Docket No. 05-057-T01
DPU Exh. No. 6.0SR (DGH-A)
Daniel G. Hansen
August 31, 2007

-BEFORE THE PUBLIC SERVICE COMMISSION OF UTAH-

In the Matter of the Joint Application	)	Docket No. 05-057-T01
of Questar Gas Company, the Division of	)	
Public Utilities, and Utah Clean Energy for	)	
the Approval of the Conservation Enabling	)	
Tariff Adjustment Option and Accounting	)	
Orders	)	

SURREBUTTAL TESTIMONY OF

DANIEL G. HANSEN

OF

CHIRISTENSEN ASSOCIATES ENERGY CONSULTING, LLC

August 31, 2007

I. Introduction

Q. Please state your name, title, and business address.

A. My name is Daniel G. Hansen. I am a Vice President at Laurits R. Christensen Associates, Inc. My business address is Suite 700, 4610 University Avenue, Madison, Wisconsin, 53705.

Q. Have you testified in this proceeding before?

A. Yes. On behalf of the Utah Division of Public Utilities (DPU), I filed testimony on June 1, 2007 with an accompanying report on natural gas decoupling mechanisms used in the United States (the “Hansen Report”); and I filed rebuttal testimony on August 8, 2007. My educational and business background may be found in Exhibit 6.2 of the June 1, 2007 testimony.

Q. What is the purpose of your testimony?

A. On behalf of the DPU, I am responding to the rebuttal testimonies of Mr. Kevin Higgins, witness for the Utah Association of Energy Users (UAE), and Dr. David Dismukes, witness for Utah Committee of Consumer Services, both filed on August 8, 2007.

Q. How is your testimony organized?

A. The remainder of my testimony is organized as follows:

- Section II: Discussion of Mr. Higgins’s Rebuttal Testimony
- Section III: Discussion of Dr. Dismukes’s Rebuttal Testimony
- Section IV: Summary and Recommendations

Q. What are the conclusions of your testimony?

A. Mr. Higgins and Dr. Dismukes concluded that the Commission should disregard the conclusion reached in Section 5.2 of Hansen Report, which is that the Conservation Enabling Tariff (CET) is not likely to lead to the shifting of risk between Questar Gas Company (Questar Gas) and its ratepayers. After reviewing their testimony, I have found that their conclusions are without merit.

Mr. Higgins improperly summarized the Hansen Report; he incorrectly believed that the test applied in Section 5.2 is “arbitrary and unduly restrictive” (Higgins, August 8, 2007, p. 6); and he incorrectly categorized the CET deferral effects associated with declining use per customer as a shift of risk from Questar Gas to its ratepayers.

Dr. Dismukes testified that the statistical model presented in Section 5.2 of the Hansen Report “is more than likely fraught with a variety of data, measurement, and estimation problems” (Dismukes, August 8, 2007, p. 12), but he failed to specifically identify even one of those problems. He continued by asserting that the published literature contains many examples illustrating that the results of Section 5.2 are implausible. However, the only result that is based on a credible analysis of Utah natural gas data (which is also from the most recent study listed) reaches the same conclusion as the Hansen Report: that there is no statistically significant relationship between price and usage levels for residential natural gas customers in Utah. (Bernstein & Griffin, 2005 Report, pp. 88-89.)

I therefore maintain the conclusion that I reached in both the Hansen Report and my rebuttal testimony filed on August 8, 2007 that the CET will not shift risk from Questar Gas to its ratepayers.

II. Discussion of Mr. Higgins's Rebuttal Testimony

Q. Please describe the rebuttal testimony of Mr. Higgins.

A. After examining my report filed on June 1, 2007 (the "Hansen Report"), Mr. Higgins disputed the analysis and conclusions contained in Section 5.2 of the report, writing that my "conclusion is overreaching and not adequately supported by the analysis" (Higgins, August 8, 2007, p. 4.) and that the theory underlying the analysis is "arbitrary and unduly restrictive." (Id., p. 6.)

Q. Please describe Section 5.2 of the Hansen Report.

A. Section 5.2 presents the results of a statistical analysis of the relationship between GS-1 use per customer and weather conditions, economic conditions, the commodity price, and a time trend. The purpose of the analysis was to determine whether changes in economic conditions and/or the commodity price affect GS-1 use per customer, and therefore whether the CET shifts risks associated with these factors from Questar Gas to its ratepayers. The analysis found no statistically significant relationship between annual GS-1 use per customer and economic conditions or the commodity price. I therefore concluded that the CET will not shift risk that can be attributed to these factors from Questar Gas to its ratepayers.

Q. Does Mr. Higgins correctly describe the Hansen Report?

A. No. His inaccurate descriptions of the Hansen Report are made most apparent on page 7 of his testimony (bold emphasis added):

> Dr. Hansen <u>deems</u> that revenue decoupling will convey **no reduction in risk to QGC** unless GS-1 usage per customer can be shown to vary significantly with changes in the natural gas price or changes in Utah economic conditions

> – irrespective of any other factors. Dr. Hansen thus rules out, by definition, any adjustments to QGC's rate of return **to reflect reduced risk from decoupling** which may be attributable to variables other than commodity price or the Utah economy.

