

Climos Response to the Canadian Review of the London Convention/London Protocol Scientific Group Statement of Concern on Ocean Fertilization

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Introduction

In preparation for the May 19-23 2008 meeting of the Scientific Group of the London Convention, Canada has submitted a review of Ocean Fertilization titled, "*Background and Literature Review Addressing Main Elements in the LC/LP Scientific Groups' Statement of Concern on Ocean Fertilization.*" Canada reviews the benefits and impacts of ocean fertilization, including ocean iron fertilization (OIF), macronutrient fertilization, and ocean pumping. The Canadian Review also discusses the appropriateness of using ocean fertilization as carbon mitigation technique that can be incorporated into global carbon markets. Finally, the Review provides some recommendations on how ocean fertilization could be regulated under the aims of the London Convention.

The Canadian Review covers multiple types of ocean fertilization. We believe that it is critical to recognize the differences between the three "ocean fertilization" techniques discussed in the Review and feel that each type of fertilization should be considered separately. OIF, for example, is fundamentally different (and stimulates different biogeochemical processes) than either macronutrient addition (e.g. adding nitrate, phosphate or urea) or artificial ocean mixing (e.g. vertical pipes pumping deep water to the surface). The scientific research regarding OIF is much more extensive than research into other techniques. For example, 6 book-length volumes of peer-reviewed scientific publications specific to individual iron enrichment experiments were published in the journal *Deep Sea Research* in addition to hundreds of peer-reviewed articles in the other scientific literature. This rich background allows more certainty in designing experiments that can inform policy decisions on OIF. Other methods have a far less extensive experimental basis upon which to make a decision to proceed, and we urge the Scientific Working Group to consider them separately. At the same time, we are generally supportive of scientifically grounded efforts to demonstrate new and potentially viable mitigation technologies through perturbation experiments (in the tradition of OIF for example) and other techniques, so long as there is little risk that the experiments themselves will be harmful.

We believe that the Canadian Review raises many important scientific issues that need to be discussed, and appreciate the thoughtful approach to the material, yet note that it: 1) does not consider some of the most recent research related to the efficiency of carbon sequestration from natural and stimulated blooms; 2) sometimes extrapolates biogeochemical and ecological data and model results concerning the impact of OIF beyond their appropriate scientific reliability or scalability; and 3) suggests several potential deleterious effects for which there is no evidence in the literature. The Canadian Review also discusses the policy implications of using OIF as a carbon mitigation technique included under the various emerging carbon markets, and focuses particular attention on the merits of participation by the private sector. The Review provides a range of policy options. We have submitted separate documents concerning the participation of the private sector in OIF demonstrations and concerning legal issues. This response will, however, provide specific answers to the questions and issues included in the Canadian Review.

Our perspective on Ocean Iron Fertilization

There is increasing recognition of the accelerating problem of climate change/global change impacts and the need to investigate mitigation options. Specifically, the interest in Ocean Iron Fertilization (OIF) as one such option arises from the culmination of decades of international oceanographic research into the coupled biogeochemistry of the oceans, earth and atmosphere that suggest iron plays a central role in the primary productivity of the ocean, and is essential to the ocean's role as a carbon sink. Several scientific insights have driven this understanding: First, the supply of iron has been recognized as a primary limiting factor to the efficiency of the 'biologic pump' in much of the world ocean [Martin and Fitzwater, 1988]. Second, recognition that the ocean is a major sink for anthropogenic atmospheric carbon dioxide -- 50 times the size of the atmospheric reservoir, and 1000 times the size of total historical anthropogenic CO₂ emissions ([Falkowski et al., 2000],[Canadell et al., 2007; Raupach et al., 2007]). Third, the iron supply during glacial time periods was an order of magnitude greater than during interglacials [Petit et al., 1999], and this enhanced iron supply resulted in increased biologic productivity in the ocean [Winckler et al., 2008]. Finally, synthesis of decades of oceanographic data together with computer models of increasing complexity suggests that enhanced iron fertilization (either natural or anthropogenic) could remove 30-40 ppm CO₂ from the atmosphere on timescales of hundreds to thousands of years [Aumont and Bopp, 2006; Cassar et al., 2007]. Compared with the current scale of accumulated human CO₂ emissions (100+ ppm), this potential CO₂ removal is a significant fraction of the total CO₂ reductions needed to stabilize climate change.

Scientific understanding of the potential of OIF comes from a substantial peer-reviewed literature reporting on 12 publicly-funded open ocean experiments, as well as a much larger body of work on the "biological pump", which has been the subject of research by multiple decade-long international programs such as VERTEX (e.g. [Knauer et al., 1990]), the Joint Global Ocean Flux Study (JGOFS) [Ducklow et al., 2001], [Karl et al., 2001], and the recent VERTIGO experiments that studied carbon flux into the "twilight zone" of the ocean below the euphotic zone (e.g. [Buesseler et al., 2007c]). Studies of the response of the ocean to the 2-20x dust flux to the oceans during glacial periods and the associated decrease in atmospheric CO₂ provide useful inference about the impact of OIF could have at larger spatial scales. Finally, a growing body of work on model simulations of OIF also provide scenarios of the response to the ocean to larger-scale OIF, and in the last few years these models have achieved significant advances in explicitly modeling the iron cycle coupled with ocean circulation, biogeochemistry and the resulting ecosystem response.

The question of *scale* in both space and time is very important in discussing the potential benefits and impacts of OIF. Many of the negative concerns suggested by the Canadian Review would only be realistic threats if OIF were conducted on spatial scales equivalent to entire ocean basins and on time scales of decades to centuries. At this time, as suggested by the scientific community, we are proposing larger demonstration experiment(s), not widespread immediate commercialization of OIF.

While it is true that OIF produces short-term *ecological* changes that are similar to those that occur during natural phytoplankton blooms, we will show that there is no *a priori* evidence from literature that OIF experiments projects might result in widespread or long-term *deleterious* ecological changes. The preponderance of evidence suggests that OIF experimentation and study, even if done at moderate scales of 200 x 200 km, will not harm ecosystems [IOC, 2008]. The detailed effects of much larger scale OIF on ecosystems cannot be deduced from the experiments that have taken place to date and therefore cannot be assumed to be positive or negative [SCOR, 2008]. We believe that it is prudent to undertake a period of demonstration and experimentation to determine the efficiency of carbon export and its impact.

We believe commercial involvement is appropriate for ocean iron fertilization. First, it is traditional that research needed to determine whether an idea has commercial potential is done in partnership with the private sector. Second, substantial resources are necessary to move from the existing basic research to development or demonstration-scale experimentation. Commercial involvement can increase the pace of research into OIF as a potential tool to mitigate accelerating climate change.

Concerns have been raised about ensuring the scientific integrity of the development research if commercial firms are involved in funding OIF. Climos is sensitive to this issue. We have proposed a number of mechanisms to ensure that a proposed commercially-funded development cruise will operate at the highest level of scientific integrity with international scientific design of the cruise, international leadership and participation for the cruise, openly published data, results, carbon verification methodology¹, and open scientific review of the cruise results. These mechanisms include suggested elements of a Code of Conduct [Climos, 2007] for the research; scientific workshops in advance of the experiment to design the sampling strategy and identify the measurements and modeling necessary to determine the efficacy and impact of OIF; review of the sampling strategy by the international science community; and review of the results of the experiment by the international science community. We believe that this activity can be a model for the way that public and private sectors can maximize their ability to work together to explore solutions to the GHG problem.

Discussion of Concerns Raised in the Canadian Review

OIF studies have not demonstrated conclusive POC export

The Canadian Review of OIF agrees that adding iron to high nutrient-low chlorophyll (HNLC) regions will stimulate a phytoplankton bloom. These are regions of the open ocean far from land that have sufficient macronutrients (nitrate, phosphate, silicate) to support additional phytoplankton growth, but in which these excess nutrients are not used. But, the Canadian Review questions whether the iron addition actually results in any additional carbon export to the deep ocean: “*However, whether the deliberate addition of iron to HNLC regions can result in significant carbon dioxide storage remains uncertain (Chisholm et al. 2001). Estimates of the potential global sequestration of carbon dioxide through iron fertilization have been decreasing for 15 years as we learn more from the fertilization experiments. . .*” We believe that this statement does not take into account the most recent scientific evidence from OIF experiments, observation of natural iron fertilization blooms, and observation of natural non-iron stimulated blooms. The Chisholm et al. [2001] policy forum article in Science magazine predates eight of the twelve OIF experiments. A more recent summary of all twelve OIF experiments found that an increase in carbon export was observed in half of the experiments [Boyd et al., 2007]. Second, as noted by the Canadian Review, as well as by peer-reviewed summaries of the experiments ([de Baar et al., 2005], [Boyd et al., 2007]), none of these experiments were specifically designed to measure carbon export, and it is likely that most suffered from experimental designs that prevented accurate observation of carbon export. Third, results from EIFEX (the most recent, and largest-scale, experiment [Smetacek et al., 2008]), observations of natural blooms [Buesseler et al., 2007b], [Blain et al., 2007b] as well as recent modeling results ([Aumont and Bopp, 2006], [Jin et

¹ A *methodology* is a detailed description of a carbon reduction project, that includes measurement and monitoring techniques of real carbon reductions, proof of additionality, permanence, and other measures of carbon quality. It also includes consideration of environmental benefits or impacts.

al., 2008]) all point towards more significant sequestration than has previously been described. In fact, estimates of carbon sequestration from natural and OIF blooms have been increasing for the past five years.

