STATEMENT TO THE ENVIRONMENT AND PUBLIC WORKS COMMITTEE OF THE UNITED STATES SENATE

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I am a Professor of Climatology at the University of Delaware and I served as the Delaware State Climatologist from 2005 to 2011. I also am an adjunct faculty member in the Department of Agricultural Economics & Statistics and the Physical Ocean Science and Engineering Program. I received a B.A. in Mathematics and Geography, a M.S. in Geography, and a Ph.D. in Climatology, all from the University of Delaware. I served on the faculty of the University of Oklahoma and Louisiana State University before returning to the University of Delaware in 1999. I was part of the US delegation that negotiated a protocol for the first climate data exchange program with the Soviet Union in 1990. I am recognized as a Certified Consulting Meteorologist by the American Meteorological Society and was the recipient of the *2002 Boeing Autometric Award* in Image Analysis and Interpretation by the American Society of Photogrammetry and Remote Sensing.

I would like to thank the Chair and the Committee for the privilege to offer my views and my thirty years of experience on climate change from the perspective of a climatologist. My expertise lies in statistical methods in climatology, particularly as it relates to the hydrologic cycle – precipitation and soil moisture. For my dissertation, I developed the first digital and gridded global precipitation and air temperature dataset that specifically incorporates biases arising from the precipitation gage measurement process. This database is still used today in climatology as a standard against which climate model-derived fields and regional assessments are compared. I also have published several important articles that discuss the impact of precipitation variability on soil moisture in regional and global studies. In the following discussion, I will address the potential impact of climate change on agriculture and relay some of my pressing concerns that are related to the treatment of climate scientists who do not agree with the anthropogenic global warming disaster scenarios.

1. Global Warming and Agricultural Impacts

One of the important questions raised by the response of increasing atmospheric carbon dioxide concentrations is the possible impacts on agriculture, aquaculture, and commercial/recreational fishing. Considering that CO_2 is food for plants and animals, this is seen as a positive and any potential negative effects are minimal. But if global surface air temperatures do rise for any reason, this will undoubtedly increase the length of the growing season which, in turn, will enhance the amount and diversity of crops that can be grown. Moreover, it will allow for more areas of the planet to be farmed, primarily in the Northern Hemisphere, thereby increasing crop productivity. Billions of acres of land in northern Canada and Russia could become cultivable. The limiting factor, however, is the moisture availability to plants as agriculture in much of the world is restricted by water availability both from precipitation and surface/groundwater reserves.

A discussion of the possible results of soil moisture availability in a warmer world depends on a complicated interaction of two factors – changes in the precipitation climatology and increases in evapotranspiration (the combined effect of soil evaporation and plant transpiration). The impacts of these two factors are opposite in sign; precipitation, when it occurs, is likely to increase but the potential for evapotranspiration also is likely to increase, both due to the increase in the saturation vapor pressure as a function of increasing air temperature. The question then is which dominates – does the increase in precipitation compensate for the increase in the evapotranspiration demand or does the increase in air temperature reduce soil moisture reserves such that droughts will become more likely? Complicating this discussion is the fact that atmospheric circulation changes may affect the patterns of precipitation so that some areas may become more drought-prone while others may become less so. **Pinpointing the exact geographical areas for which drought/increased rainfall are likely to occur lie far beyond our technology for the foreseeable future.**

To answer the questions, climatologists employ two methods. In one, historical patterns and trends over the last century are extrapolated to provide a forecast of what might happen in the future. From the demise of the Little Ice Age – a relatively cold period between about 1300 and 1850 A.D. (Soon *et al.* 2003) that is concomitant with decreased solar output – to the late 1990s, air temperatures increased about 0.6°C (~1.1°F). We can use this rising trend in air temperature to make prognostications as to what we might expect from a warming world in the future. The second method involves climate models – mathematical/statistical representations of the climate system. These models are used to simulate future climate scenarios from which patterns of climate change are inferred. We will examine the results using both of these methods.

1.1 Historical Patterns and Trends in Drought

Several analyses have focused on patterns and trends associated with drought. Hao *et al.* (2014) used satellite analysis to examine global patterns of drought from June 1982 through December 2012 (Figure 1). Only a slight decrease in abnormally dry and moderate drought conditions has occurred, though it is not statistically significant. Note particularly the increase in global drought

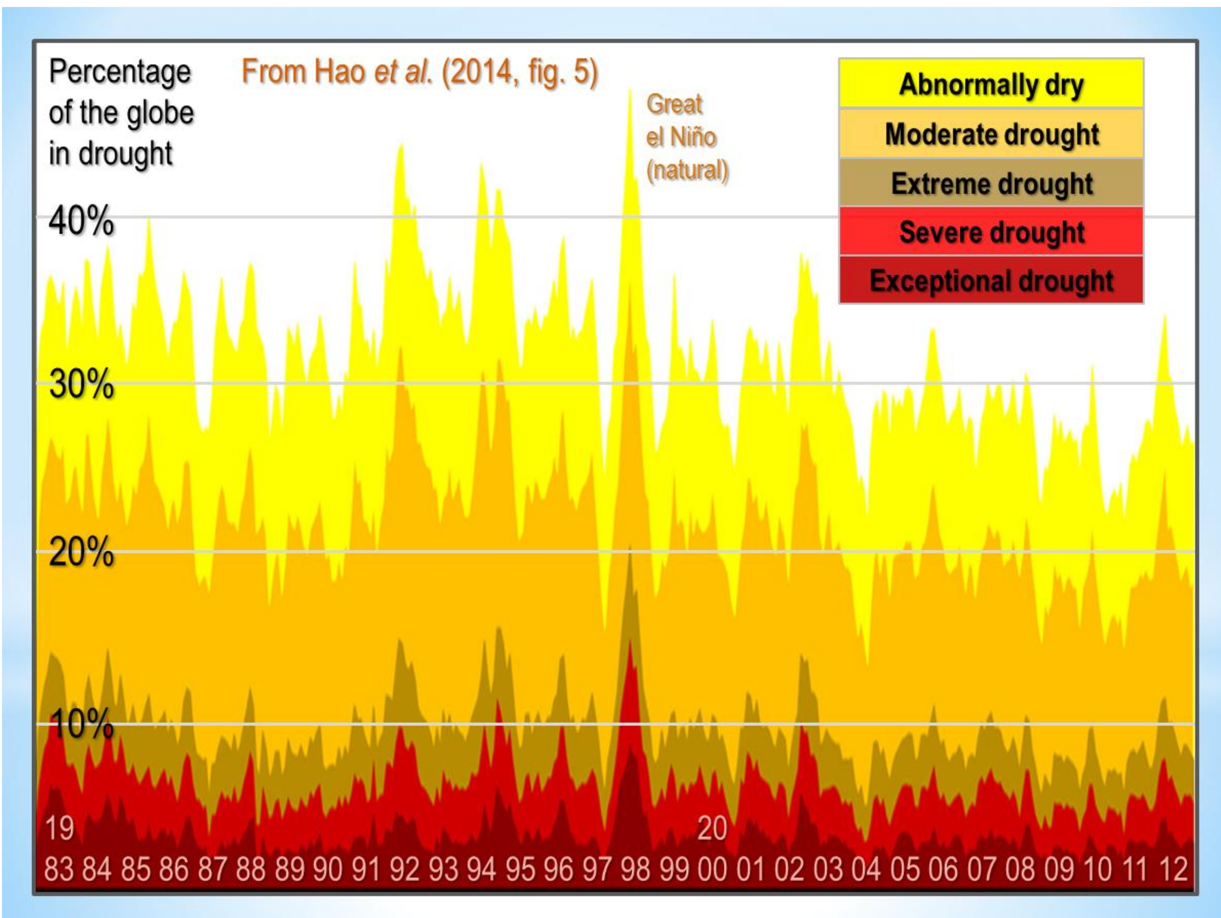

Figure 1: Fraction of the global land in D0 (abnormally dry), D1 (moderate), D2 (severe), D3 (extreme), and D4 (exceptional) drought condition (adapted from Figure 5 of Hao *et al.* (2014).

in 1998 resulting from the rather strong naturally-occurring El Niño of that year. Patterns in precipitation for the Twentieth Century show no observable trend over the entire period of record for either the globe or for either hemisphere (New *et al.* 2001 – Figure 2). Regionally, the only statistically significant pattern occurs for the upper latitudes of the Northern Hemisphere (where snowfall is better measured in the latter portion of the record due to better snow-gage instruments) and for the lower latitudes of the Northern Hemisphere (dominated by the Sahel region in Africa, where overgrazing has substantially changed the landscape and, consequently, the precipitation climate of the region). Sheffield *et al.* (2012) concur with the results of Hao *et al.* (2014): "more realistic calculations…suggest there has been little change in drought over the past 60 years."