As the bolded text indicates, Mr. Higgins has misconstrued the analysis in Section 5.2 to be about risk *reductions* for Questar Gas as opposed to risk *shifting* from Questar Gas to its ratepayers. In fact, Section 5.2 does not purport to examine whether the CET will reduce Questar Gas's risk, nor does it reach any conclusions regarding whether reductions in Questar Gas's risk that can be attributed to the CET should be accompanied by a reduction in Questar Gas's rate of return.

On page 6 of his testimony, Mr. Higgins further emphasizes his apparent confusion on this matter by writing that "Dr. Hansen's test for determining whether a **reduction in risk** should be recognized in QGC's allowed rate of return is arbitrary and unduly restrictive." (Emphasis added.) Again, Section 5.2 did not examine whether Questar Gas's risk would be reduced by the CET. The Section 5.2 analysis was conducted in an attempt to assess Dr. Dismukes's contention that "the proposed CET would **shift the risks** associated with changes in price, the economy, and other factors like greater economy-wide energy efficiency, away from the Company and to ratepayers without any offsetting shifts in rates." (Emphasis added; Dismukes, June 30, 2006, p. 28.)

These statements by Mr. Higgins reveal an apparent inability to understand that some risks can be reduced for one party without increasing risk for another party, as he uses these concepts interchangeably in his testimony.

Q. Can the CET reduce Questar Gas's risk without shifting risk onto its ratepayers?

A. Yes. Section II of my August 8, 2007 rebuttal testimony discusses the issue of risk and risk shifting in detail, and the distinction between the two is specifically addressed on pages 15 and 16. Observing that the CET will reduce the variability of Questar Gas's DNG revenues is not sufficient to conclude that the risk to *ratepayers* will increase. The *source* of the variability (risk) matters – specifically how the source of the variability affects Questar Gas and its ratepayers. A source such as weather, for which a particular outcome (e.g., a cold winter month) causes one party to be better off at the same time as the other party is worse off, will not lead to risk shifting through the CET. Alternatively, sources such as economic conditions or the commodity price, for which outcomes lead both parties to be worse off at the same time, produce the *potential* for the CET to shift risk. The potential is realized if the source of risk leads to changes in class-level use per customer, which can be tested using a statistical model such as the one presented in Section 5.2 of the Hansen Report.

Q. Are there other instances in which Mr. Higgins appears to misunderstand the Hansen Report?

A. Yes. On page 5, he writes that "Dr. Hansen summarizes his findings by concluding that weather risk from decoupling exists." Later, in his footnote 2 on page 7, Mr. Higgins adds:

> Dr. Hansen also tests for the significance of weather on usage per customer, but rules out any recognition in rate of return because "methods exist that can

> mitigate this risk for both the utility and its customers." [Report, p. 23] In fact, QGC's rate design for GS-1 already removes almost all of the weather-related volatility from revenue per customer, even without revenue decoupling.

These excerpts indicate that Mr. Higgins either did not understand or selectively quoted from the Hansen Report. He claims that I concluded that "weather risk from decoupling exists," even though I explicitly ruled out such an outcome in Section 3.3.3 of the report. He then implies that I am unaware that GS-1 revenues are adjusted for weather even though I specifically referenced Questar Gas's Weather Normalization Adjustment as an example of a mechanism that can reduce risk for both the utility and its ratepayers. (Hansen Report, p. 9.)

Q. Are there any examples of Mr. Higgins mischaracterizing the results of Section 5.2 of the Hansen Report?

A. Yes, on page 5 he writes that "The Utah GDP variable coefficient has a negative sign, suggesting (counter-intuitively) that an improvement in economic conditions reduces usage per customer," which he later writes "is suggestive of a likely (though not unusual) specification problem in his models." (Higgins, p. 6.) He fails to note that page 24 of Section 5.2 contains the following caveat about the result in question: "Again, because of the high correlation of these variables with the time trend variable, we do not believe that these estimates reflect actual customer behavior." Mr. Higgins also does not discuss the fact the coefficients on the economic variables are not statistically significantly different from zero when a time trend variable is included in the model. While such a finding may be counter to Mr.

Higgins's expectations, it is not counter-intuitive. It simply means that, on average, GS-1 customers have not changed their usage levels when economic conditions changed. He is correct that the results for the models that excluded the time trend variable indicated a potential specification problem, but he does not point out that the specification problem appears to be corrected by including the time trend variable.

Q. Earlier, you cited Mr. Higgins's concerns about the methods used in Section 5.2. Please describe the theory underlying the analysis in that section.