Two primary experimental design factors should be extended in future experiments: size and duration. With regard to size, all but the EIFEX experiment fertilized an area less than 15 x 15 km. While this area sounds large by terrestrial standards, it is extremely small for the ocean and the fertilized patches underwent so much mixing with waters from outside the patch that it was very difficult for scientists to determine whether they were in or out of the patch when they took their measurements (e.g. [Law *et al.*, 2006]). “*In general, the variability and patch dilution interfere with sampling, for example at any given day of any experiment nobody can guarantee the true core (SF₆ maximum) of the patch was sampled.*” [de Baar *et al.*, 2005], and thus, the export measurements represented a very diluted export flux. The next generation of OIF experiments designed to measure carbon export should be a size similar to the scale length of mixing for the fertilized region. This length is approximately 100 to 200 km, the dimension of mesoscale eddy circulation. Conducting OIF experiments on this scale will greatly improve the likelihood that measurements in the patch are not diluted with material outside the patch and will increase the accuracy of carbon sequestration measurements.

With regard to the duration of OIF experiments, several authors have called attention to the short observation phase of the measurements in contrast to the time over which blooms develop and move into an export phase. A comparison of two fertilization experiments in the Atlantic sector of the Southern Ocean, EisenEX [Gervais *et al.*, 2002] and EIFEX [Smetacek *et al.*, 2008] (Fig. 1) shows this very clearly:

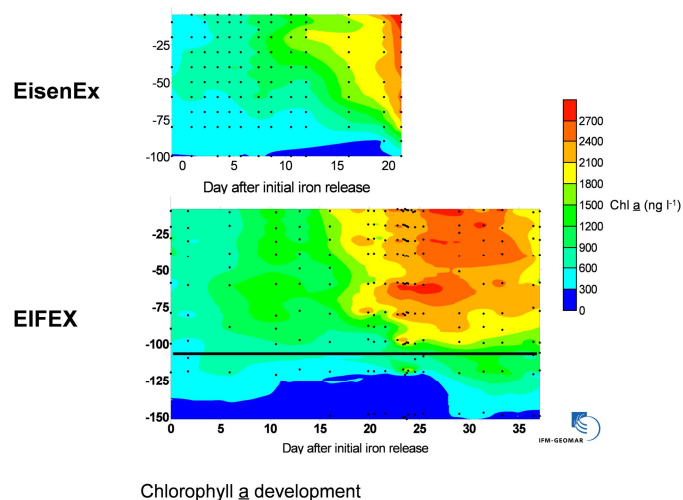


Fig. 1. Temporal evolution of Chlorophyll a in surface water during EisenEX (top panel, [Gervais *et al.*, 2002]) and EIFEX (lower panel). Vertical axis is the depth in the water column, the small black dots indicate the depths of samples included in the analysis. They are arranged by the day of measurement after the initial iron release. Both experiments show a similar development of chlorophyll in the bloom and clearly show that the EisenEX bloom experiment ended before the bloom chlorophyll was removed from the water column [Smetacek *et al.*, 2008].

The EisenEx bloom had barely entered the high chlorophyll concentration phase when the experiment ended. Thus, it is unlikely that the experiment would have measured as much export as EIFEX, which remained on station for 37 days and observed nearly 50% of the bloom biomass being exported below 1000m [Smetacek *et al.*, 2008]. Future OIF experiments would need to remain on station for the entire bloom cycle (45+ days).

Two other lines of scientific evidence suggest that OIF could provide significant potential for carbon export: 1) observations of natural iron fertilization events and non-fertilized background phytoplankton blooms, and 2) results from the most recent generation of models that include fully coupled ocean circulation and biogeochemical/ecological components. Recent measurements of a natural bloom over the Kerguelen Plateau in the southern Indian Ocean showed the highest carbon-to-iron export efficiencies ever observed [Blain *et al.*, 2007b]. At this location sediments containing iron from the 500m deep plateau were suspended by deep mixing and entrained into near surface waters. The authors cautioned that artificial OIF would be unlikely to achieve such high efficiencies, but this event highlights the potential efficacy of the biological pump.

Recent measurements of carbon export from naturally occurring seasonal phytoplankton blooms also suggest that the biological pump is much more efficient than previous observations. The VERTIGO study focused on processes occurring in the “twilight zone”, below the sunlit mixed layer, and how they affect carbon transport to the deeper ocean [Buesseler *et al.*, 2007b]. VERTIGO used the latest equipment and techniques, including neutrally buoyant sediment traps (NBSTs) to look at the fate of carbon below the surface layer. Multiple, replicate NSBT deployments for three different depths below the mixed layer provide error estimates, and suggest that export to the deep ocean (below 500 m) can be 2 to 5 times greater than the 10% used as a ‘rule of thumb’ (e.g. [Martin *et al.*, 1987]) for so long (Fig. 2).

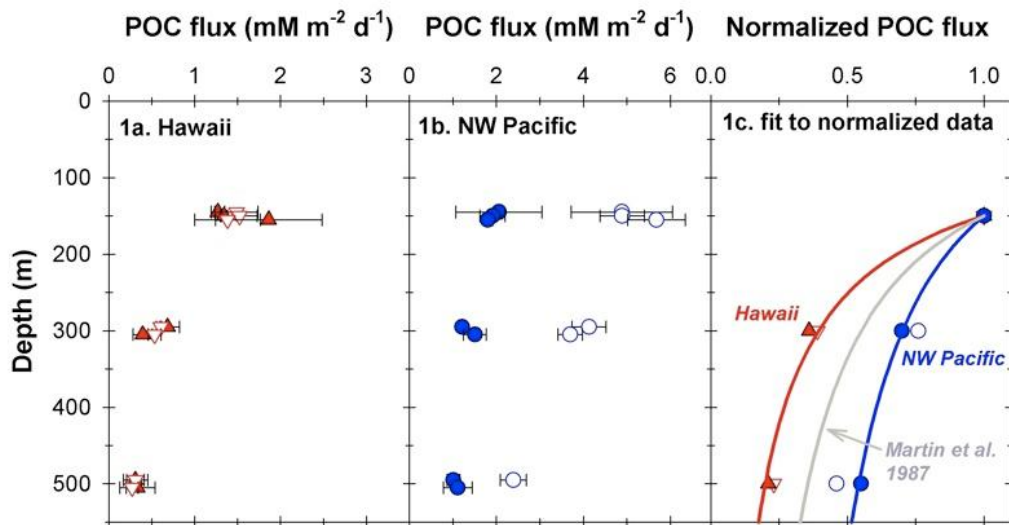


Fig. 2. POC flux versus depth at ALOHA station near Hawaii (22° 45' N, 158° W) and K2 in the northwest Pacific (47° N 160° E). K2 was the location of the SEEDS OIF experiment. (A) POC flux at ALOHA (triangles) and K2 (circles) with open and solid symbols for deployments 1 and 2, respectively. (B) Same data normalized to 150 m POC flux and compared with [Martin *et al.*, 1987]. (4) (dashed line). For each depth, up to three independent neutrally buoyant sediment traps (NBSTs) were deployed from the same launch site, and the POC fluxes are shown (A) for each NBST, with a slight vertical offset, as the mean and standard deviation of replicate POC measurements (n from 2 to 4). Fits to normalized data (B) used a power function of the form $F/F_{150} = (z/150)^{-b}$, where z is the depth of the trap, F₁₅₀ is the POC flux at the 150-m reference depth, and b describes the rate of flux attenuation. [Buesseler *et al.*, 2007b]

In the low productivity central gyre of the Pacific near the “ALOHA” station that is the location of the Hawaii Ocean Time series (HOT) , approximately 20% of the new production that left the mixed layer passed through the 500m

depth level. At a moderately productive location in the northwest Pacific (K2, 47° N 160° E, the location of the SEEDS OIF experiments), nearly 50% of the new production was transported to at least 500m [Buesseler *et al.*, 2007b]. Time series measurements at the K2 location [Honda *et al.*, 2006] show that the VERTIGO experiments took place after the major phase of spring bloom export at K2. These experiments are re-shaping thinking about carbon export, and there is no reason to believe that carefully designed OIF experiments could not achieve similar results.

Results from the latest generation of oceanographic models also suggest that OIF would be an effective means of carbon sequestration. Two separate analyses using fully coupled ocean circulation and biogeochemical/ecological models show both significant total carbon export and high atmospheric uptake efficiencies. In a 100 year simulation of global iron fertilization, [Aumont and Bopp, 2006] found an initial sharp increase in total carbon export from 8.0 GtC/yr in the baseline case to 11.5 GtC/yr in the fertilized case. As surface nutrient supplies became depleted in their model, global carbon export was reduced to 9.8 GtC/yr at the end of 100 years, however both carbon export and primary biological productivity were enhanced over the un-fertilized case. Jin *et al.* [Jin *et al.*, 2008] modeled a more realistic moderate to large scale scenario for the Tropical and Northern Pacific over 10 years, and found a 0.34 GtC/yr increase in carbon export. These modeling studies do not incorporate results from the 12 OIF experiments, but rather provide an independent estimate of the carbon sequestration potential of large-scale and continuous OIF.

Potential Impacts to the Marine Environment

The Canadian Review addresses each of the concerns raised by the Scientific Group in their Statement of Concern [Canada, 2008]. We will also address each of these concerns.

1. Concerns related to the estimated amounts and potential impacts of iron and other materials that may be released with the iron

The Canadian Review suggests that the *iron utilization efficiency* (the ratio of carbon exported to iron supplied) is “crucial to modeling the potential of iron fertilization for carbon sequestration purposes”, and notes “the lack of a reliable method for making this determination.” While we agree that this ratio is important for building accurate *models*, it is only essential for OIF as a carbon mitigation technique if the amount of carbon is not going to be measured, but instead inferred by modeling from the amount of iron used. We do not believe that this is an appropriate method of determining the carbon sequestered. Precise measures of iron are not directly related to carbon sequestered because an oversupply of iron would not generate a more vigorous response. Beyond a certain threshold, excess iron supplies will precipitate out of the system. Also, because the iron sulfate used to stimulate the blooms is relatively inexpensive, the amount of iron is also not a key factor in the *cost effectiveness* of the technique. (Nonetheless, we would certainly want to optimize the amount of iron sulfate used for both environmental and commercial reasons.)

