Anthropogenic Climate Change: Expert Credibility and the Scientific Consensus

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Any attempt to offer an overview of the climate science literature is bound to be controversial; the best known attempts are no exception. Here, we outline the methods and findings of the most important papers, examine the various criticisms levelled against them, and argue that while the critics raise some important points none of the criticisms damage in any very substantial way the findings these papers report. The majority of those working on climate science accept the proposition that anthropogenic climate change (ACC) explains most of the recent rise in global temperatures; even sceptics accept that the scientific consensus is against them. The real debate is about: how wide the consensus is – some of those keen to stress the existence of a consensus presenting evidence that it is virtually unanimous, others showing that it falls well short of that; whether the science being done by those who are convinced about the existence of ACC is better or worse than the science being done by those who are unconvinced, the evidence favouring the view that the more widely published and more widely cited climate scientists are more likely to affirm the existence of ACC; and whether the attempt to establish the existence of a consensus represents a valid response to the governments' need for policy advice or is little more than an attempt to silence an under-funded minority by obscuring the truth that the science is actually against ACC - or at best uncertain - with talk of a consensus threatening a premature end to what should be an on-going debate.

Introduction

The quickest way to establish whether there is a consensus in favour of the existence of anthropogenic climate change (ACC) – specifically, a consensus that ACC accounts for most of the observed global warming in recent years - is to turn to expert bodies or to refereed papers in highly regarded refereed scientific journals that present overviews of the scientific literature.

Of the expert bodies, the most significant is the Intergovernmental Panel on Climate Change (IPCC), a body created by the World Meteorological Organization and the United Nations Environment Program; its work draws on contributions from hundreds of scientists from a large number of countries. In its most recent report the IPCC concluded that '[t]he atmospheric concentrations of CO_2 and CH_4 in 2005 exceed by far the natural range over the last 650,000 years', that '[t]here is very high confidence that the global average net effect of human activities since 1750 has been one of warming', and that '[t]he CO_2 radiative forcing increased by 20% from 1995 to 2005, the largest change for any decade in at least the last 200 years' (IPCC 2007).

The connection between global warming and ACC has been supported, at various times in the last ten years, by the US National Academy of Sciences, the American Meteorological Society (with 11,000 members), the American Geophysical Union (with 41,000 members in 130 countries), and the American Association for the Advancement of Science (Oreskes 2007, 67-70). The Climate Congress held in Copenhagen in 2009 underscored the point (Richardson et al. 2009). And climate scientists surveyed by Dennis Bray and Hans von Storch in 2008 were more inclined to think the IPCC underestimated rather than overestimated both the impacts resulting from changes in temperature, precipitation, rising sea levels and extreme events and the magnitude of future changes in each (Bray and von Storch 2008, figs 39a-39d, 41a-41d).

Beyond the IPCC the two best known attempts to present an overview of the literature on climate change are the papers by Naomi Oreskes (2004) and William Anderegg and his colleagues (2010). Oreskes, a former geologist (Oreskes 2011, xiv-xv) from History and Science Studies at the University of California San Diego, examined the abstracts of over 900 papers published between 1993 and 2003; Anderegg, a biologist at Stanford University, and his colleagues from Toronto, Palo Alto and Stanford, examined a much larger number of papers produced between 1992 and 2009 by more than1400 researchers. The findings of the two reviews, while not of a piece, are mutually reinforcing: overwhelmingly the research conducted by climate scientists supports ACC. Oreskes found that while '[m]any details about climate interactions are not well understood, and there are ample grounds for continued research to provide a better basis for understanding climate dynamics...there is a scientific consensus on the reality of anthropogenic climate change' – by which she meant, quoting the IPCC's 2001 report, that 'most of the observed warming over the last 50 years is likely to have been due to the increase in greenhouse gas concentrations'. Anderegg et al., provide data not only on the very high level of agreement in the scientific literature about the existence of ACC but on how the level of agreement correlates with the researchers' expertise and with their prominence. It is on these two contributions to the debate that this paper is focused.

Methods

To test the hypothesis that there is a scientific consensus about the existence of ACC Oreskes' analysed 928 abstracts 'published in refereed scientific journals between 1993 and 2003 and listed in the ISI (Institute for Scientific Information) database of over 8,500 journals that used the phrase 'global climate change' (Oreskes 2005a, 2007, 70; not, as originally reported in Oreskes 2004, the phrase 'climate change). Although Oreskes subsequently referred to this as 'a *sample* based on the phrase "global climate change"' (2005b, 953, emphasis added), in the sense that she chose to look at these articles rather than those with the phrase 'global warming' (Oreskes 2007, 74), for example, her review encompassed every paper in which the keywords were used not just a sample of them.

