Exhibit No:	___
Issues:	Weather Normals Weather Normalization
Witness:	Robert E. Livezey
Exhibit Type:	Direct
Sponsoring Party:	Missouri Gas Energy
Case No:	GR-2009-___
Date:	April 2, 2009

MISSOURI PUBLIC SERVICE COMMISSION

MISSOURI GAS ENERGY

CASE NO. GR-2009-__

DIRECT TESTIMONY OF

DR. ROBERT E. LIVEZEY

Jefferson City, Missouri

April 2009

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CASE NO. GR-2009-__

INDEX TO TESTIMONY

PAGE

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Case No. GR-2009- __

QUALIFICATIONS

Q. PLEASE STATE YOUR NAME AND BUSINESS ADDRESS.

A. Dr. Robert Livezey, 5112 Lawton Drive, Bethesda, MD 20816.

Q. WHAT IS YOUR OCCUPATION?

A. Since retiring as Chief of National Weather Service ("NWS") Climate Services in 2008, I have been a self-employed consultant on matters related to climate normals, variability, change, and prediction.

Q. PLEASE DESCRIBE YOUR QUALIFICATIONS TO TESTIFY IN THIS CASE.

A. My doctoral research at the Pennsylvania State University, completed in 1973, addressed the energy balances and controls of planetary-wide wind and storm systems that regulate the globe's climate. For 33 of the intervening 36 years, my work and research has been focused on the fields of climate variability, change, and prediction.

I am considered one of the top experts in the world on climate statistics[1] and estimating and tracking weather/climate normals and post-war climate change over North America, and as possibly the leading expert worldwide on short-term North American climate variations and their prediction. I have produced almost 60 peer-refereed publications and book chapters and at least that many conference pre-prints, post-prints, and the like. Almost all of these publications are directly relevant to topics I discuss in this testimony. Awards and appointments from academia, the National Oceanographic and Atmospheric Administration ("NOAA"), and professional associations have institutionally recognized my expertise. I was awarded a Commerce Department Gold Medal in 1998 and elected as a Fellow of the American Meteorological Society ("AMS") in 1993. Earlier, I received an AMS Editor's Award and served as Editor of the prestigious AMS *Journal of Climate* ("JOC"), where I was responsible for all submissions on climate statistics and prediction. I have been a member of the AMS Committee on Climate Variability and twice the chair of the Committee on Probability and Statistics, and very recently became a member of the AMS Publication Commission.

Q. WHAT IS YOUR PROFESSIONAL EXPERIENCE?

A. From 1973 to 1976 I held two faculty positions (at Penn State and the University of Missouri-Columbia) followed by three years as a hurricane modeler in Washington. From 1980-84 I served as a journeyman climate forecaster and solidified my climate research credentials at NOAA's Climate Prediction Center ("CPC", f/k/a as the Climate Analysis

[1] I am listed in the acknowledgments or table of contents of the three primary text sources for this subject. Recently, I have been invited to be a lecturer for the prestigious 6th GKSS School of Environmental Research, the School on Statistical Analysis in Climate Research, to be held in Lecce, Italy, in October of this year (see http://coast.gkss.de/events/6thschool/syllabus.html).

Center at that time) before moving on to NASA's Goddard Space Flight Center as Chief of the Experimental Climate Forecast Center. After two years (in 1986), I returned to CPC, where I served as both Senior and Principal Scientist and was Lead Seasonal Forecaster during my tenure through 1999. In my last eight years of federal service (2000-2007), I served as Chief of NWS Climate Services, and was cited for this service through five awards, including two prestigious NOAA Administrator Awards. As head of all NWS Climate Services, I was responsible for policy, customer requirements, and management of the infrastructure for NWS climate observations, forecasts, and information. This required close external working partnerships with NOAA's National Climatic Data Center ("NCDC"), which is the organization responsible for managing climate data and producing official climate normals, with the university-based Regional Climate Centers, and with the American Association of State Climatologists. The latter organization has elected me to Associate Membership and invited me to serve *ex officio* on its Executive Committee.

Q. HAVE YOU PREVIOUSLY PROVIDED EXPERT WITNESS TESTIMONY?

A. Yes, I have. Since retirement from federal service, I have filed expert witness testimony before both the Iowa Utilities Board and the Colorado Public Utilities Commission.

INTRODUCTION

Q. FOR WHOM ARE YOU TESTIFYING IN THIS MATTER?

A. I am testifying on behalf of Missouri Gas Energy ("MGE" or "Company").

Q. WHAT IS THE PURPOSE OF YOUR PREPARED DIRECT TESTIMONY?

A. My testimony will provide an explanation of climate normals, review my team's research and conclusions regarding changing climate normals, compare various methods for predicting the current climate, and make a recommendation to the Missouri Public Services Commission ("PSC") for defining "normal" weather for purposes of ratemaking.

Q. HOW DO YOU ORGANIZE THE BALANCE OF YOUR DIRECT TESTIMONY?

A. My testimony is organized into the following sections:

- CLIMATE NORMALS, THEIR USE AND ESTIMATION
- RESEARCH ON TRACKING CLIMATE AND ESTIMATING NORMALS
- IMPLICATIONS FOR MISSOURI NORMALS
- OVERVIEW AND RECOMMENDATIONS

Q. DO YOU SPONSOR ANY SCHEDULES?

A. Yes, I do. I sponsor the following Schedules:

- Schedule REL-1 – "Estimation and Extrapolation of Climate Normals and Climatic Trends" coauthored by myself and published in the November 2007 issue of the *Journal of Applied Meteorology & Climatology*,
- Schedule REL-2 -- April 23, 2008, *USAToday* article regarding increasing opposition to the U. S. Department of Agriculture's intention to base its latest release of its official "Plant Hardiness Zones" map on 30-year average temperatures, **and**

- Schedule REL-3 – “Redefining ‘normal’” by Bob Henson in *UCAR Winter 08-09 Quarterly*.

CLIMATE NORMALS, THEIR USE AND ESTIMATION

Q. PLEASE EXPLAIN HOW YOUR EXPERIENCE LED YOU TO YOUR RESEARCH ON CLIMATE NORMALS.

A. All three of the major roles I have played in climate science (researcher, forecaster, and services manager) intersect at climate normals. Analyses of climate variability have climate normals as their frame of reference; climate forecasts are issued in terms of departures from “normal;” and official climate normals and the observations underlying them are a major joint responsibility of NCDC and NWS. Thus, early in my career I had to confront directly the problem of estimating normals from data. By the late 1990s, I came to realize that I would have to account explicitly for climate change in the estimation of weather normals. More specifically, I discovered during my tenure at CPC that cold-season United States temperatures had been increasing over most of the country over the last few decades at a surprising rate, and concluded that CPC would have to find a new way to account for these changes in its seasonal forecasts.

Q. PLEASE DISCUSS IN SIMPLE TERMS CLIMATE NORMALS AND THEIR ESTIMATION

A. Changes in weather from year to year can be and often are very large. Because we cannot forecast these year-to-year weather changes, we have to rely on what we would expect average conditions over a number of years to be. This average is what we typically refer to as “climate normals.”

If there were no such thing as climate change, then it would be easy to estimate a climate normal if we had a good data record: the climate normal would be just the average over a large number of past years (the World Meteorological Organization “WMO” convention is 30 years). The result of this averaging for heating degree days (“HDDs”) would be a good “middle-of-the-road” basis for setting utility rates; on the average, it would be expected to be far closer to what actually occurs than would, say, a 10-year or 5-year average. This is because it is more difficult to smooth out, confidently, the large year-to-year changes when there are fewer and fewer years in the average. As the averaging period gets smaller and smaller, our confidence becomes less and less that the average is near the “middle of the road,” the climate normal. When the period decreases to a single year, the “standard error,” which is the average error you would expect when using the normal to represent any other year, will be the greatest of all, and thus our confidence in the estimate is at its least.

