Framework for Ecotoxicological Modeling of mCDR (FEMM) v0.1 Comments from Public Consultation Period: July 1st - 31st, 2025

The FEMM v0.1 draft document was released for public comment on July 1, 2025 and advertised on the Hourglass Climate website, social media platforms, personal outreach, short presentations at a range of workshops and meetings (including the MACAN State-of-the-Science Annual Meeting and the Carbon Business Council monthly meeting), and an email listserv. The public comment period subsequently closed on July 31, 2025 although some comments were received after this time. Public comments were reviewed during Q3 2025 and addressed in a revised version, FEMM v1.0, published on February 5, 2026 to hourglassclimate.org. Comments from this consultation period are provided in the table below, anonymously, for traceability and transparency.

The public comment phase intended to broaden engagement and gather insights from practitioners, researchers, and members of the public with an interest in marine carbon removal, environmental risk, or ocean governance. The document was downloaded by sixty (60) individuals over the course of the comment period. In total, feedback was received from 13 reviewers across academia (7), industry (5), and government (1) spanning Europe, US, Canada, and Australia.

Future public comment periods will follow a consistent process, with public notice and an opportunity for stakeholders to provide input. After each round a summary of public feedback will be published alongside the finalized version of each update.

If you have questions or comments related to this document please contact femm@hourglassclimate.org.

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All comments on FEMM v0.1 are presented in the table below. The “Section” column identifies the relevant section number or figure from FEMM that a comment corresponds to. Any comments not tied to a specific section or figure are grouped under “general".

Section	Comment
1.1	Project developers may also use FEMM to identify low environmental risk project scales/scopes (in addition to sites).
1.1	While individual suggested pathways are designed for flexibility, the framework as a whole is intended to support consistent and transparent evaluation of environmental risk across the mCDR sector. What is meant by "individual suggested pathways?"
1.1	To clarify the scope, it would help to state explicitly whether biotic approaches (like fertilization or terrestrial biomass sinking) are included, since only OAE and DOC are mentioned.
1-2	Clearly articulate specific hypotheses or decision contexts FEMM is intended to support (e.g., regulatory approval, monitoring design). At present, objectives are too broad.
1-2	Include a diagram showing how FEMM fits into the wider ERA–MRV workflow for OAE projects.
2	Recommend using DOCS (direct ocean capture and storage) in the context of CDR. Otherwise DOC is just a way to capture CO_2 from the ocean, but the CO_2 can be utilized and not stored for CDR.
3	Develop a prioritization scheme for trace elements likely to be released from OAE feedstocks (Ni, Cr, V, Co, Fe). Without prioritization, users cannot focus limited resources.
3	Incorporate carbonate chemistry as a stressor more rigorously. The current treatment is descriptive but lacks quantitative thresholds or proposed endpoints.

3.3	I may have missed it, but I would have liked to see an explanation of why OAE and DOCCS are included as the two techniques for FEMM. OAE makes sense since there is introduction of trace metals and nutrients with these techniques, as mentioned in Section 3.2. However, Section 3.3 does not give a clear reason why DOCCS was chosen as the second technique to focus on here since it does not have as much concern with ecotoxicology. The other obvious technique to include here would be ONF. I would like to see why that DOCCS was selected here instead of ONF. Or, if ONF was not favorable to include here, then why not just focus on OAE? Omitting a section 4.3 that also focuses on potential impacts of DOCCS after showcasing OAE in 4.2 also makes it seem imbalanced. This is also imbalanced in the case studies and most of the remaining framework
3.4	When referencing the MRV and eMRV protocols, it would be great to see some concrete references, especially since there are large generalizations stated here. Given that eMRV is not always a widely accepted term, I am especially curious to know which eMRV protocols these statements are referring to. It might help to soften the language around these and say that "some procoals" rather than saying statements such as "eMRV protocols are often minimal, qualitative, and presented without structured frameworks...."
3.4	Similar comment about the regulatory pathways. I think it may be and overstatement to say that "permitting agencies can lack the internal expertise required...". Additionally, it might be worth noting that some projects are proceeding without the need to request new permits
3.4	It could be helpful context to mention that MRV protocols often rely on regulatory compliance and permitting processes to evaluate environmental risks.
4	Somewhere in the introduction please recognize and describe the multiple pathways of exposure to trace metals. Is this via dissolved metals?, direct particle exposure via burrowing or ingestion?, BioMagnification via the food web?, Other? A schematic would be very helpful.
4	Suggest discussing the importance of olivene particle size to exposure pathway of trace metals and carbonate system changes (likelihood of dissolution, ingestion, and absorption), etc.
4	Add explicit trigger values for moving from screening-level SSDs to more refined mechanistic modeling. Currently, the transition is vague.
4	Provide a worked example decision tree that shows how a project developer

