Exhibit No.:	
Issue:	Weather Normalization
Witness:	Jay R. Turner, D.Sc.
Type of Exhibit:	Surrebuttal Testimony
Sponsoring Party:	Laclede Gas Company
Case No.:	GR-99-315

LACLEDE GAS COMPANY

GR-99-315

SURREBUTTAL TESTIMONY

OF

JAY R. TURNER, D.Sc.

SURREBUTTAL TESTIMONY OF JAY ROBERT TURNER, D.Sc.

Q. Please state your name and business address.

A. My name is Jay Robert Turner, and my business address is 210 Cupples Hall II, Washington University, One Brookings Drive, St. Louis, Missouri, 63130-4899.

Q. Are you the same Jay Robert Turner who previously filed rebuttal testimony in this case?

A. Yes.

Q. What is the purpose of your testimony?

A. I will respond to certain statements contained in Staff Witness Mr. Dennis Patterson's rebuttal testimony.

Q. Did you review the August 1999 rebuttal testimony of Mr. Dennis Patterson and if so, do you have any comments regarding that testimony?

A. Yes, I did review Mr. Patterson's rebuttal testimony and have several concerns with his comments and analyses.

Q. Please elaborate on these concerns.

A. First, I will address Mr. Patterson's response to the direct testimony of Ms. Krieger that the most recent 15 years (1984-85 through 1998-99) have been warmer than the NOAA thirty year normal. Mr. Patterson acknowledges that Ms. Krieger's claim is numerically correct. However, he subsequently states:

> "When a statistical test is performed at the 95% confidence level, heating season HDD from the 15-year period chosen by Ms. Krieger are not statistically different from the group of years that includes 1961 through 1984. Similar results are found when a comparison was made between the most recent 15-year sample and a sample of annual HDD from the years including 1961 through 1990." (p. 3, lines 10-14)

Mr. Patterson separately provided the Company with the calculations used to support his statement. Unfortunately, there are several problems with his analysis that render the above claims invalid and subject to a re-analysis. For example, Mr. Patterson used a Student's t-test to reach his conclusion. However, this approach carries with it certain assumptions – such as a Gaussian distribution for the data – which cannot be assumed valid in the absence of supporting analysis. In this case, it is more appropriate to use the Wilcoxon-Mann-Whitney distribution test for the equality of the medians of the two data sets.

Q. What result does a correct analysis yield?

A. I re-analyzed the heating degree data for the period 1985-1999 compared to the period 1961-1984 using the Wilcoxon-Mann-Whitney distribution test and found that heating season HDD for the 15-year period 1985-1999 are statistically different (and namely, lower) at the 95% confidence level than heating season HDD for the period 1961-1984. Thus, Mr. Patterson's statement at page 3, lines 14-15 of his rebuttal testimony that: "No statistical support can be found for Ms. Krieger's claim" is wrong. My analysis demonstrates that the most-recent 15-year period is statistically different - and indeed warmer - than the preceding period of 1961-1984 in the context of heating degree days, regardless of whether I use NOAA official HDDs or those derived from Dr. Hu's adjusted data.

Q. In his rebuttal testimony (p. 6, lines 14-23), Mr. Patterson additionally criticized Ms. Krieger's failure to incorporate Dr. Hu's adjustments. Do you believe there are valid reasons that would justify Ms. Krieger's decision not to incorporate Dr. Hu's adjustments?