None of the papers, according to Oreskes, refuted the hypothesis that 'global climate change is occurring, and human activities are at least part of the reason why'. Three-quarters of the papers

either ‘explicitly’ endorsed the ‘consensus view’ that ACC was the main driver of climate change (about 20% did this) or did so ‘implicitly’ (roughly half did this). The papers that ‘implicitly’ endorsed ACC were concerned with the ‘evaluation of impacts’ or were concerned with ‘mitigation proposals’. The other papers were about ‘methods’ (roughly15% were of this kind) or with ‘paleoclimate analysis’ (less than 10% fell into this category); these she described as ‘essentially neutral’ (Oreskes 2004). Since none of the papers challenged the hypothesis, ‘the basic reality of anthropogenic global climate change’, as Oreskes later put it, ‘is no longer the subject of scientific debate’ (Oreskes 2007, 73); it is ‘an established scientific fact’ (Oreskes 2011, xi). Where there was debate was over more detailed issues, matters of ‘tempo and mode’, to borrow the phrase of the palaeontologist George Gaylord Simpson (Oreskes 2007, 74). If some of the authors of the other 9,000 or so papers on climate change – those that did not refer directly to ‘global climate change’ – ‘expressed sceptical or dissenting views’ their number, she ventured, would be ‘very small’ (Oreskes 2004; 2007, 71-4; see also the exchange between Pielke 2005 and Oreskes 2005b).

Testing the same hypothesis, Anderegg et al started by compiling ‘a database of 1,372 climate researchers based on authorship of scientific assessment reports and membership on multisignatory statements about ACC’ – regardless of whether those signing supported ACC or were ‘skeptics/contrarians’. Their list was not ‘comprehensive’ or ‘designed to be representative of the entire climate science community’. But it did draw ‘researchers from the most high-profile reports and public statements about ACC.

The study ‘tallied the number of climate-relevant publications authored or co-authored by each researcher’ (taken as a measure of their ‘*expertise*’) and ‘counted the number of citations for each of the researcher’s four highest-cited papers’ (the marker of their ‘*prominence*’) as reported on Google Scholar. Anderegg et al. imposed ‘an a priori criterion’ that a researcher must have authored or co-authored at least ‘20 climate publications to be considered a climate researcher’. While this reduced the database to 908 researchers the criterion was not one that made much difference: ‘[v]arying this minimum publication cutoff did not materially affect results.’

Thus defined, the climate researchers were divided into two groups: ‘researchers convinced of the evidence (CE) of ACC and those unconvinced by the evidence (UE) of ACC’. Researchers ‘who signed statements broadly agreeing with or directly endorsing the primary tenets of the Fourth Assessment Report that it is “very likely” that anthropogenic greenhouse gases have been responsible for “most” of the “Unequivocal” warming of the Earth’s global temperature in the second half of the 20th century’ were classified as CE (n = 903). Researchers who had ‘signed statements strongly dissenting from the views of the IPCC were classified as UE (n = 472). By design, the study excluded scientists neither convinced nor unconvinced – those caught in between. The distinction between the convinced and the unconvinced should not be read as demarcating a more general difference between those who contributed to the IPCC’s deliberations and those who did not. In their 2008 survey of scientific opinion (n = 375) Bray and von Storch found either minimal evidence or no statistically significant evidence of

differences - in response to questions about whether climate change was taking place, whether climate change was anthropogenic and whether the IPCC reports were soundly based - between those who had participated and those who had not participated in the work of the IPCC (Bray 2010, 5, 13-15). Their sample was drawn from three groups working in 35 countries: authors of articles in 'climate journals with the 10 highest ISI impact ratings' over the previous ten years; authors identified by Oreskes (2004); and 'climate scientists drawn from readily available email lists on institute websites' (Bray and von Storch 2008, 2 and 5).

Expertise. The difference in expertise between the CE and UE groups measured by output could hardly have been greater. Of the UE group only one was in the top 50 climate researchers, only three in the top 100 and only five in the top 200. There was a large difference in the number of publications as well: a mean of 119 and a median of 84 for the CE group compared with a mean of 60 and a median of 34 for the UE group; ratios of 2.0:1 (mean) and 2.5:1 (median).

In the CE group, less than than 10% had 20 climate publications or less; in the UE group the corresponding figure was around 80%. Thus, 'the bulk of UE researchers on the most prominent multisignatory statements about climate change' had 'not published extensively in the peer-reviewed climate literature.'

Among 'the 50 most-published (highest expertise) researchers from both groups', there were also considerable differences: the top 50 researchers in the CE group averaged 408 climate publications (a median of 344) while the top 50 UE averaged 89 (a median of 68), a ratio of 4.6: 1 (5.1: 1). In short, 'the top CE researchers' had 'much stronger expertise in climate science than those in the top UE group.'

Prominence. Citation analysis, which 'provides an independent and approximate estimate of the relative scientific significance of CE and UE publications' reveals a clear if narrower pattern of difference between the two groups. The citation analysis conducted by Anderegg et al examined climate-relevant publications as well as 'highly prominent researchers who may not be directly involved with the climate field.'

Taking 'the top four most-cited papers for each CE and UE researcher with 20 or more climate publications', the study found that papers written by members of the CE group were cited 172 times, on average, while those written by UE researchers were cited 105 times, a ratio of 1.6:1. Since single papers might be highly cited simply because they are controversial ('the single paper effect') Anderegg et al also looked at 'the average citation count' of the second, third and fourth 'most highly-cited papers for each researcher', but the differences between the two groups remained: papers written by members of the CE group were cited 133 times, on average, while those written by UE researchers were cited 84 times, a ratio of 1.6: 1. When all the researchers on the original database were included (n = 1372) papers written by members of the CE group were cited 126 times, on average, while those written by UE researchers were cited 59 times, a ratio of 2.1:1.

The critics

Both papers attracted critics; in a world where there are reputations to be made and dashed, where the political stakes are large and the material stakes considerably larger, this was hardly surprising. But the two papers were also strongly defended. What were the criticisms and how well were they defended?