	would proceed through FEMM tiers.
4	In the summary of the section, there is the mention of “site-specific”. This is critical aspect in term of sensitivity and local adaptation (e.g. Vargas et al., 2017, 2022) but it is not super well developed in the section before. I would expand.
4.1	It would be helpful to also mention the area of impact (i.e. the time period and area where changes could exceed natural variability), as the impacts are limited in space and time, and dilution increases with distance. The area of impact is also site specific, which would connect well with later discussions about how mechanistic models simulate stressor dispersion and impact under dynamic environmental conditions.
4.1	Worth mentioning that ocean acidification and climate change may also alter community composition.
4.1	Feels like the site-specific risks were not discussed that much, even though it is listed as a key takeaway. Expanding a bit more on site-specific risks could help readers who are less familiar with how factors like background ocean chemistry, physical dynamics, and species adaptation vary by location.
4.1	We suggest to change "for OAE these changes are driven by increased alkalinity though DIC stays constant" for "for OAE these changes are driven by increased non-equilibrated alkalinity"
4.1	DOC releases water with regular TA and DIC concentrations of about 20-30 umol/kg (if regular air is used to strip the beforehand acidified seawater off CO2). Hence, while pH will be high, because of low DIC, Omega will be very low as well. The water will actually be corrosive to calcite and aragonite, prior to dilution
4.1	Extrapolating OA data when it is in a quite narrow fCO2/pH range (e.g. from 400 to 1000 uatm) to OAE conditions is problematic. That's why the 2025 Bednarsek study suggested that some species might react positive to OAE, which might be the case for relatively mild application. However, there will always be a point when pH/fCO2 become so unfavourable that there will be negative effects (see Fig 3 on the right, taken from Krug et al. 2011, doi:10.5194/bg-8-771-2011). Everything is an optimum curve response, i.e. on both extreme ends there will be an effect, and depending on the experimental range, one would extrapolate positive, negative or no effects , or one might actually see the optimum curve (see Fig. 4 on the right from Bach et al, 2015, http://dx.doi.org/10.1016/j.pocean.2015.04.012). I am not

