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Subcommittee on Green Jobs and the New Economy
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Human Activity, more so than Climate Change, Affects the Number and Size of Wildfires

I am David B. South, Emeritus Professor of Forestry, Auburn University. In 1999 I was awarded the Society of American Foresters' Barrington Moore Award for research in the area of biological science and the following year I was selected as Auburn University's "Distinguished Graduate Lecturer." In 1993 I received a Fulbright award to conduct tree seedling research at the University of Stellenbosch in South Africa and in 2002 I was a Canterbury Fellow at the University of Canterbury in New Zealand. My international travels have allowed me the opportunity to plant trees on six continents.

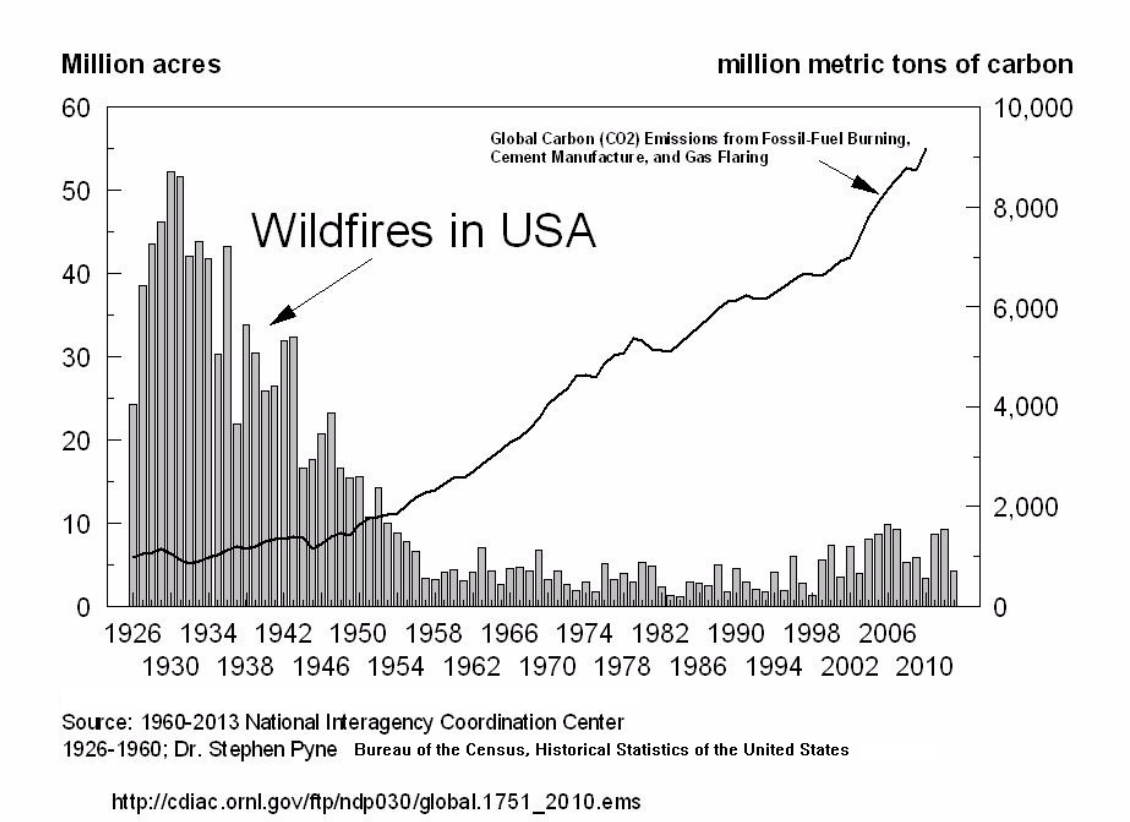
It is a privilege for me to provide some data and views on factors that affect forests and wildfires. Foresters know there are many examples of where human activity affects both the total number and size of wildfires. Policy makers who halt active forest management and kill "green" harvesting jobs in favor of a "hands-off" approach contribute to the buildup of fuels in the forest. This eventually increases the risk of catastrophic wildfires. To attribute this human-caused increase in fire risk to carbon dioxide emissions is simply unscientific. However, in today's world of climate alarmism, where accuracy doesn't matter, I am not at all surprised to see many journalists spreading the idea that carbon emissions cause large wildfires.

There is a well-known poem called the "Serenity prayer." It states "God, grant me the serenity to accept the things I cannot change, the courage to change the things I can, and wisdom to know the difference." Now that I am 63, I realize I can't change the behavior of the media and I can't change the weather. Early in my career I gave up trying to get the media to correct mistakes about forest management and to avoid exaggerations. I now concentrate on trying to get my colleagues to do a better job of sticking to facts; I leave guesses about the future to others.

Untrue claims about the underlying cause of wildfires can spread like "wildfire." For example, the false idea that "Wildfires in 2012 burned a record 9.2 million acres in the U.S." is cited in numerous articles and is found on more than 2,000 web sites across the internet. In truth, many foresters know that in 1930, wildfires burned more than 4 times that amount. Wildfire in 2012 was certainly an issue of concern, but did those who push an agenda really need to make exaggerated claims to fool the public?

Here is a graph showing a decreasing trend in wildfires from 1930 to 1970 and an increasing trend in global carbon emissions. If we "cherry pick" data from 1926 to 1970 we get a negative relationship between area burned and carbon dioxide. However, if we "cherry pick" data from 1985 to 2013 we get a positive relationship. Neither relationship proves anything about the effects of carbon dioxide on wildfires since, during dry seasons, human activity is the overwhelming factor that determines both the number and size of wildfires.

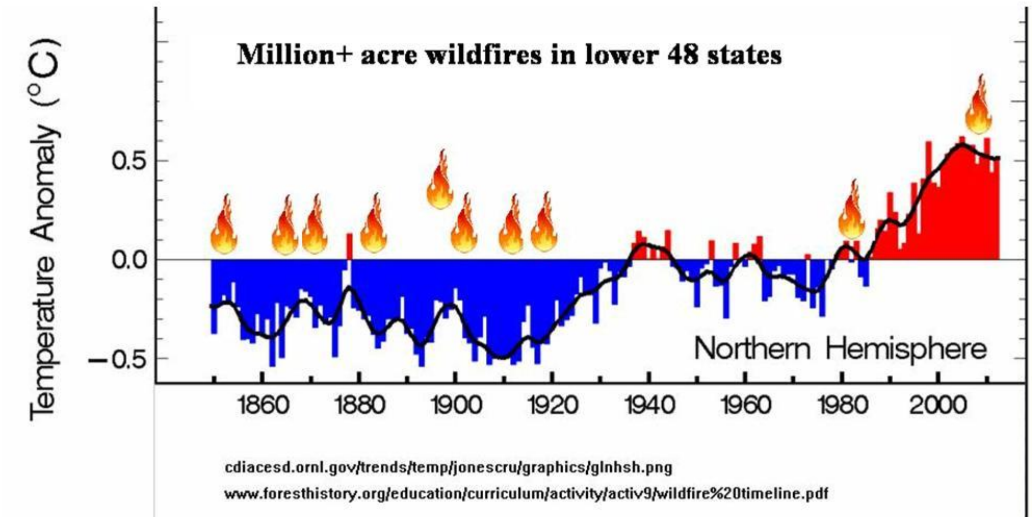
Figure 1.



In the lower 48 states there have been about ten “extreme megafires,” which I define as burning more than 1 million acres. Eight of these occurred during cooler than average decades**.** These data suggest that extremely large megafires were 4-times more common before 1940 (back when carbon dioxide concentrations were lower than 310 ppmv). What these graphs suggest is that we cannot reasonably say that anthropogenic global warming causes extremely large wildfires.

Figure 2.

A 180-year history of wildfires in the USA (lower 48 states) indicates that eight "extreme megafires" (1million+ acres) occurred during decades that were cooler than average.



Seven years ago, this Committee conducted a hearing about "Examining climate change and the media" [Senate Hearing 109-1077]. During that hearing, concern was expressed over the weather, which was mentioned 17 times, hurricanes, which were mentioned 13 times, and droughts, which were mentioned 4 times. In the 41,000 word text of that hearing, wildfires (that occur every year) were not mentioned at all. I am pleased to discuss forestry practices because, unlike hurricanes, droughts, and the polar vortex, we can actually promote forestry practices that will reduce the risk of wildfires. Unfortunately, some of our national forest management policies have, in my view, contributed to increasing the risk of catastrophic wildfires.

In conclusion, I am certain that attempts to legislate a change in the concentration of carbon dioxide in the atmosphere will have no effect on reducing the size of wildfires or the frequency of droughts. In contrast, allowing active forest management to create economically-lasting forestry jobs in the private sector might reduce the fuel load of dense forests. In years when demand for renewable resources is high, increasing the number of thinning and harvesting jobs might have a real impact in reducing wildfires.

Thank you for this opportunity to address the Subcommittee.

Additional thoughts and data

A list of names and locations of 13 megafires in North America.

Year	Fire Name	Location	Lives lost	Acres burned
1825	Miramichi	New Brunswick- Maine	> 160	3 million
1845	Great Fire	Oregon	-	1.5 million
1868	Silverton	Oregon	-	1 million
1871	Peshtigo	Wisconsin-Michigan	>1,500	3.78 million
1881	Thumb	Michigan	>280	>2.4 million
1889	Feb-15-16	South Carolina	14	3 million
1902	Yacoult	Washington and Oregon	-	> 1 million
1910	Big Blowup	Idaho Montana	85	>3 million
1918	Cloquet-Moose Lake	Minnesota	450	1.2 million
1950	Chinchaga	British Columbia Alberta	-	3.5 million
1988	Yellowstone	Montana Idaho	-	1.58 million
2004	Taylor Complex	Alaska	-	1.3 million
2008	Lightning series	California	23	>1.5 million

Figure 3 is another timeline that was constructed by examining fire scars on trees from the Southwest (Swetnam and Baisan 1996). Fire suppression/prevention activities started having an effect at the end of the 19th century and this apparently reduced the wide-scale occurrence of wildfires in the Southwest. Both of these graphs show a decline in megafires after 1920. This tells me that humans affect both the size and cycle of wildfires to a much greater extent than does increasing levels of carbon dioxide in the atmosphere.

Figure 3.

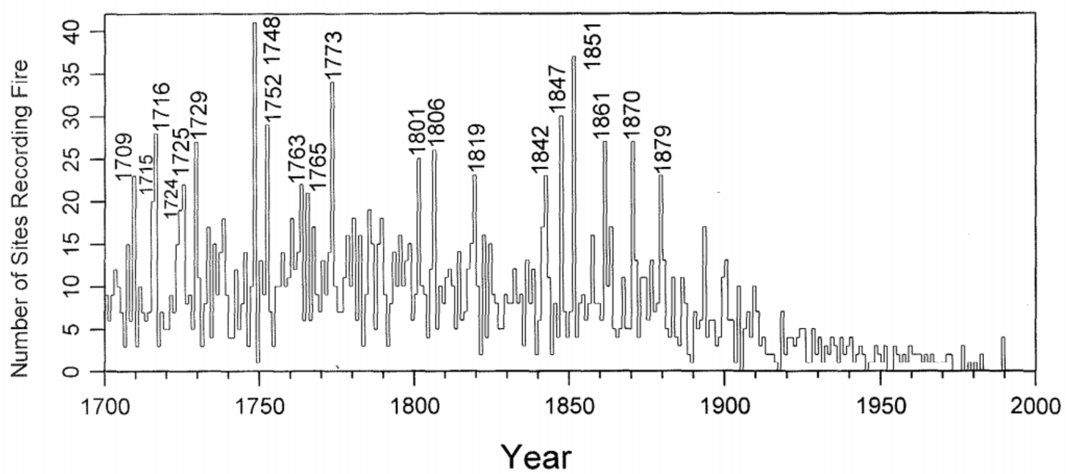


Figure 5. Regional fire occurrence time series from a network of 63 fire history sites in the Southwestern U.S. The largest 20 fire years are listed, based on the maximum numbers of sites recording these years.

http://digitalcommons.usu.edu/cgi/viewcontent.cgi?article=1085&context=barkbeetles

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The "most destructive fire" in history?

I must comment on the term "most destructive" when used in the context of wildfires. When I ask what "most destructive" actually means, I get several answers. In some articles, the number used (when there actually is a number) is calculated using nominal dollar amounts. Therefore, the rate of inflation is one factor (possibly the deciding factor) that causes fires to become more "destructive" over time. In other cases, the ranking just involves counting the number of structures burned. This takes inflation out of the equation, but it inserts urban sprawl into the equation. For example, "the number of housing units within half a mile of a national forest grew from 484,000 in 1940 to 1.8 million in 2000." Therefore, the increasing wealth of our nation (more building in fire-prone areas) can easily explain why wildfires have become "more destructive" over time. These facts are rarely mentioned by journalists who use the "most destructive" term when attributing the damage to "climate change." Scientifically, I say the term "most destructive" holds little meaning. For example, was the 1871 fire that killed over 1,500 people (possibly 2,400) and burned over 3.75 million acres the "most destructive" in US history? If not, why not?

