

Appendix 1:

**Responses to anonymized feedback
collected during Foundations
comment period**



**Cascade
Climate**

This document represents the feedback provided on the initial “Foundations” draft, which was shared on June 28th, 2024, and the eventual result of that feedback within the final “Foundations” document. The draft was shared with Working Group members and market practitioners who participated in the community process and who actively contributed knowledge and technical expertise that aided in the drafting of the “Foundations” document.

Please note that comments may have been amended for the purpose of anonymity. In addition, comments that were expressly intended for the Cascade team have also been removed.

We would like to express our deep appreciation to all of the reviewers.

Section	Reviewer comment	Cascade response	Resolution
Section 1	Dissolved Inorganic Carbon (DIC) and alkalinity use are interchangeable through the whole document. There is a slight difference between the two calculations. Dissolved Inorganic Carbon (DIC) is based on Bjerrum pH relationship (link in row 17 below) and relies on acid dissociation. Alkalinity calculation relies on water and acid dissociation (Alkalinity- USGS row 18 below). Which to use depends on instrument availability. DIC measurement is more accurate due to a better established mathematical/theoretical relationship, however, it requires a larger capital investment. Such differences will be better if highlighted in the text under section 8.3, maybe.	Thank you for pointing this out. We've made adjustments to the text to define alkalinity and DIC more explicitly in Section 2 and modified additional text as needed.	CHANGE MADE
Section 1	Consider including a more explicit introduction to the function of this document, and the principles guiding how you have written it. For example, it could be helpful to give an introduction to: <ul style="list-style-type: none"> - contents (see general comment above) - language norms (see general comment above) - accounting principles (e.g. goals the accounting content is meant to serve, role of operationalizability, use of conservative accounting choices, desire to distinguish b/w removals and avoided emissions, bias toward advocating for more research in the face of large uncertainty, etc.) - process (e.g. role of working group, steering committee, cascade team, and industry input; how the information was synthesized; how recommendations were settled on; etc) 	Thank you for this suggestion. We've added more information in Section 1 and 2 with respect to the process and terminology.	CHANGE MADE
Section 1	I was looking for a general distinction being made from weathering of technical/human-aggregated materials like concrete instead of "natural rock" feedstocks. Maybe it's in there somewhere, maybe you can highlight/add?	Making a binary distinction between "natural" feedstock and "technical/human-aggregated" feedstock is unlikely to capture the range of factors that are material for assessing feedstock-dependent impacts and risks. The Foundations document recommends practitioners to conduct the quantification assessments required to understand underlying impacts and risks.	NO CHANGE

		Specifically, Section 6.2.3 recommended an approach to understand the potential health and ecological risk assessments associated with heavy metals present in feedstocks. Section 10.1 recommended an approach to conduct life cycle emissions accounting associated with the extraction, processing and handling of the feedstock materials.	
Section 1	I would omit the word "waste" here, as it implies that other feedstocks (silicates and carbonates) are necessarily also waste. Some feedstocks are already intentionally mined and processed specifically for ERW.	Thank you - change incorporated into the document.	CHANGE MADE
Section 1	I suggest replacing the word "done" with "conducted".	Thank you - change incorporated into the document.	CHANGE MADE
Section 1	Curious as to the logic behind including carbonates as a potential feedstock? I think the argument for additionality is much more difficult to make for carbonate feedstocks, and the risk of non-carbonic acid weathering is more immediate considering CO2 would be released.	<p>Thanks for this question and discussion. Both carbonates and silicates are considered potential feedstock materials because both have netCDR potential, and the quantification considerations laid out in the Foundations document are equally applicable.</p> <p>In defining the scope of the Foundations document, the Cascade team is primarily concerned with identifying the subsets of feedstock materials (in this case, alkaline feedstock) and the subsets of terrestrial ecosystems (in this case, agricultural soils) that can use a shared framework for netCDR quantification. Inclusion or exclusion from the scope does not translate into appropriateness for crediting activities.</p>	NO CHANGE
Section 1	This document excludes ERW done in other terrestrial ecosystems such as forests and non-managed soils. Just like other decisions are very well explained throughout the document, I think it would be interesting to explain in a few	Thanks for catching this. Discussion on the rationale behind the exclusion of forests or non-managed soils is now included in Section 1.	CHANGE MADE

	words/sentences why such terrestrial ecosystems were chosen to be excluded.		
Section 1	To me, part of the executive summary of the main scope should to be: what is the point of creation of a durable sink through EW and how do we assess the risk of reversal beyond this point; for an extensive and technical document like this, info boxes & high-level take-aways throughout the Foundations would be super helpful; You discuss a point of "initiation of carbon removal" but the first point in	Thank you for this suggestion. Section 2 provides discussions on how the document defines the following: - Potential CDR, which is created at the point of feedstock dissolution and used to describe the maximum amount of CDR that can be driven by the feedstock dissolution in the Near-Field Zone. - Losses, time lags and reversals, which all refer to reductions in netCDR relative to potential CDR, but differ in locations and time scale. Please refer to Section 2 for details.	NO CHANGE
Section 1	Would love to discuss the updating process further!	Thank you for this nudge. Changes made to the document to clarify and reflect Cascade's intent. Cascade sees value in facilitating follow-on case studies, analyses, and assessment reports that reflect progress in the fundamental science underpinning CDR quantification in ERW deployments, improvements in measurement and modeling methods, and learnings from ongoing field trials and early-stage commercial deployments. One potential follow-on work could be the facilitation of a community exercise that up-versions the full Foundations document, although it's too early to say when to facilitate that, in what cadence, and whether something like a Foundations v2.0 or v3.0 is the most appropriate approach to reflect progress.	CHANGE MADE
Section 1	How confident are we that standardization over time is actually a feasible or useful goal? A possible outcome of learnings from early deployments is that a certain level of project specificity	Thank you for this discussion. Changes made to the document to clarify and reflect the intent of the Cascade statement.	CHANGE MADE

	will be required for the foreseeable future, given the degree of heterogeneity between projects.		
Section 2	<p>Over time, the feedstock dissolves in soil porewater, consuming protons and generating alkalinity in the form of base cations like Mg^{2+} and Ca^{2+}. -----→ Technically alkalinity is not a base cation. I would really like this foundations document to be clear about what alkalinity is and how it is related to cations and anions. Alkalinity is "proton deficiency of a solution relative to an arbitrarily defined zero point (zero point usually being pK value of 4.5). This definition results in $Alkalinity = -[H^+] + [OH^-] + [HCO_3^-] + 2[CO_3^{2-}] + \text{conc other weak acids/bases}$. The reason alkalinity is related to base cations is because when we do a charge balance of $[cations] - [anions]$ from strong acids and bases, the excess positive charge is in most natural waters balanced by HCO_3^-. So the EXCESS Base Cations (i.e., those exceeding anions from strong acids/bases) end up equating to bicarbonate and bicarbonate in most natural waters ends up being the main component of alkalinity. Just adding a salt to a solution (e.g., NaCl), which would add a base cation, would do nothing to alkalinity. The sentence quoted from the foundations document should really say something like: consuming protons, which increases alkalinity, and produces excess base cations. I think it is important for the ERW community to be clear about what we are tracking and why. We can't just measure cations and call that alkalinity. I would prefer for this document to explain the relationship, like I did above. I think only focusing on cations is problematic because there are processes like CO_2 degassing from streams, which do not affect alkalinity (or cation/anion balance) but do affect the carbon balance. If the waters in the stream or ocean do not re-equilibrate with the atmosphere, this is a loss of carbon back to the atmosphere even if we did nothing to alkalinity (and thus nothing to the cation charge balance).</p>	<p>Thank you for this discussion and push to have clearer language around alkalinity. We've included a new section 2.1.1 to explicitly discuss alkalinity both from a proton and charge balance perspective. We've also tightened the language in other places with respect to this. We think these comments have greatly improved the document.</p>	CHANGE MADE
Section 2	<p>why forsterite in particular. I would add a sentence before this paragraph that the amount of CO_2 sequestered depends on the type of feedstock used and provide forsterite as an example. I</p>	<p>Thank you for this note, the suggested change was made to document.</p>	CHANGE MADE

Section 2	The state symbol for water is missing in these chemical equations. For completeness, this should be indicated as H ₂ O(l) as the reactions take place in liquid water.	Thank you, we've included water in the equation for CO ₂ to carbonic acid but otherwise the (aq) after the dissolved species already indicates that the solvent is water.	NO CHANGE
Section 2	It is better to add the carbonate mineral formation chemical reaction to make it easier to visualize.	Thank you for this push, the equations for the carbonic acid system, DIC, and carbonate mineral formation have been added to Section 2.	CHANGE MADE
Section 2	The term "shallow soils" may be misleading here, as it implies that ERW deployments primarily occur in areas with thin soil profiles. I appreciated that the intended meaning is that rock dissolution predominantly takes place within the topsoil layer (where the feedstock is applied), but this could be rephrased to avoid any confusion.	Thank you, the suggested change was made to the document. –	CHANGE MADE
Section 2	I would move the paragraph right to the beginning then provide the example on the proton removal -- rearrangement will make it easier to follow for non-science background readers.	Thank you for this suggestion. Section 2 has been significantly modified and parts of this paragraph are now at the beginning of this section.	CHANGE MADE
Section 2	charge-balancing HCO ₃ ⁻ -----→ In my view, it is the bicarbonate that is fundamentally what we have pulled out of the atmosphere. We have converted CO ₂ gas into bicarbonate. I don't see the bicarbonate as simply the charge balance of the excess base cation.	Thank you for this note, Section 2 has been significantly modified to include a more thorough discussion of alkalinity and this sentence no longer exists.	CHANGE MADE
Section 2	For geochemistry applications, I think "divalent" is the more prevalent term for cations with a +2 charge.	Thank you, suggested change made to document.	CHANGE MADE
Section 2	Production of acidity or removal of base cations from solution -----→ The only reason removal of base cations from solution decreases alkalinity is because the removal is often compensated by the addition of an H ⁺ . Competitive cation exchange that just releases another base cation will have no impact. It only reduces alkalinity if it bumps H ⁺ from the soil into the water. Same with plant uptake of a cation. Only will impact alkalinity if the plant spits H ⁺ out when it takes up the K ⁺ , for example. It isn't the removal itself that matter but the change balancing process that happens when that cation is removed. If the positive charge is balanced by H ⁺ then there is	Thank you for this discussion and push to have clearer language around alkalinity. We've modified this sentence.	CHANGE MADE

	an impact on alkalinity. If the positive charge is balanced by another base cation, there is no impact of note for ERW.		
Section 2	that of acid-base reactions and proton balance, or that of base cation release and charge balance. Either can be used to quantify the carbon removal of an ERW deployment. —→ I agree that the two lenses are helpful. I just think the base cation and charge balance aspect needs to be more clearly described mechanistically. Also, as I mentioned above, there are processes where the carbon balance can be affected but the base cation charge balance is not affected, like CO2 degassing. If the waters do not equilibrate fully after degassing, then there can be a net loss of carbon even with no change in alkalinity.	Thank you for this discussion and push to have clearer language around alkalinity. We've included a new section 2.1.1 to explicitly discuss alkalinity both from a proton and charge balance perspective. We think these comments have greatly improved the document.	CHANGE MADE
Section 2	This section starts off saying we can compute netCDR with cations and charge balance, but goes on to say "this can be done by... trying to follow the carbon directly by monitoring the export of DIC". Is DIC export more consistent with the proton balance lens identified in L315-318?	Thank you for this question. Rather than a question of which frame these measurements support, it is a question of what measurements are most useful for the practitioner to quantify CDR at their deployment site. For example, monitoring the export of DIC is a measurement of the integrated weathering flux (see Section 8.1) whereas monitoring the solid phase for cations will require quantification of individual terms in the NFZ term balance.	NO CHANGE
Section 2	quantifying how many base cations have been released from a feedstock —→ I think it is VERY IMPORTANT to emphasize that this is EXCESS base cations. [Cations] - [Anions] of strong acids/bases. Not just cation concentration. It is the excess positive charge that is balanced by bicarbonate.	Thank you for this reminder. We've added in excess in front of base cations where appropriate.	CHANGE MADE
Section 2	or have locked up CO2 in a carbonate mineral lattice —→ I am not 100% following this logic. If there is carbonate ppt then there is a loss of that base cations from solution. And do you really mean CO2 here? Or Carbonate? I think of the reaction as $\text{Ca}^{+2} + 2\text{HCO}_3^- \rightarrow \text{CaCO}_3 + \text{CO}_2$	Thank you for this push for clarity. We've modified it to say inorganic carbon rather than CO2.	NO CHANGE
Section 2	tracking weathering products (i.e. cations) ----→ again, this should be EXCESS cations	Thank you, change made.	CHANGE MADE
Section 2.1	Figure 1 caption: what is the average life cycle of the secondary	Thank you for nudging this discussion. The time it	NO CHANGE

	mineral precipitation before it is transported to a water body.	takes for secondary mineral precipitates to dissolve and be transported will be variable depending on water fluxes, pH, temperature, dissolved ions, and so on.	
Section 2.1	It is not 100% clear what the red versus green arrows mean? I am assuming a loss of carbon/alkalinity (red) and a gain (green)? I think the loss of carbon/alkalinity (red) associated with secondary mineral precipitation is a bit confusing at first when one thinks about the fact that carbonate ppt does sequester carbon (as stated previously in the document). it is just that it also releases CO ₂ . I think explicitly showing the chemical reactions in the text somewhere would be helpful for all these conversions. Also - back to the language around cations and alkalinity, I would suggest clarifying the biomass cation uptake is associated with release of H ⁺ by the plants -- so something like "Biomass cation uptake and H ⁺ release". Same for cation sorption. It is really "cation sorption and release of H ⁺ ." Cation sorption and release of another cation is not going to have any impact. What is MCO ₃ over the ocean? Is this ppt? Finally, I would include some of the transport going into groundwater and exiting from groundwater into the stream.	Thank you for these suggestions and we believe they've improved the figure and text. We've modified this figure for clarity and design although we haven't added all of the suggested additional text to the figure in order to keep it digestible and readable. However, we have made changes within the document itself along these lines.	CHANGE MADE
Section 2.1	I don't understand how the changes in OC balance are supposed to be depicted here. Am I missing something?	Thanks for this push. We've made some changes to the figure and to the caption to clarify the organic carbon component.	CHANGE MADE
Section 2.1	The Near- and Far-Field Zones are abbreviated here, but the subsequent text uses both the full terms and their respective abbreviations.	Thank you for catching this, changed.	CHANGE MADE
Section 2.1	netCDR is poorly defined at this point. You use it in the section title and then show some stoichiometry for olivine dissolution, but on the whole, it's not really clear what you mean by netCDR yet. Should probably define it as the balance of sources and sinks of CO ₂ directly resulting from the ERW intervention somewhere so it's totally clear what you're talking about. This can include both LCA emissions and field derived emissions. Should probably be clearly defined before the	Thank you for this suggestion. We've changed Section 2 significantly to introduce the term balance earlier and define some terminology.	CHANGE MADE

	diagram/schematic figure above		
Section 2.1	It may be worth justifying why "models and geochemical calculations" must be used for the FFZ instead of empirical measurements, as implied by these lines (or point the reader to a section that does so).	Thank you, a link to Section 9 is now included here.	CHANGE MADE
Section 2.2	There is no guarantee that a practitioner started their deployment with baseline sampling	Thanks for pointing this out, we've kept this in due to its importance for understanding the site.	NO CHANGE
Section 2.2	For example, base cations can be incorporated into secondary carbonate minerals and other phases that commonly form within the soil profile, reducing the carbon removal efficiency by removing alkalinity from solution AND "Carbon may also be durably stored in carbonate minerals formed in the soil profile or along the flow path." ----> I understand how carbonate ppt is both durable carbon storage and how it reduces efficacy of ERW. I just think the dual role of this reaction should be clearly laid out somewhere in the intro. Otherwise folks that are less well versed are going to be very confused why this reaction is a loss but also durable carbon storage.	Thank you for pointing this out. We've modified Section 2 to include the equation for calcite formation and more of a discussion around the formation of carbonates.	CHANGE MADE
Section 2.2	Citation needed	Thank you for this suggestion, we've added links to the Sections where this is discussed in more detail.	CHANGE MADE
Section 2.3	General point, there is a lot of discussion throughout the document of potential additional CDR in the FFZ despite ultimately landing on this not being counted as part of the CDR calculation. I agree with this, but wonder if so much discussion of it throughout the document is warranted.	Thanks for your thoughts here. We've kept the discussion since there is a lot of uncertainty in the FFZ and we hope to see more research and advancements in this area.	NO CHANGE
Section 2.3	This is the first time that "conservative" accounting choices have come up, but there isn't much groundwork laid for carbon accounting and conservative decisions. Since "netCDR" is used quite a bit before here as well, it could help to define netCDR earlier on and perhaps draw out what goes into calculating it (which would lead you right up to why accounting decisions need to be made and what makes them conservative or not).	Thank you for this suggestion. We've now included the term balance and a discussion of netCDR in Section 2.	CHANGE MADE
Section 4	This section raises important questions about time dynamics that perhaps should be defined earlier. For example, what does	Thank you for this suggestion. Provided some re-wording in this section for clarity, as well as an	CHANGE MADE

	it mean for a carbon loss to not be "felt by the atmosphere", and how short-term is "transient" for processes like cation sorption and carbonate precipitation? Saying that these processes "do not need to be measured, or incorporated into carbon accounting" feels like a big step without more justification around when / why they can be ignored.	expansion on key terms in Section 2.	
Section 4	Instead of "felt by the atmosphere", I would suggest rephrasing to "carbon losses do not directly influence atmospheric CO ₂ concentrations"	Thanks for the suggestion - change made.	CHANGE MADE
Section 4	I think this is the first mention of a working group. Their relationship to this document should be clarified earlier (see Section 1 comment about adding more context).	Thanks for the suggestion - change incorporated into Section 1.	CHANGE MADE
Section 4.1	Depth approach. Perhaps (just a suggestion) another approach could be added: the depth of the water front - this defines the depth of calcic horizons (with current climate or in some instances with paleoclimates). If there is a calcic horizon in a specific soil, I would suggest considering the depth down to that horizon as it increases the chances of having secondary precipitation of CaCO ₃ . As a side note, a calcic horizon is not a pan as opposed to a petrocalcic horizon. Also, it should be noted that not only Calcisols (WRB classification) have a calcic horizon.	Thank you for this suggestion. We've incorporated this into Approach 4, related to ending the NFZ at particular soil horizons.	CHANGE MADE
Section 4.1	Some general notes on Section 4.1: a) There is a lot of emphasis on how various approaches may miss important processes occurring deeper in the soil column that impact CDR. While I agree with this, it's impossible to accurately capture everything that is happening in the soil column with any sampling approach -- even in areas where it is feasible to sample to the water table, there can be both vertical and horizontal heterogeneity that isn't fully captured even with the most well-planned sampling designs. I think it's important to be up front that none of these definitions of the near-field zone will capture everything and that the emphasis should be on capturing as much as possible within the observable zone. Not sure this document is the venue for it, but some industry-wide discussions of the	Thank you for this discussion and suggestion. For (a) we added a sentence at the beginning of 4.1 to get at the fact that this is ultimately a normative decision and there are advantages and disadvantages of each approach. For (b) - thanks for the nudge here as well - we re-worked the final recommendation to hopefully better re-inforce and align with the site-specificity we advocate for in the earlier sections.	CHANGE MADE