Oreskes' critics

Shortly after the publication of Oreskes' essay, Benny Peiser in Sport and Exercise Science at Liverpool John Moores University, subsequently 'executive head of the anti-orthodoxy Global Warming Policy Foundation' (Lawson 2010, 116), attempted to replicate the study. Peiser's search of the UK's ISI database for peer reviewed articles published between 1993 and 2003 that included the words 'global climate change' yielded 1247 documents, 1117 of which included abstracts. 'The results', Peiser argued, 'contradict Oreskes' findings and essentially falsify her study.' Whereas she said that 75% of the abstracts accepted the consensus view, 'either explicitly or implicitly', he found only 13 (1%) that 'explicitly endorse[d] the "consensus view"' and only 322 abstracts (29%) that 'implicitly accept[ed] the "consensus view"' – abstracts that 'mainly focus[ed] on impact assessments of envisaged global climate change.' Whereas Oreskes claimed that 'none of the papers explicitly disagreed with the consensus position', Peiser found 34 (3%) that 'reject[ed] or doubt[ed] the view that human activities' were 'the main drivers of "the observed warming over the last 50 years"'. And whereas Oreskes claimed that 'none' of the abstracts argued that current climate change was 'natural', Peiser found 44 in which 'natural factors' were said to have 'play[ed] a major if not the key role in recent climate change'. But the 'most significant discrepancy', he noted, was between the two accounts of 'whether human activities are the dominant driving force of recent warming'; his analysis showed 'almost three times as many abstracts' that were 'unconvinced of the notion of anthropogenic climate change' as 'those that explicitly endorse[d] it'. 'What happened', he wanted to know, to the countless research papers that show that global temperatures were similar or even higher during the Holocene Climate optimum and the Mediaeval Warm Period when atmospheric CO2 levels were much lower than today, that solar variability is a key driver of recent climate change, and that climate modelling is highly uncertain? (Pieser 2005a, Peiser et al 2005; see also Pilsen 2005).

Peiser submitted his analysis to *Science* a month after the magazine had published Oreskes' essay. Two weeks later (18 February 2005) the associate letters editor asked him to submit a shorter version. This he did. But the associate letters editor then informed him that 'the basic points of your letter have already been widely dispersed over the internet' and that as a consequence the magazine had 'reluctantly decided that we cannot publish your letter.' His reply, in which he pointed out that 'neither the details nor the results of my analysis have been cited elsewhere', was to no avail (Peiser et al 2005).While it's not true to say that the magazine 'refused to publish any of the numerous other letters...pointing out the deficiencies of Oreskes

analysis', as some have claimed (Monckton 2007, 7; cf. Pielke 2005), it did refuse to publish Peiser's.

The magazine may have done him a favour. Peiser hadn't replicated Oreskes study. While Oreskes had limited her search to 'articles', something her essay unfortunately did not make clear, Peiser had found not 928 but 1247 publications by searching 'all documents types' not just articles. 'This difference', Peiser conceded in October 2006, 'appears to explain the discrepancy between the "928" abstracts Oreskes claims to have analysed and the 1117 documents I found and considered', the number 'considered' being smaller than the number found. Still, while happy to concede that 'the 928 articles' she studied were included in the '1117 articles' he studied, Oreskes couldn't have studied 928 abstracts, he insisted, because the database only listed 905.

More importantly, he now had to concede that the 34 articles he found that 'reject or doubt the view that human activities are the main drivers of the observed warming over the last 50 years' may not have been included in any of the 928 articles selected by Oreskes. Moreover, 'some of the abstracts…included in the 34 "reject or doubt" category are very ambiguous and should not have been included.' The number of abstracts rejecting or doubting ACC, Oreskes was now said to have ignored, was just 'a few'. Later and very reluctantly he was forced to concede that 'a few' was just one - 'an opinion piece', it seems, for the journal of the American Association of Petroleum Geologists (see Hoggan with Littlemore 2009, 100ff, for an account of Peiser's background, his work and its political impact).

Nor by 2006 was further reference made to other lines of evidence Peiser had advanced in his correspondence with *Science*: 'the explicit and implicit rejection of the "consensus view"' by 'distinguished scientific organisations such as the American Association of Petroleum Geologists' (see also Peiser 2005b), hardly a disinterested body; the 2003 survey of climate scientists internationally in which 'a quarter of respondents 'still question[ed] whether human activity is responsible for most of the recent climate changes, 29% disagreeing that 'climate change is mostly the result of anthropogenic causes (Bray and von Storch 2007, fig. 30; badly misrepresented by Kellow 2007, 73, a defender of Peiser), a curious omission by a sceptic; and Tony Blair's speech at the 2005 World Economic Forum in Davos where he observed that while scientists who believed in ACC were in the majority, '[t]he majority is not always right', an observation coming from a politician that might best have been omitted in the first place. (Since leaving office, Blair has been working on 'business solutions to climate change'; Blair 2010, 681. Some self-identified conspiracy theorists see Blair as an 'alarmist' of long standing; for example, Carter 2010, 13, 166, 172).