An important result from Jin *et al.* [2008] is the examination of *atmospheric uptake efficiency* of carbon dioxide, which is the ratio of atmospheric CO₂ absorbed by the ocean to the amount of carbon exported by the biological pump to depths at which it will not re-equilibrate with the atmosphere for long periods of time. We believe this is an important measure of the efficiency of OIF as a carbon sequestration technique, because the goal of a carbon sequestration technique is to cost effectively remove CO₂ from the atmosphere. Atmospheric uptake efficiency is entirely independent of the iron utilization ratio. Jin *et al.* [2008] found atmospheric uptake efficiencies of 0.75 – 0.93, which were much higher than earlier and simpler models (e.g., [Gnanadesikan *et al.*, 2003]). Atmospheric uptake efficiency will need to be quantified in an OIF methodology and validated by OIF experiment(s).

The expected *iron utilization efficiency* is important to understand how much iron is necessary to stimulate a phytoplankton bloom in HNLC waters, although this ratio is not relevant for *measuring* carbon export. In all prior OIF experiments, iron sulfate was used as the delivery compound for iron because it is environmentally benign (as noted by the Canadian Review), and because it is more water-soluble than other common iron-containing materials like hematite dust and thus results in a higher iron utilization. The success of the OIF experiments in stimulating blooms with iron sulfate indicates that there is no need for a patented iron slurry or nanoparticles and we do not propose to use such materials for experiment.

Regardless of the material used, it should be evaluated within an environmental impact assessment for the project as part of the permit application. A preliminary indication of the potential impact of iron sulfate fertilization to achieve a 5 nmol/kg concentration (the concentration target for several of the OIF research experiments was 4 nmol/kg) is included in Table 1. Note that the concentrations all of the trace element contaminants in commercially available iron sulfate at the target concentrations are *far* below their natural concentrations in seawater.

Table 1. Trace metal concentrations in commercial grade iron sulfate and their resultant concentration upon delivery into the ocean (e.g. 5 nmol FeSO₄-H₂O) compared to the background trace metal concentrations in seawater. [NOAA SQuIRTS 1999]

Element	Maximum concentration in Fe(SO ₄) (ppm)	Concentration in 5 nmol FeSO ₄ -H ₂ O solution (mg/l)	Concentration in seawater (mg/l)	Percent concentration in 5 nmol FeSO ₄ -H ₂ O relative to seawater
As	1	0.000001	2.60	0.000%
Cd	2	0.000002	0.11	0.002%
Cr	20	0.000017	0.20	0.009%
Cu	17	0.000014	0.90	0.002%
Pb	17	0.000014	0.03	0.047%
Mg	9600	0.008156	1290000.00	0.000%
Mn	2700	0.002294	0.40	0.574%
Ni	85	0.000072	6.60	0.001%
Zn	2000	0.001699	5.00	0.034%

2. The potential impacts of gases that may be produced by the expected phytoplankton blooms or by bacteria decomposing the dead phytoplankton

Several biogenic gases are associated with phytoplankton blooms, especially dimethyl sulfide (DMS), nitrous oxide (N₂O) and methane (CH₄). Any OIF experiment should consider the potential effects of biologic activity producing “radiatively active” gases. We agree with the Canada Review that the extent and severity of these effects are often highly dependent on the location of the fertilized patch. OIF experiments should be carried out only in areas where these effects are expected to be small. Furthermore, any OIF experiment should carefully measure the generation of N₂O and CH₄ gases and subtract their effects from the total carbon credits claimed.

Dimethyl sulfide (DMS). The Canada Review mentions that OIF could alter the production of dimethyl sulfide (DMS) gas from the fertilized patch. DMS has been shown to be generated from particulate dimethylsulphoniopropionate (DMSPp; an algal precursor of DMS) from phytoplankton blooms in laboratory and mesocosm experiments (e.g. [Nguyen et al., 1988], [Levasseur et al., 1996]). DMS is not a greenhouse gas, but it is a radiatively active gas: Both theory [Charlson et al., 1987] and measurements [Bates et al., 1987] call attention to the relationship between DMS and marine sulfate aerosols and cloud condensation nuclei. Both theory and observation have associated DMS with increased cloud cover that has a net cooling effect on the surface below. Lawrence [Lawrence, 1993] reviewed the atmospheric chemistry literature associated with the connection, highlighted the strength of the feedback and calculated on the basis of empirical evidence that it could offset as much as 20% of the thermal perturbation due to CO₂. We are not aware of any dispute in the literature that DMS is associated with cloud formation and that its effect would be to cool the surface.

DMS was measured during several OIF experiments: During the SOIREE Experiment in the Southern Ocean [Boyd et al., 2000] observed an increase in DMS associated with the early prymnesiophyte-dominated portion of the bloom. [Turner et al., 2004] reviewed measurements of DMS in IronEx I and II (Equatorial Pacific), SOIREE, and EisenEx (Atlantic sector of Southern Ocean). DMS inside the OIF patches increased in all four the experiments. [Wingenter et al., 2004a] found that DMS increased in SOFEX N (Pacific sector of Southern Ocean), and a reduction in DMS was recorded in SERIES (northeast Pacific) and no change was observed in SEEDS I (northwest Pacific). While DMS decreased in the northwest Pacific SERIES experiment, it was only after it had *increased* substantially in response to the initial phase of the bloom [Levasseur et al., 2006]:

“First, DMS concentrations tended to increase more rapidly inside the patch during the initial nanoplankton bloom, leading to DMS concentrations ca. 2 times higher inside the patch than outside on day 6. Second, DMS concentrations became consistently lower inside the patch (often below our limit of quantification of 0.03 nmol L⁻¹) than outside (ca. 6 nmol L⁻¹) during the peak of the diatom bloom.” (p.2353)

We have found no published record of the SEEDS I DMS results although they were mentioned in the summary of [Takeda and Tsuda, 2005] who explained that because prymnesiophytes were not prominent in the bloom it is expected that DMS would be low. In summary, the experimental data from OIF experiments suggest that it is most likely that larger experiments or deployment would result in DMSp formation at some stage(s) of the blooms and that the DMSp would result in enhanced DMS.

It is also important to note that the lifetime of DMS in the atmosphere is short -- roughly 2 weeks during/after a bloom ([Nguyen et al., 1988], [Bates et al., 1992], [Berresheim et al., 1998]), and therefore insignificant from a 100 year GWP (Global Warming Potential) point of view. Since a region can probably only be fertilized successfully once a

year, the impact of DMS would be very short-lived. There is no evidence in the literature that this would deleterious. In fact, it has been suggested as a negative feedback contributing to cooling.

While it is difficult to determine the impact of larger OIF commercialization on DMS, important evidence of the relationship between DMS and climate comes from the study of ice cores. [Wolff *et al.*, 2006] found little change in DMS from the circum-Antarctic region preserved in ice of glacial age at Dome C in Antarctica in spite of seeing substantial changes in iron dust. The iron flux has been associated with substantial productivity changes in the Southern Ocean (e.g. [Cassar *et al.*, 2007], [Anderson *et al.*, 1998]). Thus the evidence that is currently available suggests that elevated DMS concentrations are not associated with times of great biological productivity, such as might occur during OIF experiments or larger scale deployment.

Nitrous Oxide (N₂O). Nitrous oxide has more than 300x the greenhouse gas warming potential of carbon dioxide. When organic material derived from ocean biological productivity is decomposed by microbial activity (rem mineralization), much of the nitrogen is oxidized and converted back to soluble nitrogen compounds. During this breakdown of organic nitrogen compounds, N₂O can be generated in two ways. First, in the presence of oxygen, the oxidation of ammonium to nitrate (nitrification) generates a small percentage (about 1 molecule in 1000) of N₂O [Cohen and Gordon, 1979]. Given the approximate ratio of C:N in organic material (about 6.6:1), this would generate about 1 molecule of N₂O for every 6600 molecules of CO₂ and would offset about 4-5% of the CO₂. This amount that should be accounted for in ocean fertilization, but would not be a problem compared to removal of CO₂.

When oxygen concentrations are extremely low (<50 mmol/kg), a significant fraction of the organic nitrogen can be converted directly to N₂O [Law and Owens, 1990]. We agree with the Canadian Review that N₂O can be generated from OIF but that the relative amount is likely to be highly dependent on the location of fertilization: the extremely low oxygen concentrations are most common in tropical waters. However, the Review does not point out that the biogeochemical modeling study they cited [Jin and Gruber, 2003] suggests that N₂O generated at high latitude locations would have only a small effect on the net GHG benefit. This is particularly relevant, as the only two OIF experiments that measured N₂O generation were in the Southern Ocean (EIFEX, described above, and SOIREE in the western Pacific sector of the Southern Ocean). No N₂O was detected at EIFEX [Walter *et al.*, 2005]. A 6-12% reduction in the net GHG benefit was calculated from measurements at SOIREE [Law and Ling, 2001]. The Canada Review correctly points out the results of these measurements, but does not mention that they agree with the modeling predictions made by Jin and Gruber [2003]. These results suggest that high latitude oceans and in particular the Southern Ocean, are the best locations at which to avoid a negative effect from N₂O generation.