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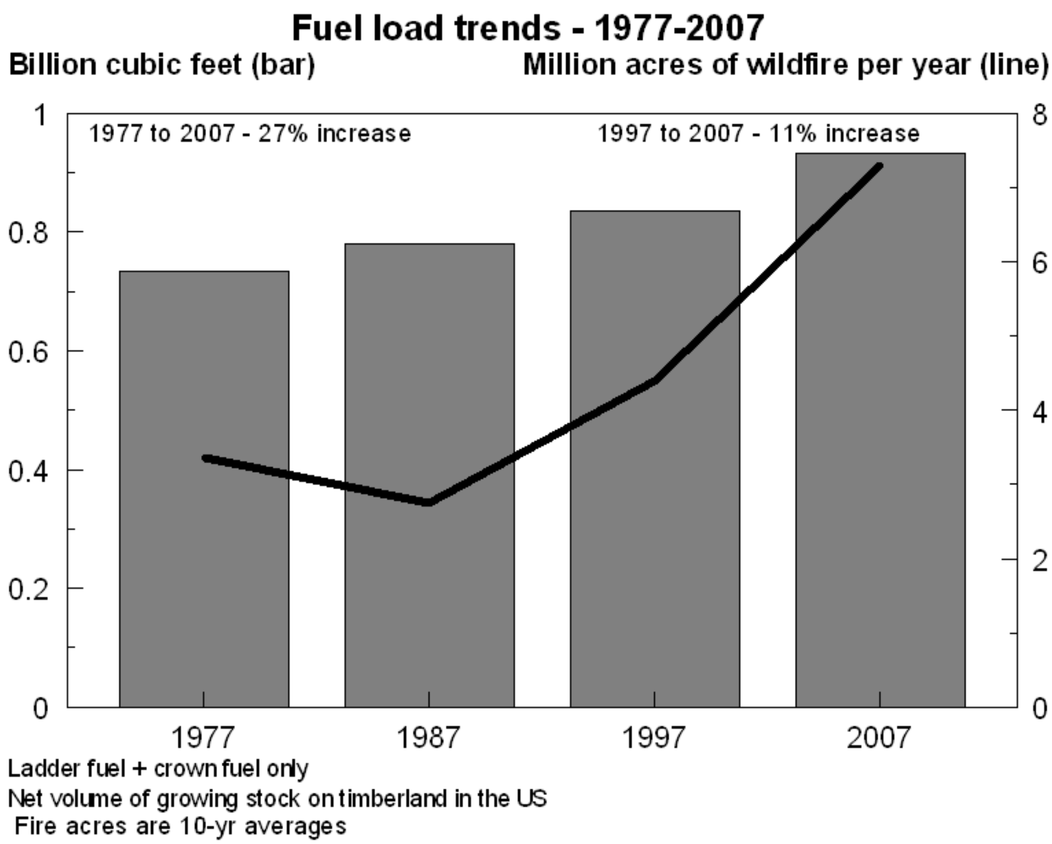
High fuel load = high wildfire danger?

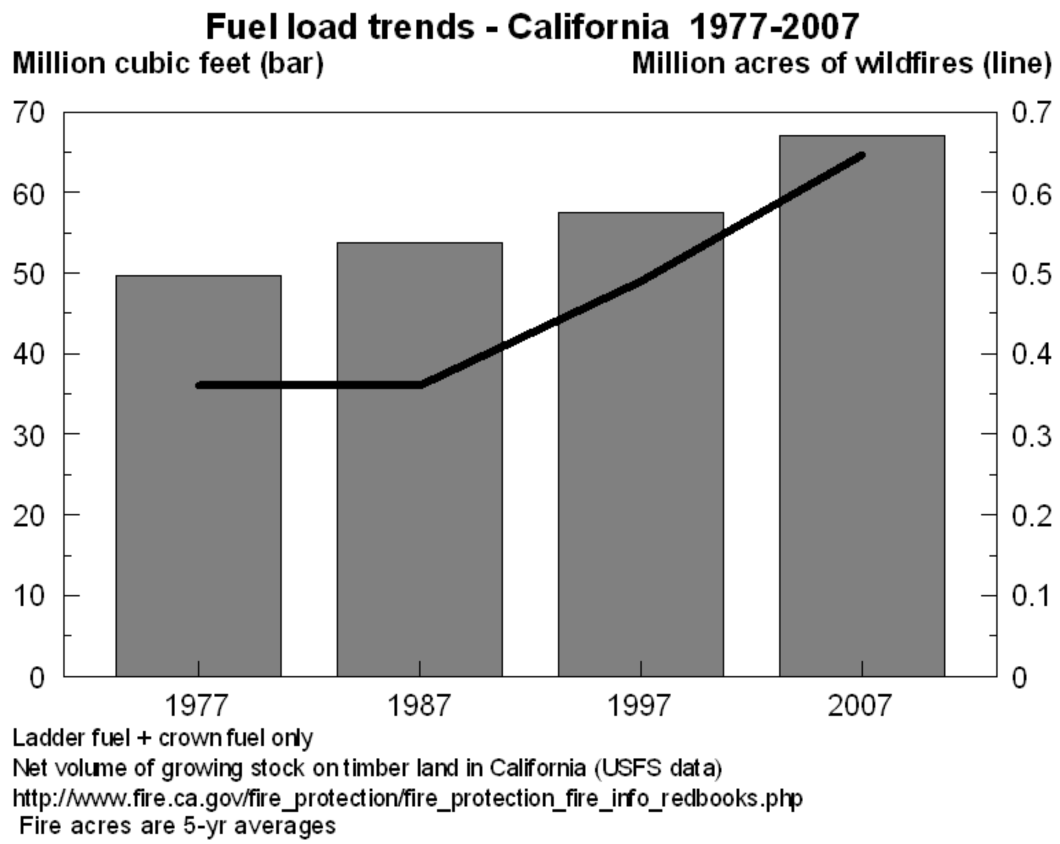
Fuel loading (or fuel volume) is reported as the amount of fuel available per acre. The higher the fuel loading, the more heat produced during a wildfire. Intense wildfires occur during dry seasons when winds are high and there is high fuel loading. The classification of fuels includes (1) surface, (2) ladder, and (3) crown fuels. The risk of wildfires since 1977 has increased on federal lands, in part, because of an increase in the "fuel load." This increase is due to tree growth plus a reduction in harvesting logs for wood products (see Figure 6). The evidence in the figure below indicates that as fuel loads on timberland increase, the area of wildfire increases.

Figure 4.



The theory that higher fuel loads cause an increase in wildfires (during dry seasons) is also supported by data from California. In just a decade, fuel loads increased on timber land by 16 percent while average wildfire size increased by 32 percent.

Figure 5.



In cases where policy allows, foresters can reduce the risk of destructive wildfires by reducing fuel loads. They can reduce ladder and crown fuels by harvesting trees and transporting the logs to a mill. This can be accomplished as final harvests, economic thinnings, firebreak thinnings and biomass thinnings (e.g. to make pellets). Surface fuels can be reduced by conducting prescribed burns (a.k.a. controlled burns). However, in the past policy has been determined by concerns expressed by journalists and activists who are against the cutting of trees. Many "preserve the forest" and "anti-forest management" policies end up increasing the risk of intense wildfires. For example, a number of climate experts recently (24 April 2014) signed a letter hoping to reduce the number of "green jobs" in North Carolina. These experts are apparently against the cutting of trees to produce wood pellets for export to the UK. They say that "a growing body of evidence suggests that trees rather than wood waste are the primary source of the wood pellets exported to the UK from the Southern US."

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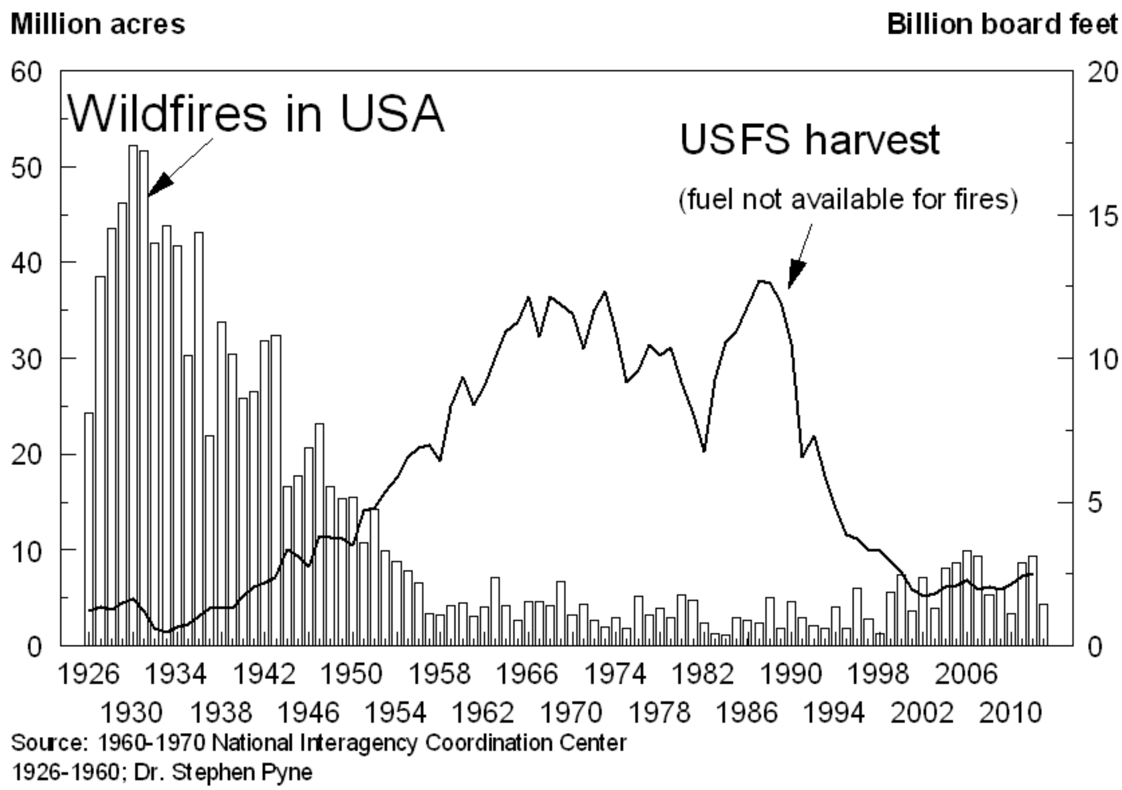
Would a return to harvesting 12 billion board feet per year reduce fuel loads on National Forests?

From about 1965 to 1990, the US Forest Service harvested about 12 billion board feet per year on National Forests. Removing this wood reduced the rate of increase in fuel loads on our National Forests. As a result, the wood volume on timber land in the West changed very little between 1977 (346.7 billion cubic feet) and 1987 (347 billion cubic feet). In contrast, wood volume over the next 10-years increased by 5 percent. Obviously stopping the harvesting of trees has increased wildfire risk in National Forests (due to increasing average wood biomass and fuel loads).

Figure 6.



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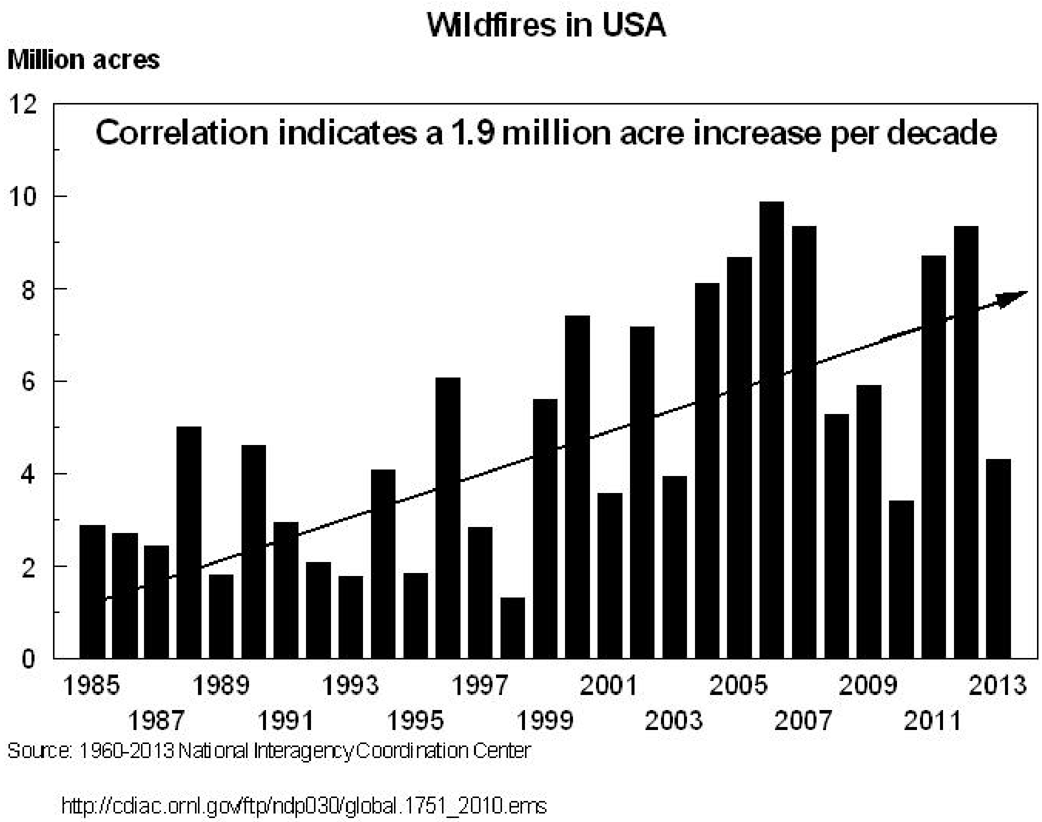
Correlation does not prove causation

I assume most Senators (and even some journalists) know that finding a "significant" trend does not prove causation (http://www.latimes.com/business/hiltzik/la-fi-mh-see-correlation-is-not-causation-20140512-column.html). In fact, a low occurrence of large megafires over the past 90 years does not prove that droughts were more common before 1950. Actual weather records or analysis of tree-rings can be used to document drought events.

Those committed to the scientific process know that the cause behind the decline in megafires is not proved by a simple correlation. Although Figure 2 (above) indicates large megafires were more common in decades with cooler temperatures, this is certainly not proof of a relationship with temperature. In reality, human activity (e.g. effective fire suppression) is the real causation for a decline in million-acre wildfires.

Figure 7 is a graph of a short-term (i.e. 28 year) trend for wildfire size in the USA. When using data from 1985 to 2013, the trend suggests the total area burned increased by 1.9 million acres per decade. This type of correlation has been the driving force behind the current media frenzy.