	saying that the 2025 Bednarsek study is not useful, it just has to be used carefully.
4.1	I would suggest structuring this section around the optimum curve concept, which explains the varying responses that have been reported, depending on the carbonate chemistry range. It is noted, though, that for some metals, the response is only negative as the metal itself is not used by the organism.
4.1	For DOC, it might be useful not only to test the rebalanced normal TA/low DIC solution with high pH and low Omega at various dilution levels, but also the intermediates, i.e. low TA after acid addition (high DIC) as well as low TA and low DIC after aeration, as any planktonic organism present in this water will be exposed to these carbonate chemistry swings.
4.1	There is a mention of “shifting balance” of the ions and “increasing carbonate ion availability” as an argument for benefit for calcifying organisms. This is a common idea in the literature but conceptually wrong. Most marine calcifiers are not using carbonate ions for calcification but rather bicarbonate ions and rarely directly from the seawater but rather from transformation of metabolic CO2 into bicarbonate. I can send references if you want and it would be good to rephrase. In that section, there is a clear mention of changes in levels of different aspects of the carbonate chemistry but I would also highlight the change in dynamics linking to mCDR implementation. A change in the timing and level of variability is likely to have strong impacts in itself. For example, we showed that increased variability related to blue carbon ecosystems was amplifying the negative effects of ocean acidification rather than benefiting it (Cossa et al., 2024).
4.2	The types of metals that may pose ecological risks are indeed material-specific. Equally important, but not addressed in the current text, are the time scales over which these metals are released and the environmental compartments in which they accumulate. These factors are also material-dependent and highly relevant for risk assessment. For example, ocean liming involves rapid dissolution in the water column over short time frames (minutes to hours), representing an acute exposure scenario to dissolved metals. In contrast, slowly dissolving minerals such as olivine primarily release metals at or within the seabed over much longer time periods (years to decades), leading to chronic exposure scenarios in which organisms are exposed via contaminated porewater, sediments, or food sources. This important distinction is not discussed in the document, although I believe it warrants attention. Which type of exposure scenario is the risk assessment in this framework intended to address? It seems primarily focused on trace metal exposure in the overlying water column.

5	I am curious what happens to the number (I assume majority) of species and environments that do not have PEC and PNEC defined for them. It would be great to see a note about that process. I see in Section 6 you recommend extending the dataset with more species if you do not have it. Maybe this could be reflected somewhere in section 5 as well
5	This is really helpful and surpsingly simple to understand (I appreciate the elegantly simple diagram, but the devil will be in the details!)
5	Acknowledge the lack of marine chronic toxicity datasets for key metals (Ni, Cr, V). Extrapolations from freshwater or acute data are not reliable.
5	Identify research priorities for carbonate chemistry SSDs, including target taxa, endpoints, and exposure ranges.
5	Provide guidance on how many species and endpoints are minimally required for a robust SSD in marine ERA.
5, Figure 1	If there are species of particular concern, could these be included in the SSD from the start to reduce the need for additional mechanistic modeling?
6	Would you consider providing guidance on what constitutes an appropriate number of species included in SSDs evaluation? [Think I answered my own question on this later on, where you note that project-oriented SSDs should include data for at least 10 species spanning 8 taxonomic groups]
6	Perhaps reframe wording of alkalinity included in the list of what induces stress through "direct physiological mechanisms" - technically, alkalinity does not directly impact organisms, it's the individual carbonate chemistry parameters that shift in response to alk perturbations that induce physiological responses e.g. HCO_3^-, CO_3^{2-}, TA:DIC (which you note here)
6	Would developing SSDs for carbonate chemistry parameters be something Hourglass might pursue? I think it might be helpful to contain a bit more narrative throughout the FEMM on where you're envisioning responsibility might lie on development of the science that is important e.g. non-profit env orgs, or project developers themselves (left unclear as written) - understand you might not view it as your role to mandate, but could be cool to introduce options for parties who could do this work particularly if you have opinions on it
6	Case Study 1 interprets ecotoxicity results in the context of the SSD for Ni,