But on two important points Peiser hadn't shifted. He insisted that 'the whole ISI data set includes just 13 abstracts (less than 2%) that "explicitly" endorse what [Oreskes] has called the "consensus view"' – an observation to which we shall return. And he noted that '[t]he vast majority of abstracts do [sic] not deal with or mention anthropogenic global warming

whatsoever'. Taken at face value, this second point might be taken to cast doubt on whether Oreskes actually adopted the selection principle she said she had; after all, it would be odd for Oreskes to have selected abstracts on the basis that they referred to 'global climate change' only for someone else to discover that not very many of them 'deal with or mention anthropogenic global warming' where this means - if Peiser's point is not to be dismissed as entirely disingenuous – 'explicitly or implicitly' (Oreskes 2004).

Beyond that, Peiser's back down was more or less complete. In particular, he now expressed no 'doubt that the overwhelming majority of climatologists is agreed that the current warming period is mostly due to human impact' (a concession also made by Monckton 2007, 10, though not by others; see, for example, Paltridge 2009, 68 and Aitkin 2011). While 'this majority consensus is far from unanimous', Peiser stressed, the 'community of sceptical researchers' – a community that 'remained extremely active' – was 'small' (Peiser 2006).

More recently, an attempt to update the Oreskes' study was undertaken by Klaus-Martin Schulte (2010), from the Department of Endocrine Surgery at King's College London. He reviewed 539 papers on 'global climate change', found on the Web of Science database from January 2004 to mid-February 2007, and reported that 45% (cf. 75% reported by Oreskes) either explicitly (7%) or implicitly (38%) endorsed ACC, while 6% (cf. 0% in Oreskes review) either explicitly (1%) or implicitly (5%) rejected it. He noted that '[o]nly 24% of the papers' were 'founded upon new data' and that '[o]nly 2% offer new data or observations directly relevant to the question of whether anthropogenic warming has prevailed over natural climate variability in the past half-century'. His conclusion: 'There appears to be little basis in the peer-reviewed literature for the degree of alarm on the issue of man-made climate change which is being expressed in the media and by politicians' (Schulte 2008, 285).

Even before it was published, Schulte's paper, widely circulated, encountered serious trouble. One reader claimed that Schulte's draft drew 'heavily' from a document prepared by Peiser, 'a document' Schulte's paper 'does not credit'. Sonja Boehmer-Christiansen - the editor of *Energy and Environment*, the journal to which Schulte's paper had been submitted, in which it was expected to appear and in which it subsequently was published – responded by observing that Schulte's 'survey of papers critical of the consensus was a bit patchy and nothing new', that it was 'not what was of interest to me', and that 'nothing has been published' (see Littlemore 2007).

A month later, in a contribution to the online Yale Forum on Climate Change & the Media, Zeke Hausfather (2007) doubted that Schulte's 'much-ballyooed analysis' would see the 'light of day in a peer-reviewed form'. The study by a 'medical researcher and endocrinologist', initially reported 'to be pending publication' in *Energy and Environment* - 'an obscure social science journal…carried in 26 libraries worldwide and not included in the standard list of peer-reviewed journals' – had been 'the subject of supportive advertisements by The Heartland Institute in *The New York Times* and *The Wall Street Journal* after an abstract had apparently been leaked to

“Daily Tech”’. (On the long history of the Heartland Institute, see Hoggan with Littlemore 2009, and Oreskes and Conway 2009). But if all this said something about how the paper had entered the policy debate it didn’t say much about the validity of its findings.

There were, Hausfather noted, three quite different conclusions one might come to from its findings: Oreskes ‘highly publicized research’ was ‘flawed’; Oreskes was ‘correct, but the consensus had wilted in the past few years’; or Schulte’s work itself was ‘flawed’. In fact, as Monckton (2007, 10) pointed out, there was another possibility – that the original research was flawed *and* that whatever consensus there might have been had wilted.

Hausfather dealt with the possibility that there was something fundamentally wrong with Oreskes paper by rehearsing the history of its reception: the original critique by Peiser - a member of the editorial board of *Energy and Environment*, a journal described by its editor as an outlet for ‘climate skeptics’ (Anon 2007); the withdrawal of Peiser’s claim that 34 papers ‘actually rejected the IPCC consensus’ (only one of the 34 did so; published in the *American Association of Petroleum Geologists Bulletin*, ‘it was not peer-reviewed and had not been included in Oreskes’s original analysis’); and an exchange between Oreskes and Schulte ‘on the website of a group called the Science and Public Policy Institute’, in the course of which Schulte had cited papers that appeared ‘to disagree with the IPCC position’ – papers ‘very similar to the now retracted list of articles from Peiser’s critique, even echoing some citation errors from Peiser’s paper’, although Schulte never cited him as a ‘source’. (In Plimer 2009, p. 452, this history is ignored; on the Science and Public Policy Institute, which also publishes the work of Monckton see: Grandia 2007; Hogan with Littlemore 2009, 86).

Had the consensus ‘wilted’? Not, Hausfather argued, if one were to judge by ‘the IPPC’s 2007 Fourth Assessment Report’ or by ‘the statement of the Joint Science Academies of the G8+5 countries’. Far from wilting, the survey work of Bray and von Storch (2007, fig 30) - published a few months earlier and good enough, as we have seen, for Peiser to cite - provided some grounds for arguing that the consensus had both grown and strengthened (see also Bray and von Storch 2008, fig 30, fig 21).

