russell, Boulton, Clarke, et al. 2010, p. 11).[12] It is argued that if climate scientists take the public spotlight, it is only reluctantly and on account of the urgency of their findings; by temperament and training they are more comfortable in the seclusion of the lab than in the glare of the public eye (Oreskes and Conway 2010, pp. 262–65—hence, these authors argue, the need for advocates such as themselves). Furthermore, when discussing the implications of their research they tend more toward caution than sensationalism (Fischer 2011), as Montford to observes.

Regarding financial rewards, it is frequently pointed out that climate scientists do not become wealthy and do not expect to.[13] On the question of funding: those with firsthand knowledge of the funding process emphasize that "no-one gets funded to demonstrate a specific result. People get funded to investigate questions" (Schmidt 2011). Regarding any alleged timidity of scientists to publish results that challenge conventional wisdom, a representative response is Michael McElroy and Daniel Schrag's statement in a review of Crichton's *State of Fear*: "Crichton, apparently, has the view that there is a herd instinct in science. This could not be further from the truth. Scientific reputations are made not by reaching conclusions drawn by others earlier, but rather by challenging the status quo. Competitive juices fuel progress" (2005, p. 26).

Climatologist Judith Curry has offered a set of observations on the subject of character and groupthink that needs to stand alone in this survey for two reasons. First, it is a critique of the mainstream climate community (2B) that comes from a mainstream scientist rather than a skeptic. Second, although it is primarily directed at mainstream community, it applies equally well to the skeptical community (2A).[14] I refer to Curry's observations on the subject of "tribalism" in science (2009). Curry argues that under pressure from ideologically motivated attacks, scientists tend to "circle the wagons" and "point the guns outward," making themselves less likely to give criticism from the outside a fair hearing and less likely to tolerate dissent among themselves. Curry herself enjoyed the protection and solidarity of a "tribe" for approximately a year while she and her colleagues were subjected to a smear campaign on account of research on climate and hurricanes, and she came to recognize its disadvantages and dysfunctions as well as its advantages and occasional necessity. She suggests that the scientists caught up in the Climategate affair were subject to "tribal" thinking over an extended period and that this affected their judgment and their ability to be objective in certain matters. Arguably,

segments of the skeptical community that are actively striving for objectivity and truth (Steve McIntyre and some of his readers, Curry implies) are equally susceptible to a tribalism that interferes with achieving those goals. The implication of this analysis is that as long as the abusive, ideological media war over anthropogenic climate change continues, both sides are likely to find their ability to be open minded about opposing views, skeptical about the views of peers, and original in their own speculations to be diminished.

Finally, we consider arguments explaining why the public should place its trust those who oppose the consensus view (2B and 3B). Against the evidence brought forward by Oreskes and others about the overwhelming numerical disparity between those qualified scientists who accept the consensus and those who reject it, one response is obfuscation—packing lists of "thousands of skeptical scientists" with individuals who are not scientists or whose views are misrepresented, insisting on equal media time for the minority view, and so forth. We are not concerned with those tactics here (but see Hoggan and Littlemore 2009 and Oreskes and Conway 2010). Skeptics have, however, found one possibly effective way to counter the numerical advantage of their opponents in debate. That is to try to introduce a distinction between quantity and quality: to profile individual contrarian scientists with impressive credentials and argue that their opinion should carry more weight than the faceless mass of (as it is alleged) their more mediocre colleagues who take the mainstream view. Solomon (2010) uses this tactic in his book *The Deniers*, establishing credibly that there are serious minds, some with impressive credentials, who have misgivings about the mainstream view. Although Solomon fails to demonstrate that the majority view is not also defended by serious minds with impressive credentials, and although the book is padded with misleading profiles of scientists who *support* the consensus view (see, e.g., Hoggan and Littlemore 2009, p. 158ff), it is rhetorically quite effective in its portrayal of contrarians. It is effective not only on account of the scientists' credentials but also because it paints a picture of the scientists' character. These men (all the profiled scientists are men) are portrayed as doggedly independent thinkers, pursuing intuition and evidence regardless of the opinion of the world. In addition, they are portrayed as eminently reasonable (i.e., devoid of arrogance), and their reasonableness is starkly contrasted with the buffeting of unreasonable prejudice they are alleged to be subjected to, particularly as regards funding. Richard Lindzen, characterized by Oreskes as a "mule," is quoted by Solomon as saying that

"alarm rather than genuine scientific curiosity, it appears, is essential to maintaining funding. And only the most senior scientists today can stand up against this alarmist gale" (2010, 55).