If the climate is changing, then determining what is “normal” becomes more difficult; the slow change has to be sifted out and distinguished from the large, almost (but not totally) random year-to-year fluctuations. Because weather changes from year to year are so large and not entirely random, in short segments of data, this “climate noise” sometimes gives the appearance that a climate change is occurring when it is not. In order to distinguish real climate change from this “climate noise,” which is necessary for us to know where the climate is today, we have to be guided by the body of knowledge, both empirical and theoretical, that meteorological and climatological science can provide. This was the basis for my work at NWS on normals described in the next section.

Q. WHY DOES NOAA CALCULATE AND REPORT NORMALS?

A. The main reason for calculating normals is to obtain representative descriptions of expected meteorological conditions at specific locations and times of the year, i.e. climate conditions, which are used for planning purposes and benchmarks for actual conditions (e.g. referring to conditions as "above" or "below" normal). In the context of "expected" conditions, normals have been used as base-line forecasts, or as best guesses of what future conditions (surface air temperatures, sea temperatures, precipitation, etc.) will be beyond the accuracy range of daily weather forecasts (5 to 10 days depending on time of year) and monthly and seasonal forecasts (out to a year).

Q. PLEASE DESCRIBE RELEVANT "PARTS" OF NOAA FOR NORMALS AND SOME HISTORY BEHIND 30-YEAR NORMALS.

A. Three parts of NOAA play the dominant roles in climate services and science, but only two of them play direct roles in the production of official normals. The two are NWS, which is responsible for the observations that are used to compute official normals, and (as previously noted) the National Environmental Satellite and Information Service's ("NESDIS") NCDC, which is responsible for normals production and dissemination. Climate prediction (forecasts beyond the range of accurate daily weather prediction) is also the responsibility of NWS and is conducted at CPC for seasonal forecasts out to a year in advance. Oceanic and Atmospheric Research ("OAR") is the third part of NOAA with a large role in climate. OAR produces multi-decadal climate projections.

Climate normal practices have evolved over many years but only became somewhat standard after the WMO recommended in 1984 the use of "climatological standard normals" consisting of 30-year averages updated at least every 30 years (1931-1960, 1961-1990, etc.). WMO also recommended updated 30-year "normals" every decade, a practice adopted by many countries including the United States. Thus, new official normals based on 1971-2000 data were released in 2003 by NCDC to replace those based on 1961-1990, and an updated set will be available in the early 2010s.

As it turns out, NOAA does not use normals at all in its routine daily weather forecasts out to 7 days. But more significantly, 30-year normals are not used at all in their "expected conditions" context for NOAA's suite of forecasts that go beyond 7 days, i.e. for all of the climate forecasts made by CPC and OAR. Weather and climate scientists have known for decades that 30-year normals are not generally of value for either day-to-day weather prediction or future climate prediction. I will discuss this point more later, but for now I would note that there is a growing recognition of this among industries and some are pursuing alternatives.

RESEARCH ON TRACKING CLIMATE CHANGE AND ESTIMATING NORMALS

Q. DID YOU PERFORM ANY ANALYSES REGARDING THE PREDICTION OF NORMAL TEMPERATURES, OR CLIMATE NORMALS?

A. Yes.

Q. PLEASE EXPLAIN.

A. Most recently, I co-authored a paper entitled, "Estimation and Extrapolation of Climate Normals and Climatic Trends" that was published in the November 2007 issue of the Journal of Applied Meteorology & Climatology. I have included a copy as Schedule REL-1. At the outset, I was guided in this work by other research I had completed in the mid-1990s. This earlier research (documented in the Livezey and Smith, 1999, citations in the recent paper and described later) provided a considerable basis for attributing U.S changes to global climate change and led to a superior new methodology for estimating normals during periods of climate change.

Q. BASED ON YOUR EXPERIENCE, DO YOU KNOW OF ANY OTHER SCIENTISTS WORLDWIDE THAT HAVE STUDIED THE PREDICTIVE VALUE OF 30-YEAR WEATHER NORMALS?

A. Yes, the key papers addressing the problem since the 1950s are cited in my attached paper (Schedule REL-1). All of these are handicapped by statistical sample problems, and none are as comprehensive as my paper in their treatment of the several superior alternatives to traditional 30 year averages for a more accurate prediction of normal temperatures. Nevertheless, they all agree with my conclusion that better alternatives often do exist.

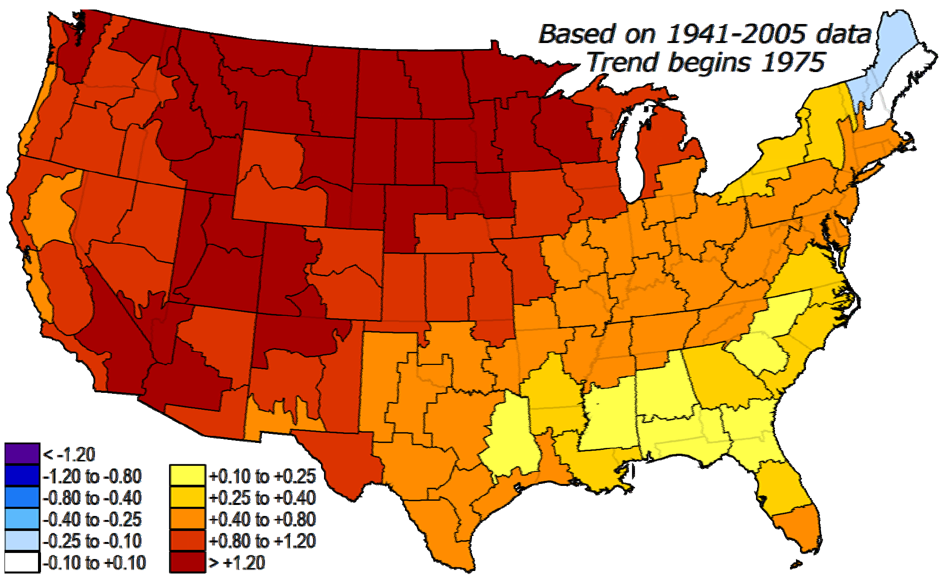
Q. IS A 30-YEAR AVERAGE STILL A REASONABLE ESTIMATE OF NORMAL TEMPERATURES?

A. No, it is not. We know that a 30-year normal will provide a relatively stable estimate when temperatures are very static, but under conditions of a warming climate, with certainty, will produce a best guess that will be cold-biased. Unfortunately, the assumption of inconsequential climate change cannot be made anymore. While there may be controversy over the cause of climate change or the seriousness of its impacts, there is virtually no reasonable controversy remaining over the fact that measurable climate change has taken place since the 1970s, globally as well as over the United States, and that the temperature increase is greatest over Northern Hemisphere continents in the wintertime. This condition is illustrated later in my testimony with some graphs of the United States.

Where it is undeniable that we have experienced decades of warming temperatures, use of a 30-year average to predict temperatures today will often result in "normal" temperatures that are significantly colder than the temperatures that will probably occur. Many individuals, businesses and organizations without knowledge of my research still mistakenly presume that the WMO 30-year standard remains a viable approach, but there is a growing intuitive awareness that new approaches are more appropriate. For example, this awareness is evidenced in an article that appeared on April 23, 2008, in *USAToday* that describes increasing opposition to the U. S. Department of Agriculture’s intention to base its latest release of its official “Plant Hardiness Zones” map on 30-year average temperatures. I have included a copy of this article as Schedule REL-2**.**

Q. PLEASE DESCRIBE THE CONCLUSIONS YOU DREW FROM YOUR RESEARCH.

A. These conclusions are set forth in the 2007 paper attached as Schedule REL-1. The paper concludes that for much of the wintertime United States, 30-year normals are a very poor choice as "best guesses" for mean temperature in a given year (absent advance knowledge, which we rarely have far in advance, of the climate noise). The underlying reason for this conclusion is illustrated in the map below that shows my estimates of how much (in degrees Celsius) January through March temperatures have warmed over the United States from 1975 to 2005. The warm shades (yellow to reds) represent consequential to extremely large warming, respectively; the country figuratively has "turned red" in the map, indicating substantially warmer temperatures.



