Enhanced Rock Weathering Methodology

Public Consultation feedback summary and responses

18 November 2025

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Public Consultation: Enhanced Rock Weathering methodology

Context

Puro.earth held a public consultation on a proposed update to its Enhanced Rock Weathering (ERW) methodology (edition 2025).

This initial public consultation was announced on Puro's homepage on the September, 8th 2025 and in Puro Newsletter on the same day. The time frame for the consultation spanned from September 8th, 2025 until September 28th, 2025.

The proposed draft with the title **Enhanced Rock Weathering** included thirteen (13) Sections. The feedback received included 144 comments from 12 organizations. This document summarises the feedback received during the public consultation, responses, and the revisions included by Puro.earth due to the comments.

We want to thank all participants for your time and contributions to helping us improve the Enhanced Rock Weathering methodology to better serve this growing ecosystem.

General Observations

1. The Public Consultation showed a significant engagement in the number of participants (12) and their comments (144).
2. Many valuable improvements and clarifications were incorporated into the methodology because of the public consultation process.
3. Even prior to this public consultation, this methodology update underwent scrutiny from a working group consisting of approximately 60 topic experts from industry and academia.

Detailed Comments and Responses

In the following tables, we will share the comments received and the responses provided by the Puro.earth Team. Comments are shared anonymously. The comments are grouped per Section in the consulted version of the **Puro Enhanced Rock Weathering** methodology.

All comment were addressed, and changes incorporated to the final draft. We want to thank all participants warmly for improving the rules and the integrity of Voluntary Carbon Markets (VCM) in general.

Public Consultation feedback and responses

Comment no.	Rule or section	Comment	Response	Action
1		Olivine or serpentine weathering requires dissoluton in liquid water followed by reaction with dissolved CO_2 to for the biicarbonate or carbonate. For minerals spread on land, where will the water come from? Rain will be intermittent. Water will pass through the mineral layer and into the water table. As water passes through or evaporates, pH and ion species will change and precipitates will form. How will this have an effect on the rate of weathering and how is this handled in the methodology?	The soil pore water or moisture enabling weathering in ERW will in fact come from rain in most cases, possibly supplemented by agricultural irrigation in some cases. The CO_2 removal supplier is free to enhance the permanence of feedstock exposure to moisture - and thereby likely weathering efficiency - by mixing in the feedstock with the soil. Enhancing weathering efficiency in this and other ways is primarily the suppliers' task and in their best interest. Additional factors affecting weathering efficiency are also descibed in section 1.4. The methodology does prescribe the accounting for carbon losses from the formation of carbonate precipitates (rule 6.3.7). Note that as weathering needs to be proven by ex-post measurements on site.	No change
2		For enhanced weathering in the ocean, the ocean is saturated in both Mg and Ca. It would seem to me that this would retard the rate of dissolution. Is this taken into account in the methodology?	Under this methodology, CDR from the direct addition of rock or minerals to the ocean or beaches is not eligible for crediting (rule 3.9.1). The weathering occurs in the soil and what arrives in the ocean are weathering products, including bicarbonate, Mg, and Ca, that are already dissolved. Carbon losses from inefficient (dissolved) carbon uptake by the receiving ocean water are covered by the requirement to account for ocean losses (rule 6.3.10).	No change
3		Lab studies in the literature showed that a carbonate crust can form on the surface of olivine particles that retards further dissolution. Is this taken into account?	Weathering needs to be proven by ex-post measurements on site. Addressing challenges in weathering efficiency, if they occur, is first and foremost for the CO_2 Removal Supplier to do. None of the eligible quantification approaches in this methodology would be expected to produce a false positive weathering signal in such a case.	No change
4	Table 1.1	I'd suggest changing the entry under limitations to "Potentially slow weathering kinetics…" as some weathering timelines can range from a few years to decades depending on feedstock reactivity, grain size, precipitation, temperature, soil pH, etc. As written is seems to suggest that weathering is always slow, which is not necessarily the case (though the uncertainty around weathering timelines is also a limitation compared to some other CDR pathways!)	Comment actually refers to table 1.2 due to a numbering error in the draft. Changed table 1.2 according to comment.	Minor change
5	p 17	Its worth mentioning here that non-carbonic acid weathering can still result in CDR by avoiding outgassing of biogenic CO_2 downstream, but that the spatial and temporal uncertainty around strong acid counterfactuals is large enough that it is treated as a loss term to be conservative.	Added footnote in section 1.6, part "non-carbonic acid weathering" in accordance with comment.	Minor change
6	Table 4.2 and 4.5.17	Under respiratory risks its worth noting that mosture content of rock dusts are a key determinent of risks, and moisture can be added to reduce risks if required.	Changed table 4.2 according to comment.	Minor change

