Peer Review of Oceanographic Studies and Dredging Program Simulation Studies

Albany Port Authority
Albany Port Expansion Project

August 2008

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Appendices

1.0 Introduction

Two independent peer reviews were undertaken of the GEMS Pty Ltd 'Oceanographic Studies and Dredging Program Simulation Studies' Report. This was done as part of the verification of information for Albany Port Authority's Albany Port Expansion Project.

The first peer review was conducted by Dr Jason Antenucci (University of Western Australia (UWA) Centre for Water Research). The outcomes from this review are detailed in Section 3.0.

The second peer review was conducted by Kathleen McInnes (CSIRO Marine and Atmospheric Research). The outcomes from this review are detailed in Section 4.0.

2.0 Scope of Reviews

Both reviewing parties were requested to assess the report based on the following points and any others that were deemed relevant.

- Does the report as a whole addresses the key meteorological and oceanographic factors in Princess Royal Harbour and King George Sound which are relevant to assessing the outcomes of the dredging project?
- Is the meteorology used shown to be adequate?
- Are the hydrodynamic modelling predictions of tides and currents shown to be adequate?
- Are the wave predictions shown to be adequate for the needs of the dredge modelling?
- Are the affects on the exchange between PRH and KGS are treated adequately?
- Are the dredging simulations undertaken in an appropriate manner?

The reviewers were provided with a copy of the GEMS Pty Ltd 'Oceanographic Studies and Dredging Program Simulation Studies' Report, and open communications for clarification of queries as they occurred.

3.0 UWA Centre for Water Research Peer Review

A full copy of the peer review report is attached as Appendix 1.

3.1 Summary of Comments from the Review

The reviewer concluded that the GEMS report as a whole addresses the key meteorological and oceanographic factors in Princess Royal Harbour and King George Sound which are relevant to assessing the outcomes of the dredging project.

The reviewer raised a concern regarding the seemingly high sediment settling velocities used in the modelling. These velocities were obtained from the CSIRO Division of Minerals Particle Analysis Service in Perth through analysis of core samples. On further investigation by GEMS Pty Ltd, it was found that the settling velocities provided by CSIRO were not calculated in sea water (Refer to Appendix 3). After discussions with CSIRO the density and viscosity values were adjusted to those of sea water and the dredge model was rerun. The adjusted settling velocities have been incorporated in all subsequent modelling works.

In general it was found that the methods used in the modelling were sound, however many queries and suggestions for improvement were made. The ways in which these comments have been considered are documented in Appendix 2.

4.0 CSIRO Marine and Atmospheric Research Peer Review

A full copy of the peer review report is attached as Appendix 4.

4.1 Summary of Comments from the Review

The reviewer concluded that the GEMS report as a whole addresses the key meteorological and oceanographic factors in Princess Royal Harbour and King George Sound which are relevant to assessing the outcomes of the dredging project.

The reviewer raised several opportunities for improvements to the readability of the report. The ways in which these comments have been considered are documented in Appendix 5.

Appendix 1: UWA Centre for Water Research Peer Review Report

THE UNIVERSITY OF WESTERN AUSTRALIA

CENTRE FOR WATER RESEARCH

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CRICOS Provider Code 00126G

Our Ref: P08/143/JA

28 March 2008

Shannon Dolbel
Ecologia Environment
1025 Wellington St
West Perth 6005
Email: shannon.dolbel@ecologia.com.au

Dear Shannon

RE: Review of "Grange Resources / Albany Port Authority Port Development Oceanographic Studies and Dredging Program Simulation Studies", GEMS report 376/06

This letter constitutes a review of the above report. Below are detailed comments on the report that require addressing. Answers to the questions posed by Ecologia as the basis for the review are then answered based on these comments.

In general the work done seems to be sufficient to answer the questions at hand, however several aspects of the reporting of that work need to be improved. These type of projects are attracting increasing levels of media interest, due to a more educated public. This is placing increasing importance on clear, concise reporting of the work completed. Many of the comments below relate to improving the clarity with which the message is delivered.

I have two major concerns with the report. Firstly, the settling velocities in Table 6 seem to be very high, at least 40% higher than what could be expected theoretically. I don't believe this is possible. Secondly, the report contains no "editorial comment" on the best or worst time to conduct the dredging program. It is clear that GEMS understands the meteorology and

oceanography of this area as good as anyone, and so is well-placed to recommend the best time of year for dredging to take place.

Specific comments:

1. It should be stated that the GCOM3D model was run in barotropic mode, meaning that no consideration has been given to currents driven by density differences. A justification for this selection should be provided.
2. Section 2.4 – this section needs to be edited and formatted more professionally, the bullet list gives no context.
3. At no point in the report is there any comment about GEMS views on the best time for dredging. It seems clear from the dredge plume simulations that starting the dredging in March, as proposed, is actually the worst time of those investigated to begin the program.
4. Section 5.1 – SI units should be used throughout (eg m s^{-1} rather than knots).
5. Section 5.2 – It is stated in the third paragraph in this section that "occasionally" adverse conditions for surface waves occur. What is the approximate frequency of these "occasional" events – once a month, once a quarter? This needs to be quantified.
6. Section 6 – What were the deficiencies in the MetOcean programme, and how were they addressed by the field studies reported here?
7. Figure 6 – This needs to be improved, presumably the "small" and "large" annotated arrows point to the bottom weights, which have been offset to the left.
8. Section 6.1 – More information needs to be provided on the ADCP deployment, in particular the depth of deployment, the vertical resolution, sampling interval, sampling frequency, averaging time, and most importantly the instrument resolution and accuracy.
9. Section 7.1.1 – At what height is the MesoLAPS wind data given? Presumably 10m above some datum?
10. Section 7.2.1 – At what heights were the wind speeds measured by the AWS? If it was not collected at 10m (as I suspect), presumably it was converted to the same height as the MesoLAPS data prior to comparison?
11. Section 7.2.2 – "Selection of a Period of Representative Winds". The reason the easterly winds would be expected to result in higher turbidity levels should be clearly stated. Is it due to increased likelihood of resuspension due to increased surface wave height when the winds are from this direction, or reduced flushing due to wind setup, or some other mechanism?
12. Figure 7.1 – Figure 7.2. The figure captions need to be improved. From what I can gather, the upper panel shows the full record, and the lower panels show sub-sections of the same record.
13. I have some concerns that the MesoLAPS data shows lower wind speeds than that measured by AWS. This comes from simply comparing the number of wind events in the both data sets that exceed 10 m s^{-1} (Figure 7.2). In particular the lowest panel of this figure seems to show MesoLAPS data exceeding 10 m s^{-1} on approximately 16 occasions, whereas the AWS data exceeds 10 m s^{-1} on close to 100 occasions. At least two new figures should be included, the

