

Bachman, Tom A.

From: Buening.Hans@epamail.epa.gov
Sent: Tuesday, April 28, 2009 5:31 PM
To: Bachman, Tom A.
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Subject: Kling Paper
Attachments: Hans H Response to Tom Bachmann's questions 4-21-09.doc



Hans H
ise to Tom Bach

Hi Tom - I have attached a document prepared by Hans Hartenstein responding to your emails of 4/15/09 & 4/16/09 on his informal "Thoughts on the Kling Paper" and accompanying calculations that I originally sent to you on 4/9/09. As you will see, Mr. Hartenstein acknowledges the mathematical errors that you cited and agrees with much of what you stated in your emails. The attached document hopefully clarifies those items that you raised and helps provide some insights on how the Kling and Crespi papers might relate to TESCRA at MRYS.

Let me know if you have any questions and thanks for bringing the problems with the previous submittal to our attention.

-Hans

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(See attached file: Hans H Response to Tom Bachmann's questions 4-21-09.doc)

Response to Tom Bachmann's questions forwarded to me by Hans Buenning via e-mails dated 4/15/09 and 4/16/09.

In his e-mail dated 4/15/09 Mr. Bachmann stated: *My calculations indicate Center lignite has an average sodium content of over 4,800 ppm (dry basis) and over 1,600 ppm of potassium (see attached calculations). This is over 4 times more than Mr. Hartenstein calculated. Of course this is an average and the sodium and potassium can be much higher. These values are higher than for the fuels Kling evaluated based on the sodium and potassium content. In addition, Minnkota is a cyclone boiler versus fluidized bed boilers and PC's. I note this for HDSCR. Of course, for a tail-end SCR the sodium and potassium in the flue gas will be much lower.*

In a follow up e-mail dated 4/16/09 Mr. Bachmann stated: *Just to follow up on my previous email, attached is a corrected version of Mr. Hartenstein's spreadsheet. The corrections are shown in green. He made 2 errors. First, when converting from a wet basis to dry basis he divided by the moisture content instead of one minus the moisture content. Second, when converting from percent Na₂O and K₂O for the Center lignite he was a factor of 10 off. 1.33% K₂O should be 13,300 ppm instead of 1,330 ppm and 4.4% Na₂O should be 44,000 ppm instead of 4,400 ppm. The difference in my calculations is that I converted Na₂O to Na and K₂O to K plus I used 35% moisture instead of 37.13%. I would be interested in any changes to his statements in the "Thoughts on Kling paper" document.*

I acknowledge both errors that Mr. Bachmann identified in my calculations as outlined in his follow up e-mail and agree with his calculations. I also do agree with his statement that the fuels Kling evaluated may have been lower in total sodium and potassium than Center lignite, which, however, would only be relevant for a HDSCR application. In a TESCR application at Minnkota the sodium and potassium containing concentration of particles in the relevant size range of less than 0.1 microns must be expected to be one to two orders of magnitude lower than what Kling exposed the catalyst to in his HDSCR studies. This argument will be supported with data shown below.

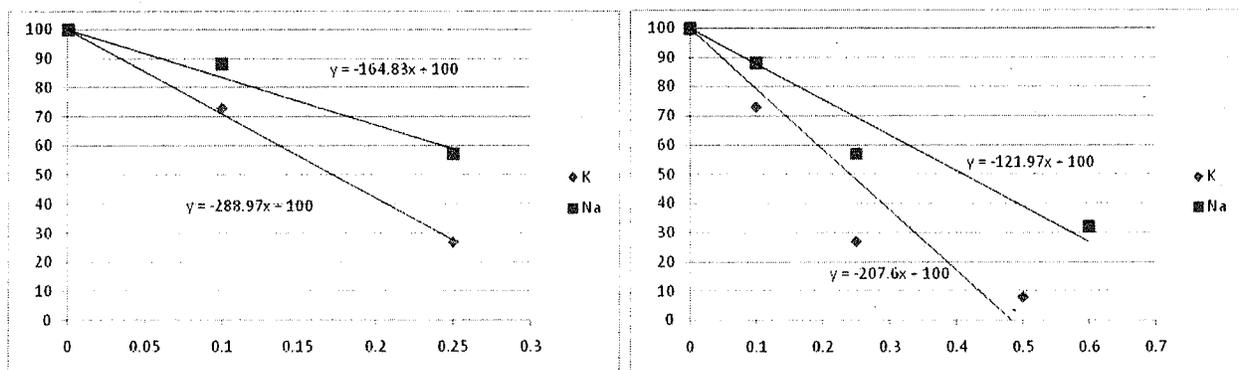
Mr. Bachmann further stated in his initial e-mail dated 4/15/09 that: *Comparison of different flue gases on a ppm/MMBtu basis is, in my opinion, not the best comparison because the amount of flue gas generated by various fuels is different on a MMBTU basis. Most researchers report the deactivation contaminants on a concentration basis (i.e. ppm or mg/Nm³)).*