What then of Schulte’s work? As Hausfather notes, Schulte and Oreskes framed the issue in very different ways - Oreskes looking for papers at odds with the consensus, Schulte looking for papers that backed it. Oreskes defended her strategy by analogy: in Hausfathers words, ‘one could hardly describe the failure of evolutionary biology papers to endorse the theory of evolution as evidence of no consensus among them on Darwinian evolution.’ Schulte’s response was to make much of the 6% of papers said to either explicitly or implicitly reject ACC.

Schulte quotes key passages from six papers that, he says, ‘reject the consensus outright’. Hausfather reports ‘a detailed analysis’ – he provides no details - of what were originally seven such excerpts in which, he says, ‘only three…reject the consensus position, with the other four remaining neutral or implicitly supporting the consensus’ (the seven are reproduced in Monckton

2007, 11-13). Hausfather appears to be correct: only three of the six published excerpts 'reject the consensus'; two of these acknowledge ACC but say it has contributed less 'than half the global warming over the past century' or 'less to temperature change than natural climate variability' (for the excerpts, see Schulte 2008, 284; for favourable comments on Schulte's paper see: Asher 2007; Cook 2007; and Monckton 2009, 3).

As well as noting the criticisms leveled at Oreskes' paper we might also note some of the criticisms *not* leveled at her work. These silences point to those parts of her approach detractors appear to regard as either perfectly defensible or of no great significance: the use of the ISI database; the decision to categorize the papers on the basis of their abstracts rather than on the basis of a more comprehensive reading; the use of 'global climate change' rather than something else as the search phrase; even her invocation of 'tempo and mode' – a phrase one could imagine sceptics seeing as a cover-all not for matters of detail but for the all-important 'conglomeration of uncertainties' (Paltridge 2009, 12; see also Aitkin 2011, 71) said to lie at the heart of climate science, and not just by sceptics (see, for example, Barratt, Pearman and Waller 2010, 144; and, more generally, Bray and von Storch 2007, fig 38).

Responding to Oreskes' report that 'none of the papers disagreed with the consensus position', Roy Spencer, formerly a Senior Scientist for Climate Studies at NASA, remarked that '[a]side from the fact that I have a stack of such papers in my office, I would wager that neither did any of those 928 articles demonstrate that our current global warming is not due to natural causes' (Spencer 2008, 44). Proof in climate science raises the bar very high. It's a wager no one has taken up.

The critics of Anderegg et al

The piece by Anderegg et al also drew critics; but unlike the critics of Oreskes' work, whose criticisms were mostly posted on the immediate and increasingly important medium of the blogosphere (Aitkin 2011, 68-9), their main criticisms – along with responses from Anderegg and his colleagues - were published in the journal in which the original article appeared. (For other critiques, largely overlapping the published ones, from those strongly opposed to the ACC thesis, see Hoffman 2010, Pielke Snr. 2010 and Shaviv 2010).

The most thoughtful was written by Lawrence Bodenstein (2010) from Columbia. He attacked the use of 'publication metrics as a surrogate for expertise'. This line of argument was developed in three ways. First, Bodenstein insisted that scientists who had 'devoted much of their careers to a certain area, with multiple relevant peer-reviewed publications, should be deemed core experts, notwithstanding that others are more or less prolific in print or that their views stand in the minority.' This seemed to be an argument for privileging the work of those whose careers had been more or less wholly devoted to climate science over those whose careers hadn't, no matter how prolific their output. While Bodenstein implied that this would have made a significant difference to the findings he offered no grounds for saying so. Nor were any grounds offered by Garth Paltridge, formerly of the CSIRO, who implied exactly the opposite – 'that many of the

major advances of the past were made by scientists who were experts in disciplines unrelated to that of the original problem' (Paltridge 2009, 97-8).

'Climate science was not a case', Bodenstein argued, 'where there was homogeneous consensus absent a few crackpot dissenters'; rather, there was 'variation among the majority, and a minority, with core competency, who question some underlying premises.' But if he meant that 'core competency' was a feature of the minority rather than majority he provided no evidence. If he didn't mean this, then the justification for privileging 'core experts' simply on the basis of their status as contributors of long-standing over more prolific and/or more widely cited scientists is far from self-evident.

Second, he argued that in the controversy about climate change 'a priori, one expects that the much larger and more "politically correct" would excel in certain publication metrics. They continue to cite each other's work in an upward spiral of self-affirmation.' While noting that a statement to the effect that '[t]he majority of climate scientists favor some form of anthropogenic CC' was 'not disputed here' he thought the fact that 'they overshadow the small minority of dissenters in certain publication metrics is…almost tautological.' The belief – for which the Climategate emails are often cited in support (see, for example, Lawson 2010, 54; Pielke 2010, ch.8; but compare Washington and Cook 2011, 43-5) - that climate science is dominated by the 'politically correct' who act as gatekeepers, protecting the orthodoxy and keeping out sceptics, is widely shared by sceptics (see, for example, Paltridge 2009, ch. 4; Lawson 2010, 23ff), some of whom argue less conspiratorially that 'by and large the peer-review process operates to support orthodoxy…in every field' (Aitkin 2011, 69; see also Lawson 2010, 219).

Responding to the suggestion that the use of publication data tipped the scales unfairly against those who challenge the orthodoxy, Anderegg et al (2010a) claimed that they had not 'implied that minority viewpoints should be ignored' but were attempting to suggest 'the relative weight and credentials of viewpoints.' Here we might recall that about a third (34%) of the researchers was classified as UE rather than CE, hardly a 'small minority' nor one that suggests a metric that is 'almost tautological.' Moreover, while the average citation rate of CE papers was greater than that of UE papers (1.6: 1.0), the gap was not huge. Assuming that citation means citing with approval – an untested assumption - then the practice of citing 'each other's work in an upward spiral of self-affirmation' might have been almost as marked a feature of the UE group as it was of the CE group.