first similar to Figure 7.3 showing the MesoLAPS vs AWS data in wind rose form, and the second similar to Figure 7.4 but for the AWS data so it can be compared directly with the energetic and low frequency components of the MesoLAPS data. Whilst is could be argued that the use of the MesoLAPS data would make the simulations "conservative" as the wind speeds are lower, the winds used still need to be representative.

14. Figure 7.4, Figure 7.5 – I don't understand why the years 1994 – 1999 and 2006 are included in both these figures if no data is shown in these years. Secondly, the vertical scale on both these figures should be the same (0 – 40%) to facilitate easier comparison.
15. Why were no simulation validation results presented for the period March – April 2006, even though this period was simulated? Data were presented for this period in Figure 8.12 – 8.17, but the equivalent model validation results also need to be shown as the dredge programme being suggested would also include the months of March and April.
16. Section 8.2 – the last sentence states "GCOM3D has been shown to represent these features with a high degree of accuracy." Whilst I believe the model is doing a good job, statements like this need to be backed up with some statistical quantification.
17. Section 9.1.1 – Similar to the previous comment, on what basis were the 'best verification results' determined?
18. Section 11.1 – there is a typographic error here, the cross-sectional areas should be reported in m^2 and not m^3.
19. Section 11.1 – Regarding whether dredging will change the exchange, and the statement that "The answer is almost certainly yes…". This needs some thought. Later it is shown that the water heights are not affected, and that the velocities are slightly diminished. There is a slight increase in the rate of flushing based on one numerical tracer release experiment (Section 11.1.2), which is a positive outcome if the simulation results hold. My concern is that with an increasingly informed public, interested media and under-staffed regulators, messages can be easily mis-interpreted. Firstly, the approximate decrease in channel velocity can be easily calculated as the ratio of the differing cross-sectional areas – 4260/5660 gives 0.75, that is after the dredging program, assuming all other things are equal (ie frictional effects don't change the flow significantly), velocities in the channel should be approximately 75% of the pre-dredged condition. The average difference in Figure 11.12 can be easily calculated and compared against this. Secondly, some definition of "exchange" needs to be provided.
20. Section 11.1.2 (3rd paragraph). Regarding the statement "further supporting the belief" – I think the report should be more factual and quantitative. That is, something like "After the 15 day simulation, 77% of the dye has left PRH in the post-dredging case compared with 72% in the pre-dredging case. This indicates a slight increase in the rate of flushing post-dredging under the conditions simulated during July 1 – July 15 2005." Belief has no place in a quantitative study….
21. Section 11.1.3 – I don't know on what basis the third conclusion can be supported by what is presented in the report. A cross-reference to the key figure or section that supports this conclusion should be given here. Similarly with the fourth conclusion. These two conclusions

suggest there is a significantly sheared flow occuring through the harbour entrance under windy conditions, but I can't see any evidence presented in the report that demonstrates this. Surely this can be shown with simulation results of velocity in the channel.

22. Table 5 – 0.5% is missing from the Rock Flour/Clay percentage, the column sums to 99.5%.
23. Table 6 – I think this table has the wrong caption, if not then it needs to be re-worded substantially. For example, the 2000 micron size is not simulated (according to Table 5). The 1589 and 893.4 micron are simulated (Table 5), but no information is given in Table 6 on the settling velocity for these particle sizes.
24. Table 6 – I have some concerns about the data presented here. My calculations suggest that these settling velocities are approximately 40% *higher* than would be expected from a theoretical calculation. This is vitally important – why are they so high?
25. Figure 12.1 – 12.3. There is a small typographic error on these three figures, the colour bar indicates this is "Turbidity mg/litre", when it should clearly be "TSS mg/litre".
26. Figure 12.10 shows no impact at the spoil grounds, presumably this is a mistake and a similar impact is expected to the other Figures (12.8, 12.9) in this region?
27. There are several reports referenced in the text of the document which are not included in the references section.

<u>Answers to questions posed by Ecologia as the basis for review:</u>

a. *Does the report as a whole addresses the key meteorological and oceanographic factors in PRH and KGS which are relevant to assessing the outcomes of the dredging project?*
   I believe so.

b. *Is the meteorology used shown to be adequate?*
   See comment 13 above.

c. *Are the hydrodynamic modelling predictions of tides and currents shown to be adequate?*
   Based on the information presented, I believe so. However, see comment 15 above.

