Peer review of studies for desalination plant discharge, Cockburn Sound

NIWA Client Report: AUS2005-002
April 2005

NIWA Project: NAU05215

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Prepared for

EPA Services Unit, Department of Environment,
Western Australia

NIWA Client Report: AUS2005-002
April 2005

NIWA Project: NAU05915

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Executive Summary

The Water Corporation of Western Australia propose to build a 180 ML/d desalination plant adjacent to the Kwinana Power Station on the shores of Cockburn Sound. The plant will discharge hypersaline return water back into Cockburn Sound. A number of concerns have been raised about the potential environmental effects of the discharge and in particular the potential for changes in the stratification and dissolved oxygen regimes in the deep basin of Cockburn Sound. Such changes could in turn, result in disruption to biogeochemical cycles and induce undesirable ecological changes.

The Water Corporation commissioned a number of studies to address the above issues. This peer review assesses the adequacy of these studies to address the concerns that have been raised, and the level of confidence that can be placed upon the conclusions of those studies.

The studies encompass modelling the stratification and dissolved oxygen issues, determining the sediment oxygen demand and contaminant releases, and assessing ecological effects. They are separate pieces of work but interlinked and cross-referenced (i.e., each study references another in support of a conclusion). Individually, the studies have been professionally carried out to a (generally) high standard. We were not privy to the brief given to the organisations conducting the studies, but it is clear that they were under significant time pressure and were constrained to using (with the exception of the sediment oxygen demand measurements) existing data.

We thought that in general van Senden and Miller (2005) have done a good job, and have fairly and clearly identified the main issues involved. They have made good use of available data and results from previous 3-D modelling to derive parameter estimates and inputs for their own simplified box model, and in this respect have generally been conservative. While their model results cannot be validated due to lack of adequate observational data and simplifications inherent in their model, they have undertaken sensitivity analyses that do allow some inferences to be drawn regarding uncertainty of their results. However because of the limitations inherent in the model (which we discuss) we think that the results for mixing and oxygen concentration are best expressed in a *relative* sense (i.e., as a comparison with existing conditions) rather than in an absolute sense as actual numerical values. We therefore don’t have a lot of confidence in the actual values presented – e.g., that expected minimum oxygen levels remain above 4.0 mg L^{-1} (and could drop to around 3.5 mg L^{-1} under a worst case scenario during longer calm periods), and that the maximum duration between mixing events is 11 days. This in turn makes for difficulties with the use of the modelling results to assess ecological consequences and set licensing conditions. Our lack of confidence in this regard is exacerbated by the period over which “worst case conditions” are modeled (we believe the calm conditions constituting ‘worst case’ are more frequent and sometimes longer than is implied by the use of this term) and by using the sediment oxygen demand rate coefficient derived by Read and Oldham (2005) in which we also have a lack of confidence.

Our lack of confidence in the absolute values predicted by the box model notwithstanding, we also believe that only a coupled 3-D hydrodynamic-ecosystem model would yield a significant improvement in predictions and would require a major field data collection program for proper validation.

The ecological effects report by Lord & Associates (2005) is a generally well-presented assessment of the potential environmental effects of discharging return water from the desalination plant to Cockburn Sound. The report relies heavily on the modelling work of van Senden and Miller (2005) and the offsite work on sediments cores (Read and Oldham, 2005) to make conclusions on likely ecological effects. The question as to whether the discharge will cause significant ecological effects is in essence the final plank in the assessment. Unfortunately we consider that the supports holding up the plank have weaknesses. These weaknesses are largely unrecognised by Lord & Associates (2005) and the information/predictions from the accompanying studies are propagated in their conclusions. While Lord & Associates (2005) do make well-argued recommendations for further studies on macrobenthos and in situ sediment chambers (that we support) they make no comment on the apparent ammonium efflux from incubated cores (Read and Oldham, 2005) and potential flow-on effects on phytoplankton production, light climate and consequent ecological effects. In addition while they identify a potential impact of the concentrate discharge on light climate, their subsequent analysis is very superficial and unsupported by any review of light penetration or other optical measurements on the Sound. Consequently the conclusion that there will be no change in light climate has little credibility. Overall we believe that the conclusions of the Lord & Associates (2005) report are too firmly stated, given the appreciable uncertainty about the type and severity of environmental impacts.

Read and Oldham (2005) aimed to determine the sediment oxygen demand in the deep basin of Cockburn Sound. They also aimed to quantify the release of nitrogen species and iron upon achieving anoxia in the sediments. While they did develop a sediment oxygen demand relationship based on ambient dissolved oxygen concentrations, the integrity of this relationship does come into question due to (i) interpolation of the relationship based on all data points, (ii) the invasive nature of the aeration of the cores, and (iii) lack of details regarding the transport of the cores (i.e., core integrity) and the overall QA/AC program employed. We were unconvinced by the studies on nitrogen and iron release and the discussion (or lack of it with respect to ammonium efflux) leads us to the conclusion that the results of this study are inadequate for the purpose intended. Ultimately, the relationships between the overlying water column dissolved oxygen concentrations and changes to nitrogen cycling needs to be addressed in order to determine critical thresholds.

Overall we were not convinced that the studies address all of the concerns raised adequately, nor do we believe that the conclusions of the reports (namely that there will be no, or negligible ecological effects) can be accepted with a high degree of confidence.

1. Introduction

The EPA Services Unit commissioned NIWA Australia to undertake a peer review of studies that assessed the potential effects of discharging hypersaline return water from a proposed 180 ML/d desalination plant, on the environment of Cockburn Sound. The principal issue addressed by this suite of studies is whether the discharge of return water will cause any change in stratification and dissolved oxygen regimes in the deep basin of Cockburn Sound, which in turn could result in significant biogeochemical or ecological effects.

These issues were addressed by separate but interlinked studies commissioned the Water Corporation; the proponent of the desalination plant. Reports from these studies submitted for review were:

1. Van Senden, D. and Miller, B.M. Stratification and dissolved oxygen issues in Cockburn Sound pertaining to discharge of brine from desalination. WRL Technical Report 2005/03. Water Research Laboratory, University of New South Wales 67 pp.
2. Worley Pty Ltd. Perth desalination plant: Stratification and dissolved oxygen issues. 188 pp.
3. D.A. Lord & Associates Pty Ltd. Ecological assessment of the effects of discharge of seawater concentrate from the Perth seawater desalination plant on Cockburn Sound. Report No 05/028/1 37 pp.
4. Read, D.J. and Oldham, C.E. Sediment oxygen demand in the Cockburn Sound deep basin. Final Report. Centre for Water Research, University of Western Australia. 19 pp.