Methane (CH₄). Methane is a greenhouse gas with roughly 20 times the greenhouse warming potential of CO₂ [Ramaswamy, 2001] that is commonly formed by the reduction of organic carbon compounds under anaerobic conditions. Methane is supersaturated in ocean surface waters by 5-75% (e.g., [Scranton and Brewer, 1978]; [Karl and Tilbrook, 1994]; [Tilbrook and Karl, 1995]). The origin of the methane and the mechanism of supersaturation are unknown, as surface waters of the ocean are saturated to slightly supersaturated in oxygen (e.g. [Talley, 2007]) and should therefore oxidize methane. Researchers have gone so far as to refer to the methane supersaturation as the "oceanic methane paradox" [Kiene, 1991]. Although at least one mechanism has been suggested to explain the supersaturation through the decomposition of methylphosphonate (D. Karl, written comm.), there is no agreement on the origin of the methane or on what might control its rate of production.

In spite of the lack of understanding of the methane supersaturation, it has been suggested that if the mechanism is related to biological productivity that the methane production could be exacerbated by productivity associated with ocean iron fertilization. Methane production was measured during few ocean fertilization experiments [Wingenter *et al.*,

2004b]. We agree with the Canadian review that lacking a known mechanism or measurements, it is impossible to assess whether this is a excess methane production would result from OIF or not. But measurements of methane production inside and outside ocean fertilization experiments should be undertaken.

3. The estimated extent and potential impacts of bacterial decay of the expected phytoplankton blooms, including the reduced oxygen concentrations

The cycle of primary productivity in the ocean is tightly coupled to oxygen in both the atmosphere and the ocean. Oxygen is produced in surface waters by photosynthesis, and oxygen is consumed as dead phytoplankton and other organic matter are decomposed by metabolic activity. In the deep ocean, two factors control oxygen concentration. First, oxygen is produced by photosynthesis resulting in high oxygen concentrations in surface waters. In regions where surface water sinks to form the bottom waters of the ocean, such as the Southern Ocean and the North Atlantic, the oxygen is carried with the sinking water and ensures that the bottom waters are oxygenated. Second, oxygen is consumed as dead phytoplankton and other organic matter sink and are decomposed by metabolic activity. This process contributes to an oxygen minimum zone that lies at approximately 1000m.

While we agree with the Canadian Review that the current ability of biogeochemical models to predict the anoxia effects from OIF are relatively poor, we disagree with their justification for potential negative effects. The Review states, "*What is clear is that anoxia, even over relatively brief time periods, could be catastrophic for the organisms in affected areas, and could trigger several biogeochemical consequences (Fuhrman and Capone 1991).*" The paper by Fuhrman and Capone [1991] draws its conclusions from what is now a seventeen year old box model simulation of OIF by Sarmiento and Orr [Sarmiento and Orr, 1991]. While the Sarmiento and Orr work was a pivotal study in the OIF lexicon and has helped to shape scientific investigation of both efficacy and ecological effects of OIF, it was conducted before even the first mesoscale iron fertilization experiment and before the advent of modern coupled ocean/atmosphere models with biogeochemistry and ecological components.

Sarmiento and Orr [1991] sought to understand through modeling the maximum CO₂ drawdown that could occur if as much of the macronutrient content of the surface ocean as possible was drawn down by fertilization. They used a simple box model to drive the surface phosphate concentration toward zero over vast reaches of the Southern Ocean, the tropics, the North Pacific, and the North Atlantic for a period of 100 years. All surface phosphorus was assumed to be taken up by organic material in the Redfield Ratio (106 C: 16 N: 1 P) during the entire year. The model then simulated export of all organic material directly to the deep ocean and respiration was allowed to decompose the organic material. In this rudimentary model of the ocean -- that did not include atmospheric and ocean circulation, nor biogeochemical recycling -- fertilization of the Southern Ocean was seen to be most effective in reducing CO₂. The model indicated that anoxic conditions in the bottom waters would be generated under these unrealistic conditions.

While this model was useful to call attention to the relationship between deepwater oxygen consumption by respiration and OIF, it was, and still is, wholly unrealistic for OIF for many reasons. First, it assumes that you *could* draw phosphorus down to zero concentration over extended periods by OIF. This has not happened in the OIF experiments even when four applications of iron were used in the patch [Coale *et al.*, 2004] and therefore would be unlikely in larger scale deployment (this is because it is not generally phosphorus or nitrogen that limit the growth of phytoplankton in regions appropriate for OIF). The model also draws phosphorus to zero in regions of the ocean that are not limited by iron and would be unlikely targets for OIF, including much of the Atlantic. Finally, the model

includes no physical or biological regeneration of nutrients and organic material in the surface waters. In the past seventeen years, experimental results and more realistic models have produced significant advances that call the results of this simple model into question.

In recent years the sophistication and predictive capabilities of models have improved substantially, and a recent survey of twelve global ocean circulation and coupled biogeochemical models by Najjar et al. [2007] examined in detail the ability of these models to predict deep ocean oxygen profiles. They found that models consistently overestimated the amount of oxygen depletion at depth, and in particular questioned the realism of the Sarmiento and Orr [1991] method of using phosphate levels to control oxygen consumption at depth.

Do we know how OIF would affect deep ocean oxygen levels? Certainly an increase in OIF-driven carbon export would consume oxygen in midwaters, however modeling the relative impact of this increase in consumption is complicated by the dual roles of carbon export and ocean circulation. Deepwater oxygen levels are a balance between consumption of oxygen by organic material that has fallen into deep water and “ventilation” of the deep ocean by high oxygen surface water. We agree with the general thesis of Sec 3.20 in the Canadian Review, which states that recent experimental evidence and modeling studies suggest that anoxia is not likely to be a significant consequence of OIF. Given the difficulty of accurately reproducing deep ocean oxygen levels in models, OIF policy should not be based on predictions of deep ocean anoxia from older general circulation models [Sarmiento and Orr, 1991]. Rather, improving the performance of newer models should be carried out through assimilation of the data from further OIF experiments, particularly in the Southern Ocean. Such activity would develop a more complete observational record required to test the models, and in particular satisfy the “need for greater spatial coverage and for methods to extrapolate measurements to basin-wide scales with error estimates” [Najjar et al., 2007].

4. The types of phytoplankton that are expected to bloom and the potential impacts of any harmful algal blooms that may develop

We agree with the Canadian Review that, “*in the short term, iron fertilization shifts phytoplankton communities to favour larger diatom species (Coale et al. 2004 in Boyd 2004; de Baar et al. 2005) and generally increases the abundance of grazing species (Gall et al. 2001a and Gervais et al. 2002 in Hoffmann et al. 2006).*” (Sec 3.25). Short-term changes to species composition occur *whenever* a phytoplankton bloom occurs. Natural blooms have been studied for decades, of course, and a rich literature based on theory, laboratory experiment and observations at sea and from satellite have resulted in basic empirical understanding of bloom dynamics. The most comprehensive of studies of open ocean blooms are the JGOFS process studies, which invested enormous international oceanographic expertise in evaluating the time evolution of blooms from the standpoint of photosynthesis, zooplankton dynamics, biogeochemistry, and carbon export. These studies conclusively show that natural phytoplankton blooms in all of the major biogeographical regions of the ocean result in changes in the proportion of various species in the photosynthetic community throughout the progress of the bloom: e.g.: JGOFS North Atlantic Bloom Experiment [Lochte et al., 1993]; JGOFS Equatorial Pacific Experiment [Barber et al., 1996]; JGOFS Arabian Sea Experiment [Shalapyonok et al., 2001]; JGOFS Southern Ocean Experiment [Bathmann et al., 2000]. This is because photosynthesizers have evolved different strategies for capitalizing on the nutrient and light resources from the ocean (e.g. [Longhurst, 2006]) and because zooplankton graze picoplankton with different efficiencies than they do larger photosynthesizers like diatoms (e.g. [Morel, 1997]).

We also agree with the Canadian Review that more research is needed on potential ecosystem effects. However, the Review suggests that the observed short-term shifts can *directly* result in long-term changes: “Chisholm *et al.* (2001) stress that the oceans are “a tightly linked system” and that these community shifts will alter poorly understood biogeochemical cycles in unintended ways.” There is no evidence cited by Chisholm *et al.* [2001] that the short-term community shifts during OIF blooms will result in long term community shifts or that those shifts would alter biogeochemical cycles in unintended ways. This is an assertion. Hoffman and coauthors clarify that: “It is still unknown whether the observed changes in community structure and elemental composition would be sustained during long-term fertilization and would have lasting effects on the ecosystem structure.” [Hoffmann *et al.*, 2006] p. 1218. Our point is not to argue that we *know* that there will be no effects from long-term fertilization, but that the effects are unknown. Short-term fertilization experiments have not been shown to have harmful ecosystem effects, but will facilitate further research to understand potential long-term effects.

Regarding Harmful Algal Blooms, we agree with the Review that, “Harmful algal blooms (HABs) are predominantly a coastal phenomenon and there is no evidence of such blooms arising from iron fertilization experiments.” (Sec 3.27). However, the Review also mentions *Pseudonitzschia*, which we discuss briefly here.

Pseudonitzschia is one of the species that may occur in blooms in many open ocean regions (e.g. [Smetacek *et al.*, 2002]). Some, but not all, species of *Pseudonitzschia* are capable of producing domoic acid (DA), a neurotoxin. There is an important difference between having the genetic capability of producing DA and doing so. If phytoplankton capable of making DA are common in open ocean phytoplankton blooms, do they express this gene and make DA? There have been few measurements published related to the *expression* of the gene for DA in open ocean phytoplankton. Wells and coworkers [Wells *et al.*, 2005] are one of the few groups who have studied *Pseudonitzschia* in the open ocean. They indicate that DA plays an important role, together with copper, in ensuring that *Pseudonitzschia* can survive under very low iron conditions in the open ocean. “This system may explain why *Pseudonitzschia* spp. are persistent seed populations in oceanic HNLC regions, as well as in some neritic regions. Our findings also indicate that in the absence of an adequate copper supply, iron-limited natural *Pseudonitzschia* populations will become increasingly toxic.” (p. 1908). We know from studies of *Pseudonitzschia* strains from coastal waters that they are more likely to make DA when iron-stressed than when growing under iron-replete conditions: “Our findings suggest that DA production during exponential growth of these two toxigenic *Pseudonitzschia* species is directly induced by Fe-deficient or Cu stress conditions and that 95% of this DA is actively released into the medium.” (p. 515) [Maldonado *et al.*, 2002].