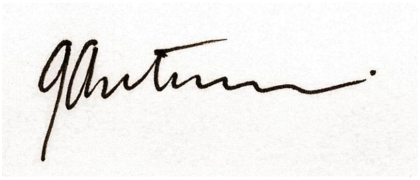
d. *Are the wave predictions shown to be adequate for the needs of the dredge modelling?*
   Based on the information, I believe so. There is significant difficulty in predicting the resuspension flux, which has been commented on in the report.

e. *Are the affects on the exchange between PRH and KGS are treated adequately?*
   Generally, yes, however see comment 21. I'm sure the information is available in the simulations completed, but it hasn't been adequately presented to back up the conclusions referred to in this comment.

f. *Are the dredging simulations undertaken in an appropriate manner?*

I have some concerns on the sedimentation velocities quoted in Table 6, they seem quite high. A couple of items need addressing (see comments 22 to 24).

Yours sincerely,



Dr Jason Antenucci
Deputy Director
Centre for Water Research
The University of Western Australia

Appendix 2: Response to UWA Peer Review

Response to UWA Peer Review Comments

Reviewer Comments	GEMS Response	Section in revised report
1. It should be stated that the GCOM3D model was run in barotropic mode, meaning that no consideration has been given to currents driven by density differences. A justification for this selection should be provided.	Comments included in the report.	
2. Section 2.4 – this section needs to be edited and formatted more professionally, the bullet list gives no context.	Amended	2.4
3. At no point in the report is there any comment about GEMS views on the best time for dredging. It seems clear from the dredge plume simulations that starting the dredging in March, as proposed, is actually the worst time of those investigated to begin the program.	This was not considered to be the role of GEMS in the project.	
4. Section 5.1 – SI units should be used throughout (eg m s-1 rather than knots).	This comment is undoubtedly correct for a scientific publication but GEMS considers that a document which is provided for public review needs to communicate in the units best understood by the public.	
5. Section 5.2 – It is stated in the third paragraph in this section that “occasionally” adverse conditions for surface waves occur. What is the approximate frequency of these “occasional” events – once a month, once a quarter? This needs to be quantified.	The 3 month wave period studied for the verification (July – September) shows 5 wave height events at the channel wave rider buoy exceeding 2 metres.	
6. Section 6 – What were the deficiencies in the MetOcean programme, and how were they addressed by the field studies reported here?	The original program carried out by MetOcean focused on deploying instruments along the channel. Further studies were required because the MetOcean data was only in winter and also because they were not deployed in sufficient locations to determine the response of King George Sound to var	
7. Figure 6 – This needs to be improved, presumably the “small” and “large” annotated arrows point to the bottom weights, which have been offset to the left.	The figure has been amended.	
8. Section 6.1 – More information needs to be provided on the ADCP deployment, in particular the depth of deployment, the vertical resolution, sampling interval, sampling frequency, averaging time, and most importantly the instrument resolution and accuracy.	Included.	
9. Section 7.1.1 – At what height is the MesoLAPS wind data given? Presumably 10m above some datum?	This assumption is correct and has been included in the report	
10. Section 7.2.1 – At what heights were the wind speeds measured by the AWS? If it was not collected at 10m (as I suspect), presumably it was converted to the same height as the MesoLAPS data prior to comparison?	This assumption is correct and has been included in the report	

11.	Section 7.2.2 – "Selection of a Period of Representative Winds". The reason the easterly winds would be expected to result in higher turbidity levels should be clearly stated. Is it due to increased likelihood of resuspension due to increased surface wave height when the winds are from this direction, or reduced flushing due to wind setup, or some other mechanism?	The main reason is that flushing of surface waters is better during westerly winds than during easterly winds. This is discussed in the report.	
12.	Figure 7.1 – Figure 7.2. The figure captions need to be improved. From what I can gather, the upper panel shows the full record, and the lower panels show sub-sections of the same record.	Amended	
13.	I have some concerns that the MesoLAPS data shows lower wind speeds than that measured by AWS. This comes from simply comparing the number of wind events in the both data sets that exceed 10 m s-1 (Figure 7.2). In particular the lowest panel of this figure seems to show MesoLAPS data exceeding 10 m s-1 on approximately 16 occasions, whereas the AWS data exceeds 10 m s-1 on close to 100 occasions. At least two new figures should be included, the first similar to Figure 7.3 showing the MesoLAPS vs AWS data in wind rose form, and the second similar to Figure 7.4 but for the AWS data so it can be compared directly with the energetic and low frequency components of the MesoLAPS data. Whilst is could be argued that the use of the MesoLAPS data would make the simulations "conservative" as the wind speeds are lower, the winds used still need to be representative.	The MesoLAPS winds provide the best available representation of the wind fields in the region. However there is no suggestion that this representation is "perfect". The plots are meant to illustrate the degree of agreement and the issues the reviewer notes are correct. It is also agreed that the wind speeds are lower at times, leading to a conservative prediction of flushing of dredge plumes.	
14.	Figure 7.4, Figure 7.5 – I don't understand why the years 1994 – 1999 and 2006 are included in both these figures if no data is shown in these years. Secondly, the vertical scale on both these figures should be the same (0 – 40%) to facilitate easier comparison.	Figures have been amended	
15.	Why were no simulation validation results presented for the period March – April 2006, even though this period was simulated? Data were presented for this period in Figure 8.12 – 8.17, but the equivalent model validation results also need to be shown as the dredge programme being suggested would also include the months of March and April.	Validation results are shown during a period of a month from January 21 to February 12. This is considered a sufficient duration to exhibit the performance of GCOM3D. Furthermore the plots become very difficult to interpret if longer periods are shown.	
16.	Section 8.2 – the last sentence states "GCOM3D has been shown to represent these features with a high degree of accuracy." Whilst I believe the model is doing a good job, statements like this need to be backed up with some statistical quantification.	The correlation between data and model predictions has been included for the current components in the west-east direction (84%) and south-north direction (89%) at site ADCP4.	
17.	Section 9.1.1 – Similar to the previous comment, on what basis were the 'best verification results' determined?	As explained in the report the SWAN wave model can be set up with different directional resolutions and different frictional schemes. The choice of directional resolution and frictional scheme was based on agreement with data.	

