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BEFORE THE ENVIRONMENTAL QUALITY COUNCIL
STATE OF WYOMING

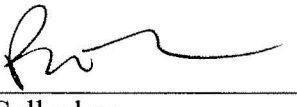
IN THE MATTER OF:	)	Docket No. 09-2801
MEDICINE BOW FUEL & POWER, LLC	)	
AIR PERMIT CT-5873	)	

REBUTTAL REPORT OF RANAJIT SAHU

Sierra Club hereby submits *Ranajit Sahu's Rebuttal Report*.

Respectfully submitted this 30th day of September, 2009.

FOR PETITIONER SIERRA CLUB



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REBUTTAL REPORT OF RANAJIT SAHU

A. Qualifications

My Qualifications are the same as discussed in my main Report on this matter submitted on September 1, 2009 and I incorporate that by reference.

The following is a listing of the items provided by the rules of procedure:

1. This Rebuttal Report contains my opinions, conclusions and the reasons therefor; and,
2. Data or other information I used in forming my opinions are provided in footnotes throughout the Rebuttal Report; and,
3. A statement of my qualifications is contained in Attachment A of my main Report; and,
4. A list of publications I have authored within the last ten years is shown in Attachment A of my main Report; and,
5. My compensation for the preparation of this Report is $125/hour and my compensation for testimony is $150/hour; and,
6. A statement of my previous testimony within the preceding four years as an expert at trial or by deposition is contained in Attachment A of my main Report.

This Rebuttal Report contains conclusions based on information in my possession at the time of submittal of the Rebuttal Report. Should additional data or other documentation become

available I reserve the right to appropriately revise my analysis, calculations, and conclusions and to supplement the contents of this Rebuttal Report.

B. Introduction

In this Rebuttal Report, I provide rebuttal discussion to the various issues raised and discussed in the Expert Report of Ms. Katrina Winborn relating to the proposed IGL plant by Medicine Bow Fuel & Power LLC (MBFP). Like Ms. Winborn, I will address the issues in the order they are discussed in her report.

C. Medicine Bow is a Major Source of SO_2 Emissions

As discussed in my main report, I disagree with the potential to emit (PTE) calculations presented by MBFP in which emissions (of SO2 and other pollutants) were excluded from cold startup events, thereby deeming the proposed plant to be a minor source of SO2 emissions. It appears that Ms. Winborn agreed with me that emissions from normal activities should be included in the PTE calculations. The question then is whether cold-start emissions are normal for the proposed facility. It is my opinion that they are and, as discussed in my main report, this is also consistent with prior EPA guidance. There is no disagreement between MBFP and me that: (i) periodic cold start events are an essential part of the facility's operations; (ii) that they will likely occur at least every four or so years; and (iii) that when they occur, they will be significant as far as SO2 emissions are concerned. As such, these events are predictable, foreseeable, planned, and essential for long term facility operations. Just because they do not occur annually or more frequently does not make them not normal. In addition, just because they may require a different type of pre-planning or may require "large-scale maintenance work" does not make them not normal. It is my opinion that events that may occur predictably but perhaps not as frequently as annually, are nonetheless, normal. For example, no one would deny that, as part of normal maintenance, a house may need to be painted every few years. Just as valid, the same lawn for the same house may need to be mowed every week. Both are normal activities. While they may have different characteristics, both are essential, predictable, and therefore normal.

Ms. Winborn makes the interesting observation that it may be detrimental to include in the PTE emissions from cold-start emissions because that would result in a higher PTE for the facility, and thereby allow greater emissions during time periods that do not include cold-start emissions. It is my opinion that her concern in this regard is easily handled by proper drafting of the permit conditions restricting the allowable emissions from the facility. Very simply, the permit can be drafted to exclude emissions from cold-start events when these events are not expected to occur.