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7	6.2.4	Why are we only using a 50-year period for counterfactual weathering if the resulting CORCs are for a period of >50 years? Shouldn't the timeframes of the CDR claims match those of the counterfactual?	The 50-year time horizon aligns with the treatment of this issue in other Puro.earth methodologies ("Carbonated Materials", and "Direct Air Capture and Ocean Storage") as well as the view of Puro.earth's scientific advisory board. Assessing the feedstock weathering counterfactual requires assumptions about the counterfactual fate of the material, including its storage conditions and reactivity, and making such assumptions on 100- or 1000-year time horizons is unrealistic. A 50-year time horizon was chosen for this conservative measure to address the near- and mid-term climate relevant counterfactual removals. Note further that the reaction rate of the material in the counterfactual weathering would not be uniform over time, and would be further significantly limited by e.g. formation of carbonate crusts in tailings piles, or landfilling of the material (sealed and capped), limiting access to water and air. It is thus expected that a significant majority of the potential counterfactual weathering would therefore have occurred by the 50 year mark.	No change
8	6.3.4	As noted above, strong acid losses are not necessarily permanent given that they likely avoid outgassing elsewhere in the system. Its worth at least noting this, while emphasizing the choice to treat them as a loss is conservative given uncertainties in spatial and temporal carbon cycle responses to strong acids over time.	Added explanatory footnote in rule 6.3.4 in accordance with comment, cross referencing similar note made in section 1.6.	Minor change
9	6.3.7 and 6.3.8	How are we ensuring that there is no secondary mineral formation in deeper soils below the measured NFZ; I know there is ongoing research here, and its worth at least nodding to that risk (and emerging science on it) even if its not practical to measure for commercial projects today.	A sentence was added in this regard to the introductory paragraph on secondary carbonate losses (section 1.6). The introductory paragraph on secondary silicates in the same section already contained a reference to this.	Minor change
10	7.2.2	The protocol should note that the return trip of transportation vehicles (e.g. trucks) should also be counted toward transportation emissions unless projects can demonstrate that those trucks are not returning empty (e.g. being utilized from the project site to transport goods elsewhere). This has been an issue in some past project LCAs where only the trip from the quarry to the farm is counted and not the return trip.	Thank your for your comment. We will specify the requirements for the calculation of transportation emissions in the corresponding rule.	Minor change
11	Table 9.2	While SOC, SIC, heavy metals, and agronomic measurements clearly need to be used to monitor for any negative outcomes, it is not clear to me why they do or do not require soil type, bulk soil major cations, bulk density, pH, and CEC across the three time points. Sure, these are basic steps to characterize a soil, but how are they used in the CORC calculation itself? Section 11 explains the different quantification approaches, which is what I think the "required" measurements should be based from.	While quantification of CDR is a cornerstone of the methodology, other aspects of the ERW activity need to be considered as well. Measurements of the soil properties defined in table 9.2, while mainly not directly used in quantification of CORCs (although e.g. bulk density is also used in certain quantification approaches), serve several other useful purposes. For example, they provide information about the heterogeneity of the soil to inform proper design of sampling and stratification (see e.g. rule 11.1.1 d). Furthermore, shifts in the value of these parameters over time over time can serve as a useful indicator of potential environmental risks or risks of reversal (e.g. if a significant drop in pH would be observed). As soils are not static, we believe that monitoring these key soil properties should be done throughout the project.	No change

