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Dear Dr Eckerman,

Thank you for your email and the thoughts regarding peer review. First let me repeat that I have had some interesting experiences with peer review, two of which I shared with you and the audience at the meeting. The Fallujah paper was sent to The Lancet which didn't even send it out to referees, so no peer review there. The other was the uranium photoelectron paper which the Royal Society Interface editor, William Bonfield rejected against the advice of 4 referees; of course, it would have been embarrassing to the Royal Society since their committee ruled that uranium from weapons was not a problem.

But to return to the issue of the argument about breast cancer and the young man. This man came to the meeting fully primed to attack my argument about Chernobyl effects. He had printed out his graphs and was all set to argue that I had cherry picked my data to achieve my conclusions. I since recall that I used the 84/85 years to get as close to before Chernobyl as possible, since there was a rising background which might have affected the result. After all, you can take any cancer before and after Chernobyl on a rising slope and argue for an increase due to Chernobyl. But the problem is that the inland counties rates all fall after Chernobyl. I was concerned about his accusation and went back to look and see what would happen if, as he suggested to the audience, I had used more years before. The result is (as I might expect) the effect gets worse. The results are in the table below.

The point about screening is relevant, but I can't see how there can be a decrease in breast cancer over the period inland in all 3 counties driven by screening. The point I wish to make about all this is that this quick look at the data supports my thesis, which was based on earlier work in the Irish Sea and near sea coasts close to nuclear power stations, that breast cancer increases in populations living in such areas, and that the trend with distance from the coast can be explained by sea to land transfer and inhalation of radioactive particles. This is a serious matter in Sweden since the coast here is highly contaminated. I talked with Timo Hakkulinen in Helsinki, he says that they have found the same coastal effect and can't explain it: they didn't publish their findings. Everyone is frightened of the power of the nuclear industry: look what happened to Tondel! The same thing happened to Jean Francois Viel in the late 90s after he found the child leukemias near la Hague. You doctors for the environment should be on our side, not attacking us. People are dying from this and no one is looking. What happens is some smart young

nuclear physicist is sent along to my presentations primed to attack my work and make me look silly. It seems the joke is on him.

Anyway, what's needed is a proper study, using small areas, and that needs more resources than I currently have. What we need is significant funding to go after this effect, like 3 years to measure radioisotopes and examine the municipal data, about 200,000 Euros what with the measurements and staff.

We are following up the Fallujah epidemiology with measurements, but that also costs money.

Best regards

Chris

P.s. Gotland shows an increase in my earlier analysis and a decrease in my new one. The populations of Gotland are small so I would imagine that is the reason. The reason I didn't use Gotland at first is that it wasn't on the map I downloaded from the Internet so I didn't know where it was. I since see it is an island.

COASTAL	84-85 old	82-85 new	88-91	%incr old	%incr new
Stockholm	114	115	131	+15	+16
Blekinge	103	98	132	+28	+34
Kalmar	103	104	112	+9	+8
Uppsala	110	104	125	+13	+21
Gavleborg	80	84	92	+15	+8
Vasternorrla	99	93	115	+16	+22
Skane	114	108	125	+13	+17
Hallands	106	103	118	+19	+15
Vastergottla	105	103	126	+21	+23
<b>Mean Coastal</b>				<b>+16.6</b>	<b>+18.2</b>
INLAND					
Varmlands	93	97	93	-1	-4
Dalarnas	114	111	101	-11	-10
Jamtlands	101	103	90	-11	-13
<b>Mean Inland</b>				<b>-7.6</b>	<b>-9.0</b>
Gotland	76	91	80	+4	-11