Perhaps because of this adaptation that gives *Pseudonitzschia* spp. a survival advantage under low iron conditions, they are a common component in the artificially fertilized OIF blooms as well as natural blooms: IronEx I and II [Cavender-Bares *et al.*, 1999], SERIES [Denman *et al.*, 2006], SOFEX [Coale *et al.*, 2004], EisenEX [Gervais *et al.*, 2002], and a minor component of the diatom bloom during SEEDS I that was replaced by a centric diatom [Saito *et al.*, 2005]. To our knowledge there have been no studies during the fertilization experiments to determine whether *Pseudonitzschia* were generating domoic acid during the fertilization.

Given that *Pseudonitzschia* is present in both the natural and fertilized blooms in these regions, is there evidence that it is harmful to other organisms? *Pseudonitzschia* diatoms are also sometimes present in coastal areas and have been associated with some HABs in these regions that have affected marine mammals and seabirds [Schnetzer *et al.*, 2007]. In fact, all of the references linking HAB activity from *Pseudonitzschia* and marine mammals or seabirds have described in coastal waters. We have not been able to find a reference to harmful effects on marine mammals, seabirds or fish from *open ocean* phytoplankton blooms, natural or otherwise, containing *Pseudonitzschia*. Indeed, it would seem unusual to have *Pseudonitzschia* be a dominant organism in natural phytoplankton blooms in the open ocean and at the same time be toxic to the organisms that feed off of those blooms. Although our assessment of this situation is that there is no *a priori* evidence the *Pseudonitzschia* blooms associated with OIF experiments are harmful to the organisms from

the region, and that under iron replete conditions DA production may be reduced, we believe that study of this association should be a high priority for future OIF experimentation.

5. The nature and extent of potential impacts on the marine ecosystem including naturally occurring marine species and communities

The Canadian Review discusses a serious issue that has been raised several times concerning the potential that OIF, especially if continued for several years, would result in ecosystem shifts that could have negative consequences for ocean ecology in general and for fisheries specifically. The Review states, "*It is clear that iron fertilization generates shifts in phytoplankton community composition (de Baar et al. 2005; Hoffmann et al. 2006), and since phytoplankton are the foundation for many intricately linked marine food chains, it seems likely these shifts will have impacts on other marine species and communities (Chisholm et al. 2001). However, it is impossible to predict, given our current knowledge, exactly what those impacts may be and to what extent they may occur.*" (Sec 3.30). As discussed in the previous section, the cited references clearly state that community ecosystem shifts are short term and that it is unknown whether long-term sustained fertilization would result in long-term ecosystem shifts. The suggestion that OIF would have "likely" impacts on other marine species and communities is speculative and was not supported by evidence in the original reference.

With regard to phytoplankton dynamics from the iron-stimulated blooms, De Baar et al. [2005] emphasize that all of the OIF experiments with the exception of IronEx I (during which the bloom was subducted after four days of development) culminated in well-defined diatom blooms. Martin et al. [Martin et al., 1994], Coale et al. [Coale et al., 1996], Mann and Chisholm, [Mann and Chisholm, 2000], and Landry [Landry, 2002] have studied the phytoplankton in the equatorial Pacific and Southern Ocean iron enrichment experiments. They found that the non-diatom photosynthesizers were iron limited [Mann and Chisholm, 2000] and increased quickly in response to iron enrichment. After a few days they reached a higher concentration level, but ultimately were held from further increases by zooplankton grazers [Landry et al., 2000]. Diatoms also began to increase quickly but, as is the case with natural blooms, their initial concentrations were low and it generally took several days for them to dominate the assemblage (Barber and Hiscock, 2006).

Lindley and Barber [Lindley and Barber, 1998] have studied the response of photosynthesizers to natural iron stimulation in waters that are in the wake of the iron-rich Galapagos Island downstream circulation. They found that the phytoplankton response was identical to that during the IronEx II experiment. [Blain et al., 2007a] also found a succession in photosynthesizers in the natural Kerguelen Plateau iron enrichment. Thus, natural blooms stimulated by iron, natural blooms stimulated by increased light or nutrients, and artificially stimulated blooms all show a succession of phytoplankton, often ending with dominance by a group that was rare before the bloom originated. This is natural and not an indication that artificially stimulated blooms change phytoplankton ecology in some new or unexpected way.

What causes these changes in dominance? There is growing evidence that one assemblage does not *replace* another, but that the success of the picoplankton and other non-diatom photosynthesizers is controlled by different factors than those that control diatoms. Barber and Hiscock discuss this issue in detail and observe that "...over the years a few very careful observers from Ryther, [Ryther, 1963] to Landry [2002], who work in oceanic as opposed to coastal habitats, have quietly noted that there is no replacement of the ambient non-diatom assemblage during diatom bloom formation." (p.2. Barber and Hiscock, 2006). Instead, the non-diatom photosynthesizers continue to grow at higher concentrations than under non-bloom conditions, but zooplankton effectively keep them in check at this higher

concentration. In contrast, diatoms are not effectively grazed by the zooplankton and can continue to grow, using the available nutrients. Thus, there is also no evidence that the climax assemblage of phytoplankton in natural or artificial blooms eliminates or replaces the non-diatom assemblage resulting in some permanent change to the ecosystem. There are still debates in the phytoplankton community about *why* the diatoms “overprint” the non-diatoms rather than displace them (e.g. [Morel *et al.*, 1991] vs. Barber and Hiscock [2006]), but modern literature agrees that replacement is not happening. Barber and Hiscock [2006] emphasize the self-limiting nature of the fertilized blooms. They also point out that in order to be effective, OIF must occur intermittently, not continuously:

Continuous iron fertilization will not produce efficient sequestration of carbon because as the mesozooplankton become abundant they can continuously graze and recycle a large proportion of the newly produced diatom biomass in the surface layer. This increased grazing rate prevents the accumulation of the diatom biomass needed for efficient export. Therefore, efficient engineered carbon sequestration requires episodic Fe enrichment with a return to the ambient picoplankton-dominated assemblage between enrichments.”

As with comments we made previously, our point is not to assert that no change will take place, but to point out that there is no evidence in the literature that a change *will* take place. Further experimentation is needed to answer these questions.

Nutrient depletion is another serious issue that has been raised by many concerned about the impact of OIF. The Canadian Review states that “models predict” the potential of macro-nutrient depletion caused by sustained large-scale OIF, and suggest that this, “*would in turn cause all of part of the phytoplankton community to crash, reducing the efficacy of the ocean as a carbon dioxide sink.*” (Sec 3.31). No references are given in support of this assertion, however both [Sarmiento and Orr, 1991] and [Gnanadesikan *et al.*, 2003] make these predictions based on the results of general circulation models coupled to a nutrient-restoration model, and we presume that the Review is referring to these studies as they are the most common citations on this issue.

A nutrient-restoration approach explicitly forces the depletion of macro-nutrients to *simulate* the action of biological productivity, and was a necessary simplification in older models that did not have an explicit ecological model linked to an iron cycle. This simplification results in the unrealistic consumption of macro-nutrients, and in particular the dire consequences predicted by each study are based on an extreme scenario that artificially forces the complete consumption of all surface macro-nutrients. This results in an order-of-magnitude greater consumption of nutrients than is in the norm in nature. Also, in the case of the Gnanadesikan *et al.* [2003] model, a second unrealistic assumption was made: that 100% of the exported organic matter would not remineralize until reaching the sea floor. This would have the effect of removing far too many nutrients from surface waters. Modern experiments like VERTIGO have measured only 20-50% of exported organic material sinking below 500m [Buesseler *et al.*, 2007c]. Given the extremely unrealistic assumptions of the Sarmiento and Orr [1991] and Gnanadesikan *et al.* [2003] models, it is no surprise that “catastrophic” effects are predicted.

An assessment of potential consequences based on more realistic ocean models was completed by Aumont and Bopp [2006]. They modeled 100 year large-scale fertilization rather than ‘patches’, and their simulation is therefore similar in scale to that of Sarmiento and Orr [1991] and Gnanadesikan *et al.* [2003]. Aumont and Bopp [2006] also addressed the issue of downstream nutrients:

*“Previous studies have suggested that iron fertilization may alter the current patterns of primary productivity, even far away from the enrichment sites [Sarmiento and Orr, 1991; Gnanadesikan *et al.*, 2003]. However, because they were using the nutrient-restoring approach, export production was predicted to drop to zero after the stop of the iron*

supply. Of course, as shown by our model, this result is unrealistic. Primary productivity is not decreased in the core of the HNLC regions, neither during the fertilization nor after it."