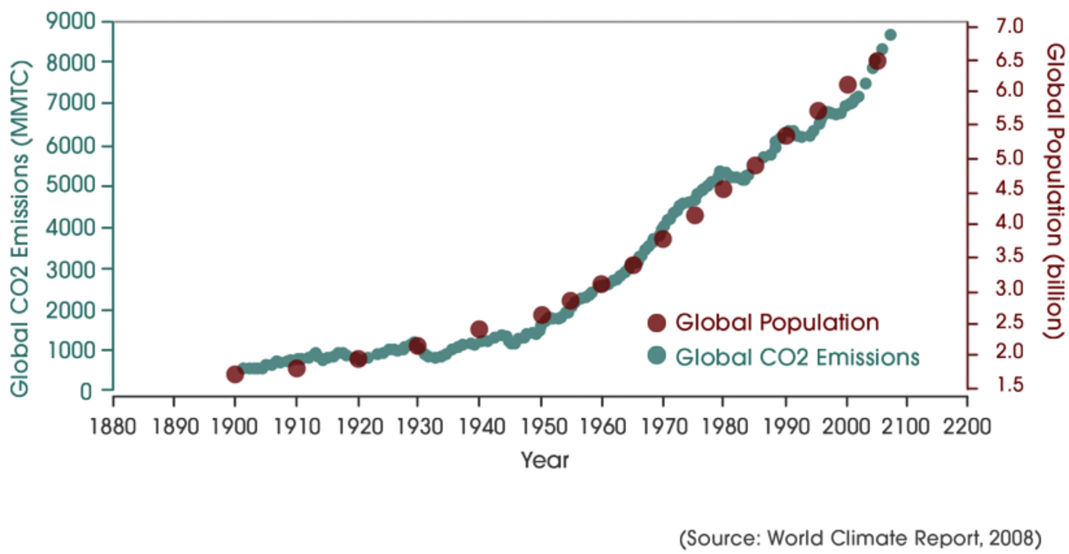
Figure 7.



Regarding a trend line similar to that in Figure 7, here is what one journalist wrote: "US wildfires have gotten much bigger over the past three decades. There's some variation from year to year, but the overall trend is upward. One recent study in *Geophysical Research Letters* found that wildfires in the western United States grew at a rate of 90,000 acres per year between 1984 and 2011. What's more, the authors found, the increase was statistically unlikely to be due to random chance."

In contrast, Figure 1 illustrates that, for the lower 48 states, the amount of wildfires ***declined*** at a rate of 400,000 acres per year between 1926 and 2013. This decline was also statistically "unlikely to be due to random chance" (i.e. 1 chance out of 10,000). [Note: The rate of decline from 1926 to 1956 was about 1.3 million acres per year]. I have never seen the print media publish a graph like Figure 1, even though similar ones are easy to find on the internet (http://www.fao.org/docrep/010/ai412e/ai412e09.jpg). They are either reluctant to inform the public about the history of wildfires, or they simply don't know the information is available. Either way, they might not realize a "statistically significant" relationship reported in their article does not mean the relationship has any real meaning.

Figure 8.



Here is an example of how the wrong conclusion can be made even with a "significant correlation." Let's assume that people cause wildfires and that more people cause more wildfires. We know that people cause carbon emissions and more people cause more carbon emissions (Figure 8). Journalists might assume that carbon emissions are causing more wildfires (due to a significant trend), but the driving force behind more wildfires is likely due to people causing more wildfires. Good scientists point out to the public all the various factors that might explain an increase in wildfires. In contrast, those with an agenda will tell the public only about the factors that support their agenda (or beliefs). They ignore scientists who warn readers that: "Due to complex interacting influences on fire regimes across the western U.S. and the relatively short period analyzed by this study, care must be exercised in directly attributing increases in fire activity to anthropogenic climate change."

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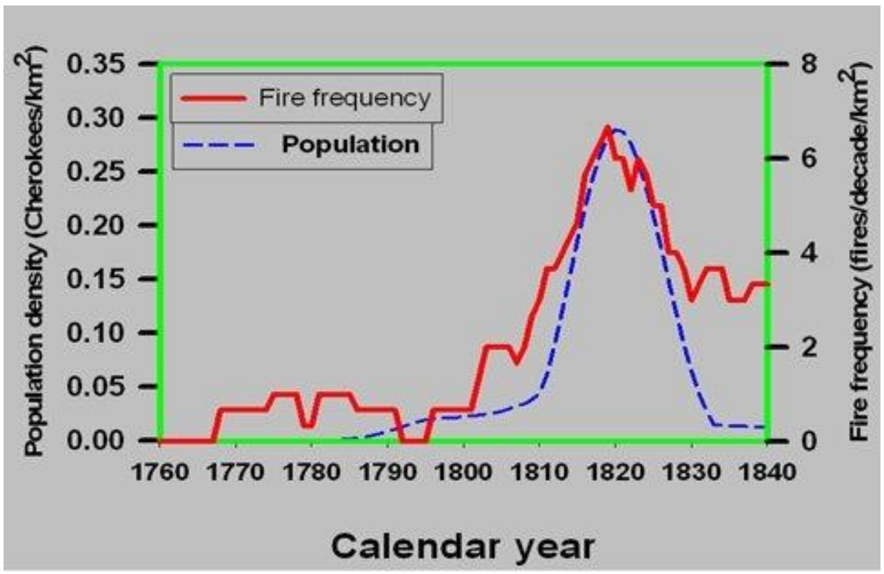
In reality, people affect both the size and number of wildfires

Unlike hurricanes, droughts and tornadoes, humans cause many wildfires. During the 19th century, Native Americans and European immigrants increased the number of wildfires. The following graph suggests the fires in the Boston Mountains of Arkansas were related to the population of Cherokee Indians (Guyette, Spetich and Stambaugh 2006).

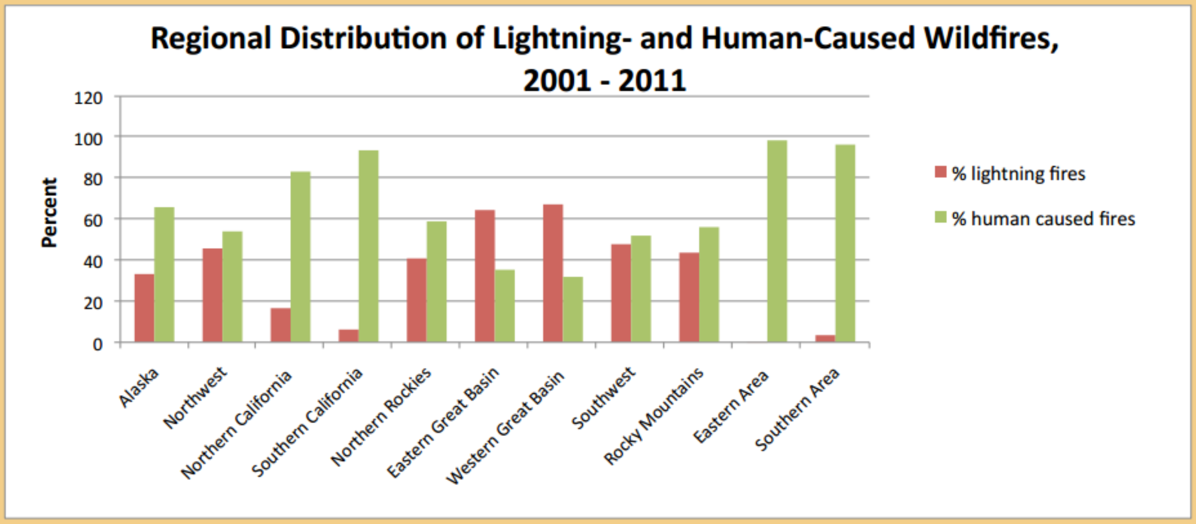
Figure 9.



Guyette, Spetich and Stambaugh, 2006

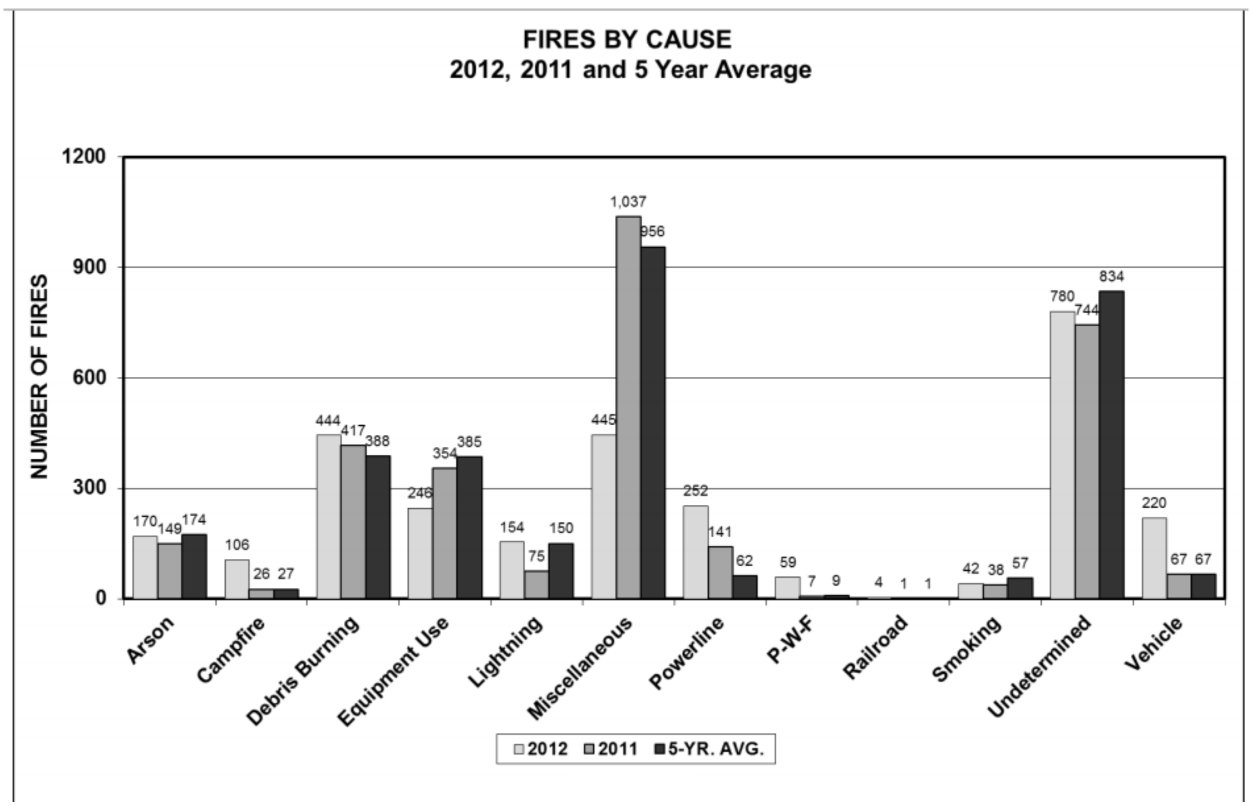
In most places in the US, humans are the major cause of wildfires. In 2012, only about 5 percent of fires in California were caused by lightning. The Rim Fire (100 miles east of San Francisco) was ignited by a campfire in 2013 and was perhaps the third largest fire in California. Even so, some (who might be against cutting of trees to lower fuel levels) contend severe fire seasons are the result of prolonged drought combined with lightning. If this human-caused wildfire had not occurred, the amount of wildfires in California that year would have been reduced by 44%. Since one human fire can increase acres burned by over 250,000 acres, I say it is unscientific to attribute trends in wildfires to carbon dioxide levels without accounting for the various ways humans actually affect wildfires (e.g. arson, smoking, target practice, accidents, etc.).

Figure 10.



Percentage of lightning fires in the Eastern Area is too low (1.4%) to show up on this graph.

Figure 11.



In areas that are unpopulated, fire fighters can concentrate their limited resources on suppressing the fire. However, in areas where population growth has increased the density of houses, some crews are diverted to protecting property instead of attacking the fire. As a result, the relative size of the fire increases. The policy of allowing more homes to be built in fire-prone areas likely has increase the size of future fires (if more resources are devoted to protecting the homes). Randy Eardley (a spokesperson for the Bureau of Land Management) said that in the past, “it was rare that you would have to deal with fire and structures,” “Nowadays, it’s the opposite. It’s rare to have a fire that doesn’t involve structures.” In fact, I was recently told that one of the primary reasons for increased burned acres is that - in the interest of firefighter safety, cost, and biotic benefits, “fire officers are more willing to back off” and let the wildfire burn out.

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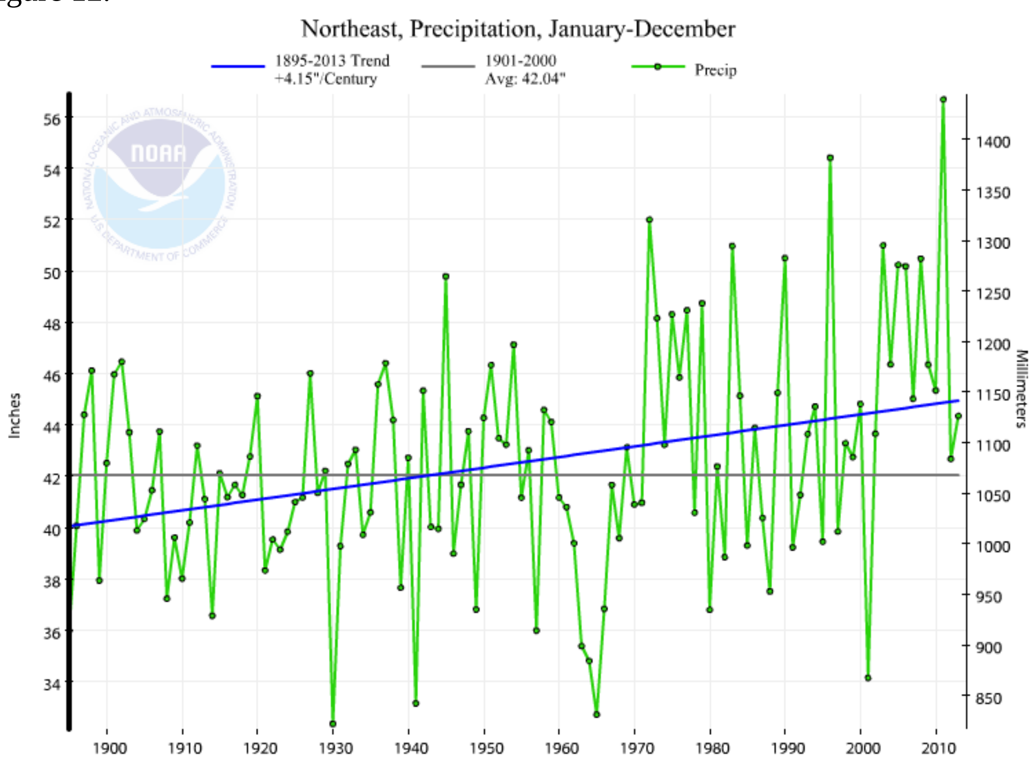
Some forests receive more rainfall now than 100 years ago

Examining historical weather data shows that some forests now receive more rainfall on average, than occurred a century ago. For example, precipitation in the Northeast has increased about 10%. Of course rainfall pattern is very important in the cycle of droughts, but one advantage of an increase in rainfall might be an increase in growth of trees. The following are trends in precipitation for various regions in the lower 48 states: Northeast +4.1” per century; Upper Midwest +2.8”; South +2.5”; Southeast +0.6”; Southwest -0.2”; West no change; Northern Rockies and Plains +0.5”; Northwest +0.7”.