This juxtaposition gets to the heart of the issue of character in the climate science debate. As the research of Dan Kahan and others (e.g., Kahan, Jenkins-Smith, and Braman 2011) has shown, "cultural cognition" appears to play a role in determining whether a person will accept or doubt the findings of the climate science community. Among the factors that predict doubt are an "individualist" orientation. Individualists tend to value independence in its many manifestations, from economic self-sufficiency and limited government to isolationism in international affairs and, of course, intellectual autonomy. Intellectual autonomy is, according to the heuristic framework we have been examining, an essential ingredient in the normative structure of science for guarding against groupthink. Individuals with an 'individualist' precognitive orientation who view the spectacle of a lone scientist sticking to his convictions in opposition to a faceless majority see a heroic figure, a hero in the same mold as the man who was martyred for his religious convictions in an earlier era.[15] The question of the recalcitrant scientist then becomes a subtle matter of moral perception: from one point of view he is a stubborn mule, suffering from a deficit of humility. From another point of view he is a paragon of independence. Both virtues are prized in the pursuit of science; only a small shift in perspective separates the presence of one from the absence of the other, making the difference between a hero and a fool.

SUMMARY AND CONCLUDING REMARKS ON THE LIMITATIONS OF CHARACTER ASSESSMENT

In this paper I have sketched a heuristic framework for evaluating the quality of a scientific consensus: the extent to which it is "hard won" in the give and take of scientific discourse. We have seen empirical evidence from the climate science debates that such a framework is widely shared. Significantly for those interested in science studies, we have seen reason to believe that Mertonian norms of science are alive and well in the mind and expectations of the public (and among at least some scientists), interpreted in terms of scientists' character.

In this paper we have also gotten some insight into the dynamics of a major contemporary controversy over science and expertise. As there is so little effective dialogue between the factions in the climate science wars, their accounts of reality have diverged to the extent that they sometimes

seem to be inhabiting different universes. There is value, I think, in compressing those universes together and seeing how they might be made to conform. One outcome is an appreciation of the extent of common ground they share: in particular, common standards for judging the behavior and character of scientists. Thus it would be profoundly wrong for science communicators and climate change policy advocates to assume that the bulk of climate skeptics are "antiscience." Even though they may at times hold expertise in contempt, many skeptics have a strong commitment to the norms of science. At times this commitment seems even more puritanical than that of the scientific establishment itself: as seen, for example, in skeptics' demands for more inclusive participation (universalism) and more open sharing of data and methods (communalism).

Since climate science is so controversial and polarizing, the reader might wonder whether my account of the debate has been a fair-minded and reliable one. In fact, by this point the reader will undoubtedly have already formed an impression based on the tone of the article, its reasoning and source attribution, comparison of statements of fact with the reader's own prior knowledge of the subject matter, and so forth. There is no escaping that a critical reader will judge the article and form an impression of the author, the two judgments being intertwined.

And, in fact, the ordinary canons of good scholarship bear a strong family resemblance to the scientific norms. Whether the author is a scientist, a humanist, or a journalist, we want to know: is she speaking for herself or serving a partisan agenda? Is he reasonable or arrogant, willing or unwilling to entertain alternative points of view? In the heuristic framework we have been exploring, character is taken to be an indicator of the reliability of information. When we first encounter an author on the page, however, inferences are drawn in the other direction: we judge the text and from that form an impression of the author.

