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Newfoundland and Labrador Region
Canadian Science Advisory Secretariat
Science Response 2020/036

REVIEW OF DRILL RELEASE RISK ASSESSMENTS FOR EXPLORATION DRILLING PROJECTS PROPOSED BY BHP CANADA (ORPHAN BASIN REGION) AND CHEVRON CANADA LIMITED (WEST FLEMISH PASS)

Context

Two oil and gas proponents have proposed to carry out exploratory drilling in the Newfoundland region. Specifically, BHP Canada proposes to conduct its work in the Orphan Basin region, located approximately 350 km northeast of St. John's, NL and northwest of the Flemish Pass. Chevron Canada Limited is proposing to carry out exploratory drilling in West Flemish Pass, located approximately 500 km northeast of St. John's, NL including portions of the northern Flemish Pass and northwestern Flemish Cap. Both companies have submitted Environmental Impact Statements (EIS) to the Impact Assessment Agency of Canada (the Agency) for review. The Agency has requested, through the Fish and Fish Habitat Protection Program (FFHPP) that Fisheries and Oceans Canada (DFO) conduct a technical review of specific components of the EIS documents, specifically the Drill Release Risk Assessments.

In accordance with the legislative provisions of the *Canadian Environmental Assessment Act* (*CEAA 2012*), DFO is required to provide specialist or expert information or knowledge, pertaining to the Department's mandate, with respect to a designated project that is subject to an environmental assessment. FFHPP is requesting that Science undertake a review of the project-specific drill cutting dispersion modelling due to the importance of the modelling in predicting the significance of effects on benthic habitat as well as the appropriateness of proposed mitigation measures. Science was not requested to review the EISs associated with these projects; therefore, no advice on the EISs is being provided by Science.

The submission to Science for review consists of two documents:

1. Appendix C – Chevron Canada Limited West Flemish Pass Exploration Drilling Project (2021-2030) Drill Release Risk Assessment.
2. Appendix D – BHP Canada Orphan Basin Region Exploration Drilling Project (2019-2028) Drill Release Risk Assessment.

This Science Response Report results from the Science Response Process of April 29, 2020 on the Review of Drill Release Risk Assessments for Exploration Drilling Projects proposed by BHP Canada (Orphan Basin Region) and Chevron Canada Limited (West Flemish Pass). As the Drill Release Risk Assessments for both projects were carried out by the same professional consulting firm (RPS), and the methodologies and analyses were very similar, it was determined that a single Science Response Process would be conducted to review both projects.

Analysis and Response

As requested by the Fish and Fish Habitat Protection Program (FFHPP), the comments provided by DFO Science, Newfoundland and Labrador (NL) and Maritimes (MAR) Regions are specifically related to the Drill Risk Assessment Reports. It should be noted that the reports are identical in their approach, models used, and methods. Therefore, large portions of the review are identical for both reports. **As such, general and specific comments below apply to both reports unless otherwise explicitly stated**. Further, specific comments for each report are also provided.

General Comments

Both risk assessments evaluated simulations of drilling discharge using RPS' MUDMAP software (Spaulding et al. 1994). This model is used to predict the transport, dispersion and deposition of drilling solids and produced waters resulting from offshore operations. While the model seems adequate for the purpose targeted, the approach excludes stochastic and sensitivity analyses. These sensitivity studies should include parameters such as particle size distribution, mixing coefficients frequency of model output and environmental conditions (currents, water density, etc.). The model and forcing have not been validated and the results are based on a single run using Hybrid Coordinate Ocean Model (HYCOM) currents from 2012 (one run for spring and one for summer). There remains unanswered questions such as a clear indication of the vertical resolution of the HYCOM model and if it adequately resolves the vertical structure in the currents/density fields.

It is difficult to evaluate the total duration of the simulations and if they are long enough to estimate the accumulation on the sea floor. More details are required to better understand the results being presented in both reports. In both the BHP Canada and Chevron Canada documents, it is stated that "*several days were required to allow for all particles to reach the seabed*". It is estimated that a time period of 200 days is needed for fine particles with velocities 2.37 m/day released at the surface, to settle to 500 m depth. This represents the shallowest of the sites reported in all cases; therefore, longer time periods in the simulations would be required for deeper locations. Even if released at 20 m above the sea floor, these particles would take 8 days to settle. Additional information is requested regarding the fate of these particles. Are they advected out of the domain of interest? If so, where do they ultimately settle and accumulate? What volume of dispersal is represented by Table 3-1 and how does that compare to the release volume? The assumptions made regarding the fate of the particles needs to be clearly articulated in the reports. This is considered a major deficiency in this study.