A. The theory underlying this analysis is quite simple, and can be described in a few bullet points:

- The CET affects ratepayer bills through deferrals.
- The CET will only produce deferral activity when class-level revenue per customer deviates from its allowed level.
- Deviations in class-level revenue per customer are driven by changes in class-level *use* per customer.
- Therefore, if a factor (such as economic conditions) does not affect class-level use per customer, the CET cannot produce a shift in risk from the utility to its ratepayers due to variation in that factor.

For example, Section 5.2 finds that economic conditions are not related to GS-1 use per customer in historical data. This indicates that future changes in economic conditions are not expected to lead to changes in class-level use per customer, and therefore will not lead to any CET deferral activity. In the absence of CET deferral activity, there can be no shift in risk from Questar Gas to its ratepayers.

The Hansen Report and subsequent rebuttal testimony add an important caveat to this simple argument: in order for risk shifting to occur, the potential source of risk must make both the utility and its ratepayers worse off at the same time (i.e., the risk must be "in the same direction" for the two parties). Therefore, factors such as the weather do not lead to risk shifting, while factors such as economic conditions and the commodity price may. Pages 5 through 10 of my August 8, 2007 rebuttal testimony describe this argument in more detail.

Q. Is this the test that Mr. Higgins found to be "arbitrary and unduly restrictive"?

A. It is unclear from his testimony whether Mr. Higgins believes that the theory itself is incorrect, and if so, what aspect he believes to be incorrect. It is clear that Mr. Higgins objects to the fact that Section 5.2 examined only the potential for risk shifting due to changes in the commodity price and economic conditions.

Q. What other factors does Mr. Higgins believe should have been examined?

A. He only lists the downward trend in use per customer as a factor that he believes should have been examined. Indeed, because the models that include only heating degree days (as a proxy for weather conditions) and a time trend variable explain over 96 percent of the variation in GS-1 use per customer, it would be difficult to conceive of very many additional factors that both explain variations in use per customer and have risks that are in the same direction for Questar Gas and its ratepayers. The only factors that I believed had the potential to meet these criteria were economic conditions and the commodity price.

Q. Can you describe Mr. Higgins's concern regarding reductions in use per customer over time?

A. Yes. His argument is summarized on page 7 of his testimony, which follows below:

> Second, in drawing his policy conclusion that there is no need to consider adjusting rate of return, Dr. Hansen ignores the very evidence that QGC presented in introducing its revenue decoupling proposal at the outset: namely that usage per customer has been declining for over 25 years and this decline reduces QGC's distribution non-gas ("DNG") revenue per customer in between rate cases. Even Dr. Hansen's own regression analysis demonstrates that the "annual time trend" variable is statistically significant in "explaining" the decline in usage per customer. Yet despite the fact that revenue decoupling will insulate QGC's revenue per customer from this downward usage trend, Dr. Hansen concludes that no risk reduction will occur from decoupling, and that no rate of return adjustment is warranted. This conclusion is not only unwarranted, it is difficult to fathom.

His argument appears to be this: everyone knows that use per customer is going to go down in the future (independent of any effects associated with economic conditions, commodity prices, or weather conditions). He confirms this view by writing that "GS-1 usage per customer has declined as a function of time… [this] was described and demonstrated in detail by QGC from the outset of this proceeding, and is not disputed." (Id., p. 6.) Because of this reduction in use per customer over time, Questar Gas will under-recover DNG revenues in between rate cases in the absence of the CET or the use of a forecast test year. He contends that because the CET will "insulate QGC's revenue per customer from this downward usage trend," Questar Gas's allowed rate of return should be reduced. (Id., p. 7.)

Q. Do you agree with this view?

A. No. I do not regard the observed downward trend in use per customer as a "risk." By definition, risk is associated with an *uncertain* outcome. The reduction in use per customer is something that has occurred over a long period of time and is expected to occur in the future. Mr. Higgins himself wrote that the reduction in use per customer over time "is not disputed." (Id., p. 6.) Any reduction in use per customer that is *expected* to occur should be accounted for in the design of the GS-1 DNG rates. Failing to account for the expected reduction in use per customer when setting rates will, all else equal, lead to under-recovery of the utility's DNG costs and under-payment of DNG revenues by ratepayers. It therefore appears that Mr. Higgins is merely interested in maintaining a transfer of dollars from Questar Gas to its ratepayers by retaining a flawed ratemaking method (i.e., the use of an historical test year in the absence of decoupling).

Q. Has any other testimony been offered that is consistent with your view that accounting for expected reductions in use per customer does not constitute a shift in risk from Questar Gas to its ratepayers?

A. Yes. Section VIII of Dr. Dismukes's June 1, 2007 testimony suggested using a forecast test year to deal with the revenue effects associated with declining use per customer. As reflected on page 32 of my August 8, 2007 rebuttal testimony, I agree that a forecast test year is an adequate substitute for the CET in addressing these effects (but forecast test years do not resolve utility conservation incentive issues as the CET does). Regarding whether the use of a forecast test year shifts risks from the utility to its ratepayers, Dr. Dismukes wrote "most importantly, the current risk associated with changes in sales would remain with the Company and its shareholders, and not shifted to ratepayers." (Dismukes, June 1, 2007, p. 54.) At no point in his detailed discussion of how the use of a forecast test year would work does Dr. Dismukes mention that it would require a reduction in Questar Gas's rate of return.