Q. ARE BETTER "BEST GUESS" CHOICES AVAILABLE?

Guided by my own earlier work and the vast, pooled work of the Intergovernmental Panel on Climate Change[2] **("**IPCC") compiled in its report (Solomon *et al.*, Eds., 2007: *Climate Change, 2007: The Physical Science Basis.* Cambridge University Press), my colleagues and I have analyzed the relative performance of several alternatives for tracking changing normals; i.e., alternative best guesses for the coming winter's temperatures. Of these, we recommend one or another of two, the so-called "optimum climate normal" ("OCN") and "hinge fit," where the best method at a location depends on the easily estimated statistical character of both the climate change and climate noise. We find that the expected performance of these alternatives is generally superior to the use of 30-year normals. A conclusion from my research is that this finding is true for Missouri in particular.

Q. HOW HAS THE SCIENTIFIC COMMUNITY AND WEATHER INDUSTRY REACTED TO YOUR RESEARCH AND 2007 PAPER?

A. So far, the conclusions in the 2007 paper have not been challenged, either formally or informally, and have been favorably received by two governmental agencies, the CPC and the NCDC. For prediction purposes, the CPC has used and will continue to use variations of the alternatives (OCN or hinge fit) to the 30-year average recommended in my 2007 paper. Recall my point earlier that 30-year normals were originally intended to serve two purposes, as estimates of expected conditions (i.e. a forecast role) and as

[2] The IPCC is a scientific intergovernmental body set up by the WMO and by the United Nations Environment Programme (UNEP). It is open to all member countries of WMO and UNEP. U.S. participation includes every Department and Agency concerned with or impacted by changing environmental conditions.

benchmarks for current or actual conditions (i.e. a reference role). “Official” CPC forecasts no longer rely on 30-year normals as a forecast tool. While not used to forecast, the CPC does continue their use as references (in the form of “below normal,” “above normal,” etc.) as a convenience for the public. In other words, the “official” 30-year normals are used now only in packaging CPC forecasts, not in making them.

The other agency favorably reacting to my paper, NCDC, has initiated work that will lead to the release soon of both alternative statistics recommended in my 2007 paper to provide users the opportunity to consider their use.[3] Thus, my work is being taken seriously by official agencies that produce and rely on normals, and has not been challenged to date.

Q. WHAT HAVE BEEN THE REACTIONS TO YOUR RESEARCH AND CONCLUSIONS BY THE OFFICIAL AGENCIES YOU HAVE MENTIONED?

A. Official NOAA climate forecasters (CPC) had previously decided not to use 30-year averages at all to arrive at their best guess for future seasons and my work gave them additional support for their position and new alternatives to consider. Likewise, NOAA’s official climatologists (NCDC) have fully acknowledged the need to augment, if not totally replace, 30-year normals in response to my advice. I should also point out that my research was conducted in my capacity as a government official, and the 2007 paper was published with the approval of NOAA.

[3] NCDC’s progress and release plans are described in an article by Bob Henson in the *UCAR Winter 08-09 Quarterly* included as Schedule REL-3.

Q. PLEASE GENERALLY DESCRIBE THE RESEARCH THAT LED TO THE CONCLUSIONS OF THE 2007 PAPER.

A. In the mid-1990s, I performed research directed at trying to relate winter-to-winter changes over the United States to global climate observations. Even though I was not searching for a climate change signal and was not explicitly computing trends, I found that when the effects of climate noise (*e.g.*, El Nino/La Nina and the North Atlantic Oscillation)[4] are removed, there is a relationship between a global-scale pattern in ocean temperatures and U. S. winter temperature patterns. This relationship showed little or no change in average temperatures from one decade to the next for the U.S. and large key areas over the global ocean from about 1940 to around the mid-1970s, and relatively steady warming thereafter for both. If this relationship was shown graphically, the viewer would note a 30-plus-year period of stable temperatures until about 1975, with a clear upward trend thereafter, with a pivot point around the year 1975. It resembles a hinge, which is why we used the term “hinge fit” in the 2007 paper. I found that this “hinge” shape accurately described the graphical representation of the post-1940 behavior of the global mean annual temperature also, as I will illustrate below. I also noted from other researcher’s papers that the global ocean temperature pattern associated with global climate change was the same as the pattern I found associated with the U. S. wintertime changes. Thus, my completely independent analysis ties the climate change patterns in the oceans and in the global average temperatures over the last 60 years to changes observed in U. S. temperatures. I did my work with an entirely different methodology

[4] El Nino/La Nina and the North Atlantic Oscillation are major year-to-year swings in central equatorial Pacific ocean temperatures and North Atlantic wind and pressure systems respectively that have a substantial impact on U.S. winters.

from other existing global change studies, lending additional confidence to the conclusions.

Q. WHAT WAS THE NEXT STEP IN YOUR WORK?

A. My next step was to see whether I could repeat my results (discovering that the "hinge" shape describes the winter warming pattern and its post-1940 changes) by making changes in the input data to my analysis; *i.e.* to see whether the results were robust. The essence of the United States pattern and its evolution in time were unchanged when I included data prior to 1940, and for a broader range of locations, including Canada, Alaska, as well as the lower 48 states.

Q. WHAT CONCLUSION DID YOU DRAW FROM THESE MID-1990S ANALYSES?

A. My conclusion in 1998 was that climate change over the United States is substantially tracking global climate change.

Q. DO OTHER SCIENTISTS OR ORGANIZATIONS AGREE WITH YOUR CONCLUSION?

A. Yes. A large number of independent studies undertaken since 1998 have reached the same conclusion. These are summarized in the IPCC report (Solomon *et al.*, 2008) referenced earlier, often referred to as Working Group 1's Fourth Assessment Report ("WG1/AR4"). Figures SPM.4 and 3, shown below, are taken from the IPCC WG1/AR4's Summary for Policy Makers. In Figure SPM.4, the hinge-shaped increase in temperatures can be seen globally, for annual mean land and sea temperatures, and for all the sub-regions depicted: The graphs for each continent show little change in annual mean temperature from around 1940 until sometime in the 1970s, then increases thereafter. The seemingly large decline from 1940 to 1970 over North America is an artifact of the use of 10-year averages in the graph and a few years of extraordinarily cold conditions in the 1970s and should not be interpreted as a cooling climate.

Figure SPM.3 corroborates the fact that the globe has warmed over the last several decades by depicting consistent changes in sea level and global snow pack melting. Another feature of the temperature trends shown in the graphs is an increase in temperatures from about 1910 to 1940. All three tendencies – increasing temperatures until about 1940, then level temperatures into the 1970s, followed by a return to increasing temperatures right through to the present – are apparent in the United States graphs I will show next.