All reports were dated March 2005. Reports 1 and 2 above are interlinked, with the Worley report providing support to the Van Senden and Miller study. After consultation with EPA, we reviewed the Van Senden and Miller in detail (as the main modelling report) but referred extensively to the Worley studies as appropriate.

1.1 Scope of Work

We were requested to peer review the above technical studies, giving particular attention to:

- the inclusion and treatment of relevant hydrodynamic, biogeochemical and ecological processes;
- the adequacy of environmental characteristisation and key parameter estimation for the purposes of these studies;
- the appropriateness of investigation design, assumptions used and application of methods, including validation;
- the interpretation of results, and whether the conclusions are justified by the material presented in the report; and
- the level of confidence/uncertainty that can be placed on the conclusions, including the ecological change predicted.

2. The modelling studies (Van Senden and Miller, 2005)

Reviewer. Dr Bob Spigel.

2.1 Introduction and overview

Van Senden's and Miller's draft report addresses the question: Will brine discharge from the proposed Perth Desalination Plant adversely affect the dissolved oxygen regime of Cockburn Sound, and if so, by how much? Since the dissolved oxygen regime in turn exerts an important influence on biological and chemical processes in the sound, from cycling of nutrients and metals to quality of fish habitat, the results of this study underpin the other reports that assess impacts on water quality and various aspects of ecosystem health.

The short answer, according to the report, is "Yes", there will be some effect on the oxygen regime, but it is likely to be very small.

In arriving at this result, van Senden and Miller make use of a body of work carried out prior to their study: field measurements from the 1990s (DEP, 1996; D'Adamo, 2002), supplemented by more recent limited field work (Read and Oldham, 2005; Pattiaratchi 2005; Worley 2004); together with results of three-dimensional (3-D) hydrodynamic numerical modelling completed in 2004 (Worley, 2004; Worley 2005). Van Senden and Miller use this prior work to provide justification for their own model design, and to supply values for ambient stratification, dissolved oxygen depletion rates, and near-field/intermediate-field dilution factors for the brine plume from the desalination plant. The final outputs are figures showing a one-year time series of modeled oxygen concentrations, and tables of frequency distributions for modeled dissolved oxygen; both the figures and tables refer to concentrations near the seabed in the deep waters of Cockburn Sound, and compare the cases of "with" and "without" the discharge of brine from the proposed desalination plant.

Because of its very simple design and the assumptions it makes about the physical structure of the water column in Cockburn Sound, I do not think that Van Senden's and Miller's model gives results that are predictive in any absolute sense, i.e., it does not predict time series of oxygen concentrations that one could expect to go out and measure or compare against existing data for purposes of validation. Rather, its results should only be interpreted in a comparative sense. Moreover, adequate data for validation of predicted time series does not exist.

The following review follows the structure outlined by EPA Services Unit in their Scope of Work.

2.2 Inclusion and treatment of relevant hydrodynamic, biogeochemical and ecological processes

In order to predict dissolved oxygen levels near the seabed, it is necessary to quantify the balance between depletion of oxygen by biochemical processes, versus the resupply of oxygen by horizontal advection and vertical wind mixing. The desalination plume has two opposing effects on oxygen concentrations above the seabed. On one hand, being saturated with oxygen after dilution at the diffuser, the plume supplies oxygen to the bottom waters by advection. On the other hand, being generally denser than the waters of the Sound, the plume contributes to oxygen depletion by adding to the strength of density stratification that must be overcome by wind-mixing in order to reoxygenate bottom waters.

The processes that must be considered therefore include:

1. initial dilution of the plume by the diffuser (near-field mixing);

2. subsequent dilution as the plume flows as a gravity current along the bed of the shallow eastern basin, possibly confined to the Calista Shipping Channel (termed “intermediate-field mixing” by Van Senden and Miller);

3. further dilution of the plume as it flows down the steep side of the deep basin and then forms a bottom layer over the bed of the deep basin or possibly an intermediate-depth intrusion layer (far-field dilution);

4. density stratification and mixing of the water column by hydrodynamic processes within the main basin of Cockburn Sound;

5. depletion of oxygen within the water column and at the sediment-water interface in the main basin of Cockburn Sound.

Because van Senden and Miller have clearly documented their mixing model and provided source code for it, it has been possible to determine how each of the above processes has been treated, as described below.

2.2.1 Initial dilution

Initial dilution of the plume is based on Worley's (2004) outfall design, which in turn is based on a paper by Roberts et al. (1997), whose work on jets and plumes is well known and widely used. Roberts et al.'s (1997) paper applies to a single jet and does not take account of possible reductions in dilution from interference of multiple jets issuing from a multi-port diffuser. However, the possibility of reduced entrainment due to multiple jets is noted by Worley (2004), and it appears that this has been checked in their design. Worley (2004) give an initial dilution factor of 45, resulting in a salinity excess $\Delta S = 0.8$ psu at the edge of the mixing zone, in a gravity current 2.5 m thick moving downslope away from the diffuser toward the main basin.

Van Senden and Miller state (Executive Summary and p. 26): "on the conservative basis we would expect that the initial dilution be in the range 45-60." It is not clear where the value of 60 comes from. However, it appears that a value of 45 has been used as the basis for Van Senden and Miller's "worst-case scenario", which agrees with Worley's (2004, p. 24) specifications.

2.2.2 "Intermediate field" dilution

Van Senden and Miller use output from the 3-D Environmental Fluid Dynamics Code (EFDC), extracted from 2004 model runs by Worley (2004), to estimate dilution of the brine plume that takes place as the plume flows as a gravity current over the bed of the shallower eastern basin to the "drop-off point", the start of the relatively steep slope leading to the main deep basin of Cockburn Sound. This is the only place in the study that output from the 3-D model is used directly.

The EFDC model does show that during periods of weak wind, practically no dilution occurs in the intermediate field. This gives me some confidence in the EFDC model, as some numerical hydrodynamic models can be overly diffusive. One can do an independent check on the likelihood of mixing of the submerged plume in the eastern basin under calm conditions. A simple calculation (Simpson, 1997, p. 176) based on a gravity current thickness of 3 m, total water depth 10 m, ambient salinity 36.5 psu, salinity excess in the current of 0.8 psu, gives a Richardson number (Ri) for the gravity current of approximately 0.5, indicating that the current will be quite stable. In the absence of energy available for mixing from winds or ambient currents, the current is unlikely to experience much dilution in the intermediate field ($Ri < 0.25$ for instability; Turner, 1973).

The EFDC simulations predict that in the presence of strong winds and ambient currents, however, the intermediate-field dilution can be substantial. This is also realistic.

Van Senden and Miller effectively assume no dilution in the intermediate field for their “worst case” scenario. This is reasonable. However, the term “worst case” should not be taken to mean “extremely rare” – it is what one would expect under calm conditions and extended periods of weak winds.