Figure 12.



In some places the extra rainfall might have resulted in a reduction in wildfires. For example, summer precipitation in British Columbia increased from 1920 to 2000. In one region the increase may have been over 45%. Authors of the study (Meyn et al. 2013) observed a “significant decrease in province-wide area burned” and they said this decrease was “strongly related to increasing precipitation, more so than to changing temperature or drought severity.” In some areas, a benefit of an increase in precipitation could be fewer wildfires.

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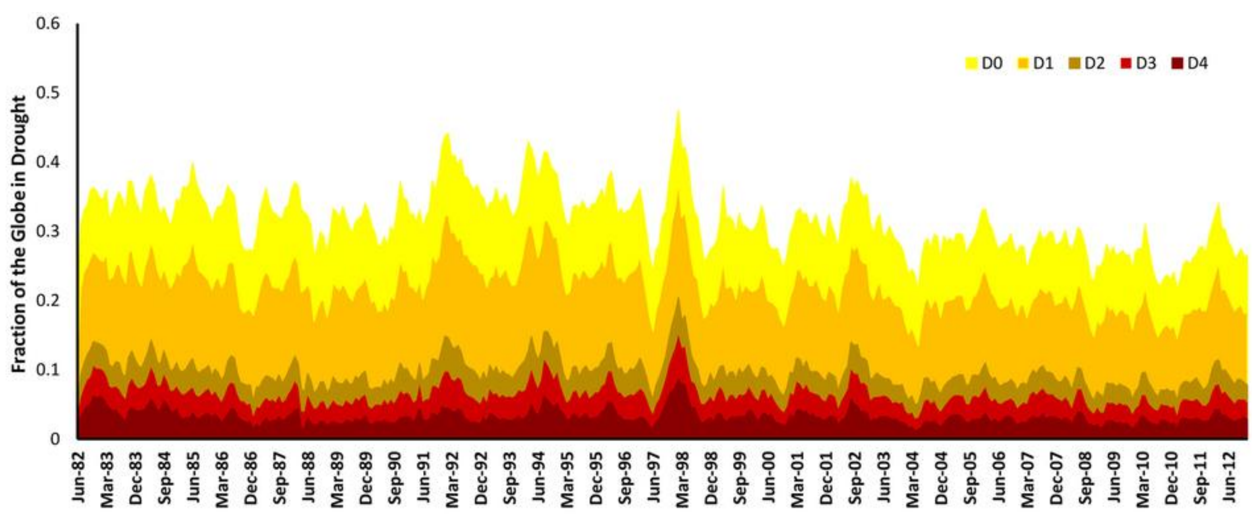
Some forests receive less rainfall now than 40 years ago

Drought increases the risk of wildfire. The extent of wildfires for any given year will depend on if a drought occurs that year. One should expect some variability in the occurrence of droughts, and we can document various drought cycles by using the NOAA web site “Climate at a Glance.” We might also expect a single, large wildfire to burn more acres in a drought year than in a rainy year. Therefore, it is not surprising that total area burned is higher in drought years than in non-drought years.

As previously mentioned, some journalists are spreading the idea that carbon dioxide is causing more droughts. But if it were true, we should see droughts increasing globally (not just in one drought-prone region of the US). The following figure illustrates the global pattern of drought since 1982 and it clearly suggests that droughts globally have not gotten worse over the two decade timeframe (Hao et al. 2014). It appears that some journalists are not aware of this global pattern. Of course some might be aware of this pattern but it does not fit their narrative. As a result, they report that droughts for a specific location increased during a decade.

Figure 13.



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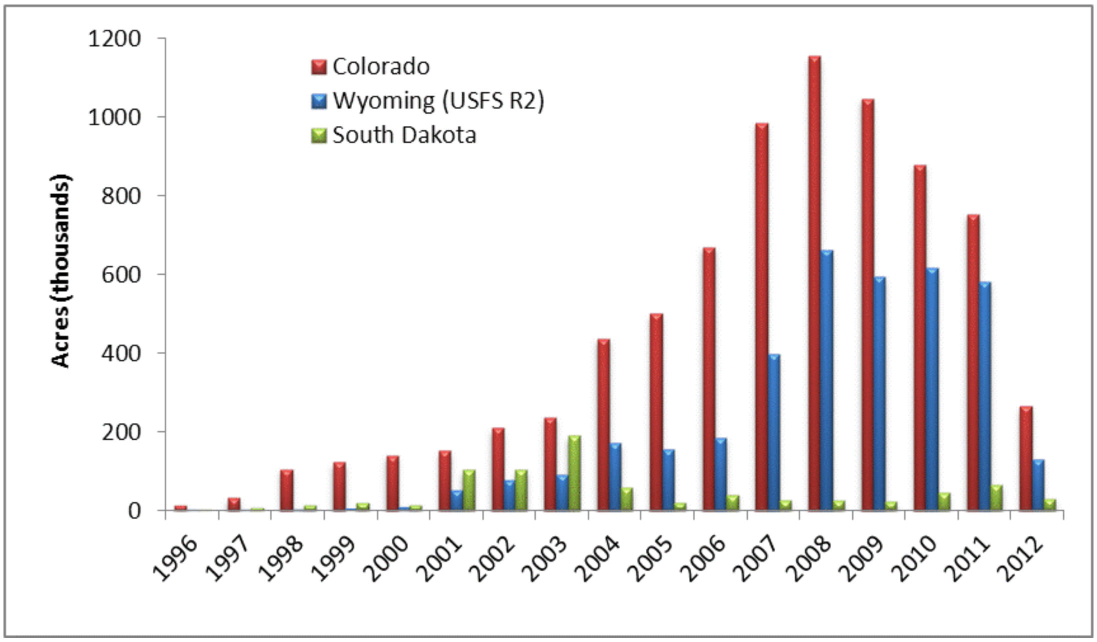
Risk of pine beetles increase on forests with no thinning

Pine beetles have killed millions of trees in Canada and in the United States. Foresters and entomologists know that pine beetle outbreaks are cyclical in nature. When pine trees are under stress, they attract pine beetles. Trees undergo stress when they are too close together (i.e. too dense) and things get worse when there is a drought. Once conditions are right, the beetles thrive in stressed trees and the progeny attack more trees and the domino effect begins. Foresters and ecologists know that pine beetle cycles have occurred naturally over thousands of years.

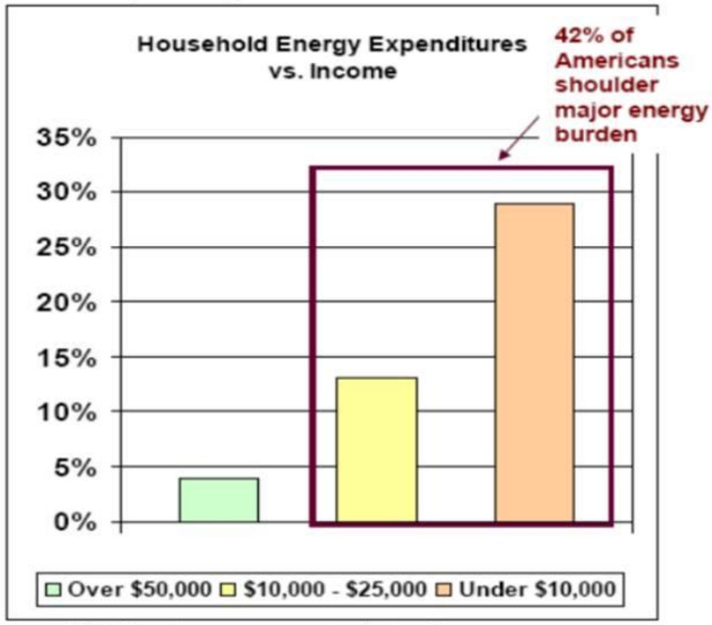
Figure 14.



One factor that increases the risk of a beetle outbreak are policies that do not permit the thinning of trees. State and national forestry organizations know the risk of a beetle outbreak is higher in counties occupied by National Forests. For example, in Texas, the US Forest Service says that "Very little suppression took place during the last outbreak. A majority of those treatments were designed to protect RCW habitat as mandated by the Endangered Species act. SPB were left alone in most of the wilderness and killed large acreages." In contrast, some "environmental" groups object to beetle suppression methods that involve cutting trees in wilderness areas. As a result, thinning operations are delayed, beetle attack stressed trees, and then large populations of beetles spread to adjacent privately-owned forests. After the trees die, the risk of wildfire increases. Wildfires start (due to carelessness or accidents or arson) and large expenditures are made to put the fire out. Journalists then report that carbon dioxide caused the inferno. The public concern over wildfires might cause some in Washington to want to increase the cost of energy. For example, this month my electrical cooperative sent me an e-mail suggesting that new EPA regulations could increase my bill by 50%. Of course we know that increasing the cost of energy will hurt the poor more than the wealthy.

Figure 15.

http://www.americaspower.org/sites/default/files/Social-Benefits-of-Carbon.pdf



Helping the poor means lowering energy costs
Hurting the poor means increasing energy costs

Source: American Association of Blacks in Energy

Foresters tell the public that the best way to prevent a beetle outbreak is to thin the forest to will increase tree health. We also know that planting too many seedlings per acre will also increase the risk of beetles.
http://www.forestry.state.al.us/Publications/TREASURED_Forest_Magazine/2005%20Summer/How%20to%20Grow%20Beetle%20Bait%20-%20Revisited.pdfly-owned forests.

In contrast, the public also tells foresters how to manage beetle risks in wilderness areas. The following is just two pages of a seven-page document illustrating how much time and man-hours are wasted before operations to reduce the risk of pine beetles can precede in wilderness areas.

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Dr. South offers a bet on sea level rise for year 2024

In the past, I have had the good fortune to make a few bets with professors (http://www.aaes.auburn.edu/comm/pubs/highlightsonline/summer99/south.html). For example, I won a bet on the future price of oil and was successful in betting against Dr. Julian Simon on the price of sawtimber (i.e. he sent me a check a year after making the bet). Five years ago, I offered to bet on an "ice free" Arctic by the summer of 2013, but a BBC journalist [who wrote a 2007 article entitled "Arctic summers ice-free 'by 2013' "] and several ice experts declined my offer. To date, the number of bets I have made has been limited since I have a hard time finding individuals who are confident enough to make a wager on their predictions.

I would like to take this opportunity to offer another "global warming" bet. This time the outcome will be based on sea level data for Charleston, SC. Recently I was told that "If we do nothing to stop climate change, scientific models project that there is a real possibility of sea level increasing by as much as 4 feet by the end of this century."

At Charleston, the rate of increase in sea level has been about 3.15 mm per year. A four foot increase (over the next 86 years) could be achieved by rate of 14 mm per year. I am willing to bet $1,000 that the mean value (e.g. the 3.10 number for year 2012 in Figure 16) will not be greater than 7.0 mm/yr for the year 2024. I wonder, is anyone really convinced the sea will rise by four feet, and if so, will they take me up on my offer? Dr. Julian Simon said making bets was a good way to see who was serious about their beliefs and who is just "talking the talk."

Figure 16.

Annual change in sea level at Charleston, SC.