Finally, Ms. Winborn makes the assertion that even if cold-start emissions were included in the PTE and the facility was deemed to be a major source of SO_2 emissions, the Best Available Control Technology (BACT) analysis for the facility would result in no additional limitations than in the proposed permit and the SSEM Plan as currently proposed would be BACT. I disagree. First, regardless of outcome, a complete top-down BACT analysis should be completed as a result of the source being deemed major. As part of the BACT analysis, numerical emissions limits from the flare should be considered as discussed later. A work practice standard such as the SSEM Plan is only allowable as BACT if numerical limits are not possible. That is far from being the case. It is possible to include numerical limits for the flare and the means available to assess compliance against such numerical limits using techniques such as the long-path infrared measurements discussed in my main report. Ms. Winborn mischaracterizes my main report relating to long-path measurements as being irrelevant as controls for flares. I agree that such measurements are not controls. However, by providing the ability to directly measure emissions from flares, they permit the setting of numerical limits on flare emissions and thus allow for more stringent monitoring of flare emissions. I also disagree with Ms. Winborn that such techniques are not worthy of consideration because they are "not yet in common use." The degree of common use, in and of itself, is not a BACT consideration, if the controls are available. In fact, BACT is technology-forcing and including the degree of common use in BACT determinations would be fundamentally counter to inclusion of new technologies and the forward-looking nature of BACT.

For all these reasons, I believe that the proposed facility is a major source of SO_2 emissions and it should therefore be required to conduct a full top-down BACT analysis for SO_2.

As an aside, I continue to hold that flares, by their very design, do not meet the requirement of VOC control devices since they do not incorporate and consideration of minimum gas residence time. While all flares do provide some destruction, such destruction efficiency is coincidental and not enforceable. This is engineering reality.

D. The SSEM Plan is Insufficient as BACT for Flare SSM Emissions

I have no quarrel with Ms. Winborn's characterization of the Plan in question as being the SSEM Plan. I will use the same terminology. I disagree that it is BACT, however.

First, it is not disputed that an actual SO_2 BACT analysis was not conducted by MBFP and the DEQ. Had a proper BACT analysis been done it would have resulted in at least some significant analysis that then would have been reflected in the Application and the record.

Second, it is undisputed that the SSEM Plan represents a work practice standard since it has no numerical limits.

Third, it is undisputed that the BACT definition is an emissions limit, namely a numerical emissions limit. Only if it can be shown that no numerical limits are feasible, can a BACT analysis rely on a work practice standard. In fact Ms. Winborn's report itself makes this clear when she properly quotes the BACT definition, as follows:

> "If the Administrator determines that technological or economic limitations on the application of measurement methodology to a particular emissions unit would make the imposition of an emissions standard infeasible, a design, equipment, work practice, operational standard, or combination thereof, may be prescribed instead to satisfy the requirement for the application of best available control technology. (40 CFR 52.21(b)(12)."

As the definition above makes clear, relying on a work practice standard is conditioned upon findings that "…technological or economic limitations would make the imposition of an emissions standard infeasible…" No such determination was made here.

Thus, it is misleading for Ms. Winborn to note "..that a work practice or operational standard is an acceptable means to establish BACT…" Acceptable, yes, but only as conditioned above.

In fact, nothing prevents numerical limits from being placed on flares. A recent BACT determination from the state of Iowa for startup/shutdown flares fired on natural gas or syngas from a gasification process at Homeland Energy Solutions, LLC provided flare BACT limits of 0.0076 lb/MMBtu for PM or PM_{10}, 0.200 lb/MMBtu for NO_X, 1.1 lb/MMBtu for CO, and 0.0060 lb/ MMBtu for VOC.[1]

Thus, assuming that the SSEM Plan is BACT is premature and erroneous.

Fourth, the SSEM Plan does not meet BACT requirements. Contrary to Ms, Winborn's assertion that it is a "specific and enforceable standard," I find that it is non-specific and generally unenforceable. As currently written, it is a preliminary collection of statements that are mainly goals. Where there are numerical (and therefore enforceable) values, there is no rationale or basis provided. Therefore one cannot assume that the goal of minimizing gas flows or duration times to the flares is actually met.