A. Yes, for numerous reasons I have concerns regarding the reliability of Dr. Hu's findings, including his application of the double mass analysis and the completeness of his study. These reasons not only justify but positively mandate Ms. Krieger's decision not to incorporate Dr. Hu's adjustments. They include:

(1) At page 6 of his rebuttal testimony (lines 18-20), Mr. Patterson states that three adjustments to the St. Louis Lambert data would be required to make the data consistent for the period 1961-present. He is apparently referring to the direct testimony of Dr. Hu that indicates that the events triggering the need for adjustments (e.g., temperature sensor and/or location change) occurred in 1979, 1988 and 1996. Dr. Hu selectively investigated four time periods - 1979, 1985, 1988, and 1996 - which correspond to periods defined by Dr. Hu as having sensor type and/or location changes. It is well-accepted that metadata (e.g., station history records) can be incomplete. For example, Dr. Hu's 1988 Lambert location change is not documented in the official station history. Therefore, the identification of inhomogeneities should be approached by analyzing both the metadata records and the temperature data trends for the entire period in order to detect all potential points of bias. A double mass analysis over the time period 1961-1990 for Lambert versus the Elsberry and Union reference stations reveals discontinuities other than the four periods studied by Dr. Hu. These would need to be investigated to see where the discontinuity occurred – Lambert or the reference station – before the time series at Lambert could be considered to be homogeneous. I must emphasize that the approach taken by Staff to adjusting the Lambert temperature data requires that all discontinuities from 1961 to present be identified and correctly quantified; the entire analysis unravels if even only

one significant discontinuity over this entire time period is missed or if any adjustment is miscalculated. This arises from the interrelationships between the adjustments. In creating a homogeneous database, the data for current conditions is considered correct, and historical data is adjusted by working backwards through the time series and applying adjustments to make prior periods consistent with the current condition. Under such circumstances, overlooking or incorrectly calculating even one discontinuity will invalidate all the data prior to that time, just like a missing or incorrect entry in a checkbook register will make all future calculations of a checking account balance incorrect.

(2) Dr. Hu arrived at his adjustments using a double mass analysis on daily temperature data in some cases (1996) and monthly temperature data in other cases (1979, 1985, and 1988). Given that the adjustments are used by Mr. Patterson to correct daily data, the double mass analysis should be performed using only daily data to avoid the effect of data smoothing introduced by using monthly values. Thus, the daily adjustments calculated for 1996 are not consistent with the adjustments calculated for other periods in the data set, resulting in a data set with inhomogeneous methodology.

(3) Another issue concerns the selection of reference stations. According to the Staff response to Company Data Information Request #20, the reference stations used by Dr. Hu were selected by comparing candidate reference stations to the average of "consistent" (adjusted) temperature time series from seven stations of the United States Historical Climatology Network (USHCN). However, upon applying the adjustments proposed by Dr. Hu to the monthly Lambert data and performing a separate double mass analysis between Lambert and each of the seven USHCN

stations used by Dr. Hu, I still found noticeable slope changes. This demonstrates that either the proposed adjustments to the Lambert data are incorrect, and/or the USHCN data is not clean (that is, the inhomogeneities in the data have not been completely removed). If the data is not clean, it is wrong to use an average across the USHCN stations to determine the suitability of candidate reference stations for making adjustments as this average will be contaminated by the stations that have not been adequately cleansed of inhomogeneities. Of particular note is that at least three of the seven comparisons show a distinct increase in slope since 1996. Dr. Hu's double mass analyses should produce comparable results when testing the target station (Lambert) against any other reliable reference station. However, Dr. Hu's results are not comparable in this regard and therefore, his results are not statistically reliable for the purpose of adjusting the Lambert data.

(4) Another significant problem is that Dr. Hu's methodology produced annual average slope changes. That is, the amount to be adjusted is the same for all Lambert temperature data regardless of month. This methodology was used despite obvious seasonal differences inherent in the data used by Dr. Hu to arrive at his proposed adjustments. Schedule 1 hereto shows the accumulated temperature difference in daily maximum and minimum temperatures for the 1996 change in sensor type and location at Lambert. These graphs were prepared using the temperature data from Dr. Hu's work papers. The data show distinct annual oscillations that reflect a seasonal variation in the temperature bias between the target and reference stations. Seasonal variations exist and any adjustments proposed to the data should adequately reflect the seasonal variability. Thus, Dr Hu's single value for the adjustment is wrong and

seasonal effects must be taken into consideration. For example, you cannot apply an adjustment to December and expect it to also be valid for June. Company Witness Mr. Timothy Waldron will further elaborate on this issue in his surrebuttal testimony.