	tradeoffs associated with how the NFZ is defined (e.g. sampling at a fixed depth but requiring more measurements vs fewer measurements of deeper samples) would be useful. b) In terms of clarity, I think this section and the following sections are a little misleading given the final recommendation. It seems to me like you are advocating for a great deal of site specificity in defining the NFZ throughout Section 4, so I was a bit surprised when I got to the actual recommendations and saw a fixed depth recommended. Not that I disagree with this approach; I would just like to understand how you landed there.		
Section 4.1	It would be very helpful to add a figure or illustration that shows each approach, making it easier to visualize.	Thank you for the suggestion and while we agree that diagrams could be helpful here and in other sections of the document, for the sake of time we chose not to create them for this iteration of the document.	NO CHANGE
Section 4.1	We think that the only approaches that would be feasible for deep soils and deep root systems (up to 6m) are 5 and 6. Approach 4 could work but would require opening and analyzing soil profiles in most deployment sites which would not be feasible operationally. Most crucially, sampling events will take place with crops still in the soil. Deep measurements require machinery that can't be used without potential damage to the crop.	Thank you for this discussion on deeply rooted perennial cropping systems; we added some clarification to Section 4.3 on operational considerations with taking deeper measurements in perennial cropping system	CHANGE MADE
Section 4.1	Sampling soil at depths of 1 meter or more presents significant operational challenges and substantially increased costs. In some places, quotes for "deep soil sampling" can be nearly ten times higher than those for standard agronomic sampling depths (typically around 20cm). This dramatic cost increase, when applied to every sample, can render operations economically unfeasible. Moreover, some soil profiles often don't extend to such depths, with underlying stones frequently encountered before reaching 1 meter.	Thanks for the additional context here. Please see the discussion in Section 4.3 of operational and economic challenges associated with deep sampling.	NO CHANGE
Section 4.1	Consider changing "losses" to "export" in this paragraph. Here, "loss" seems to refer to export of C to the FFZ resulting in "near-immediate removal of CO2 from the atmosphere". But	Thank you for pointing this out. We meant to say that "losses" above this NFZ depth would lead to reversals (not removals) - edits made in text.	CHANGE MADE

	elsewhere, losses are synonymous with the release of CO2 to the atmosphere.		
Section 4.1	<p>Definition of B horizon is not correct. (i) Not all B horizons are illuvial soil layers. There are also Bw horizons (w for in situ weathering). A typical example are Cambisols (in WRB classification system) or Inceptisols (in Soil Taxonomy), both with a cambic horizon. (ii) Not all B horizons are base-saturated. In fact, this is a key difference between Alfisols and Ultisols (in Soil Taxonomy) where the Bt (t for ton, which is clay in German). In the Alfisol, base-saturation is high whereas in the Ultisol is low. In the WRB, Luvisols are base-saturated, but Alisols, Lixisols and Acrisols probably not (please check with a pedologist). Other soils non base-saturated are Podzols and generally Ferralsols (in WRB), Spodosols and Oxisols (in Soil Taxonomy).</p>	Thanks for the clarification; we clarified that B-horizons *often* represent eluviated soil layers, but that is certainly not always the case.	CHANGE MADE
Section 4.2	<p>This section is meant to identify a short-coming of 1D modeling frameworks, but I think impermeable layers and groundwater are relatively manageable in 1D models. For example, if someone really needed to solve the impermeable layer problem, it may not be prohibitively hard to build a 1D model that follows the flow path itself in one dimension, rather than just simulating the vertical dimension. Consider motivating this section as "the conventional case of vertical infiltration through undersaturated soil is convenient for using tools like 1D models, but there are prominent examples where this case doesn't apply". This could help clarify that these special cases don't exclude 1D models, but rather would warrant extra care / considerations when applying them.</p>	Thanks for the feedback - we revised the paragraph to move away from a discussion of 1D modeling frameworks, and more clearly point out that an assumption of 1D vertical infiltration is not warranted in many systems.	CHANGE MADE
Section 4.2	<p>In these cases, it may be necessary for the NFZ to extend only down to this layer, though cation sorption and secondary mineral formation along the horizontal flow path should also be considered -----→ I don't totally agree with this statement. If there is a plow pan or other hardpan layer, there is most definitely going to be lateral flow. And once that lateral flow finds a high conductivity lens, it will flow downward into the</p>	Thank you for this comment and suggestion. We added a sentence to clarify this point, and also to more broadly suggest the need to consider the flow path of water when designing a sampling plan in these circumstances.	CHANGE MADE

	deep soil profile and GW system. Like in a rice field, research shows the flow goes laterally into the bunds and then down. If folks just stopped at the plow pan they would miss all of the actual recharge happening through the bund. The same would apply for other systems where there is variability in the conductivity.		
Section 4.2	I wondered whether rice paddy soils offer an advantageous way of measuring outgoing alkalinity and assuming a conservative X% loss through the cracks.	Agreed - we recommend tracking alkalinity export with some measurements within the bunds to characterize any loss pathways that occur in this downward flow regime.	NO CHANGE
Section 4.2	misleading statement: the bunds are where the anisotropy in permeability is favorable for vertical infiltration of the draining water by porous flow. Flowing down bund will make it sound like water is displaced further "horizontally" akin to flowing "downstream" etc.	Thanks for the comment - clarification made in text.	CHANGE MADE
Section 4.2	Statement is tough to follow. I'd agree if we were talking about making measurements in the NFZ that were only solution, or fluid based measurements. I think that implying that deeper measurements under bunds likely being a requirement for measurement and defining the NFZ is a premature statement and might be a little bit overkill	Thanks for the comment - clarification made in text.	CHANGE MADE
Section 4.3	collecting deep soil cores often requires bringing heavy machinery (e.g., power augers) to the field site ---→ I think it depends on how deep we are trying to get. Some cores can be retrieved to >150cm using a sequence of hammered in core barrels.	Thanks for the suggestion - we modified the statement to say that deep sampling "may require" the use of heavy machinery (but not definitely).	CHANGE MADE
Section 4.3	Are these parameters more variable at depth than they are in the first, say, 30 cm of the soil profile? If not, it could help to clarify that the things being measured are variable and require high sampling densities to characterize them correctly – this makes expensive, deep sampling even more impractical.	Thanks for the comment - the degree of variability will depend on the particular variable and specific soil context; given that we aren't providing detailed sampling guidance on each parameter in this section, we chose not to go into further detail here.	CHANGE MADE
Section 4.4	A hybrid approach is a good start however even incorporating a small representative area for deeper samples can present large operational costs. These deeper soil samples require machinery	Thank you for this comment and we've updated our recommendation at the end of Section 4. We agree and also want to prioritize deployment driven	CHANGE MADE

	either at a capex or through contracting out. Creating another area for this type of sampling also adds complexity to the project when paired with other areas such as control plots and treatment plots.	learning and better understanding of processes at depth.	
Section 4.4	So does this imply that at-depth baselining is required? Presumably that would need to be done across the deployment area in order to designate the subplots. Is it expected that sampling density in the deep-soil subplots will be similar to shallower sample density? And do these subplots also require controls? Feels like there should be some more detail about how this is functionally implemented.	Thank you for these questions and we've added a clarification in-text. In order for the deep measurement data to meaningfully inform quantitative CDR estimates, at-depth baselining is required, and sampling density must be sufficient to overcome baseline variability of the parameters being measured.	CHANGE MADE
Section 4.4	Is the first use of a "control plot"? May need to be defined here.	Thanks for catching this. We've added reference to section 7.2 on control plots.	CHANGE MADE
Section 4.4	Is this point the same as that in 661-663? Only needs to be made once if so.	Thanks for the flag - we decided to keep both points given their somewhat different meanings.	NO CHANGE
Section 4.4	If there is a deployment area of 100 hectares, collecting deep samples in 10 hectares with heavy machinery will not be feasible. Moreover, deployments in the global south already have the challenge of finding implements to spread the rock dust, although not commonplace, these implements are used to spread other ag inputs. Mechanical drills however are not used and practitioners would have to buy, rent or help finance.	Thank you for this comment. We've updated our recommendation at the end of Section 4 to be a bit broader and more site-specific while still advocating for the collection of deeper measurements to quantify cation loss and lag pathways in a smaller subset of the deployment area when feasible and contextually relevant.	CHANGE MADE
Section 4.4	This number seems a little arbitrary to me. Also, I suspect that this number will be pretty burdensome to implement. Moreover, due to the high level of heterogeneity in these fields, I don't really know what this buys us. I know large data sets are better, but I think carefully targeted deep measurements and demonstration sites will work better in the short term. They would be easier to monitor and not disrupt farm operations as much. One of my biggest worries with these statements about needing lots of "deeper" measurements is that we end up wrecking corners of farmers' fields in service of this goal.	Thanks for the comment - we evolved our recommendation at the end of Section 4 to remove arbitrary numbers, while advocating for deeper measurements (at least on a small subset of deployment area) to constrain cation loss pathways occurring in deep soils when feasible and contextually relevant.	CHANGE MADE
Section 5.1	Might the term counterfactual be changed to baseline? Or natural? Or unperturbed? I don't feel it is as intuitive as some of	Thank you for this suggestion to clarify the terminology used. We have maintained the use of	CHANGE MADE

	the others.	'counterfactual' throughout the document, but have defined it in Section 2 as part of a new sub-section on 'carbon removal terminology'.	
Section 5.1	It is probably OK to consider all field area within the same geographic area as long as the sites are then stratified based on pedoclimatic and hydraulic conditions.	Thank you for this feedback. We agree and have left the text unchanged.	NO CHANGE
Section 5.1	Why does the feedstock have to be the same for a deployment to be considered a "single" deployment? If the project manager knows the locations/spreading/distribution of multiple feedstocks and can account for this, I don't know why this has to be the case.	Thank you for this question. The 1 feedstock per 'deployment' convention is used to simplify considerations of feedstock characterization and monitoring requirements. If multiple feedstocks are deployed at the same time at sites within the same geographic area, the sites could be considered part of the same deployment if the deployment is stratified based on feedstock composition and application rate. We have elected to maintain the current definition in this v1.0 document.	NO CHANGE
Section 5.2	This is a nitpick, but I think the way the CDR equation (5.2.1) is presented is a bit confusing. I know the counterfactual is included in the terms in Eqns. 5.2.2-5.2.4, but I think it is more clear to incorporate this as a single counterfactual term in Eqn. 5.2.1 and then break the term down into the individual components; that way a reader can immediately see that the counterfactual is accounted for in the main equation.	Thanks for the suggestion. We played around with ways of reframing the high level term balance, but ended up sticking with having the highest level equation only include NFZ, FFZ, and LCA terms, where each of those terms is further decomposed into a treatment and counterfactual term.	NO CHANGE
Section 5.2	Some simplified version of this equation should happen much earlier in the document.	Thank you for this suggestion. We agree and have now introduced this high-level term balance in Section 2.3.	CHANGE MADE
Section 5.2	In the equation, it is confusing that the CO2 removal in the near field zone and CO2 loss are both positive values, when one is removal and the other is loss. One might expect removal and loss to have different signs and be added together (or give them different subscripts, like gain and loss)	Thank you for this suggestion. We agree that using a positive sign convention for both carbon removal and carbon loss terms without clear delineation in the subscript was confusing. We have now amended the term balance equation to reflect FFZ CDR as a positive term (such that FFZ losses are negative).	CHANGE MADE
Section 5.2	I think that the early portions of this doc would be more	Thanks for this suggestion. We've revamped	CHANGE MADE

	understandable if counterfactual were defined when you explain why this document focuses on managed ag land	Section 2 of the document and included a 'CDR terminology' subsection. Counterfactual (including the context of counterfactual agronomic practice) is now defined within that section.	
Section 5.2	It may be worth explicitly adding "MAX(0, CO2e_CF*)" to the rhs of equations 5.2.2-5.2.4 to enforce non-negative values. It's worth noting that it's conceivable that avoided emissions can be wrapped into netCDR even when the CF terms are strictly positive (particularly if there is counterfactual lime use to account for).	Thanks for the suggestion. We considered this change, but realized it is slightly different from the accounting approach we recommend in the document. We recommend first carrying out the netCDR calculation with whatever counterfactual value is calculated, and then afterwards to identify and quantify the avoided emissions that may have gotten wrapped up in the netCDR calculation, so they can be tracked separately from removals and credited with fit-for-purpose accounting schemes. See a longer discussion of this in Section 5.5.4.1 and 5.5.4.2.	NO CHANGE
Section 5.3	I somewhat appreciate why 'geophysically' is used but I cannot help but suggest that 'geochemically' is more appropriate - or even biogeochemically (as certainly microbes are involved). All of the CDR processes - solution or mineral based are geochemical.	We agree that 'geophysically' is an awkward fit and have updated it to biogeochemically as suggested. Thank you!	CHANGE MADE
Section 5.3	Is there a particular reason carbon removal is only considered when in water in exchange with the atmosphere? What about groundwater reservoirs? Or is that considered the next step - carbon storage - perhaps? If so, perhaps shortly mention this?	Thank you for this comment that has helped us sharpen the language in this section. We have sharpened the language to clarify that CDR doesn't stop once DIC is exported from the NFZ or the water mass is outside of diffusive exchange with the atmosphere.	CHANGE MADE
Section 5.3	It may help to point out that, although C removal by this definition *can* occur "in downstream surface water systems", it wouldn't count by the constraints of the Foundations document since the FFZ (which includes downstream surface waters by definition) strictly counts against netCDR.	We appreciate the suggestion, and have added the clarification that net carbon removal generated in the FFZ is not considered in the current accounting framework.	CHANGE MADE
Section 5.3	Consider changing to: "Carbon credits that are used as offsets should represent ex-post outcomes, i.e. atmospheric CDR that has already occurred. This is most clearly guaranteed by	Thank you for this suggestion. We have sharpened the discussion of ex-post outcomes in the text.	CHANGE MADE

	issuing credits as CDR is achieved through time." Although it's true operational decisions and precedent will play a role in crediting norms, we think it's worth holding a clear line about what "ex-post" means in the context of a CDR pathway that happens over time.		
Section 5.3.1	The option of considering NFZ temporal dynamics, with some crediting at dissolution, can help resolve the cash-flow problem. Given that rock dissolution rates may be well understood under generalizable conditions, EW practitioners can have a relatively good way of estimating when a significant fraction of the revenue will be materialized (some suggest at least 50%). Although option 2 is clearly more desirable from a pure carbon accounting perspective, it will likely make several conditions (e.g., deep NFZ, perennial crops) unfeasible. Most startups will find it difficult to stay afloat with the cash flow profile that option 2 creates.	We appreciate this perspective. Given the existing discussion of potential 'hybrid' NFZ time accounting frameworks, we haven't made any revisions to the text.	NO CHANGE
Section 5.3.1	Discussion of near-field zone time accounting (Section 5.3.1) from a theoretical framing specifically was well-received. However, how these theories are stress-tested ought to be considered carefully as there are direct implications on accrediting that may create inconsistencies in delivery definitions across the field.	We couldn't agree more, and look forward to working with practitioners across the space to help develop case studies that consider the implications of these different time accounting frameworks in detail for example real-world deployment contexts. Unfortunately this is out of scope for the current document.	NO CHANGE
Section 5.3.1	Adding a brief statement of why net FFZ loss occurs could be useful to the uninitiated reader rather than send them to Section 5.3.2	Thank you for this recommendation. For brevity's sake, we have maintained the reference to the subsequent section for further consideration of amortizing net carbon loss in the FFZ.	NO CHANGE
Section 5.3.1	Is this conservative? Maybe tentative?	We use 'conservative' here in the sense that assuming future losses occur at the time of crediting is more conservative than assigning those losses to future reporting periods (or attempting a form of time discounting). We have kept that term but have sharpened that the FFZ losses are estimated at the time of NFZ CDR generation, to	CHANGE MADE

		communicate that tentative nature. Thank you for the suggestion!	
Section 5.3.1.1	To clarify, in this situation "alkalinity released from feedstock dissolution" would equate to detecting a signal at a shallower depth? May be worth defining this in terms of where a signal is detected rather than the more subjective "dissolution has occurred".	Thank you for this suggestion. We have sharpened this paragraph to tie credit generation to the measurement of base cation release and/or associated DIC generation due to feedstock dissolution within the NFZ.	CHANGE MADE
Section 5.3.1.1	This section could benefit with clear differentiation about what constitutes a "lag" vs a "loss" (especially with respect to "secondary carbonates that later dissolve").	Thank you for this recommendation; we have now explicitly defined what is considered to be a 'lag' versus a 'loss' in Section 2.3.	CHANGE MADE
Section 5.3.1.1	Consider changing the final sentence to: "Crediting at feedstock dissolution will thus result in credits being issued before carbon removal has actually occurred".	Thank you for this recommendation; we have incorporated a version of the suggested phrasing into the text.	CHANGE MADE
Section 5.3.1.1	When I first read this paragraph I thought "loss processes" referred to losses in a reporting period (which could include cation exchange and transient secondary carbonate). I'm not sure how, but it could help to clarify the language about what constitutes a loss in this context.	Thank you for this push for clarification - we have sharpened the text to reference 'permanent losses and reductions in CDR efficiency within the NFZ'.	CHANGE MADE
Section 5.3.1.2	This section is pretty citation light....this means we haven't figured this out yet as a scientific community. Please make sure to highlight that this is a work in progress for EVERYONE so that folks doing due diligence with this document understand that.	We very much concur and have added an additional emphasis that developing cohesive frameworks for time accounting within and across CDR pathways is very much a work in progress to Section 5.3. Thank you!	CHANGE MADE
Section 5.3.1.2	Should be clear that CO2 stored as carbonates in the NFZ is 50 cents on the dollar compared to CDR from silicate feedstock	That's been clarified in the text - thank you!	CHANGE MADE
Section 5.3.1.2	Maybe it's just me but the wording here is a little unclear. What you are advocating for is multi-phase redundancy (right?) and I think you should say that explicitly.	Thanks for the feedback! We have sharpened the call to action in this section.	CHANGE MADE
Section 5.3.1.3	Another option not considered here would be issuing credit upfront and assigning a vintage to them based on predicted (upfront) or observed removal time dynamics.	Thank you for this suggestion, and we agree that that is another option to consider. We've opted not to introduce that into the discussion here, as it's similar to some of the concepts introduced in Section 5.3.1.3 but required more detailed	NO CHANGE

		discussion of VCM crediting systems. However, we hope the discussion introduced here inspires further thought and work along those lines.	
Section 5.3.1.3	See the potential with this approach and how it will help with cash flow problems. Holding back 50% may still be quite a high proportion and companies may not be able to make this work. Operations often have very tight profit margins and hence the business will always be in a loss even with this approach.	Thank you for sharing this perspective. We've kept the example of 25-50% for discussion's sake here. We don't mean to advocate for a specific value; the range is presented solely as an arguably reasonable example.	NO CHANGE
Section 5.3.1.3	This paragraph calls out the operational challenges of delayed CDR in option 2 but, transient lags aside, the same challenge occurs from option 1 (i.e., there is a time-delay associated with feedstock dissolution).	Thank you for emphasizing this; we've now added a short discussion that speaks to this in the introduction to Section 5.3.1.3.	CHANGE MADE
Section 5.3.1.3	Any form of economic discounting breaks a claim of physical equivalence that is assumed to underpin offset claims. In the absence of careful thinking, consensus-building, and clear communication around how to translate projected CDR timing → reduction in climate damages → dollars, we do not think this is an appropriate option. We would recommend excluding this option from the foundations document.	Thank you for this constructive feedback. We have decided to keep a discussion of economic discounting in this section, but have further emphasized the need for careful discussion and consensus building before such a framework is considered within a crediting system.	CHANGE MADE
Section 5.3.1.3	The concern about our inability to model CDR from source to sink applies equally to Option 1.	That is true! In some ways. But we would argue that the ability to model the temporal dynamics of CDR in the FFZ becomes more important in the 'temporal discounting' option and have thus left the point of emphasis in that discussion.	NO CHANGE
Section 5.3.1.3	While these analogous frameworks for time accounting exist for BECCS and biochar, the most standard approach for accounting for waste biomass is to have a list of fast decomposing/decaying feedstocks or a combustion counterfactual. So while the statement itself is true, it is misleading to someone unfamiliar with those other frameworks. We would suggest to add a statement at the end of the paragraph caveating this type of accounting (e.g. "The standard in biomass accounting is however a clear counterfactual of fast decay or combustion.")	Thank you for recommending this clarification. We have added a clarifying sentence along the lines you suggest.	CHANGE MADE