At the very outset, the SSEM itself notes that "[S]pecific startup and shutdown operating procedures for all process units shall incorporate the elements of this Plan to the greatest extent possible." Clearly, this means that this Plan is not the actual operating guide. It also means that "specific" procedures will be developed later, contrary to Ms. Winborn's assertion. I also do not believe that "greatest extent possible" is an enforceable standard. As the EPA noted in its comments on this matter, noting that the Plan's goal is to provide "…guidelines and

[1] http://cfpub.epa.gov/rblc/cfm/ProcDetl.cfm?facnum=26711&Procnum=106236, US EPA, "RACT/BACT/LAER Clearing House Standard Search: Process Type: Miscellaneous Combustion: Flares", Accessed 07/16/2008.

suggestions…", "…[I]f the Plan is a meaningful tool, it should provide requirements rather than suggestions…"[2]

As an example of a numerical value the SSME Plan, it notes that "[O]ne gasifier will be started at a time at 50% design flow rate…" However, there is no discussion in the record as to why this is the appropriate level or why starting at this level minimizes the amount of flaring. Nor is there discussion of the feasibility or infeasibility of starting up at lower or higher values which might minimize flaring. Of course, the SSEM Plan goes on to note that low pressure and normal operating pressure checks are required, without any numerical specificity – making them unenforceable.

These are examples, but a simple review of the three and half page SSEM Plan makes it clear that it is not the specific procedure that will be actually used in practice; it is mostly general; it does not justify any of the few numerical choices that are contained in it; it is likely to change in the future[3] without public review,[4] and it does not provide any discussion that connects it to the professed goal of minimizing flare gases and SO_2 emissions during startup.

For all of these reasons, I disagree that the SSEM Plan is BACT.

E. Fugitive VOC Emissions Improperly Estimated

With regards to the estimation of fugitive emissions, I reiterate the following. My dispute with the calculations was not that the appropriate spreadsheets were not provided as Ms. Winborn seems to believe. Rather, it was the content of the spreadsheets. Specifically, my comments pertained to the lack of support for almost all of the variables that are used in these calculations.

[2] Paragraph XI, Letter from Callie A. Videtich, EPA Region 8 to David A. Finley, Administrator, DEQ, dated August 4, 2008

[3] As Ms. Winborn notes, "…MBFP will be allowed to make changes to the plan, and this should be expected as staff gains operational familiarity with the equipment and lean how to more efficiently operate the facility." While I do not disagree that an operational plan such as the SSEM should become more efficient and should incorporate staff experience, such an evolving plan cannot be enforceable and it is therefore not suitable as BACT.

[4] As the EPA has noted in its comments, paragraph XI, Letter from Callie A. Videtich, EPA Region 8 to David A. Finley, Administrator, DEQ, dated August 4, 2008

First, let me repeat that the record contains no specific design details or vendor information as to any of the fugitive components. Vendor data are commonly included as part of permit applications. While all of the required engineering may not have been completed, it is unlikely that none of it has been started. Thus, some engineering data and detail must be available. In any case, the application has to include enough engineering detail in order to justify the assumptions made.

Second, without knowledge of the component (be it a valve, a pump, or whatever), it is incorrect to blindly assign an average emission factor to represent its emissions. As a matter of basic engineering, one should ascertain if the component(s) for which the emission factor(s) was developed bears any resemblance to the component in the project for which the emission is being estimated. For example, does it have the same number of possible fugitive emission points and pathways? Is it the same size? Is the line pressure comparable? In this case MBFP has used "SOCMI[5] Average" emission factors[6] without establishing their applicability in any manner whatsoever. That such emission factors are applicable is an unverifiable assumption. Similarly, in its Decision Document and Permit Analysis DEQ also fails to establish the applicability of the emission factors selected by MBFP as evidenced by no discussion of this issue in the record. The fact that average emission factors may under-represent emissions is well known. For

[5] Synthetic Organic Chemical Manufacturing Industry

[6] I note also that as a technical matter, the support Ms. Winborn provides for use of the average factors actually makes the opposite case. She notes that "[T]he only document on EPA's Emission Inventory Improvement Program (EIIP) website addressing equipment leak emissions was issued in November 1996, and notes in its Chapter 4 (Preferred Method for Estimating Emissions) that "[t]he EPA correlation equation approach is the preferred method when actual screening values are available….[f]or new sources, when no actual screening values are available, average emission factors can be used temporarily to determine fugitive emissions from equipment leaks until specific and/or better data are available." As this guidance makes clear, average emission factors for new sources are only to be used "temporarily" until "specific and/or better" data are available. While specific and/or better data can be gathered in the future after facility operation, that is not the only source of specific and/or better data. For example, data from the vendors of these components are specific and could have been used. It is my experience that all vendors of pumps, valves and other fugitive components conduct tests on their components and therefore have very specific data directly applicable to their products. Additionally, one can get specific and/or better actual emissions data from other plants where such components are in use. It is unlikely that MBFP will be using components that have never been used elsewhere. However, there is nothing in the record to support that MBFP or the DEQ obtained or tried to obtain specific and/or better data before using the average values.

example, in the late 1990s, the state of California acknowledged that use of EPA average emission factors may under represent actual emissions.[7]

Third, as to component counts, Ms. Winborn provides no new basis for the assumed number counts. So, my original criticism stands.