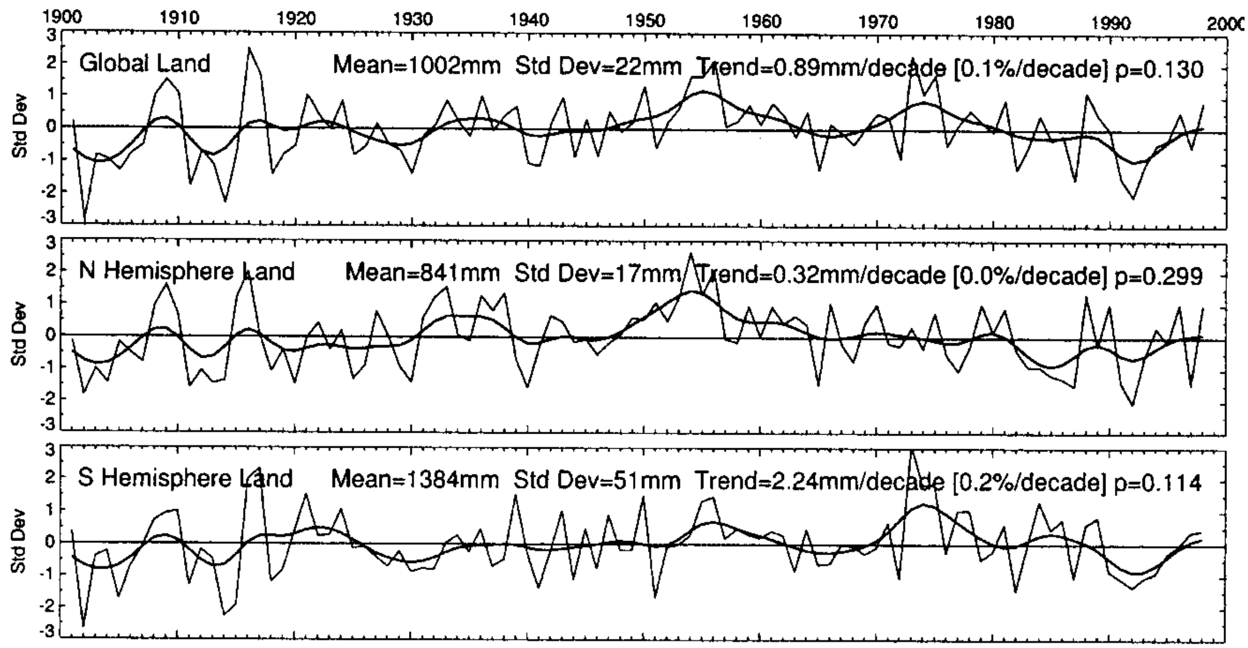

Figure 2: Precipitation for the globe and both hemispheres for the Twentieth Century (from Figure 3 of New *et al.* (2001). *p*-values indicate that none of these trends are statistically significant.

Much more research, however, has been conducted in the United States where observations are more dense and reliable. Generally, precipitation tended to increase over much of the United States between 1895 to 2012, although with much lower certainty in the record prior to 1950 (Vose *et al.* 2014). Groisman and Knight (2008), however, argued that "prolonged dry episodes" of precipitation have increased over the southwestern United States. McCabe *et al.* (2010) addressed this issue by examining a more complete dataset and concluded that there is "little evidence of long-term positive trends in dry event length in the southwestern United States." We concluded that El Niño and La Niña events and the Pacific Decadal Oscillation are largely responsible for the variability in trends in dry event length in the southwestern United States. Station network limitations and the treatment of missing data adversely affected the results of Groisman and Knight (2008).

Again, however, the main concern focuses on the change in precipitation relative to the change in evapotranspirative demand. Senate testimony by John Christy of the University of Alabama in Huntsville (Christy 2012) has shown that the daily all-time record high temperatures from 970 weather stations with at least eighty years of record peaked in the 1930s and the numbers since 1955 have not increased (Figure 3). This trend also is consistent for a subset of stations in the central United States and along the US West Coast (Christy 2012).