I absolutely agree with this statement and therefore all following comments are based on these units only. Not as an excuse for my error but simply as an explanation as to why I introduced the ppm/MMBtu unit may serve the fact that the Kling paper only reports potassium and sodium concentrations of particles with an aerodynamic diameter of less than 0.1 microns in flue gas concentration units in mg/Nm³ (Table 5) rather than for all particle sizes. I merely – and certainly incorrectly – tried to quickly calculate some total sodium and potassium concentrations for Center lignite, which, however as we all agree upon, are not really relevant anyway. Mr. Bachmann confirms this by stating in his e-mail dated 4/15/09 that: *In item No. 6, Mr. Hartenstein talks about the form of the sodium and potassium and the effect on deactivation of the SCR. I agree that the form is important. He attributes the ability to inject SBS to control SO₃ to the form of the sodium injected. However, if you review the URS slide presentation, it*

Based on Figure 4 of Crespi, the slopes of the catalyst deactivations due to sodium and potassium up to a V molar ratio of about 0.25 differ by a factor of approximately 1.76. Up to a V molar ratio of about 0.5 the slopes differ by a factor of about 1.70 and past a V molar ratio of 0.5 the slopes flatten out to almost 0 past a V molar ratio of 1. In the following, the determination of these factors will be detailed.

In his e-mail dated 4/16/09 Mr. Bachmann commented: *There is no need to send the Crespi paper as I had requested if Mr. Hartenstein is referring to the paper titled "The influence of biomass burning in the design of an SCR installation". Mr. Hartenstein indicated that potassium is 1.7 times more potent as a catalyst deactivating chemical than sodium based on the Crespi paper. I believe he is referring to Figure 6 in the paper. If so, my interpretation of that figure is only that the molecular weight of potassium is 1.7 times that of sodium (39.1 versus 23.0). The x-axis indicates weight% K equivalent, not potency. The only way you can get a weight percent equivalent is using the molecular weights. If Crespi was trying to indicate a deactivation potency, I believe the formula should be $K + Na/1.7$; not, $K+1.7*Na$, since potassium is the more potent chemical.*

Even though Mr. Bachmann's statement concerning the representations on Figure 6 is absolutely correct, I based my statement of Figure 4. Coincidentally, the number 1.7 appears in both cases. Extracted from Figure 4 data, the two graphs below show the approximations of the linear deactivations caused by sodium and potassium between V molar ratios of 0 and 0.25 as well as 0 and 0.5 with the equations of the best fit straight lines displayed on the graphs. As can easily be calculated the ratios of the slopes are in both cases approximately 1.7.



Based on this analysis of the data presented by Crespi, I stated that Crespi established a deactivation potency factor between potassium and sodium of approximately 1.7. In other words, potassium is 1.7 times more potent in deactivating catalyst – at least up to a V molar ratio of about 0.5 – than sodium. Therefore, it is correctly represented as $1.7K + Na$ or $K + Na/1.7$.

However, the most relevant question is really what is the concentration of sodium and potassium containing particles with an aerodynamic diameter of less than 0.1 microns in case of Minnkota downstream of their scrubber and how do these concentrations compare to the concentrations found by Kling.

don't result in any significant changes of my previous statements concerning the Kling paper and the relevance of its data for this case with respect to the technical feasibility of TESCR for Minnkota's units burning Center lignite.

Table 3 of the Kling paper represents that in the lab standard SCR catalyst deactivated 15% over a period of 1,488 hours in a HDSCR of a PC-fired 400 MW unit burning Uppsala peat + 15% wood, which is a far cry from the 40% claimed in Minnkota's Supplement for Consideration. It also needs to be born in mind that Kling points out that the deactivation of the short catalyst samples tested by Kling in the lab cannot directly be compared with the deactivation of full length catalyst in a full scale SCR reactor. Kling states that since the poisonous components accumulate at the inlet of the catalyst, the catalyst deactivation of the short samples used in the lab will be higher than what would be experienced in a full scale SCR burning the same fuel.