	considering realistic Ni concentrations in real environments. Why is this same consideration not explicitly applied to alkalinity additions—particularly relevant for trial tests where the PEC of alkalinity falls within the range of normal variation?
6	Highlight that few marine BLMs exist beyond copper; urgent development is needed for Ni, Cr, and Co.
6	Clarify data requirements for TKTD models and acknowledge that most OAE-relevant species lack such datasets.
6	Explicitly state that PBTK and community-level models are not yet validated and remain research priorities.
6	Add guidance on scaling from individual- to population- or community-level outcomes.
6.1	ECx and LCx used in parenthetical before defining these terms
6.1	Freshwater SSDs dictate the inclusion of a minimum of 10 species... Is there reason to expect that this standard might not be sufficient for marine environments? Or guidance for how the recommended minimum might vary by location if a site is located in a particularly species-rich environment or sensitive habitat?
6.1.1	In addition to citing the DeForest et al. (2013) paper on nickel species sensitivity distributions (SSDs), I recommend including the study by Gissi et al. (2020) (Environmental Toxicology and Chemistry, 39(12), 2540–2551). This paper presents SSDs based on more recent ecotoxicological data and offers a comparison between temperate and tropical species, an aspect that is mentioned in the current text and is therefore highly relevant to the discussion.
6.2	The illustrative Ni example here was great for seeing how this could be applied to a project. Great inclusion! I'm still a little confused about a few things, though: 1) how does the SSD curve apply to the species found in the project? Is the objective to show that for the species found at the project site, the impact from the project will be 0? If so, why construct the SSD at all, rather than just show the minimum value at which a species in the project could be impacted and see if the project will change the chemistry above that? 2) What do suppliers do if there is not sufficient data to create the SSD from ecotox studies? 3) ecotox studies generally refer to acute shock, but many of the concerns related to changing chemistry are

	related to chronic exposure. How do you deal with that? 4) How do suppliers identify which species might be of concern in areas with relatively low governance (i.e. the regulatory agencies cannot be relied upon to determine species of concern)?
6.3	Extrapolations should ideally not be required when for each application (certain material sourced from a certain area/supplier with a certain size distribution) in a certain location, no data will be available. Hence, a new FEMM should be required. Hence, there will be no data available. Also, what about seasonality, as sensitivity probably changes with, for instance, temperature?
6.3	The term "high quality data" should be clarified, as there are established guidelines for what constitutes high quality ecotoxicological data. For example, Table 1 in Gissi et al. (2018) (Environmental Pollution, 218, 1308–1323) outlines specific criteria used for data evaluation. I recommend briefly elaborating on this in the text to provide context and transparency regarding the data selection process.
6.3.1	This is super helpful and addresses one of my concerns from above!
6.3.2	Does freshwater to saltwater extrapolation mainly apply to trace metals or could this also be applied for e.g., pH or alkalinity? If not, note limitation.
6.3.2	Given the uncertainties with including or excluding tropical/temperate species in the Case Study 1 example, I don't think that Freshwater data should be used for a marine assessment (it's often also species that are not found in a marine environment). Furthermore, as the assessment should be site/application specific, there will be most likely no data that can be used. I am not sure, but it would be good to advocate for treating not mineral, such as olivine, as the same, as it will be different (in composition but also environmental impact because of varying dissolution kinetics), depending on where it was sourced and what grain size distribution it has. On this note, dissolution kinetics (and secondary CaCO3 precipitation) are highly temperature dependent. So, only data with a representative temperature should be used in an SSD (seasonality).
6.3.3	How can a QSAR be used to assess species responses to changes in pH/fCO2, if they infer toxicity from the molecular structure of a substance?
6.3.4	I don't think that adding species through ICE estimates will make an SSD for a certain location/application more robust. If anything, it will add more uncertainty.