18.	Section 11.1 – there is a typographic error here, the cross-sectional areas should be reported in m2 and not m3.	Corrected.	
19.	Section 11.1 – Regarding whether dredging will change the exchange, and the statement that "The answer is almost certainly yes…". This needs some thought. Later it is shown that the water heights are not affected, and that the velocities are slightly diminished. There is a slight increase in the rate of flushing based on one numerical tracer release experiment (Section 11.1.2), which is a positive outcome if the simulation results hold. My concern is that with an increasingly informed public, interested media and under-staffed regulators, messages can be easily mis-interpreted. Firstly, the approximate decrease in channel velocity can be easily calculated as the ratio of the differing cross-sectional areas – 4260/5660 gives 0.75, that is after the dredging program, assuming all other things are equal (ie frictional effects don't change the flow significantly), velocities in the channel should be approximately 75% of the pre-dredged condition. The average difference in Figure 11.12 can be easily calculated and compared against this. Secondly, some definition of "exchange" needs to be provided.	These differences were modeled and shown in the report. They are in broad agreement with these statements.	
20.	Section 11.1.2 (3rd paragraph). Regarding the statement "further supporting the belief" – I think the report should be more factual and quantitative. That is, something like "After the 15 day simulation, 77% of the dye has left PRH in the post-dredging case compared with 72% in the pre-dredging case. This indicates a slight increase in the rate of flushing post-dredging under the conditions simulated during July 1 – July 15 2005." Belief has no place in a quantitative study.	Accepted.	
21.	Section 11.1.3 – I don't know on what basis the third conclusion can be supported by what is presented in the report. A cross-reference to the key figure or section that supports this conclusion should be given here. Similarly with the fourth conclusion. These two conclusions suggest there is a significantly sheared flow occurring through the harbour entrance under windy conditions, but I can't see any evidence presented in the report that demonstrates this. Surely this can be shown with simulation results of velocity in the channel.	The modeling does show sheared flow during strong easterly wind events in both KGS and PRH. This was reported for KGS but the detail was not considered necessary for PRH. The report was written for a specific purpose and the emphasis on what was included was based on a judgement of what was most relevant to the impacts of the dredging. The sheared flow in PRH exists now and will still occur after the dredging is completed.	
22.	Table 5 – 0.5% is missing from the Rock Flour/Clay percentage, the column sums to 99.5%.	Corrected.	
23.	Table 6 – I think this table has the wrong caption, if not then it needs to be re-worded substantially. For example, the 2000 micron size is not simulated (according to Table 5). The 1589 and 893.4 micron are simulated (Table 5), but no information is given in Table 6 on the settling velocity for these particle sizes.	Corrected	

24.	Table 6 – I have some concerns about the data presented here. My calculations suggest that these settling velocities are approximately 40% higher than would be expected from a theoretical calculation. This is vitally important – why are they so high?	The velocities used were based on data from sample analysis conducted by CSIRO PAS. It has discovered that this was in a fresh water medium instead of sea water (Appendix 3) and has since been addressed.	
25.	Figure 12.1 – 12.3. There is a small typographic error on these three figures, the colour bar indicates this is “Turbidity mg/litre”, when it should clearly be “TSS mg/litre”.	Corrected	
26.	Figure 12.10 shows no impact at the spoil grounds, presumably this is a mistake and a similar impact is expected to the other Figures (12.8, 12.9) in this region?	This was a plotting error.	
27.	There are several reports referenced in the text of the document which are not included in the references section.	Corrected.	

Appendix 3: Settling Velocities Clarification

Tammy Souster

From: Rick.Hughes@csiro.au
Sent: 15 April 2008 17:02
To: Paul Mackey; bcorry@grangeresources.com.au
Cc: shannon.dolbel@ecologia.com.au; graeme.hubbert@gems-aus.com; alex@grangeresources.com.au; PAS-Enquiries@csiro.au
Subject: RE: confidence levels in the sedigraph results for APA and Grange
Follow Up Flag: Follow up
Flag Status: Flagged

Hi Paul/Brendan!

Thank you for your enquiry regarding the sedimentation process conducted by our Sedigraph 5100.

My apologies for the time taken to formulate this response but our workload at present has left everyone extremely busy and heavily over-commited.

The analyser is detailed more specifically at the manufacturer's website: http://www.micromeritics.com/products/sedigraph_overview.aspx Although this site describes the newer model to the one operated by CSIRO Minerals (Sedigraph 5120 as opposed to our Sedigraph 5100) the technique and technology used is basically unchanged. Additions to the software make reporting and ISO compliance issues easier and more regulated are the major differences in the two models.