This is certainly true of my own reading. A few pages of Sussman's (2010) erratic source attribution and mean-spirited diatribes was enough to produce the impression of a bully out of his depth on whom I could not rely as a source of credible skeptical arguments. Small but crucial discrepancies between my own knowledge of particular historical episodes in the climate wars and Solomon's (2010) otherwise compelling descriptions produced the impression of a clever spin doctor and inclined me to distrust him. By contrast, Montford (2010)—and on the other side, Oreskes and Conway (2010)—though clearly impassioned, impressed me as scrupulous in scholarship: diligent with source attributions, respectful of the reader,

and careful to point out alternative interpretations before drawing conclusions. Having formed an impression of them through their texts, I would gladly read and be inclined to rely on further writings by these authors.

If character is a stable, inherent quality of a person at all, it can only be gauged through intimate personal interaction, especially in unguarded moments. In most casual personal interaction, in secondhand impressions formed through the media, and in reading an individual's writing, we encounter not character itself but a facsimile, a persona fashioned by the subject himself or by others. Such a persona, though a facsimile, may nevertheless be stable and reliable. If an author writes one piece of good scholarship, we might reasonably expect the next piece written in the same persona to be of similar quality. Some sources of information about character are more reliable that others: the wide divergence in popular views of the character of climate scientists suggests the general unreliability of impressions acquired at second hand through mass media and the Internet. If the heuristic model of the "hard-won" consensus has validity, a truly reliable assessment of the independent-mindedness and disinterestedness of scientists would require more intimate knowledge than most laypersons have access to. Those within the scientific community, we have already noted, form and act on impressions of each other's character. It may be possible for social scientists to evaluate the character of scientific actors through survey instruments (e.g., along the lines of Antes, Brown, Murphy, et al., 2007). For the layperson, the best source of firsthand knowledge of a scientist's character is probably through the scientist's own writings—technical writings, nontechnical writings, and (sadly) purloined emails.

But the elusiveness of character is not the only obstacle to assessing it. There is also the subjectivity of our judgment. We are fallible and prone to jump to conclusions. When I see a commentator use the expression "hide the decline" in a way that suggests climate scientists have conspired to "hide" from the public a "decline" in global temperatures, my first instinct is to write off the individual as an unscrupulous partisan, a hack whose mind is closed to reason. After all, from even a glance at the very short email that is the source of the phrase it should be obvious that the author, climatologist Phil Jones, was not referring to a decline in real temperatures (Climategate email 0942777075.txt).[16] But my snap judgment of character could be mistaken. It is possible, after all, that the commentator honestly believes Jones to have been talking about declining real temperatures. He may have been relying on trusted sources, for example, whose reporting

of the error was so consistent that he never thought to verify it independently. He might deserve to be judged a poor fact-checker in such a case but not condemned as hopelessly beyond reasoned debate.

These sorts of errors are legion in the climate debates, on both sides. And we should not be surprised. Even the most scrupulous among us don't check every fact as a matter of course. We rely on the presumed accuracy of sources we trust—individuals whose character, well known to us, recommends them as reliable (Shapin 1995, 302–3). In today's political and cultural climate, there are two large camps of climate commentators, and any given individual's trusted sources are likely to lie almost exclusively in one camp or the other. Although I have undertaken to ground-truth facts drawn from sources on both sides, I know it would be the height of folly to presume that I could not have made similar sorts of blunders. If I have, I hope I will be judged naive rather than cunning or obtuse, and that a reader would not think it wasted effort to point out my errors to me.

The lesson I wish to draw from this concluding discussion is that too-reflexive judgments of character may preclude a "charitable" reading and close off opportunities (however rare, to the jaded climate warrior, they might seem likely to be) for improved understanding. A charitable reading is one that attributes honest, worthy intentions to the author. In case of a disagreement, it inclines one to engage rather than disengage—to delve more deeply into the issue, possibly to engage the other in dialogue, efforts that may deepen the understanding of one or both parties.

Alongside humility and independent judgment, charity is another Christian virtue (though, naturally, hardly unique to Christianity) that was recognized by some early modern Europeans as advantageous to the practice of natural philosophy. Manifested as tolerance, charity meant being receptive to contributions from any quarter—from antagonists, from the uncredentialed, from those of the "wrong" class or nationality or religion. It does not come easily; it is a discipline. And it bears a family resemblance to Mertonian universalism, which makes similar demands of inclusivity on the scientific community. Merton (1973 [1942], p. 272) illustrates universalism with the early, precedent-setting example of the admission into the inner sanctum of the Royal Society of London of a talented researcher of the "wrong" class (the demographer John Graunt, a haberdasher by trade).