Q. Are you aware of other situations where seasonal adjustments have been proposed or investigated for similar situations?

A. Yes, I am. First, adjustments made by NOAA in the 1990 Lambert sequential temperature data included varying adjustments by month. Second, seasonal variations in the adjustment for discontinuities have been addressed by D.T. Keiser and J.F. Griffiths (International Journal of Climatology, *17*, 497-510 (1994)) who studied the impact of a location change for the first-order NWS weather station in Valentine, NE. In this case, Keiser and Griffiths observed a strong seasonal variation to the temperature difference and calculated varying adjustments by month. On an annual basis, the adjustment was 1.47°F; however, the month-specific adjustments ranged from 0.08°F in February to 2.74°F in April. This peer-reviewed research clearly demonstrates the potential impacts from seasonal variations. An analysis that does not investigate the significance of seasonal effects on proposed adjustments is unacceptable for the purpose of adjusting temperatures used to calculate heating degree days that predominantly occur in the winter. Dr. Hu's study is incomplete and his adjustments are inaccurate. In light of these considerations, even if Dr. Hu's 1.875°F cooling adjustment was correct on an annual basis, the appropriate adjustment for the winter period could be zero (no adjustment) or even a warming adjustment. Since the vast majority of the Company's sales occur during the winter season, the failure to consider the seasonal differences in the adjustment is fatal. In short, the 1996 adjustment proposed by Dr. Hu and referred to by Mr. Patterson

is wrong, because it does not account for seasonal variations. At this time, there is not enough data subsequent to the 1996 ASOS change to adjust data on a seasonal basis to obtain a statistically reliable result.

Q. If Staff were to make the changes to its analysis that you mentioned, would you find the analysis procedure acceptable?

A. No. The peer-reviewed scientific literature describes the appropriate handling of discontinuities in climatological time series. Nonetheless, it appears that Dr. Hu and the Staff are not familiar with – or have chosen to ignore - the current state-of-knowledge regarding application of the double mass analysis and alternative methodologies for making adjustments. For example, D.A. Rhoades and M.J. Salinger (International Journal of Climatology, *13*, 899-913 (1993)) present a detailed description of applying the double mass analysis to make adjustments to temperature time series. Their approach includes the construction of plots for the accumulated temperature difference versus time that differs from the approach of Dr. Hu only in that Rhoades and Salinger (1993) have first removed seasonal effects from the data. This peer-reviewed paper proceeds to describe a methodology for calculating the temperature bias caused by a discontinuity, formulas for the standard error and confidence interval, and a strategy for weighting the results obtained using different reference stations. *An acceptable analysis must, at the minimum, include these steps to determine whether adjustments are appropriate.* The analysis conducted by Dr. Hu – as demonstrated in his work papers – lacks the technical rigor to justify its use.

Furthermore, a proper application of the double mass analysis alone is not sufficient to support adjustment(s). Numerous papers have appeared in the scientific

literature this decade that address alternative approaches for isolating and quantifying biases introduced by discontinuities. The motivation for exploring such methods are often premised on the shortcomings of the double mass analysis for certain applications. For example, D.R. Easterling and T.C. Peterson, who are members of the NOAA staff, have developed several alternative methods, because they find the double mass analysis methodology lacking (International Journal of Climatology, *15*, 269-377 (1995)). Researchers generally advocate the application of two-or-more different analysis methods for identifying inhomogeneities and quantifying the adjustments. If similar results are obtained using the different methods, then the adjustment can be made with at least some degree of confidence. Dr. Hu has not tested the results of his double mass analysis against results provided by even one other method. For all of these reasons, I strongly disagree with Mr. Patterson's statement in his rebuttal testimony that Ms. Krieger's "calculations were not appropriate for weather normalization in the present case, because they were based on official temperature data that did not contain these necessary [proposed by Dr. Hu] adjustments."(p. 6, lines 21-23) .