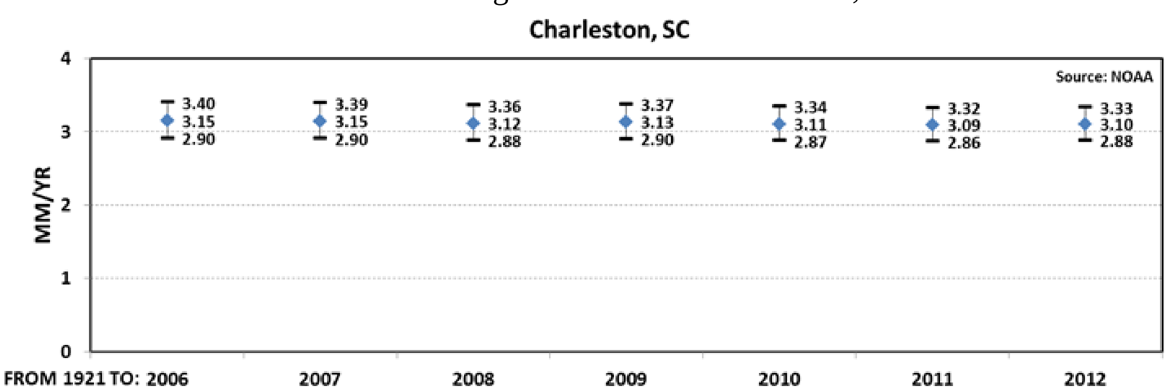
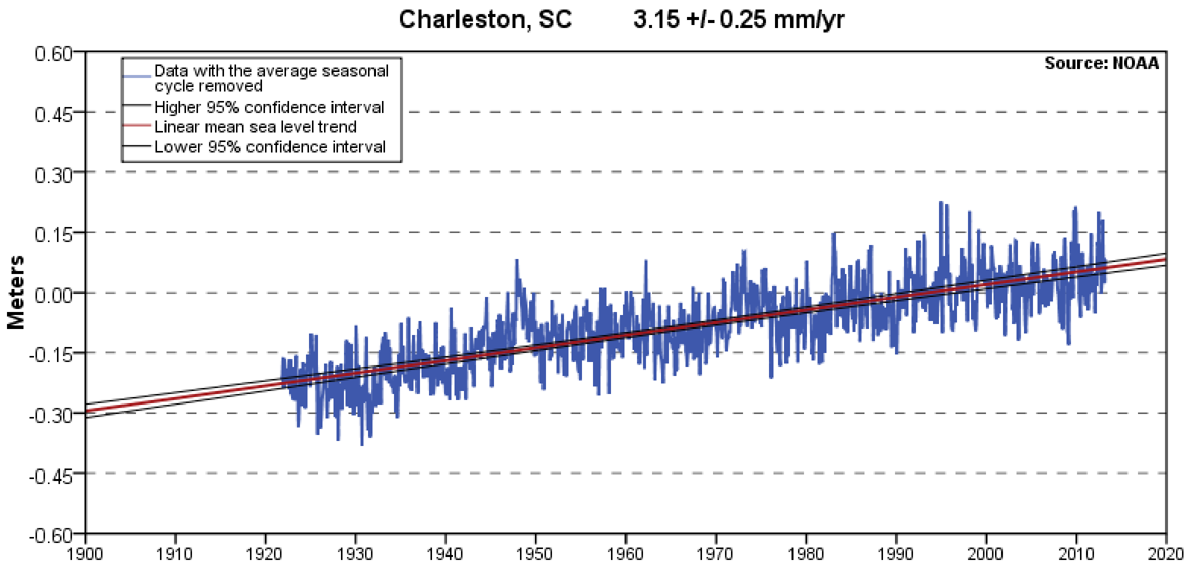


Figure 17.



http://tidesandcurrents.noaa.gov/sltrends/sltrends_station.shtml?stnid=8665530

Selected References

Arno S.F. 1980. Forest fire history in the northern Rockies. Journal of Forestry, 78:460-465.

Botts H., Jeffery T., Kolk S., McCabe S., Stueck B. Suhr L. 2013. Wildfire hazard risk Report. Residential Wildfire Exposure Estimates for the Western United States. CoreLogic Inc. http://www.corelogic.com/about-us/researchtrends/wildfire-hazard-risk-report.aspx

Dennison P.E., Brewer S.C., Arnold J.D., Moritz M.A. 2014. Large wildfire trends in the western United States, 1984–2011. Geophysical Research Letters.

Guyette R.P., Spetich M.A., Stambaugh M.C. 2006. Historic fire regime dynamics and forcing factors in the Boston Mountains, Arkansas, USA. Forest Ecol. Manage. 234:293–304.

Hao Z., AghaKouchak A., Nakhjiri N., Farahmand A. 2014. Global integrated drought monitoring and prediction system. Scientific Data, 1. Article number: 140001.

Layser EF. 1980. Forestry and climatic change. Journal of Forestry, 78:678-682.

Meyn A., Schmidtlein S., Taylor SW., Girardin MP., Thonicke K., Cramer, W. 2013. Precipitation-driven decrease in wildfires in British Columbia. Regional Environmental Change, 13(1), 165-177.

Stein SM., Menakis J., Carr MA., Comas SJ., Stewart SI., Cleveland H., Bramwell L., Radeloff VC. 2013. Wildfire, wildlands, and people: understanding and preparing for wildfire in the wildland-urban interface—a Forests on the Edge report. Gen. Tech. Rep. RMRS-GTR-299. Fort Collins, CO. U.S. Department of Agriculture, Forest Service, Rocky Mountain Research Station. 36 p.

Stephens, SL., Mclver JD., Boerner REJ., Fettig CJ., Fontaine JB., Hartsough BR., Kennedy PL., Dylan WS. 2012. Effects of forest fuel-reduction treatments in the United States. Bioscience 63:549-560.

Swetnam T., Baisan C. 1996. Historical fire regime patterns in the southwestern United States since AD 1700. In: CD Allen (ed) Fire Effects in Southwestern Fortest : Proceedings of the 2nd La Mesa Fire Symposium, pp. 11-32. USDA Forest Service, Rocky Mountain Research Station, General Technical Report RM-GTR-286.

What If Our Guesses Are Wrong?

This old professor would like to comment on four "climate change" articles. A 1973 article entitled "Brace yourself for another ice age" (*Science Digest* 57:57–61) contained the following quote: "Man is doing these things… such as industrial pollution and deforestation that have effects on the environment." A 1975 article about "Weather and world food" (*Bulletin of the American Meteorological Society* 56:1078–1083) indicated the return of an ice age would decrease food production. The author said "there is an urgent need for a better understanding and utilization of information on weather variability and climate change…" Soon afterwards, Earle Layser wrote a paper about "Forests and climate" (*Journal of Forestry* 78:678–682). The following is an excerpt from his 1980 paper: "One degree [F] may hardly seem significant, but this small change has reduced the growing season in middle latitudes by two weeks, created severe ice conditions in the Arctic, caused midsummer frosts to return to the upper midwestern United States, altered rainfall patterns, and in the winter of 1971–1972 suddenly increased the snow and ice cover of the northern hemisphere by about 13 percent, to levels where it has since remained" (Bryson 1974). Spurr (1953) attributed significant changes in the forest composition in New England to mean temperature changes of as little as 2 degrees. Generally, the immediate effects of climatic change are the most striking near the edge of the Arctic (Sutcliffe 1969, p. 167) where such things as the period of time ports are ice-free are readily apparent. However, other examples cited in this article show that subtle but important effects occur over broad areas, particularly in ecotonal situations such as the northern and southern limits of the boreal forest or along the periphery of a species' range.

Among these papers, Layser's paper has been cited more often (> 20 times), but for some reason, it has been ignored by several authors (e.g., it has not been cited in any *Journal of Forestry* papers). Perhaps it is fortunate that extension personnel did not choose to believe the guesses about a coming ice age. If they had chosen this "opportunity for outreach," landowners might have been advised to plant locally adapted genotypes further South (to lessen the impending threat to healthy forests). Since the cooling trend ended, such a recommendation would have likely reduced economic returns for the landowner.

A fourth article was about "state service foresters' attitudes toward using climate and weather information" (*Journal of Forestry* 112:9–14). The authors refer to guesses about the future as "climate information" and, in just a few cases, they confuse the reader by mixing the terms "climate" and "weather." For example, a forecast that next winter will be colder than the 30-year average is not an example of a "seasonal climate forecast." Such a guess is actually a "weather forecast" (like the ones available from www.almanac.com/weather/longrange). Everyone should know that the World Meteorological Organization defines a "climate normal" as an average of 30 years of weather data (e.g., 1961–1990). A 3-month or 10-year guess about future rainfall patterns is too short a period to qualify as a "future climate condition." Therefore, young foresters (<50 years old) are not able to answer the question "have you noticed a change in the climate" since they have only experienced one climate cycle. They can answer the question "have you noticed a change in the weather over your lifetime?" However, 70-year-olds can answer the question since they can compare two 30-year periods (assuming they still have a good memory).

Flawed computer models have overestimated (1) the moon's average temperature, (2) the rate of global warming since the turn of the century, (3) the rate of melting of Arctic sea ice, (4) the number of major Atlantic hurricanes for 2013, (5) the average February 2014 temperature in Wisconsin (−13.6° C), etc. Therefore, some state service foresters may be skeptical of modelers who predict an increase in trapped heat and then, a few years later, attempt to explain away the "missing heat." Overestimations might explain why only 34 out of 69 surveyed foresters said they were interested in "long-range climate outlooks." Some of us retired foresters remember that cooling predictions made during the 1970s were wrong. Even "intermediate-term" forecasts for atmospheric methane (made a few years ago with the aid of superfast computers) were wrong. Therefore, I am willing to bet money that the "long-range outlooks of climate suitability" for red oak will not decline by the amount predicted (i.e., tinyurl.com/kykschq). I do wonder why 37 foresters (out of 69 surveyed) would desire such guesses if outreach professionals are not willing to bet money on these predictions.

I know several dedicated outreach personnel who strive to provide the public with facts regarding silviculture (e.g., on most sites, loblolly pine seedlings should be planted in a deep hole with the root collar 13–15 cm belowground). However, if "right-thinking" outreach personnel try to convince landowners to alter their forest management based on flawed climate models, then I fear public support for forestry extension might decline. I wonder, will the public trust us if we don't know the difference between "climate" and "weather," won't distinguish between facts and guesses, and won't bet money on species suitability predictions for the year 2050?

David B. South
Pickens, SC

Unsafe Practices

On the cover of the January 2014 issue, I see at least a baker's dozen foresters and loggers standing in the woods and not a single hardhat is in sight.

We often hear how we should be mentoring young people and new foresters. I don't believe unsafe practices should be championed on the cover of American forestry's principal publication.

Douglas G. Turner
Newtown, PA

Some basic questions about climate models

David B. South, Peter Brown and Bill Dyck[1]

Some foresters are concerned about increasing CO_2 levels in the atmosphere while others doubt that CO_2 has been the main driver of climate change over the past million years or over the past two centuries (Brown *et al.* 2008). We three admit that (1) we do not know what the future climate will be in the year 2100, (2) we do not pretend to know the strength of individual feedback factors, (3) we do not know how much 600 ppm of CO_2 will warm the Earth and (4) we do not know how the climate will affect the price of pine sawlogs in the year 2050 (in either relative or absolute terms). The climate is not a simple system and therefore we believe it is important to ask questions. The following 15 questions deal mainly with global climate models (GCM).

A LIST OF QUESTIONS

1: Have any of the climate models been verified?

Relying on an unverified computer model can be costly. NASA relies on computer models when sending rockets to Mars and the model is verified when the landing is successful. However, when using one unverified computer model, a $125 million Mars Climate Orbiter crashed on September 23, 1999. The model was developed by one team of researchers using English units while another used metric units. This crash demonstrates how costly an unverified computer model can be to taxpayers. At the time, Edward Weiler, NASA's Associate Administrator for Space Science said "People sometimes make errors".

Is it possible that people sometimes make errors when developing complex models that simulate the Earth's climate? Is it possible that some models might have "cause and effect" wrong in the case of feedback from clouds? Is it possible to construct models that produce precise (but inaccurate) estimates of temperature in the future? Do some researchers believe in computer predictions more than real data?

A report by the International Panel on Climate Change (IPCC) shows a predicted "hot zone" in the troposphere about 10 km above the surface of the equator (IPCC 2007b; Figure 9.1f). Why has this "hot zone" not been observed? We do not know of any paper that reports the presence of this, theoretical, hot spot. Is the absence of this hot zone (Douglass *et al.* 2007) sufficient to invalidate the climate models? If not, why not?

IPCC figure TS.26 includes computer projections of four CO_2 emission scenarios for the years 2000 to 2025 (IPCC 2007a). Figure 1 is an updated version with extra data points. The mean of the projections for global temperatures are jagged, suggesting that for some years the temperature is predicted to increase (e.g. 2007) while in others the temperature is predicted to decline slightly (e.g. 2008). However, observed data for 2006, 2007 and 2008 all fall below the projections. Although several models suggest the temperature for 2008 should be about 0.59 °C above the 1961-1990 mean, the value in 2008 was 0.328°C (are all three digits past the decimal point significant?). Although we should not expect any given year to lie on the line, this value is outside the range of "uncertainty" listed for green, red and blue lines and is almost outside the uncertainty range for the orange line. If the observed data falls outside the range of uncertainty for eight years into the future, why should foresters be "believe" the models will be accurate (ie. lie within the uncertainty bar) 100 years into the future? At what point do we admit the Earth's climate is not tracking with the "virtual" climate inside a computer? Is the theoretical "hot spot" above the equator a result of programming error? More importantly, how much money are foresters willing to spend on the output of unverified computer models?

2: Is it possible to validate climate models?

"Verification and validation of numerical models of natural systems is impossible. This is because natural systems are never closed and because model results are always non-unique. Models can be confirmed by the demonstration of agreement between observation and prediction, but confirmation is inherently partial. Complete confirmation is logically precluded by the fallacy of affirming the consequent and by incomplete access to natural phenomena. Models can only be evaluated in relative terms, and their predictive value is always open to question. The primary value of models is heuristic". (Oreskes *et al.* 1994).

3: How accurate are the predictions of climate models?

Australian Bureau of Meteorology uses computer models to project weather outlook for three months into the future. The Bureau's web page states that "These outlooks should be used as a tool in risk management and decision making. The benefits accrue from long-term use, say over ten years. At any given time, the probabilities may seem inaccurate, but taken over several years, the advantages of taking

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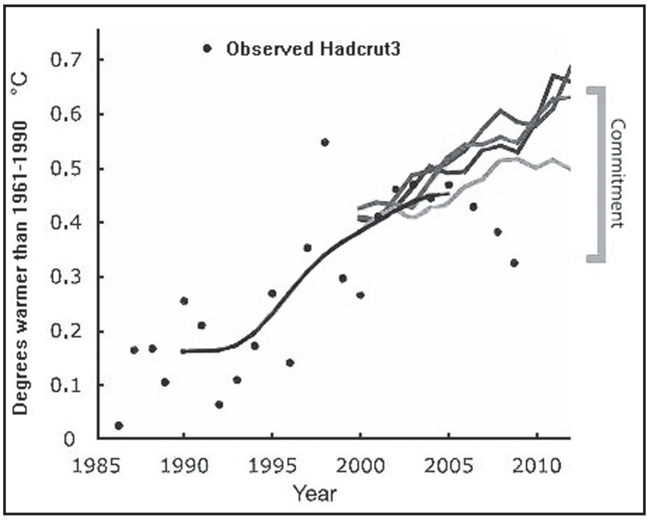

Figure 1. A comparison of observed surface temperature means (Hadcrut3) with model projections of Hadcrut3 global averages (adapted from Figure TS.26 in IPCC technical summary - page 69). Observed annual mean temperatures are shown (black dots) along with decadal averages (1990-2005 line). Multi-model mean projections (2000-2013 lines) from the IPCC (2007a) report for the SRES B1, A1B and A2 scenarios (top three lines) and a "commitment" scenario. The orange "commitment" curve shows means of 16 model projections of warming if greenhouse gas and aerosol concentrations were held constant from the year 2000. The uncertainty range indicated against the right-hand axis is for the "commitment" scenario only. Observed values for 2006, 2007 and 2008 are all below the "commitment" line and the observed value for 2008 might lie below the uncertainty range.

account of the risks should outweigh the disadvantages." Is this statement simply a hope or is it supportable by data? These computer model predictions can be compared with actual temperature data over a ten year period. The results could illustrate if farmers (who invest money based on the predictions) have benefited from the models or have they suffered from use of the models. The difference can provide evidence to illustrate if the 3-month forecasts are any better than flipping a coin. One reason why many farmers do not use these 3-month forecasts is because in some areas, the models are no better than a random guess.