In response to your questions below:

1. The repeatability of the instrument is better than 1% with a well dispersed 10%w/v suspension. If the sample concentration falls significantly below this concentration then the x-ray attenuation signal will contain more noise and repeatability will be detrimentally affected. If a slurry is not fully dispersed and remains either partially agglomerated or of significantly low enough surface charge (zeta potential) allowing agglomeration to occur, then repeatability may be so severely affected that no two analyses will bear any likeness no matter how many times they are analysed. Typically, the standard deviation on five measurements conducted on a garnet standard is around 0.2% for every size channel down from 300µm to 1µm. Below 1µm, the standard deviation increases to around 1% in the finest channel, 0.2-0.18µm.
2. The sensitivity is highest in the coarse size fractions due to the high settling rates (meaning less chance of being measured) and in the least frequently populated size channels.
    a. Factors which also affect the size fractions measurement is the heterogeneity of sample density. The instrument assumes homogeneity, one value of density is used in the software to calculate the size/cell height. Multiple phases with differing densities will settle at differing rates controlled by both diameter and density. If density is varying, then differing diameters of differing phases will report to the same size fraction.
    b. Particle voidage may affect both buoyancy and x-ray absorbance for a given phase. If particles contain micro-voids or inconsistencies in their structure, then both x-ray attenuation and settling rate will be affected.
3. As the instrument calculates the amount of material remaining in suspension for given incremental periods of time and, using Stokes Law, calculates the size at a given time/cell height, there should be no difference between actual and theoretical. The theory is used in the experiment to calculate what size fraction remaining at a specific height in the cell given the parameters (density of the particle, density of the suspending medium and viscosity of the suspending medium). The x-ray absorbance at that cell height gives the percentage of material at that size based on the difference between the baseline (suspending liquid only) and the full-scale line (fully homogenised and dispersed slurry). Any disparity between 'theoretical' and 'Sedigraph' reported size distribution is likely due to either agglomeration occurring in the 'natural' state (as opposed to our fully dispersed state), inconsistencies in the density value (either a wrong value totally or the sample contains mixed phases) or differences in the density and viscosity of the 'natural' state solution. The tests are conducted in a 100ppm sodium hexametaphosphate solution. The density and viscosity for these values are 0.994g/cm3 and 0.717cp respectively. The settling velocity of the particles may be different to that reported because in their 'natural' state they may be in sea water with both a higher density (1.019g/cm3) and viscosity

(1.114cp). Appropriate values for these parameters can be entered into the result spreadsheets sent to the client and the resultant settling velocities should be more relevant to the 'natural' state system.

4. The Sedigraph analysis reports the Stokes or hydrodynamic diameter. For all particles, the hydrodynamic diameter is the diameter that controls the particles settling rate and other properties when in suspension. For a spherical particle this is simply the diameter. For all non-spherical particles (eg clays which are platelets) the hydrodynamic diameter is typically the diameter which presents the largest surface area towards the direction of movement (settling). This is the diameter calculated by all sedimentation techniques and may not actually be a real 'measurable' quantity if the particle is viewed in an electron microscope. It is a calculated diameter based on what is observed when the particle is settling. The diameter may actually include surface charge layers which travel with the particle and interact with the suspending medium as if adhered to the particle.

   As with any particle sizing technique there are always going to be anomalies and limitations that mean a number will be reported that may or may not bear relevance to what is observed in the 'natural' state. Laser diffraction utilises the scattered light of a dispersed particulate system to infer particle size based on Braggs law. Particle refractive index and absorption for particular wavelengths of light need to be known for accurate analyses. Surface roughness and particle shape have a huge impact on what is optically 'interpreted'.

   For any system, the result is only as good as the knowledge of the particles themselves. In a perfect world all particles would be spherical and one number would represent each size. In reality, particles are 3-D objects that present differently depending on the situation they are in and how they are being analysed. Reporting one number for a 3-D object is fraught with assumptions. With Sedimentation techniques, particles tend to orientate themselves so the same diameter is being presented/orientated under laminar settling conditions.

   The Sedigraph is both a highly reproducible and highly comparable technique, both between samples and with real systems, for calculating the settling velocity under laminar conditions.

Please feel free to respond with any further queries/questions.

Regards

Rick

From: Paul Mackey [mailto:Paul.Mackey@albanyport.com.au]
Sent: Friday, 4 April 2008 4:09 PM
To: Hughes, Rick (Minerals, Waterford)
Cc: bcorry@grangeresources.com.au; Shannon Dolbel; graeme.hubbert@gems-aus.com; alex@grangeresources.com.au
Subject: confidence levels in the sedigraph results for APA and Grange

Hi Rick,

As discussed we've had an independent review of the modelling work we've been doing for our port upgrade and the reviewer, Dr Jason Antenucci, has some questions he'd like answered in regards to the work CSIRO PAS did for us (batch number 9953 22/2/07) as follows:

1.What is the confidence level in the settling velocity values (+/-)?
2.What, if any, is the influence on the results of different particle size fractions? (i.e. are the tests size sensitive)?
3.Could you please provide an explanation of the difference between the actual results derived vs the theoretical values anticipated using Stokes Law? I think specifically, that the reviewer anticipated that the actual velocities would be lower than those anticipated in theory and the results indicate the reverse was actually realized.
4.Does the analysis work better for some particle shapes, sizes, materials than for others?

We certainly appreciate your efforts Rick and look forward to receiving your responses in due course (nominally by Wednesday next week, or earlier if your workload permits).

Thanks again and regards

Paul

Paul Mackey
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Appendix 4: CSIRO Marine and Atmospheric Research Peer Review Report

COMMONWEALTH SCIENTIFIC AND INDUSTRIAL RESEARCH ORGANISATION

Division of Marine & Atmospheric Research

Review of GEMS Pty Ltd Report: 'Oceanographic Studies and Dredging Program Simulation Studies'

Undertaken for Grange Resources and Albany Port Authority by CSIRO Marine and Atmospheric Research,

May 2008

Review of GEMS Pty Ltd Report
'Oceanographic Studies and Dredging Program Simulation Studies'
Undertaken for Grange Resources and Albany Port Authority July 2007

Review undertaken by Kathleen McInnes, CSIRO Marine and Atmospheric Research

Introduction

The review by CSIRO of the GEMS modelling study followed from discussions between CSIRO (Dr Kathleen McInnes) and the Albany Port Authority (Mr Paul Mackey) in March 2008. From these discussions it was agreed that the review of the GEMS study would focus on the following key aspects.