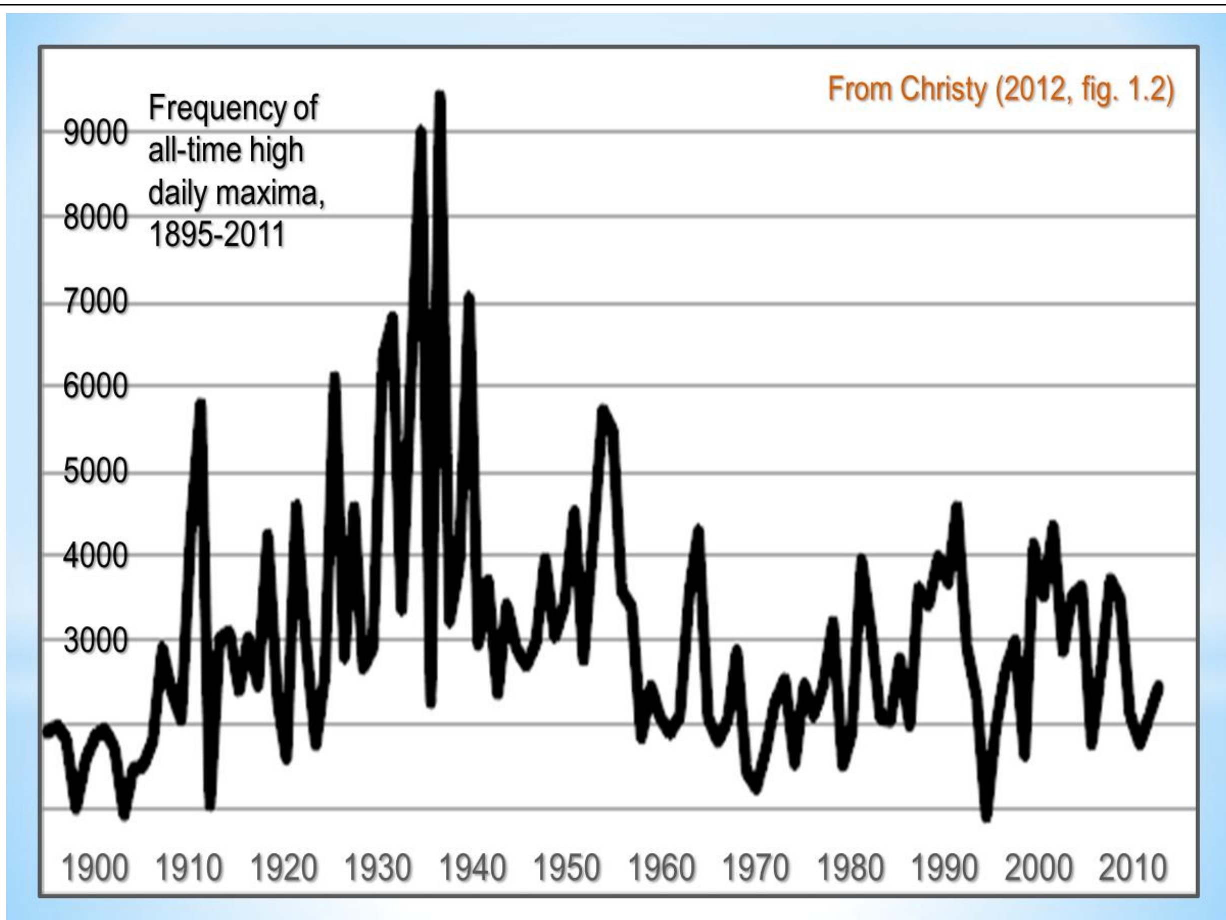

Figure 3: Frequency of all-time high maximum daily air temperatures, 1895 to 2011, at 970 USHCN stations with at least eighty years of observations (from Figure 1.2 of Christy 2012).

However, our primary concern in agriculture is the statistics of drought – changes in its intensity, frequency, and duration. Woodhouse and Overpeck (1998), comparing drought variability in the Central United States over the last two millennia concluded, "The droughts of the 20th century have been characterized by moderate severity and comparatively short duration, relative to the full range of past drought variability." A plot of the Palmer Drought Severity Index, averaged for the contiguous United States, shows considerable variability from 1900 to 2012 with the droughts of the 1930s standing out, but without any long-term trend. This pattern has also been noted by the US Climate Change Science Program (2008) – "When averaged across the entire United States, there is no clear tendency for a trend…long-term trends (1925-2003)…show that droughts have, for the most part, become shorter, less frequent, and cover a smaller portion of the United States over the last century."

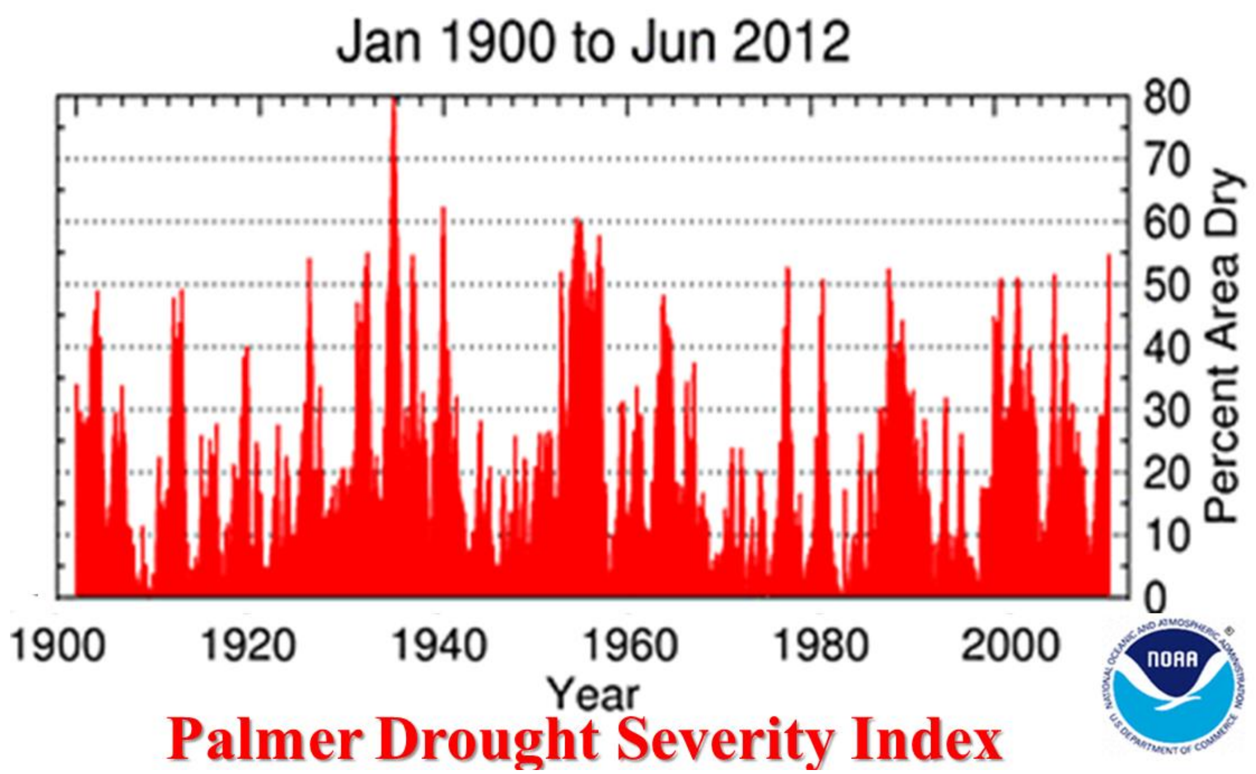

Figure 4: Percentage of the United States in moderate or extreme drought from January 1900 to June 2012 (from the National Climate Data Center, Asheville NC).

Regionally, there have been numerous studies and their results have been similar. For example, Bekker *et al.* (2014), using a 576-year reconstruction of flood conditions, concluded that droughts of greater magnitude, duration, and intensity have occurred previously in Utah. Knapp *et al.* (2002) found that the period since 1950 was "anomalous in the context of this [500-year] record for having no notable multiyear drought events." For the Idaho, Montana, and Wyoming region, Wise (2010) argued that "the instrumental record (*i.e.*, since the late 1800s) does not contain a drought of the extent seen in the mid-1600s." Gray *et al.* (2004) found that dry conditions in the Sixteenth Century (*i.e.*, during the Little Ice Age) were greater in magnitude and duration than anything seen in the Twentieth Century for the same region.

Andreadis and Lettenmaier (2006) concluded that Midwestern droughts "have, for the most part, become shorter, less frequent, less severe, and cover a smaller portion of the country over the last century." Even the Special Report of the IPCC (IPCC 2012) concluded "…In some regions droughts have become less frequent, less intense, or shorter, for example, in Central North America." Indeed, NOAA scientists (NOAA 2013) concluded that the 2012 Central Great Plains drought "resulted mostly from natural variations in weather."

In the Eastern United States, Pederson *et al.* (2012) concluded that recent droughts are not unprecedented over the last 346 years, with more frequent droughts occurring between 1696 and 1820 A.D. during the colder conditions of the Little Ice Age. Indeed, Quiring (2004) concurred that "…the recent growing-season moisture anomalies that occurred during 2002 and 2003 can only be considered rare events if they are evaluated with respect to the relatively short instrumental record (1895-2003)" and that condition during the 16th Century were longer and more severe.

My overall conclusion is that droughts in the United States are more frequent and more intense during colder periods. Thus, the historical record does not warrant a claim that global warming is likely to negatively impact agricultural activities.