Global and Continental Temperature Change

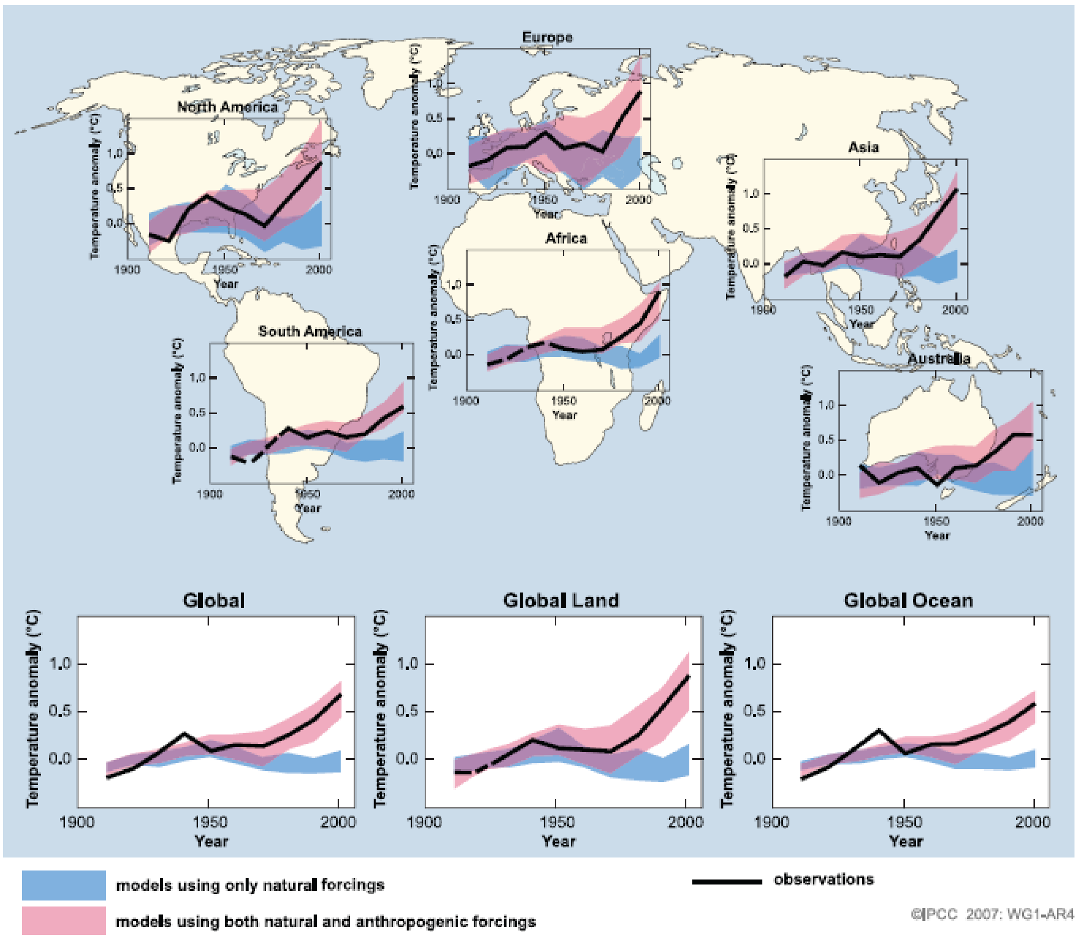


Figure SPM.4. *Comparison of observed continental- and global-scale changes in surface temperature with results simulated by climate models using natural and anthropogenic forcings. Decadal averages of observations are shown for the period 1906 to 2005 (black line) plotted against the centre of the decade and relative to the corresponding average for 1901–1950. Lines are dashed where spatial coverage is less than 50%. Blue shaded bands show the 5–95% range for 19 simulations from five climate models using only the natural forcings due to solar activity and volcanoes. Red shaded bands show the 5–95% range for 58 simulations from 14 climate models using both natural and anthropogenic forcings. {FAQ 9.2, Figure 1}*

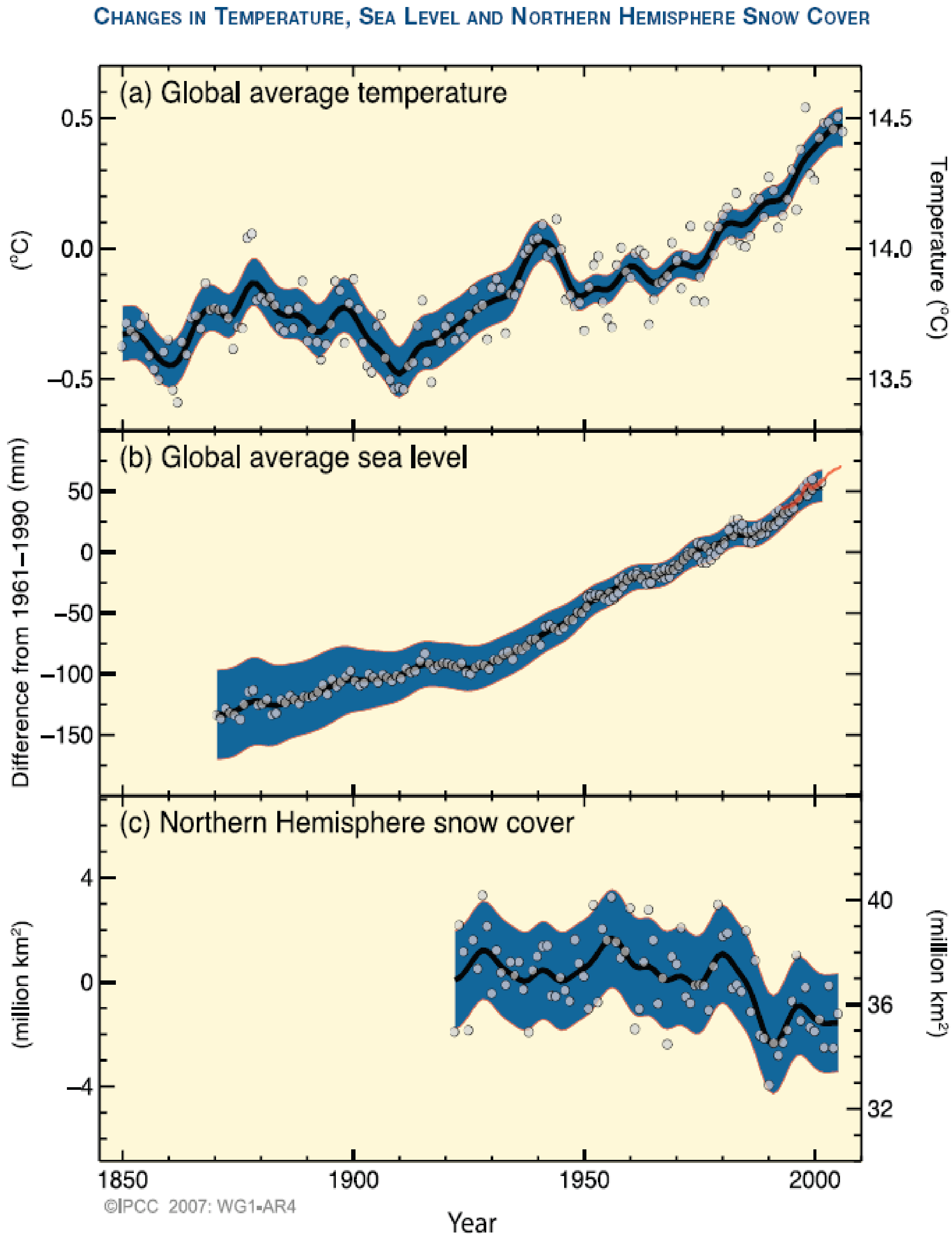


Figure SPM.3. *Observed changes in (a) global average surface temperature, (b) global average sea level from tide gauge (blue) and satellite (red) data and (c) Northern Hemisphere snow cover for March-April. All changes are relative to corresponding averages for the period 1961–1990. Smoothed curves represent decadal average values while circles show yearly values. The shaded areas are the uncertainty intervals estimated from a comprehensive analysis of known uncertainties (a and b) and from the time series (c). {FAQ 3.1, Figure 1, Figure 4.2, Figure 5.13}*

Q. DO YOU HAVE AN OPINION ON WHETHER THE TEMPERATURES IN THE UNITED STATES REFLECT THE HINGE FIT AND WARMING TEMPERATURES SINCE THE MID-1970s?