Both reports state that seven years are analyzed; however, the information presented indicates that only 2012 was used for the modelling. There is a comparison to the 2006-2012 period but the full seven-year period was not used. Using only one year for this analysis does not give confidence that the results are representative. Observations show that the strength of the Labrador current may vary by more than 15% on inter-annual to decadal timescales (e.g., Cyr et al. 2020). Instead of choosing a specific year, it is preferred to use an "ensemble-like" approach by running with every year and calculating some statistics on the average thickness/extent on the depositional area. This approach would give much more confidence that the results are representative.

The results of this study are dependent on a 1/12 degree global ocean reanalysis model HYCOM. There is no discussion of how accurate this model is in the study region. There are statements such as "*the data used is sufficient for this type of modelling*" without references or justification to support this assertion . Has there been any work to evaluate the accuracy of this model in this region? "*HYCOM uses Mercator projections between 78°S and 47°N and a bipolar patch for regions north of 47°N to avoid computational problems associated with the convergence of the meridians at the pole"* (text from HYCOM Manual, recalled here in the reports). Simulations are just north of 47°N. Does this grid patching/merging affect the quality of the current forcing at this latitude? Please indicate why this model was chosen over other available reanalysis products (e.g., CMEMS 1993-2018 1/12 degree global reanalysis).

The impacted areas by drilling, and the spatial length scales, are ~ less than 2 km, while the resolution of HYCOM is about 7 km. It is assumed that MUDMAP has a much finer resolution than 7 km, and it is important to mention this in the report. More details about MUDMAP, including resolution parameters, should be provided.

The choice of daily current output is not justified. A high frequency output is recommended in particle simulations because errors accumulate over time, particularly in regions like the Project Area where high frequency motions (e.g., winds, tides, inertial oscillations) are observed. Both reports state that the area has "*extremely energetic and variable frontal systems and eddies*" and that winds account for "*approximately 10% of current variability*". A daily frequency could be justified by performing a sensitivity study to compare results between hourly and daily outputs.

In several places in both reports, it is stated that the MUDMAP simulations use the environmental conditions from the ocean model which include currents and density yet only the currents are discussed in detail. The water column density changes throughout the year. As such, statements like "*the predicted results are applicable outside of this window*" are not defensible. A detailed analysis of the ocean model density structure is needed to support that claim. Additionally, the settling velocities were taken from a study in the Gulf of Mexico which has a very different density structure than the Project Area. Are these settling velocities applicable to the Project Areas? A more detailed justification is required.

Both documents state *"The discharges modelled in this study may be considered representative of other potential discharges in the Project Area".* There is no basis for this statement because the assessments for both projects yield different results using a similar approach and are in the same Project Area. These statements should be quantified and based on the results of the studies.

Overall, there are significant issues regarding the mixing parameters. Determination of mixing parameters is arguably one of the largest sources of uncertainties in numerical modelling. The numbers provided here are K_h (horizontal) = 2.0 m^2/s and K_z (vertical) = 10-3 m^2/s. The report claims that these values are selected based upon "*professional judgment and previous experience*" and that they "*represent typical conditions of the deep marine environment*". These statements pose several issues.

- Firstly, these judgement statements should be supported by peer-reviewed literature.
- Second, horizontal diffusivity (K_h), a parameter used to parametrize horizontal processes happening at a scale smaller than the model resolution (e.g., eddies, swirls, fronts, etc.), is highly dependent on the model grid and input resolution. Yet, these reports do not provide information on the resolution of the model (the grid, time steps). For example, a study by Bourgault et al. (2014) suggests that, when possible, hourly currents combined with gradient-based eddy diffusivity (e.g., Smagorinsky-based models) should be used in highly energetic areas to model dispersion of tracers. When this is not possible (e.g., when averaged currents are used), they found that K_h ~10^2 m^2/s best suited their observations. The latter value is 2 orders of magnitude higher than what was used here; therefore, clarification is needed about the present approach.
- Third, the statement about the fact that the value of K_z used represents "*deep marine environment*" is flawed. There is a lot of literature suggesting that the value of K_z used here

is likely 1 to 2 orders of magnitude larger than what is measured in the deep ocean (~10^{-5} m^2/s above 1000m and ~10^{-4} m^2/s below 1000m) (see for example Waterhouse et al. 2014). What is the effect of this over-estimation?