1.2 Model-derived Trends and Patterns of Drought

Global climate models (or General Circulation Models – GCMs) are only as good as their ability to simulate precipitation. They are descriptions of the full three-dimensional structure of the Earth's climate and often are used in a variety of applications, including the investigation of the possible role of various climate forcing mechanisms and the simulation of past and future climates. There are, however, several important issues to remember with GCMs. First, they are limited by our incomplete understanding of the climate system and how the various atmospheric, land surface, oceanic, and ice components interact with one another. They are further limited by our ability to transform this incomplete understanding into mathematical representations. We may have a general feel for the complex interrelationships between the atmosphere and the oceans, for example, but expressing this understanding in a set of mathematical equations is much more difficult. Second, GCMs are limited by their own spatial and temporal resolutions. Computational complexity and finite restrictions on computing power reduce GCM simulations to coarse generalities. As a result, many small-scale features, which may have significant impact on the local, regional, or even global climate, are not represented. Thus, we must recognize that GCMs, at best, can only present a gross thumbnail sketch. Regional assessments over areas encompassing many GCM grid cells are the finest scale resolution that can be expected. It is inappropriate, and grossly misleading, to select results from a single grid cell and apply it locally. It cannot be over emphasized that GCM representations of the climate can be evaluated at a spatial resolution no finer than large regional areas, seldom smaller than a region defined by a square several hundred miles (at least several GCM grid cells) on a side. Even the use of "nested grid models" (models which take GCM output and resolve it to finer scale resolutions) does not overcome this limitation, since results from the GCM simulation drives such models and no mechanism is available to feedback the results of such finer-scale models to the GCM.

Another limitation in GCMs is that given the restrictions in our understanding of the climate system and its computational complexity, some known phenomena are simply not reproduced in climate models. Hurricanes and most other forms of severe weather (*e.g.*, nor'easters, thunderstorms, and tornadoes) simply cannot be represented in a GCM owing to the coarse spatial resolution. Other more complex phenomena resulting from interactions among the elements that drive the climate system may be limited or even not simulated at all. Such indicators should be flags that something fundamental is lacking in the GCM. These phenomena should be produced in the model as a result of our specification of climate interactions and driving mechanisms; their absence indicates a fundamental flaw in our understanding of the climate system, our mathematical representation of the process, the spatial and temporal limitations imposed by finite computational power, or a combination of the above.

An assessment of the efficacy of any climate model, therefore, must focus on the ability of the model to simulate present climate conditions. If a model cannot simulate what we know to be true, then it is unlikely that model prognostications of climate change are believable. However, a word of caution is warranted. It is common practice to "tune" climate models so that they better resemble present conditions. This is widely acceptable, because many parameters in GCMs cannot be specified directly and their values must be determined through empirical trial-and-error. However, this raises the concern that a GCM may adequately simulate the present climate, not because the model correctly represents the processes that drive the climate; but rather, because it has been tuned to do so. Thus, the model may appear to provide a good simulation of the climate, when in fact the model may poorly simulate climate change mechanisms. **In other words, a GCM may provide an adequate simulation of the present-day climate conditions, but it does so for the wrong reasons.** Model efficacy in simulating present-day conditions, therefore, is not a guarantee that model-derived climate change scenarios will be reasonable. To address this question, modelers often employ simulations of past climates, such as the Holocene or the Pleistocene, to see if the model provides the kind of climate that we can infer existed during such epochs. Of course, our knowledge of pre-historical climate conditions is tenuous and extremely crude, which limits the utility of such evaluations.

A final limitation in climate modeling is that in the climate system, everything is interconnected. In short, anything you do wrong in a climate model will adversely affect the simulation of every other variable. The most problematic variable is precipitation. Precipitation requires moisture in the atmosphere and a mechanism to cause it to condense (causing the air to rise over mountains, by surface heating, as a result of weather fronts, or by cyclonic rotation). Any errors in representing the atmospheric moisture content or precipitation-causing mechanisms will result in errors in the simulation of precipitation. Thus, GCM simulations of precipitation will be affected by limitations in the representation and simulation of topography, since mountains force air to rise and condense to produce orographic (mountain-induced) precipitation (*e.g.*, the coastal mountain ranges of Washington and Oregon). Incorrect simulations of air temperature also will adversely affect the simulation of precipitation since the ability of the atmosphere to store moisture is directly related to its temperature. If winds, air pressure, and atmospheric circulation are inadequately represented, then precipitation will be adversely affected since the atmospheric flow of moisture that may condense into precipitation will be incorrect. Plant transpiration and soil evaporation also provide moisture for precipitation; therefore, errors in the simulation of soil moisture conditions will adversely affect the simulation of precipitation. Simulation of clouds solar energy reaching the ground will affect estimates of surface heating which adversely affects the simulation of precipitation. Even problems in simulating oceanic circulation or sea ice concentrations will affect weather patterns, which affect precipitation simulations.

Equally important is the fact that inaccuracies in simulating precipitation, in turn, will adversely affect the simulation of virtually every other climate variable. Condensation releases heat to the atmosphere and forms clouds, which reflect energy from the sun and trap heat from the Earth's surface – both of which affect the simulation of air temperature. As a result, this can affect the simulation of winds, air pressure, and atmospheric circulation. Since winds drive the circulation of the upper layers of the ocean, the simulation of ocean circulation also is affected. Air temperature conditions also contribute to the model simulation of sea ice formation, which would be adversely affected. Precipitation is the only source of soil moisture; hence, inadequate simulations of precipitation will adversely affect soil moisture conditions and land surface hydrology. Vegetation also responds to precipitation availability so that the entire representation of the biosphere can be adversely affected. Clearly, the interrelationships among the various components that comprise the climate system make climate modeling difficult. Keep in mind, however, that it is not just the long-term average and seasonal variations that are of interest. Demonstrating that precipitation is highest over the tropical rainforests and lowest in the subtropical deserts is not enough. Climate change is likely to manifest itself in small regional fluctuations. Moreover, we also are interested in intra-annual (year-to-year) variability. Much of the character of the earth's climate is in how it varies over time. A GCM that simulates essentially the same conditions year after year clearly is missing an important component of the earth's climate. Thus, the evaluation of climate change prognostications using GCMs must be made in light of the model's ability to represent the holistic nature of the climate and its variability. **In sum, the simulation of precipitation, and subsequently soil moisture, is adversely affected by inaccuracies in the simulation of virtually every other climate variable while, in turn, inaccuracies in simulating precipitation adversely affect virtually every other variable in the model.**

It should be noted that GCMs are not weather prediction models. Their utility is not in predicting, for example, whether it will rain in southern England on the morning of July 14, 2087. Rather, we are interested in determining whether the probability of precipitation will be substantially different from what it is today – in both the frequency and intensity of precipitation events. In general, we want to know whether the summer of 2087 is likely to be wetter or drier than present conditions, and by how much. As such, GCMs are only used appropriately to address the likelihood of changes over large spatial and temporal scales -- assessing changes for specific dates or locations are beyond the scope of GCM utility.

But this is my biggest concern. If a climate model simulates an increase in precipitation for the near or distant future, I want to know why. In particular, I want to verify that it is because a specific precipitation-producing mechanism has changed. Are there more tropical storms or nor'easters simulated? More frontal precipitation? Is there more convective activity from surface heating that leads to more rising air? Or has the atmospheric circulation changed such that orographic precipitation is enhanced?

Unfortunately, this is where over-reliance on GCMs forecasts can betray us. In these models, precipitation is produced almost exclusively from a single mechanism – surface convection – and is often termed "popcorn precipitation" since it occurs over large regions and relatively frequently (see Zolina 2014)). When models are averaged over seasons, the classic pattern of global precipitation emerges with a moist equatorial region, decreased precipitation in the Subtropics, and increased precipitation in mid-latitudes that tapers off with colder temperatures toward the poles. While this may *appear* correct in the aggregate, it has achieved its apparent success without properly simulating the mechanisms that create precipitation in the real world. How possibly, therefore, can the models make accurate prognostications of precipitation when they do not simulate correctly the mechanisms that drive precipitation? And if precipitation is not modeled properly, how then can soil moisture estimates be used to prepare farmers for an uncertain future?