As outlined above, a TESCR after a Center lignite fired Minnkota unit would see even in a absolutely unrealistic worst case scenario a concentration of at least 30 times (more likely 60 – 100 times) less of the relevant combined alkali concentration in particles smaller than 0.1 microns downstream of the scrubber. A factor of 30 would result in an expected catalyst life of at least 45,000 hours. Crespi reports typical TESCR deactivation rates using biofuels at 40% after 50,000 hours – see figure below.

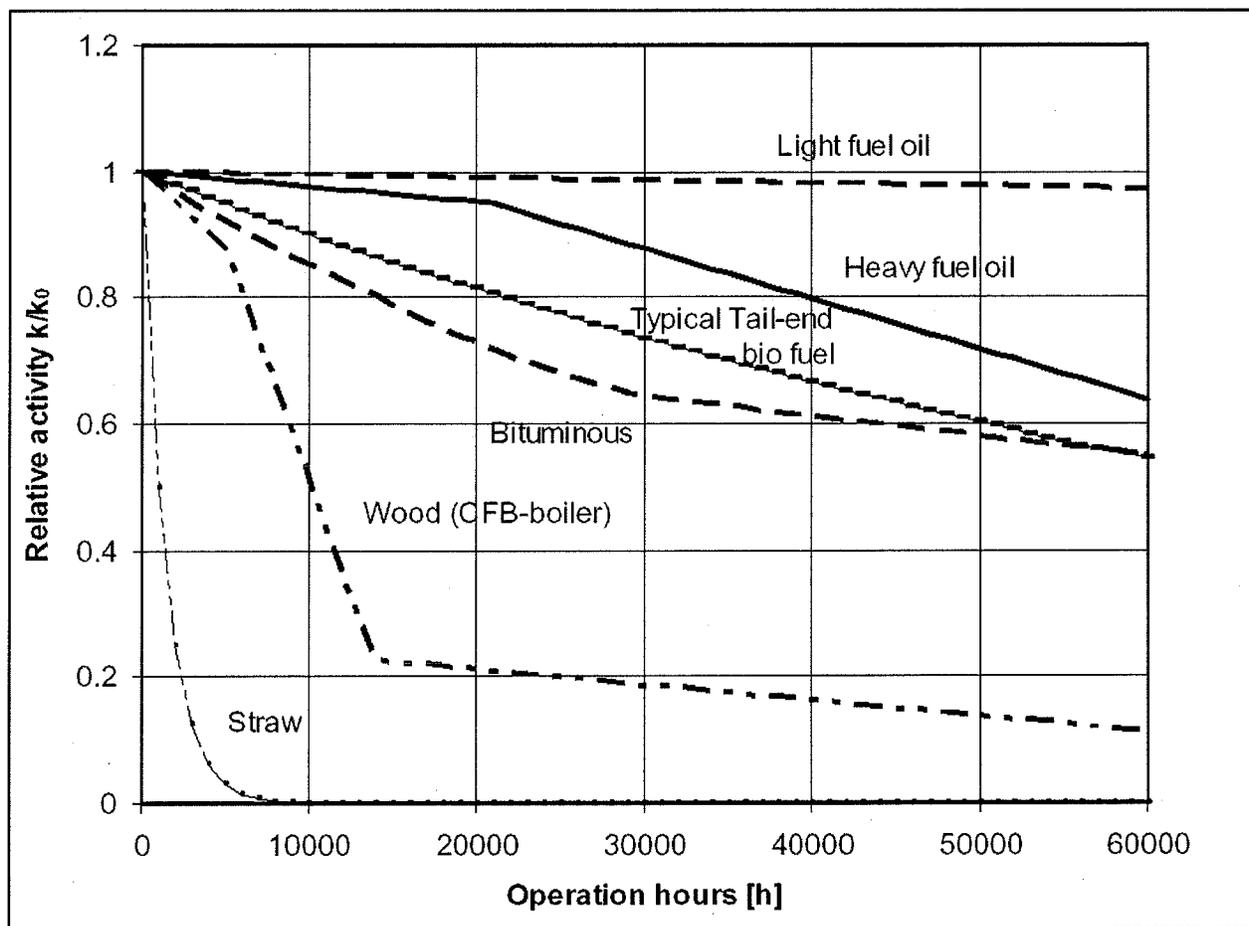


Figure 3: typical catalyst deactivation ratios for different fuel and plant configurations [6]