Section 5.3.1.3	nice inclusion of comparison to other CDR technologies and how they handle the same problems. Would love this point to be highlights in any summary or abstract for the document and even draw on examples more	Thank you for this suggestion. While we agree that this topic could benefit from a much more extended discussion, more detailed discussion is unfortunately outside the scope of this v1.0 document. We hope the discussion started here spurs substantial further work on the temporal dynamics that impact many CDR pathways and options for time accounting frameworks that can be consistently applied across approaches.	NO CHANGE
Section 5.3.1.3	One could argue that it's actually not clear that the time dynamics around biomass decay are more knowable than ERW processes as they are based in a counterfactual, as you note, and highly contingent specific choices around biomass management. Consider dropping the first sentence and focusing solely on the challenges of applying this approach to ERW.	Thank you for this recommendation. We have sharpened the discussion of the biomass counterfactual analog in response to your comments.	CHANGE MADE
Section 5.3.1.3	Is the "some crediting at dissolution" method much more favorable to suppliers than option 2? Field studies seem to find at least a similar fraction of released alkalinity (if not more) is acting toward CDR (rather than getting caught up on exchange sites, for example) in the first year. If that's the case, then a lower percentage of crediting upon dissolution could be harmful to project proponents (antithetical to the motivation laid out at the top of 5.3.1.3).	In the 'some crediting at dissolution' option, if CDR has been demonstrated in or exported from the NFZ, it should be credited. This is primarily setting up an option for crediting in deployment environments where lag times may be substantial; in environments with limited transient cation sinks and fast export from the NFZ partial crediting at dissolution could indeed generate fewer credits than simply measuring/demonstrating the full term balance for early reporting periods. Thank you for pressure testing this example option; no change has been made to the document.	NO CHANGE
Section 5.3.1.3	a good hybrid option if Option 3 is a default option given the financial and time restrictions of operators.	Thank you for sharing this perspective; no change has been made in the document.	NO CHANGE
Section 5.3.1.3	Super interesting concept. Would love to have some conversations about practical implications of this, especially how buffer pools would functionally be designated -- I imagine it would be highly reliant on CDR potential, though would	Thank you for sharing this perspective and we'd be delighted to continue the discussion; no change has been made in the document.	NO CHANGE

	inefficiencies such as passivation also need to be accounted for?		
Section 5.3.1.4	I think predeployment site characterization is missing from the discussion above. If you've got environmental and pre-existing soil texture factors that are less conducive to laggies this should be taken into account. For example, highly weathered soils with low CEC where water fluxes are high. The way this section reads it sounds like all soils and agro-ecological zones are equal (they aren't).	Great point; we've revised this paragraph to emphasize how these time dynamics (and associated implications for financing challenges) will vary substantially across deployment contexts. Thank you!	CHANGE MADE
Section 5.3.2	Not sure I fully understand the distinction between deducting losses equally across a project lifetime or proportionally at the end of a reporting period. Does this mean that more losses are deducted early in the project when alkalinity release is higher?	Yes, proportionally would mean accounting for more downstream losses when a reporting period is associated with higher NFZ CDR. We have added clarifying language to the discussion here. Thank you!	CHANGE MADE
Section 5.3.2	What if a project is terminated early? If deductions are spread equally across a project lifetime, this would mean that credits generated would not adequately account for losses. Would it make sense to add in a time threshold, similar to LCA emissions (e.g. downstream losses must be accounted for by the time 50% of the rock has weathered or something like that)?	This is a good point; we have explicitly called out the need to have provisions in place to deal with early project termination. Thank you!	CHANGE MADE
Section 5.3.3	It would be very helpful to add an example of a table calculation, such as a 10-year projection, showing the amortization of upstream life cycle emissions.	Thank you for this recommendation. Given the scope of this document, we have decided to leave such detailed guidance for future work and/or individual protocols.	NO CHANGE
Section 5.4.1	Does this suggest having a separate methodology that can possibly credit organic carbon alongside inorganic carbon? - or just report it for transparency. Reporting changes in organic carbon will likely be crucial, and this situation presents an opportunity for developers to combine various carbon credits. However, this approach could lead to complications regarding the ownership of organic carbon credits, a concern frequently voiced by farmers. Many farmers wish to retain ownership of the organic carbon credits generated on their land. This scenario might incentivize Enhanced Rock Weathering (ERW) developers to stake claims on these credits, potentially giving	This is a critical point and much-needed discussion for the ERW community. We have now explicitly addressed this point here but leave a more extensive discussion for future work. Defining process and guardrails used to navigate between crediting schemes for (inorganic) CDR driven by ERW deployments and SOC as well as other terrestrial carbon stores (e.g. increases in standing biomass in perennial or forested systems) will be a key need in the coming years. Thank you for emphasizing this in your feedback!	CHANGE MADE

	rise to issues such as double accounting and disputes over credit ownership. It's important to address these concerns to ensure a fair and transparent system for all parties involved in carbon credit initiatives.		
Section 5.4.1	more of a comment than applicable feedback - that OC gains or losses are tracked separately its likely not an ideal path forward - this has been the approach (at best) to date but initial data is suggesting that it is the mechanistic interaction of processes governing both OC and DIC that govern these gains or losses (see recent papers by Sokol et al. 2024 (also there is one in review by Soong et al. -Science o the Total Environment)	Thank you for this recommendation; it's certainly something we grappled with extensively throughout this process. The details of considering SOC dynamics alongside inorganic carbon quantification is discussed much more extensively in Section 8.8. We've left the general conclusion that at this stage of the field organic and inorganic carbon components of the net carbon balance should be considered separately (certainly for crediting net accrual of SOC associated with ERW deployments) but have re-emphasized in this section that better understanding the potential for meaningful net loss of organic carbon - either transiently or over longer timescales - as a result of ERW deployments is a very important research priority in the coming years.	CHANGE MADE
Section 5.4.1.1	General thought/concern for any measurements done within the NFZ (as opposed to export flux at the bottom of the NFZ): depending on the depth of the NFZ and local soil characteristics, it is likely that soil parameters such as SIC, CEC, even DIC in pore water vary with different soil horizons. So it can be tricky to measure these at 1 depth and interpolate/assume to be the same over the entire NFZ soil column depth? One might take the secondary carbonate amount from a particularly DIC enriched horizon, or a horizon with little secondary silicates,... Not sure how to solve this without requiring a screening of these parameters across the soil depth which would incur additional sampling and analysis efforts and costs, but wanted to point it out.	<p>This is very true; any measurements would need to be representative of the entirety of the NFZ. This could include looking at different depth intervals (homogenizing the sample over that depth interval) and producing a weighted average, considering the entirety of the NFZ in a single sample (likely only feasible for sites where the NFZ is shallow).</p> <p>In the discussion we emphasize that any solid-phase mass balance components or aqueous phase measurements would need to be calculated for the integrated soil profile in the NFZ, and have thus not made any specific revisions to the text. But we very much appreciate your emphasis on this point.</p>	NO CHANGE

Section 5.4.1.1	<p>There seems to be an inconsistency between Section 4 and Equation 5.4.1.1.1. Section 4 highlights the particular value of DIC exported out of the NFZ as a potentially durable form of CDR. However, Equation 5.4.1.1.1 treats DIC within the NFZ as equally valuable for long-term carbon removal. I think this is problematic because NFZ carbon is (by definition) more vulnerable to reversal risks.</p>	<p>Thank you for highlighting this point and that practitioners should be cognizant of this higher vulnerability. We have presented term balance equations that allow for consideration of DIC in the NFZ as part of a holistic accounting. Importantly, in the solid phase mass balance approach, DIC in the NFZ is counted as CDR that has been generated. We have now sharpened text throughout the document to distinguish between these concepts of potential CDR, CDR that has been generated, and durable CDR and also added a paragraph in 5.4.1 to specifically address the consideration of DIC in the NFZ as CDR that has been generated.</p>	CHANGE MADE
Section 5.4.1.1	<p>If you (rightfully) require the DIC in the soil pore water within the NFZ of RP2 to be subtracted with this value from RP1 to only take into account the additional DIC from RP2, I believe the same should be done for the secondary calcite measurement in the soil? It is not so easy to understand whether carbonates initially present in a soil are primary (from carbonate bedrock) or secondary (from precipitation from soil pore water) let alone to know what carbonates are precipitated only in the latest period. Perhaps measuring the SIC content at the end of an RP and subtracting from this the SIC measured at the end of the previous RP (or, for the first RP this would be the initial SIC content) is a good strategy?</p>	<p>Absolutely. We've added clarifying language that emphasizes that this would be measured from the difference in SIC stocks between the current and previous reporting periods in Section 5.4.1.1. Thank you for recommending this clarification.</p>	CHANGE MADE
Section 5.4.1.1	<p>This sentence is a little misleading as it implies that measurements from a single treatment plot can be compared against those from a single control plot. As set out in Section 5.6.6, multiple replicate plots are required to account for spatial heterogeneity and demonstrate causality.</p>	<p>Thank you for this clarifying comment! We have elected to maintain the current phrasing, as in many instances, measurements from a (single) representative control plot will be compared against a treatment plot. But multiple control-treatment pairs are required to robustly demonstrate netCDR in aggregate.</p>	NO CHANGE
Section 5.4.1.2	<p>Due to the explicit subtraction of the previous RP measurement from the current RP for DIC inside the NFZ in Endmember 1 (which I believe is excellent to put like this to avoid confusion) I</p>	<p>Thank you for this note. We've emphasized that this is the difference over the course of the reporting period.</p>	CHANGE MADE

	feel that it might be helpful to also express this more for other terms of these equations, besides SIC in Endmember 1. I see that it is written clearly in the text that for Endmember 2 one needs to take the changes with the previous RP into account. But maybe it is good to add explicitly that this means that these terms are thus the difference between these values now and in the last RP (Feedstock dissolution, secondary carbonates/silicates,...)		
Section 5.4.1.2	I think it should read 'is the decrease in CDR over the reporting period due to alkalinity...'	Indeed - thank you for catching that typo!	CHANGE MADE
Section 5.4.2	Re: accounting for losses in the lower vadose zone and along groundwater transport paths, I may be missing something here but what losses would you expect outside of tapping for irrigation? Or is that the primary loss you are considering here? Some specificity of what losses need to be accounted for would be helpful.	We very much appreciate this suggestion. Given that the fluxes and processes of interest are discussed in great detail in the FFZ section, we have elected to simply call out the spatial/temporal regimes here and link to the relevant detailed suggestions.	NO CHANGE
Section 5.4.2	I might have missed something, but I do not understand why the surface water and ocean CDR losses are subtracted from the groundwater loss? I imagine each of these 3 terms to be a value in tonnes that is positive if it is a loss, so if we lose 1 tonne in the groundwater, 2 tonnes in surface water and another 3 tonnes in ocean (totally arbitrary numbers) we lost a total of 6 tonnes. Which can then be used in equation 5.2.1 (line 709) as $-CO_2e(FFZ,RP)$ which will then subtract 6 tonnes from the tonnes CDR in the NFZ? As it is written now, it would be $1 - 2 - 3 = -5$ resulting in $-(-5)$ adding 5 tonnes to the overall CDR term balance?	Thank you for catching this error; Eq. 5.4.2.1 has been updated to correctly reflect that the net losses should be added together, not subtracted.	CHANGE MADE
Section 5.4.2	Here the net losses should be added together, not subtracted	Thank you for catching this error; Eq. 5.4.2.1 has been updated to correctly reflect that the net losses should be added together, not subtracted.	CHANGE MADE
Section 5.5	Throughout section 5.5, we are reminded to use the counterfactual (e.g., sections 5.5.2.2, 5.5.2.3, etc). But there's a lot hiding in that reminder. It might help to state upfront (in these lines) something like "Defining the counterfactual scenario is tricky – someone must decide what a farmer would	We added a sentence about the difficulty of identifying a counterfactual, but opted not to introduce the complexity of a farmer's potential financial conflicts. Thank you for this suggestion.	CHANGE MADE

	have done without being able to actually observe it. For this, the farmer is the most informed decision-maker but also among the most financially conflicted. For now, we identify the counterfactual cases that should be considered, recognizing there lies a broader challenge in drafting the rules for defining it in the first place". The actual method for defining the counterfactual comes up in the pH control section, 5.5.2.4 (line 1293), but it feels a tad out of place since it hasn't been pointed out that the method is tricky basically everywhere (in fertilizer, irrigation, etc).		
Section 5.5.2.4	The practice cannot be described as common despite the legal framework in Brazil, please delete This statement will have implications for the perception of additionality and we suggest to remove. Less than 1% of Brazil's agronomic area uses rock powders, and even then many of them are not CDR effective. There are significant barriers that prevent the use of silicate rock powders and there are also identified use cases globally in Africa, Korea, China, Australia, Canada, US and Europe. it is just that Brazil has led the academic and political frameworks around them. The legal framework is highly specific and creates a certified feedstock, but the use of rock powders in agriculture is covered by broader legal tools in europe and elsewhere	Thanks for the clarification - changed the language to no longer suggest that these practices are common, and emphasized their (limited) use in other geographies as well.	CHANGE MADE
Section 5.5.3.1	Does this suggest that any other practices should not be carried out within the fields that have had feedstock spread? That is, farmers will still continue to use fertiliser and possibly lime.	Thank you for this question. Liming or fertilizer use can continue on the treatment plot; here we suggest that BAU plots themselves should represent the full counterfactual scenario of agronomic practice.	NO CHANGE
Section 5.5.3.1	Control plots are difficult to implement in practice and can make some operations financially unviable. We come across this particular problem when farms are typically very small. We have implemented controls within deployments however we do often get resistance surrounding this. This is particularly the case when control plots are an area within the same field as treatment plots. Farmers do not like to see variable treatments within fields.	Thank you for this insight. We appreciate the feedback and have augmented the discussion of potential operational challenges - see Section 7.2 for additional discussion on the operational challenges associated with control plots.	CHANGE MADE

Section 5.5.3.1	This is highly unlikely in most cases and incredibly conservative. From a financial perspective, this could be a driver to make ERW operations unviable. I think at a minimum uncertainties should be included in the netCDR of liming.	Thank you for this discussion. While this is a conservative estimate, without sufficiently validated models it is hard to account for these losses in the counterfactual if not directly measuring them in a BAU plot. The exception, however, is any corrections for non-carbonic acid weathering, which we agree could occur in both the treatment and counterfactual cases. Edits made in text to clarify.	CHANGE MADE
Section 5.5.4	Would be good to have some discussion here of defining the counterfactual scenario in areas without historic liming practices	Thank you for this suggestion. We discuss this in Section 5.5.2.	NO CHANGE
Section 5.5.4.1	How would a practitioner convince anyone that their BAU test plot was capturing natural baseline fluctuations and counterfactual weathering products? Nice thing to think about, but how would any of this actually be reported? Seems like this might involve ever-more control plots which could be cumbersome for suppliers	Thanks for bringing this up. This seems to point to the challenge of ensuring that control plots generally are sufficiently representative of their corresponding treatment plots - added a sentence to clarify this in-text.	CHANGE MADE
Section 5.5.4.1	This is an important point and feels like it could get buried a bit in the counterfactual section. Appreciate the detail here, but I think it would be useful to have some discussion of baselines earlier in the quantitative framework. In general, the calculations seem rather spaced out throughout the document and keeping the equations grouped to the extent that it is possible and doesn't ruin the flow may be beneficial. Grouping the equations may prevent important info from being missed!	Thank you for this suggestion. We added both 1) an expansion of Section 2 which includes some initial discussion of baselines, and 2) an appendix of equations at the end of the document.	CHANGE MADE
Section 5.5.4.2	Curious as to why avoided anthropogenic emissions must be accounted for separately rather than considered as part of the LCA?	Sometimes in the process of subtracting counterfactual NFZ, FFZ, and LCA emissions from the corresponding carbon fluxes in the treatment scenario, avoided emissions make their way into the netCDR quantification in addition to removals. The process described here intends to help distinguish between the two. Thanks for this question.	NO CHANGE
Section	I don't agree that ERW could be considered an avoidance by	Thanks for the comment - we ended up removing	CHANGE MADE