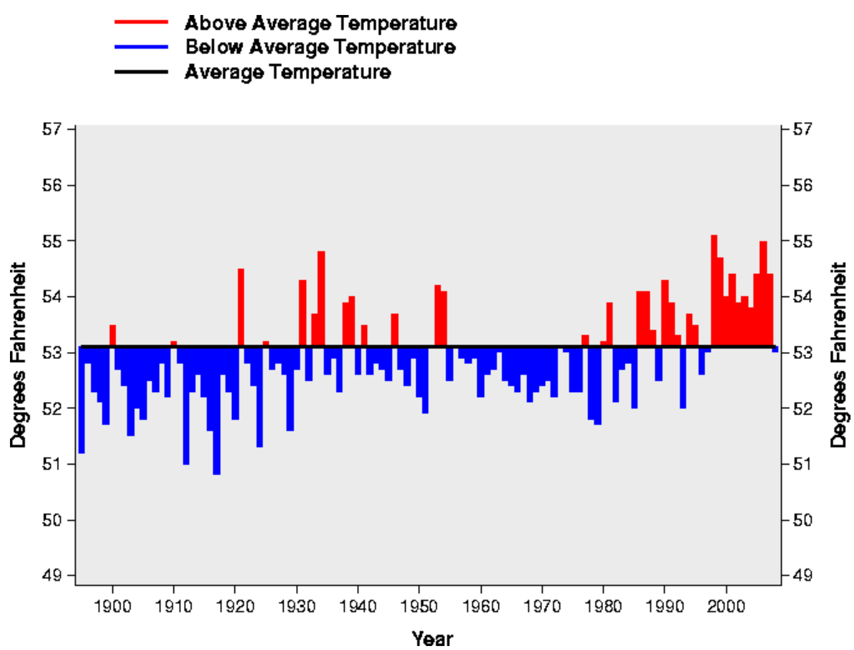
A. Yes. Generally, the temperatures in the United States (including Missouri) reflect the "hinge fit" pattern. Because Figs. SPM.4 and 3 are for larger areas than our focus here, and because those charts show annual mean temperatures, I have plotted the following three figures to show the average annual temperatures for the United States and the average winter period temperatures for the United States and Missouri. The plots show average temperatures, rather than HDDs derived from them, to follow usual NOAA practice to not emphasize just one application area.[5]

With the exception of the anomalous, unprecedented (in over 110 years), brief cold period experienced in the latter part of the 1970s, these temperature histories clearly reflect the same "warm-no change-warm" (double-hinge shape) trends previously shown for each continent in the global analysis. Temperature histories become "noisier" (show more variability) as the analysis focuses on smaller geographic areas, so to aid visualization for the Missouri winter history, I overlaid schematically the double-hinge shape reflected in the global analyses in Fig. SPM. 4. Two things should be noted about the Missouri winter temperature history. First, the trend to warmer temperatures in recent decades is not as obvious as in the other maps shown. In addition to being a smaller area, Missouri is in the zone of transition for the United States between modest temperature trends to the southeast and very large trends to the northwest (see the map on p. 14 of this

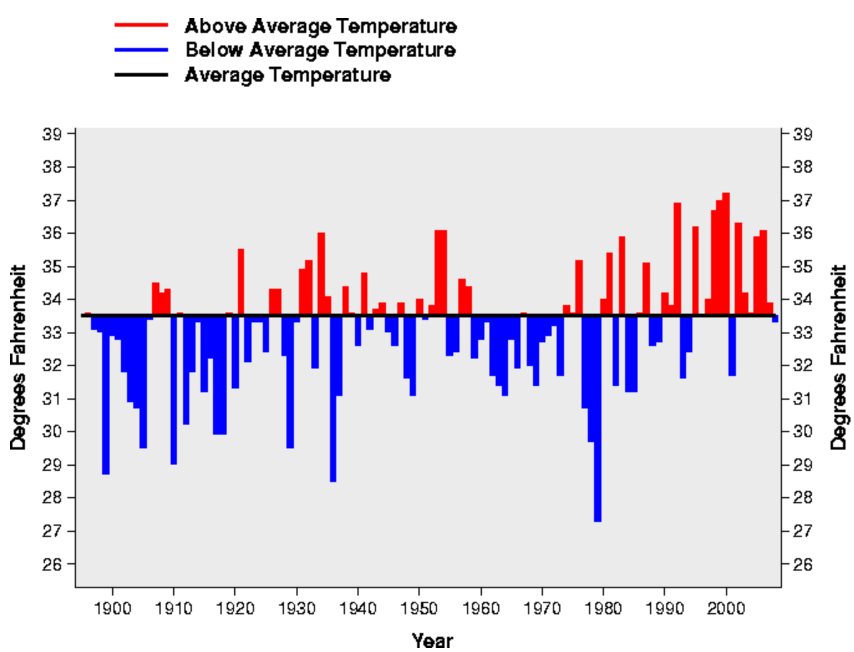
[5] The reference lines on the three graphs are the average temperatures respectively for 1971-2000.

testimony). Second, Missouri temperature records in other seasons (see Schedule REL-1, Fig. A1) indicate no trends whatsoever, underlying the significance of the winter trends.