- Finally, given the uncertainties associated with these parameters, a sensitivity analysis ***must*** be conducted in order to determine how they affect the results (e.g., how changing one parameter by an order of magnitude would impact the area affected by a layer of a certain thickness, etc.). The latter is key to provide a range of realistic scenarios and confidence in the model.

Finally, the reports are difficult to review because there is no ability to search for text in the electronic file as presented (.pdf format) and no ability to copy and paste text. Further, some acronyms are undefined (WBM, PSDs) making the text difficult to understand for readers unfamiliar with those acronyms. Please elaborate the list of acronyms to include all those used in the documents.

Specific Comments on both reports

Particle size distribution for simulations (Chevron – Table 2.4; BHP – Table 2.3): How are these values obtained? A unique distribution is used instead of a range of possibilities, which is incorrect. A sensitivity analysis should be performed, which is particularly important when it is later stated that "*The extent to which discharged drilling fluids and cuttings accumulate on the seabed is largely controlled by the particle settling velocities, which are a function of size and density...*".

Section 1.2 in both reports: The description of the Labrador Current is incomplete. Figure 1-2 does not reflect observed currents. Wang et al. (2015) describes the current system in the region as "*The main features of circulation over the Newfoundland shelf consist of the equatorward inshore Labrador Current (ILC) along the coast, the offshore Labrador Current (OLC) along the shelf edge, and the cross-shelf flows following the topography of seaward trenches and canyons*". Other details on the ILC and OLC can be found in Wang et al. (2015).

Further, the OLC and the North Atlantic Current (NAC) carry water masses with different origins. The OLC carries Denmark Strait Overflow Water (DSOW), Labrador Sea Water (LSW), and Iceland-Scotland Overflow Water (ISOW) from the north into the Flemish Cap region, while the NAC transports warm and saline Gulf Stream water from the south.

Statistics of the model velocities are presented in Figures 1-6, 1-7 and 1-8 focus on average and 95th percentile. It seems that the 5th percentile is more relevant to this study because slower currents would result in more deposition in the study area.

Figure 1-2 in both reports: This figure mostly represents surface currents. The deep currents, particularly in the Flemish Cap region, are different from the surface. The east-northward arrow to the southeast of the Flemish Cap is not correct, and it should include the continuation of the cyclonic current around the Flemish Cap and turns eastward at around this location. Figure 1 in Wang et al. (2015) provides a good representation of the Labrador current system.

Figure 1-3 in both reports: The document states that "*The discharges modelled in this study may be considered representative of other potential discharges in the Project Area as the depths of the sites (500m to 1500m) are similar in depth to other potential sites in the Project Area*" but the figure indicates that this is not the case. It would be helpful to have the Project Area highlighted on this map. Also, an indication of the size of the Project Area would be useful in determining how well the HYCOM model resolves the Project Area.

Section 2.1 in both reports: The statement: "*MUDMAP does not account for resuspension and transport of previously discharge solids; therefore, it provides a conservative estimate of the potential seafloor depositions.*". The word conservative cannot be concluded. The estimate might be conservative for the total amount deposited as one can hypothesize that re-suspension has the potential to bring more sediments out of the domain. However, near bottom processes also have the potential to reorganize the sediments after deposition and thus change the maximum thickness layer and/or the maximum area affected in a fashion like sand dunes at the seafloor. In other words, the ability of the model to pile-up material and potentially modify the thickness of the deposition is not possible.

Section 2.3 in both reports: "*Current trends for the two model periods during 2012 were congruent with the overall 7-year trend and were thus deemed suitable for the entire modelling period.*". How was this decided? Why not show this analysis in section 1.2?

Section 2.4 in both reports: The documents state that "*Given the absence of local sample data, representative size distribution based on published values by Brandsma and Smith (1999)*". Brandsma and Smith (1999) is missing from the list of references. This reference seems to be an inaccessible industry report from Exxon. How are these data representative of the Flemish Pass area? In addition, the settling velocity is dependent on water density, which varies from one region to another with depth. A sensitivity analysis is required to ensure that reference velocities from another part of the world are representative.