For their “most likely” scenario, van Senden and Miller allow for a dilution in the intermediate field on the order of 1.5.

2.2.3 Far field dilution

Before the plume is inserted on the bed of the deep basin of Cockburn Sound, van Senden and Miller assume that the plume undergoes further dilution as it flows down the steep slope from the “drop-off” point to the deeper basin. They use theoretical results from Bonnecaze and Lister (1999; an extension of earlier, widely-used work by Ellison and Turner, 1959) to predict a further dilution down the slope by a factor of 1.3. This calculation is documented in an appendix to their report; the parameter values and final result seem reasonable.

Hence van Senden and Miller arrive at a final dilution, from the diffuser to the bottom layer in Cockburn sound, of 60 (≈ 1.3 x 45) for their “worst case” scenario and 90 (≈ 1.3 x 1.5 x 45) for their “most likely” scenario.

Van Senden’s and Miller’s “most likely” scenario is slightly more conservative than Worley’s (2005) conclusion following their 3-D EFDC simulations of 150 days from 1 February to 30 June 2000: “In general, the plume is expected to be dynamic, subject to strong mixing on the shallow Jervois bank region, and periodic mixing in the deeper areas. The overall dilution of the discharge is predicted to be around 100 fold [cf van Senden’s and Miller’s 90] by the time the plume enters the deep areas, contributing to a density difference within natural variations. Some reduction in the number of full depth mixing events may result.”

We now turn to how van Senden and Miller assess the possibility that “some reduction in the number of full depth mixing events may result.” This is the core of their report and concerns their model of wind mixing and oxygen depletion.

2.2.4 Density stratification and mixing of the water column by hydrodynamic processes within the main basin of Cockburn Sound

Up to this point van Senden and Miller have used prior work (mostly Worley's) to fix some of the input values for their model. I will briefly describe what the model does, then review the assumptions.

Van Senden's and Miller's model idealises Cockburn Sound as a basin of constant depth and constant horizontal area (vertical sides), containing three horizontal layers of water – a wind-mixed, oxygen saturated surface layer; a middle ("baroclinic") layer consisting of water that has drained from the shallower eastern basin into the main basin because of its higher salinity (due to concentration by evaporation in summer-autumn); and a bottom layer of diluted brine.

Each layer has a fixed density, specified at the start of each model run, but the depth of each layer and the dissolved oxygen concentrations of the bottom two layers change for each model time step.

A run consists of a 120-day simulation using wind data from year 2000 as being "the 'worst' year on record" in terms of having the weakest winds (van Senden and Miller, 2005, p. 13). Model time steps are the time intervals between each wind-speed data point.

At the start of each time step, thickness of the bottom layer is increased due to inflow of diluted brine (180 ML day^{-1} x dilution factor described above). The thickness of the middle layer is also increased; by the inflow of eastern shelf water into the middle layer (varies depending on the thickness of the surface layer). The difference in depth between total basin depth and the combined thickness of the bottom and middle layer is the depth of the surface layer; the combined depth of all three layers cannot exceed the specified constant basin depth.

Following the adjustment of layer depths due to inflows to the two bottom layers, the depth to which wind mixing (and hence reoxygenation) penetrates is calculated using a formula based on the work of Pollard et al. (1973). Wind mixing only occurs if wind speed exceeds a threshold value of 5 m s^{-1}. This is appropriate for the Pollard et al (1973) formulation. Van Senden and Miller (p. 12) provide adequate justification for the use of the formula based on the expected mixing regime for the basin. They also cite "experimental work of D'Adamo (2002)" as justification, but D'Adamo (2002) does not include any experimental work. He does use the Pollard et al. (1973) formulation to assess the effects of isolated wind events on basin stratification.

2.2.5 Depletion of oxygen within the water column and at the sediment-water interface within the main basin of Cockburn Sound

At each time step, after inflows and wind mixing have been simulated, oxygen depletion in the bottom two layers is calculated. Separate calculations are made for the two layers. In the middle layer, “water column oxygen demand” is modeled as a first order process (i.e., depletion rate linearly proportional to the amount of oxygen present); with a rate coefficient made up of a base rate plus a rate that varies diurnally (accounts for reduced depletion during the day due to photosynthesis). In the bottom layer, in addition to water column oxygen demand, a “sediment oxygen demand” is modeled as a first-order process with a constant rate coefficient (no reduction in depletion for photosynthesis). The rate coefficients for the water column demand are based on oxygen measurements made during the Southern Metropolitan Coastal Waters Study (DEP, 1996); the rate coefficient for sediment oxygen demand is taken from the results of core studies reported by Read and Oldham (2005).

Oxygen depletion within the water column and at the sediment water interface involves many complex and interrelated biological and geochemical processes. These processes vary throughout the year as well as over a single day, depending on light, temperature, nutrient availability, and the types and quantity of algae, bacteria and other organisms that are present. To model such complex processes as first order depletions with fixed rate coefficients is clearly a gross simplification, but perhaps the only feasible approach. The only possible alternative may be a full ecosystem process model incorporating nutrients, phytoplankton and possibly zooplankton; this is discussed in more detail later. Van Senden and Miller seem to have been conservative in their choice of rate coefficients, but their choice does depend on the validity of the sediment core experiments of Read and Oldham (2005). Their sensitivity analyses (Table 4.4) also indicate that model results are sensitive to the value of depletion rate coefficient, especially at lower oxygen concentrations.

2.2.6 Limitations, assumptions and possible criticisms - Density stratification and mixing of the water column by hydrodynamic processes, and oxygen depletion

1. The model is one-dimensional – it cannot account for variation in mixing and oxygen depletion within the Sound, although it is clearly stated that weaker mixing and a greater potential for deoxygenation exists in the southern part of the Sound compared with the northern part. Hence the results need to be interpreted as in some sense a spatial average.

2. The use of fixed densities for the three layers over the 120-day simulation period is perhaps the single most “unphysical” aspect of the model. Densities in the Sound are variable, as shown in Figures 4.2-4.3. Moreover, van Senden and Miller point out that results are sensitive to the densities chosen, although the relative results (comparison of cases with brine versus without brine) do not change very much.

3. Temperature effects and heat exchange with the atmosphere are completely neglected. Although salinity may dominate density stratification, temperature does have some effect and this has not been discussed. It may be that in autumn the mixing caused by nighttime cooling offsets the stratification set up by solar heating during the day, but again this has not been assessed (see D’Adamo 2002, section 6.4, for an overview of this aspect).

4. Calculation of flow from the shallow eastern basin to the deeper main basin appears to be based on a momentum balance at the head of a two-dimensional steady gravity current with no energy loss (e.g., Benjamin, 1968) and as such can be expected to give only an order-of-magnitude estimate for the flow. The conceptual model itself for the exchange only applies to late summer and autumn, but this is consistent with autumn having been identified as the critical period for mixing. Moreover, exchange between the ocean and the Sound is not explicitly modeled.