1. the report as a whole addresses the key meteorological and oceanographic factors in PRH and KGS which are relevant to assessing the outcomes of the dredging project
2. the meteorology used is shown to be adequate
3. the hydrodynamic modelling predictions of tides and currents are shown to be adequate
4. the wave predictions are shown to be adequate for the needs of the dredge modelling
5. the affects on the exchange between PRH and KGS are treated adequately
6. the dredging simulations are undertaken in an appropriate manner.

Overview and scope of GEMS report

This report documents a suite of numerical modelling simulations undertaken to evaluate the potential impacts of dredging of the shipping channels leading into Albany Port. The expansion of the port is being undertaken to provide greater facilities for the Grange Resources Southdown Magnatite Project. Specifically, the proposed dredging operations consist of extending and deepening the main shipping channel in King George Sound (KGS) which leads to Princess Royal Harbour (PRH).

The scope of the work undertaken by GEMS involved several components which are summarised as follows:

- a field program to measure surface currents and sub-surface currents using drifter technology and Acoustic Doppler Current Profilers,
- validation of data sources and a suite of numerical models required for the simulation of the impacts of the proposed dredging program,
- utilising the models to investigate the impacts of the dredging program under different meteorological and oceanographic conditions.

1 Key Meteorological and Oceanographic Factors

Chapter 4 describes the key climatological features of the region. During the summer months, the subtropical ridge has reached its southernmost extent meaning that Albany is under the influence of winds of a predominantly easterly direction. As the ridge moves

northwards in autumn and winter, the easterlies are replaced by predominantly westerly winds that accompany the passage of cold fronts. The report notes the dominant transitions in the meteorology over the months that the proposed port dredging is to take place.

The report notes the influence that the local winds have on circulation within the PRH and KGS. For PRH, this is one of clockwise circulation under east to southeast winds in summer and anti-clockwise circulation under west to northwest winds in winter. In KGS, the authors note that for winds over the summer months from the south to southeast and southeast to northeast directions, the effect on the water circulation are those of predominantly anti-clockwise and clockwise circulations respectively. However, they do not comment on the circulation associated with the winter time westerly to northwesterly winds in KGS. The authors note the presence during the winter months of a coastally trapped wave on the continental shelf outside KGS although do not provide any evidence in support of this statement. However, later in the report in Figure 11.21 it is shown that the surge in current speeds appears to lead the surge in wind forcing by one to two hours. Such assumptions could be tested using simple shelf wave speed calculations although is outside the scope of the present study.

With respect to the wave climate, it is noted that the southeasterly to easterly aspect of KGS provides protection from the energetic waves generated in the westerly wind belt to the south of the continent. However, it is noted that higher energy southeast waves (which due to the orientation of KGS would have greater impact) can occur through the development of a high-pressure system at higher latitudes. In summary, the key meteorological and oceanographic factors in Princess Royal Harbour and King George Sound which are relevant to assessing the outcomes of the dredging project appear to have been addressed.

2 Meteorology

Comparison of wind speed and direction at an AWS at the entrance to King George Sound with the equivalent fields from the Bureau of Meteorology's Mesolaps model demonstrate that the Mesolaps model adequately captures the wind field in this region and as such is appropriate to use as a driver for the hydrodynamic and wave modelling components of the study. The authors have also investigated the interannual variability in wind conditions to ensure that the interval over which the dredge modelling was undertaken was appropriate. The authors argue that 2005 is a suitable year for modelling experiments because the winds during this year are broadly typical but have a bias towards more frequent easterly events which are the conditions most likely to contribute to greater turbidity levels. In summary, the Mesolaps data has been shown to well represent the winds in the study site and are appropriate to force the model. The choice of interval over which to conduct the modelling seems to be appropriate.

3 Tide and current simulations

The report does not document the resolution or accuracy of the bathymetric data used to develop the model grids and the sources of additional data used to augment the primary data sets used by GEMS. It also does not provide the resolution of the tidal datasets (those developed for AMSA) used to provide tidal boundary conditions for the model.

Figures 8.4 to 8.11 indicate that the model reproduces observed current speeds and directions at the near-surface and near-bed levels at the locations of ADCP4 and 5 over the period from January 21 to February 12 reasonably well although the modelled current speeds are slightly underestimated. Figures 8.12 to 8.17 which show measured current speeds and directions at ADCP6, ADCP5 and ADCP4 respectively appear to have been smoothed and it is not clear why this has been done. If it is to clarify the relationship between the near surface and near bed currents, this should be stated. Also it is stated that these currents are over the period of March 12 to April 29 (42 days inclusive), yet Figures 8.12 to 8.13 appear to show 46 days of data, Figures 8.14 to 8.15 appear to show 28 days of data and Figures 8.16 to 8.17 appear to show 21 days of data. These errors should be corrected. Since these figures do not show the equivalent modelled information, it could be argued that the figures do not belong in this chapter which is dedicated to model validation.

The comparisons between modelled drift tracks with measured drift tracks show good agreement. The discussion of the anticlockwise circulation resulting from the south to southeasterly wind regime (Figure 8.18) with the generally clockwise circulation resulting from east to northeasterly wind regime (Figure 8.19) is difficult to assess since the start and end points of the drift tracks are not labelled.

In summary, GCOM3D has been shown to well represent the currents through the water column within the study site and is therefore appropriate to investigate the impact of dredging in the study region.