5.5.4.2	most standard definitions, as a decrease in biogenic emissions is typically considered CDR. Not sure if it was your intention to cast doubt on that but would encourage you to rephrase this section.	this paragraph from the final text.	
Section 5.5.4.2	Does this mean deducted from the total CDR or just not accounted for as removals and separated to be accounted for as avoided? It is not too clear.	Thank you for catching this. We mean the latter - they should be deducted from netCDR and can be separately accounted for as avoided emissions.	CHANGE MADE
Section 5.5.4.2	I do agree that intuitively avoided emissions are not equal to carbon removals so it makes sense to account for them and put them separately if the CF LCA emissions are more than the treatment LCA emissions, as well as for more FFZ losses (emissions) in FZ than in treatment. If not, these 'negative' differences between treatment and FC for LCA emissions and FFZ losses would in equation 5.2.1 result in 'carbon removals' which did not take place. However, somehow it feels that in case of the netCDR in the NFZ, subtracting any CO2 emissions from the CF in the NFZ feels not correct - real carbon removals will then be 'downgraded' to emissions which might not have the same value on the carbon market? Just a thought...	Thanks for this discussion and we made a clarification in the text. An illustrative example where avoided emissions might be counted in the NFZ is if liming in the counterfactual scenario would lead to emissions, which are avoided given the use of a silicate feedstock. In this case, the positive emissions in the counterfactual that were avoided should be deducted from netCDR and can be accounted for as avoided emissions.	CHANGE MADE
Section 5.6	I think it's very important that t tests or confidence intervals (more simplistic statistical tests for determining significance) are used as a starting point in any MRV practice. Any more complex statistical technique must be demonstrated to be reliable in showing a "real" signal - and the details, derivations and worked examples of these statistical methods should be published openly (peer-review, or on a platform like GitHub) so that non-commercial entities can assess the validity of the technique. I'm not sure that just sharing them with protocol reviewers is adequate.	Thank you for this discussion. We broadly agree with this sentiment, but also there's complexity here given that simpler methods may be inappropriate / not applicable given the high dimensionality of the data (e.g. not just comparing the same variable across two groups). Transparency for validity assessment will be important here. No change made in text, but we kept a strong call for transparency later in the section.	NO CHANGE
Section 5.6.1	I am not a statistician and might be missing something, but just want to note that using standard error will invariably cause smaller uncertainties than using standard deviations which most academic papers have at this point. Not sure if this is accounted for in the $z_{\alpha/2}$ - but might be worth defining what the required n of samples is to make using SE vs. SD acceptable.	Using the standard error is appropriate in this context of creating a confidence interval.	NO CHANGE

Section 5.6.3	<p>This sounds like a real problem to me. How do you prevent that this approach is gamed and the most favorable assumptions are being used rather than most realistic? I see that the assumptions and parameters need to be reported, but might be necessary to define acceptable ranges at some point? Else the people doing it the most thoroughly/honestly will end up being punished.</p>	<p>Thank you for pointing this out and we agree. At this current stage of the field, we think the main solution for this is transparent documentation of the statistical approach used, while building towards consistency over time and building robust processes for validation of statistical methods (e.g. section 5.6.5)</p>	NO CHANGE
Section 5.6.4	<p>This is a fair point, but this document is designed to guide best practice for rigour among private suppliers of carbon credits by EW. Yes, sharing of data underpinning uncertainty estimates is important, and in fact, should be required. I would argue that this document should go a bit further than what it does already in lines 1690-1706 and specify what components should be included in uncertainty estimates, as well as outlining a set of statistical methods that could be used to generate these uncertainty estimates. These should include uncertainty in: weathering rate and any measurements that go into calculating it (where analytical and sampling errors are fully described and reported, and propagated), feedstock composition (likewise), downstream leakage (this will likely be modeled, and in this case models should be published openly (or at least to independent protocol reviewers) so that the error can be independently assessed). Different methodologies may mean that in some cases, uncertainty envelopes include within them errors from multiple measurements or calculations - if this is the case it is incumbent on suppliers to fully explain why this is the case, and demonstrate this with data and worked examples. Regarding setting prices at lower bounds of uncertainty estimates: different methodologies will mean that in some cases, reported envelopes may not be directly comparable. It should be an incentive for companies to reduce uncertainty in their accounting by using more resources to make measurements and/or developing more rigorous techniques for analysis, and so if a company succeeds in a 80% confidence interval being narrower and yielding more CDR, that is something that should be rewarded - however, it must be</p>	<p>Thanks for sharing this discussion and feedback! We updated the beginning of Section 5.6 to include a longer (but still non-comprehensive) list of the potential sources of uncertainty, including sub sampling uncertainty. More detailed discussion of specific statistical methods is outside the scope of the current document but look forward to advancements here.</p>	CHANGE MADE

	demonstrated that this is rigorous, which is why transparency is the key thing that is emphasized here, rightly. I think a distinction needs to be made here between two different types of carbon crediting: one as is described in this paragraph currently, which is where suppliers are carrying out "exploratory" EW; and one which does price at the bottom end of an uncertainty envelope which has to account for the aspects I mentioned above, which is where suppliers are carrying out "commercial" EW, selling to buyers who are offsetting their emissions, for example. I agree with the proposal as it is outlined in 1690-1706.		
Section 5.6.5	I think this is good enough for now, but in the future what should replace this is example data sets from well-characterized pilot trials.	Thank you for this note and we agree; synthetic datasets used for validation should be informed by real datasets (e.g. via bootstrapping).	NO CHANGE
Section 5.6.6	Sounds good when you write it, but might not always work in practice. We try not to place control plots downslope or down hill from places where we deploy because we know we will undoubtedly contaminate downstream land with our weathering signal. This one should be thought through a little more carefully accounting for topography and other surface-geographical features...	Thanks for the context and feedback, updated this Section 5.6.6 and Section 7.2.3 to take this into account.	CHANGE MADE
Section 5.6.6	I wonder if this section would benefit from some further discussion of what random designation means from an operational perspective? There are a few sections later in the document that seem a bit contradictory if this is taken as truly random (e.g. no consideration is given to soil characteristics).	Thank you for the call for clarification. Revisions and distinction made here and in Section 7.2.3 to clarify that this should not be a totally random designation.	CHANGE MADE
Section 5.6.6	Having multiple control areas interspersed in a deployment area would be a significant operational challenge for a few reasons: 1) the spreader would need to start and stop multiple times, each of which causes variability, edge effects, additional time and perhaps also additional energy; 2) there is often wind causing the rock to be blown, and could potentially contaminate a control area. This is more likely to happen with more and smaller control areas; 3) any instrumentation (e.g. lysimeters) in the control areas would be more difficult to access (eg for	Thank you for this context and discussion. We added these operational challenges to the discussion both here and in Section 7.2.3 on control plots.	CHANGE MADE

	sampling) when not placed in specific areas, and may need to be removed before other applications or harvests or risk being damaged. It is easier for the farmer to avoid one area with instruments than several areas.		
Section 5.6.6	Interspersed controls and treatment plots are not realistic for operations	Thank you for this nudge. We made changes to distinguish between ideal experimental design vs what may be operationally feasible. More revised discussion in Section 7.2.3 as well.	CHANGE MADE
Section 6	When assessing the risk of feedstock chemical composition of causing an environmental impact, the impact on soil biodiversity should be kept in mind. In addition to heavy metals, if large rates are applied, or a feedstock that contains high soluble alkalinity (e.g., CaO, Ca(OH) ₂), the impact on soil biodiversity should be evaluated.	Thanks for flagging this - added to the Section 6.2.3.3 on mineralogical composition effects on ecosystem risk.	CHANGE MADE
Section 6.1	Feedstock heterogeneity. How does one know the heterogeneity of the feedstock or rock formation before doing the characterization? Are you suggesting that sampling frequency could be reduced over time?	Thank you for this question. We added clarification in Section 6.1 that there should be an initial stage of upfront feedstock source characterization, which may have a different sampling frequency than ongoing characterizations.	CHANGE MADE
Section 6.1	In principle I like this approach, but without defining clear lower limits it is possible to reward people that do a bad job and punish the people that do a good job. One could for example say that for the initial scouting of a new quarry at least 10 samples have to be taken from different areas that are all going to be mined, and a similar rule to sample from stockpiles. Not sure if there are any good objective criteria on what this number could be based on. I don't think the rules are sufficiently robust against gaming the system.	Thanks for the feedback - we clarified the need for a robust initial stage to characterize feedstock source heterogeneity in Section 6.1.	CHANGE MADE
Section 6.1	I have a problem with this re-characterization for its potential economic and practical side. For academics, that might be feasible but farmers will likely find the re-characterization process a burden considering the high cost of an XRF analysis. Maybe a label can be issued from the source provider.	Thank you for bringing up this point about potential burden to farmers. In our view, the responsibility for ensuring that the feedstock has been properly characterized ultimately falls on the project developer responsible for the project. Standardized characterizations and labeling of feedstock source materials could definitely be helpful as the industry	NO CHANGE

		matures.	
Section 6.2.1	Would it also be advisable for practitioners to calculate the maximum potential "measured" CDR if, for example, their MRV doesn't track all cations?	Thank you for this nudge. We added clarification in Section 6.2.2.	CHANGE MADE
Section 6.2.1	Should practitioners calculate CDR potential assuming mineral trapping if that seems to be the likely pathway?	Thank you for this question. This could be factored when calculating a "project-specific" CDR potential as described in Section 6.2.2, and can be incorporated through the value of "n" in the Steinhour equation.	CHANGE MADE
Section 6.2.1	I think specific surface area (e.g. BET SSA) should also be included in this section. This is because surface roughness is likely not taken into account when the only measure of particle geometry is particle size distribution.	Thank you for this suggestion. We've added a sentence in Section 6.2.1.3 suggesting the use of specific surface area measurements in this context.	CHANGE MADE
Section 6.2.1.1	Perhaps mentioned elsewhere, what about quantifying trace elements, including Ni and Cr?	This is described below in Section 6.2.3.2 - we separated out the analyses for the purpose of understanding CDR potential vs those for health/safety risk analysis. Thank you for this question.	NO CHANGE
Section 6.2.1.1	It would be helpful here to understand the methodologies and expected detection limits for these elements. The suggested certainty for cations seems low, given that many affordable methods such as colorimetric now he should be able to get to 0.1% by weight. In contrast, the assessment of metals for health and safety reasons would require access to less available and more expensive analytical tools such as mass spec or NAA. At first glance, this would seem to me to be excessive, as epa screening levels for these elements in soils are on the order of tens of milligrams per kilogram of soil.	Thanks for this feedback - we decided to remove references to measurement detection limits in the final draft.	CHANGE MADE
Section 6.2.1.2	I would suggest changing "toxic trace elements" to "potentially toxic trace elements", as many of the key elements of concern (Ni, Cr, Cu etc.) are also trace nutrients for some organisms.	Thank you for identifying this and we made changes across Section 6.	CHANGE MADE
Section 6.2.1.3	The importance of measuring Particle Size Distribution (PSD) down to 2.5 microns is clear, particularly from a health perspective. However, this level of precision presents a	Thank you for this discussion and suggestion. We made a change in Section 6.2.1.3, specifying that conservative assumptions should be used in	CHANGE MADE

	methodological challenge. Traditional sieve analysis, which is beneficial for its ability to process large, representative samples (typically around 20kg) from feedstock stockpiles, is limited to particles no smaller than about 63 microns. To capture the finer particles down to 2.5 microns, an additional lab-based analysis would be necessary. This supplementary testing introduces a new layer of complexity and expense to an already costly testing regimen. If only sieve analysis is carried out, possibly the fraction below 63 microns could be treated as posing a health risk (a conservative approach).	deciding what constitutes the respirable particle fraction.	
Section 6.2.1.4	What does immediately mean here?	Thank you for this question and nudge. We made a clarification in Section 6.2.1.4, specifying that samples should be taken at the deployment site (immediately prior to spreading), to account for changes in water content that may occur during feedstock processing and transportation.	CHANGE MADE
Section 6.2.2	Again, this assumes only solubility trapping, correct?	Thank you for this question. The factor n , which represents the molar ratio of CO ₂ sequestered per divalent cation in the feedstock, should be used to reflect predictions about the degree to which secondary carbonates will play a role in carbon storage. We added a sentence in Section 6.2.2 to clarify.	CHANGE MADE
Section 6.2.2	This is imo the best equation indeed. Maybe going too much into detail now, but the final CDR potential depends on which numbers are eventually used for the pH dependent coefficients - practitioners should be encouraged to document their choice based on the site specific pH - and the value chosen for η which depends on the feedstock material (about 1 for calcite, about 2 for olivine or wollastonite) as well as at what point in the ERW system we want to calculate this theoretical max CDR potential of the feedstock. In the soil right after dissolution (no losses) or when carbonates reached the ocean reservoir (around 10-20% lost)? Would it be useful for the practitioners to explicitly share this choice of numbers and how they got there,	Thank you for this discussion and question. We added a recommendation that project developers should document and justify their choice of coefficients used (Section 6.2.2).	CHANGE MADE

	to make the CDR potential values comparable between one another?		
Section 6.2.2	Consider explaining n in the steinour equation in more detail- especially how it changes from river water to ocean water. For example, this is how it was described in a recent paper "n is the molar amount of atmospheric CO2 needed to balance one mole of Ca2+ and Mg2+ or two moles of Na+ derived from weathering. The value of n depends on the molar ratio of HCO3- to CO32- in water. In pure water at pH 7, all carbonate species exist as HCO3-, so a divalent cation sourced from silicates will be balanced by two moles of atmospheric CO2 as HCO3- (n =2). A divalent cation from carbonates will be balanced by one mole of HCO3- from the atmosphere and one from the rock itself (i.e. Equation 2), making n =1. In seawater, the HCO3- to CO32- ratio decreases, influenced by temperature, salinity and pCO2 of the oceanic water column (Renforth & Henderson, 2017). Thus n decreases to 1.4-1.7 for silicates and 0.4-0.7 for carbonates. "	Thanks for the suggestion - we added a sentence in Section 6.2.2 that lists the considerations that should be used to determine a value of n.	CHANGE MADE
Section 6.2.3.1	I think these sections should also require suppliers to provide a mitigation strategy in case that for unforeseen reasons (e.g., the feedstock composition was different than anticipated) acceptable thresholds of e.g. heavy metals in soils (but also applies to all other things you defined) are surpassed. This could for example include requiring soil remediation either via biochar or phytoremediation in case too many heavy metals etc. are released. In general, what is their strategy if things go wrong?	Thank you for this suggestion and we agree that considering post-deployment monitoring and remediation is an important component of a full health and safety risk assessment. However, this section is only scoped around the intersection of health/safety with feedback characterization. We added some clarifications throughout the section to emphasize this.	CHANGE MADE
Section 6.2.3.1	When evaluating the potential impact on soils, biodiversity should be included therein.	Thank you for this nudge. Potential impacts on organisms and biodiversity are now explicitly referenced in Section 6.2.3.1.	CHANGE MADE
Section 6.2.3.1	additionally mention ecosystems and organisms here.	Thank you for this nudge. Potential impacts on organisms and biodiversity are now explicitly referenced in Section 6.2.3.1.	CHANGE MADE
Section 6.2.3.2	Should trace element analysis be included in the characterization section?	Trace element analysis is described in Section 6.2.3.2. Thank you for this question.	NO CHANGE

Section 6.2.3.3	suggest giving some guidance that there is a floor for asbestos exposure. the US OSHA 1910 regulations could provide a guide. https://www.osha.gov/laws-regs/regulations/standardnumber/1910/1910SubpartZ	Thank you for this nudge. Added a suggestion to refer to OSHA 1910 regs, as one example of jurisdictional regulations that may be applicable.	CHANGE MADE
Section 6.2.3.3	Is quantification required if PPE and handling follows standard practices to mitigate risk?	Thank you for this question. We made a change in Section 6.2.3.4 to reflect that quantification is required where needed to comply with jurisdictional regulations and ensure safe handling. Conservative assumptions should be used when deciding what risk factors may be present (e.g. if not measured, assume RCS fractions are present).	CHANGE MADE
Section 6.2.3.4	Understand the importance of analysing these however this needs to be balanced with cost. Unless a company has the capabilities of characterising the feedstock with the full suite, it becomes very costly when contracting this out.	Thank you for this discussion item. We decided to keep this section to highlight that, at minimum, it is important for project developers to meet jurisdictional regulations for radionuclides.	NO CHANGE
Section 6.2.3.4	suggest providing a floor in case there are not local regulations. OSHA could again provide a guide.	Thank you for this suggestion, however, providing thresholds for environmental risk factors is outside of the scope of this document.	NO CHANGE
Section 7.1	Though pre-registration may provide useful information from a sampling perspective (Section 7.1), the operational feasibility significantly limits these permutations of investigation. Additionally, accounting for delays in project start (post rock-application) may have negative impacts on project development financeability due to increased delivery lag.	Thank you for bringing this up. It would be helpful to hear more about how pre-registration in particular creates delays to delivery. We note that financeability is discussed more in Section 5.3.	NO CHANGE
Section 7.1	Analyses or groupings cannot be altered to produce a more favourable estimate of CDR after data is collected'' - that is a real concern indeed. In different greenhouse and field trials, quite different CDR signals are registered between days or seasons, making it possible for practitioners to 'choose' the day/data that suits them best... This should indeed somehow be minimised and monitored.	Thank you for this note and we hope that pre-registration efforts can help solve for some of this challenge.	NO CHANGE
Section 7.2	Control Plots. I recall some concerns with using the term control plot and thus the term counterfactual was agreed upon to include anything that is not amended/BAU	Thank you for this note. Control plots are used here as a general umbrella term, encompassing both BAU and negative controls as defined in Section 5.5	NO CHANGE

		of the document.	
Section 7.2.2	Example of a section that could be seen as contradictory to the idea of random designation of control plots if you're not explicit about what random designation functionally means.	Thank you for pointing out this area of inconsistency. We've added a clarification in Section 5.6 and Section 7.2.3.	CHANGE MADE
Section 7.2.2	So from a practical perspective I think what many deployers are doing is they go to a certain field site, decide where they will do control and treatment, sample accordingly, immediately deploy and then in hindsight find arguments why the control is appropriate. Many do not actually sample everything first and then make this decision based on the data. Often they do not want to wait with deployments if that means they miss a whole growing season etc. This is going to be a problem also moving forward. If we were serious about preventing this, I think one would need to prevent this through rules, e.g., that sampling must happen a certain amount before actual deployments, or that they can demonstrate that the data that fed into this decision was actually generated before the deployments started.	Thank you for this discussion. We leave these sorts of stipulations to protocols and methodologies, but maintain in the text that it is important to ensure representativeness before a control plot location is decided upon.	NO CHANGE
Section 7.2.2	I would add some soil organic matter or organic C component to this - I find it quite important to have a handle on how ERW affects SOM over time, and this needs to be accurately reflected in controls.	Thank you for this perspective. We added SOC stocks as a parameter for control plot representativeness.	CHANGE MADE
Section 7.2.2	Also SOC and SIC. Note that both of these will have pH dependence, and so these should always be reported with pH and date/amount of last liming agent and fertiliser addition.	Thank you for this nudge. We added SOC and SIC stocks as initial parameters for control plot representativeness.	CHANGE MADE
Section 7.2.2	I would add "aspect", i.e.: "...and mean elevation, slope and aspect."	Thanks for catching this - change made.	CHANGE MADE
Section 7.2.2	"initial buffer capacity of the soil" will be affected by Aglime treatments that may or may not continue after rock powder applications	Thank you for this note. We've included a nod to this in the text.	CHANGE MADE
Section 7.2.2	Buffer-pH should be defined somewhere. Not sure what this refers to, but if this does not include soil pH-buffering capacity, this might need to be added. In my opinion, the different soil pH-buffers need to be understood before adding rock dust.	Thank you for this note. We've modified the text to include buffer capacity in the list.	CHANGE MADE