Third, Bodenstein argued that having defined *expertise* (italics in the original) in terms of 'number of publications', Anderegg et al. had then dropped the italics, so that in an act 'pregnant with self-fulfillment' the metrics were morphed 'into the conclusion of expertise (not italicized)'. An alternative to the 'ad hominem argument', focused on 'the characteristics, qualities, or failings of adversaries', Bodenstein argued, would be 'to critique the scientific evidence than count up statistics, publications, and the like.'

Strangely, this put his adversaries on the defensive. Instead of defending their approach as a way of showing whether there was a scientific consensus about ACC, and hence a clear view of the truth, Anderegg et al (2010a) pointed out that their

> paper did not claim to be proving any scientific truth. On the contrary, we stated that the distribution of experts and their credentials has been a hitherto underconsidered element in the broader climate change discourse, which can lead to media bias.

Here, the phrase '[o]n the contrary' is out of place. To establish that the distribution of scientific papers overwhelmingly supports a particular view of the matter is to suggest where the truth lies. Oreskes goes a good deal further. For her 'a consensus about the reality about global warming and its human causes' – like the consensus that the Sun orbits the Earth – means the scientific debate is 'closed' (Oreskes and Conway 2009, 214-15).

Saffron J. O'Neill (Resource Management and Geography, Melbourne) and Max Boykoff (Science and Technology, Colorado) expressed different concerns from those Bodenstein raised: concerns about the assigning of papers to 'two distinct "convinced" and "unconvinced" camps' and about 'the lack of nuance in defining the terms "climate deniers", "skeptics" and "contrarians"; 'both', they thought, 'oversimplify and increase polarization within the climate debate', though it was with 'climate deniers', 'skeptics' and 'contrarians' that their letter was almost wholly concerned.

The objection raised by O'Neill and Boykoff was easily dealt with. While a differentiation within the UE group 'would be ideal', Anderegg et al agreed - the recognition of differences within both camps being real and important (see, for example, Giddens 2009, 22-27) - the assignment of authors to one or other categories was based on 'objective' criteria 'not our subjective determination of individuals' beliefs and/or motives as would be needed to create O'Neill and Boykoff's distinctions.' The implication, as they saw it, that they had 'labeled' their 'entire UE group…as "contrarians"', was one they rejected; they had 'use[d] "skeptic/contrarian" in the article's introduction and conclusion in accordance with existing social scientific literature…as encouraged by our reviewers in multiple rounds of peer review'. In any event, they might have added, 'contrarians' was a term they could not have deployed, certainly not on any consistent basis, since in the early years covered by their research there was scarcely a 'false consensus' for contrarians to attack. While the 'UE category' may have 'encompass[ed] climate change skeptics, deniers, and contrarians', they had made 'no assumptions about individuals funding, ideology or motives' – assumptions they would have had to make if the label 'contrarian', based on 'frequent financial support from industry or ideological motives', as O'Neill and Boykoff understood the term, were to have been applied (Anderegg et al. 2010b).

The last of the critiques, by Jarle Aarstad (Engineering, Bergen), argued that what the majority of articles showed was not necessarily the truth of the matter; worse still, 'publications in highly cited journals are relatively prone to be incorrect' (Aarstad 2010) - a claim that might have meant

either publications in highly cited journals are prone to be incorrect compared to publications in less highly cited journals or publications in highly cited journals more often than not prove to be incorrect – neither of which Anderegg et al responded to, perhaps because neither is particularly plausible.

Aarstad also argued a variant of a criticism raised by Bodenstein: that young researchers might find that '[i]n the research game, and thereby the papers game, you can't displease your colleagues'; climate researchers were unlikely to believe that they would 'get the same grants, publications, or citations by embracing the minority view'. Pushing up against a '"world view" everyone has embraced…has always been met by massive resistance', he suggested; indeed, research had shown 'that people are willing to accept obvious untruths in the presence of strong group pressure' (Aarstad 2010). Others, implicitly denying the choice to please or displease, talked of a world in which the young were 'educationally brainwashed' (Carter 2010, 240).

Against this, Anderegg et al. argued that 'claims of group-think or conspiracy-driven patterns in climate science fundamentally lack data and therefore credibility'. An argument, ostensibly separate, that 'such unsubstantiated points contribute no substance to the discourse regarding climate science', was little more than embroidery. Far from believing in the power of 'peer pressure' they suggested that

> Any young scientists with a wealth of robust data from well-executed research would become famous by overturning a part of a consensus paradigm. Every young scientist dreams of being the next Darwin or Galileo.

This is not a proposition sceptics are keen to accept. The scientific community is so 'one-eyed', according to Paltridge, that it's 'unlikely' anyone anywhere 'will produce a single scientific result powerful enough to blow the idea of disastrous global warming out of the water (2009, 107-8).