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12	9.5.2	More a organization note, but its rather hard to interpret this part without constantly reference back and forth between this and section 11; would it make more sense to merge with section 11?	Rule 9.5.2 has been moved into Section 11.3 (as 11.3.1) and into the Quatification section for clarity. Explanatory text has been added to the start of 6.1 to introduce the concepts of quantification and validation approaches and their role within the methodology.	Minor change
13	9.5.2	I like the idea of requiring complementary quantification and validation approaches that measure two different phases, but the requirements described here do not incorporate the uncertainty in the validation value (it is just checking if the single validation value is within three s.d.'s of the quantification value). Since the validation value will likely have even more uncertainty than the quantification value, my first thought is to just compare distributions to make sure they represent the same population, but I would need to think more about that.	The corresponding validation requirement was changed to test for equality of means at the 99% confidence level. This will better incorporate the uncertainty in both quantification and validation measurements and make the test slightly stricter compared to the 3 standard deviations limit. (Note that rule 9.5.2 was moved to 11.3.1)	Minor change
14	10.5.3	While I appreciate the detail on uncertainty calculation and propegation, I'm left a bit unclear how this calculation would play out in practice. An example might be useful here; what would be the resulting F_c be for a project with CDR of 2 tons per hectare +/- 0.4 tons? Is F_c always effectively a % discount based on the ratio of CDR uncertainty to net CDR? How does this compare to other approaches (e.g. 1 standard deviation in Isometric, or 90% confidence in Foundations)?	The result is essentially the same as crediting at the lower bound of the two-sided 80% confidence interval (assuming normal distribution). In the example given, if 2 tonnes is the net CDR and 0.4 tonnes is the combined standard uncertainty, the uncertainty factor would be calculated as follows (ignoring for simplicity that in the methodology, uncertainty would be calculated based on total tonnes, not tonnes / ha): Expanded uncertainty: 1.28 * 0.4 t = 0.512 t Relative expanded uncertainty: 0.512 t / 2t = 25.6 % Fc: 100% - 25.6% = 74.4% CDR credited: 74.4% * 2t = 1.488 t Note that this is the same as 2 t - 1.28 * 0.4 t = 1.488t, where the factor 1.28 is the expansion coefficient to get 80% CI from the standard uncertainty. Therefore the approach in this methodology would be essentially the same as following the 80% CI recommendation in Foundations (note that Foundations section 5.6.4 mentions a two-sided 80% CI, not 90%) and more conservative than 1 SD in Isometric	No change
15	10.5.5	They are asking for holistic UQ here, which is fantastic.	Thank you for the comment.	No change
16	10.5.9-13	This is the meat of the calculation. They outline the three general methods for UQ and appear to ask for all of the necessary pieces, but it is still quite general, especially 10.5.12 and 10.5.13 — these two sections are essentially just asking for a description of the method without setting any requirements specific to those methods. I'm not sure it makes sense to set specific requirements here, but since it is vague, I think the delivery review process will need to be extremely thorough at this step.	Note that in rules 10.5.12 (Monte Carlo simulations) and 10.5.13 (Bayesian inference) the methodology does provide several references for technical guidance and implementation, including ISO standards, relevant academic research and software packages. We consider this quidance to be sufficient and do not consider it necessary for the methodology to be highly prescriptive on the technical implementation of these approaches. In our experience, problems with uncertainty quantification are rarely if ever associated with substandard technical implementation of the calculation approach, but rather with omitting relevant sources of uncertainty partly or in full (which the methodology does address in detail).	No change

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17	Tables 11.1 and 11.2	I can appreciate that they are trying to provide specific guidance on control areas and sampling densities, but, in my opinion, minimum sampling densities are entirely dependent on the target uncertainty (which they don't set), quantification approach, and spatial/temporal heterogeneity. I think minimum sampling density/frequency should be more closely tied to the UQ process (section 10).	The methodology does set minimum sampling density requirements, but does indeed not set an overall target or limit to uncertainty. Instead, the approach adopted in this methodology incentivises project developers to minimize uncertainty by requiring discounting of uncertainty. While it is true that e.g. the sample size is inversely proportional to the desired margin of error, the precise targets are for the project developer to set based on a project-specific consideration of the balance between operational costs and more credits issued. The methodology can of course make this link more explicit, and a note to the methodolgy was added to address this.	Minor change
18	11.1.3	It was a bit unclear from section 9 if the validation approach is only applied to the evaluation area or if its applied to a subset of the broader application area. Might be useful to clarify here.	A point has been added to rule 11.2.7 (which pertains to sampling density requirements) to clarify that the 50% sampling rate for the validation approach applies to the control, evaluation, and application areas.	Minor change
19	11.2.9	Given the importance of sampling the full NFZ for calculating loss terms (e.g. in section 6), what is the basis for the selection of these particular depths? For example, are we sure a NFZ defined as 5 cm below the tillage depth would preclude interactions with the atmosphere associated with cation storage in the deeper soils?	It is not possible to know in the generic case (if even in the specific case) at what depth interactions with the atmosphere cease to play a role (see Mills et al., 2023- Cascade Foundations). This requirement strikes a balance between being as conservative (deep) as possible to exclude atmospheric interaction below the NFZ on one side, and the practical difficulties of sampling below certain depths on the other side. Note that rule 11.2.9 requires the sampling depth to be at least 20 cm with an additional 5 cm buffer added to the tillage depth if the latter is >15 cm.	No change
20	Approach 1	Shouldn't loss of unweathered material be constrained in some way in this approach (vs Approach 2 where its accounted for)? Seems like at a minimum it should be a potential loss term. This term could be statistically constrained if large-scale deployments use both Approach 1 and Approach 2 where there is an available detrital tracer and compare the results, than extrapolate the resulting difference/uncertainties to fields where only Approach 1 is possible.	Failing to quantify the loss of unwethered material due to runoff or other (non-dissolution) removal is a potential weakness of Approach 1 (CAT_wm) relative to Approach 2 (CAT_ti). Approach 2 is the more robust approach, especially for removal suppliers who are able to perform post-application soil sampling (as required by CAT_wm). We also recognize that some feedstocks may not contain a consistent, reliable immobile trace element, and so allow the CAT_wm approach. All quantification approaches (including CAT_wm) require a supplementary validation approach. If substantial feedstock losses are occurring, for reasons other than weathering, the expectation is that the discrepancy will be discovered in the results of the validation approach.	No change
21	Approaches	Are there any plans to add a gasious phase option for validation? I know its one that some suppliers have used in deployments.	Gas phase monitoring was considered as a validation option during methodology development, but was ultimately cut from the public consultation draft following discussions with subject matter experts which highlighted higher uncertainty and numerous added practical difficulties (e.g. cost, MRV design) associated with gas phase measurements compared to solid or aqueous phase measurements. Gas phase measurements (and other quantification and/or validation options) will be reconsidered in future updates to the methodology.	No change