Some claim it is more difficult to predict weather three months into the future than it is to predict the climate 100 years into the future. We question this belief system. What is the record of predicting climate 100 years into the future? Which of the 23 climate models is the most accurate when predicting past events? Is a complex computer program that predicts the average temperature for NZ in the past more accurate than one that predicts the average temperature for the Earth 100 years from now? Which prediction would be more accurate (determined by predicted minus actual °C)? Which set of comparisons has the greater standard deviation?

We know that climate models can vary widely in their guesses about how much rain a specific region on Earth might receive (Singer 2008). So how accurate are climate models when predicting the past? When models predict precipitation for a given location, we can compare the prediction with actual records. For example, Lim and Roderick (2009) provided predictions of annual precipitation for the last three decades of the 20th Century. Examination of the output from 39 computer scenarios reveals that predictions of NZ annual precipitation (Figure 2) ranged from 936 mm to 1851mm/yr (mean of 1293 mm; standard deviation was 226 mm). The recorded mean rainfall/precipitation of 29 AWIS stations (located mostly at towns or cities) for the years 1971-2000 was 1419 mm, but the mean of 27 AWIS stations (not including Milford Sound and Mount Cook) was 1115 mm. Neither value represents the actual mean precipitation value for NZ, in fact we do not know of an accurate estimate. One cannot take 268,680 km^2 and multiply it by some number (say 1.3 m) to determine the mass of water that fell on NZ in 1999. Of the 39 computer estimates of past NZ precipitation, how can we identify the one that is closest to the actual value for NZ if we cannot even determine the actual value?

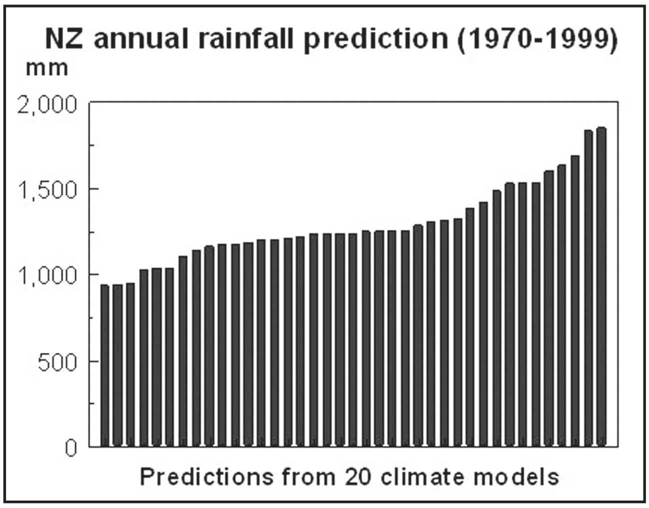

Figure 2. A comparison of predicted rainfall from 20 climate models (adapted from Lim and Roderick 2009). There are 39 output scenarios (bars) with some climate models producing seven estimates and some with only one estimate. Nobody knows the mass of precipitation that fell on NZ during the 30 year period and therefore we do not know which computer simulation is closest to the actual value for average rainfall in NZ.

4: Most climate models have clouds as a positive feedback mechanism. If clouds actually produce a negative feedback, then CO_2 caused global warming is a non-issue (i.e. warming over then next 100 years might be 0.5 °C). Do climate models have clouds modelled correctly?

"All 23 IPCC climate models now exhibit positive cloud and water vapour feedback" (Roy Spencer, personal

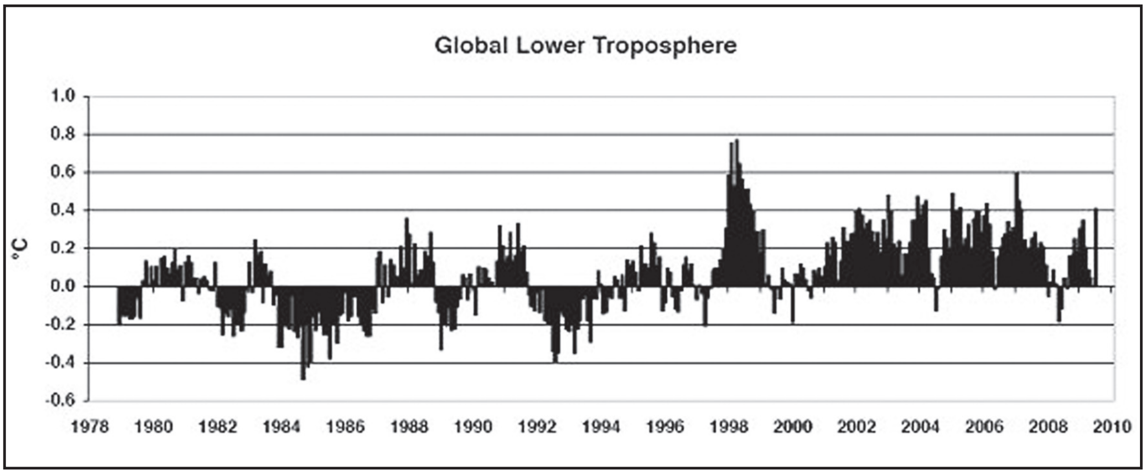

Figure 4. Globally averaged satellite-based temperature of the lower atmosphere (where zero = 20 year average from 1979 to 1998). February, 1998 was 0.76 °C above the 20-year average. Data provided by Professors John Christy and Roy Spencer, University of Alabama, Huntsville.

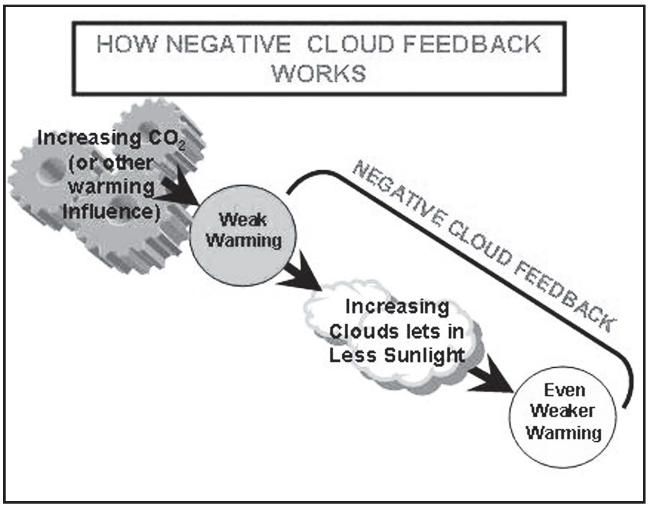

Figure 3 A negative cloud feedback would increase the Earth's albedo (figure provided by Dr. Roy Spencer).

communication). Most climate modellers assume that weak warming will decrease the amount of clouds which reduces the albedo of the Earth. A lower albedo (ie. less cloud cover) results in more warming.

In contrast, Spencer and Braswell (2008) suggest that clouds likely produce a negative feedback. Weak warming seems to increase the amount of clouds which increases the albedo of the Earth (Figure 3). If increases in CO_2 results in more clouds, this will invalidate most climate models. Roy Spencer said that "if feedbacks are indeed negative, then manmade global warming becomes, for all practical purposes, a non-issue." What real-world data prove that increasing CO_2 will result in fewer clouds?

In 1988 Steven Schneider said "Clouds are an important factor about which little is known" (Revkin 1988). "When I first started looking at this in 1972, we didn't know much about the feedback from clouds. We don't know any more now than we did then."

Did climate models have the feedback from clouds correct in 1988? Is the feedback from clouds any different now than it was three decades ago? Does the magnetic activity of the sun affect cosmic rays and the formation of clouds (Svensmark and Calder 2007)? Do climate modellers include cosmic rays in their models? Do climate modellers really believe their 2009 models have the formation of clouds correct in their models?

5: Can we estimate how much of the +0.76°C temperature departure recorded in February 1998 (Figure 4) can be attributed to El Niño and how much can be attributed to the CO_2 that originates from burning of fossil fuels?

Steven Schneider (Revkin 1988) said "To begin with, the magnitude of the various perturbations (to use the scientists' delicate word) of the environment are difficult to predict. And estimates of even the immediate effects of those perturbations are unreliable. Still harder to predict are the ground-level consequences of these effects - for example, the number of feet by which sea level will rise given a particular rise in the temperature of the globe, or the effects on phytoplankton of a particular increase in ultraviolet radiation caused by a particular reduction in the ozone layer. Harder yet to predict - lying, really, entirely in the realm of speculation - are the synergistic consequences of all or some of these effects. And lying completely beyond prediction are any effects that have not yet been anticipated."

"For all these reasons, the margin for error is immense. And that, of course, is the real lesson to be learned from the world's earlier attempts at predicting global perils. What the mistakes show is that in these questions even the most disinterested and professional predictions are filled with uncertainty. Uncertainty in such forecasts is not a detail, soon to be cleared up; it is part and parcel of the new situation - as inextricably bound up with it as mounting levels of carbon dioxide or declining levels of ozone. For the scientists' difficulties do not stem merely from some imperfections in their instruments or a few distortions in their computer models; they stem from the fundamental fact that at this particular moment in history mankind has gained the power to intervene in drastic and fateful ways in a mechanism - the ecosphere - whose overall structure and workings we have barely begun to grasp."

6: How did the IPCC determine that it is extremely unlikely that warming in the past 50 years was caused by natural fluctuations?

Table 9.4 in WG1 (page 792; IPCC 2007b) provides a synthesis of "climate change detection results." Regarding surface temperature, the authors state that it is extremely likely (>95%) that "warming during the past half century cannot be explained without external radiative forcing." We wonder, exactly what does this statement mean? Are the authors simply predicting that researchers (e.g. Svensmark and Calder 2007; Spencer and Braswell 2008; Klotzbach *et al*. 2009) will never publish papers to suggest that natural variation in clouds could explain the warming?

We agree that humans have altered surface temperatures by construction of roads and cities, afforestation, producing black carbon (i.e. soot), burning of fuel (which releases heat and water vapour). We have no doubt that temperatures records are biased upwards because of "heat islands" and because thermometers are often located in improper locations (Klotzbach *et al*. 2009). However, it is not clear how the ">95% likelihood" value was obtained. Was it obtained from "an elicitation of expert views" (IPCC 2005) or from a quantitative analysis of output from climate models (Tett *et al*. 1999)?

7: What system was sampled when declaring an anthropogenic change has been detected with less than 1% probability?

In 2001, the IPCC panel concluded that "most of the observed warming over the last 50 years is likely due to increases in greenhouse gas concentrations due to human activities." In 2007, the IPCC authors go on to say that "Anthropogenic change has been detected in surface temperature with very high significance levels (less than 1% error probability)"(IPCC 2007b). We wonder how the authors went about calculating a p-value of <1% if there is confounding between CO_2 increases and natural changes in clouds? We asked a few IPCC experts, they said the p-value was obtained by generating a data set from a computer model. In other words, you create a virtual world without people, generate hypothetical temperatures from the virtual world, compare the two sets (virtual world with people and virtual world without people) and then generate a p-value.

In 2007, Dr. Bob Carter (Adjunct Professorial Research Fellow - James Cook University) wrote "In the present state of knowledge, no scientist can justify the statement: 'Most of the observed increase in globally averaged temperature since the mid-20th century is very likely due [90 per cent probable] to the observed increase in anthropogenic greenhouse gas concentrations,' as stated in the IPCC's 2007 Summary for Policy Makers." We agree with Dr. Carter. We assume that virtual worlds were sampled to determine the 1% probability. We claim that the 1% probability was applied to output from climate models and not to replications made from the real world.

8. One climate model suggests that increasing the albedo of the Earth's surface from deforestation is stronger than the CO_2 effect from deforestation. Would harvesting native forests in temperate and boreal zones (plus making wood furniture and lumber from the harvested logs) and converting the land to pastureland cool the Earth?

After examining a virtual Earth, Bala *et al*. (2007) said "We find that global-scale deforestation has a net cooling influence on Earth's climate, because the warming carbon-cycle effects of deforestation are overwhelmed by the net cooling associated with changes in albedo and evapotranspiration." Has this climate model been verified? If an increase the albedo (from deforestation) is more powerful than the CO_2 effect (South 2008a), why are albedo credits (South and Laband 2008) not included in Climate Trading Schemes?

9. IPCC authors predict an increase in the number of record hot temperatures and that this will often cause a decline in the number of record cold temperatures. Are there data to support this claim? Is it true that an increase in record high temperatures will result in a decline in record low temperatures?

Solomon and others (IPCC 2007a) say that "linking a particular extreme event to a single, specific cause is problematic" and we concur. However, the authors go on to say that "An increase in the frequency of one extreme (e.g., the number of hot days) will often be accompanied by a decline in the opposite extreme (in this case the number of cold days such as frosts)." We do not know of a reference to support this claim. We question the claim that the probability of a record cold event in January or July is less now than it was in the 19th century. In fact, in 2009, six U.S. states set cold temperature records (115 year data) for the month of July (IA, IL, IN, OH, PA, WV). Why did these records occur if the probability of a cold July is less now than it was in 1893?