In their original paper, Anderegg et al. noted that while they had 'no a priori basis for assuming any citation (e.g., self-citation rates) or demographic differences (e.g., age effect on publications or citations) between CE and UE groups', their '[p]reliminary evidence' suggested 'these differences would likely favor the UE group' since the 'mean year of receiving a PhD for UE researchers was 1977, versus 1987 for CE researchers, implying that UE researchers should have on average more publications due to an age effect alone'. If there were networks protecting various patches, they might have added, it was unclear that they are any more powerful on the CE side against the unconvinced than on the UE side against the convinced. While the sceptics regularly attack the kind of science favoured by the IPCC, much of it characterised as politically-driven (see, for example, Paltridge 2009, 11-12, 73ff; Plimer 2009, passim; Pielke, Jr. 2010, 20; Aitkin 2011, 68), the CE side has its own very different stories about the way political influences have frustrated attempts to get the best science recognised within the IPCC (see Oreskes and

Conway 2009, 197-215) or circulated outside research institutes funded by sceptical governments (for Australia, see Lowe 2007/2010, 34-8).

The other response of Anderegg et al. was to insist that the 'risk management' faced by climate change policy makers 'presents a more relevant and explicit framework for assessing scientific confidence' around ACC 'than does waiting for history's judgment of truth'. Although they acknowledged as 'self-evident' the truth that 'predominating paradigms', as Aarstad put it, 'can prove to be wrong', this was as much a piece of contextualisation as it was a response. For the question of whether any consensus currently existed was important largely because of the need to guide policy-makers. It was the point they were most keen to stress (Anderegg et al 2010c).

Acknowledging as legitimate the pressure exerted on the scientific community by governments having to 'make serious decisions based on scientific input', Paltridge claims as a 'fact' that 'the vast majority' of scientists 'find it extremely difficult to make any statement at all about questions which do not at this time (and maybe not at any time) have a definite answer' (2009, 92). But this is not what surveys of scientists show. On the contrary in none of the surveys is a disinclination to express a view one way or the other very marked. And, as we have noted, the scientific consensus, defined by Paltridge as the views of 'most scientists' (2009, 91), clearly supports the existence of ACC. Contrary to Paltridge's claim that 'it is certainly not the consensus of the majority of scientists that the actual impact on humans will be significant – or indeed that it will be detrimental' (2009, 106), the clear consensus of scientists surveyed by Bray and von Storch in 1998, 2003 and 2008 was that 'global climate change' would be both. A large and increasing proportion saw 'global climate change as one of the leading problems facing humanity' (1996, 2003) or at least saw its 'potential impact' as 'as one of the leading problems' (2008); 65% in 1996, 69% in 2003, and 83% in 2008 saw it in these terms (Bray and von Storch 2007, fig. 29; 2008, fig. 28). In 2008, three-quarters (78%) of the respondents agreed that 'climate change poses a very serious and dangerous threat to humanity' (2008, fig 22), the same proportion saying 'the mitigation of anthropogenic climate change' would require 'enforced regulation' over 'voluntary action' (2008, fig. 36).

As with the reception of the Oreskes' paper, we might note the criticisms one might have expected to see leveled that weren't. In particular, no objection was raised to the database, biased as it was to those we might describe as active in the climate change debate, either by agreeing with the IPCC's Fourth Assessment or 'strongly dissenting' from it. The explanation may lie in the fact that it was not biased in any obvious way in favour of either set of protagonists.

Conclusion

Most of those working on climate science accept the proposition that anthropogenic climate change explains a large part of the recent rise in global temperatures. Surveys of the literature show this. So, too, do surveys of climate scientists whether broadly or narrowly defined. That

different kinds of evidence point in the same direction suggests that the conclusion is robust. Even many of the sceptics accept that most scientific opinion is presently against them.

None of the criticisms levelled at Oreskes or Anderegg et al. undermine their findings in any substantial way. In the case of the Oreskes paper the main criticisms made by Peiser and recycled by Schulte were withdrawn. In the case of Anderegg et al. the criticisms while more thoughtful are not, on the evidence at hand, persuasive. The consensus about the science reported in the two papers is a consensus about the existence of ACC and – whether stated or not – a consensus that ACC should be a matter of considerable concern.

While the consensus is nothing less than this it is equally important to note that it is not a lot more than this. It is not a consensus about specific aspects of climate science, in favour of particular actions that need to be taken, or about 'alarmist' or 'panicky predictions' – unless of course to believe in the existence of ACC as a serious issue for public policy is itself to be 'alarmist' or 'panicky' (the position adopted by Monckton 2007, 5, 13, 20; see also Paltridge 2009, 94, 107). Oreskes cautions that the proposition that global warming is 'the greatest global challenge facing mankind' is 'the sort of extreme statement that most involved with the IPCC would not support' (Oreskes and Conway 2009, 206).

The survey evidence is similarly nuanced. No more than a third (32%) of the respondents in the 2008 survey conducted by Bray and von Storch wanted to 'attribute recent climate related disasters to climate change' (2008, fig. 24), not many more than a third (36%) of the respondents in their 1996 survey – 44% in 2003 - agreed that if 'climate change' were to occur it would 'occur so suddenly that a lack of preparation could result in devastation of some areas of the world' (Bray and von Storch 2007, fig. 33), and while in 2008 a third (37%) anticipated 'some positive effects' for the country in which they lived nearly a half (45%) anticipated 'positive effects' for 'other parts of the world' (Bray and von Storch 2008, figs. 27a, 27b). Respondents across each of their surveys were also alert to where the science was strong and where it was relatively weak; for example, majorities agreed that the science was well enough developed in relation to 'green-house gasses emitted from anthropogenic sources' (59-65%) and 'surface albedo' (57-64%) but not in relation to 'turbulence' (23-32%), 'land surface processes' (26-42%) or 'sea-ice' (33-41%); see Bray and von Storch (2007, figs 11-15; 2008, figs 15a-15e).