Chevron – Table 2-4 and 2-5; BHP – Table 2.3 and 2.4: One table has 6 size classes while the other table has 10 classes which creates confusion and should be corrected and/or clarified.

Section 2.5.1 – Sedimentation Effect and Thresholds: A reference to Cordes et al. (2016) should be cited in addition to Ellis et al., 2012.

Comments specific to Chevron Canada Limited Report

Page 5: The document states: *"maximum thickness of 2.38* km^2*"*, should this be different units such as mm?

Section 1.1 Paragraph 1: Line 5 - Depth should be "depths of".

Line 8: Please add "m" after 400-2,500.

Line 9: Consider rewriting this sentence. Message is not accurate.

Section 3.1, 1st paragraph: "*Based off the depth at …*" should be "Based on the depth at …".

Figure 3-1 and Figure 3-2: In Section 1.2, it has been demonstrated that there is general lack of seasonality of the currents at least at site 1, and this can be seen in Figure 1-4, however Figure 3-1 clearly demonstrates that the existence of seasonality has an impact on the deposition of the discharge mud and cuttings. It is difficult to understand how the currents in summer and spring can lead to these significant differences. The top panel of Figure 3-1 and Figure 3-2 implies the currents in summer are almost not flowing. The bottom panels of Figures 3-1 and 3-2 are consistent with currents in this region. It is suggested that the Proponent re-examine the simulations.

Section 4 – Conclusion: "*While this dispersion modelling targeted the most likely drilling window for the project (April to May and June to July), the predicted results are applicable outside this temporal window*". The difference between spring and summer is already quite large, so how it is possible to justify that it is applicable for other temporal windows if not assessed?

Comments specific to BHP Canada Report

Section 4 – Conclusion: "*This dispersion modelling targeted the most likely drilling window for the project (July-August), as well as an alternate season (October to November) selected because of its differences from the target season, so the predicted results are applicable outside this temporal window*". Similar to the Chevron report, it is not possible to justify that it is applicable for other temporal windows if not assessed. Also, the "difference from the target season" has not been evaluated.

General Conclusions

Both reports use a predicted no-effect concentration threshold of 6.5 mm to occur (Smit et al 2008). However, none of the scenarios presented ever went above the thresholds. Since this no-effect threshold seems important and was not exceeded during these scenarios, it is worth recalling some hypotheses that may have led to this result:

1. Particle size distribution of cuttings are unknown (stated Section 2.4). A choice was made towards using a single distribution (rather than a range of possibilities), see Table 2.4. The rationale for using this distribution is not provided. This distribution contains a large fraction (60-70% of fine silt/clay) that likely never settles in the model and thus does not contribute to the accumulation here. It is suggested to make other scenarios with different distributions in order to have a range of possibilities.
2. Vertical diffusivities used in the model (K_z mentioned above) are possibly 1 to 2 orders of magnitude higher than what is measured in the deep ocean. Numerically, this has the consequence of keeping particles in the water column and preventing them from settling faster. A more appropriate parameter may increase the deposition at the bottom.
3. Flocculation / agglomeration of fine particles is not accounted for in the model. Although difficult to model, this process is known to occur and has the consequence of increasing particle settling velocities (by forming larger aggregates). If this process was taken into account, more sediment would reach the seafloor.
4. A single run was made per season using currents from 2012 only (currents that have *not* been validated with observations). How is it possible to know that this is not just a coincidence and that using currents from another year would lead to another distribution? A stochastic analysis (repeating the same scenarios over multiple conditions) is recommended.
5. In the Chevron report, the mud is released 20 m above the seabed and 5 m below the sea surface but in BHP it is released 5 m above the seabed and 10 m below the sea surface. Is this difference related to different shapes in the well/drill or is it an arbitrary choice? How sensitive are the results to these choices? An explanation of why the depths of the release points are different is required.

The importance of thorough sensitivity analyses and validation is critical in any study such as those presented in these two reports. Further, the assumptions must be clearly articulated and based on scientific literature. This approach was lacking in the reports and should be corrected to ensure the best available information is used during decision making.

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Sources of Information

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