They found that the Southern Ocean did not cause downstream nutrient depletion because of the deep winter mixed layer depth that restored summertime nutrient levels, and because light limitation and the short growing season prevented the uptake of all available nutrients even upon iron fertilization [Aumont and Bopp, 2006]. They also found that nutrient depletion did occur in Tropical oceans, but also found that total global biological productivity increased by 20.0 GtC/yr in the first year of fertilization, and remained elevated by 9.8 GtC/yr in the 100th year of fertilization when compared to the unfertilized case. This increase is significant in magnitude when compared to global primary productivity (~48 GtC/yr - [Behrenfeld et al., 2006]),

Interestingly, the Jin et al. [2008] findings suggest that nutrient depletion may not actually cause a reduction in the uptake of atmospheric CO₂. In a ten year simulation after a one-time fertilization event, they found enhanced export for several years due to multi-year iron retention in the surface waters. Subsequently, they found reduced productivity and carbon export due to nutrient depletion. However they also found that net atmospheric uptake efficiency remained relatively unchanged throughout the process: "*since the cumulative fluxes of both air-sea exchange and vertical export decrease at a similar rate after year 3, the atmospheric uptake efficiency remains relatively constant thereafter.*" [Jin et al., 2008] (p.394).

Two conclusions can be drawn from the above discussion. First, the more realistic coupled physical and biogeochemical/ecological models show that both the anoxia and the downstream nutrient depletion were model-dependent phenomena and that locations and scale of OIF can be tailored to reduce impacts. Second, the Southern Ocean is likely to have less effect on downstream nutrients because other factors such as a shorter grown season and a deep mixed layer prevent the uptake of all available nutrients [Aumont and Bopp, 2006].

Finally, there is substantial paleoceanographic evidence that ecosystem shifts, anoxia, and downstream nutrient depletion would not be "catastrophic" results from a program of sustained large scale OIF. Glacial periods during the Quaternary (the past 2.4 million years) were characterized by increases in dust flux of 2-20x to the ocean (e.g., [Jickells et al., 2005], [Winckler et al., 2008]), and accompanied by substantial increases in ocean biological productivity [Cassar et al., 2007]. Synchronous increases in productivity would be unlikely if downstream nutrient depletion was a substantial problem. With regard to ecosystem shifts, while there are certainly changes in the plankton assemblages preserved in sediments that are associated with glacial times that are different than those associated with interglacial times (e.g. [Hays et al., 1969]), there is no evidence from microfossils or pigments that these changes eliminated other species. Furthermore, while there is evidence during this time of *global* productivity changes, enhanced export [Moore et al., 2000] and lower oxygen concentrations at the sea floor, there is no evidence of anoxia [Francois et al., 2002].

6. The estimated amounts and timescales of carbon sequestration, taking into account of partitioning between sediments and water.

The Canadian Review raises concerns about the detailed methods for quantifying the amount and timescale of sequestration for future OIF projects. Estimating carbon sequestration from OIF requires measurements that quantify the carbon that sinks below a level where it readily exchanges with the atmosphere (accounting for CO₂ drawdown) as well as understanding the duration of sequestration or "permanence". *Real carbon reduction* is the total carbon sequestration benefit that might be claimed from a project, and is a combination of total permanent *carbon export*, *atmospheric uptake* by surface waters as a consequence of carbon export, and *leakage* effects that result in changes

in GHG emissions outside of the project boundary. These terms and their method of quantification are discussed in detail below.

It is perhaps useful to note that the UNFCCC and the international carbon market community have developed procedures and protocols for quantifying carbon reductions, and ensuring their accuracy and validity. In addition to the rigorous scientific standards for measurements that are prerequisites, it is also important that commercial experiments follow market protocols to estimate the amount of carbon sequestration. The following discussion will adhere to these international standards for evaluating carbon reduction projects.

Permanence. Permanence quantifies the duration of the carbon reduction. While some carbon sequestration projects result in no chance that the carbon will return to the atmosphere (e.g. methane flaring, etc.), many projects rely on a statistical model for the length of sequestration; this includes projects from terrestrial forestation (in which sequestration is based on models of the expected lifetime and age distribution in a forest) to geologic sequestration (in which sequestration is based on models of the amount of leakage from the reservoir and the risk of release).

Time length of storage in the ocean can be calculated on the basis on the age and depth of the waters into which the carbon is exported. The upper ocean is well mixed on a regular basis and any organic carbon in this zone could be remineralized to CO₂ and escape back to the atmosphere within months. This zone of routine mixing is generally equivalent to the depth of the winter mixed layer. Above this layer water is likely to re-equilibrate with the atmosphere on a yearly basis. Below this layer, the water (and the CO₂ in it) takes longer before it mixes with the atmosphere again. We know the general patterns of deepwater circulation from our knowledge of the processes that control sinking (deep water formation) in the ocean and the distribution of chemical species in the deep ocean. We know the more detailed age-depth pattern of this circulation from measurements of anthropogenic tracers, like chlorofluorocarbons (for which we know the history of manufacture), radioactive carbon from atmospheric nuclear testing (with a known date of suspension) and other tracers with limited lifetimes.

These tracers and other important chemical components of the ocean were measured synoptically through the ocean during the World Ocean Circulation Experiment (WOCE) (e.g. [Talley, 2007]). Deepwater circulation models were tested against the profiles that document the 'invasion' of these tracers into the ocean and other chemical distributions to form a model of the mid- and deepwater circulation of the ocean. Matsumoto [2007] used a general circulation model calibrated with the WOCE data, to produce a global map of water *circulation age* below 1500m depth (Fig 3) [Matsumoto, 2007]. This figure shows the "age" of deep water below 1500m depth relative to the last time in which this water was at the ocean surface. Thus the youngest ages imply waters that were most recently at the surface of the ocean and oldest ages imply waters that have not equilibrated with the atmosphere in over 1000 years. As a result, the figure clearly shows where deepwater "forms" at high latitude in the Atlantic and Pacific Oceans. These young bottom waters move very slowly through the ocean basins so that bottom waters in the North Pacific have "ages" of over 1000 years. This figure is primarily meant to demonstrate the ability of models that have assimilated the WOCE data to trace the trajectories of specific waters in the deep ocean. While this figure shows the circulation age, which is a different quantity than what will be needed to determine permanence depth, the *time before ventilation* of deep waters can also be modeled in the same way. The fate – or residence time – of carbon sinking into deep waters can be calculated by modeling the trajectory of the water at any specific depth.

In the case of OIF, the permanence time period depends first on depth of export for each ton of carbon. Particulate organic carbon (e.g. dead phytoplankton or fecal pellets) sinks and is *remineralized* into inorganic nutrients, at which point it stops sinking. Then, the trajectory of the associated water mass controls the length of time before the carbon could re-equilibrate (or "ventilate") with the atmosphere. While we have simplified this explanation, in general

permanence depends on knowledge of the profile of exported carbon and the modeled age and trajectory of the water into which the carbon is exported. Thus total carbon export flux can be measured at any depth, and the time until ventilation can be determined for that depth. A small percentage of the exported carbon reaches the seafloor sediment surface, escapes remineralization at the seafloor and in the sediment and is sequestered for thousands of years or more.

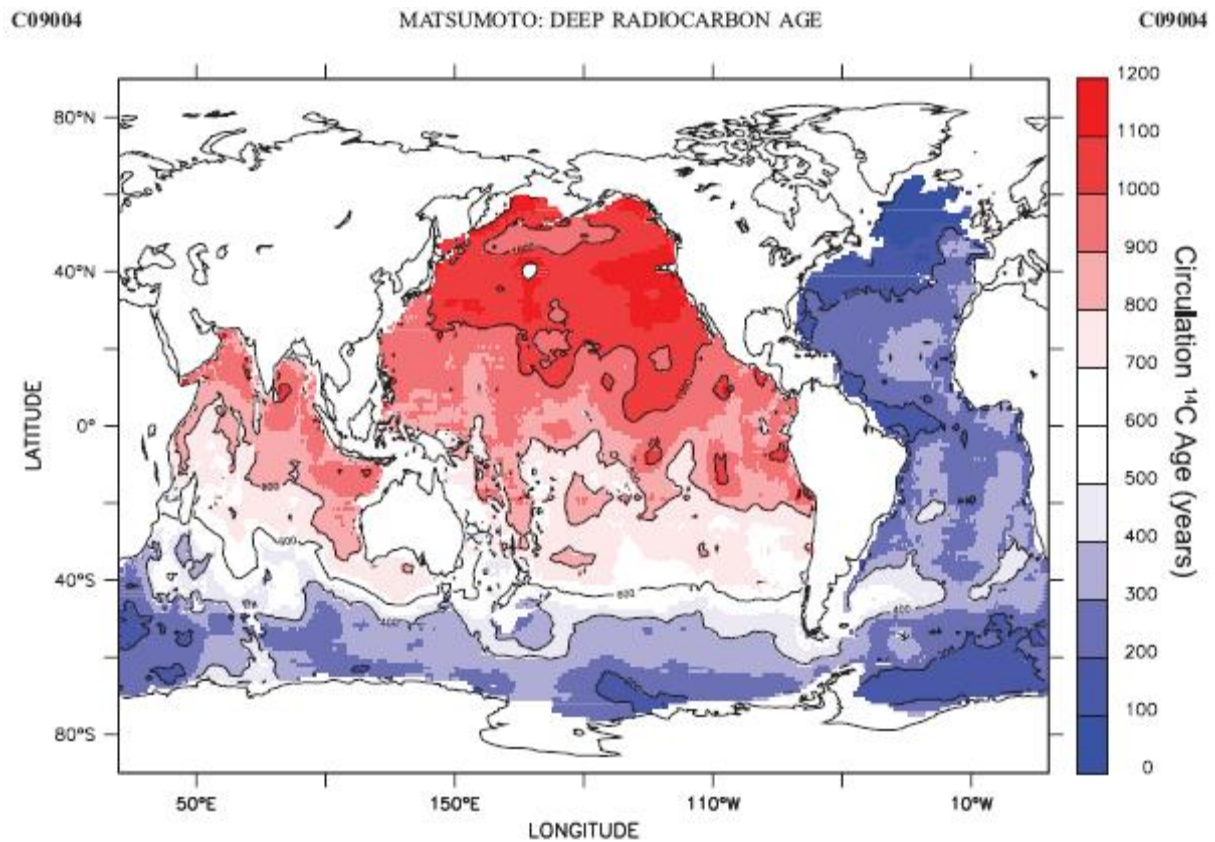


Figure 3. Global map of the “circulation age” of waters beneath 1500 meters depth, based on measurements of C-14 by the World Ocean Circulation Experiment (WOCE) combined with circulation models. The data in this map shows how long before present a deep water parcel existed at the ocean surface. OIF would require a slightly different map to determine the *permanence depth*, showing the time before surface ventilation of a deep water parcel. However this map is illustrative of the resolution of data generated from circulation models. [Matsumoto, 2007]