Therefore, Dr. Dismukes and I appear to agree that a rate mechanism that accounts for *expected* reductions in use per customer over time does not require a reduction in the utility's rate of return. Such a mechanism simply corrects an inequity that was allowed to persist through the use of imperfect ratemaking practices (i.e., the use of an historical test year in the absence of decoupling).

Q. Doesn't the CET also compensate Questar Gas for *unexpected* changes in use per customer over time?

A. Yes. There have been, and certainly will continue to be deviations from the expected (or average) reduction in use per customer. For example, Section 5.2 of the Hansen Report found that GS-1 use per customer declined by approximately 2.3 decatherms per year from 1980 through 2005. If the reduction in a particular year (controlling for other factors, such as weather) was actually lower, say 2.7 decatherms, the CET would increase Questar Gas's DNG revenues to make up for the difference.

Q. Does that mean that the CET shifts this risk from Questar Gas to its ratepayers?

A. No, because the risk associated with deviations from the trend in use per customer is "in opposite directions" for the utility and its ratepayers. That is, if a forecast test year had been implemented instead of the CET, the goal of the ratemaking process would be to arrive at an unbiased estimate (i.e., as likely to be too high as it is to be too low) of the change in use per customer over time and account for the expected reduction when setting rates. In a particular year, if the reduction in use per customer is smaller than the assumed value, the utility would be better off in the absence of the CET (i.e., it would over-recover DNG revenues relative to the outcome under a forecast test year), but ratepayers would be worse off (i.e., their bills would be higher than they would have been under a forecast test year). However, the outcome could be reversed in the following year. Therefore, the only difference between using a forecast test year and the CET to address declining use per customer is that the CET will smooth out the revenue and bill impacts associated with deviations from the expected reduction in use per customer over time. This should not harm, and may benefit, both the utility and its ratepayers.

Q. How would you summarize Mr. Higgins's rebuttal testimony?

A. Mr. Higgins's conclusions regarding the Hansen Report should be disregarded by the Commission. He did not provide an accurate summary of the analysis and conclusions contained in Section 5.2 of the Hansen Report, he provides no basis for his assertion that the theory underlying the Section 5.2 analysis is "arbitrary and unduly restrictive" (Higgins, p. 6.), and his concern about the omission of declining use per customer as a source of risk shifting is unwarranted.

III. Discussion of Dr. Dismukes's Rebuttal Testimony

Q. Please describe Hansen Report's conclusions regarding whether the CET is expected to shift economic or commodity price risks from Questar Gas to its ratepayers.

A. As described in the previous section, the Hansen Report concluded that the CET will not shift economic or commodity price risks from Questar Gas to its ratepayers. The conclusion was based on a statistical analysis of GS-1 use per customer data from 1980 through 2005, which found no statistically significant relationship between use per customer and economic conditions or the commodity price.

Q. Did this finding meet with any opposition in rebuttal testimony?

A. Yes. I have already addressed Mr. Higgins's testimony that the findings are "suggestive of a likely (though not unusual) specification problem in his models." (Higgins, p. 6.) In addition to this, Dr. Dismukes wrote that "the empirical results are completely at odds with about 40 years of academic research and industry practice" (Dismukes, p. 10) and that "it is more than likely fraught with a variety of data, measurement, and estimation problems that make any of the empirical conclusions reached in the study unusable in this proceeding." (Id., p. 12.)

Q. Does Dr. Dismukes explicitly identify any of the problems he believes are "likely" to have affected the statistical analysis?

A. No. Dr. Dismukes did not make any specific comments regarding shortcomings in the data sources or estimation methods used in the analysis.

Q. Does Dr. Dismukes cite any specific results from academic research and industry practice?

A. Yes, he includes Rebuttal Exhibit CCS-1.3R, which contains brief summaries of twenty estimates of price elasticities (i.e., the source of commodity price risk) and twelve estimates of income elasticities (i.e., the source of economic risk). Some of the estimates are taken from the same study, so that there are actually twelve studies of price elasticities and seven studies of income elasticities referenced in the exhibit. In addition, all of the studies of income elasticities also appear on the list of studies of price elasticities.