Section 7.2.2	Buffer pH is not a common analysis test globally. This particular test is more commonly associated with agricultural testing in the United States. This may not reflect established practices in other countries.	Thank you for this discussion and note. We've kept buffer pH in this list for now but this is a good thing to consider for future versions.	NO CHANGE
Section 7.2.2	The requirement for these paired measurements introduces an additional layer of complexity to project designs. This complexity is further compounded by significant time constraints, as these pairings need to be determined before deployment. The challenge is particularly acute given the narrow windows of opportunity within arable farming cycles. The demand for such detailed preparation and analysis places a considerable burden on project teams, especially when they are small in size. It requires a high level of coordination, expertise, and resources to accomplish these tasks within the limited timeframe dictated by agricultural schedules.	Thank you for this discussion and context. We've re-written Section 7.2.3 to emphasize some of these operational constraints.	CHANGE MADE
Section 7.2.3	Operations will limit the designation of smaller vs larger control plots. Some have previously established 2 control plots to capture variability, but they still need to be relatively large to be viable.	Thank you for this nudge. We've added a note in Section 7.2.3 describing challenges associated with control plot size.	CHANGE MADE
Section 7.2.3	Selecting the control area randomly introduces the possibility of contamination (if located downwind or down the flow path of treated areas), and can also challenge accessibility, spreading challenges.	Thank you for pointing this out. We've added a few sentences in Section 7.2.3 describing challenges associated with selecting a control plot location.	CHANGE MADE
Section 7.2.3	Would be useful to add that operational viability can be considered for siting, such as access to the control plot. One would not choose a random location in the middle of a farm with no access roads, for example.	Thank you for pointing this out. We've added a few sentences in Section 7.2.3 describing challenges associated with selecting a control plot location.	CHANGE MADE
Section 7.3	Are "blocks" ever defined in the protocol? Are these simply fields?	Thank you for this question. We've added a clearer definition at the beginning of Section 7.3.	CHANGE MADE
Section 7.3	Implementing this approach presents significant operational challenges. Farmers are generally reluctant to have randomly distributed elements across their fields, as it can interfere with their established farming practices and routines. Their concerns stem from practical considerations about field management,	Thank you for this valuable perspective and context. We've updated section 7.2.3 to include a discussion around this.	CHANGE MADE

	potential disruptions to their workflows, and the impact on their ability to gather reliable yield data. This farmer pushback creates an additional hurdle for implementing the proposed methodology, highlighting the need for strategies that balance scientific rigor with the practical realities and preferences of the agricultural community.		
Section 7.3	Isn't this a "do whatever you want" clause for suppliers? I would recommend not including something like this.	Thank you for this note. Given that this isn't a commercial standard, we don't want to over-prescribe particular deployment designs. Thus, we tried to articulate the tensions between experimental design rigor and operational constraints, which will need to be navigated in a site-specific way.	NO CHANGE
Section 7.4.1	The requirement for four separate sampling rounds introduces a significant financial burden to the project. Moreover, this high-frequency sampling approach not only impacts the financial aspects but also demands considerable time and logistical coordination. For smaller teams or projects with limited resources, managing four distinct sampling rounds can be particularly challenging, potentially affecting the overall efficiency and feasibility of the project.	Thank you for this nudge. We've made a clarification in the text; this list of sampling rounds is just intended to give names and definitions to potential rounds that might be done for further discussion, rather than suggesting that all 4 must be performed.	CHANGE MADE
Section 7.4.1	This variability in sampling frequencies adds another layer of complexity to project management. It requires meticulous planning, precise scheduling, and robust tracking systems to ensure that each project adheres to its specific sampling requirements. I believe it is viable to have a standardised sampling frequency across all farms as long as this standard is conservative, ensuring it will capture heterogeneity.	Thank you for this comment. If the goal of taking soil measurements is to demonstrate a weathering signal above baseline soil heterogeneity, it is necessary that sampling density be determined based on site-specific understanding of the baseline variability of the parameters being measured (whether that measured on a given field or assumed based on estimates of worst-case variability). Choosing a sample density that is too low will lead to a lack of statistically significant signal.	NO CHANGE
Section 7.4.1.1	Clarity or examples for the power analysis would be helpful guidance	Thank you for this suggestion. We agree, but this is out of scope for this document.	NO CHANGE
Section 7.4.1.2	Presumably erosion and particle transport will impact immobile	Thank you for this question. The immobile tracer	NO CHANGE

	tracers. I suppose it's assumed that these processes affect all particles in the amendment the same, correct?	method assumes that chemical dissolution is the primary mechanism for mobile element removal. As far as the physical transport of particles, that is not approached in Foundations, however, Type C sampling is designed to capture any initial feedstock mobilization post spreading.	
Section 7.4.1.2	I am not convinced of the utility of Post-spreading sampling. It is not clear how information on application rates can be used. I would just remove any reference to this.	Thank you for this comment - we've heard comments both for and against suggesting a post-spreading sampling round, so we opted to include the section to present both sides, while also acknowledging the lack of consensus.	CHANGE MADE
Section 7.4.1.2	Would it make sense to discuss sampling considerations for variable rate spreaders (VRT)? My understanding is these are in common use, which would have big implications for geo-located sampling and generally integrating a field-wide signal from discrete samples.	Thank you for this suggestion. We suggest stratification based on application rate in these instances and added a change in Section 7.3 related to variable application rates.	CHANGE MADE
Section 7.4.1.2	Specify "in certain cases" - in theory there should be no need to do this if the tracer is immobile as described in the following sentences.	Thanks for the suggestion - change made.	CHANGE MADE
Section 7.4.1.2	Suggest removing reference to 'baseline' as a baseline should be pre-application	Thanks for the suggestion - change made.	CHANGE MADE
Section 7.4.1.2	I disagree with the assumption that no differences in porewater or exchangeable cation concentrations can be resolved between samples collected immediately before and after rock spreading. Feedstocks may contain appreciable quantities of readily soluble salts, which can rapidly alter solution chemistry. Indeed, Type C sampling could help to identify the presence of such phases (which do not lead to CDR) and (in subsequent, Type D, sampling rounds) facilitate differentiation between genuine primary mineral weathering and post-deployment artifacts.	Thank you for this note and change made.	CHANGE MADE
Section 7.4.3	This assumption is perhaps not true for many properties, which vary on much shorter spatial scales than a few meters.	Thank you for this note. We re-worded the text; even when properties vary on small spatial scales, paired / geo-located sampling will be no worse than	CHANGE MADE

		random sampling in terms of standard error.	
Section 7.4.4	Would be curious to see some discussion of how this applies to deep samples	Thank you for this note and we agree - this is an important area for sampling and R&D work.	NO CHANGE
Section 7.4.4	I would argue there is a difference between compositing samples and homogenizing samples. Placing multiple cores into a single bag is compositing, but then ensuring thorough mixing to homogenize the sample is a different matter. It takes effort to ensure samples are homogenized.	Thank you for this note. We added a clarification in-text that both combining and thoroughly mixing the cores is intended in this discussion of compositing.	CHANGE MADE
Section 7.4.4	Here, subsampling uncertainty could be propagated in the same way as for feedstock uncertainty.	Thank you for this recommendation; sub-sampling uncertainty is now referenced in Section 5.6.	CHANGE MADE
Section 7.4.4	6-12 cores is a lot in practice. Especially when working with a relatively large (~3+ cm) corer, this can be very large amounts of soil (3 cores of that size can be >1kg). I think getting representative cores (complete cores, equally representing all depths), may be more important than the total number of cores.	Thank you for this note. We keep the text as is in deference to the standard agronomic practice as described in soil sampling guidelines put out by ag extensions for this level of compositing.	NO CHANGE
Section 7.4.4	One could also argue that using the standard error and larger sample size will cause unrealistically small uncertainties. I do not see a practical reason why pooling samples from different parts of the fields (maybe even in different ways) would necessarily lead to lower quality estimates (if they are based on the same amount of soil samples). I would also like to highlight that I think there is an additional component to this uncertainty which is overlooked so far (by pretty much everyone) - and that is how the final large sample is sub sampled for ICP-MS analysis, and what the uncertainty is that this introduces. Most of the time just 1 0.1 mg sample is taken from a large soil sample after a smaller subsample of the large soil sample has been pulverized. The sample amount or relative fraction that the pulverized sub-sample out of the total sample, and the fraction that is digested for ICP-MS out of the pulverized sample will also have a (I guess) pretty big impact. Would be an interesting student project...	Thanks for this discussion. Spertus et al 2021 is a helpful reference demonstrating the effect of compositing on standard error / uncertainty in estimates in an SOC context. We appreciated the comment on subsampling uncertainty as well and we added this to our list of sources of uncertainty at the beginning of Section 5.6.	CHANGE MADE
Section 7.5	Another advantage of this method is the much larger mass/volume of soil that is being sampled. Suction lysimeters	Thanks for pointing this out, we've added it to the list.	CHANGE MADE

	may pull water from a ~cubic metre of soil, or 100s kg of soil, whereas many solid soil analyses are done on grams (milligrams?) of soil.		
Section 7.5	True, but if soil moisture is low there may not be a lot of weathering occurring.	Thank you for the note and look forward to results along these lines.	NO CHANGE
Section 7.5.2	I would like to stress the importance of physical measurements over models (Penman-Monteith, PM) in every step, especially at the beginning of ERW deployments. As such, there are ways to measure the evapotranspiration (ET) rate with weighing lysimeters (expensive) and with the eddy covariance method. Due to financial considerations, these methods should not be required but at least mentioned here. Also with more physical measurements in place, a comparison between the PM-equation and the measured actual ET should be undertaken, to ensure that the PM-equation is in fact estimating ET correctly in an ERW context. Lastly, other mathematical approximations of ET have been suggested in the past that fit ET better at a wider temperature range (e.g. McColl 2020, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020WR027106#:~:text=Practically%2C%20the%20new%20equation%20is,sites%20available%20for%20the%20analysis.). This is especially important in tropic climates. Final consideration is that the PM-equation generally underestimates ET flux (Fig 1e of McColl), thus leading to a consistent overestimation of water flux $q(t)$ and thus DIC _{export} . This underlines the need for physical measurements before full implementation of the PM-equation to estimate ET.	Thank you for this discussion and suggestion. We've added the note on direct measurements for evapotranspiration as well as new citations along these lines.	CHANGE MADE
Section 7.5.2	and potentially supplemented by ongoing measurements such as soil water content, soil water potential, or net infiltration (e.g., with a weighing lysimeter) ----> I think if someone is going to model the hydrology they SHOULD have measurements of soil water content. Not potentially. or at least some measurements to confirm the model is not crazy.	Thank you for this note and we've made a change to the document	CHANGE MADE
Section 7.5.3	I think there can be situations where there is a mismatch in the data frequency between discharge and DIC/cation	Thank you for this comment and suggestion. We've modified section 7.5.3 to include more of a	CHANGE MADE

	measurements, particularly if q data is generated from weather station data rather than local measurements. In this case I think it might be necessary to account for Q-C dynamics when extrapolating DIC/Cat measurements from limited measurements to the rest of the record based on assumed discharge. In rivers at least q often has an impact on c, and c neither stays constant nor varies linearly (see e.g., LOADEST). I do not know how this is necessarily for soils.	discussion of the dependence of concentration on soil water storage and flux. We've also listed it as an R&D priority.	
Section 7.5.3	FYI - Regarding novel approaches of in situ soil sensors attempting to use EC as a proxy for TA: Carbon Drawdown Initiative have recently written a working paper on this topic (https://www.carbon-drawdown.de/blog/could-ec-sensors-or-leachate-ec-data-be-used-to-monitor-cdr), looking at in situ soil EC sensor and ex situ soil water EC measurements and how they compare to the soil water TA (data from field and pot experiments, there seems to be a correlation indeed, but not always).	Thank you for pointing us to this white paper and we've added it to the citations.	CHANGE MADE
Section 7.5.4	I just want to flag that such language "Ideally,... " will not lead to any real world impacts. I think this makes it easy for deployers to argue why their setting is not ideal and then just not do it. I am aware this is a V1 and we are at an early stage, but I hope moving forward we can move to clear requirements if we truly want to safeguard quality. I think this section would be much stronger if a minimum defined amount of samples per area would still be suggested.	Thank you for this note and push. As you point out, at this time we don't have a firm recommendation for the number of samples and instead propose a flexible sampling scheme that is justified by the supplier according to their site-specific conditions. We expect the number of samples to be context dependent. We look forward to advancements in this area as the field develops.	NO CHANGE
Section 7.5.5	Are there examples of successful using this approach to quantify CDR? If not, should this still be a recommendation?	Thanks for this note. We do include experimental methods throughout the guidance to encourage R&D, innovation, and progress in the field. This is a method that is under consideration and may be important for certain deployment contexts.	NO CHANGE
Section 7.5.5	Same comment as above: I think there can be situations where there is a mismatch in the data frequency between discharge and DIC/cation measurements, particularly if q data is generated from weather station data rather than local measurements. In this case I think it might be necessary to	See comment above.	CHANGE MADE

	account for Q-C dynamics when extrapolating DIC/Cat measurements from limited measurements to the rest of the record based on assumed discharge. In rivers at least q often has an impact on c, and c neither stays constant nor varies linearly (see e.g., LOADEST). I do not know how this is necessarily for soils.		
Section 7.5.5	Project developers should be aware that in addition to hydrological flow paths, temporary storage of weathered cations on cation exchange sites will be a critical parameter that will need to be evaluated.	Thank you for this note and we've made the suggested change to the document.	CHANGE MADE
Section 7.6	The Carbon Drawdown Initiative greenhouse ERW experiment built a set of 40+ soil CO ₂ flux chambers to simultaneously measure the efflux from replicate controls and treatments with soil pCO ₂ sensors. They wanted to see if they could deduce CO ₂ soil efflux from simple in situ soil pCO ₂ sensors by applying Fick's first law but it turns out not to be so straightforward (the conclusion of any ERW related investigation..). More detailed information and preliminary conclusions on how soil pCO ₂ and soil CO ₂ eflux data correlate with soil water TA can be found in this recently published working paper: https://www.carbon-drawdown.de/blog/2024-04-11-insights-from-monitoring-400-weathering-experiments	Thank you for pointing us towards this work and we are excited to see more of it!	NO CHANGE
Section 7.6	This is something that I am not sure has actually been verified (definitely not an expert!) but there is a reason why people use flux chambers rather than CO ₂ concentration sensors. Assuming that one can calculate diffusion based on Ficks law is assuming that other than diffusion, there are no chemical reactions between the sensor depth and soil surface that consume (or produce) CO ₂ . I am not sure this assumption is very realistic.	Thank you for pointing this out. CO ₂ profile measurements are often used to determine the gradient and flux of CO ₂ in the soil. They are commonly paired with other gas measurements (such as O ₂) or a conservative tracer to determine sinks/sources, unexpected behavior, or the diffusion coefficient. We've added some additional context around the need to either make assumptions about soil diffusivity or independently constrain diffusivity, as well as options for constraining how non-diffusional processes may be impacting CO ₂ concentrations.	CHANGE MADE

Section 7.6	I think Paessler et al were using chambers, not in situ soil CO2 sensors?	Thank you for this question - both are used in the study.	NO CHANGE
Section 8	This section's readability could be improved by a graphic depiction of the fluxes. This is already shown in the inset of figure 1, but could be highlighted again.	Thank you for this suggestion. In addition to the figure in Section 2 we've added a table of NFZ fluxes to Section 2.	NO CHANGE
Section 8.1	Could add here that charge balance between cations and anions may also be used as a proxy for alkalinity, as long as this is validated through field measurements. (Noting that alkalinity may not equal charge balance alkalinity if e.g., concentrations of DOC are high in soil waters.)	Thanks for this note - we added a nod to DOC contributions to alkalinity.	CHANGE MADE
Section 8.1	Just to flag this but this implicitly allows for enhanced weathering using carbonate minerals if I understand correctly. Not sure if this is intended.	Thank you for this clarifying question. Carbonate mineral feedstocks are also considered within the scope of this document.	NO CHANGE
Section 8.1	I fully agree that carbonates should be measured in the feedstock and would clarify here that this should also be done for silicates like basalt, as they may have partially carbonated already before applying them to the soil. If this is not included, the NFZ measurements are likely to overestimate the CDR that occurred in the field.	Thank you for this suggestion. We've added the clarification to the final paragraph in this section.	CHANGE MADE
Section 8.1	Note that this discussion is also important for counterfactual CDR calculations when liming is involved. May be worth having some discussion about this.	Thank you for making this point - we agree and have added in a nod to this consideration in the counterfactual.	CHANGE MADE
Section 8.1	Please consider citing M. Grace Andrews, Andrew D. Jacobson, The radiogenic and stable Sr isotope geochemistry of basalt weathering in Iceland: Role of hydrothermal calcite and implications for long-term climate regulation, Geochimica et Cosmochimica Acta, Volume 215, 2017, Pages 247-262, https://doi.org/10.1016/j.gca.2017.08.012 . or another work by these authors. They have priority on the use of Sr isotopes to partition weathering fluxes.	Thank you for pointing this out, citations were added.	CHANGE MADE
Section 8.2	Could you stress here how the dissolution does not equate to carbon export. I think this is elsewhere, but useful to tie it together	Thank you for this nudge. Please see the paragraph beginning with "In the presentation..." where we state that dissolution is potential CDR and not carbon export. We've also added a terminology	CHANGE MADE