Stephens *et al.* (2010) identifies this problem with three state-of-the-art climate models and numerical weather prediction models. Using high resolution CloudSat observations over the oceans (where precipitation is more uniform spatially), they demonstrate that the differences between the models and the observations are much greater than observational and averaging errors. They conclude "the general tendency is for models to produce precipitation that is far too frequent, especially in midlatitudes" (*i.e.*, the United States). Note that tropical precipitation is largely convective (although some stratiform precipitation does occur – Janowiak *et al.* 1995) but that in midlatitudes, precipitation arises from a variety of mechanisms. Instead of simulating frontal passages and organized weather systems, the models exhibit "popcorn precipitation" where it rains far too often. As a consequence of having it rain too frequently, the intensity of modeled precipitation is that when it occurs, its intensity is much lower than observed. Thus, the total precipitation is reasonable but its distribution (frequency and intensity) is grossly in error. Even models that have spatial resolutions as fine as 7-14 km (4.4-8.8 mi) exhibit these problems. When averaged to seasonal averages for the globe, the models do remarkably well. However, they achieve this level of success for the wrong reasons. Regionally, the GCMs "tend to produce too much precipitation over the tropical oceans and too little in midlatitudes". Moreover, this is where soil moisture is greatly affected – models that rain too frequently with lighter amounts will necessarily overestimate soil moisture conditions because soil moisture responds not just to the amount of precipitation but is very dependent on its timing.

As Dr. John Christy demonstrated in his Senate Testimony (Christy 2012), the March-to-July precipitation, as simulated by most climate models, exhibits considerable variability between the models but does not exhibit a long-term trend. For the Southeastern United States (Figure 5, top), the models vary from an average of less than 15 inches to more than 25 inches and most models tend to underestimate the observed precipitation from 1890 to 2012. Similarly, the models also vary from below 15 inches to more than 24 inches for the Midwestern United States (Figure 5, bottom) although the models tend to be wetter than observations.

If models indicate that precipitation is not forecast to change over this century, how do models suggest an adverse impact on agriculture will occur? Models suggest that air temperatures will increase substantially over the next century, rising by as much as 6°C (10.8°F). This indicates that the evapotranspirative demand will increase substantially and result in lower soil moisture conditions and hence more droughts. However, these models have significantly overestimated the warming of the last fifteen years (Figure 6) such that they command little confidence.

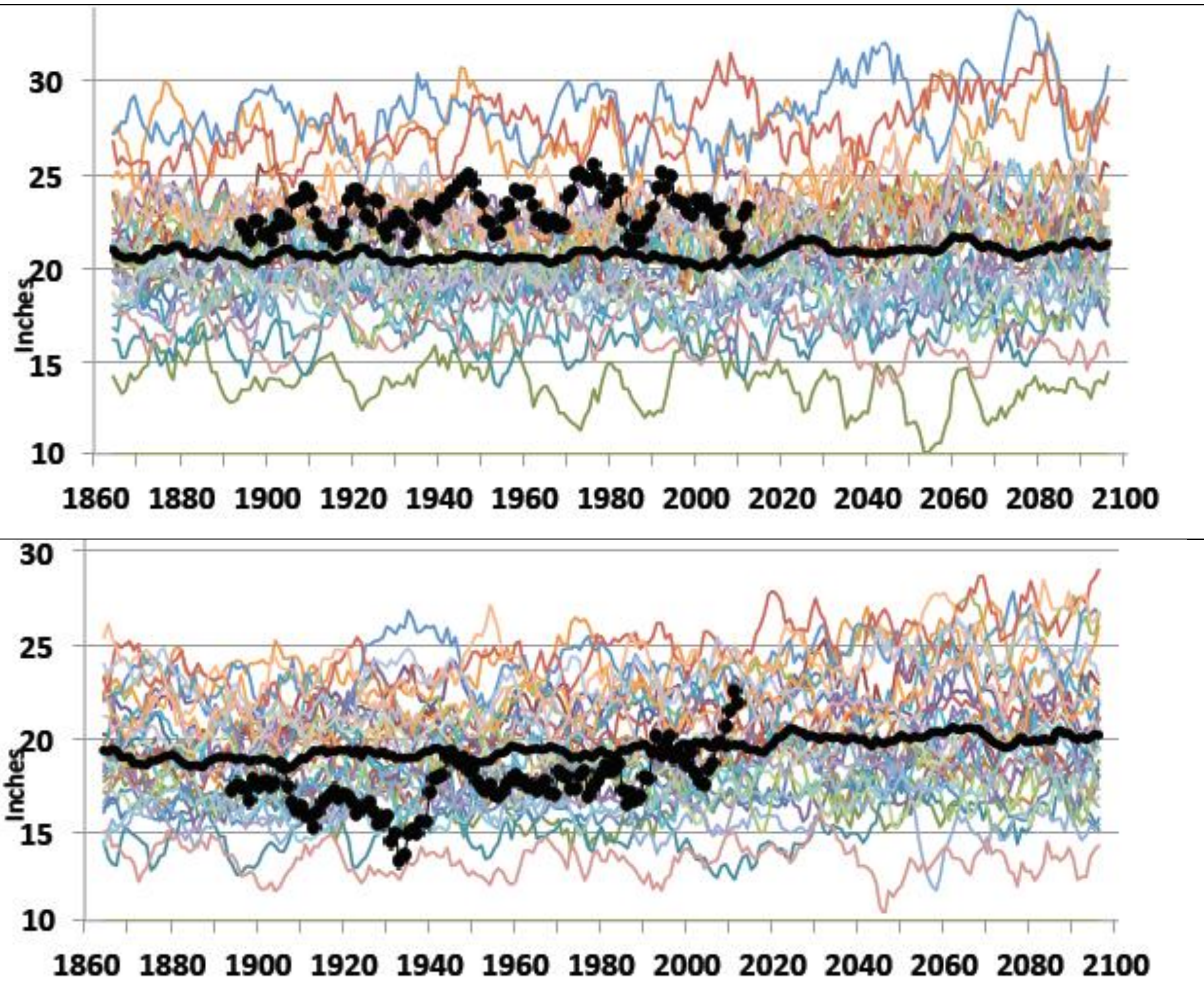

Figure 5: Southeastern (top) and Midwestern (bottom) United States precipitation as measured from observations (black dots) and models (colored lines; median in solid black) for 1860 to 2100 (from Figures 2.2 and 2.3 in Christy 2012).

The consistent and substantial over-predictions of the general-circulation climate models are reflected in those of the Intergovernmental Panel on Climate Change, which, in 1990, predicted that near-term warming would occur at a rate exactly double what has actually occurred. Furthermore, none of the models predicted that for 17 years 9 months, or more than half the entire satellite temperature record, there would be no global warming at all (Figure 7).