4 Wave simulations

The wave model simulations are performed to provide orbital velocities for the model DREDGE3D for the calculation of sediment re-suspension. Although there is no data with which to validate the modelled orbital velocities, it is assumed that if the model can reproduce observed surface wave heights, then the representation of the near-bed orbital velocities will be suitable for the sediment modelling. This is a reasonable assumption. The three-month simulation of wave heights at WRB just inside KGS and AWAC-1 near to the entrance to the harbour shows that the model is reasonably capturing wave heights across the Sound. It is not stated in the report whether there was measured directional wave data with which to validate the model and if so, why this data wasn't also used for validation purposes.

5 Exchange between PRH and KGS

There is no specific attention given to the exchange of water between PRH and KGS in chapter 8 of the report which deals with model validation. It is not clear from the report whether the current meter located in PRH that is indicated in Figure 6.2 could have been used to validate the currents in GCOM3D although possibly since it was deployed by MetOcean, the data coverage may not span that of the ADCP's deployed by GEMS. However, the resolution of GCOM3D is adequate to model the exchange between PRH and KGS and evidence that the model is performing well in this capacity is suggested by the good agreement between the modelled and observed drifter tracks inside PRH as shown in Figure 8.18.

Chapter 11 further addresses this issue of exchange between PRH and KGS. Model simulations of the pre-dredged and post-dredged bathymetric profiles are used in fourteen day simulations of the water circulation. Water depths are shown to be unaffected by the deeper channel while currents through the channel decrease once the channel is dredged.

Numerical dye experiments suggest that there will be a slight increase in the exchange of water between the PRH and KGS as inferred from the greater spreading of the aerial extent of the dye in the post-dredging simulation

The issue of wave propagation into PRH is touched on but there are few details of the simulations undertaken and no results from the high resolution wave modelling simulations presented.

6 Dredging simulations

The four month dredge modelling simulations have been undertaken at different times of the year to capture the different prevailing wind conditions and hence ensure that the fate of dredge material is explored as widely as possible. The inputs to the numerical dredge program are the three-dimensional currents simulated by GCOM3D for the duration of the dredging program, fields of orbital velocities from the wave model and the dredge log which details the location, volume and distribution of particles released during the dredging. The output of dredge modelling includes spatial maps turbidity as defined by the Total Suspended Solids (TSS), time series of TSS throughout the four month dredging program at five locations and maps of sea grass mortality using thresholds of turbidity on seagrass mortality supplied via a separate study.

Additionally the stability of the spoil grounds is investigated by running the model for an additional 12 months to investigate any potential migration of dredge material.

Based on the results presented, the results appear to indicate that the impact of dredging is minimised if undertaken during a predominantly westerly wind regime, although no such discussion of the results is presented by the authors. However, given the range of uncertainties that are present in such modelling, the authors are careful not to overstate the results, particularly with regards to the stability of the spoil ground where knowledge of the reliability of orbital velocities from SWAN and the availability of particles for re-suspension are not known.

Summary

This study has provided a comprehensive investigation of the meteorology, the dynamics of the circulation around KGS and PRH and the impact of several scenarios of dredge programs for the shipping channel connecting the two. Overall I believe that the study has addressed the criteria stipulated in the introduction of this review sufficiently well. The readability of the report could be improved by the correction or clarification of some points that are summarised in the following section.

Summary of grammatical errors

While it was not a requirement to comment on technical errors in the report, a number of these, some of which hinder the readability and interpretation of the results in the report, were identified during the review process and so are summarised in the following table.

Page	*Paragraph (p): line no,*	*Correction required*
9	Figure 1	There should be labels on this figure showing PRH, Oyster Harbour and KGS
10	p4:1	the source of the bathymetric data should be cited.
12	p3:1	'Through it links..' should be 'Through its links
16	p4:11	What is the evidence for a shelf wave
18	p2:2	'..because they location…' should be '..because they are location..'
p18	p3:4	I think the reference to section 5.2 should be 9.2
p25	p3:1	'If the peak winds are in fact…' should be 'If the peak winds used to force the circulation model are in fact…'
p25	last line	what is 'mesoblast'? Do you mean MesoLaps?
p26	p1:3	mesoblast? Ditto for previous comment.
p26	p2:1	The polar diagrams you refer to here and elsewhere are more commonly called wind roses
p26	p5:3	'…provides representation of the winds..' would be simpler as just '…represents the winds…'
p 27	caption	correct 'direcions'
p 28	Figure 7.2	the fourth panel is a duplication of panel 3
p 29	Figure 7.3	The dates over which the wind roses are compiled should be stated in the figure captions
p 33	p1:1	'…ebb tide in KGS during and …' remove 'during
p 44	p2:2	'..the southern ocean a resolution…' should be '..the southern ocean with a resolution…'
p 44	p4:1	'..2005 measurements form the..' change 'form' to 'from'. Note also the font change in this para
p 45	p2:4	'..it is key location..' change to '...it is a key location..'
p 45	p3:2	'..correspond with southeasterly (from) wave directions.' This could be more clearly stated as '..correspond with waves propagating from a southeasterly direction.'
p 46	p2:2	'..the fact that model..' change to '..the fact that the model..'
p 52	Figure 9.7	should state in the figure caption that the wave induced bottom velocities are from SWAN and not from the instruments at these locations
p 53	p4	Should reference Appendix A for the results of the Geraldton study here
p 53	p7:2	TSS is used here but not defined until page 70
p 56	p3:7	Unlike the other models used in the study there is no documentation on PLUME3D.
p 57	dot point 3	it is not clear on what evidence this conclusion has been drawn
p 67	p1 & 2	there is no model output presented to support this conclusion
p 71	p1	it would be useful to cite the specific marine division