		section in Section 2 that also explicitly states this.	
Section 8.2	I don't find this formulation very clear. The exchangeable cations are in dynamic equilibrium with the cations in the soil solution; it should not matter whether a cation released from the feedstock through carbonic acid hydrolysis ends up retained on an exchange site or not - unless they displace exchangeable acidity. The process is much better explained L 2719-2726; perhaps it would be clearer to just refer to this section?	Thank you for this nudge. We've clarified the explanation to highlight the proton balance implications.	CHANGE MADE
Section 8.3	I'm not sure if anywhere in the main document you've listed the equations for strong acid weathering? (Sulfuric and nitric- ie see column F). Perhaps you can add these somewhere, which you can then refer back to when you talk about CO2 release due to strong acids?	Thanks for the suggestion, equations were added to the document.	CHANGE MADE
Section 8.3	I view the pH dependent CO2 speciation and weathering driven by non-carbonic acids as two separate mechanisms, and would suggest to separate these out into two sections and variables	Thank you for this nudge! While we agree that speciation and strong acid inputs may be constrained by different empirical approaches, it's ultimately the proton balance of the system. We have thus maintained the single combined discussion.	NO CHANGE
Section 8.3	Non-carbonic acid weathering does not lead to base cations charge-balanced by DIC, but does neutralize acidity that may have otherwise reacted with bicarbonate and caused CO2 degassing. Definition of "non-carbonic acid weathering" is needed somewhere in the text. I might have missed it. My understanding is that "non-carbonic acid weathering" refers to those non-carbonic acids that buffer the alkalinity generated during weathering - this is why I am finding the above sentence unclear. Such non-carbonic acids would include (i) strong inorganic acids (nitric, sulphuric), but also (ii) organic acids (hard to monitor as they might become microbially degraded). Ligand exchange reactions between OH and both organic and inorganic ligands (e.g., sulphate) might need to be considered as well, but perhaps as part of the surface reactions described further down the text of the Foundations document.	Thank you for pointing this out. We added a clarification to the text and weathering equations are also included for clarity.	CHANGE MADE
Section 8.3	I wonder whether in this section it is worth considering the work	This is an interesting environmental consideration	NO CHANGE

	being carried out on nitrate attenuation (through denitrification to N ₂) in groundwaters in the case that it becomes impacted by changes in alkalinity. There is a lot of work on nitrate attenuation being carried out in New Zealand by Ranvir Singh and others.	for ERW in the FFZ, thank you for bringing it up. However, we think it is beyond the scope of this section and broadly outside the scope of this document. This is briefly mentioned in Section 9.3.1 with respect to nutrient cycling.	
Section 8.3	The Dietzen & Rosing (2023) method of accounting for strong acid weathering should be included here. While the method of measuring pH for use in this calculation can be debated and more research is needed on how to best determine in situ soil pH, the calculations based on carbonate chemistry remain valid. pH of soil samples tends to be lower than pH of porewater samples (one could argue that using soil pH is therefore a conservative method of calculating this), which may cause concern about the validity of using that measurement, but porewater samples are not necessarily representative of the full soil porewater system either, as they can be skewed by preferential flow from macropores which tend to have higher pH than micropores, and therefore may be an overestimate of the overall pH of the system.	Thank you for this note. We've included a callout for the calculation at the end of the section, highlighting the importance of data and continued development to support this kind of tool development.	CHANGE MADE
Section 8.3	and organic acids?? Although once released they may have been microbially decomposed	Thanks for the nudge. We've added a brief discussion of organic acids and included the Working Group recommendation.	CHANGE MADE
Section 8.3	To be clear, is redundancy required only for constraining non-carbonic acid weathering?	Thank you for this question. We see this as a general call to action. Understanding the impact of additional sources of acidity in the soil system is one example where having redundant measurements is incredibly useful so we include this call to action here. Notably, this document does not represent crediting requirements.	NO CHANGE
Section 8.3	and organic acids?? Although once released they may have been microbially decomposed	See above.	CHANGE MADE
Section 8.3	Exchangeable acidity needs definition. In many instances is aluminium what is being exchanged at sites. Only at pH values below 4.5, protons are found at exchange sites. Regardless, as indicated further down the Foundations document, in both	Thank you for this suggestion. We've added a discussion of exchangeable acidity at the beginning of the section.	CHANGE MADE

	instances (Al, H), their presence undoes CDR.		
Section 8.4	When base cations exchange for acidic cations on exchange sites (e.g., H ⁺ , Al ³⁺), the removal of those cations from solution 'undoes' any CDR they were driving, or equivalently the proton release into solution drives CO ₂ evasion. --————→ I like that this is explicitly considering that the base cations must exchange for acidic cations. I don't think the 'undoes' CDR aspect is helpful for the mechanism. The base cations exchange for acidic cations, which enter the water and reduce alkalinity and decrease pH, these changes in turn drive CO ₂ evasion.	Thank you, we clarified this statement.	CHANGE MADE
Section 8.4	As base cation concentrations in the soil solution decrease, or the soil begins to re-acidify, these cations eventually can re-enter the soil solution and drive CDR ----————→ This is more or less the same framing that I have been highlighting throughout the document. It technically isn't the cations that are driving CDR. It is the pH and alkalinity shifts that are resulting in CO ₂ entering or leaving the water.	Thank you, we've added in additional clarifying information in Section 8.3 around this topic as well as edited this paragraph.	CHANGE MADE
Section 8.4	This is a detail, but a dunite is > 90% olivine (vs olivine-bearing, which makes it sound like olivine could be just an accessory phase).	Thank you for catching this - change made.	CHANGE MADE
Section 8.4.1	General remark regarding timespan: unless I missed it, there is no indication of how long it may take for alkalinity/DIC to leave the NFZ. The time needed will depend strongly on the system (soil properties etc), but will probably be quite long (years), as several soil column experiments showed very little (or even no) increase in alkalinity leaching over the course of the experiment. It may be good to add a note to make practitioners aware of that drawback (if not already mentioned elsewhere). From the text, they may get the impression that the NFZ,RP method is ideal (no measurements on plants, carbonates, ... required), while the long delay and difficulty in detecting changes is a serious complicating factor.	Thanks for this question and we agree that time lags are a key research question for the field over the next 5 years and extremely important for practitioners. We do include the Kanzaki paper (which provides estimates of lag times in different representative suites of conditions in a model-only study) in reference to conditions that will have lower time lags, as well as a discussion of the results from mesocosm papers that highlight lower bounds on the timescale of such lags given the study length.	NO CHANGE
Section 8.5	(Secondary carbonate formation) – For all the reasons discussed in Sect. 9.1.1, I am still not convinced that treating secondary calcite as CDR is the right approach for conservative	Thank you for this discussion. There are a few places where we wrestle with considering secondary calcite precipitation as CDR, one being	CHANGE MADE

	accounting, especially for fields where it's negligible before the deployment. Secondary calcite is not durable. Often, the conditions that allow net calcite accumulation will be directly caused by the project. When the project wraps up, that calcite might dissolve directly with the next fertilizer spreading, driving widespread non-carbonic acid dissolution that undoes the credited CDR. This is mentioned in section 9.1.1, but quickly followed up with "Despite this... crediting the pedogenic carbonate generated... should be sufficiently conservative". I think more support is needed to make that jump.	Section 8.5, the other Section 9.1.1, and the first being section 5.4.1. We've added a new paragraph to Section 5.4.1 to address the reasoning to include secondary carbonate precipitation in the NFZ as CDR in the term balance while also acknowledging the reversal risk and risk for non-carbonic acid weathering of carbonates. Briefly, the term balance includes DIC in the NFZ as CDR that was generated and therefore we reason that carbonates in the NFZ should also be included as CDR that was generated. We've added a nod to the discussion in 5.4.1 in this section as well. We've also addressed the comment for 9.1.1 (see below).	
Section 8.5	Great to see the equation. I wonder if it would be helpful to have some of this earlier in the document to minimize confusion for folks. Per some of my previous comments about carbonate formation acting as a sink and as a release mechanism.	Great suggestion - we have incorporated the reaction for carbonate mineral formation (in circumneutral conditions) in Section 2 - now Eq. 2.1.2.1	CHANGE MADE
Section 8.5.1	Don't want to make it overly long, but is it important to mention the role of biology?	Thanks for this suggestion - we've added a nod to biologic mechanisms for pedogenic carbon formation.	CHANGE MADE
Section 8.5.1	Didn't Deng et al., 2023 (Sci. Rep.) find substantial secondary magnesite precipitation in response to ERW in their modeling experiments? Is their parameterization / setup faulty?	Thank you for this note. We've added a nod to this in the text that with very high Mg concentrations and in certain conditions, Mg-carbonates may form.	CHANGE MADE
Section 8.5.2.1.1	It should be noted that Calcisols belong to the WRB classification and NOT the Soil Taxonomy. Also, that there are other soil types that have calcic horizons, such as Kastanozems and Luvisols (in the WRB) and many Aridisols, Mollisols and Alfisols (in the Soil Taxonomy). I am not a pedologist. It would be good a pedologist could check the text where soil classification is included.	Thank you for catching our loose use of soil classifications. We have revised the text to specify 'soils that contain calcic horizons' instead of a suite of soil classifications.	CHANGE MADE
Section 8.5.2.1.2	More details here would be helpful - this is actually very complicated and not trivial	While we very much agree, such 'standards of practice' measurement guidance is outside the scope of this document.	NO CHANGE
Section 8.6.1.1	Another scenario could be organo-Al complexes where OH	Thank you for highlighting this. We've added a nod	CHANGE MADE

	generated through silicate weathering displaces the organic ligands of organo-Al complexes, and the "freed"Al precipitates as Al hydroxide. In such a situation, that alkalinity (now neutralised) would not contribute to CO2 removal.	to this in the paragraph.	
Section 8.6.1.1	Fe- and Al-(oxy)hydroxides can reduce CDR if they lock up base cations. It may be relevant to note here that it is inorganic CDR that is reduced but this may be countered by increased sequestration of organic C, as Fe- and Al-(oxy)hydroxides have a strong potential for binding organic C. (this is also the case for secondary clays)	Thank you for this recommendation. We had tangentially alluded to this in Section 8.8 (organic carbon dynamics) but have now directly nodded to the role of Fe/Al oxy-hydroxides and clays [particularly short-range order minerals] in stabilizing SOC.	CHANGE MADE
Section 8.6.1.2	XRD would not allow detection of non-crystalline forms.	Great point - this has now been clarified in Section 8.6.1.2. Thank you!	CHANGE MADE
Section 8.6.1	I agree that considering the type of secondary phase likely to form is the place to start. I would like to point out that the secondary phases most likely to form rapidly in the field are short-range order (SRO) phases (vs crystalline clays), such as for instance imogolite-type material. I suggest adding the detection and quantification of these SRO phases to the Call to action, and / or to the research priorities of 8.8.7 (investigating SOC stabilization on SRO phases), bearing in mind that they provide most of the organic C sorption capacity in soils in which they occur.	Thank you for calling this out! We have more explicitly discussed both the importance of detecting and quantifying SRO phases and the potential for SOC stabilization by SRO phases in this section.	CHANGE MADE
Section 8.6.1	It would be good to consider the equilibrium relationships in the SiO ₂ -Al ₂ O ₃ -H ₂ O system (Macías et al., 2008). The mineral phases with greater stability are gibbsite, kaolinite and pyrophyllite/smectite, this being fundamentally dependent on the concentration of Si in solution. However, as recognized by the Ostwald ripening law, when saturation conditions of a specific mineral are attained, this will form as a meta-stable phase and evolve over time toward a mineral of greater stability. Therefore, under conditions in which kaolinite is the thermodynamically most stable form, conditions of the soil solution could be such that meta-stable constituents, such as allophane, gibbsite, halloysite or imogolite are formed instead. The slow kinetics of crystallization of kaolinite favors the	Thank you for this push to further discuss metastable phases. We have supplemented the discussion to more specifically emphasize the need to consider metastable phase formation (and persistence).	CHANGE MADE

	<p>persistence of other meta-stable phases under specific conditions, such as those of Andosols. Macias, F., Camps Arbestain, M., Chesworth, W., 2008. Acid soils. In: Chesworth, W. (Ed.), Encyclopedia of Soil Science. Springer, Dordrecht, pp. 7–10.</p>		
Section 8.7	<p>I also think that plant uptake is important for cation accounting in general, when using cations as a measure of extent of weathering. If a plant takes up cations, then those cations will appear to be lost, and thus assumed to be weathered.</p>	<p>Totally agree here; thank you for this suggestion. We have added a clarifying sentence that emphasizes the implications for cation accounting specifically.</p>	CHANGE MADE
Section 8.7	<p>Accounting for alkalinity uptake into plant biomass is thus important for a holistic accounting of netCDR ----> This is not technically correct. The plant is taking up cations and releasing H⁺. It is not taking up alkalinity. It is the release of H⁺ that reduces alkalinity outside of the plant.</p>	<p>Thank you for the recommendation to sharpen this! We have revised the section to specify base cation uptake into plant biomass.</p>	CHANGE MADE
Section 8.7	<p>Significantly more data on how base cation uptake varies across crop types and as a function of growth rate, growing conditions, soil enzyme and microbial activity, and soil chemistry (pH, base cation concentrations, nutrient ratios, etc.) is needed to produce such a predictive understanding -----> I agree that predictive understanding is not yet possible. But I think there is enough data in published literature to get some bounds on the uptake rates. (http://link.springer.com/10.1007/s11104-016-3089-5) See the SI table. There are large ranges in uptake rates, but agricultural crops are relatively well studied compared to natural vegetation in terms of uptake, etc.</p>	<p>Thank you for pointing us towards this data compilation! We have now referenced it in the discussion.</p>	CHANGE MADE
Section 8.7.1	<p>An important section and well covered! It would be good to note that the methods for examining the chemistry of biomass vary per species and I think it would be rare to consider the entire crop in a single measurement approach. My understanding is that certain parts of the vegetation are sampled and then calculation approaches are used to estimate entire changes in biomass - which are standard methods in agronomy. Could be worth mentioning then, how vegetation is sampled is important</p>	<p>Thank you for this recommendation! We have included a caveat in our sampling recommendations for annual crops to directly measure the total base cation content of harvested biomass, which includes all relevant portions of the plant. However, it is currently out of scope for this document to recommend specific sampling</p>	CHANGE MADE
Section 8.7.1	<p>It would be better to rewrite Eq. 5.4.1.2.1 in the chapter, so there</p>	<p>Thank you for this suggestion! We have now</p>	CHANGE MADE

	is no need to refer to the previous sections, which are very far back.	provided an equation appendix to allow for more accessible referencing of equations throughout the document.	
Section 8.7.1	I would clarify here that the goal is to quantify base cations in the EXPORTED biomass. Crop residues left on the field will decompose and the alkalinity which was taken up will be released again. This is assuming that the soil organic carbon content does not increase significantly over time (which is a reasonable assumption in many agricultural fields). This reasoning is implicit in the paragraph, but it might be worth stating it more explicitly.	In the text, we recommend that "practitioners focus on quantifying the magnitude of base cation uptake into biomass that is removed from the field for netCDR quantification, but to consider base cation incorporation into below-ground biomass and biomass that is returned to the field in research studies" which we believe appropriately addresses the need for stating the goal of quantifying base cations present in exported biomass.	CHANGE MADE
Section 8.7.1	I agree on plant nutrient uptake measurements. Reading 'harvested biomass (line 3303)' I now wonder whether quantification of mass and plant nutrients of only the harvested above ground biomass is sufficient, or if practitioners should also take the root system into account as this might also be affected size and composition wise from the ERW deployment? Or is anything that is not harvested and remains on the field considered in the following section of 'organic carbon'?	Thank you for this question. Because of the current complexities in accounting for base cations in root biomass, and because base cations incorporated into root biomass can be re-released into the soil during decomposition (annuals) or to move up into biomass that is then exported (perennials), we will not currently be recommending measurement of increases in root biomass. We currently recommend in the text that "practitioners focus on quantifying the magnitude of base cation uptake into biomass that is removed from the field for netCDR quantification, but to consider base cation incorporation into below-ground biomass and biomass that is returned to the field in research studies."	CHANGE MADE
Section 8.7.1	The requirement to measure base cation content in harvested biomass introduces an additional financial burden to the project. This specialized analysis represents yet another cost in an already expensive testing regime, further straining project budgets and resources. Moreover, obtaining representative samples for this analysis presents its own set of challenges. Coordinating with farmers to collect these samples can be complex, as it requires careful timing and potentially disrupts	Thank you for this perspective. In order to provide a holistic carbon accounting, cations should be accounted for regardless of the additional financial burdens placed on the project.	NO CHANGE

	their normal harvesting routines. Farmers may be reluctant to allow sampling of their crops, especially if it involves removing a portion of their harvest or interfering with their post-harvest processes (dependent on sample size required).		
Section 8.8	Please consider whether it would be appropriate to cite Rowley, M.C., Grand, S. & Verrecchia, É.P. Calcium-mediated stabilisation of soil organic carbon. Biogeochemistry 137, 27–49 (2018). https://doi.org/10.1007/s10533-017-0410-1	Thank you for the recommendation. Change made and citation included.	CHANGE MADE
Section 8.8	I think it is important to recognise the different timescales of operation. For example how do changes in SOC on short timescales relate to longer timescale SIC fluxes?	We agree that it is important to recognize that SIC and SOC operate on different timescales. We state that "carbon stored as soil organic carbon has a substantially different permanence profile than that of ERW and therefore should not be bundled within the same accounting framework" which we believe sufficiently captures this sentiment.	NO CHANGE
Section 8.8	Cite the next section (8.8.1) here and the table.	Thank you for this suggestion. Included section reference.	CHANGE MADE
Section 8.8.1	It would be convenient that a pedologist could review this section. Perhaps soils with either an "andic" or "umbric" qualifier would have an intermediate risk? Lower than an alu-andic Andosol and an Umbrisol.	Although true that there is potential for soils with an "andic" or "umbric" qualifier to have more of an intermediate risk rather than high, we will keep the current "high-risk" delineation until evidence comes forth in favor of a lower risk profile for these soils.	NO CHANGE
Section 8.8.1	I also wonder whether soils in which there is a dominance of weatherable minerals, as is the case of Andosols on basalt deposits, there might be too much background noise with the presence of weatherable minerals. Is this considered anywhere in the text?	Thank you for the flag. Such considerations are also now emphasized in Section 3 alongside discussions in Sections 8.1 and 8.2, as well as in Section 7.5.	CHANGE MADE
Section 8.8.1	In the table, the threshold for SOC in soils is put at 5wt%. What is this threshold based on (besides the calculation that is made in the text)? I think this threshold is rather high and would recommend reducing it, perhaps to 3wt% or even less.	We have added in the text that the 5% is somewhat of an arbitrary threshold and have both noted the potential for large SOC loss scales with increasing SOC content in soils as well as encouraged increased caution in soils that have OC content in the 3-5% range.	CHANGE MADE
Section 8.8.1	The point about acidic andosols is tricky, if the soils are being	Thank you for this comment. To address this	CHANGE MADE