5. The application of the wind mixing formula (Appendix A5 and Source Code) does not account for the change in density that occurs as deeper water is entrained into the wind mixed layer. In this sense van Senden’s and Miller’s model is conservative, since it will predict longer mixing times than if changes in density were accounted for.

6. The wind mixing routine does not include a time limitation on energy input, associated with Coriolis effects in large basins (Pollard et al. 1973, Eq 3.2) or end-wall effects in smaller basins. This could lead to greater mixing than would be the case if a cut-off time were included.

2.3 Adequacy of environmental characterisation and key parameter estimation for the purposes of these studies

Estimation of parameters by van Senden and Miller, such as dilution factors, oxygen depletion rates and mixing coefficients seem to have been done carefully, making the best use of information available to them.

There are some shortcomings in environmental characterization, as identified in points 1 – 4 in the previous sub-section. These all have to do with the simplifications and assumptions underlying the mixing model.

On one hand, I feel that these shortcomings restrict the confidence that can be placed in the absolute values of oxygen concentrations predicted by their model. For example, D'Adamo (2002), in his discussion of autumn stratification and mixing in Cockburn Sound (Section 6.4.3) also estimates frequency of deep mixing events. He uses the mixing model of Pollard et al. (1973), not as applied to a continuous time series of wind speeds, but rather to identify a range of wind speeds that could cause complete mixing during a single wind event. He also considers the effects of solar heating and nighttime cooling, and considers a range of density stratifications. His wind data was for the autumns of 1993 and 1994. He concluded that "extended periods (up to 24 days) can occur when typical strength vertical density stratification is not eliminated by the mixing action of wind-stress and penetrative convection. The analysis assumed average initial stratification condition, and therefore, for stronger density stratification the potential for extended periods of less than full-depth mixing may be even greater." D'Adamo's analysis is also documented in D'Adamo and Mills (1995). It can be compared with van Senden's and Miller's conclusion (p. 14 and Table 3.3) that "a typical period between vertical wind mixing events (i.e., the water column is fully mixed) is 6 days and a maximum is 11 days."

On the other hand, it seems likely that the relative results comparing the cases with and without brine probably would not differ very much given different parameter values (as shown in van Senden's and Miller's sensitivity analyses) or a more complex model. This is because "the energy from the winds that would mix through the baroclinic [middle] layer is sufficient to also mix through the diluted brine layer (which is only marginally higher in density).” (van Senden and Miller, p.29). This is consistent with Worley’s (2005, p. 42) conclusions based on their 3-D hydrodynamic model, that the effects of the desalination discharge on stratification on the Jervois Bank and deep basin regions of Cockburn Sound is likely to be minor, within natural variations, and of insufficient magnitude to effect the wind mixing processes in the Sound.”

2.4 Appropriateness of investigation design: Assumptions used and application of methods, including validation

Limitations associated with the investigation design, assumptions used and application of methods has been discussed above. The question of validation has been mentioned but will be considered more directly. The model predicts an annual time series for layer depths and dissolved oxygen concentration near the bed (Figures 4.4 and 4.5) and summaries of cumulative frequency for oxygen depletion (Tables 3.9, 4.1, 4.2). These are the outputs that I should think are subject to validation, in terms of comparing observed versus predicted quantities.

As far as I can tell, no validation has been done, either for the wind mixing predictions or for the dissolved oxygen predictions, for two reasons. First is that the field data necessary to carry out such validation is not available. Possibly Pattiaratchi’s (2005) study is an effort in obtaining such data, but his results are fragmentary. Second is that, due to the simplifications inherent in the box model (e.g., neglect of thermal processes, heat exchange with the atmosphere, water exchange with the ocean, and the parameterization of water and salt exchange between shallow and deep basins of the sound based only on late summer and autumn conditions), a comparison of model predictions with observations made in 2000 (the year for which wind data are used) would not be meaningful, even if data were available.

2.5 The interpretation of results, and whether the conclusions are justified by the material presented in the report

The results for mixing and oxygen concentration are expressed in two ways – in a relative sense, in terms of a comparison with existing conditions, and in an absolute sense, as actual numerical values. I think, in view of the limitations of the model as discussed above, the relative, comparative results are more meaningful than the absolute ones. The main relative result is that “the occurrence of low oxygen levels near the bed do not significantly change with the introduction of a diluted brine”, and that “in periods of calm winds when oxygen levels currently drop for short periods

(<11 days) the introduction of a diluted brine plume will cause the levels to drop by a further 1 to 2 mg L^{-1}" (p. 34). Leaving aside the fact that the actual level of "significant change" is not quantified, my own interpretation would be that while the discharge will certainly not improve conditions in the Sound, it will not have a major effect. There already exists considerable year-to-year variability in mixing frequency and strength (and hence presumably in minimum oxygen levels). I would expect the overall shift in median or mean measures of mixing and oxygen to be within the range of natural variation now observed. I think this conclusion is reasonably well supported by the box model and the results presented from the 3-D EFDC model.

However, I would have less confidence in the actual values presented – e.g., that expected minimum oxygen levels remain above 4.0 mg L^{-1} (and could drop to around 3.5 mg L^{-1} under a worst case scenario during longer calm periods), and that the maximum duration between mixing events is 11 days. (See also discussion below regarding level of uncertainty.)

2.6 The level of confidence / uncertainty that can be placed on the conclusions, including the ecological change predicted

It is difficult to quantify a level of confidence or uncertainty in the model predictions. Normally one could refer to validation results to derive some quantitative measure of how well the model performs.

Some measure of uncertainty can be inferred by comparing the absolute minimum predicted dissolved oxygen level of "3.5 mg L^{-1} under a worst case scenario" with the observations that are available. On the one hand, the predicted minimum level is consistent with results from cores presented by Read and Oldham (however we have some concerns on the validity of these estimates – see section 4). On the other hand, in situ measurements of oxygen showed oxygen levels as low as 1.9 - 3.8 mg L^{-1} in the southern part of Cockburn Sound in 1994 during one of the few intensive field campaigns that have been undertaken in the Sound (DEP, 1996, cited in DAL, 2005, p. 8).

Uncertainty associated with mixing frequency can be inferred by comparison with the predictions of D'Adamo (2002) cited earlier, that intervals of up to 24 days can separate full-depth mixing events (cf 11 days, van Senden and Miller). D'Adamo (2002) used the same mixing formulation as van Senden and Miller, although applied in a slightly different context.