6.4.1 6.4.2	I would advocate to first do a whole-mixture assessment, and then if of interest (in the end it does not matter if an organism dies because of high pH conditions or the leached nickel). an individual-constituents test (which can also be a lot, when there are a number of trace metals present in a feedstock). On that note, we have also seen that the amount of trace metal present in a material, is a poor predictor of how much actually dissolves in seawater (see Fig 3 to the right taken from Moras et al. 2024, doi: 10.3389/fclim.2024.1396487). So, how much gets into solution would need to be determined for each element, on order to ge thresholds for the mineral. The only exception might be an application where dissolution kinetics are so slow that the feedstock would accumulate in the sediment and metals could accumulate in pore waters.
6.4.3	This diagram is also super helpful! Do the 10 species identified matter depending on their level of sensitivity or protection?
6.4.3	As mentioned above, the whole-mixture approach is what is important in the end. It matters how pH is changed, either by a liquid, a slow dissolving mineral or a fast dissolving one. Also, direct particle effects on higher trophic levels also depend on grain size. So, the actual prodcut that will be used for OAE must be tested. Concerning the problem of changing a supplier /getting a different batch, the chemical composition should be tested and the most senstive species could be re-tested if the deviation is larger than XX%. Finally (these are just my 2 cents and more a gut feeling based on our slag tests), I think that the carbonate chemistry changes are what impacts organisms (apart from potential direct partilce effects), not trace metal contamination. If so, then slight changes in chemical composition do not matter if we are looking at pH/fCO2 thresholds.
6.4.4	The last sentence of this section is not finished: "Mixture models have also been used to".
6.4.4	Sudden end of the subsection?
7	Providing more guidance on model requirements—especially around validation and data needs—would be helpful. For example, is one validation of a species-chemical pair sufficient, or is ongoing validation needed?
7	Takeaways do not mention anything about the PECs, that are, in many ways as important as SSDs.
7	Avoid implying readiness for deployment; emphasize that FEMM is a conceptual framework awaiting validation.

7	Provide a roadmap with timelines, responsible parties, and priority experiments for developing FEMM into an operational tool.
7	Strengthen the ERA–monitoring link: show how FEMM outputs (e.g., PEC/PNEC ratios) inform field monitoring design, choice of sentinel species, or biomarkers.
7	I don't think that this is needed for carbon credit accounting and risk assessment for a particular OAE operation at a certain location. There, one only wants to make sure that a certain feedstock does not do harm, and that needs simple holistic testing at representative environmental conditions using key organisms. Mechanistic models are nice for a deeper understanding but even for Ocean Acidification and 20 years of research, there is little mechanistic understanding (apart from the optimum curve response which is been explored so far in two coccolithophores only and has shown that there are interactive effects with light and temperature that have to be known if doing predictions/projections). Hence, for OAE mechanistic understanding for certain taxa might evolve over time, but that would need dedicated scientific studies.
7.3	The section on future directions for mechanistic modeling should be labeled as 7.4 rather than 7.3.
7.2.1	It may be useful to specify for which type of stressors fate or transport models may be most appropriate. For example, I can imagine that in the context of carbonate chemistry changes, transport models are typically more relevant than fate models, as fate models are more geared towards the introduction of pollutants.
7.2.1	This comment is about the sentence: "Outputs from these models—typically PECs—can be compared to EQS thresholds—like those derived from SSDs— or passed into downstream biological effect models for further refinement." Ocean outfalls are a standard approach envisioned by most OAE and DOC initiatives, so this example would be very valuable to the mCDR community. For example, we suggest to add "In the case of coastal outfalls, PECs are the concentrations beyond the mixing zone, where water quality standards are typically required to be met."

7.3.4	In section 7.3.4 Effect Modeling for Carbonate Chemistry Perturbations, the statement that “To date, no mechanistic modeling approaches have been developed to assess the potential effects of localized carbonate chemistry changes” is probably too strong. There is a community of researchers who have spent some time developing mechanistic, physiological models of calcification in various marine organisms (corals, coccolithophores, bivalves, etc.), both in the course of their normal life history and in response to ocean acidification. Here’s just one example of such a study among others. Although they do not reference CDR or OAE specifically, these types of papers would serve as the natural jumping-off point for mCDR and OAE-related work.
7.3.4 figure 8	What is meant by "is bioavailability a relevant concern for your site?"
8	Would you advocate for use in both site characterization/selection, AND project specific risk assessment?
8 Priority 1	I think it's excellent to indicate where data collection can go beyond the minimum needed for the immediate priority to support progress toward the longer-term priorities. Are there any other items for which similar recommendations could be indicated? This is especially relevant for the research community as we think about experimental design frameworks.
Appendix A	Annelida are not just surface deposit feeders (especially Capitella - which is a subsurface feeder). Surface feeders coud include spiondis or terebellids. Subsurface feeding worms, such as lugworms or Capitella would ingest particles in sediment.
Appendix A and elsewhere	Recognize vulnerability of different life stages to metals and carbonate system change, in addition to differences among species. Larval assays should be proposed in Appendix A.
Case study 1	I would refer to Gissi et al. (2020) here instead of Deforest et al. (2013)
Case Study 1	This case study was very helpful for illustrating the difference in general vs. region-specific SSDs; it might be helpful for the reader to include a plain language statement earlier in Section 6.1 or 6.1.1 describing this distinction, as I don't think Section 6.1 was explicitly noted as describing "generalized" SSDs until the idea of region-specific SSDs was introduced in 6.2.
Case Study 1: SSDs for Nickel in Marine Environments	For the SSDs, which metric is used for the various species, e.g. EC10, EC20 or LCx. In general, there should be only one metric that is recommended, right?