p77	p7	it is not clear why you wouldn't sum the turbidity from all levels of your model rather than using the maximum turbidity level in the available layers/cells.
p,4, 71 & 77		section numbering is wrong. Two sections are both numbered 12.2
P 79	p4:1	'dredge plume' model. I assume this is DREDGE3D and not the previously mentioned PLUME3D?
p 79	p7 dot pt 1	'The spoil ground …is over 30 m deep…' Do you mean that the spoil ground is in 30 m of water or that the depth of the spoil pile after disposal is 30 m high?
p 86-87		The level 2 contours look orange to me – not magenta
p 91	References	The reference list is incomplete. For example McInnes and Hubbert (1999) cited on page 54 is not in the reference list and may be wrong anyway. I think it probably should be McInnes, K. L., Hubbert, G. D., Oliver, S., and Abbs, D. J. (2000). Storm tide return periods and 1974 floodwater modelling: for Gold Coast City Council. Aspendale, Vic.: CSIRO Atmospheric Research. 45 p. http://www.dar.csiro.au/publications/mcinnes_2000b.pdf .
p 106		CSD and TSHD are used whilst not defined until p 107

Appendix 5: Response to CSIRO Peer Review Report

Response to CSIRO Peer Review Report

Reviewer Comments			**GEMS Response**	**Section in revised report**
Page	*Paragraph (p): line no,*	*Correction required*		
9	Figure 1	There should be labels on this figure showing PRH, Oyster Harbour and KGS	Corrected	
10	p4:1	the source of the bathymetric data should be cited.	Referenced in section 8.1.1	
12	p3:1	'Through it links..' should be 'Through its links	Corrected	
16	p4:11	What is the evidence for a shelf wave	This is explained in the report where the correlation between the wind speed and the current speed is illustrated.	
18	p2:2	'..because they location…' should be '..because they are location..'	Corrected.	
p18	p3:4	I think the reference to section 5.2 should be 9.2	Corrected	
p25	p3:1	'If the peak winds are in fact…' should be 'If the peak winds used to force the circulation model are in fact…'	Corrected	
p25	last line	what is 'mesoblast'? Do you mean MesoLaps?	Corrected	
p26	p1:3	mesoblast? Ditto for previous comment.	Corrected	
p26	p2:1	The polar diagrams you refer to here and elsewhere are more commonly called wind roses	This depends on the audience.	
p26	p5:3	'…provides representation of the winds..' would be simpler as just '…represents the winds…'	Adjusted	
p 27	caption	correct 'direcions'	Corrected	
p 28	Figure 7.2	the fourth panel is a duplication of panel 3	Corrected	
p 29	Figure 7.3	The dates over which the wind roses are compiled should be stated in the figure captions	Added	
p 33	p1:1	'…ebb tide in KGS during and …' remove 'during	Corrected	
p 44	p2:2	'..the southern ocean a resolution…' should be '..the southern ocean with a resolution…'	Corrected	
p 44	p4:1	'..2005 measurements form the..' change 'form' to 'from'. Note also the font change in this para	Corrected	
p 45	p2:4	'..it is key location..' change to '...it is a key location..'	Corrected	
p 45	p3:2	'..correspond with southeasterly (from) wave directions.' This could be more clearly stated as '..correspond with waves propagating from a southeasterly direction.'	Changed	

p 46	p2:2	'..the fact that model..' change to '..the fact that the model..'	Corrected	
p 52	Figure 9.7	should state in the figure caption that the wave induced bottom velocities are from SWAN and not from the instruments at these locations	Added	
p 53	p4	Should reference Appendix A for the results of the Geraldton study here	Added	
p 53	p7:2	TSS is used here but not defined until page 70	Defined	
p 56	p3:7	Unlike the other models used in the study there is no documentation on PLUME3D.	Appendix Included	
p 57	dot point 3	it is not clear on what evidence this conclusion has been drawn	The modeling does show sheared flow during strong easterly wind events in both KGS and PRH. This was reported for KGS but the detail was not considered necessary for PRH. The report was written for a specific purpose and the emphasis on what was included was based on a judgement of what was most relevant to the impacts of the dredging. The sheared flow in PRH exists now and will still occur after the dredging is completed.	
p 67	p1 & 2	there is no model output presented to support this conclusion	Correct – it was not considered useful to show a plot of wave heights exactly overlaying each other. The finding is reported in the text.	
p 71	p1	it would be useful to cite the specific marine division	Corrected	
p77	p7	it is not clear why you wouldn't sum the turbidity from all levels of your model rather than using the maximum turbidity level in the available layers/cells.	This approach was taken purely to derive the most conservative result.	
p,4, 71 & 77		section numbering is wrong. Two sections are both numbered 12.2	Corrected	
P 79	p4:1	'dredge plume' model. I assume this is DREDGE3D and not the previously mentioned PLUME3D?	Correct	
p 79	p7 dot pt 1	'The spoil ground …is over 30 m deep…' Do you mean that the spoil ground is in 30 m of water or that the depth of the spoil pile after disposal is 30 m high?	The former – this has been clarified in the text.	
p 86-87		The level 2 contours look orange to me – not magenta	Corrected	
p 91	References	The reference list is incomplete. For example McInnes	Corrected	

		and Hubbert (1999) cited on page 54 is not in the reference list and may be wrong anyway. I think it probably should be McInnes, K. L., Hubbert, G. D., Oliver, S., and Abbs, D. J. (2000). Storm tide return periods and 1974 floodwater modelling: for Gold Coast City Council. Aspendale, Vic.: CSIRO Atmospheric Research. 45 p. http://www.dar.csiro.au/publications/mcinnes_2000b.pdf .		
p 106		CSD and TSHD are used whilst not defined until p 107	Corrected	