Further information is available from the results of sensitivity analyses presented by van Senden and Miller, particularly with respect to wind threshold and density differences. The ranges of density difference (0.2 – 0.4 kg m^{-3}) and threshold wind speed (5 – 7 m s^{-1}) are both within observed or realistic ranges (D'Adamo 2002), and the variations in results are on the order of a factor of 1.5 - 2, consistent with the variability cited above for minimum oxygen and mixing interval.

Additional uncertainty arises from the choice of oxygen depletion rate coefficients for the water column and for the sediment. Sensitivity analysis (Table 4.4) shows that increasing the base water column oxygen decay rate from 0.07 day^{-1} to 0.12 day^{-1} causes increases in the number of days that oxygen concentrations drop below a given level. The variation is most marked (generally greater than a factor of 2) at lower oxygen concentrations, and becomes much less marked at higher oxygen concentrations.

Hence, I think caution needs to be exercised when using the mixing and oxygen levels predicted by van Senden and Miller as a basis for predicting ecological impacts.

2.7 Conclusions

I think that in general van Senden and Miller have done a good job within the limitations of time allocated for their work. They have fairly and clearly identified the main issues involved. They have made the good use of available data and results from previous 3-D modelling to derive parameter estimates and inputs for their own model and in this respect have generally been conservative. While their model results cannot be validated due to lack of adequate observational data and simplifications inherent in their model, they have undertaken sensitivity analyses that do allow some inferences to be drawn regarding uncertainty of their results. However, because of the lack of any explicit validation of their predictions, and because of simplifications inherent in their model, I feel that their results are not predictive in an absolute sense, that rather they should only be interpreted in a comparative sense.

Van Senden and Miller have clearly explained the way their model works, and provided excellent documentation that allows one to identify their assumptions and the details of their mixing and oxygen depletion algorithms.

I have discussed the extensive simplifications and assumptions inherent in their model. While their model is clearly simplistic, the question arises of what alternatives there are to the general approach taken by van Senden and Miller. I do not think that a more sophisticated one-dimensional model would be an improvement; one is still left with the unknown horizontal exchanges both with the ocean and between the deep and shallow regions of the Sound. A laterally averaged two-dimensional model similarly could not predict all of these exchanges. A two-dimensional depth-averaged model cannot account for density stratification, which is crucial for the dynamics of the Sound.

Hence one is left with the alternative of extending the capabilities of the EFDC model to include oxygen dynamics. Two ways to do this would be to acquire a “water quality extension” to the model (Worley 2004, p. 26), or to modify the hydrodynamic code to include a tracer with a first order decay to simulate oxygen. Neither of these would be trivial to implement and would probably require extensive resources (especially in terms of time). In view of the small differences now predicted by the 3-D model for salinity and temperatures for the cases with and without brine, it would not be surprising if the 3-D oxygen predictions were also similar for these cases (as they are for the simple box model of van Senden and Miller.) Finally, I note that data available for validating the 3-D hydrodynamic model seem to be extremely limited, and I expect that this difficulty would be an order of magnitude greater if water quality predictions had to be included. A significant improvement in predictability cannot be expected without a major commitment of time and resources to implement the modelling and to obtain the necessary field data.

In his comprehensive study, D’Adamo (2002, p.1) describes Cockburn Sound as “a poorly flushed coastal basin requiring remedial action to redress ecological problems caused by the chronic input of contaminants from industrial and domestic discharges over the past 30-40 years.” While I think that van Senden and Miller’s conclusion that the desalination discharge is unlikely to have a major effect is reasonable, the discharge will certainly not improve conditions in the Sound. In view of the simplifications inherent in their model and the lack of data to validate such a model, I think caution needs to be exercised when using their predicted mixing and oxygen levels as a basis for predicting ecological impacts. Van Senden and Miller in fact recommend that their conclusions “be assessed through ongoing monitoring of DO levels in the deep basin of Cockburn Sound.”

Finally I wish to call attention to a cautionary note sounded by van Senden and Miller in the main body of their report, which does not seem to come through very clearly in their conclusions. In their conclusions, van Senden and Miller suggest a possible “adaptive management scheme” that involves “introducing periods of desalination plant shutdown and/or higher dilution if trigger DO levels are achieved. It is concluded that timely plant shutdown is an effective means of mitigating the lowest oxygen events should such events be exacerbated by the operation of the desalination plant.” Such a strategy is in fact examined in their report, where it is reported that (p.32): “The results … indicate that under likely conditions this would assist, *however, under worst case conditions to remove the oxygen stream carried in by the brine would make matters worse*. [italics added]” As noted above, “worst case” conditions apply whenever there is an extended calm period. This implies that by the time low oxygen levels are detected, stopping the brine discharge could actually make matters worse if conditions remain calm

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3. The ecological implications (D.A. Lord & Associates, 2005)

Reviewer. Dr Rob Davies-Colley

3.1 Introduction and overview

The report by Lord and Associates Pty Ltd (2005) provides a fairly comprehensive assessment of the ecological impacts of discharge of wastewater (seawater concentrate, potentially containing some biocides as well as seawater salts and suspended solids) from the proposed Perth desalination plant to Cockburn Sound. The report relies heavily on modelling of plume behaviour and resulting dissolved oxygen (DO) in the Sound (refer van Senden & Miller 2005), and (offsite) work on sediment cores (Read & Oldham 2005), both of which are reviewed elsewhere in this report. Otherwise the Lord and Assoc. report is based mainly on a critical review of the literature (Chapter 2).

The report seems generally well-designed, with appropriate discussion and analysis. A simple, but effective, schematic diagram (Figure 4.1, repeated in the Executive Summary) is noteworthy. Possible impacts of the brine concentrate discharge are categorised as:

- effects of salinity increase;
- effects of stratification and resulting lowered dissolved oxygen (in bottom waters and underlying sediments); and
- effects of changes in light climate.

Unfortunately, the Lord & Associates report has numerous minor technical errors that should have been picked up in proof reading. For example, in Table 1.1 TDS is given in units of ppm, not ppt as stated. And salinity (measured on the practical salinity scale) is not "numerically the same value as TDS" as stated in the footnote on p 2[1].

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[1] Salinity is a (dimensionless) ratio of conductivities reflecting ionic content of water, whereas TDS is residue after evaporation of a filtrate. Both TDS and salinity may be considered indices of the salt content of waters, but although numerically similar they are not identical.

More importantly, we believe that Lord & Associates too readily dismiss potential ecological effects of the desalinisation plant discharge, particularly as regards reduced bottom water dissolved oxygen. Furthermore, the report's conclusions as regards possible change in light penetration due to the discharge are dubious because the analysis of light penetration is very superficial and no optical information on the Sound and hypersaline discharge was presented.