Case Study 1: SSDs for Nickel in Marine Environments	In general, I am not sure what regulators typically require, but deriving an HC5 from an SSD with 10 species is a bit problematic. The reason being that there are no datapoints (1 species represents 10%) in the region where the fit is used to derive HC5, so the fit is not constrained. The case study kind of nicely shows that that HC4 can change by a factor of more than 5, depending on including or excluding some data point (and this study had 17 species). So, there is a inherent and large uncertainty in HC5 based on 10 species, so somehow a conservative and cautious interpretation should be adopted.
Case Study 1: SSDs for Nickel in Marine Environments	How do regulators go about the potential problem of testing twice as many species as required, and then constructing the SSD from half of the species most insensitive? Furthermore, it should be mandatory to test both larval and adult stages, as the latter are often more vulnerable. Then the LC10 value for the more sensitive life stage in the SSD should be used.
Case study 2	Bottom water concentrations are used for the risk assessment; however, the figure reference to these concentrations (Fig. 8A) appears to be incorrect, as the corresponding data are not shown in the document. Additionally, for sediment-dwelling organisms, porewater metal concentrations may be more relevant than those in bottom water. It may be worth mentioning this distinction to clarify the ecological relevance of the chosen exposure concentrations.
Case study 2	How frequently should environmental metal concentrations be monitored in the context of risk assessment, and how should predicted environmental concentrations (PECs) be estimated prior to the deployment of alkaline minerals? Including a dedicated section on PECs—covering both estimation methods and monitoring strategies—would be highly valuable for guiding future risk assessments.
General	In some places, there are regulations on metal loads in marine sediments (solid phase) which for OAE would likely be interpreted at the metal content of the feedstock, but likely should also consider the metal load of the sediment after it enters the ocean, begins dissolving and metals adsorb onto sediment surfaces. How does FEMM address solid phase regulation and safety? As an example, there is such a regulation in Florida (e.g. McDonald paper) and mandated sediment testing.
General	I appreciate how this framework is leaning on existing measure to assess the impacts and mirrors existing pathways. I like the diagrams for each portion of the framework looking at the necessary steps. I would have liked to see a figure that outlined the overall different modulees and highlighted when a project should use the modules. This would have been nice in the beginning or end to