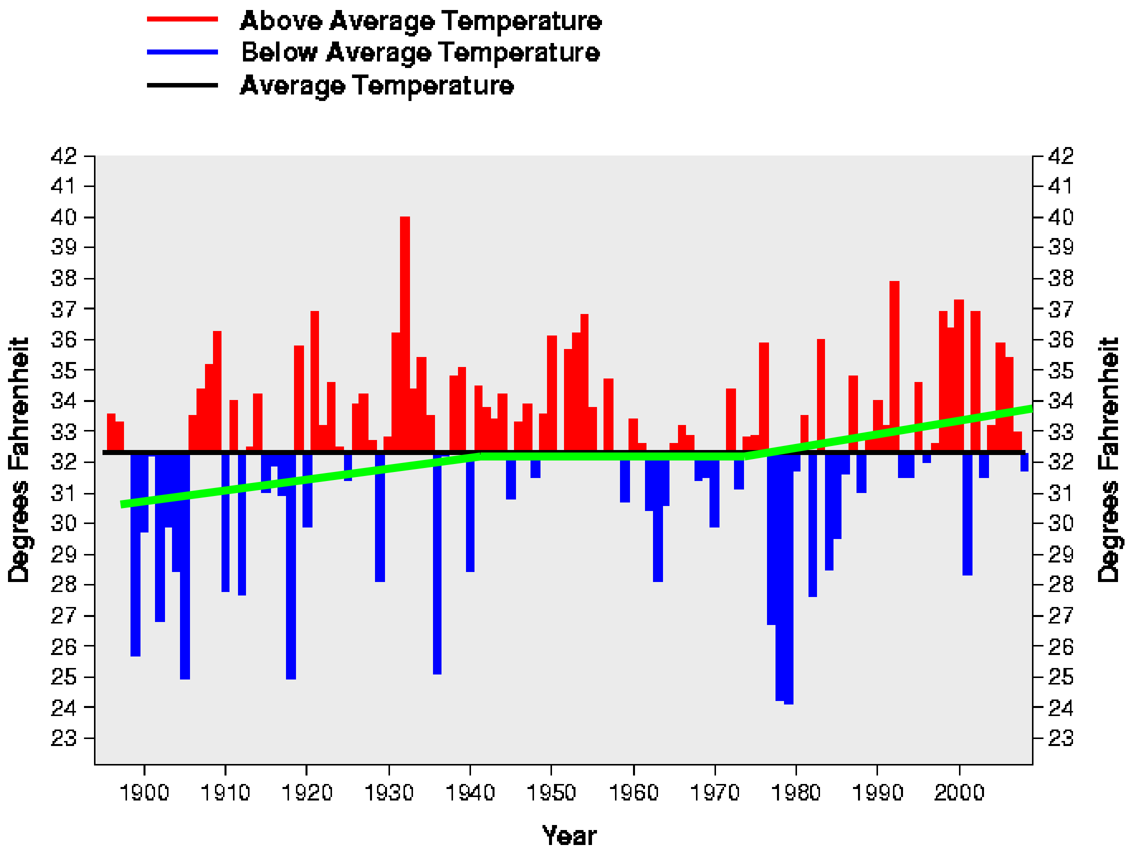
U.S. Annual Mean Temperature History



U. S. Winter Temperature History



Missouri Winter Temperature History



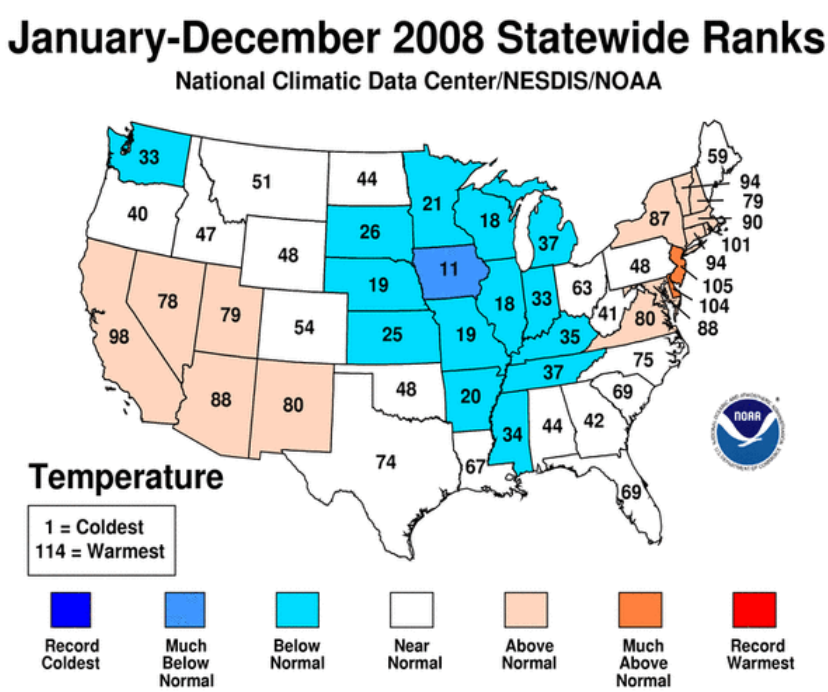
Q. 2008 WAS THE COLDEST YEAR IN A DECADE FOR THE UNITED STATES.. DOES THIS SIGNAL A SHIFT TO A PERIOD OF COLDER TEMPERATURES?

A. No. NOAA scientists have reached the preliminary conclusion that the cold U. S. temperatures for 2008 were a result of climate noise:

> What then caused the 2008 U.S. coolness? Although colder than many recent years experienced for the U.S., it was well within the range of variability associated with[in] natural internal climate fluctuations. The year of coolness does not cast doubt on the reality of global warming, but it does serve to remind that on regional and annual scales, the GHG [greenhouse gases] signal of temperature

change is still modest in amplitude compared to the intensity of natural variability.[6]

Winter, the season with the greatest warming over the last few decades for the United States and the only season with consequential warming for Missouri, was less unusual for 2008, *i.e.* the coldest in the last 7 years for the United States and last 5 years for Missouri. Globally, the relative cooling in 2008 hardly registered at all; the year was the seventh warmest year on record according to NOAA. The United States was the only land mass worldwide that exhibited a substantial area that was relatively cool, reinforcing the conclusion that it was the result of a random climate fluctuation. The map below shows state-by-state ranks of annual average temperatures (coldest in 114 years is denoted "1"; e.g. Missouri had its 19th coldest year); from a global perspective, the cold area is quite small, but Missouri was close to its epicenter in Iowa.



[6] Preliminary NOAA CSI Report, Dr. Martin Hoerling, ESRL/OAR/NOAA, lead.

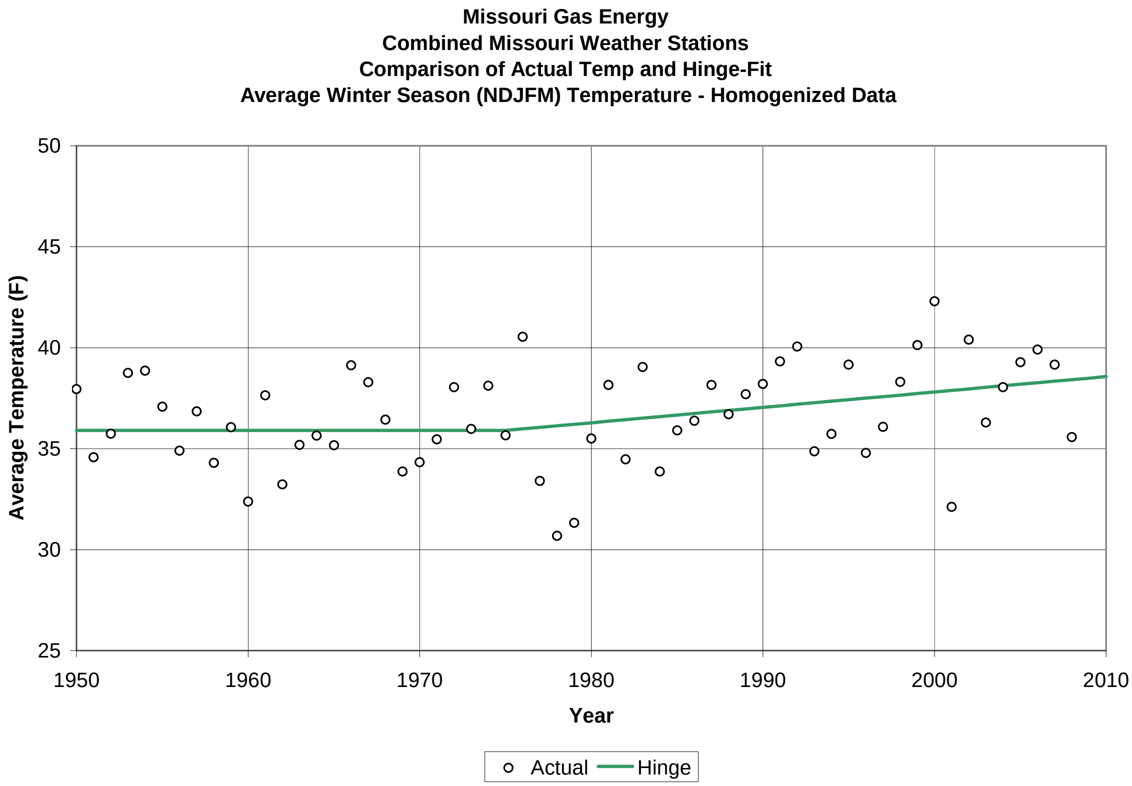
IMPLICATIONS FOR MISSOURI NORMALS

Q. WHAT CONCLUSIONS CAN BE DRAWN FROM THE UNITED STATES AND MISSOURI DATA?

A. The U.S. and Missouri winter data clearly fit the hinge shape that our research validated as a tool for tracking global climate change. Therefore, the hinge fit methodology should be much more accurate than 30-year normals in these cases. A major benefit of using pre-1975 data is that it enormously increases the confidence, in both ordinary and statistical meanings, in post-1975 temperature trend estimates. This can be seen in the Missouri winter history, illustrated above, where the two coldest years in the record occurred in the late 1970s, which should be considered a statistical aberration. A trend estimate based on data from the late 1970s to the present would dramatically overestimate the rate of winter warming in Missouri because of those two winters. Fitting a hinge averages the impact of these anomalous winters by anchoring the beginning of the trend to the average conditions over the 1940 to mid-1970s period.

The statistical technique for calculating the 2008 (or 2009) expected temperatures in Missouri would be to find the least-squares fit to the hinge shape for post-1940 data, where the fit will be especially good. An example of the calculation with post-1948, November through March ("NDJFM") data for eight stations[7] in western Missouri (representing MGE's service area) by MGE witness Larry Loos is shown below. The hinge shape represents the data well:

[7] The station temperature records used in the figure were obtained from NWS (who used them to make official local seasonal forecasts) but were produced by NCDC. The records are the same used by Mr. Loos to produce the "Homogenized HDDs" cited in his testimony and later in this testimony.