Q. Please describe the studies of price elasticities included in Rebuttal Exhibit CCS-1.3R.

A. The exhibit sorts the studies in descending order of date. The first study included on the list, Bernstein & Griffin (2005), was conducted in 2005 and examines state-level data, including data for the state of Utah. For the next study listed, Hsing (1992), Dr. Dismukes lists a price elasticity for the state of Alaska. It is unclear why he chose to do this, as the Hsing study purports to estimate elasticities for all states except Hawaii. The next four listed results are from a study conducted 18 years ago that examined data from France and West Germany. The remaining studies on the list were conducted between 26 and 56 years ago. Half of the listed results[1] are not even related to natural gas price response – they are analyses of residential *electricity* price response, which may be very different from natural gas price response because of the differences in the end uses for which each is used. The summaries of at least four of the results are incorrect: the short-run price elasticity estimated in Houthakker et al (1973) is listed as -0.9, where the correct value (-0.09) is an order of magnitude

[1] If the rows of table on Page 1 of Rebuttal Exhibit CCS-1.3R were numbered 1 through 20, the results that are taken from analyses of electricity demand appear on rows 10, 11, 12, 13, 14, 15, 16, 17, 19, and 20.

smaller; and three of the results from Beierlein, Dunn, & McConnon (1981) that are described as short-run price elasticities are in fact cross-price elasticities (showing the change in electricity demand as the price of natural gas changes).

Of the studies listed in Exhibit CCS-1.3R, only the most recent study by Bernstein & Griffin (2005) appears to be relevant to the issue at hand, in that it uses relatively recent data that includes information specific to Utah and attempts to estimate *natural gas* price elasticities. The Hsing (1992) study included information specific to Utah, but does not produce relevant results for reasons described below.[2]

Q. Did Dr. Dismukes correctly characterize the findings of the Bernstein & Griffin (2005) study?

A. Not really. His summary of a -0.18 short-run price elasticity and a -0.44 long-run price elasticity for the mountain region (which includes Arizona, Colorado, Idaho, Montana, Nevada, New Mexico, Utah, and Wyoming) is contained in the study. However, he fails to note that the study contains *state-level* results as well as regional results, which are described in Chapter 5 with the associated regression results presented in Appendix D.

When examining the price elasticities specifically for Utah, Bernstein & Griffin estimate a short-run price elasticity of -0.031 and a long-run price elasticity of -0.061, which are considerably lower (in absolute value) than the estimated price elasticities for the mountain region as a whole that Dr. Dismukes chose to report.

[2] At the time that surrebuttal testimony was due, Dr. Dismukes had not yet provided copies of the Mount, Chapman, & Tyrrell (1973) or Wilson (1971) studies, as we had requested. However, the titles of the articles indicate that they relate to electricity demand and not natural gas demand. Therefore, I do not believe that the studies are relevant to this proceeding. If the opportunity to review the studies reveals information that I believe is important to this proceeding, I will provide it in supplemental testimony.

More importantly, *neither of these estimated price elasticity values for the state of Utah is statistically significantly different from zero*. In fact, neither result is even *close* to being statistically significant. Traditionally, if the “p-value” associated with an estimated coefficient is less than 0.10 or 0.05, the coefficient would be regarded as being statistically significantly different from zero. The p-values associated with the Utah state specific short- and long-run price elasticity estimates are far higher than these traditional standards, at 0.771 and 0.776, respectively, and therefore one should regard these estimates as being no different from zero.

In summary, the findings of the Bernstein & Griffin (2005) study are completely consistent with the findings contained in Section 5.2 of the Hansen Report, in that both show no statistically significant relationship between Utah residential natural gas consumption and natural gas prices.

Q. Please describe the Hsing (1992) study.

A. This study attempted to examine differences in natural gas price and income elasticities across states. The results are based on five years of data, from 1985 through 1989, for each state except Hawaii (for which no data were available). At the national level, the study estimates a price elasticity of -0.738 and an income elasticity of 0.476 (using the double-log specification). For Utah (based on 1989 data), it estimates a price elasticity of -0.55 and an income elasticity of 0.39.

Q. Do you find these results to be credible?

A. No. There are several reasons that the results either are not credible or are not relevant to the present Questar Gas proceeding. A practical and fairly non-technical reason for questioning the results is that they are based on data from a relatively short period of time, 1985 through 1989, during which natural gas prices in Utah did not experience much variation. Because the range of experience with prices is fairly narrow during that time, the elasticity estimates may not be representative of response in subsequent years, in which natural gas prices were considerably more volatile.

A more important, but also more technical (and therefore less easily conveyed) reason for disregarding the estimates in the Hsing study is because of serious flaws in the methods used. The intent of the study is to examine differences in price elasticities across states. However, the study only estimates one aggregate relationship between use per customer and the natural gas price. Though the data set contains information from 49 states, the estimated price coefficient represents an average, national-level effect (though it does not appear to be properly weighted by state population).

In one of the models (the “double-log” model), the estimated coefficient can be directly interpreted as an elasticity. In the other two models (the “General” and “Linear” models) the estimated coefficient must be multiplied by the ratio of the natural gas price divided by the use per customer in order to be interpreted as an elasticity (which is defined as the percentage change in quantity divided by the percentage change in price or income). The study performs this adjustment using the 1989 data from each state and interprets the results as showing that the “price elasticities varied widely.” (Hsing, p. 256.) In fact, the study has only demonstrated that the ratio of the natural gas price to use per customer has varied widely across states.