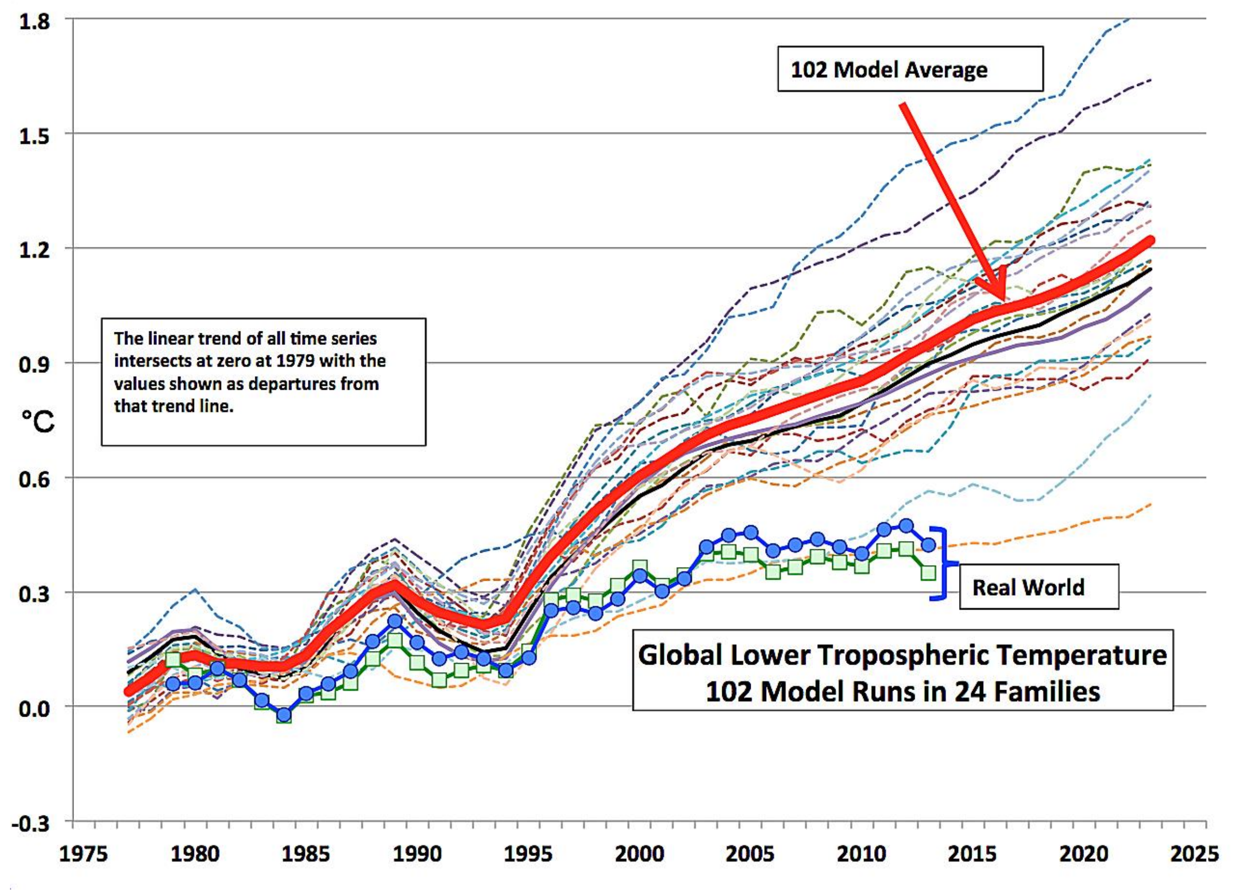

Figure 6: Global surface air temperature (in °C) as simulated by 102 climate models runs (colored, dashed lines). The solid red line is the model average, the circles are the balloon observations, and the squares are the satellite measurements (from Pielke Sr. 2014).

As Dirmeyer (2014) argues, "The problem is that coupled land-atmosphere models used for weather and climate forecasting and research have never been thoroughly validated in terms of their simulation of the coupled processes that provide predictability." Even if the land surface model was perfect, it will provide bad simulations if forced by "an atmospheric model with serious systematic biases or inadequately represented physical processes" (see also Steinhaeuser and Tsonis 2014). Given the limitations of the models not only in predicting global air temperatures but also in estimating precipitation and soil moisture conditions, it seems that a more reasonable approach is not to rely on the model prognostications; but rather, to focus on policies that allow for adaptation to the observed variability in precipitation and soil moisture. **Droughts that have happened in the past are likely to occur again, and with likely similar frequencies and intensities; thus, preparation for their return is a better strategy than trying to mitigate them through draconian CO_2 emission control policies.**

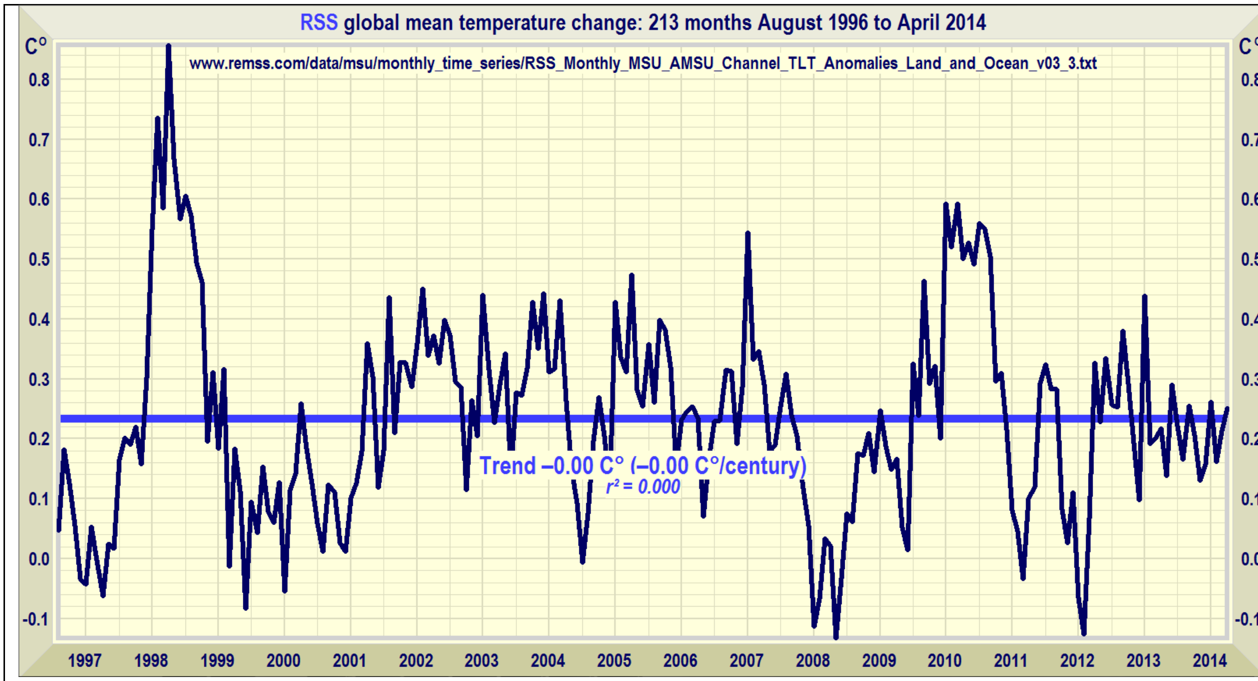

Figure 7: Monthly global mean surface air temperature anomalies monitored by satellite (RSS), August 1996 to April 2014. For 213 months, or more than half the entire satellite record, the least-squares linear-regression (thick bright blue line) has been zero.

1.3 The Scientific Method versus Post-Normal Science

The scientific method has long been the 'gold standard' among scientists. It is the empirical evidence that separates science from mythology and is the key to finding scientific truth (Legates *et al.* 2013). Indeed, it is the evaluation of theories with observations that have trumped appeals to authority or consensus or the longevity of a theory (Legates *et al.* 2014). As Legates *et al.* (2013) argued, "results from climate models are often erroneously posited as observations themselves or even data and even when they diverge considerably from the real observations, they are used to drive theory construction…results from climate models should be used with extreme care and not be taught as scientific fact."

As a response to policy-making when a 'solution' is demanded immediately and the facts are obscured by error, widely divergent views exist, models are inherently uncertain, Post-Normal Science emerges where 'science by consensus' reigns. It has been strongly argued that even in its early days, the Intergovernmental Panel on Climate Change abandoned the scientific method in favor of this new paradigm (Saloranta 2001, Legates *et al.* 2013). This inherently morphs the role of the scientist from an impartial observer and seeker of the truth to one who dons the hat of an advocate. This is where the so-called 'consensus arguments' arise where an appeal to some very large percentage of scientists appears to give credibility to a particular viewpoint. Most of these consensuses are contrived (see Legates *et al.* 2014) and serve to push an agenda that diverges widely from truth-seeking. The scientific method has been abandoned by many in the climate change discussion with an appeal to the masses through an imaginary consensus of scientists. This has greatly undermined both the quest for truth in this debate and the respect the general public has for scientists who advocate for anthropogenic global warming disaster scenarios.