The choice of a permanence time period is important because it sets the minimum depth at which carbon export from any OIF project can be considered sequestered. An appropriate choice can be informed from scientific and policy deliberations on Global Warming Potential, which is a method to normalize the heat trapping capabilities of the important greenhouse gases (e.g. N₂O, CH₄, HFCs). GWP works by dividing the cumulative lifetime radiative forcing per molecule of each gas against a reference time period of cumulative radiative forcing of a molecule of carbon dioxide [IPCC, 1995]. The IPCC [1995] calculated GWP for time periods of 20, 100, and 500 years, and characterized the choice of “time horizon” as a policy decision. In discussion of this, they noted:

“Policy-relevant climate-change phenomena exist at both ends of the climate-change time spectrum:

1. *If the policy emphasis is to help guard against the possible occurrence of potentially abrupt, non-linear climate responses in the relatively near future, then a choice of a 20-year time horizon would yield an index that is relevant to making such decisions regarding appropriate greenhouse gas abatement strategies. In addition, if the speed of potential climate change is of greatest interest (rather than the eventual magnitude), then a focus on shorter time horizons can be used.*
2. *Similarly, if the policy emphasis is to help guard against long-term, quasi-irreversible climate or climate-related changes (e.g., the very slow build up of and recovery from sea level changes that are controlled by slow processes such as warming of the ocean), then a choice of a 100-year or 500-year time horizon would yield an index that is relevant to making such decisions regarding appropriate greenhouse gas abatement strategies.*

With this awareness, policies could choose to be a mix of emphases. GWPs with differing time horizons can aid in establishing such a mix. Indeed, that was the case in the Montreal Protocol deliberations, in which the long-lived, high-ODP gases were the initial focus and the shorter-lived, lower-ODP gases were subsequent focus.” [IPCC, 1995] p.229

The Kyoto Protocol adopted 100 years as the only time horizon for calculating GWP [UNFCCC, 1997]. All subsequent carbon credits have been normalized by this standard, including emissions reductions from sinks (e.g. forestry) as defined by the UNFCCC Reporting Guidelines:

“Annex I Parties should report aggregate emissions and removals of greenhouse gases, expressed in CO₂ equivalent terms at summary inventory level, using GWP values provided by the IPCC in its Second Assessment Report.” [UNFCCC, 1996] (para. 20)

Subsequent discussion by the IPCC on the definition of permanence from forestry projects shows how the GWP approach can also be applied for calculating the benefit of any sequestration project of variable length as compared to a reference time period of 100 years:

“Absolute Global Warming Potentials (AGWPs) are calculated by integrating the total radiative forcing of an emissions pulse over a 100-year time horizon with no discounting. Relative GWPs are the ratio of this integral for a given GHG to that of CO₂, which serves as the reference gas. This approach could be applied to compare carbon sequestration projects of different lengths, although there is no requirement in the Protocol to use the same conventions in this context. The reference is “permanent” (more than 100 years) removal (or emission) of 1 t CO₂.” [IPCC, 2000] (p.87)

We have proposed that a methodology for carbon sequestration by OIF would claim reductions when the modeled trajectory of exported carbon resulted in 100+ years permanence. We agree with the Canadian Review that, *“Given the current state of knowledge, iron fertilization may be able to meet this 100-year definition of permanence.” (p.14)*. We disagree with the Canadian Review’s suggestion that using 100 years as the standard for permanence may not be the best choice. The Review suggests that 100 years was chosen because it *“reflects the estimated duration of sequestration through forestry” (Sec 3.46)*. As mentioned above, 100 years was chosen as the standard to normalize the radiative forcing benefit of all covered greenhouse gases. As such, it is one of the most fundamental decisions of the Kyoto Protocol. This choice by policy makers incorporated consideration for both long-term benefits and short-term benefits, and is the best definition available for “viable sequestration”. Choosing a different time period for carbon sequestered by OIF would call into question the sequestration ‘viability’ of a wide variety of other unrelated techniques for the entire spectrum of greenhouse gases. Although we do agree that consideration must be given to the feasibility of conducting OIF operations continuously for decades to centuries, this is a separate issue from the determination of permanence. Also, it should be noted that the permanence threshold for a project is a *minimum*, much of the claimed benefit would be sequestered deeper and therefore longer.

Carbon Export. A variety of techniques are available to measure sinking carbon flux as particulate organic carbon (POC). Particle flux can be estimated from the inventory of ^{234}Th (e.g. [Buesseler et al., 2004]). Techniques for improving the accuracy of POC measurements using Neutrally Buoyant Sediment Traps (NBST) are described by Buesseler et al. [Buesseler et al., 2007a]. An autonomous profiler was used during SOFEX-N to allow continuous estimation of particulate concentrations in the water column by transmissometry [Bishop et al., 2004]. Underwater autonomous vehicles capable of collecting samples over long distances are undergoing rapid development (e.g. [Tozzi et al., 2006]). These techniques together with established methods for characterizing dissolved fluxes (e.g. water column sampling for oxygen, nutrients and carbon system parameters) provide redundant techniques for characterizing the export of carbon from the surface to deeper waters. The techniques and equations for quantifying carbon export would be explicitly defined in the *methodology* for the project.

Baseline. Carbon reductions must be measured against a baseline. In the case of an OIF project, this would require making a full suite of measurements in an adjacent unfertilized patch of ocean. The difference between the fertilized and unfertilized patches would represent the *real carbon reductions* generated by the project. The baseline would also incorporate emissions from the combustion of fossil fuels from ship operations, which Climos estimates to be less than 3% of the total carbon reductions expected from a 200 x 200 km patch.

Biogenic Gases. The production of biogenic gases such as N_2O , and CH_4 would need to be measured both inside and outside the fertilized patch. The difference in production can be quantified and subtracted from the total measured carbon export.

Leakage. Leakage refers to changes in GHG inventories that occur outside of the project boundary as a result of the project. In the case of OIF, this could occur from the consumption of downstream nutrients that prevents natural biologic productivity that would have occurred at a later date. We have discussed this issue earlier in this response. The effects of this leakage are highly dependent on the location of the fertilized patch, as demonstrated in the model by Aumont and Bopp [2006].

7. The estimated carbon mass balance for the operation

The Canadian Review also questions whether OIF deployed at a basin-wide scale could sequester sufficient carbon to make it worthwhile. The Canadian Review uses the term “small” to suggest that OIF is unlikely to have any significant net benefit, and writes, “*Even if it were possible to fertilize the ocean globally, with complete alleviation of iron limitation, the expected impact on atmospheric CO_2 growth under continuing industrial emissions is small (Aumont and Bopp 2006; Zahariev et al. 2008)*” (Sec 3.34). The results from the Aumont and Bopp [2006] simulations suggest that OIF could remove 33 ppm CO_2 from the atmosphere after 100 years of continuous fertilization. This is not a small number, as it represents 1/3 of the current elevation of atmospheric CO_2 levels. Zahariev et al. [2008] using a different set of model assumptions calculated that global OIF would absorb approximately 11% of annual anthropogenic emissions [Zahariev et al., 2008]. In contrast to the language used by the Review, these numbers would suggest that OIF has the potential to become one of the single largest individual techniques to mitigate atmospheric CO_2 levels.

However, OIF should not be expected to decrease the entire burden of anthropogenic CO_2 emissions removal. This is neither realistic nor desired. We do not impose this requirement on other technologies or techniques such as forestation, deep geologic sequestration, wind turbines, or photovoltaics. We believe that this issue is a legacy of very early suggestions that OIF could be used to draw down a significant portion of the accumulated anthropogenic CO_2 .

We have never made such a claim. If OIF is assumed to work in parallel with the myriad other mitigation responses, then the total potential removal is large compared to most other options.

Discussion of Recommended Actions to Address Section 3 of the Scientific Group's Statement of Concern

1. The purposes and circumstances of proposed large-scale ocean iron fertilization operations and whether these are compatible with the aims of the London Convention and the Protocol

The Canadian Review discusses compatibility of OIF with the LC/LP in the context of whether iron is considered industrial waste, whether the iron is placed for a purpose other than mere disposal, and whether carbon credits should be issued.

On the issue of whether iron is considered waste, the Canadian Review suggests that, "Industrial waste may be involved in some of the large scale fertilization proposals" (Sec 5.1). The London Convention Annex I defines industrial waste as, "waste materials generated by manufacturing or processing operations." While the iron sulfate originally is derived from a co-product of either steel manufacturing or titanium dioxide manufacturing, it is further processed by commercial formulators according to specifications appropriate for use in a variety of land-based applications (e.g. fertilization of plants and nutritional supplements for animals). The iron sulfate that will be used by Climos will not be a "waste" within the meaning of the Convention but rather a product, processed and engineered to specifications appropriate for this application. As a result, the iron sulfate compounds used in OIF would not fall within Annex I.