Fourth, Ms. Winborn asserts that the permit conditions for component counts and stream compositions are not necessary because "..permitting agencies typically note the allowable (permitted) VOC and HAP emission rates in the permit, and entrust the permittee with the responsibility to comply with the agreed-upon emission limits." This is contrary to my experience. Especially in a situation as this (as will be discussed in the next section) where the facility is claiming potential emissions below a certain threshold in order to avoid triggering a regulatory program, "entrusting" the permittee with self-compliance is improper for obvious reasons. In fact, why bother with the permit at all?

Fifth, as to the assumption on leak efficiency, my criticism was based on the fact that actual audit data by EPA shows that components leak at far greater rates than assumed. Thus, just because MBFP assumed that leak threshold definitions are 500 ppm for valves and connectors and 2000 ppm for pumps, does not mean that there will not be actual leakers at greater rates. Thus, the question is not whether adequate detail was provided; rather it is one of whether the assumptions in the calculations are justified. Here MBFP has failed to provide sufficient information in order for the public to verify these assumptions.

For all of the reasons above, it is my opinion that the emissions estimates for fugitive components cannot be relied upon.

[7] See page 2/4, Appendix A-1, Summary of Agreements, Key Outstanding Issues, and Proposed Resolutions, California Implementation Guidelines for Estimating Mass Emissions for Fugitive Hydrocarbon Leaks at Petroleum Facilities, February 1999, developed by the California Air Pollution Control Officers Association (CAPCOA) and the California Air Resources Board (CARB).

F. BACT for Fugitive Emissions is Inappropriately Determined

Nothing in Ms. Winborn's report refuted my previous opinion that BACT for the fugitive emissions was improperly determined. In fact, Ms. Winborn continued to assert that the choice of leak thresholds (i.e., 500 ppm for valves and flanges and 2000 ppm for pumps) was based on NESHAPs regulations. As I have discussed earlier, NESHAPS are not BACT. Just because the DEQ wrongly accepted these NESHAP limits as BACT does not make them BACT. Finally, as described by Ms. Winborn, just because MBFP's initial recommendation was higher (i.e., a leak threshold of 10,000 ppm) and later lowered to these NESHAP limits at the behest of the DEQ, does not make the NESHAP limits BACT.

In dismissing consideration of lower emitting technologies like leak less components, Ms. Winborn notes that such technologies were considered and rejected in earlier NSPS (40 CFR Subpart VV) rulemaking. While that may be the case, it is not clear how it therefore follows, as Ms. Winborn asserts, that "…installing leak less equipment is not a potential BACT control option…" Ms. Winborn conflates the NSPS and BACT. I have clearly shown that NSPS and NESHAPs limits set the floor for BACT determinations and are invariably less stringent than BACT. Thus, just because some control option is rejected in a NSPS rulemaking, does not eliminate that control option from being considered in the top-down BACT analysis. Ms. Winborn's assertions in this regard simply reinforce the point that the fugitive BACT analysis was incorrect.

Going further, Ms. Winborn curiously justifies the lack of consideration of leak less technology as follows:

> …However, due to the large number and variety of components necessary for the MBFP facility (more than 4,000), it seems highly unlikely that a leak less valve make/model would be available for all valve and pump types located at the facility. Numerous other problems present themselves when considering this as a possible BACT control option: the cost and time required to obtain an adequate cost estimate to

use in BACT analysis; the questions as to what percentage of the facility will require leak less technology; questions about monitoring frequency, etc….

I draw one conclusion and offer two points in rebuttal of Ms. Winborn's statement.

First, it confirms that the number of actual fugitive components (i.e., more than 4000, as Ms. Winborn asserts) can probably be reasonably estimated from engineering drawings. In fact, the detailed counts of components for each stream, as provided in the application would have been difficult to estimate without sufficient amount of engineering detail. Thus, with the available engineering detail, it should be possible to describe the components in more detail as well, in order to establish the appropriateness of the emission factors used.