Compounding the difficulty in deriving any meaning from the results, Hsing failed to estimate the standard error associated with any of the state-level results. It is therefore impossible for a reader to determine whether the state-level effects are statistically significantly different from one another, or whether they are different from zero.

In addition, Hsing does not employ standard statistical methods for the type of data that he uses. The data used in the analysis are referred to as "panel" data, which combines "time series" data (e.g., information across time for one state) and "cross-sectional" data (e.g., information across states for one year). Specifically, according to A Guide to Econometrics by Peter Kennedy[3], "Fixed and random effects models are usually employed when the number of cross-sectional units is large and the number of time periods over which those units are observed is small." (Kennedy, p. 225.)

Bernstein & Griffin (2005), which also employs panel data, uses a fixed effects model, as reflected by the s_i parameter in first equation on page 59. In order to estimate state-level price elasticities, Bernstein & Griffin interact the state-level variables (s_i) with the natural gas price variable. (Bernstein & Griffin, p. 60.) In contrast to the methods used in Hsing (1992), this is a valid method for estimating differences in price elasticities across states. It allows the estimate of price responsiveness to vary across states and produces a standard error for each state-level estimate, against which the level of statistical significance of each estimate may be judged.

[3] Published by The MIT Press in Cambridge, MA in 1992.

As noted above, the valid methods employed by Bernstein & Griffin produced Utah price elasticity estimates that were not statistically significantly different from zero. The results of Hsing (1992) are based on flawed methods and should therefore be disregarded by the Commission.

Q. Please describe the studies of income elasticities included in Rebuttal Exhibit CCS-1.3R.

A. These studies are a subset of those provided regarding price elasticities. The estimate contained in Hsing (1992) should be disregarded for the reasons described above. (That is, all of the problems that I have described for its estimates of price elasticities are equally applicable to its estimates of income elasticities.) Dr. Dismukes lists eight (out of twelve) results that are not related to natural gas usage (they are studies of electricity usage). This leaves only the first three results of the Beierlein, Dunn, & McConnon (1981) study, which uses data for the northeastern United States between 1967 and 1977. In summary, the studies listed on page 2 of Exhibit CCS-1.3R should not be regarded as relevant to the current proceeding, as they incorporate some or all of the following traits: estimates based on flawed methods, data from other locations, elasticity values for industries other than natural gas, and information that is decades out of date.

Q. Dr. Dismukes is concerned that "if the Division's results are accepted, then increases in natural gas prices since the winter of 2000-2001 have had no material impact on customer usage." (Dismukes, p. 10.) Is there any evidence that such an impact occurred?

A. Perhaps surprisingly, there is no evidence that the large increases in natural gas prices during that winter led to significant reductions in customer usage. Dr. Dismukes's hypothesis can be explicitly tested by expanding upon the analysis contained in Section 5.2 of the Hansen Report.

The statistical model presented in column 6 of Table 1B of the Hansen Report shows that including only heating degree days and a time trend variable accounts for 96.4 percent of the variation in GS-1 use per customer during the 1980 through 2005 time period. In order for Dr. Dismukes's hypothesis to be correct, the high natural gas prices that began in the winter of 2000-2001 would need to cause one of two effects to occur in the subsequent years: a reduction in the average use per customer, or in an increase in the *rate* of reduction of use per customer (i.e., a more steeply declining time trend).

DPU Exhibit 6.1SR shows the findings associated with this analysis. Column 1 replicates the results from Table 1B in Section 5.2 of the Hansen Report. Column 2 shows the results when an indicator variable is added for the years 2001 through 2005. The coefficient for this variable is therefore an estimate of the average change in GS-1 use per customer after December 2000, controlling for the effects of weather and the overall decline in use per customer since 1980. The estimated coefficient is negative (which indicates a reduction in use per customer following the year 2000), but is not even close to being statistically significantly different from zero. The p-value for this coefficient is 0.705. (Recall that p-values less then 0.10 or 0.05 are traditionally considered as representing statistical significance.) The conclusion from this model is therefore that GS-1 use per customer was not, on average, lower from

2001 through 2005 than it was from 1980 through 2000, controlling for the effects of weather and the downward trend in use per customer.

Column 3 of DPU Exhibit 6.1SR shows the results of a test for whether the *rate* of decline in use per customer changed in 2001 to 2005 relative to 1980 to 2000. This is tested using an "interaction" variable that is defined as the product of the annual time trend variable and the indicator variable for the years 2001 to 2005. To be consistent with Dr. Dismukes's hypothesis that increases in natural gas prices had a material effect on customer usage, the estimated coefficient on this variable needs to be negative (i.e., customers are reducing usage at a faster rate than they were prior to 2001) and statistically significantly different from zero. The results show an estimated coefficient that is negative, but is not statistically significantly different from zero (with a p-value of 0.735). Therefore, the conclusion from this model is that the rate of reduction in use per customer was not statistically significantly different from 2001 to 2005 than it was from 1980 to 2000.