Q. Mr. Patterson has conducted an experiment to evaluate the use of a 10-year normal. Do you agree with the findings of his analysis which he addresses on pages 4-5 of his rebuttal testimony?

A. No, I do not. Once again, Mr. Patterson's analysis is fundamentally incorrect and does not support his claim concerning the inappropriateness of a 10-year normal. As shown by Column 3 of Schedule 1-3 to his rebuttal testimony, Mr. Patterson makes the critical error of using the same 30-year normal value – 5094.65 HDD – for each year during the period 1961-1998. This 30-year normal was calculated from the actual data for the 1961-

1990 period as adjusted in accordance with Dr. Hu's recommendations. By retrospectively applying actual, current year, and future year data for his 30-year analysis (i.e. by effectively using what actually happened to predict what retrospectively happened), Mr. Patterson virtually guarantees that the 30-year analysis will show superior results. Unless one's goal is to perpetuate a self-fulfilling prophecy, for this analysis it is simply not appropriate to compare data to a parameter calculated from the same data. Moreover, even if Mr. Patterson's analysis was not flawed, his results actually argue in favor of using a 10-year normal over a 30-year normal. Mr. Patterson's analysis suggests that the two approaches produce relatively close results over the long-term period. On a year-to-year basis, however, his analysis also shows that a 10-year normal will result in less revenue volatility. More importantly, his analysis indicates that a 10-year normal compared to a 30-year normal will result in the Company collecting less from its customers when the weather is colder than normal and their bills are already higher because of increased usage and, conversely, more from its customers when weather is warmer than normal and customer bills are already lower because of reduced usage. In view of what I have been advised regarding Staff's preference for rate stability, I would think Mr. Patterson would view his analysis as a reason for adopting a 10-year normal.

Q. Do you believe the NOAA 30-year normal is the appropriate normal for predicting heating degree days as suggested by Staff in its rebuttal testimony?

A. No, I do not. As I stated in my rebuttal testimony, the NOAA 30-year normal is appropriate for the comparison of historical data but is in no way representative of current and ongoing weather expectations. I have performed an analysis on heating season HDD

data based on Lambert unadjusted temperatures (it was necessary to use unadjusted temperature data because the adjustments proposed by Staff are incorrect). Several normals were considered, including the NOAA 30-year normal and a series of rolling averages ranging in length from 1 to 25 years. These normals were compared to actual heating season HDD for a defined test year. I repeated the analysis using each year over the period 1981-1998 as a test year to ensure that my results could be generalized beyond a particular test year. I found that the optimum normal was a 5-year rolling average. The 10-year rolling average proposed by the Company, while not as good as the 5-year rolling average, was still superior to the NOAA 30-year normal.

Q. Is there any evidence in the peer-reviewed scientific literature to support your finding that normals shorter than the NOAA 30-year normal are more appropriate for predicting heating degree days?

A. Yes. Subsequent to my analysis from which I reached the conclusion that a 5-year normal is optimum, I reviewed a publication by P.J. Lamb and S.A. Changnon, Jr. (Journal of Applied Meteorology, *20*, 1383-1390 (1981)). Lamb and Changnon initiated their research "when it became apparent that the standard meteorological practice of using 30-year temperature and precipitation normals might not be the best in the foregoing [utility ratemaking] contexts." (p. 1384) Based on the results of their research, which was conducted in Illinois, Lamb and Changnon concluded:

> "Normals for 5, 10, 15, 20, and 25 years were considered here, in addition to 30-year ones. 5-year normals were found to most frequently provide the closest estimate of the next year's summer and winter mean temperature and total precipitation. [...] 10-year normals were also found to have a high probability of being the best predictors of the parameters in question, whereas 20-year normals have a particularly low probability of such success. The standard 30-year normals were likewise found to

> perform poorly in this regard. [...] The general similarity of the results obtained along the entire 500 km north-south Illinois transect suggests that they should be reasonably transferable to other parts of the central United States." (p. 1389)