	show how to interact with the different sections. Overall, I think this is a great addition to the community and wonderful, detailed work!
General	- in general I strive to not focus solely on CO2 removal for all of my projects. But it has multiple advantages of which CDR is one. But I realise I am still one of the only with this view. Let me give some examples: - not adding olivine or basalt to agricultural lands for the sake of CO2 removal. But start with the plant nutrient requirements and then determine if olivine (or basalt or other materials) can deliver this. Then calculate the nutrient requirement and from this the dosage. And then finally as a outcome the CO2 uptake. - for greensand with their civil olivine applications, this is more about replacing conventional sand/gravel. And secondly about the CO2 storing (which as a consequence will be limited as the replacement volume, particle sizes are determining the CO2 uptake. - at Paebbl we are storing CO2 and making an alternative for cement. This approach avoids quite some pitfalls and makes it more acceptable and also more economically. Now applying this to marine applications. I would perform the OAE experiments in combination with i.e. sand replenishments. While doing this (in order to prevent sea erosion or making coastal defences) the nature is already disturbed, with turbidity, movement etc. Then adding these minerals to this will probably have less consequences then applying this at a more untouched beach. And secondly as the materials will have a different physical behaviour it can also be used for making these sand replenishments last longer. Olivine is more dense as normal sand, it can be grinded to coarser materials. And placing it at the right place can result in less coastal erosion. Another example for DOC is to apply this at the outlet of desalination plants. Where I expect also the negative effects will be smaller as compared to the open ocean. Both of these approaches will limit the CDR potential, but I hope with less possible negative consequences. But I realise quite some of the new companies want to be able to scale to Gigatons ;-(.
General	Would be helpful to make explicit which parts of the framework are ready to go today.

General	This is an excellent and much-needed resource that I found to be both accessible and rigorous in detail. Thank you for doing the hard work of bringing existing ERA processes into the mCDR space! The key takeaways at the end of each section are quite helpful for the reader, and I wonder if adding a short, plain language summary at the top of each technical section would further increase accessibility for non-specialists.
General	taxa' is plural and is used incorrectly in a few cases (the term taxa-specific doesn't make sense; do you mean taxon-specific?
General	An alternative to using the terminology 'direct ocean capture' (DOC) could be 'direct ocean carbon capture' (DOCC). This avoids the ambiguity with 'dissolved organic carbon'. Combined with storage (for CDR) or utilization, the term DOCCS and DOCCU can be used respectively, but DOCC remains agnostic about how captured CO2 is further processed. Figure 4 mentions DOC, probably in the context of dissolved organic carbon, highlighting why such disambiguation is useful.
General	When selecting organisms to include in ecotox tests, relevant ones should be picked, e.g. planktonic/watercolumn species when feedstock will fully dissolve in water column, and also benthic ones if that can not be guaranteed (slurries or slow dissolving minerals such as olivine).
General	We would like to suggest adding a short section titled “Applicability to Sediments,” which explicitly explains that (a) the FEMM framework is fully applicable to solid phases like olivine concentration in sediments, but also clarifies that (b) the phase of the material is paramount when considering solids/sediments. For example, the toxicities of Ni metal, stainless steel, olivine, chemical plating waste, and absorbed/exchangeable Ni are likely to be very different. This likely requires either assessments of specific materials (olivine in sediment, for example) or an assessment of a “bioavailability factor” (e.g., TCLP test, exchangeable nickel, weak acid leach, etc). Simply specifying the allowable bulk concentration of nickel in sediments might well be a conservative regulatory limit. Still, it’s almost certainly overly conservative because, by necessity, such a limit would have to assume the most bioavailable forms of solid-phase nickel to ensure it was fully protective.
General	We recall in previous discussions that this would be offered as an online tool, and would encourage mentioning that this is the intended evolution of this framework. We believe that numerous stakeholders will find the tool useful, so highlighting that future work seems like it would be helpful in generating excitement about this project.