3.2 The inclusion and treatment of relevant hydrodynamic, biogeochemical and ecological processes

Relevant processes have been identified and considered. Potential effects of the hypersaline discharge that have been analysed include: **salinity change** causing osmotic stress, reduced **light penetration** to the sediment bed, and **stratification and bottom water hypoxia** (and related potential sediment anoxia and release of contaminants). Bottom water hypoxia is identified as the single greatest threat. We do not consider that the possibility of change in light climate has been given adequate consideration (see 3.3).

3.3 The adequacy of environmental characterisation and key parameter estimation for the purposes of these studies

We believe that the issues with the desalinisation plant discharge are correctly identified. However the analysis of effects on light climate in Cockburn Sound is very superficial (and unsupported by any review of light penetration or other optical measurements on the Sound). Increased phytoplankton biomass in Cockburn Sound is identified as being a particular issue with regard to increases in bioavailable nutrients because this has historically caused destabilisation of seagrass communities (p 11). Lord & Assoc. do not cite any reports or papers making this connection, but the typical effect of phytoplankton on seagrasses is via light attenuation (by phytoplankton) reducing light at the sediment bed for seagrass photosynthesis. A more complete analysis of light penetration into Cockburn Sound as it may be affected by desalination plant discharge seems called for. The data given by Read and Oldham (2005) suggest that there is potential for sediment nitrogen release which would most likely increase phytoplankton biomass and reduce light penetration.

Lord & Assoc (2005) recognise the possibility that the desalination plant discharge may reduce light penetration to the bed of Cockburn Sound. They review the change in refractive index with salinity, and conclude (correctly) that a change in refractive index (alone) will not affect light attenuation, because dissolved salts do not absorb or scatter light. However, they ignore the fact that (light-attenuating) solids filtered out of the seawater intake will be returned with the concentrate discharge (Table 1.1) and may be expected to increase light attenuation in that discharged water. Furthermore, induced stratification may slow the settling of (light attenuating) suspensoids resulting in reduced light penetration to the bed of Cockburn Sound. (Turbid layers due to concentration of slowly settling suspensoids are commonly observed by SCUBA divers and by profiling with optical instruments at density interfaces in waters.)

No optical information for Cockburn Sound is presented and no attempt is made to analyse optical properties in relation to other aspects of water quality (phytoplankton concentrations) in the Sound water – guided by a standard text such as that of Kirk (1994). Overall the assessment of optical impacts is very superficial.

The review of desalination processes identifies biocides as an issue as well as brine, but no information is given about potential biocide use (if any) at the proposed Perth desalination plant.

Lord & Associates (2005) rely heavily on van Senderen and Miller (2005) and Read and Oldham (2005) in their report on potential ecological effects, and their interpretation of the results of these studies is at times misleading. For example Figure 2.1 (from van Senderen & Miller 2005) suggests that DO in the (stratified) bottom waters of Cockburn Sound may be about 1 ppm lower than without the desalination plant discharge. However, the discussion on p7 (Lord & Associates, 2005) interpreting this information states that "low DO conditions (<4 mg/l) are unchanged by brine discharge….". Actually, the probability of < 4 ppm DO would surely be increased by the brine discharge, albeit still rare. More useful than Table 2.3 would be a *graph* of probability of occurrence versus DO with and without the desalination plant discharge. Such a graph would indicate at least the theoretical probability of increased incidence of low DO conditions with the brine discharge.

Further on the issue of low dissolved oxygen, a report by DEP (1996, as cited in Lord & Associates 2005) is quoted on monitoring of DO in calm weather (p 8). Bottom water DOs as low as 1.9-3.8 mg/l were recorded in areas of apparently high sediment oxygen demand, and a little higher DO in deeper water (as low as 4 mg/l). Given that modeled DO in the presence of the discharge is typically about 1 ppm lower than without the discharge (van Senderen & Miller 2005, Figure 2.1), we might expect DOs as low as 3 mg/l at the (same) deeper sites with the discharge present.

A quote attributed to the DEP (1996) report is given on p8: “Any change in the hydrodynamic regime which restricts oxygen replenishment of deep basin waters (e.g., increased vertical stratification) will increase the likelihood of sediment anoxia events”. No commentary is made by Lord & Associates (2005), but we interpret this to mean that the increased stratification that will be induced by the desalination plant discharge may well increase the frequency of low DO events.

3.4 The appropriateness of investigation design, assumptions used and application of methods, including validation

Generally the analyses made are appropriate. An exception is the investigation of reduced light penetration as a result of discharge of concentrate from the proposed desalination plant which is very superficial, as discussed above.

Lord & Assoc (2005) rely heavily on the Read & Oldham (2005) report on an experiment to measure oxygen demand of (and material release from) sediments in cores taken from Cockburn Sound (p 8-12). The key processes investigated were metal (iron and manganese) and nutrient (phosphorus and nitrogen) releases into overlying water. The Read & Oldham (2005) experiments did not measure DO levels below 3.5 mg/l and, not surprisingly, under these (still well-oxygenated) conditions, iron and phosphorus were not released. The assumption is made that similar conditions will prevail *in situ*. However, biogeochemistry of nitrogen (unlike P) can be affected by relatively subtle changes in oxygen conditions, so the conclusion that nutrient release will be low seems too ‘dismissive’, at least as regards nitrogen.

3.5 The interpretation of results, and whether the conclusions are justified by the material presented in the report

The conclusions are justified as regards nil effects of (very slightly) elevated salinity, but are less robust as regards effects of stratification and resulting hypoxia of bottom waters. We believe that, contrary to some ‘dismissive’ statements in the report, there will be <u>an increased probability</u> of low DO in bottom waters due to the desalination plant discharge.

The analysis of effects on light climate in Cockburn Sound is very superficial (and unsupported by any review of light penetration or other optical measurements on the Sound), consequently the conclusion that there will be no change in light climate has little credibility. We think there may be somewhat reduced lighting at the sediment bed – with possible ecological effects.

The report makes valuable and well-argued **recommendations**, viz:

- *In situ* benthic chamber experiments should be employed to study the sediment response to low oxygen conditions.

- A comprehensive survey of the macrobenthos of Cockburn Sound should be carried out.

We agree that *in situ* benthic chamber experiments are highly desirable in order to assess the response of sediments to low oxygen conditions that may be promoted by stratification due to the concentrate discharge. The *ex situ* experiments of Read & Oldham (2005) on sediment cores are less robust as a basis for predicting ecological effects of the concentrate discharge. We also agree that a comprehensive macrobenthos survey should be conducted annually in Cockburn Sound. That is, the macrobenthos could be used as an *ecological indicator*, based on surveys to a standard design, started *before* the desalination plant is commissioned so as to provide a baseline. We would recommend, further, that this macrobenthos survey should incorporate 'control' sites (*un*affected by stratified bottom waters) as well as 'impact' sites.