Q, WHY DO YOU USE "HOMOGENIZED" DATA IN THIS GRAPH AND LATER IN YOUR TESTIMONY INSTEAD OF THE ORIGINAL DATA AT THE EIGHT WESTERN MISSOURI LOCATIONS REPRESENTING MGE'S SERVICE AREA?

A. All historical temperature records have problems associated with them, including a variety of errors, missing data, and inconsistencies in their sites, instruments and observing practices. The records available for MGE's western Missouri service area turn out to be especially problematic, particularly with respect to inconsistencies over time.

Most of these sites have such pronounced inconsistencies that they seriously compromise the utility of the records for tracking the stations' climates, our objective here:

> Ideally, for the purposes of climate research, the period of record for U.S. in situ observations would be free of changes and inconsistencies in observational practices (e.g., station relocations, instrumentation changes, differing daily observation schedules). When present, these inconsistencies can lead to a nonclimatic bias in one period of a station's climate record relative to another, or in observations from one station relative to another. In such cases the data record is considered to be heterogeneous or "inhomogeneous".[8]

NCDC experts produced the homogenized data records by correcting for previously-documented errors and newly-identified gross inconsistencies from quality-control checks, by filling in missing data to ensure spatial (*i.e.* to other highly-correlated locations) consistency, but most importantly by correcting for the temporal inconsistencies which make the records inhomogeneous. The most serious inhomogeneities tend to be station relocations and daily observing schedule changes (mentioned above in the NCDC documentation), but modification of the environment of the observation site, either abruptly or over a long period of time (like paving an adjacent area or encroaching development respectively) can either mask or falsely indicate a pervasive climate change.

Artificial biases in the records from identified inhomogeneities are corrected by NCDC to the recent record, because it is the relevant part of the record for forecasting (the use to

[8] From the internal report by NCDC scientists documenting the production of the "homogenized" records.

which NWS puts the homogenized data) and planning (including for rate-making purposes). Consequently, inhomogeneity adjustments tend to be minor or non-existent for the last few decades, so they have practically no impact on normals based on shorter-term averages (discussed in detail next). In contrast, these bias adjustments are critical to precise estimation of how current climate is trending, particularly the slope of the hinge fit. Lastly, most (if not all) of the corrections used in the homogenized records after 1980 ultimately will be used by NCDC to produce the next generation (1981-2010) 30-year normals. Given these considerations, use of the original records to track the climate would be misleading and not productive.

Q. IN ADDITION TO THE HINGE FIT, WHAT IS THE OTHER MAIN ALTERNATIVE YOU HAVE EXAMINED FOR TRACKING CLIMATE CHANGES?

A. Another approach commonly proposed for tracking changing climate involves use of averaging periods shorter than 30 years.

Q. HAS YOUR RESEARCH LED TO ANY CONCLUSIONS ABOUT THE USE OF SHORTER-TERM NORMALS?

A. Yes, because of climate change, in almost all instances shorter-term normals will be superior to 30-year normals. However, my research also has shown that direct analyses from data to determine the best averaging period are very unstable; i.e. extremely sensitive to the particular data sample. The shorter the averaging period is, the greater the instability. This feature was, in fact, a principal motivation for originally adopting normals based on a 30-year period. One of the objectives of my statistical analysis and research was to assess how to determine the best averaging period as well as its expected error in estimating the current climate. I used similar methods to assess the performance of the hinge model and fit.

Q. WHAT OBJECTIVE OR INFORMATION DO YOU SEEK WHEN YOU DETERMINE THE PERFORMANCE OF A TEMPERATURE NORMAL?

A. To reiterate, my main goal here is to determine the best estimate for what the current year's climate is, so different methods are assessed based on how well they do this. The CPC's focus, however, is on next year, but the assessment methods I employ are just as applicable for this target. Further, conclusions about a method's relative performance in describing the current climate can be applied for describing next year's also.

In the context of my stated objective, we know that a 30-year normal will provide a relatively stable estimate, but under conditions of a warming climate, with certainty, will produce a best estimate that will be cold-biased. For parts of MGE's service area I estimate that this cold bias for NCDC 1971-2000 normals could be as much as 3 degrees Fahrenheit for the coldest months of the winter. In other words, the 1971-2000 winter normal for Springfield Regional Airport (for example) is probably more appropriate for the current climate at Kansas City International with Springfield being correspondingly warmer. Further, substantial evidence supports the conclusion that the North American normal temperature increase reflects global increases and both the global and North American increases have been relatively steady over the last several decades. This implies that the most recent 30-year average temperature for North American locations is likely more representative of the climate about 15 years ago than the climate today. With a steadily warming climate, a shorter period average, say 20 years, intuitively would seem to be a better choice for calculating a normal than a 30-year period. This is because such a normal will be most representative of the climate just 10 years ago, rather than 15 years ago as is the case with the 30-year normal. But neither the 30-year normal nor the 20-year normal is appropriate where the data shows a substantive warming trend, as is the case for much of the United States (and Missouri) in winter, because both will be unacceptably cold-biased.

Q. THEN WHY NOT USE A VERY SHORT-PERIOD NORMAL, LIKE A 5-YEAR AVERAGE, TO MINIMIZE THE COLD BIAS?

A. A five-year normal will have a much smaller cold bias than a 20- or 30-year normal if the climate is warming, so the most recent five-year normal might be a more accurate predictor of next year's conditions than a 30-year normal for Missouri. However, shorter-period averages are also much more sensitive than the longer-period averages to unusually cold and warm winters that occur from time to time because of climate noise (independent of the overall warming trend). These outlier winters tend to average out in the longer-period normals, but lead to somewhat large year-to-year changes in the shorter-period averages as these normals are updated each year (by adding the data from the just-completed year and removing the data from the earliest year in the period). This lack of consistency in year-to-year values makes large errors in estimates of next year's conditions common, offsetting any advantage from the smaller cold bias.

A good illustration of this last point is the recent NDJFM temperature record for Missouri shown on p. 27 of this testimony. Because there has been a several-decade warming trend (the green line), recent 5-year averages collectively are warmer than earlier in the record and overall track this change. However, individually the 5-year normals exhibit considerable instability as estimates of the changing climate. For example, there is a large decrease in value (37.8 to 37.2 deg F; an increase of 90 HDDs for NDJFM) when the 5-year average is updated from 2000-2004 to 2001-2005, followed by a much larger increase (37.2 to 38.8 deg F; a decrease of 240 HDDs) in the update from 2001-2005 to 2002-2006. This inconsistency makes 5-year normals unacceptable for use with Missouri winter temperatures.

Q. WHAT THEN DETERMINES THE BEST AVERAGING PERIOD FOR A NORMAL?

A. The best averaging period for use as a normal in a warming climate will be somewhere between 30 and 5 years and represent a balance between the cold biased, but stable longer-period estimates and the relatively unbiased, but outlier-sensitive shorter-period estimates. The averaging period representing the best tradeoff will depend on the strength of the warming trend and characteristics of the climate noise; the length of the period has to be long enough so that a single year with extreme temperatures has minor impact, but short enough to reflect the recent trend.

This best compromise is the OCN, one of the two methods recommended in the 2007 paper. Calculations with the eight weather station records used in the previous figure (representative of MGE's service area)[9] and the results of my research suggest that the OCN is around 15 years for the eight service area stations collectively (as in all normals

[9] The stations are Carrollton, Joplin, Kansas City International Airport, Lee's Summit, Sedalia, Springfield, St. Joseph, and Warrensburg.

calculations, the results for individual weather stations vary somewhat, but average 15 years as well). For the eight locations as a group, the expected standard error using 30-year normals will be about double that using the shorter-period averages. In other words, for these stations, an OCN of around 15 years is expected to have about half the error of a 30-year normal. In using the full 30 years, the error introduced because temperatures have increased over the whole period more than negates the reduction of the error from adding the additional years.