We also question the claim that "In some cases, it may be possible to estimate the anthropogenic contribution to such changes in the probability of occurrence of extremes." How is this possible? Other than simply guessing, we fail to see how a scientist could estimate an anthropogenic contribution to an increase in frequency of record cold/high

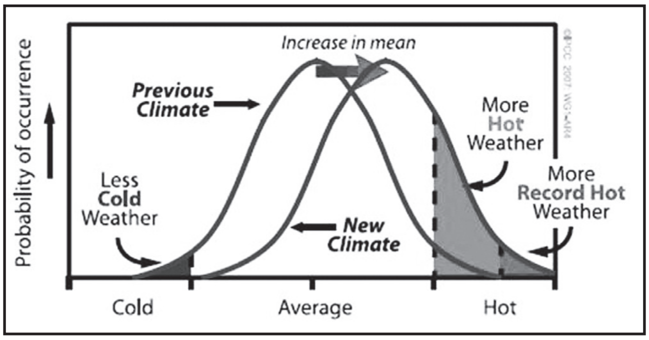

Figure 5. Schematic showing the IPCC view that little or no skew and kurtosis occurs when the mode shifts by +0.7 °C. The authors suggest the probability of extreme low temperatures decrease in proportion to the probability of high temperature (Figure 1, Box TS.5 from IPCC 2007a).

temperatures. Rare events do occur in nature. Researchers can certainly show a correlation, but how would they determine how much of the 0.76 °C departure in Figure 4 is anthropogenic? We "estimate" that 99% of this value is due to El Niño but we admit this estimate can not be verified.

Solomon, Qin, Manning and others suggest temperatures for a given region or for the Earth follow a "familiar 'bell' curve" and when the climate warms (for whatever reason), the entire distribution is shifted to the right (Figure 5). They suggest that a histogram of the pattern of temperature occurrences is similar for both the "previous climate"

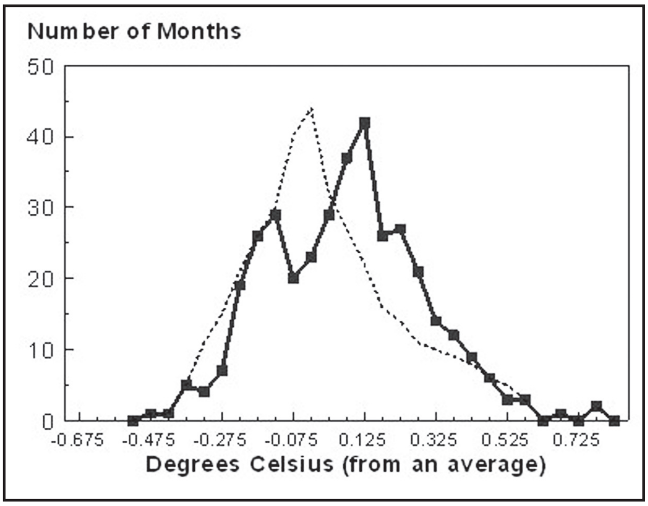

Figure 6. Histogram showing actual data (N = 367) from satellites over the period (December 1978 to June 2009). Each solid square represents the number of months that the temperature of the troposphere (above the southern hemisphere oceans) varied from an arbitrary mean value. Data (ie. solid squares) obtained from the Climate Center University of Alabama at Huntsville (http://www.ncdc.noaa.gov/oa/climate/research/uahncdc.lt). The dashed line represents a hypothetical distribution from a cooler period in the past. In this graph, the tails from both curves are deliberately identical. The hypothetical line was drawn so that the probability of extreme events is not changed.

Table 1. Dates of record high and low temperatures for some southern hemisphere locations (as of December 2008). Note that in these cases, the record low temperature occurred after the record high temperature. Although these records do not prove anything, they are not hypothetical. Note that no record high temperature occurred after 1975 and all record low temperatures but one occur after 1970.

Country/location	Record	°C	Date
Antarctica	High	14.6	5 January, 1974
	Low	-89.2	21 July, 1983
Argentina	High	48.9	11 December, 1905
	Low	-33	1 June, 1907
Australia	High	50.7	2 January, 1960
	Low	-23	29 June, 1994
New Zealand	High	42.4	7 February, 1973
	Low	-21.6	3 July, 1995
South Africa	High	50	3 November, 1918
	Low	-18.6	28 June, 1996
South America	High	49.1	2 January, 1920
	Low	-39	17 July, 1972

and the "new" warmer climate. We propose an alternate hypothesis (Figure 6). The distribution is negatively skewed with the tails about the same as before. A third hypothesis suggests that the warmed distribution becomes negatively skewed and flatter (i.e. platykurkic). This hypothesis is supported by predictions of ocean temperatures by the Max Planck Institute (National Assessment Synthesis Team 2000; page 83). Are there any actual data to support the IPCC hypothesis that assumes no change in kurtosis or skewness?

In Table 1, we provide some extreme high and low temperatures for selected land based locations in the Southern Hemisphere. Note that for these locations, no record high temperature occurred after 1975 and all but one record low temperature occurred after 1970. The occurrence of extreme low temperatures following record high temperatures in the southern hemisphere is interesting, especially since this is counter to the "no change in skew or kurtosis" hypothesis. The theory presented in Figure 5 suggests a 0% probability of a record extreme cold event occurring after global warming.

We predict that one or more of the records in Table 1 will be broken by the year 2100. If Antarctica drops below -90 °C, someone might claim it was caused by humans (perhaps due to chemicals depleting the ozone layer). Likewise, if a record high temperature occurs in Australia or New Zealand, we will likely read that it was caused by humans. The experts quoted might even take an unscientific approach and provide a probability in an attempt to prove the event was anthropogenic.

10. Solar irradiance that reaches the Earth's surface has declined since 1950. How much of reduction in irradiance is due to an increase in clouds and how much is due to an increase in pollution (i.e. soot and aerosols)?

"As the average global temperature increases, it is generally expected that the air will become drier and that evaporation from terrestrial water bodies will increase. Paradoxically, terrestrial observations over the past 50 years show the reverse" (Roderick and Farquhar 2002). How much of the "global dimming" (Stanhill 2005) is due to humans caused air pollution and how much is due to a negative feedback from clouds?

11. Why do some forest researchers use statistical downscaling approaches when the scenarios have largely been regarded as unreliable and too difficult to interpret?

Wilby and others (2004) have pointed out that some modellers combine coarse-scale (i.e. hundreds of kilometres), global climate models with higher spatial resolution, regional models sometimes having a resolution as fine as tens of kilometres. Most of the statistical downscaling approaches "are practiced by climatologists rather than by impact analysts undertaking fully fledged, policy oriented impact assessments. This is because the scenarios have largely been regarded as unreliable, too difficult to interpret, or do not embrace the range of uncertainties in GCM projections in the same way that simpler interpolation methods do. This means that downscaled scenarios based on single GCMs or emission scenarios, when translated into an impact study, can give the misleading impression of increased resolution equating to increased confidence in the projections" (Wilby *et al*. 2004).

12. When comparing similar locations and the same number of weather stations in NZ, has the average temperature changed much since 1860?

We agree that natural events affect the Earth's temperature (e.g. McLean *et al*. 2009). We also agree that human activities such as deforestation, afforestation, irrigation, road construction, city construction, etc. can alter the albedo of the Earth's surface. However, we are uncertain that average temperatures experienced in NZ during 1971 to 2000 are that much different than the temperatures experienced from 1861 to 1866 (Table 2). Why do temperatures records from Hokitika, NZ (since 1866) show no increase in temperature (Gray 2000)?

Predicted annual temperature changes (in °C) relative to 1980-1999 have been predicted for 12 climate models (Table A2.1 Ministry for the Environment. 2008). All 12 models predict an increase in temperature for NZ (for the period 2030 to 2049). A German model predicts only a 0.33 °C increase while a Japanese model predicts a 2 °C increase. In contrast, an older model (of unknown origin), predicts that NZ will be cooler in July 2029 than it was in July of 1987 (Revkin 1988). There are only about two decades to go before the year 2030, so it will be interesting to see which of the 13 models is closest to the observed data. When compared to 1987, will NZ be cooler in the winter of 2028 than most other locations in the world (Revkin 1988) or will it be about 2 °C warmer (e.g. miroc32 hires)?

13. Do outputs from climate models allow some researchers to selectively ignore real-world observations?

Farman *et al*. (1985) were the first to report a reduction

Table 2: A comparison of temperature data from five locations in New Zealand with predicted temperature in 2040. Pre-1868 data are from New Zealand Institute Transactions and Proceedings 1868 (http://tinyurl.com/7ycpl6) and post-1970 data are from National Institute of Water and Air Research (http://tinyurl.com/a5nj3c). Guesses for annual mean temperature for the year 2040 are in brackets (from Table 2.2 Ministry for the Environment. 2008). Table adapted from Vincent Gray.

Station	Years of data	Before 1867	Years of data	1971-2000	2040
		°C		°C	°C
Auckland	15	15.7	25	15.1	[16.0]
Taranaki - New Plymouth	12	13.7	20	13.6	[14.5]
Nelson	16	12.8	25	12.6	[13.5]
Christchurch	11	12.8	26	12.1	[13.0]
Dunedin	15	10.4	26	11.0	[11.9]
Mean		13.1		12.9	

in the Antarctic ozone hole. Some experts at first dismissed the observations of the British scientist since Farman's findings differed with predictions generated using NASA computer models (Schell 1989). This is not the only case where output from an unverified computer model was initially given more credence than actual observations. Recently, Svensmark and Calder (2007) provide data to propose a new theory of global warming. Have researchers relied on an unverified computer model to disprove a new theory of climate change (Pierce and Adams 2009)?

14. Do foresters rely on predicted timber prices that are generated from combining three complex computer models?

A climate model, a biogeochemistry model and an economics model were used to predict standing timber prices for the United States (Joyce *et al*. 2001). Prices were predicted to increase by 5 to 7% from 2000 to 2010 but no error bars were included the graph. In contrast, actual prices for standing sawlogs in 2009 are generally lower than they were in 2000 (in some cases 40% lower). Would any forestry consultant rely on 10-year price forecasts generated by combining three complex computer models? Do researchers actually believe they can determine what the price of standing timber would be in the year 2050 if CO_2 levels in the atmosphere were kept at 355 ppmv (Ireland *et al*. 2001)?

15. To capture the public imagination, should foresters offer up scary scenarios?

Stephen Schneider (Schell 1989) said "as scientists, we are ethically bound to the scientific method, in effect promising to tell the truth, the whole truth, and nothing but - which means that we must include all the doubts, the caveats, the ifs, ands, and buts. On the other hand, we are not just scientists but human beings as well. And like most people we'd like to see the world a better place, which in this context translates into our working to reduce the risk of potentially disastrous climatic change. To do that we need to get some broad-based support, to capture the public's imagination. That, of course, entails getting loads of media coverage. So we have to offer up scary scenarios, make simplified, dramatic statements, and make little mention of any doubts we might have. This 'double ethical bind' we frequently find ourselves in cannot be solved by any formula. Each of us has to decide what the right balance is between being effective and being honest. I hope that means being both."

Conclusions

We are concerned the scientific method is being downplayed in today's world. Hypothesis testing is an irreplaceable tool in science, but some no longer test hypothesis and others do not declare their doubts. Now, all that is needed to set policy is an unverified computer model, some warnings about the future, some name calling, and a good marketing program. Debate is essential to scientific progress, but it seems it is no longer in vogue. Sometimes, those who ask questions (like the 15 above) are ignored, suppressed, or attacked with name calling (e.g. see Witze 2006; Seymour and Gainor 2008; South 2008b).

Our profession should be a place where questions about computer models (either process based forestry models or three-dimensional climate models) are welcomed. Debate should be encouraged and hypotheses should be tested (not simply proposed). However, it now seems a number of researchers and foresters have accepted the hypothesis that CO_2 is the primary driver of a changing climate. Some ignore factors such as changes in cloud cover, changes in surface albedo (Gibbard *et al*. 2005), changes in cosmic rays, increases in soot (in air and on ice), and the Pacific Decadal Oscillation. Ignoring these factors appears to be driven by the idea that the Earth's complex climate system is relatively easy to control by planting more trees on temperate and boreal grasslands.

We hope our profession will rise above soothsaying and will encourage debate on topics and policies that affect our forests. As NZIF members, if we choose not to question authority, we might be accused of violating our code of ethics.

What if Climate Models are wrong?

People who trust IPCC climate projections (e.g. Figure 1) also believe that Earth's atmospheric Greenhouse Effect is a radiative phenomenon and that it is responsible for raising the average surface temperature by 33°C compared to an airless environment. According to IPCC Third Assessment Report (2001): "*For the Earth to radiate 235 watts per square meter, it should radiate at an effective emission temperature of -19°C with typical wavelengths in the infrared part of the spectrum. This is 33°C lower than the average temperature of 14°C at the Earth's surface*." Mainstream climate science relies on a simple formula based on Stefan-Boltzmann (S-B) radiation law to calculate Earth's average temperature without an atmosphere (i.e. -19°C). This formula is also employed to predict Moon's average temperature at -20 C (253K) (e.g. NASA Planetary Fact Sheet). But is the magnitude of the atmospheric greenhouse effect really 33 C? What if the surface temperature of Earth without an atmosphere were much colder? What if the popular mean temperature estimate for the Moon were off by more than 50 C?

Although we cannot experimentally verify the -19 C temperature prediction for a hypothetical airless Earth, we could check if the predicted -20 C average temperature for the Moon is correct. After all, the Moon can be viewed as a natural grey-body equivalent of Earth, since it orbits at the same distance from the Sun and has virtually no atmosphere (the gas pressure at the lunar surface is only about 3×10^{-10} Pa). Recent data from the Diviner instrument aboard NASA's Lunar Reconnaissance Orbiter as well as results from detailed thermo-physical models (e.g. Vasavada et al. 1999, 2012) indicate that the Moon average surface temperature is actually -76 C (197.3K). Diviner measurements discussed by Vasavada et al. (2012) show that, even at the lunar equator (the warmest latitude on the Moon), the mean annual temperature is -60 C (213K) or 40 C *cooler* than the above theoretical global estimate. Why such a large discrepancy between observed and calculated lunar temperatures?