Lord & Associates (2005) summarise the ecological effects at different ranges of concentration (mainly) of DO in bottom waters. Uncertainties about the modelling notwithstanding, the predicted bottom water DO range is expected to fall from 4.5-8 to 3.5-8 (minima dropping by about 1 mg/l). To us this implies that, contrary to statements in Chapter 2 (e.g., p 7), the frequency of low DO 'events' *will* be increased by the desalination plant discharge. Lord & Associates (2005) also cite several biological surveys of Cockburn Sound suggesting that the benthos is structured by dissolved oxygen conditions in bottom waters (p 13). They conclude (usefully, p 16) that "increased frequency and severity of low oxygen conditions will….reduce species richness and [increase] dominance by small opportunistic highly fecund species". This suggests to us that lowered DO is the single greatest potential threat to the ecology of Cockburn Sound posed by the desalination plant discharge.

Given the importance of bottom water DO, we are surprised that the authors do not recommend (as we do) continuous DO monitoring (and associated alarm systems) to avoid hypoxia events induced or exacerbated by hypersaline discharge from the proposed desalination plant.

3.6 The level of confidence/uncertainty that can be placed on the conclusions, including the ecological change predicted

In our view the **conclusions** of the report are too firmly stated, given the appreciable uncertainty about the type and severity of environmental impacts. We agree that the small expected salinity increase (< 0.5 PSU in the far field) seems unlikely to cause any direct (osmotic) stress on organisms in the Sound. However, possible effects of stratification and decreased dissolved oxygen in the bottom waters (with flow-on effects on benthos and on sediment biogeochemistry) are too readily dismissed by Lord & Associates (2005). At the very least the authors should have exercised a little more caution where they state (as a potentially misleading 'absolute') (p 24) "…the discharge of seawater concentrate…will not contribute to the exacerbation of low-oxygen conditions in Cockburn Sound.".

There can be little confidence in the conclusion that light climate will be unaffected by the discharge, because this is based on a very superficial analysis and no optical data for the Sound.

3.7 Conclusions and recommendations

1. That the recommendations by Lord & Associates (2005) outlined in Section 3.5, above be implemented.

2. That further analysis is conducted on the potential effects of the desalination plant discharge on the bottom water dissolved oxygen and light climate in Cockburn Sound.

3. That if the desalination plant proposal is accepted, then additional monitoring of *other indicators* of the ecological condition of Cockburn Sound be implemented, including: DO in bottom waters, light penetration into Sound waters, and (annual) surveys of the biomass and extent of seagrass meadows. Because the most severe potential effects are related to hypoxia in the bottom waters isolated by stratification, we recommend that bottom water DO is monitored continuously. An alarm system could be set up, triggered when DO falls below, say, 4 mg/l, possibly with a requirement to cease discharge from the desalination plant, at some still-lower DO, say 3.5 mg/l. Such monitoring would be consistent with the State Environmental Policy criterion of 60% DO for no more than 6 weeks.

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4. Sediment Oxygen Demand (Read and Oldham, 2005)

Reviewer Dr Daniel Spooner

4.1 Introduction and overview

Read's and Oldham's final report aimed to determine the sediment oxygen demand in the deep basin of Cockburn Sound. They also aimed to quantify the release of nitrogen species and iron upon achieving anoxia in the sediments. The short answer is that they did develop a sediment oxygen demand relationship based on ambient dissolved oxygen concentrations, but the integrity of this relationship does come into question due to (i) interpolation of the relationship based on all data points (i.e., a closer examination of the lower end of SOD would improve the analysis), (ii) the invasive nature of the aeration of the cores, and (iii) lack of details regarding the transport of the cores (i.e., core integrity) and the overall QA/AC program employed.

The secondary aim of the study clearly stated that nitrogen and iron release were the focus. There appears to be a lack of understanding regarding nitrogen biogeochemical cycling in marine sediments. This resulted in the omission of critically important observations (i.e., ammonia concentration changes and contribution to the nitrogen pool) and lead to the conclusion that this aim was not achieved. Ultimately, the relationships between overlying water column DO concentration and changes to nitrogen cycling needs further examination to determine critical thresholds that trigger management intervention. We recommend that manipulative experiments to assess the response of sediment processes to low oxygen conditions of sediment from the northern and southern regions of Cockburn Sound be undertaken. We support the use of *in situ* benthic chamber experiments for this purpose.

The following review follows the structure outlined by EPA Services Unit in their Scope of Work

4.2 Inclusion and treatment of relevant hydrodynamic, biogeochemical and ecological processes

There appears to be a lack of understanding regarding nitrogen biogeochemical cycling in marine sediments. There was an expectation that the carbonate sands in Cockburn Sound are unlikely to release nutrients and metals. The authors justified this statement by indicating that carbonate sands typically have very little hydrous oxides associated with them and therefore little contaminant release would be expected if the sediment did become anoxic.

These conclusions (and conceptual understanding) are based on phosphorus and trace metal biogeochemistry (of which phosphorus and manganese were not included in the report aims). The aim of the study clearly stated that nitrogen and iron release was the focus. Nitrogen biogeochemistry is significantly different to trace metal and phosphorus biogeochemistry. It involves a solid, dissolved and importantly a gaseous phase. The cycling of solid, dissolved, and ultimately the gaseous phases are regulated by bacteria, which are influenced by the physico-chemical environment within the sediment profile. Subtle changes in Dissolved Oxygen (DO) concentration will alter the nitrogen cycling pathways (unlike P, which requires anoxia for dissolution from particulate phases) and the balance between nitrogen loss (i.e., denitrification) and release of dissolved nitrogen (i.e., ammonia via anaerobic ammonification) into the water column is lost and ammonia release dominates. This appears to be occurring in the experiments undertaken in this study (i.e., Figure 5) and the ammonia concentration changes do not appear to be insignificant (i.e., up to 2.5 mg/L). Furthermore, it appears that the percentage of the total nitrogen pool occurring as ammonia increases throughout the incubation, and in some case the ammonia fraction represent all of the total nitrogen observed. This trend suggests that the sediment pore water dissolved oxygen concentrations are very low and anaerobic ammonification is dominant. This is further supported by the fact that there was very little NO_x (i.e., reducing conditions prevailed). This is a significant finding and requires further examination.

There appears to be an error in Appendix D. The Total Nitrogen (TN) concentrations are often far less than the Dissolved Nitrogen (DN) concentration (if DN = Dissolved Nitrogen), which is conceptually impossible.

4.3 The adequacy of environmental characterisation and key parameter estimation for the purposes of these studies

Generally, all the parameters selected for reporting are well founded, but there were several extras included (i.e., P, Mn) that appeared to cloud the overall interpretation of the results. The inclusion of free CO_2 would have provided some valuable insight into the respiration dynamics (i.e., in the report they claim that a lack of organic carbon in the sediment may have inhibited respiration - CO_2 data would help support his statement).