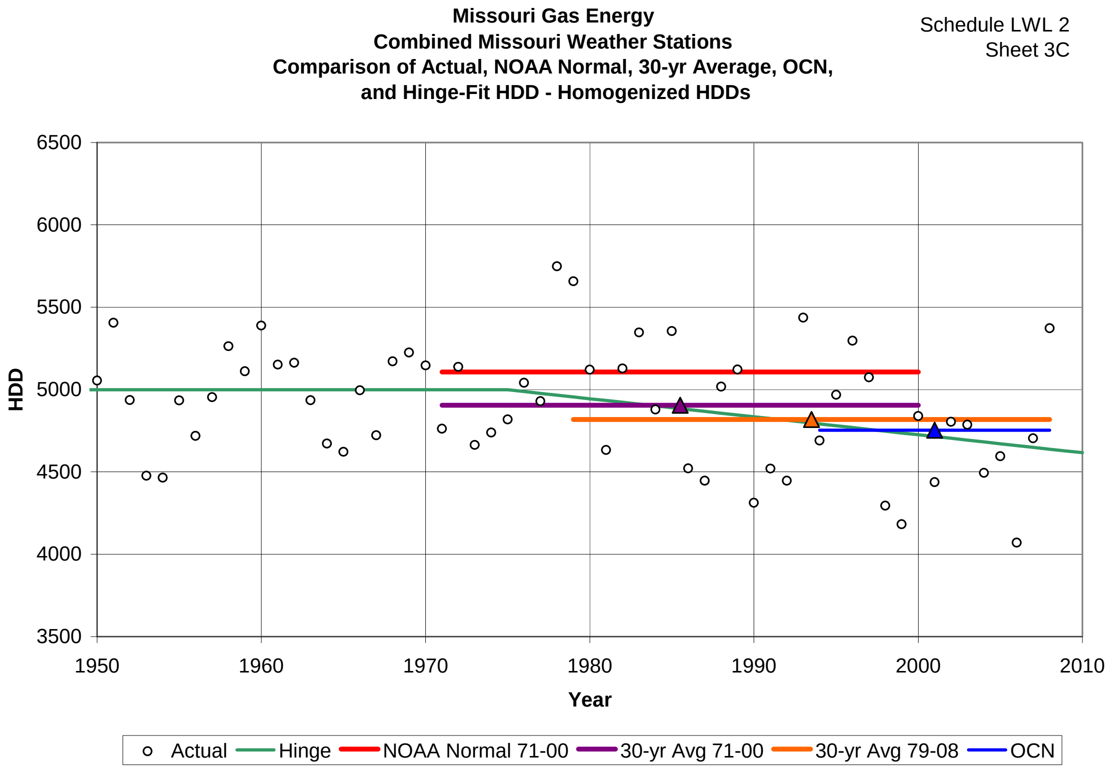
Q. IS THERE A BETTER CHOICE THAN OCN FOR CALCULATING MISSOURI NORMALS?

A. For MGE's gas service territory, my research suggests an even more accurate choice than OCN exists; namely, finding the least-squares fit of the "hinge" model to the data (like in the example shown above) and using the most current point on the upward trend (in average temperature, downward trend in HDDs) part of the hinge as the best estimate for the current climate. This would involve determining the slope of the 1975-2008 trend line portion of the hinge, and then using that slope to determine the temperature during the test year. If desired, the slope could be extended to the first year under new rates, or even the year after that. The hinge technique uses much *more than 30 years of data*, including pre-1975 data that serves to reduce the error in estimating the temperature trends over the last several decades. In effect, it eliminates the weakness of the OCN, which always involves a bias towards a past climate, in favor of a bias towards current trends. Trends for most of the eight locations I examined to represent MGE's service area, as well as their collective trend, are large enough to ensure that the hinge estimate will have a smaller expected error than that of the OCN

Q. CAN YOU SUMMARIZE THE RELATIVE ERRORS FOR DIFFERENT METHODS FOR CLIMATE NORMALS WHEN THE CLIMATE IS CHANGING, LIKE IT IS IN MISSOURI?

A. Yes, I can do this with a graph of yearly total Heating Degree Days ("HDDs") from 1950 to the present averaged over the eight study locations cited above. Because winter temperatures in Missouri have been increasing, HDDs have been decreasing, so the fitted hinge trend for HDDs in the graph should point downward instead of upward as it does in the temperatures graph. Horizontal lines are also drawn on the graph to represent the calculated 15-year OCN (blue line), the most recent 30-year average (orange line), the calculated 1971-2000 average (purple line) and the average of the published NOAA 1971-2000 averages (red line).[10]

[10] The most recent reported NOAA normals are for the period 1971-2000, and were reported by the agency in 2003. Therefore, the net warming experienced in Missouri from 2001-2008 will not be reflected in NOAA normals until the year 2013, assuming no change in NOAA's reporting process.



Notice first how the four time-average estimates successively misrepresent the last ten years more and more as the time period varies, where the 15-year OCN is the least misleading, and the NOAA-convention normals are the most misleading of the four. Clearly, the most representative and best estimate is the endpoint of the hinge trend, which splits the ten most recent HDDs in half.

Next, note on the graph that triangles are placed at the middle-years of all but the NOAA time-average methods. Recall in earlier discussion that for a steadily changing climate, these midyears should be where the respective methods are most representative. For example, if you average 30 years during a period of steadily increasing temperatures, then the average should be warmer than most of the years in the first half of the period and colder than most of them in the second half. All three of the triangles (for time-averages calculated directly from the data) lie on or very close to the hinge trend line, providing considerable confidence that the hinge is accurately representing changing normals in MGE's gas service territory.

OVERVIEW AND RECOMMENDATIONS

Q. PLEASE REVIEW THE ALTERNATIVES TO 30-YEAR WEATHER NORMS YOU HAVE CONSIDERED.

A. Let me now step back and review the alternatives for Missouri (specifically MGE's service area) and their pros and cons:

(1) Trends, likely tied to global scale changes, have been and will likely continue to be a source of considerable error when 30-year normals are used to estimate current and immediate future temperature. If these normals are only updated every 10 years, following conventional NOAA practice, the error quickly becomes overwhelming in the intervening period between updates. Thirty-year normals produce estimates under current circumstances that are always biased to at least 15 years ago.

(2) Use of OCN estimates (around 15-year averages) will reduce estimation error from the most recent 30-year normal by a factor of about two, because it reduces the bias of estimates to as little as 7 to 8 years ago. The OCN's error reduction from the use of published NOAA normals will be much greater. OCN is a simple intuitive step from use of the past 30-years.

(3) An even better choice for much of Missouri is use of the hinge fit, because it uses a long record (up to almost 60 years here versus 30 or fewer years) to reduce the error in trend estimates and it also removes the bias to past climates inherent in the OCN and 30-year normal methods.

(4) Both the OCN and hinge fit methods are relatively simple to implement and routine to compute. Both will produce estimates with similar expected error in all instances, but the hinge fit will outperform OCN for most of the locations in the service area. Both techniques may be available from NOAA within the next year or so and likely will be routinely updated.

Q. DO YOU HAVE A RECOMMENDATION FOR THE COMMISSION?

A. Yes. Since the hinge fit method is more accurate and reliable than 30-year normals and at least as accurate as OCN everywhere in MGE's service area, it should be adopted by the Commission in this Docket instead of the 30-year normals.

Q. HAVE YOU APPLIED YOUR RECOMMENDATIONS TO THE HISTORICAL WEATHER DATA FOR MISSOURI?

A. Company witness Mr. Larry W. Loos has applied my recommendations to calculate the expected weather in 2009 for MGE's service territory in Missouri. The eight-station example of different methods shown above is extracted from his exhibits.

Q. DOES THIS COMPLETE YOUR DIRECT TESTIMONY?

A. Yes.