Second, just because leak less technology may not be available for all (i.e., each and every) components does not mean that their use should be rejected without regard to their merits for where they are available.

Third, while I agree that it may take additional "cost and time" to conduct the necessary analysis and obtain the cost estimates that are needed in the BACT analysis, these are hardly justifiable reasons for not doing so. I do not know of any BACT definition or guidance that states that otherwise valid control options need not be considered simply because it may take more time and cost to gather the data and do the analysis.

G. The Facility is Not a Minor Source of HAP Emissions

While she provides an account of how the facility HAP PTE emission estimates evolved over time, nothing in Ms. Winborn's report changes my opinion that the facility is likely to be a major source of HAP emissions. She asserts but provides no support for the proposition that MBFP's HAP estimate is "conservative." By accepting MBFP's proposed HAP PTE estimate, DEQ has accepted a number of unsupported assumptions that have been discussed earlier. It is my opinion that, considered together, these assumptions render the MBFP HAP PTE estimate non-conservative.

I would like to reiterate that by desiring to stay below the 10/25 tpy potential emissions thresholds in order to be a minor source, MBFP is subjecting itself to a higher burden, so that its emissions need to be accurately and conservatively estimated. As noted previously, there is a razor thin margin (based on the current methanol PTE of 9.2 tpy) that supposedly makes the proposed facility a minor HAP source. A small increase of just 0.8 tpy of methanol emissions (or a mere 8.7% greater than 9.2 tpy) is all that separates MBFP from being a major source.

Thus, it is imperative to examine the robustness of all of the underlying assumptions that go into MBFP's methanol PTE calculations in order to determine if, in fact, the methanol PTE cannot be greater by a mere 8.7% of what it is calculated. It is my opinion that the various assumptions that underlie MBFP's calculations are far from robust and that MBFP has not carried this burden of robustness in its emission estimate.

As noted in the previous discussion on VOC fugitive emissions (which directly apply here, since the HAPs in question are VOC fugitives) and in my main expert report, those emissions (and therefore the methanol emissions) have been, more likely than not, greatly underestimated. The reasons for this likely underestimation includes: use of unsupported and likely low-bias emission factors, overestimation of assumed control efficiencies, and likely undercount of components.

For this reason, it is clear that MBFP should not, under the MACT standards, be deemed to be a minor source of HAP emissions.

H. Failure to Consider $PM_{2.5}$

That $PM_{2.5}$ should have been independently assessed in all of its aspects (emissions, BACT, modeling, etc.) rather than relying on the so-called PM_{10} surrogate policy was extensively discussed in my main report. Various recent EPA pronouncements unambiguously indicate that EPA has been moving away from the surrogate policy, with good reason – namely that the underlying technical reasons for the surrogate policy no longer exist and have not existed for some time.

Faced with these difficult facts, and in response, Ms. Winborn provides two arguments.

First, she argues that while EPA policy was, in fact, moving away from the surrogate policy, at the time of the issuance of the MBFP permit (i.e., March 4, 2009) EPA policy was apparently not sufficiently clear. But this is a poor excuse and not grounded in any technical basis. The fact is that there is nothing magical about 2009 or 2008 or 2007 with regards to the technical basis for analyzing $PM_{2.5}$ on its own. Technical tools for $PM_{2.5}$ analysis were available throughout the period of relevance for the MBFP permit. That the applicant chose not to apply proper technical analysis merely demonstrates the gamble that was undertaken. In my expert report, I gave examples of other state agencies that required $PM_{2.5}$ analysis well before 2009.

Secondly, faced with an even more detrimental development in the recent EPA Administrative Order in the LG&E Trimble County case (August 2009), Ms. Winborn attempts to argue that on purely technical grounds the PM_{10} surrogate policy still makes sense for MBFP. However, her technical analysis is incorrect and unsupported. As Ms. Winborn notes, the Trimble County Administrative Order provides suggested methods to evaluate the reasonableness of applying the Surrogate Policy to specific cases, including an assessment of the relationship between PM_{10} and $PM_{2.5}$ emissions from the emission unit, and a demonstration that the degree of $PM_{2.5}$ control by the control technology selected in the PM_{10} BACT analysis would be at least as effective as the technology that would have been selected if a BACT analysis specific to $PM_{2.5}$ emissions had been conducted. She then attempts her own reasonableness evaluation.