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22	Overall	I think this section could be condensed to a paragraph, requiring a peer-reviewed process-based model and employing one of the UQ approaches described earlier (10.5.12 or 10.5.13). As is, I think most of this section should be interpreted as describing some ways models can be used, rather than firmly establishing that this is how they should be used.	Section 12 is already fairly condensed, containing a brief overview of ERW models in the literature and requirements for process based models. We consider the overview to be important in provideing context and clarification for the types of models eligible in the methodology (as this has raised questions in past versions). Contrary to the comment, the requirements in this section are mainly binding requirements (shall) for modelling rather than descriptions of possibilities (can). The requirements around uncertainty treatment in the modeling section have been modified (improved) also in response to other public comments.	No change
23	Overall	Overall, they are asking for A LOT, which is great, but some of it seems unjustified (soil characterization, sampling densities, modeling approaches), while some of it is vague and will still require close scrutiny in the review process (UQ approaches).	Thank you for the comment. The concerns regarding unjustified requirements are addressed in the commenters separate comments regarding these points.	No change
24	4.5.1	SOC monitoring should be based on stocks, not concentrations- it is critical to also include bulk density measurements to assess this	Thank you for this astute comment, we changed the requirement for SOC monitoring from concentrations to stocks in rule 4.5.1.	Minor change
25	8.3.1	For slag, it is not appropriate to calculate leakage as the impact on carbon stocks associated with forest areas being converted into barren lands as a proxy for the expansion of mining operations. This is because slag is a residue, not the main product of mining.	Thank you for your comment. A clarification sentence has been added to the text.	Minor change
26	11.1.3	Regarding Table 11.1: The percentage of control site area and evaluation area are excessively high, which makes projects with only a few hectares unfeasible. The suggestion is to set the control area and evaluation area at 1%, regardless of the total number of hectares.	It is our opinion that the suggested universal 1% limit would result in too small absolute sizes of the areas in smaller strata. While a fixed value could in principle be utilized, we feel that the current scaling approach strikes a reasonable balance between robustness of quantification and operationalizability. Note further that aside from initial proof-of-concept sites, projects with only a few hectares would very likely be unfeasible in any case for both project developers and credit issuers alike due to costs associated with project management and credit issuance.	No change
27	9.5.2 (i) and 11.3	The suggestion in this item is to allow the use of methods not listed in tables 6.1 and 11.3. We are currently developing a project, with the participation of several professionals and academics in the field of ERW, that is creating a new method for measuring weathering. The quantification and validation approaches for carbon measurements are based on publicly available scientific references. However, our own quantification and validation method, although technically and scientifically grounded, has not yet been made public, as it is under patent application and remains confidential. We can present it to the Puro team in a call while it is not yet disclosed. We would like to be able to apply this method in our project.	Quantification and validation approaches based on novel methods can be added to the methodology via (minor) updates (note that there can be several minor updates between the larger updates such as the present major update). Such minor updates can be initiated at any point in time and with relatively short turn-around time following evaluation of the Puro.earth science team and scientific advisory board of new methods that are brought to our attention. However, for transparency, such new methods would need to be published (e.g., in pre-print), and preferably passed scientific peer review.	No change
28	6.2.4 - 6.2.12	Solid outline of assessment needs for baseline/counterfactual feedstock weathering, could be improved and made more prescriptive by adding a recommended list of parameters/datapoints that could be included to detail counterfactual weathering, in table like you have done for other sections of the methodology.(e.g XRD mineralogy data, mining documentation or geophysical measurements showing feedstock deposition/storage geometry, model parameters etc...).	Requirements for feedstock characterization (incl. XRD mineralogy etc.) are already presented in table 3.1. While the requirements regarding baseline feedstock weathering prescribe the methods to assess the baseline, they do not prescribe additional specific measurements. While it is true that in some cases, additional measurements might be necessary (e.g. the geophysical measurements mentioned), such information might also be obtained via other sources (e.g. data records form the mining company). Note also that a more detailed listing of all the necessary pieces of evidence will be made available to suppliers in the supporting audit guidance documents.	No change