According to a new analysis by Volokin & ReLlez (2014), climate scientists have grossly overestimated Moon's average temperature and Earth's black body temperature for decades due to a mathematically incorrect application of the S-B law to a sphere. The current approach adopted by climate science equates the mean physical temperature of an airless planet (T_{gb}, K) with its effective emission temperature (T_e, K) calculated from the equation:

$$T_e = \left[\frac{S_o(1-\alpha_p)}{4\epsilon\sigma}\right]^{0.25} \qquad (1)$$

where S_o is the solar irradiance (W m^{-2}), i.e. the shortwave flux incident on a plane perpendicular to solar rays above the planet's atmosphere, α_p is the planet average shortwave albedo, ϵ is the surface thermal emissivity ($0.95 \le \epsilon \le 0.99$), and $\sigma = 5.6704\times10^{-8}$ W m^{-2} K^{-4} is the

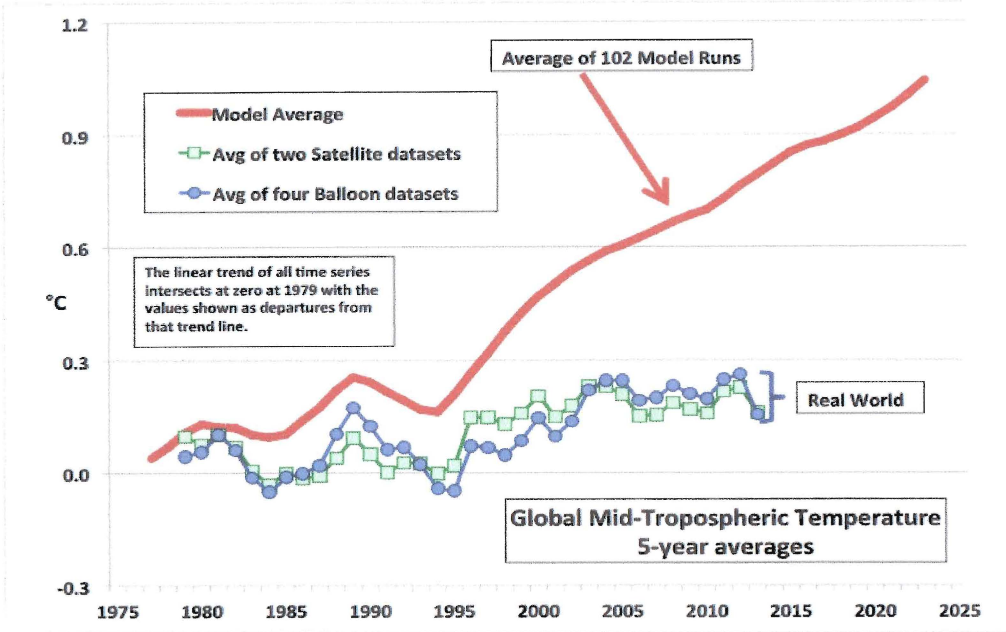

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Figure 1. Model projections of global mid-tropospheric temperature (red line) compared to observed temperatures (blue and green lines). Figure courtesy of Dr. John Christy

S-B constant. The factor ¼ serves to re-distribute the solar flux from a flat surface to a sphere. It arises from the fact that the surface area of a sphere ($4\pi R^2$) is 4 times larger than the surface area of a flat disk (πR^2) with the same radius R. Inserting appropriate parameter values for Earth in Eq. (1), i.e. $S_o = 1361.7$ W m^{-2}, $\alpha_p = 0.305$, and $\epsilon = 1.0$, produces $T_e = 254.2$K (-19 C), which is the basis for the above IPCC statement. We note that the -20 C (253K) temperature estimate for the Moon is obtained from Eq. (1) using $\alpha_p = 0.305$, which is Earth's albedo that includes the effect of clouds and water vapor on shortwave reflectivity. However, the correct albedo value is the Moon 0.12 - 0.13, which yields ~270 K (- 3 C) for the Moon average temperature according to Eq. (1).

Equation (1) employs a spatially averaged absorbed solar flux to calculate a mean surface temperature. This implies a uniform distribution of the absorbed solar energy across the planet surface and a homogeneous temperature field. However, these assumptions are grossly inaccurate, because sunlight absorption on a spherical surface varies greatly with latitude and time of day resulting in a highly non-uniform distribution of surface temperatures. This fact along with the non-linear (4^{th} root) dependence of temperature on radiative flux according to S-B law creates a relationship known in mathematics as Hölder's inequality between integrals (e.g. Abualrub and Sulaiman 2009; Wikipedia: Hölder's inequality). Hölder's inequality applies to certain types of non-linear functions and states that, in such functions, the use of an arithmetic average for the independent distributed variable will *not* produce a physically correct mean value for the dependent variable. In our case, due to a non-linear relationship between temperature and radiative flux and a strong dependence of the absorbed solar flux on latitude, one cannot correctly calculate the true mean temperature of a uni-directionally illuminated planet from the spatially averaged radiative flux as attempted in Eq. (1). Due to Hölder's inequality, the effective emission temperature produced by Eq. (1) will always be significantly higher than the physical mean temperature of an airless planet, i.e. $T_e \gg T_{gb}$.

Volokin & ReLlez (2014) showed that, in order to derive a correct formula for the mean physical temperature of a spherical body, one must *first* take the 4th root of the absorbed radiation at every point on the planet surface, and *then* average (integrate) the resulting temperature field rather than calculate a temperature from the spatially averaged solar flux as done in Eq. (1). Using proper spherical integration and accounting for the effect of regolith heat storage on nighttime temperatures, Volokin & ReLlez (2012) derived a new analytical formula for the mean surface temperature of airless planets, i.e.:

$$T_{gb} = \frac{2}{5}\left[\frac{S_o\,(1-\alpha_e)}{\epsilon\sigma}\right]^{0.25} \Phi(\eta_e) \qquad (2)$$

where $\Phi(\eta_e)$ is given by:

$$\Phi(\eta_e) = (1-\eta_e)^{0.25} + 0.931\,\eta_e^{\;0.25} \qquad (3)$$

Here, α_e is the effective shortwave albedo of the planet surface, η_e (eta) is the effective fraction of absorbed solar flux stored as heat in the regolith through conduction, and $\Phi(\eta_e) \geq 1.0$ is a dimensionless scaling factor that boosts the average global temperature above the level expected from a planet with zero thermal inertia, i.e. if the surface were completely non-conductive to heat. Thanks to $\eta_e > 0$ (non-zero storage of solar energy in the regolith), the night side of airless celestial bodies remains at a significantly higher temperature than expected from the cosmic microwave background radiation (CMBR) alone. This increases the mean global planetary temperature. The fraction of solar flux stored in regolith can theoretically vary in the range $0.0 \leq \eta_e \leq 1.0$. In reality, however, due to physical constrains imposed by the regolith thermal conductivity, this range is much narrower, i.e. $0.005 < \eta_e < 0.015$, which limits the temperature enhancement factor to $1.25 < \Phi(\eta_e) < 1.32$. According to Eq. (3), $\Phi(\eta_e)$ has a non-linear dependence on η_e - it increases for $0.0 \leq \eta_e \leq 0.5$ and decreases when $0.5 \leq \eta_e \leq 1.0$ reaching a maximum value of 1.627 at $\eta_e = 0.5$. However, since it is physically impossible for a planet's regolith to store on average as much as 50% of the absorbed solar flux as heat, $\Phi(\eta_e)$ cannot practically ever reach its theoretical maximum.

Independent thermo-physical calculations along lunar latitudes yielded $\eta_e = 0.00971$ for the Moon, hence $\Phi(\eta_e) = 1.29$ according to Eq. (3). Due to the lack of moisture and convective heat transport between soil particles in an airless environment, the apparent thermal conductivity of the regolith of celestial bodies without atmosphere is much lower than that on Erath resulting in values for η_e close to 0.01. Volokin & ReLlez (2014) showed that Eq. (2) quite accurately predicts Moon's true average surface temperature of 197.3 K (within 0.25 K) using observed and independently derived values for $S_o = 1361.7$ W m^{-2}, $\alpha_e = 0.13$, and

$\epsilon = 0.98$, and η_e. In general, Formula (2) is expected to be valid for any airless spherical body provided $S_o \geq 0.15$ W m^{-2}. If solar irradiance is lower than 0.15 W m^{-2}, then the relative contribution of CMBR to planet's temperature becomes significant and another, more elaborate formula for T_{gb} needs be used (see Volokin & ReLlez 2014).

Equation (2) demonstrates that T_{gb} is physically incompatible with T_e via the following comparison. Using $S_o = 1{,}361.7$, $\alpha_e = 0.13$ and $\epsilon = 0.98$ in Eq. (1) yields $T_e = 270.2$K for the Moon. This estimate is 21.5K *higher* than the *maximum* theoretically possible temperature $T_{gb} = 248.7$K produced by Eq. (2) using the same input parameters and a physically *unreachable* peak value of $\Phi(\eta) = 1.627$ corresponding to $\eta_e = 0.5$. Therefore, it is principally impossible for an airless planet to reach an average global temperature as high as its effective emission temperature! This renders T_e a pure mathematical construct rather than a measurable physical quantity implying that T_e is principally different from T_{gb} and should not be confused it.

Earth's atmospheric greenhouse effect (AGE) can be measured as a difference between the actual average global surface temperature (T_s) and the mean temperature of an equivalent grey body with no atmosphere orbiting at the same distance from the Sun such as the Moon. Adopting T_e as the grey-body's mean temperature, however, produces a meaningless result for AGE because a non-physical (immeasurable) temperature (T_e) is being compared to an actual physical temperature (T_s). Hence, the correct approach to estimating the magnitude of AGE is to take the difference between T_s and T_{gb}, i.e. two physical palatable temperatures. Using the current observed average global surface temperature of 14.4°C (287.6K) (NOAA National Climate Data Center: Global Surface Temperature Anomalies) and the above estimate of Earth's true gray-body mean temperature (i.e. Moon's actual temperature), we obtain AGE = 287.6 K – 197.3 K = 90.3 K. In other words, the greenhouse effect of our atmosphere is nearly 3 times larger than presently assumed! This raises the question: can so-called greenhouse gases, which collectively amount to less than 0.4% of the total atmospheric mass, trap enough radiant heat to boost Earth's average near-surface temperature by more than 90 K? Or is there another mechanism responsible for this sizable atmospheric thermal effect in the lower troposphere? Observations show that the lower troposphere emits on average 343 W m^{-2} of long-wave radiation towards the surface (e.g. Gupta et al. 1999; Pavlakis et al. 2003; Trenberth et al. 2009). Such a thermal flux is 44% *larger* than the global averaged solar flux absorbed by the *entire* Earth-atmosphere system (i.e. 238-239 W m^{-2}) (Lin et al. 2008; Trenberth et al. 2009). This fact implies that the lower troposphere contains more kinetic energy than can be accounted for by the solar input alone. Considering the negligible heat storage capacity of air, these measurements suggest the plausibility of an alternative non-radiative AGE mechanism. Consequently, if another major AGE mechanism existed that is not considered by the current climate science, what would this imply for the reliability and accuracy of climate-model projections based on the present radiative Greenhouse paradigm?

In closing, we concur with physicist and Nobel Prize laureate Richard Feynman, who said: "*It does not make any difference how beautiful your guess is, it does not make any difference how smart you are, who made the guess, or what his name is - if it disagrees with experiment, it is wrong! That is all there is to it.*" (1964 lecture at Cornell University).

References

Abualrub, N.S. and W.T. Sulaiman (2009) A Note on Hölder's Inequality. *International Mathematical Forum*, 4(40):1993-1995.
http://www.m-hikari.com/imf-password2009/37-40-2009/abualrubIMF37-40-2009-2.pdf

Gupta, S. K., Ritchey, N. A., Wilber, A. C., and Whitlock, C. A. (1999). A Climatology of Surface Radiation Budget Derived from Satellite Data, *J. Climate*, 12, 2691–2710.

IPCC Third Assessment Report (2001), Working Group I: The Scientific Basis. Chapter 1
http://www.ipcc.ch/ipccreports/tar/wg1/pdf/TAR-01.PDF

Lin, B., Paul W. Stackhouse Jr., Patrick Minnis, Bruce A. Wielicki, Yongxiang Hu, Wenbo Sun, Tai-Fang Fan, and Laura M. Hinkelman (2008). Assessment of global annual atmospheric energy balance from satellite observations. J. Geoph. Res. Vol. 113, p. D16114.

NASA Planetary Facts Sheet (http://nssdc.gsfc.nasa.gov/planetary/factsheet/)

NOAA National Climate Data Center: Global Surface Temperature Anomalies (http://www.ncdc.noaa.gov/cmb-faq/anomalies.php)

Pavlakis, K. G., D. Hatzidimitriou, C. Matsoukas, E. Drakakis, N. Hatzianastassiou, and I. Vardavas (2003). Ten-year global distribution of downwelling longwave radiation. *Atmos. Chem. Phys. Discuss*., 3, 5099-5137.

Trenberth, K.E., J.T. Fasullo, and J. Kiehl (2009). Earth's global energy budget. *BAMS*, March:311-323

Vasavada, A. R., J. L. Bandfield, B. T. Greenhagen, P. O. Hayne, M. A. Siegler, J.-P. Williams, and D. A. Paige (2012). Lunar equatorial surface temperatures and regolith properties from the Diviner Lunar Radiometer Experiment. *J. Geophys. Res*., 117, E00H18, doi:10.1029/2011JE003987.

Vasavada, A. R., D. A. Paige, and S. E. Wood (1999). Near-surface temperatures on Mercury and the Moon and the stability of polar ice deposits. *Icarus*, 141, 179–193, doi:10.1006/icar.1999.6175.

Volokin, D. and ReLlez, L. (2014). On the Average Temperature of Airless Spherical Bodies and the Magnitude of Earth's Atmospheric Thermal Effect. (*under review*).

Wikipedia: Hölder's inequality (http://en.wikipedia.org/wiki/H%C3%B6lder's_inequality)