For natural or syngas-fired combustion turbines, without providing any emissions data or test data, she asserts, relying on EPA AP-42, that all of the PM_{10} emissions are actually $PM_{2.5}$ (or PM_1). In other words she asserts that none of the particulate emissions from combustion turbines fall in the $PM_{2.5}$-PM_{10} range. The Trimble decision, however, makes it clear that one cannot simply rely on AP-42. It notes that…"[I]n developing such correlation, a simple ratio of AP-42 emissions factors or of the results of a single compliance stack test would not appear to be sufficient. Instead, reasonable consideration would be given to whether and how the $PM_{2.5}$:PM_{10}

ratio may vary with source operating conditions, including variations in the fuel rate and in control equipment condition and operation." In effect, Ms. Winborn has argued, based only on AP-42, that there is perfect correlation between PM_{10} and $PM_{2.5}$ for combustion turbines, under all situations. There is no technical basis for such an assumption.

Ms. Winborn's arguments for the other fugitive particulate emission sources (i.e., from coal storage and material handling which will result from a variety of activities, including wind erosion from the outdoor coal stockpile, coal transport via bulldozer, addition to coal stockpile, and temporary road dust emissions from truck transport of coal, etc.) is even more tortured. Again, against the explicit directive not to rely on AP-42 as noted in the Trimble decision, she relies only on AP-42 to argue that there is a constant ("seven times") or almost constant ("seven to ten times") relationship between PM_{10} and $PM_{2.5}$ emissions. This is simply incorrect. The fact is that the PM_{10} to $PM_{2.5}$ emissions from such sources will vary significantly depending on numerous factors such as wind velocity, surface friction factor, degree and manner of wear and friability of the materials, and others. Thus the constancy that Ms. Winborn presumes is simply not true. She then argues that in effect by using PM_{10} as the surrogate, she has used a "conservative" surrogate for $PM_{2.5}$. That too is false. Without independently (i.e., not relying on AP-42) demonstrating that the PM_{10}:$PM_{2.5}$ ratio is constant and large, she cannot claim that it is with any degree of plausibility.

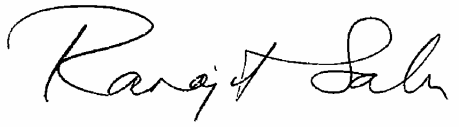
Finally recognizing that Trimble, in fact, admonishes against drawing conclusions simply based on AP-42, Ms. Winborn argues that the "…focus should turn from emission quantification to emission control…" and provides a discussion of $PM_{2.5}$ emissions controls. For combustion turbines, since no particulate emissions controls were accepted, this discussion is moot. For the fugitive emissions sources, she argues that since the only controls proposed are work practices such as watering and the use of wetting agents, the effect of these is the same on PM_{10} and $PM_{2.5}$. But she provides no support for this argument. The fact is that these work practices can provide different degrees of control for different sized particles. While these practices may be the same, the degree to which they are applied (such as amounts, frequency, etc.) may vary depending on whether one is controlling the larger (and harder to entrain) PM_{10} or the smaller (and therefore

easier to entrain) $PM_{2.5}$. It is my opinion that Ms. Winborn does not make a plausible case for the use of PM_{10} controls as a surrogate for $PM_{2.5}$ controls.

Based on the above, it is my opinion that MBFP should independently analyze $PM_{2.5}$ in all of its aspects.

I. Failure To Include Fugitive Particulate Emissions In Dispersion Modeling

Ms. Winborn disagreed with me that fugitive particulate emissions should have been included in dispersion modeling. She claims that they were. I disagree. I was referring explicitly to the modeling of short term (i.e., 24-hour) fugitive emissions. As the permit application itself notes[8] this modeling included "…only mining related point sources (no fugitives)…". Fugitive emissions should have been included.



Dated: September 30, 2009

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[8] See Note 1 to Table 6-1, on page 6-3 of the Permit Application by URS, revised 4/23/08.

CERTIFICATE OF SERVICE

I hereby certify that I have served a true and correct copy of the forgoing *Rebuttal Report of Ranajit Sahu* via electronic mail on this the 30th day of September, 2009 to the following:

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