2. The Silencing of the Dissenters

In my Senate Testimony in 2003 regarding the so-called "Hockey Stick" graph of global air temperature (Legates 2003), I concluded with the statement

> I'm sorry that a discussion that is best conducted among scientists has made its way to a United States Senate committee. But hopefully it has become evident that a healthy scientific debate is being compromised and that only by bringing this discussion into the light can it be properly addressed.

At that time, an attack had been made on the scientific process. Editors at two journals were harassed to the extent that an abrogation of their commitment to reviewer confidentiality had been demanded of them. One of the journals, *Climate Research,* was threatened with an organized boycott and the Director of its parent organization, who first evaluated the situation and exonerated the managing editor, recanted in the face of this boycott. The newly appointed Senior Editor had moved to bar two scientists from future publication in *Climate Research* – without a hearing and without even an accusation of fraud or plagiarism.

I would like to provide you with an update on how the state of science has progressed in the intervening eleven years as it regards climate change. In 2009, a release of documents from the University of East Anglia in the United Kingdom (known colloquially as 'ClimateGate') shed light on how the scientific process was being subverted. With respect to me personally, I learned that in 2001, I had been denied publication of an important rebuttal due to collusion between an author and an editor. In the Second Assessment Report (SAR) of the IPCC, the phrase "balance of evidence suggests a discernible human influence on global climate" had been inserted, and that five separate statements to the contrary had been removed by a single author. Dr. Robert E. Davis and I examined the citations given in support by Dr. Ben Santer, Dr. Thomas Wigley, and their colleagues. We had found that the statistic they used to make their conclusions was seriously flawed and published our results. Wigley and his colleagues published a rebuttal and we were denied a response since "we did not add anything significant to the discussion." I assumed we had not done enough to sway an impartial editor.

But in an e-mail, Dr. Wigley explained how he had engineered his rebuttal and suggested it be used as a template for others. He indicated he had contacted the editor, complained that any such publication criticizing his research should have been cleared by him first, and the two agreed that his rebuttal would be treated as a new submission and any response Davis and I made were to be squelched by the editor. We had always suspected such events might have occurred but it took the ClimateGate documents to provide the proof.

But these issues were to pale in comparison to what was about to happen. On December 16, 2009, I received a letter that, due to the ClimateGate revelations and pursuant to the Delaware Freedom of Information Act (FOIA), Greenpeace requested my "email correspondence and financial and conflict-of-interest disclosures" that were "in the possession of or generated by the Office of the Delaware State Climatologist" from January 1, 2000 regarding 'global climate change' and containing any of 22 additional keywords.

The Delaware FOIA statute is fairly terse with respect to the University. It simply states that the University of Delaware is exempt from State FOIA except for the conduct of the Board of Trustees of the University and documents relating to the expenditure of public funds. Although during my tenure as the State Climatologist, the Office obtained no funds from either the State or the University – I provided goodwill climate services to the State on behalf of the University – and I had conducted no business that could be construed as climate change related. Technically, nothing should have been produced.

Shortly after receiving the request from Greenpeace, I met with the University Vice President and General Counsel, Mr. Lawrence White. He summarily informed me that I was required to turn over not just documents related to the State Climate Office and what Greenpeace requested, but ALL documents that I had in my possession relating to 'global climate change' – whether or not they were produced through the State Climate Office. I was told that as a faculty member, I must comply with the request of a senior University official.

On January 26, 2010, Mr. White received a letter from the Competitive Enterprise Institute (CEI) making a nearly identical request of three other faculty members who had contributed to the Intergovernmental Panel on Climate Change. One of those faculty members was from my own department (Dr. Frederick E. Nelson) and had an office down the hall from me. Mr. White sent me an e-mail containing this FOIA request and indicated "this one will probably be answered with a short 'no'." After a follow-up letter by CEI on February 3, Mr. White finally responded that "because the information you seek does not relate to the expenditure of public funds, the University respectfully declines your records request."

I subsequently met with Mr. White to obtain an explanation as to why I was being treated differently. He explained to me that I did not understand the law. As he sees it, even though the law may not require the University to produce e-mails and documents, the law does not prohibit him from requiring me to produce them for his perusal and potential release to Greenpeace. As such, I was again instructed to turn over all the documentation he requested to him ASAP.

At that point, I sought outside legal counsel. On February 9, 2010 and after questions raised by my lawyer, Mr. White agreed to a 'do-over'. After further review, Mr. White indicated in a letter to CEI that he wished to retract his email to them and "reconsider the substance of your FOIA request" because his initial response "did not take sufficient account of the legal analysis required under the Act." Mr. White indicated to CEI and to my lawyer that their FOIA would be handled in a manner identical to my Greenpeace FOIA.

In a telephone conversation between me and the Dean of the College of Earth, Ocean, and Environment, I subsequently was told that as a University faculty member, Mr. White represented both me and the University. She insisted that he worked for me, was indeed working in my best interest in this instance, and that I must follow all of his instructions. I objected and indicated that I felt I was being treated differently from other faculty members and that treatment was simply unfair. Finally, she concluded that because I had hired my own lawyer, the College would no longer support me and she ceased communicating with me on this matter.

Shortly after our discussion, the Deputy Dean informed me that the Dean had decided that she wanted me to resign as the Delaware State Climatologist so he could take over that role. Subsequently, I was removed as the State Climatologist. I also was removed as co-Director of the Delaware Environmental Observing System (an observational network I had spent nearly a decade to develop), as faculty advisor to the Student Chapter of the American Meteorological Society, and from all my committee assignments within my department. The Chair of the Department attempted to remove me from several grants that I had obtained. I have since been restricted from serving on any departmental committees.

In a discussion with my colleague, Dr. Frederick Nelson, I learned that he had met with one of Mr. White's assistants'. Dr. Nelson related to me that she shared all she could about my FOIA discussions but then left the meeting without providing instructions regarding his FOIA. He subsequently sent a follow-up email to both her and Mr. White asking for specifics of what he was to produce. As of July 2012, he had yet to hear back from either of them. He has since retired from the University.

On June 20, 2011 – 472 days or exactly 1 year and 3.5 months – I again heard from Mr. White. He had now hired a 3rd year law student to go through the materials I had provided to him over a year earlier. But why the delay and now the sudden flurry of activity? Less than a month earlier, on May 25, 2011, the Virginia Supreme Court had ruled on the case between Attorney General Ken Cuccinelli and the University of Virginia that emails by former professor Dr. Michael Mann and in the University of Virginia's possession must be turned over to the Attorney General's Office. Interestingly, all this began as a result of a CEI FOIA of Dr. Mann that followed a similar Greenpeace FOIA on Dr. Patrick Michaels – a former faculty member at the University of Virginia. The American Association of University Professors (AAUP) and several professional organizations including the American Meteorological Society and the American Geophysical Union (of which both Dr. Michaels and I are members) vehemently protested the FOIA request. The AAUP stressed to the University of Virginia that "we urge you to use every legal avenue at your disposal to resist providing the information demanded in the [civil investigative demand]" arguing that "documents and e-mail communications that were part of an ongoing scientific discussion might be taken out of that context, and used to create an impression of wrongdoing." They concluded that "it is the University's obligation to protect academic freedom by seeing that legal tools such as this…are not used to intimidate scientists whose methods or conclusions are controversial."