A major assumption in this report related to the sediment composition in Cockburn Sound. It would have been highly beneficial for the report to include sediment organic matter content as a key variable (liability assessment would have also been useful).

The investigation focused on the Cockburn Sounds deep basin, which may accumulate organic matter.

An indication of the *in situ* light regime at the sediment surface would have also been useful. This data would have indicated if photosynthesis is likely to occur at the sediment surface (i.e., this will influence the biogeochemical cycling of the nutrients and this would not have been represented in the core experiments).

4.4 The appropriateness of investigation design, assumptions used and application of methods, including validation

Studies based on collecting cores for laboratory-based experiments of biogeochemical processes are fraught with danger. *In situ* chamber methods provide a much more robust estimation of the biogeochemical processes occurring in the environment. They have been developed in recognition of (i) experimental limitations in measuring nutrient and other metabolite gradients over sub-mm scales, (ii) the existence of environmental microniches, complex microbial ecology, and supply and demand of food and energy sources at the sediment-water interface, and (iii) a variety of transport processes are responsible for moving solutes across the sediment-water interface, including the highly variable (if not unpredictable) activities of benthic fauna (Heggie, 1999). The *in situ* chamber technique also provides the opportunity to measure benthic fluxes *in situ* based on natural transport processes responsible for moving solutes between the sediments and the overlying water (Heggie, 1999). Heggie (1999b) also stated that benthic chambers are the only technique that provides true *in situ* light conditions and a best approximation of local seafloor hydrodynamics.

If laboratory based core experiments is the only option available, then the integrity of the sediment in the core is paramount for any confidence to be placed on the observed trends. The authors did indicate the procedure for capping and storing the core but did not indicate how they maintained the physical integrity of the cores while transporting them back to the laboratory. The 9.3 cm diameter of the core is a small area of the sediment surface for extrapolation to m^2 (i.e., 9.3 cm diameter = $0.0679m^2$). The nature of biogeochemical processes often results in large variation across sediment surfaces, therefore to capture this variation emphasis need to be placed on increasing the surface area of the sediment sample. The authors also mention that they tried to minimize air contact with the cores but any exposure is likely to have major implication for biogeochemical processes within the sediment.

The authors did not document how the site water was stored. This could have influenced the biogeochemical reaction rates (i.e., if the water was warmer). Moreover, the authors state that the cores were allowed to equilibrate to 19 °C but gave no indication as to why this temperature was chosen (i.e., the core temperature should have been maintained at the observed temperature of the benthic waters the cores were taken from).

The authors did not document how much water they removed from the cores for laboratory analysis, nor did they document any QA/QC procedures for the laboratory analysis. The authors also state that when they removed the core water they replaced it with site water and then aerated the core water for 5 minutes to saturate the water column. This procedure seems very invasive and the aeration action could have introduced the potential for major changes to the surface layer of the sediment, which is a critically important component that must remain stable during a biogeochemical study of this nature. Moreover, this procedure seems contrary to the second aim of the report (i.e., N and Fe release under anoxic conditions). It appears that this would require the core to be replenished with water of the same DO concentration to reduce the introduction of dissolved oxygen.

Table 2 does not indicate the phosphorus and manganese analysis schedule.

The authors report the Sediment Oxygen Demand (SOD) as a flux rate ($g/m^{-2}/d^{-1}$) which is typical of the biogeochemical literature. The details of this calculation are very brief, they include:

Consumption of oxygen (difference between days) X core water column height.

There was no core water column height data presented in the report (we assume that this term means volume of water in core). The authors do suggest that the cores were replenished with ‘site water’, but there is no indication whether this was taken into consideration when the SOD calculations were made. If so, more information is required to clearly document how these flux rates were calculated. Overall, the interpretation of all the results using linear and polynomial relationships was very simplistic. Closer examination of the lower SOD and DO concentration observations is required to improve the analysis. Therefore, caution needs to be placed on adopting the SOD relationship derived.

4.5 The interpretation of results, and whether the conclusions are justified by the material presented in the report

The interpretation of the results were reasonably sound for the first aim (i.e., SOD), but the linear interpolation of the SOD relationship needs to be treated with great caution due to the method of data analysis. The relationship was developed using a linear/polynomial line of best fit for all the data. The scatter of the data point in the lower portion of Figure 3 suggest that this approach is likely to yield a misleading relationship. Further investigation is required to explore the response of SOD to low benthic water oxygen concentrations. The inclusion of CO_2 analysis would have helped support some of the authors' thoughts for sediment respiration dynamics. The interpretation of the results relating to the second aim (N and Fe flux) were marginal. A lack of understanding of marine sediment nitrogen cycling has led to this situation. There was no mention of any of the critical nitrogen cycling pathways (i.e., ammonification, denitrification, nitrification).

The conclusion that nutrient release from the carbonate sands is low is not supported by the results. There was an observed increase in ammonia concentrations that appeared to become a significant component of the total nitrogen pool (i.e., TN).

4.6 The level of confidence/uncertainty that can be placed on the conclusions, including the ecological change predicted

Some confidence can be placed on the Sediment Oxygen Demand (SOD) assessment made by this report. But great caution needs to be placed on the adoption of the SOD relationships. To assume that SOD = 0 at ~ 4.2 mg/L oxygen may be very misleading. Further clarification on the calculation of the flux rates would be useful, and the inclusion of CO_2 would have also improved the author's interpretation of respiration dynamics. It appears that this study was designed to assess SOD, but the conceptual nature of assessing SOD was not clear. Ultimately, it appears that the experiment did provide a range of SOD based on various DO concentrations but the relationship between the two was interpreted in a linear fashion, which is unlikely to be the case. An improved estimate of SOD will be derived by employing *in situ* chambers.

There is, in our minds, great uncertainly on the conclusion that nutrient release from the carbonate sands is low and insignificant. The high ammonia concentrations observed support the notion that even though oxygen concentrations are above 3 mg/L in the water column the oxygen concentrations in the sediment porewater are likely to be much lower (i.e., leading to anaerobic ammonification – sulphate reduction - and ammonia release). The high ammonia concentrations observed in this study (up to 2.5

mg/L) can be toxic for some benthic fauna and the ammonia can fuel algal blooms in the water column. The relationships between overlying water column DO concentration and changes to nitrogen cycling needs further examination to determine critical thresholds that trigger management intervention. This conclusion is also supported by Lord & Associates (2005), who state the following recommendation that we also support:

> “1) undertake manipulative experiments to assess the response of sediment processes to low oxygen conditions, of sediment from the northern and southern regions of Cockburn Sound. In situ benthic chamber experiments are recommended for this purpose. This work will provide numerical triggers for parameters to be included in the Water Corporation Water Quality management Plan.”

4.7 References

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