However, respondents were not complacent. In 2008, 78% agreed that 'climate change poses a very serious and dangerous threat to humanity'. Similar proportions agreed that 'the potential impact of global climate change' was 'one of the leading problems for humanity in terms of social and economic issues (77%) and 'for eco-systems (i.e. species extinction, land degradation, etc.)' (83%); see Bray and von Storch (2008, figs 22, 28a, 28b). And few agreed (15% in 1996 and 15% in 2003) that there was 'enough uncertainty about the phenomenon of global warming' that there was 'no need for immediate policy decisions' (Bray and von Storch 2007, fig. 34).

In many ways the real debate is about three things. First, about how wide the consensus is. Although Oreskes concedes that among climate scientists who have written on 'global climate change' there might be a few non-believers, her own study threw up little evidence of the ACC thesis being challenged. That things haven't changed since is suggested by the work reported by Doran and Zimmerman (2008; Zimmerman 2008 for the detail) who surveyed members of the American Geological Institute in 2008 and found that of 'those who listed climate science as their area of expertise' and who had 'published more than 50% of their recent peer-reviewed papers on the subject of climate change', 97% agreed that 'human activity is a significant contributing factor in changing mean global temperatures' (see also the survey of scientists involved in the IPCC 2007 report reported by Milloy 2007, a sceptic). However, the work of Anderegg at al., who uncovered 472 scientists who had 'signed statements strongly dissenting from the views of the IPCC', compared with 903 who had supported it, and the survey research of Bray and Storch, who in 2003 found 29% disagreeing that 'climate change is mostly the result of anthropogenic causes' (see also the survey reported by Lichter 2008), suggests that while there is a scientific consensus in favour the view that ACC accounts for most of the global warming, with upwards of two-thirds of those who publish in the area supporting it, it is a consensus that falls well short of unanimity; there are well-credentialed scientists, not all of them sponsored by the petroleum industry, who reject it (see, for example, Spencer 2008, Paltridge 2009 and Carter 2010).

The second area of contention is over whether the science being done by those who are convinced about ACC is better or worse than the science being done by those who are unconvinced. The evidence presented by Anderegg et al., as well as that presented by Doran and Zimmerman, supports the view that the more widely published the climate scientist, the more widely cited, or the more a climate scientist identifies as a climate scientist, the more likely they are to affirm the existence of ACC. None of this will change the minds of sceptics who think the work of 'the world expert in the field' (Paltridge 2009, 101 on Stephen McIntyre and the 'hockey-stick' debate; emphasis in the original) has been ignored; the only thing likely to do that - and rightly so - is detailed refutation of the work (see, Washington and Cook 2011, 56-7, for a defence of the 'hockey-stick' data, and compare Montford 2010), though perhaps not even then.

It is certainly possible that a sort of closed-shop is at work, with the majority of those who side with the orthodoxy applying tougher standards to those that challenge orthodox views than they apply to those like themselves who work within the consensus, and that the differential publication and citation figures reflect this. But those who insist that this is the case provide no systematic evidence for it. Of course, arguments from first principles apart (see, for example, Guston 2006, 383, for a Kuhnian gloss), the operation of a closed-shop is always difficult to show.

The third area of debate is whether the attempt to establish the existence of a consensus is little more than an attempt to silence the minority, either by obscuring truths established by the minority or by bringing to a premature end what sceptics of various kinds see as an on-going

debate. That a motive of this kind is everywhere at work seems unlikely – or if ubiquitous then sometimes misplaced. The initial survey conducted in 1996 by Bray and von Storch reported no such consensus with less than half (40%) of the respondents agreeing that 'climate change is mostly the result of anthropogenic causes' (Bray and von Storch 2007, fig. 30). Moreover, work of Bray and von Storch that has nothing to do with their survey work, is often cited favorably by those for whom the language of consensus is deeply unscientific (see, for example, Carter 2010, 157, 199; see also Montford 2010, 387, 481, Pielke Jr. 2010, 204).

Motives to one side, a process may be at work whereby the existence of a widely recognised majority view makes minority views less likely to be formulated and, via 'a spiral of silence' (Noelle-Neumann 1993), less likely to be expressed; peer-pressure applies in many social situations and it may well be that the dynamics among scientists are no different. How far such pressures affect the search for truth remains an open question, but on the evidence produced by Anderegg et al. it would be easy to exaggerate the extent to which the work of sceptics has been marginalised in the literature.

If the dangers inherent in the language of consensus are clear the dangers of rejecting the term out of hand should be equally clear. To claim, as some sceptics have done (e.g. Plimer 2009, 449-50), but not all (cf. Monckton 2007), that the language of consensus is the language of politics not science – 'authoritarian' or 'collectivist' politics at that - is misleading. The principal reason interested bystanders' – from parliamentarians to the general public – have for believing that certain things are very likely to be true is precisely that very few in the relevant field doubt them. And in public policy risk management is not so much a matter of working on scientific certainties as on modelling a range of probabilities (Barratt, Pearman and Waller 2010, 146).

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