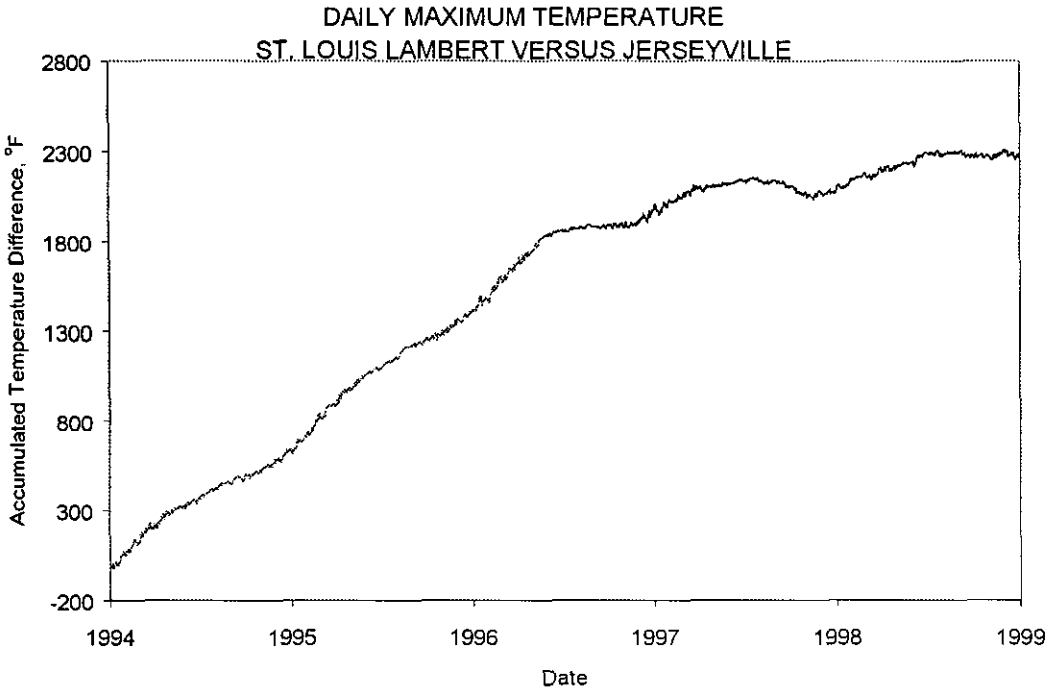
Thus, my analysis and the research of Lamb and Changnon – conducted almost two decades apart – arrived at the same conclusion: The standard practice of using a 30-year normal should be abandoned in favor of a normal with a much shorter time period.

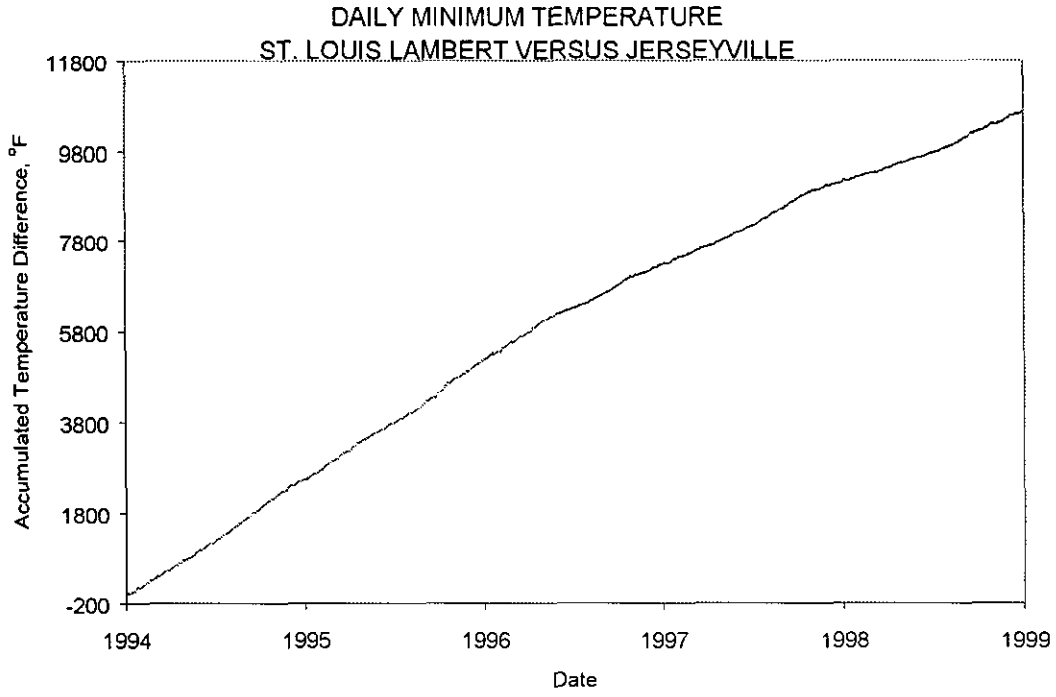
Q. So is it your position that the NOAA 30-year published normals should be replaced with one utilizing a shorter timeframe for the ratemaking process?