On the purpose of fertilization, the Canadian Review questions whether OIF can actually sequester carbon in sufficient quantity or duration. We have discussed at length the evidence that suggests OIF can in fact do this. The Review's summary assessment is, "*If fertilization is shown to have a significant new benefit in the context of global warming, and if the potential ecological consequences have been thoroughly assessed, then perhaps this alternative purpose for the disposal of iron should be considered a legitimate beneficial use.*" (Sec 5.3). While we agree that if OIF has new benefits it should be considered, we think that the use of the term "disposal of iron" is inappropriate. The purpose of OIF is carbon sequestration, not iron disposal.

Finally, on the question of commercialization, the Review states that, "*At the present time, there is no reliable means of determining how much, if any carbon is sequestered (Hoffmann et al. 2006), nor is there an international consensus on what time scale of sequestration is adequate. Until these issues are resolved there is no credible basis for issuing credits. This intention would therefore be unrealistic until measurement and verification techniques improve substantially.*" (Sec 5.4). We have discussed these issues in detail earlier in this document. We believe that existing measurement techniques and GHG market protocols for verification are sufficiently advanced today to give a conservative estimate of permanently sequestered carbon credits. This also has been discussed at length earlier in the document. Therefore, we believe that there is a rationale for issuing carbon credits—clearly the decision to do so should be made according to the merits of individual projects and on a case by case basis.

On the issue of scale we have suggested that a new round of "moderate scale" demonstrations on the order of 200 x 200 km are needed to fully understand the carbon mitigation potential and ecological effects of large scale OIF. This is also the position of the International Ocean Commission's Ad Hoc Working Group on Ocean Fertilization:

A clear justification of the need for experiments at scales of order 200 km by 200 km

- *Ocean waters are continuously stirred, with currents at different depths moving at different speeds and in different directions. Both the fertilized patch and any sinking carbon will be transported along with the currents. In the small-scale experiments (tens of kilometers) so far performed, the results are strongly influenced by dilution of unfertilized water into the patch, such that it is difficult to extrapolate the results to larger scales, or to longer times. In particular, estimates of amounts of carbon sequestered to depth from extrapolations of these experiments are very uncertain.*
- *The effects on the fertilized patch of stirring and mixing with water that has not received the fertilization treatment becomes less important near the center of the patch as patch size increases. This would provide incentive to develop experiments at scales of order 200 km by 200 km, this scale being larger than that of typical ocean eddies. For the same reason, it may be easier to assess the influence of surface manipulations on the sinking fluxes of particles when the experiments are at this scale.*
- *Experiments designed to study the impact of ocean fertilization on the lifecycles of megafauna, such as fish, may require spatial scales of order 200 km by 200km. [IOC, 2008]*

2. The need, and potential mechanisms, for regulation of such operations

We agree generally with the Canadian Review on the need for regulation of OIF operations. The LC/LP framework that requires permits for the release of materials at sea provides a good first order control mechanism. While we would argue the OIF is not actually disposal of waste, we strongly agree that every OIF project, commercial or otherwise, should as a minimum obtain a permit from a country that has ratified the London Convention.

We agree with the Canadian Review's position that seeking permits under established LC mechanisms would be an effective control strategy. *"Control mechanisms may include requiring permits for projects that would be preceded by assessments that consider the location of the proposed operation, proximity to other operations (spatially and temporally), the material to be used in fertilization, the secondary effects (such as environmental alteration, community shift, and alterations of biogeochemical cycles), and the means of quantifying carbon sequestration and the release of other greenhouse gases."* (Sec 5.7). Each of these considerations has already been outlined in the Code of Conduct which we published last fall [Climos, 2007]. We have already begun implementation of each of these steps, including a thorough development of an environmental impact assessment, a detailed methodology for conducting measurement of sequestration and permanence in a project (including measurement and deduction of the global warming potential of any secondary gasses), purity analyses for the material to be used, and so forth.

The existing carbon offset mechanisms already possess methods of quality control to ensure that carbon credits represent high quality reductions of greenhouse gases. Furthermore, the level of scrutiny applied to both voluntary and regulated carbon offset projects has increased over the past several years, making it increasingly difficult for low-quality offsets to be sold. It is a reasonable assumption that, over the long term, commercialization of OIF would not be possible *unless* real atmospheric reductions and a benign environmental effect can be demonstrated.

On the issue of centralized control, we feel that ultimately this may be necessary and desirable, but that in any event it would take several years for the various parties to negotiate and agree on an approach to such a framework., delaying

research and demonstration activities. Furthermore the necessary scientific knowledge to create effective centralized control still needs significant development, progress towards which could be made in the near future by several moderate scale experiments. As the Canadian Review states, “no single, isolated fertilization event is likely to lead to ecological catastrophe” (Sec 5.5). Therefore centralized control infrastructure should not delay the next round of experiments.

On the issue of a moratorium on commercial OIF, the Canadian Review suggests that this would provide time for research efforts to prove the viability of OIF, and “eliminate the economic incentive that is currently driving iron fertilization proponents, at least until the technique is proven and its likely impacts are better understood” (Sec 5.9). We disagree with the idea of a moratorium for several reasons and have discussed this at greater length in a review of commercialization concerns. First, we believe that the technique has been sufficiently proven through research experiments and will be sufficiently developed in workshops 7 to expect an accurate measurement of carbon sequestration, as well as avoidance of any significant negative impacts. Second, Climos believes that a moratorium on commercial projects would result in shifting the burden for commercial development entirely to the public sector, an inappropriate funding model. Even if governments were willing to undertake development of a technology for private sector deployment and profit, the pace of government funding would be substantially slower than that in the private sector and would needlessly delay understanding of whether OIF is an acceptable mitigation strategy for the accelerating impacts of anthropogenic CO₂. We believe that these two arguments are the most important considerations for rejecting the idea of a moratorium until impacts are known.

The Canadian Review suggests conditions under which a moratorium would be lifted. “If such a moratorium were ever to be lifted, it is proposed that the obligation to verify that carbon has in fact been sequestered, and to quantify other offsetting impacts should rest with the party claiming the credits.” (Sec 5.9). We believe that there is no *a priori* reason that a commercial project would not be able to satisfy these conditions, and indeed this is the standard practice for carbon offset project developers. The burden of proof has always rested with the project developer, including 3rd party review and the open publishing of the carbon offset methodology. Furthermore, Climos plans to take the burden of proof beyond the norm by hiring independent scientists to conduct all measurements during the project, and by openly publishing all data collected during the cruise.

3. The desirability of bringing to attention of other international instruments and institutions proposals for such operations.

Regarding the desirability of bringing other regulatory instruments and institutions that would, “enhance the ability to control and monitor iron fertilization activities”, it is important to ensure that all OIF activities are required to demonstrate both scientific integrity and carbon sequestration accuracy, and to ensure that no unacceptable environmental harm occurs. To the extent that other institutions may fill important gaps, then this may be desirable. However, we would also strongly recommend that unnecessary regulatory overhead be avoided so that important research into OIF may proceed. This sentiment is also shared by the International Ocean Commission’s submission to the LC/LP Scientific Group:

We should promote better scientific understanding of the ocean. Manipulative experiments, including ocean fertilization, are important tools that scientists use to develop a better understanding of the marine environment. Such scientific research should be promoted with a minimum of additional bureaucratic burden. For example, the scientists conducting the experiment should be free to decide which parameters (beyond those required to assure the detection of any significant

environmental damage that might reasonably be anticipated to occur) need to be measured to address the questions motivating the experiment. [IOC, 2008]

We believe that there is no need to “inform Kyoto and its successor... to suggest the removal of iron fertilization from the list of practices eligible for carbon credits” (sec 5.11). Neither the Kyoto Protocol nor its two flexible mechanisms, Joint Implementation and the Clean Development Mechanism, currently recognize carbon credits from OIF –or have the structural ability to do so as OIF projects take place outside territorial boundaries. In fact, no regulatory carbon framework in existence currently does so. Until there is a change, all OIF credits would be sold in the voluntary market, likely under one of the emerging voluntary carbon standards. We believe that the Voluntary Carbon Standard (the VCS) developed by the World Bank and the International Emissions Trading Association represents the highest quality standard available, and would seek certification under that framework.

Conclusion

We believe that we have made substantial effort to:

- 1) Understand the history of biological export and its relationship to OIF, the most recent experimentation and the insight it provides concerning the “efficiency” of the biological pump, the details of the OIF experiments, and the degree to which their results were designed to answer questions of sequestration;
- 2) Understand the research related to the environmental impacts of OIF; especially trying to avoid inappropriate extrapolation of results – for example, the use of coastal zone data where it is inappropriate to the open ocean; the inappropriate extrapolation of results from small experiments to larger scale activities; and the extrapolation of conflicting measurements to a single conclusion;
- 3) Understand the most recent complex ocean biogeochemistry/ecology models and the applicability of models to the problem of understanding ocean iron fertilization and the limitations of models; especially trying to understand the limitations of models that do not mimic ecological processes but parameterize them (for good reason).
- 4) Identify the limitations of OIF, for example, focusing on the regions that would result in the lowest risk for negative environmental impact; and
- 5) Exceed industry standard practice for carbon offset project developers to pursue a rigorous and open methodology with 3rd party measurement and verification of claimed carbon credits.

Thus we disagree with the statement that,

Very little can be said with certainty when it comes to iron fertilization, its effectiveness as a sequestration technique, or the likelihood and severity of its potential impacts. For this reason a precautionary approach is strongly advised. This view is reflected in the peer-reviewed literature (Chisolm et al. 2001; Furhman and Capone 1991; Johnson and Karl 2002; Reay et al. 2007). (Sec 7.2)

As we have demonstrated, there are several decades of scientific information from which to determine the efficacy and safety of OIF, as well as significant advances in recent scientific literature that encourage further research. While we agree that a cautious approach is valuable, this should not preclude commercial participation in the research and development of ocean iron fertilization.

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