Interestingly, Dr. Joan DelFattore, president of the AAUP Chapter at the University of Delaware had recently published an article on academic freedom at the University of Delaware. Citing her appreciation for having a general council (*i.e.*, Mr. White) who understands the importance of academic freedom, she wrote:

> "It is also useful to consider that once an administration silences any speech, it may be assumed that the university is endorsing whatever speech it fails to suppress. A university's real interest lies in fostering the exchange of divergent views on the understanding that the university itself does not necessarily endorse any of them and certainly does not endorse all of them."

I therefore decided to elicit her assistance through the AAUP. While her comments sounded laudable, her response to me was that FOIA matters "would not fall within the scope of the AAUP". This, of course, is in direct contrast to the stance taken by the AAUP in the Cuccinelli vs. University of Virginia where the AAUP President, Cary Nelson, wrote:

> "We are urging the University of Virginia to…publicly [resist] the threat to scholarly communication and academic freedom represented by the concerted effort to obtain faculty emails…Whatever people may think of climate research, the climate for academic freedom must not be allowed to deteriorate. If scientists think every email they send may be subject to a politically motivated attack, it will create a chilling effect on their discourse and hurt scientific research.'"

Indeed, the AAUP defended Dr. Mann at the University of Virginia but refused to become involved in my similar case at the University of Delaware, citing that they stood firmly behind Mr. White's actions.

Finally, on July 22, 2011, I was provided a list of what Mr. White had decided to release to Greenpeace – pending my permission. Mr. White further reiterated that he was indeed treating the subjects of the CEI FOIA in an identical manner. Communication I had with Dr. Nelson and the response by the 3rd year law student to my query – she indicated I was the only faculty member whose documents were being examined – suggests otherwise. If I am being singled out for my views – punish the 'skeptics' while protecting the 'believers' as happened by the disparate treatment at the University of Virginia regarding Drs. Mann and Michaels – then doesn't that make the entire discussion of academic freedom at the University of Delaware by Dr. DelFattore into a lie? Again, Dr. DelFattore wrote that "once an administration silences any speech, it may be assumed that the university is endorsing whatever speech it fails to suppress." On this topic I cannot agree more.

Mr. White wrote "if you object to the release on any of these documents, then I would inform the groups requesting this information that there are some documents in Dr. Legates' custody that we have not produced and that they should direct further questions about the documents to you." I am puzzled as to why I have the right to object to the release of any documents. If Mr. White's interpretation of FOIA as it pertains to the university is correct, then why should I or any other faculty member be allowed to object to their release? Doesn't the law trump my protests? But if he has decided to release documents outside of the FOIA just because he can, as he explained to me at the outset, then the University has unfairly targeted me. On this there can be no middle ground.

Through my attorney, I subsequently requested several questions be answered by Mr. White. Until my letter, I had not indicated to Mr. White that I had been in contact with Dr. Nelson regarding his FOIA case. At this point, I informed Mr. White that I knew he had not asked Dr. Nelson to produce any documents, despite the fact that on three occasions, Mr. White had asserted he would treat all of us equally.

The next day, February 2, 2012, Mr. White responded to questions posed to him – not to the ones contained in my letters but to questions he had already answered on August 2, 2011. Most interesting is Mr. White's response to question 1 of that exchange which explicitly addressed the equal treatment of me and those targeted by the CEI FOIA request:

> "Attached is a .pdf of an email exchange we had on February 10, 2010, memorializing our agreement on how this matter would proceed. Term 5 provides: "Dr. Legates and the University of Delaware professors who are the subject of the Competitive Enterprise Institute's FOIA request (dated Feb. 3, 2010) will be subject to the same process—that is, they too will be required to produce documents for your review—and they will be subject to the same legal standard for determining whether and to what extent FOIA applies."

On August 2, 2011, Mr. White had provided a short, one word response to that question – "Confirmed." But on February 2, 2012, his reply to the same question indicates he had not been truthful:

> "I have not yet dealt with FOIA requests directed at faculty members other than Dr. Legates. I reiterate that, if and when additional documents are gathered relating to other FOIA requests on this subject matter, you will be allowed to review those documents before they are produced."

In February of 2010, Mr. White had agreed that all parties would be subject to the same procedures and insisted that he was proceeding in exactly the same manner with them. Now, he asserts that "if and when additional documents are gathered" I will be allowed to review those documents. Why should I have the right to look at the documents of others? More importantly, two years had passed since CEI submitted its FOIA request and Mr. White indicated that "I have not yet dealt with FOIA requests directed at faculty members other than Dr. Legates." This clearly indicates that he had no intent to honor his 'do-over' request on February 9, 2010 – in essence, I *will* be treated differently than other faculty because he has every right to treat me that way.

I have since become aware of a case that involved the University of Delaware in 1991. In the Gottfredson/Blits federal arbitration case of 1991, the University of Delaware explicitly conceded (and it was upheld by the arbiter) that the University's review of research and teaching notes would violate a faculty member's academic freedom. The University's Faculty Senate Committee on Research that had investigated Professor Linda Gottfredson stated that, "the Committee has never directed its attention to the content or method of any faculty member's research or teaching, and would oppose any attempt to restrict a colleague's rights in these protected areas" (*i.e.*, areas of academic freedom and contract rights). In a meeting with the Chief Budget Officer of the University, I learned that my faculty salary only includes my teaching workload since FY2009 when that was transferred to state support. Thus, the *only* item that could be covered by State funds (and hence covered under the State FOIA) was my teaching materials since September 2008. No e-mails, no unfunded research, no service assignments were covered. Mr. White's actions violate a federal ruling to which the University has agreed to abide by.

Thus, there were no documents that fell under the Greenpeace FOIA – nothing I did as Delaware State Climatologist related to global climate change and none of my teaching duties were accomplished as the Delaware State Climatologist. On April 8, 2014, my documents were finally returned to me.

Thus, it appears that Mr. White arbitrarily decided to gather, examine, and potentially release files to Greenpeace simply because he, acting as an officer of the University has chosen to harass and try to silence me for deviating from 'University-approved' scientific views. I chose to resist the release of these materials – not because I have anything to hide – but to protect my academic freedom and the freedom of others and to reject the University's attempts "to intimidate scientists whose methods or conclusions are controversial," as the AAUP argued at the University of Virginia. If one faculty member can be bullied by a heavy-handed administration, then certainly other faculty will be under attack in the future.

Over the years, I have applied for several federal grants. Two in particular, submitted to NASA and the USDA (the latter involved using precipitation estimates by weather radar to enhance agricultural planning which had nothing to do with climate change), were never reviewed. It is not that I have received bad reviews; indeed, I have received no reviews at all. Program officers refuse to provide reviews and even to respond by e-mail or telephone. My understanding is that this is related to Anderegg *et al.* (2010) which often is used as a type of 'black list' to identify "researchers unconvinced of anthropogenic global warming," to use their terminology.

As existed in the case of Lysenkoism in the Soviet Union, a healthy scientific discussion is being subverted for political and personal gain. With the recent case of Professor Lennart Bengtsson and the story I have told here, scientists who deviate from the anthropogenic global warming playbook are likely to be harassed, have grants and proposals rejected without review, be treated more harshly than their peers, and be removed from positions of power and influence. I would have hoped that in the past decade, the discussion has become more civil. Indeed, a civil discussion can be had with some scientists that believe in the extreme scenarios of anthropogenic global warming. But too many in places of prominence and with loud voices have made this a war zone. Scientists like Bengtsson and myself have tenure or its equivalent and are somewhat insulated from the extreme attacks. But young scientists quickly learn to 'do what is expected of them' or at least remain quiet, lest they lose their career before it begins.

I leave you with this thought*:* When scientific views come under political attack, so too does independent thinking and good policy-making because all require rational thought to be effective.

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