An anonymous reviewer of an early draft of this article asked whether judging scientists by their character compromises the norm of universalism. This is a fascinating question. One way of formulating the norm of univer-

salism is to say that scientists should evaluate each and every contribution to scientific discourse on its technical merits, regardless of its source. If by scientific discourse we mean the formal peer-reviewed literature, there is no question that character judgments could be prejudicial and therefore would be inappropriate. Hence the importance of blind peer review. If "scientific discourse" is interpreted more broadly, the norm quickly becomes unattainable, even in principle. With limited time to devote to the evaluation of others' work, a scientist is bound to perform some sort of triage. And appreciation of others' character is likely to inform decisions about what is worth spending time on and what is not. It would be hard to fault scientists for making such judgments. But the observations about charitable reading apply here as well: character judgment, though useful, is fallible. Valid and valuable contributions might come from surprising quarters. The (idealized) institution of formal blind peer review in science, like a charitable reading in ordinary written discourse, serves to temporarily suspend character judgments and create a clearing where the unexpected can occasionally take root.

NOTES

1. A veritable consensus of classic science studies authors agrees on this point. John Ziman writes that "the objective of Science . . . is a *consensus* of rational opinion over the widest possible field" (1968, p. 9, emphasis in the original). In Robert K. Merton's words, "The institutional goal of science is the extension of certified knowledge" (1973, p. 270). See also statements to a similar effect by Norman Campbell (1953, p. 27), C. P. Snow (1971, p. 94), and Warren Hagstrom (1965, pp. 273, 281).
2. See also Merton's essays on "Science and the Social Order" and "Priorities in Scientific Discovery" in the same 1973 collection.
3. One of the most widely cited contributions from this period, an empirical investigation of "counternorms" diametrically opposed to the canonical norms conducted by Ian Mitroff (1974), is difficult to categorize. Though some have viewed it as a refutation of Merton, Mitroff himself considered it rather an extension of Merton's work on sociological ambivalence. For Merton's own views, see Merton (1976, p. 56ff.) and Cronin (2004, pp. 44–45).
4. Unless the subject happens to be geology, in which case the reader might find that scientific papers explicitly lay out multiple possible explanations for their observations rather than merely seeking to test a single hypothesis. The "method of multiple working hypotheses" was promoted by geologist Thomas Chrowder Chamberlin in the late nineteenth century, and genera-

tions of geologists have been trained in it. Chamberlin's aim in introducing his method was to counter "the dangers of parental affection for a favorite theory" (1965 [1890])—a tendency we have identified as exemplifying both bias and interest. For contrasting views on the continuing utility of Chamberlin's method, see Johnson 1990 and Elliott and Brooks 2007.

5. For example, Hagstrom has observed that scientists develop reputations among their peers as heavyweights or lightweights (in publishing valid and valuable research), as conscientious or sloppy (in reviewing the work of peers), as team players or leeches (in pulling their own weight in collaborative efforts), as trustworthy or unscrupulous (in refraining from stealing others' research ideas), and as possessing or lacking a proper humility, objectivity, and civility (1965, pp. 18, 24, 28, 86, 115). From the Climategate emails, it is clear that in their private communications climate scientists share and form impressions of each others' character, for better or worse (e.g., who is opportunistic and publicity seeking, who is brilliant but hot headed). Climate scientist Gavin Schmidt, in a description of the peer-reviewed scientific grant-making process, notes that the reputation of a researcher as a capable project administrator counts for a good deal in the award of new grants (2011). Collins and Evans provide further examples (2007, pp. 50, 67). "One of the reasons for going to meetings," they quote one scientist as saying, "is to meet the scientists in one's field so that one can form an opinion of them and judge their work" (2007, p. 67).
6. Also, the survey is not intended to resolve or arbitrate disputes. But I do not hesitate to point out the strength or weakness of particular arguments. In the spirit of full disclosure, I should make my own orientation clear: having made a detailed study of the controversy, I do not find that the skeptical case (either in terms of first-order arguments about the science, insofar as I am equipped to evaluate them, or second-order arguments of the kind discussed in this paper) meets the high threshold that would be required to convince me to reject the mainstream view. However, like climatologist Judith Curry and others who have immersed themselves in the debates, I recognize that skeptics have made some valid points.
7. For an introduction to this controversy, see Pearce 2010, p. 55ff.
8. The Climategate emails are referred to here by their conventional numerical filenames, which appear to have been assigned before the hack. No authoritative version of the emails has been published or archived for researchers, but the collection is still available at various advocacy and grassroots websites.
9. The social constructivist strain of the SSK tradition, what has been called the "second wave" of science studies (Collins and Evans 2002), has also promoted a flattening of expertise, though on different theoretical grounds.