Q. Doesn't your analysis build upon a model to which Dr. Dismukes has already objected?

A. Yes, so it is worth addressing the potential concerns that he raised in his rebuttal testimony. The data used in the analysis (use per customer and heating degree day data) are taken directly from Questar Gas's databases. That would seem to remove Dr. Dismukes's concern about potential measurement problems for the models presented in DPU Exhibit 6.1SR. In addition, though he may assert that "estimation problems" continue to exist, the model presented in column 1 of DPU Exhibit 6.1SR demonstrates a relationship between use per customer and weather and a time trend that even Mr. Higgins described as "obvious."[4] The models shown in the next two columns simply add variables that are a function of time (and therefore not subject to measurement error) to test whether use per customer differed either on average, or in terms of the rate of change, following the increases in natural gas prices that began in the winter of 2000 to 2001. If customer response to the increase in prices had been sufficiently large to affect class-level use per customer between 2001 and 2005, these models would have been able to identify the response with statistically significant estimates. No such effect was found.

Q. Do your results indicate that customers were not worse off after the large increases in prices, or were somehow indifferent to them?

A. Not at all. The results do not diminish the hardship that increases in natural gas prices place on customers. On the contrary, the results indicate that customers appear to value the services that natural gas delivers so highly that even a fifty percent increase in the delivered price (between January 2000 and January 2001) does not produce a significant change in class-level use per customer.

Q. Dr. Dismukes also points out that Questar Gas "estimates a -0.06 price elasticity of demand that is derived from its load forecasts supporting its Integrated Resource Plan ("IRP"). (Page 11) Doesn't this contradict your findings?

A. It's hard to say. Questar Gas's response to Data Request No. CCS 4.05 from May 5, 2006 indicates that the estimated price elasticity was derived "[b]y varying the model inputs for income and gas prices." (Page 3.) This indicates that the reported

[4] Mr. Higgins wrote: "The only clear implications of Dr. Hansen's statistical results are the obvious conclusion that GS-1 usage per customer is a function of temperature and a confirmation that GS-1 usage per customer has declined as a function of time. The latter phenomenon was described and demonstrated in detail by QGC from the outset of this proceeding, and is not disputed." (Higgins, p. 6.)

elasticity value was *simulated* rather than *estimated*, and Questar Gas does not report the standard error associated with this simulated value. That is, the elasticity value was generated by plugging values into a model, as opposed to being directly estimated from data. I cannot therefore say with any statistical certainty whether the estimated value is statistically significantly different from zero.

In any case, in discussing this estimate Dr. Dismukes wrote that it "appears to be small" (p. 11), even though he had earlier found the value to be sufficient for use in a statistical re-coupling approach that he recommended in his June 30, 2006 testimony.[5]

Remember, Dr. Dismukes is not only proposing that customer price response *exists* for Utah's GS-1 customers (which is a reasonable hypothesis to test), but that the *level* of price response – and therefore the magnitude of the shift in risk from Questar Gas to its ratepayers – is so large that the CET should be discontinued.

Q. How would you summarize Dr. Dismukes's criticisms of the estimates contained in Section 5.2 of the Hansen Report?

A. Dr. Dismukes asserted that the analysis is "more than likely fraught with a variety of data, measurement, and estimation problems" (page 12), but provided no indication of what those problems might be.

Dr. Dismukes asserted that the price increases during the winter of 2000 to 2001 *must* have reduced customer usage levels, but provided no evidence that it occurred. An analysis presented here indicates that no such effect can be observed in the data.

[5] He wrote: "The elasticity estimates (and trend adjustment) could come from the Company's most recent IRP that includes an income elasticity of 0.05 and a price elasticity of -0.06 on a use per customer basis." (Dismukes, June 30, 2006, p. 11.)

Dr. Dismukes provided twenty estimates of price elasticities from published literature as evidence that the results of the Hansen Report are not plausible. However, virtually all of these results are not applicable to the current proceeding: five of the estimates use data from Europe, ten of the estimates do not examine natural gas data, and fourteen estimates are taken from studies that are more than twenty-five years out of date. Two studies that he cites examine natural gas usage in the state of Utah: one is based on flawed methods and uses only data from 1985 to 1989 (when very little variation in natural gas prices occurred relative to recent years); and the other study reaches the same conclusion as the Hansen Report: that there is no statistically significant relationship between residential usage and price.

Q. Please describe Dr. Dismukes's concern about the relationship between the CET and the WNA.

A. On pages 16-17 of his testimony, Dr. Dismukes provides an excerpt from the Hansen Report regarding the relationship between decoupling and weather normalization mechanisms, that "[d]ecoupling mechanisms improve the functioning of weather normalization mechanisms by 'cleaning up' any errors in the definition of normal weather." (Hansen Report, p. 14.) He believes that this indicates a problem with the CET because "the motivating factor for its adoption was to promote DSM, not to correct for deficiencies in the Weather Normalization Adjustment." (Dismukes, August 8, 2007, pp. 16-17.)