	limed in the BAU case, then the SOC would be lost anyway?	comment, we have included the text: "Any site where commercial soil pH modification (e.g. liming) is a current practice should be evaluated on a case by case basis and the risk of SOC loss (compared to the counterfactual scenario) should still be strongly considered on these sites." We agree that acid Andosols are a complex situation if they are being limed in the BAU case, and therefore should be evaluated on a case-by-case basis to understand the risk of SOC loss from the ERW deployment.	
Section 8.8.2	This may be too long of a window to guarantee, as farmers are often hesitant to sign longer term contracts (beyond a few years), even if it's just for additional sampling and no other treatments	Thank you for this comment. Although true that the window may be too long to guarantee, 10 years is still recommended as a bar to strive for in deployments to get the most accurate read of any changes in SOC dynamics, especially as it can take several years for an effect on SOC stocks to become apparent.	NO CHANGE
Section 8.8.2	The proposed monitoring period of 10 years presents a significant challenge in terms of practicality and sustainability. A couple of challenges come to mind surrounding sustaining monitoring methods, maintaining engagement of farmers and data consistency.	Thank you for this comment. Because of the longer timescales upon which it takes for changes in SOC fluxes to become apparent, the recommended 10-year timeframe should be implemented whenever feasible.	NO CHANGE
Section 8.8.5	Perhaps it should be specified, in line with the consideration of the dilution effect, that soil bulk density soon after the rock application would need to be taken into account?	Thank you for this recommendation! Bulk density measurements are already recommended to be performed in baseline and post-application soil sampling events (Section 8.8.2)	NO CHANGE
Section 8.8.5	If the expectation is to undertake SOC monitoring at the same scale as used for SOC crediting there will be severe implications for costs. This only works out if it is possible to claim a credit for SOC increases but currently the IC/EW and SOC protocols cannot be combined.	Thank you for this comment. This has been considered, and hence these SOC monitoring recommendations are purposely not included in the netCDR carbon accounting of the deployment (Section 8.8). We included an augmented discussion regarding the need to navigate between different carbon quantification and crediting regimes in section 5.4.1. SOC is currently a key R&D	NO CHANGE

		priority and monitoring should be undertaken at the highest resolution possible to advance the field's understanding of ERW-SOC interactions.	
Section 8.8.6	This section feels like it has content that is relevant for other parts of section 8, including secondary calcite and clay precipitation. It felt a bit odd that the suggestion of an annual meta-analysis showed up here, with regards to SOC. Maybe this is a section to pull out and make more general?	Thank you for this suggestion! We agree that the need for regular meta-analysis applies far more broadly than just ERW-SOC interactions. We have maintained the call to action regarding regularly revisiting new findings in the SOC section, but have added a new section (8.11) that addresses the broader need for regular meta-analysis.	CHANGE MADE
Section 8.8.6	I find it a bit strange to say that a meta-analysis is needed at least once per year. Frequent re-evaluation is good, but is it useful to recommend a new meta-analysis every year? First, we'll need to wait until we actually have enough data to conduct a meaningful meta-analysis. This will then inform where more data are needed and as these are becoming available, new meta-analyses can be conducted. I don't think it is really useful to specify this as 'at least one per year is needed'.	Thank you for this suggestion. We have removed the call for a meta-analysis on a specific time interval following your recommendation.	CHANGE MADE
Section 8.8.7	Considerations for future research priorities: do we prefer fewer experiments where we have more complete datasets, or rather more experiments with only few of the data of interest? For SOC-ERW interactions, long-term experiments will be essential to capture the full extent of the silicate effect. As modeling will be crucial for MRV of ERW, the measurement approaches in experiments should also take into account modeling requirements. A strong model-experiment interaction would greatly help to advance our knowledge on ERW and SOC-ERW interactions faster.	Thank you for this discussion. This has been taken into consideration for future research priorities in ERW-SOC interactions.	OTHER
Section 8.8.7	I think another important R&D angle is looking at SOC fractions, as they have different durabilities. The amount of SOC may not change in a given year but if the form of SOC changes it may be more at risk of being lost	Thank you for this nudge. We included it in the future research priorities for ERW-SOC interactions.	CHANGE MADE
Section 8.10	Some of RTMs have been used as ERW simulator without further specific implementation of additional functionality useful for ERW (e.g., bio-mixing, particle size tracking). Do they need	Thank you for this question. While we think this could be helpful, we consider more detailed discussion outside the scope of the high-level	NO CHANGE

	to be recast as ERW simulator and require additional documentation as ERW simulator in addition to the manuals already available for those RTMs?	considerations presented in this document.	
Section 8.10	How about code availability? Are models supposed to be available to anyone or just documentation is enough? Is code availability important for the automatization process for model validation or not?	Thank you for this very important question that the broader ERW community will need to navigate over the coming years; we left it out of scope of this document given the complexities of the discussion.	NO CHANGE
Section 8.10	Where results are expected to be seen? Publication or/and some websites?	Thank you for this question, however, it is outside the scope of this document.	NO CHANGE
Section 8.10	Sensitivity analysis might overlap some other model evaluation especially intermodel comparison or model validation given that it can be basis for those other aspects of model evaluation	We agree! We see all of these components as highly interrelated and complementary.	NO CHANGE
Section 8.10	This section clearly states that models have to be compared, peer reviewed etc. While the poll at ERW24 showed that no one thinks that models are ready for deployment. There is a big drive by key market players to move to models earlier rather than later (i.e. within the next 2-3 years). As such, this section would benefit from guardrails, as presented in other sections of the document. Specifically, at what point in time can measurement density be reduced by models. The data we have seen so far is not convincing.	Thank you for this discussion. We agree that a third-party system for model validation is needed, but more precise rules for how this may work (beyond the high-level discussion provided) is out of scope for this document.	NO CHANGE
Section 8.10	Models: would like Cascade to be a bit more specific on the modeling side around what is required in terms of the data and the predictiveness of the models—have seen wild models that do not like the data It was brought up in the survey at ERW24 that indicated from the general group that the models are not ready yet and the tension that suppliers have timelines for model integration and use in quantification that are very quick (2-3 years) with reduced sampling density	Thank you for this discussion. Out of scope for this iteration of the document; much more work to do here though!	NO CHANGE
Section 8.10	To be more general, here perhaps I would distinguish between process-based and data-driven models. Then among the process-based, common examples are RTMs (Beerling etc.)	Thanks for the feedback - we revised our terms / categorization to match your suggestion.	CHANGE MADE

	and integrated soil models (Bertagni etc.).		
Section 8.10	Process-based reactive transport models (RTMs) are currently being developed that computationally simulate the soil physics and chemical processes in ERW -----→ I think there are existing RTM codes that can be used as well. Like MIN3P (http://onlinelibrary.wiley.com/doi/10.1029/2001WR000862/abstract) and I imagine others as well	Thank you for this nudge and we made a clarification in the text.	CHANGE MADE
Section 8.10.3	You could just say "model validation". I am not sure if "model-data validation" is appropriate, since here we validate the model, not the data. Alternatively, you could say "model-data comparison"	Thank you for this suggestion - we modified the text.	CHANGE MADE
Section 8.10.3	The two sections Benchmarking and Model Intercomparison could be merged, since they really go together. For example, the ILAMB framework evaluates models against data and compares the performance of each model, at the same time. So the same reason why you have sensitivity analysis and uncertainty quantification together.	Thanks for the suggestion - we merged these two sections in the final draft under the section header of "Benchmarking for ERW".	CHANGE MADE
Section 8.10.4	Taylor et al., 2023	Thank you for catching that - we've fixed the reference in the text.	CHANGE MADE
Section 9	River evasion—river data: looking at supersaturation—Carbon Tipper they say it's 1 but the data isn't 1 Moras, C. A., Bach, L. T., Cyronak, T., Joannes-Boyau, R., and Schulz, K. G.: Ocean alkalinity enhancement – avoiding runaway CaCO ₃ precipitation during quick and hydrated lime dissolution, Biogeosciences, 19, 3537–3557, https://doi.org/10.5194/bg-19-3537-2022 , 2022.	Thank you for highlighting this. See Section 8.5 for a more extensive discussion on potential supersaturation state thresholds to consider. We agree that SI=1 may be relatively high and recommend that it be considered the upper limit of saturation state thresholds.	NO CHANGE
Section 9	General point on the FFZ -- what about other bodies of water, such as lakes?	An interesting possibility! We've added a nod to the process in lakes. This would need to be explored further before being used as part of a crediting system.	CHANGE MADE
Section 9.1.1	This sentence could be simplified. My suggestion is that the initial part about high temperature liberation be omitted, because it's not really relevant here. Instead simplify to something like "Carbonates formed from ERW dissolution	Thank you for this suggestion and we've modified the text to be clearer in intent.	CHANGE MADE

	products in the soil profile or along downstream flow paths are liable to redissolution and export, which should be considered.		
Section 9.1.1	accounting for the lower potential CDR efficiency of the base cation released from the alkaline feedstock ----→ I don't follow this inset in the sentence. A cation is captured in the mineral along with carbon. However, the reaction releases CO2 as well. Is that what is meant here? I don't understand the 'base cation released from the alkaline feedstock' aspect.	Thank you for pointing this out - we have removed the inset from this sentence for clarity. The reduction in CDR efficiency associated with carbonate mineral formation is discussed elsewhere in the text.	CHANGE MADE
Section 9.1.2	Does allowing for storage in long-residence time groundwater systems lead to some increased uncertainty in the final storage reservoir? This would probably depend largely on flow rate I guess and how well-characterized the groundwater system in question is. Some guardrails would also need to be in place to ensure that the groundwater system isn't tapped for irrigation or wells or anything like that (could possibly be factored into a buffer pool or as an uncertainty discount).	Yes we couldn't agree more. Practitioners who use this as a storage reservoir will need to approach cautiously.	NO CHANGE
Section 9.1.2	Following a pedogenic carbonate section, this title led me to assume we'd now turn to groundwater carbonates (rather than groundwater residence times). Was the topic of groundwater carbonates considered for either section 8 or 9? This feels like a potentially critical alkalinity sink in certain environments, especially those where the groundwater table is quite seasonal. Apologies if this was discussed somewhere and we missed it!	Yes we couldn't agree more that it's important to consider and we discuss this in Section 9.2.	NO CHANGE
Section 9.2	Small point- it might be worth redefining R&D here, considering how big the document is	Thank you we added a list of acronyms in the appendix.	CHANGE MADE
Section 9.3	To net loss of carbon and a decline in CDR efficiency -- → there can also be reactions that change alkalinity, not just carbon. Degassing changes carbon but not alkalinity. Carbonate ppt changes both, etc.	Thank you for this note. While this is absolutely true, we're chosen to just focus on the carbon implications in this sentence.	NO CHANGE
Section 9.3	The first sentence of the two here is intuitive to me, but I find the link between this and the second sentence a little unclear. I think the clarity might be helped by defining strong acid weathering somewhere else in the document, and then referring back to equations here. Do you mean strong acid weathering by	Thank you for this suggestion. We did indeed mean sulfuric and nitric when strong acids are referenced and we've split this sentence to separate out the two thoughts. We've also added a note that the counter ions must be removed before DIC can be	CHANGE MADE

	nitric and sulphuric acids (These acids are typically referred to as strong acids in literature)? Or just low pH soils with high carbonic acid? I would remove the words 'strong acids' if you were not referring to nitric/sulphuric. If you do mean sulphuric and nitric acids, the cations weathered by these acids will be balanced by DIC only if the nitrate and sulphate formed (see equations) have been removed from the system (i.e. nitrate is removed by nitrification). So to me that's a different point...	balanced.	
Section 9.3	The base cations released by weathering in low-pH soils or through strong acid weathering reactions can be charge-balanced by DIC in higher pH downstream systems ----→ I am struggling a bit to see how this is additional CDR. Those base cations are charge-balanced by bicarbonate in the near field zone and continue to be balanced by bicarbonate in the far field zone. Is this assuming non-carbonate weathering or strong acid weathering? In this context, I can see how it works. A strong acid weathers the feedstock instead of carbonic acid and it is only later in the process that we impact the carbon budget.	Thank you for this question. Yes this is what is being discussed here - the delay in CDR until the FFZ due to high acidity and/or strong acid weathering in the NFZ. We've modified this section for clarity.	CHANGE MADE
Section 9.3	This one is tricky. With DIC/ALK at 1:1 ratio from ERW (such as carbonate rock weathering), then the receiving river will immediately have a higher pCO ₂ level and more CO ₂ degassing. So whether we will see immediate more or less CO ₂ degassing depends on the incoming DIC/ALK ratio. Yes. ERW will increase Alkalinity, but it will also increase DIC. In addition, the receiving water will mostly have a higher pH values but if the original pH value is already big (for example, ~7.9), then the river will first have a lower pH with incoming DIC/ALK at 1:1 ratio, then with more CO ₂ degassing, we might see a higher pH value. Again, whether pH is higher or lower depends on the original pH value and the DIC/ALK input ratio, as well as the time frame. So this second point might not always be correct.	Thank you for this discussion. This bullet point is geared towards how CDR could be driven in downstream systems rather than all the possibilities based on receiving water chemistry. We've added a note to emphasize that.	CHANGE MADE
Section 9.3	Increasing alkalinity levels and pH in the receiving waters of ERW deployments could reduce existing levels of CO ₂ degassing due to carbonate buffering, particularly in low order	Thank you for these thoughts and we agree that this is an important consideration for the FFZ moving forward. We have discussion along these	NO CHANGE

	streams --- → I have been thinking about degassing a lot recently. I think that if the water ultimately equilibrates with the atmosphere within the ocean, it doesn't really matter if degassing happened in a stream. Because CO2 will be released or taken up according to the ocean chemistry. To impact the ocean in the case of equilibration with atmosphere we really need to impact alkalinity. However, it is possible that water does not fully equilibrate with the atmosphere. so in this case, the degassing does matter. I guess this is not so much a comment on the document per se but an explanation of my thoughts.	lines in Section 9.3.2.1.	
Section 9.3	Addressing uncertainties and being conservative is important, but must be balanced against financial viability. Capping uncertainties at acceptable levels is necessary to keep operations economically feasible. This requires prioritizing critical issues, setting reasonable thresholds, and using cost-effective methods	Thank you for sharing this perspective. No change has been made in the document.	NO CHANGE
Section 9.3.1	I am not sure if we could use "organic CO2 emission". I don't recall seeing something called "organic CO2 emission". I feel a better way is to frame this is "CO2 emission due to DOC cycling". We might also consider changing "organic carbon emissions" to "carbon emission due to DOC decomposition"?	Thank you for this nudge to clarify the language! We have amended the text along the lines you suggest.	CHANGE MADE
Section 9.3.1	I might be inclined to simplify this sentence as it is a little unclear to me, to something like: "Another aspect of the net carbon balance that may need attention as ERW is scaled up is whether it alters the alkalinity produced by natural bedrock weathering"	Thank you for this nudge to clarify the language! We have amended the text along the lines you suggest.	CHANGE MADE
Section 9.3.2	Link to the sections, i.e. "to place particular emphasis on potential losses due to carbonate system equilibration (9.3.2.1) and carbonate mineral burial (9.3.2.2)"	For clarity, we've chosen not to include links to those upcoming sections here.	NO CHANGE
Section 9.3.2	I wonder whether this section needs a bit of restructuring-I find it a little confusing because it goes from recommended practises, to complications, to R&D, then back to recommended practises. I would be tempted to keep all the recommended practises together, and put complications and R&D after, in a new section, something like "9.3.2.3 Future research needed".	Thank you for this suggestion, however, we opted to keep this section as is for now in this version with the recommended practices and research embedded.	NO CHANGE

	Therefore my suggestion is to put this first sentence (4127-4129) after the bullet points 4124, and go straight to the more detailed 9.3.2.1, and keep the rest of the text in this section until later.		
Section 9.3.2	A balanced approach could be considered to address workforce load concerns while maintaining project specificity. This approach would involve treating the majority of uncertainties with a standard, conservative loss estimate, while reserving site-specific assessments for uncertainties with the highest levels of uncertainty.	Thank you for sharing this perspective. No change has been made in the document.	NO CHANGE
Section 9.3.2	I'd be more specific about "data- products" and list what data-products you are referring to. My suggestions might be river chemistry and catchment bedrock chemistry data?	Thank you for this suggestion, we left the data products open for interpretation for practitioners.	NO CHANGE
Section 9.3.2	I may not be the target audience for this, but I think how ERW influences the kinetics of riverine carbonate precipitation is a big research priority. For example, do increased fluxes of Mg make precipitation less favorable? Conversely, does increased nutrient flux increase microbial activity that drives precipitation?	Thank you for sharing this perspective! It is certainly an interesting avenue for future inquiry.	NO CHANGE
Section 9.3.2	R&D add ons: More than just models- lab studies/field studies exploring chemical changes using river water. Research that shows how ERW derived DIC might change DOC in rivers. More baseline chemical data and more bedrock data needed for different regions. More research using river chemistry to understand strong acid weathering (i.e. you can use SO4 and 34S isotopes in rivers to predict how much sulphuric acid weathering has happened, and you can use NO3 concentrations to make a first order prediction of nitric acids role in weathering).	Thank you for sharing this perspective! This is also an interesting avenue for future inquiry.	NO CHANGE
Section 9.3.2.1	There are earlier carbonate speciation calculations I might reference before a 2022 one. Like Zeebe and Wolf Gladrow 2001	Thank you for suggesting earlier publications that lay out carbonate system calculations. We've pointed to the Zeebe and Wolf-Gladrow book as well.	CHANGE MADE
Section 9.3.2.1	For the immediate discharge zone- are we being specific about how many measurements they should take- i.e. at just one	A good question but we are not making a specific recommendation because it will vary by location	NO CHANGE

	location at the discharge zone? Multiple?	and catchment.	
Section 9.3.2.1	At two points: in the immediate discharge zone (i.e., the surface water system the weathering flux from a deployment is draining into), and in the major river system of the deployment catchment (i.e., the highest order segment in the deployment catchment) --> Given that degassing does not change alkalinity and that re-equilibration can happen again in the ocean, I am wondering if degassing in the river system can be informed by what will likely happen in the ocean? If the river draining into the ocean will mix at the surface and likely equilibrate with the atmosphere, then it doesn't matter what degassing happens in the river. But if the river water gets drawn down deep and does not have the chance to equilibrate, then degassing does matter. This might (?) simplify things?	We agree - this is an important line for research in the near term. Thank you for the discussion.	NO CHANGE
Section 9.3.2.1	When practitioners do these measurements, should they be made to be open access? I.e. river chemistry pH measurements before and after?	This is outside of the scope of this technical guidance document. We have included a number of calls for data transparency throughout, but refrain from specifying specific requirements here. Thank you for this question.	NO CHANGE
Section 9.3.2.1	I feel the most direct way is to measure pCO2 directly instead of pH. Also, seasonal measurement (or even monthly) might be needed considering the impact of seasonal hydrologic conditions on pCO2. Later, we could change to something like: "If direct measurement of pCO2 is not feasible, then either direct estimates of temperature, pH, and Alkalinity, or a conservative estimate of those carbonate system parameters in the two spatial areas of interest". From experience with water chemistry (for example, USGS), they generally produce pH and alkalinity, instead of DIC (DIC data are much less compared with pH and alkalinity).	Thank you for this comment. We've highlighted the need for measurements of DIC and pCO2 in addition to pH and alkalinity on sub-annual timescales. However, we maintain there is value in collecting alkalinity and not just pCO2 for other purposes beyond CO2 evasion.	CHANGE MADE
Section 9.3.2.1	DIC/pCO2 ----> I believe for the USA there is a data product available that gives pCO2 for all the major rivers. Liu, S., Kuhn, C., Amatulli, G., Aho, K., Butman, D. E., Allen, G. H., et al. (2022). The importance of hydrology in routing terrestrial carbon to the atmosphere via global streams and rivers.	Thank you for pointing that out, we haven't referenced data products for specific regions given the global implementation of ERW.	NO CHANGE