General	The FEMM document talks more about the chemical risks of mineral OAE than electrochemical approaches. That is fair given that minerals contain more trace metals. However, most desalination plants, for example, have to extensively pretreat the water, including flocculation, filtration, chlorination/dechlorination, anti-scaling compounds, etc. (see here for one review: https://doi.org/10.1016/j.jwpe.2019.100926). It seems likely that electrochemical OAE suppliers would probably have to adopt similar approaches to avoid biofouling and pretreat their process water, etc. Moreover, these electrochemical approaches may also release trace metals from electrodes and may generate undesirable electrochemical byproducts, such as trihalomethanes (THMs) and haloacetic acids (HAAs), dioxin, furans, etc, which are currently regulated in the US. Here's one paper discussing this: https://doi.org/10.1021/acs.est.0c03916. That reference isn't specific to electrochemical OAE, but all these electrochemical reactions have similar risks of forming chlorinated byproducts, especially when there are complex trace organics in the feed water. Hourglass may want to review or inquire what work, if any, other companies have done on these issues to add to FEMM.
General	What do suppliers do if the outcome of one of the flow pathways determines that low risk is not established? Do you include mitigation efforts that then must be remodeled to establish that low risk is established?
General	**Initial feedback:** [FEMM] works well in the context of toxicants such as metals associated with OAE. In that case, you expect a dose-response curve, some relatively well-defined thresholds (and established standardize methodologies). You make excellent point on how to deal with multiple different stressors using a "concentration addition" approach, assuming no-interaction and lead to non-linear additivity. If done right (and based on good data for the individual stressors and good understanding of their mode of action), that can be implemented and tested. pH, pCO_2, or other aspects of the carbonate system are not toxicants and part of the environmental niche. They vary significantly in space and time and are biologically relevant (in particular pH and pCO_2, see comments below). This leads to local adaptation and strong differences in sensitivity (e.g. see Vargas et al., 2017; 2022 in the context of ocean acidification). Another consequence is that thresholds for such parameters are extremely location specific (population/species) and also highly depending on other parameters. For example, pH is critical to life and there are many key physiological processes to maintain pH inside cells and fluids. These are quite energetically costly (acid-base regulation) and thus very sensitive to other energy-related parameters such as temperature, food, oxygen, etc. In other words, do not expect one threshold, these thresholds are never absolute but relative to the present range of variability, and rather a landscape of response.

	Follow-up feedback: Carbonate system stress is meaningfully different from toxicant stress. Carbonate chemistry is part of the environmental niche, and local adaptation can lead to strong sensitivity differences. However, if "dose" is described as deviation from baseline, more traditional toxicant approaches may be helpful in describing these stressor-response relationships because it accounts for the extreme location-specificity of baseline carbonate system parameters.
General	**Initial feedback:** The performance curves of such parameters are not following a classic ecotox dose-response but rather a unimodal response. You have theories justifying these curves and how they are modulated (e.g. see Falkenberg et al. 2018). You can expect direct (e.g. change in toxicity) and indirect (e.g. change in sensitivity) interactions between carbonate chemistry changes associated with mCDR and toxicants (e.g. Nickel and other metals). This seriously complexify the resolution of the multiple stressor problem and cannot be solved through the mixture approach that you are proposing. This would require models that are considering ecotox approach with a natural driver approach, considering non-linear effects using different types of performance curves (dose response, unimodal) and interactions. **Follow-up feedback:** A number of distributions will have to be explored to determine the most appropriate statistical models for describing carbonate system stressor-response relationships. This will likely include both monotonic and non-monotonic models. The magnitude of responses to stressors will also change with environmental factors, such as temperature. DRCs and SSDs will inherently flatten that nuance. It is important to ensure that this simplification does not lead to the underestimation of risk and that SSD-based screening assessment is adequately protective. There is more potential to incorporate these types of dynamics [temperature dependency, etc.] into more advanced mechanistic effect modelling.
General	**Initial feedback:** These parameters tend to be highly dynamic in time (e.g. diurnal variability) and mCDR can mess up with this. This is a parameter to consider by itself (e.g. see Cossa et al. 2024; Duvane et al., 2024). **Follow-up feedback:** An SSD-based screening approach will not be able to capture time-dependent processes. There is more capacity to address dynamic processes through mechanistic modelling. Carbonate chemistry parameters are not toxicants, but they do induce physiological stress. The common currency of physiological stress is energy (relates to metabolism and other downstream biological processes). This is also the foundational principle of DEB modelling, so

	it creates a natural segue into the mechanistic modelling approaches. More advanced mechanistic modelling approaches can more easily incorporate dynamism and nuance.