Q. Do you think this is a valid criticism of the CET?

A. No. Nowhere in the Hansen Report or my subsequent rebuttal testimony did I identify a deficiency in the definition of normal weather used in the WNA.[6] In addition, I have not asserted that the purpose of the CET is to "correct for deficiencies in the Weather Normalization Adjustment." However, it seems unwise to propose that the CET should be abolished because it has the ability to correct for any problems that *may* exist in the definition of normal weather used in the WNA. It is as though he is suggesting that a program should not be approved if it produces benefits that are unrelated to its primary purpose.

Q. Dr. Dismukes cites the observed deferrals for the CET to date as "an alternative measure for the magnitude of risk shifting between GS-1 customers and the Company." (Id., p. 13.) Do you agree with this conclusion?

A. No. The *source* of the deferrals matter; and Dr. Dismukes has made no attempt to identify the cause of the deferrals.

For example, it could be that the deferral represents the effects associated with ongoing declines in GS-1 use per customer. Dr. Dismukes has testified that Questar Gas should be compensated for expected declines in use per customer through the use of a forecast test year, and that doing so does not represent a shift in risk.

The expected CET deferral associated with declining use per customer can be estimated from information presented earlier. This amount is equivalent to the amount that GS-1 DNG rates would be increased during the rate case with the use of a forecast test year.

[6] Errors in setting the definition of normal weather used in a weather normalization mechanism will skew payments toward either the utility or its ratepayers, depending upon the direction of the error.

Suppose that there are 820,000 GS-1 customers (the approximate average number between July 2006 and April 2007), that use per customer declines 2.3 Dth per year (consistent with the time trend coefficient estimate shown in DPU Exhibit 6.1SR), and that the DNG price per Dth is $1.85 (in between the first block prices for the winter and summer seasons). Under these conditions, one would expect a CET deferral of about $3.5 million, if the reduction occurred on January 1st and lasted the full year. If the reduction instead occurred steadily over the year, a deferral of about $1.75 million would be expected (half of the $3.5 million).

As this (admittedly somewhat crude) estimate of the expected effect associated with declining use per customer shows, one can expect the CET to produce deferrals in Questar Gas's favor that are of the same magnitude as the observed deferrals for this reason alone.

Q. Could the observed deferrals have been caused by a shifting of commodity price or economic risks?

A. No. Even if one does not accept the conclusions of the Hansen Report regarding the CET's potential for shifting these risks onto ratepayers, the conditions required in order for ratepayers to be made worse off by these risk shifts are not present.

Commodity price risk adversely affects ratepayers when they reduce usage levels in response to increases in prices. However, Questar Gas's GS-1 rates (including both non-gas and commodity components) have actually gone down since the CET pilot began in July 2006. Economic risk adversely affects ratepayers when deteriorating economic conditions cause customers to reduce usage levels. However, according to data from the Bureau of Labor Statistics, Utah unemployment rates are lower now than they were in July 2006.

Therefore, the conditions required for ratepayers to be made worse off by these risks have not existed during the CET pilot.

Q. Does Dr. Dismukes acknowledge the ratepayer risk inherent in GS-1 DNG rates?

A. No. The GS-1 DNG rates contain risk for ratepayers, in that any fluctuation in usage levels changes the amount that they pay for fixed DNG costs. Another way of looking at the observed deferral could therefore be that it offsets ratepayer under-payment for DNG services. In subsequent periods, usage levels could lead to ratepayer *over*-payment for DNG services, which the CET would also correct.

IV. Summary and Recommendations

Q. Please summarize your surrebuttal testimony.

A. Mr. Higgins and Dr. Dismukes concluded in their rebuttal testimony that the Commission should disregard the conclusion reached in Section 5.2 of Hansen Report, which is that the CET is not likely to lead to the shifting of risk between Questar Gas and its ratepayers.

However, Mr. Higgins improperly summarized the Hansen Report; he incorrectly believed that the test applied in Section 5.2 is “arbitrary and unduly restrictive” (Higgins, p. 6); and he incorrectly categorized the CET deferral effects associated with declining use per customer as a shift of risk from Questar Gas to its ratepayers.

Dr. Dismukes believed that the statistical model presented in Section 5.2 of the Hansen Report “is more than likely fraught with a variety of data, measurement, and estimation problems" (Dismukes, p. 12), but he failed to specifically identify even one of those problems. He continued by asserting that the published literature contains many examples illustrating that the results of Section 5.2 are implausible. However, the only study that he presents that credibly analyzes Utah natural gas data (which is also the most recent study listed by more than a decade) reaches the same conclusion as the Hansen Report: that there is not a statistically significant relationship between price and usage levels for residential natural gas customers in Utah.

Q. What are your recommendations based on this testimony?

A. The Commission should disregard the criticisms of Hansen Report contained in the testimony of Mr. Higgins and Dr. Dismukes. I continue to recommend that the Commission retain the CET.

Q. Does this conclude your testimony?

A. Yes.