	Proceedings of the National Academy of Sciences, 119(11), e2106322119. https://doi.org/10.1073/pnas.2106322119		
Section 9.3.2.1	Maybe we could add "worst-case" before conservative? Then later, I think you mean "full equilibration of air-water CO2 level"? For the water part, I think we always assume full equilibration of carbonate system (such as CO2, HCO3, CO3). It is only the disequilibrium between water CO2 and air pCO2 that leads to kinetic gas exchange. And if you assume equilibrium of this air-water CO2 level, then you will have a worst-case conservative estimate (calculated CO2 loss is big compared with the calculation using kinetic equation).	Thank you for that sharpening comment. We have clarified the text along the lines you recommend.	CHANGE MADE
Section 9.3.2.1	I was wondering if this sentence might be a little vague. For an average practitioner, would they be aware of what process models they could use for this? Are there any links to studies that use them? There could be an appendix with some recommended online tools? i.e. like PHREEQC	Thank you for this recommendation. While providing further detail on the specific geochemical solvers, etc. that could be used is outside the scope of the current document, we have referenced existing process-based models as an example.	NO CHANGE
Section 9.3.2.1	data collected over one hydrological year... At what frequency? Perhaps either weekly or monthly dependent on budget?	This is a good question and will depend on the catchment and deployment. We've left it open for the practitioner.	NO CHANGE
Section 9.3.2.2	It might be useful if you have any data on timescales to include this. Just a minor point. Like is there any timescales for how long it takes precipitated carbonates to re-dissolve?	This will very much depend on the details of the system of interest; we have thus refrained from attempting to bound the timescale of dissolution in generality.	NO CHANGE
Section 9.3.2.2	recommended that practitioners utilize conservative accounting, and thus consider carbonate precipitation without subsequent dissolution ---> If the carbonate stays precipitated, that is a long term storage of carbon. Though only half of what was weathered in the case of silicate. If that carbonate dissolves, it then travels downstream and could not be stored on the long term. I am not 100% certain that assuming it stays ppt is actually the conservative situation in all cases? Also the CO2 released in the reaction could get converted back to bicarbonate if the pH in the river is such that it favors that before the CO2 gets degassed, etc. Not straightforward, basically.	Thanks for the discussion. We agree this is not straightforward, however, we do think this is the conservative path for the current state of the field and models being used.	NO CHANGE

Section 9.3.3	I think that this paragraph 4287-4303 could be condensed/simplified. I would remove all the text 'there was a consensus amongst the working group...' ending with "may not be sufficient". Removing this allows the practitioner to go straight to what he/she needs to do. If you want, you could include this removed text in the R&D section I mentioned in the earlier comment.	Thank you for this simplification, the text was condensed as suggested.	CHANGE MADE
Section 9.3.3	after 'clearly documented' maybe to add: and publically available(?)	Thank you, we added the suggestion.	CHANGE MADE
Section 9.4.1.1	: I think you mean "increase of biogenic calcification rates", instead of "changes to biogenic calcification rates"?	Thank you for catching this--changed.	CHANGE MADE
Section 9.4.2	I think you can link this point back to the Steinhilber equation. The n value in this equation refers to carbonic acid equilibration in the ocean and is often given as between n= 1.4-1.7 (Renforth and Henderson 2017).	Thank you for this suggestion. We've maintained the description of a conservative estimate of evasion driven by carbonic acid system equilibration in the surface ocean instead of pointing to the Steinhilber equation in this case.	NO CHANGE
Section 9.4.2	I don't see Kanzaki et al. (2023) mentions that their estimate is conservative. I think the point here is that they are using a coarsely gridded Earth system model (c-genie). Maybe we could delete "conservative" here?	Thank you for catching this--changed.	CHANGE MADE
Section 10	Is there more to add in this section around defining system boundaries (e.g. for waste materials) or excluding avoided emissions impacts (e.g. reduction in fertilizer use)?	Thank you for this question. Discussions on handling of waste and byproduct streams in accounting for upstream lifecycle emissions and on handling of avoided emissions are now included in Section 10.	CHANGE MADE
Section 10	Expand on the linkage between the quantification of life cycle emissions to-date and the the rest of the foundations document & principles.	Thank you for this suggestion. Change made to clarify the linkage between Section 10---which focuses on the quantification of life cycle emissions---and the rest of the Foundations document.	CHANGE MADE
Section 10.1	How emissions ought to be considered for waste products of a commercial process is unclear (Section 10.1). We believe in exclusion of these emissions given proof of a waste product, though this ought to be clarified in writing within the CQS	Discussions on handling of waste and byproduct streams in accounting for upstream lifecycle emissions is now included in Section 10. Thank you for the suggestion.	CHANGE MADE

	document - this applies for many feedstocks such as basalt fines or industrial waste products.		
Section 10.1	If the feedstock is a waste or a byproduct that would have been produced in any case, should we still consider the emissions related to its extraction / processing ?	Discussion of handling of waste and byproduct streams in accounting for upstream lifecycle emissions is now included in Section 10. Thank you for this question.	CHANGE MADE
Section 10.1	As you are describing in detail all handling of the feedstock material, I am surprised you did not also include the final incorporation of the feedstock into the soil? Likely this is done with a different machine from the one that did the spreading - so extra fuel, electricity, water, ... Now I think of it, maybe it is worth to also mention (higher up in document) that timing and means of incorporating/mixing of feedstock into the soil after it has been spread is also an important step that needs to be planned and best done asap after spreading to avoid feedstock blowing away by wind (health hazard as well as not right amount added anymore to deployment area) but also to get wet from rain or irrigation (fine grained feedstock will then 'clump' together which makes homogenous incorporation into soil afterwards more challenging)	Thank you for this discussion and suggestion. Change incorporated into the document.	CHANGE MADE
Section 10.1	If the statement refers to routine agricultural maintenance practices that would occur regardless of the project, it's reasonable to question their inclusion in the Life Cycle Assessment (LCA). Standard farming activities that are not specifically tied to the project being assessed should typically be excluded from the LCA to maintain accuracy and relevance in the analysis.	Thank you for this suggestion. Change incorporated into the document.	CHANGE MADE
Section 10.1	We understand the reason why the working group recommended excluding MRV related emissions, but we are wondering if this could represent (or not) a risk of red flag in case of third-party audit of an ERW commercial deployment. → If that was discussed in the working group, we would be interested in having feedback.	The Working Group reached a consensus to exclude emissions associated with monitoring, reporting and verification (traveling to site, sampling, analysis, model simulations) from the system boundary. With ERW being in early deployment phases, direct and sometimes redundant measurements are necessary. Given such, inclusion of emissions associated with	CHANGE MADE

		monitoring and verification could skew results and disincentivize robust MRV. Therefore, the recommendation was that emissions associated with monitoring and verification need not be included in lifecycle emissions accounting. This recommendation was intended to prioritize more data to be collected and analyzed from direct and redundant MRV, and was explicitly not a recommendation for crediting activities. It is still strongly recommended that practitioners should continue to closely track emissions associated with these ongoing MRV activities for the purpose of carbon crediting, and it is best practice for carbon credit issuing programs to require the inclusion of ongoing MRV emissions in lifecycle emission accounting.	
Section 10.1	control or BAU plots' - I thought 'control' was the general term for both BAU and negative controls? So maybe the word 'negative' is missing here in front of 'control'?	Thank you for catching this. Change incorporated into the document.	CHANGE MADE
Section 10.1	Really interesting take! Isometric are currently requiring these to be included. Would be good to gather data on how significant these emissions are and how they can be accounted for in operational emissions.	The Working Group reached a consensus to exclude emissions associated with monitoring, reporting and verification (traveling to site, sampling, analysis, model simulations) from the system boundary. With ERW being in early deployment phases, direct and sometimes redundant measurements are necessary. Given such, inclusion of emissions associated with monitoring and verification could skew results and disincentivize robust MRV. Therefore, the recommendation was that emissions associated with monitoring and verification need not be included in lifecycle emissions accounting. This recommendation was intended to prioritize more data to be collected and analyzed from direct and redundant MRV, and was explicitly not a recommendation for crediting activities. It is still	CHANGE MADE

		strongly recommended that practitioners should continue to closely track emissions associated with these ongoing MRV activities for the purpose of carbon crediting, and it is best practice for carbon credit issuing programs to require the inclusion of ongoing MRV emissions in lifecycle emission accounting.	
General	This document is loaded with information, and so to help practitioners effectively use it, maybe you could consider including an executive summary, either at the beginning of the document, or each section. This summary could list the required measurements, and then direct the reader to the detailed sections. I think this could really benefit the document, to make it clearer exactly what is the (a) minimum required measurements and (b) what are useful additional measurements.	Thank you for this suggestion. We've edited Section 2 significantly to provide some additional overview information as well as including a Technical Summary to the document. There will also be audience-specific explainers.	CHANGE MADE
General	Similar to the executive summary, could you also provide a table at the end listing all the critical R&D needed? I think this could also be a really useful table.	Thank you for this suggestion and we have made an R&D appendix that includes the areas where R&D initiatives are mentioned throughout the document.	CHANGE MADE
General	The reviewed sections are very detailed, well written, and supported by numerous references. I commend the writers for their excellent work! My main criticism is that the text can at times feel vague. I suspect this approach is intentional as to be flexible in how these procedures are implemented, but it may be difficult for users, especially those new to ERW, to grasp some of these concepts without examples. While creating examples or hypothetical case studies would be a lot of effort, it would give users more details about how to implement these procedures.	We absolutely concur. Developing case studies that provide 'worked examples' of different components of this document is something that Cascade as an organization is keen to explore in the future, but is outside the scope of this v1.0 document.	NO CHANGE
General	It would be very helpful to add as many figures and illustrations as possible for better understanding and easier visualization.	Thank you for this suggestion and agree that figures can be helpful. However, we've decided to keep figures to a minimum and focus on the text for this initial version.	NO CHANGE
General	It would be very helpful to bold the terms or vocabulary to emphasize key points in the explanation.	Thank you for this suggestion. We've added some terminology explanations to Section 2 as well as an	CHANGE MADE

		acronym appendix.	
General	<p>This document contains a rich mix of content. It proposes a conceptual framework to organize processes that are relevant to netCDR (e.g. Section 2). It summarizes the state of science for individual processes that we know influence ERW outcomes (e.g. Section 8.5.1). It summarizes technical approaches to understanding the influence of particular processes on netCDR (e.g. Section 8.5.2), and identifies areas where it's clear we need more R&D (e.g. Section 8.5.2). It outlines an accounting framework for calculating netCDR in a way that could underpin crediting (e.g. Section 5.4.1). It specifies accounting options for incorporating specific processes into that accounting framework (e.g. Section 5.3). And in some cases, it takes a stance on a recommended or required technical or accounting practice (e.g. Section 5.3.1.4). It might be helpful to hand-hold readers so they can more systematically identify these different types of content and relate them to one another. Three ideas in that vein:</p> <p>(1) Would it be possible to name different families of content (e.g. "technical" vs "accounting") and process endpoints (e.g. "synthesis", "framework", "option", "recommendation", "requirement", "research flag"), define those terms upfront, and try to carry them consistently through the document?</p> <p>(2) Would it be helpful to add a section or figure upfront that succinctly summarizes what endpoint different topics reached?</p> <p>(3) Would it be possible to standardize (to the extent possible) how content of the same type is structured / formatted in different sections? For example, in Section 4.1, options for defining the near-field zone are presented as "approaches" in paragraph form; in Section 5.3, options for dealing with near-field zone time dynamics are presented under numbered subheadings; and in Section 5.3, end member options for LCA emission amortization are presented as bullet points.</p>	<p>Thank you for this synthesis and suggestions. We've modified Section 2 to include some terminology and tables with high level takeaways for the NFZ and FFZ. We've also followed the suggestion to standardize language around 'approaches' in the text and added an R&D appendix to indicate areas in the text that reference this.</p>	CHANGE MADE

General	Consider establishing a clear role for prescriptive language (e.g. "should", "must", "needed", "requirement") and defining when you use it upfront (e.g. obvious technical best-practice, agreed-upon assessment from a working group, etc.). This feels particularly important given the reframing of this document as a foundations document rather than a standard.	Thank you for this suggestion. We've added some of this explanation to Section 2 and have indicated where there was Working Group consensus in the text.	CHANGE MADE
General	Recognizing the figures might just be drafts, but if not, you may want to check for colorblind friendliness.	Many thanks for checking on this! We made sure to design with colorblind friendliness in mind in the production of final figures.	CHANGE MADE
General	There are at least two places where the foundations document takes a less conservative stance than the Isometric protocol. We wanted to flag them for additional consideration / scrutiny if it feels appropriate. (1) In the aglime swap case, our understanding is Isometric doesn't allow project proponents to subtract any counterfactual LCA emissions from the project LCA (while, of course, requiring project proponents to subtract counterfactual removals). This might be overly strict, but it guarantees that avoided emissions aren't roped into CDR which the foundations document approach does not necessarily do. (2) Isometric treats secondary calcite as a lag, not as CDR. Given how little we know about secondary calcite dynamics this seems like an appropriately conservative option (discussed more below in section 8).	Thanks for flagging these! We've substantially expanded the discussion of the durability of pedogenic carbonates and implications for how we should consider carbonate formation in the NFZ in CDR quantification (drawing analogy to consideration of DIC within the NFZ in CDR quantification). See response to previous targeted comments for specific areas of the text that have been expanded. Given that this isn't formally a crediting framework and the extended discussion of avoided emissions, we've opted to maintain the existing discussion of counterfactual LCA emissions.	NO CHANGE
General	Might have missed it, but how do you account for multiple doses of rock dust being applied to the same field over a time sequence, do we expect the already existing rock dust to reduce the atmospheric CO2 reactivity	Thank you for this question. We do not discuss this in the text but this will be a consideration for practitioners as they design their deployment.	NO CHANGE
General	Was hoping for a section on do no harm - and under what circumstances can we be sure that even where the net removal isn't super accurately quantified, we can safely assume it's net negative and environmental harm is marginal even at large scale application?	This is an interesting suggestion and we agree that it would be useful for this framework to be developed, however, it is outside the scope of this document.	NO CHANGE
General	In places where recommendations cannot be given, can Cascade say: "there is a scientific hypothesis but the recommendations haven't been given because the hypothesis	Thank you for this suggestion. We've included possible approaches where there is more than one option.	NO CHANGE

	needs to be validated/inter-compared by operational field data", instead of suggesting that "it's too early to tell".		
General	<p>Please note that the comments are rooted in practical experience from an operator's standpoint. They reflect the real-world challenges and constraints encountered in day-to-day operations, logistics and financial aspects of the work.</p> <p>I agree with the majority of the draft. It is encouraging to see that soil solid phase measurements are considered a sufficient approach by themselves for quantifying Carbon Dioxide Removal (CDR). However, I have some reservations regarding the extent of the foundation and how it might be off-putting for developers, particularly those with smaller team resources and less experience in the field. I am concerned that this approach might not allow the community to scale as quickly and as extensively as we need. There is a risk that the complexity of the requirements could potentially hinder the growth and expansion of the CDR community, especially among smaller or less experienced teams.</p> <p>Nevertheless, I understand that this high level of rigor is necessary while the data is still in its infancy. Establishing a strong foundation with stringent standards is crucial at this early stage to ensure the credibility and reliability of CDR measurements as the field continues to develop. However, we might need to consider making some trade-offs to allow for companies and operators to join the space and grow.</p>	<p>The Cascade team intends the Foundations document to be a technical reference for project developers who are seeking to quantify netCDR in ERW deployments; it is explicitly not setting generalized methodological requirements for quantification. More work and data from deployments across a range of environments and agronomic settings are required before the field can more reliably characterize the optimal implementation for quantification and coalesce around generalized requirements.</p> <p>Given the early stage of the pathway, the Cascade team places emphasis on the critical role of project developers in contributing to the field measurement, data collection and analytical work required to maximize learning. It is expected that tradeoffs factoring into operational, logistical and financial constraints will have to be made when conducting field measurements and analyses. We recommend transparency and documentation of the why's behind the different tradeoff choices, so that we can gain experience and evolve our learning of implementation practicalities along the way.</p>	NO CHANGE
General	Another big long-term question is whether or not quantification frameworks should account for changing climate, and if so, how. For example, increased temperatures = higher weathering rates but also decreased capacity for water to store CO2. Just spitballing here.	This is certainly an important question, but we have decided not to dive into this within the scope of this document. We look forward to future collaborations along these lines!	NO CHANGE
General	A flow chart or table would be useful to visualize the conclusions of Sections 4-9 (i.e. what is known/unknown at this	Thank you for this suggestion. We've added in two tables to Section 2 with high level summaries of the	CHANGE MADE

	time, in/out of scope etc)	NFZ and FFZ.	
General	consider long term instead of permanent in "permanent" vs. "transient" or at least define what time horizon is meant by each of these terms → glossary	Thank you for this suggestion. We've modified Section 2 to include a discussion of durability and other sections of the text as needed.	CHANGE MADE
General	The terms "tons" and "tonnes" are used interchangeably throughout the text. I would suggest sticking to one or the other.	Thank you for your thorough review and for catching this! The document has been updated to use tonnes throughout.	CHANGE MADE
General	Continued refinement on the document's purpose and focus is desired, especially in the later portions of the document that delve into specific geochemical sub-processes. This may be more convoluting than elucidating for non-technical buyers.	Thank you for this perspective. We've expanded Section 1 to discuss the scope of this document, added a Technical Summary to summarize the key parts for practitioners, and will be releasing other audience-specific pieces. This main document is intended for a technical audience.	CHANGE MADE
General	Throughout the text, I would use Enhanced Weathering model (i.e., EW model), instead of always referring to RTM. So we stay open to the possibility of this being any process-based or data-driven model.	Thank you for this feedback and suggestion. The discussion of Near Field Zone modeling has been broadened to be more inclusive of the diversity of potential modeling approaches (in addition to reactive transport modeling!).	CHANGE MADE