A. Yes, it is. A rolling 5-year normal would be optimum, and a rolling 10-year normal as proposed by the Company would still be a marked improvement. I would stress that whichever timeframe is chosen, it should roll forward to capture the most recent data.

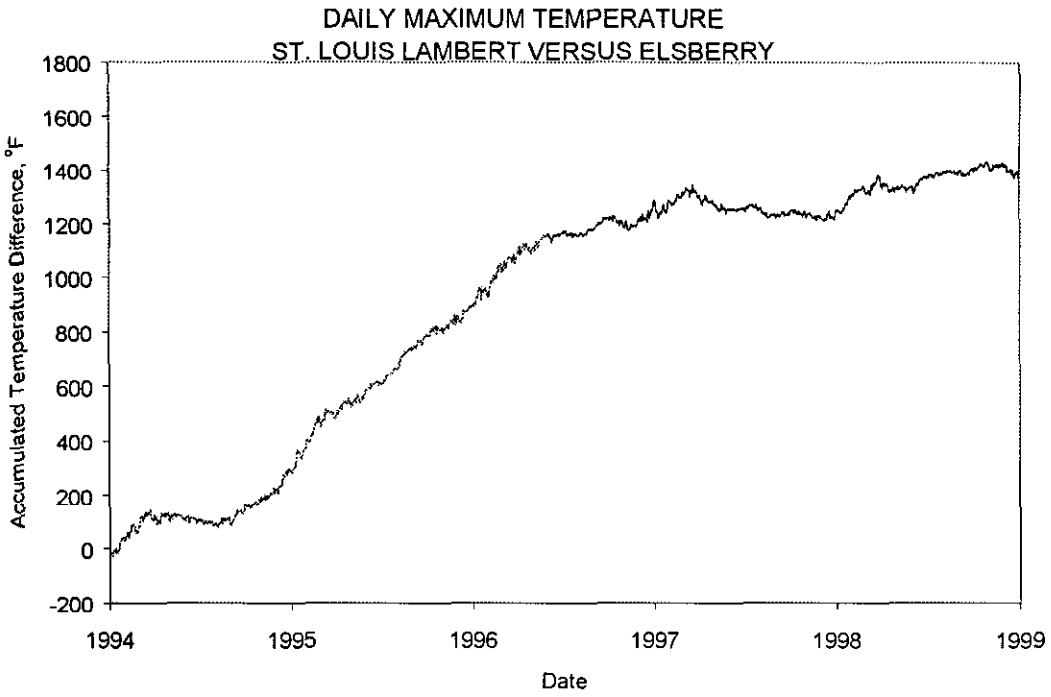
Q. Does this conclude your surrebuttal testimony?