10. Even bringing in professional statisticians might not be enough to resolve a controversy. The history of statistics includes notorious intradisciplinary controversies (e.g., Baysians versus frequentists on the right way to interpret probabilities, Ronald Fisher versus Jerzy Neyman and Egon Pearson on hypothesis testing and the use of confidence intervals). I owe the metaphor of "ladders" of expertise to Elizabeth Anderson of the University of Michigan.
11. The notion of widespread fraud in climate science, though common enough in certain corners of the blogosphere, goes far beyond the ordinary allegations made by public spokespersons of skepticism, several of whom are climate scientists themselves. As far as I am aware, Perry has not yet publicly backed down from his bold assertion, nor has he indicated which scientists or which data he was referring to or what his sources are.
12. The response from the skeptical community has included raising concerns about bias, interest, and groupthink among the Climategate investigators themselves (e.g., Montford, 2010b).
13. See, for example, the discussion underneath the blog post by Schmidt (2011). When this point was made recently by Al Gore (German 2011), he was pounced on by right-wing pundits for supposed hypocrisy, as he himself has made a substantial income from *An Inconvenient Truth* and allied efforts to publicize anthropogenic global warming. Gore, of course, is not a scientist and does not claim to be one. A more elaborate rebuttal by defenders of the consensus view—I have not seen it made in print—would be to emphasize the distinction between the roles of the salaried scientist and the entrepreneurial advocate. It is those who engage in advocacy (writing and promoting books, cultivating reputations as public speakers, etc.) who face gross incentives to sensationalize, overstate, or simply stir up controversy. On the side of the consensus, the two roles are relatively distinct—it is the rare research scientist who takes the time to participate directly in the public climate science wars or develop a public persona at all (Boykoff 2011, 86–88). On the side of the skeptics, there appears to be considerable overlap. Skeptics actively participating in climate science tend also to be advocates (or are sought out for the role by the think tank network), and advocates tend to emphasize their scientific credentials, such as they are.
14. This is my interpretation, but it is also suggested by the author herself, who applies the keyword "tribal" to the skeptics in one instance. See also Pearce 2010, chapter 18.
15. A distinction, it would seem, needs to be made between individualism as a cultural value and individualism as an ideology. A partisan attachment to an individualist ideology is the explanation Oreskes and Conway (2010) give, plausibly, for the stand taken by Singer and Seitz, cold warriors and vigorous

anticommunists, on the range of public health and environmental issues they dabbled in: they always took the stand on the side of the scientific issue that argued against government regulation. In other words, the scientific issues were implicitly turned into ideological battlefields between individualism and collectivism (with ordinary environmental or public health regulation standing in for Soviet-style communism). The most strident critics of the consensus view on global warming today are perfectly explicit about dragging their ideological baggage into the scientific arena—for example, talk show host and meteorologist Brian Sussman, who works himself into a lather to connect global warming to Karl Marx (who, we are confidently told, "loathed . . . America") by way of intermediary supervillains like Rachel Carson (Sussman 2010).

16. Jones writes of *adding* real temperatures to the time series in question, in order to hide something else not stated explicitly in the email: namely, a spurious declining trend in the proxy data. See ch. 14 of Pearce 2010 for details on the procedure, the email, and the controversy.

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