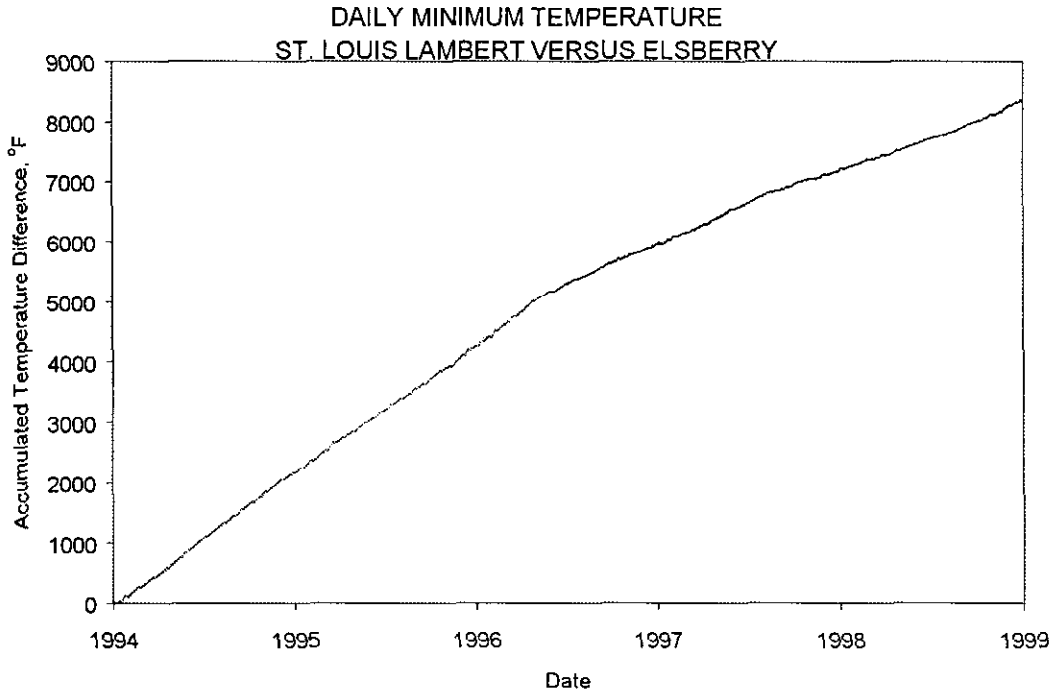
A. Yes, it does.





Schedule 1-1. Accumulated difference in daily maximum and minimum temperature between St. Louis Lambert and Jerseyville. The oscillations in the maximum temperature plot indicate seasonal variations in the temperature difference between these stations.





Schedule 1-2. Accumulated difference in daily maximum and minimum temperature between St. Louis Lambert and Elsberry. The oscillations in the maximum temperature plot indicate seasonal variations in the temperature difference between these stations.

BEFORE THE PUBLIC SERVICE COMMISSION

OF THE STATE OF MISSOURI

In the Matter of Laclede Gas Company's Tariff to Revise Natural Gas Rate Schedules.	)	Case No. GR-99-315

AFFIDAVIT

STATE OF MISSOURI	)	
	)	SS.
CITY OF ST. LOUIS	)	

Jay R. Turner, of lawful age, being first duly sworn, deposes and states:

1. My name is Jay R. Turner. My business address is 210 Cupples Hall II, Washington University, One Brookings Drive, St. Louis, Missouri 63130-4899; and I am Assistant Professor of Chemical Engineering in the School of Engineering and Applied Sciences for Washington University in St. Louis.

2. Attached hereto and made part hereof for all purposes is my surrebuttal testimony, consisting of pages 1 to 11, inclusive; and Schedule 1.

3. I hereby swear and affirm that my answers contained in the attached testimony to the questions therein propounded and the information contained in the attached schedule are true and correct to the best of my knowledge and belief.

Jay R. Turner

Subscribed and sworn to before me this 19th day of August, 1999.

PATRICIA P. HIC
Notary Public — Notary Se.
STATE OF MISSOURI
City of St. Louis
My Commission Expires: June 27 200

Patricia P. Hicks