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29	10.1	Typo in sentence: "For the purposes of this methodology, the uncertainties themselves can be also divided into two broad broad classes based on the source and nature of the uncertainties:" - broad is repeated twice.	Thank you, we removed the typo.	Minor change
30	11.1.4	Could consider adding a short comment to emphasize the need to prioritise operationally simple and realistic deployment designs/sampling strategies, which are more compatible with everyday agronomic field activities (e.g split field over randomized plot segmentation of strata) . I believe this would reinforce intention for this methodology to be used for crediting of commercial ERW deployments rather than research trials.	Thank you for the suggstion. This is a commercial crediting and verification standard, and we allow and expect that suppliers will align their sampling strategies with their agronomic field activities. However, we also want to encourage scientific and statistical rigor while keeping the rules and requirements brief, concise, and clear. While we agree with the intention of this comment, adding this language in the rule would in our opinion introduce more ambuguity than clarifcation.	No change
31	p. 9	Carbon can also be safely sequestered on long timescales (thousands to tens of thousands of years) in groundwater systems	Thank you for your comment. Your suggestion has been added to the text.	Minor change
32	p. 11	Awkward phrasing in this sentence, consider revising: "The way of the ERW products (bicarbonate and associated cations) to their ocean storage site begins after their release into soil pore water, dissolution products begin moving through the terrestrial environment."	The text has been modified to improve readability.	Minor change
33	p. 13	In the "role of pH in ERW" section, it may helpful to note that you can "get back" any degassed CO2 once weathering products (e.g., Ca) reach a surface water system, which typically have higher pH than soil waters.	Thank you for your comment. Your suggestion has been added to the text.	Minor change
34	p. 14	There are some interesting results from Burke et al. (2025) showing that the relationship between grain size and surface area is not straightforward; very large grains (2-10 mm) can have the same SSA as much finer particles. Potentially worth citing here.	Thank you for your comment. Your suggestion has been incorporated within the text.	Minor change
35	p. 14	In the "Appropriate application settings" section, it may be helpful to discuss settings that promote easier or more effective MRV. For solid phase mass balance approaches, this will be sandier soils, where baseline concentrations of cations and immobile tracers will be lower compared to heavier soils. For aqueous approaches, this will be areas with well-constrained hydrology.	A note was added to the subsection mentioning that soil properties can further impact the ease and effectiveness of the overall MRV process	Minor change
36	p. 18	Under the "Non-carbonic acid weathering" section, it would be helpful to note that strong acid weathering can eventually lead to CDR after, e.g., denitrification of nitrate in the soil or in downstream systems, even if this is not considered in carbon crediting.	Thank you for your comment. This concern has been already addressed in response to comment #8.	Minor change
37	p. 20	In "Downstream river loss in rivers and oceans" I generally agree with the assertion that abiotic carbonate formation will not be a major loss pathway for ERW, however this could be a significant factor in certain locations (e.g., if outlet flows into a warm, hypersaline area e.g., Red Sea, Bahamian Platform where abiotic carbonates are common)	Added footnote in p. 20 pointing out observed abiotic carbonate precipitation in certain localities.	Minor change

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38	p. 24	Perhaps this is covered later, but do you handle the durability of carbonate minerals vs. DIC? Is there a way to demonstrate 1000 year durability of the carbonate minerals? This is complicated in practice (if dissolution of carbonates leads to bicarbonate production, it's a gain, but if it leads to carbonic acid production or is done by strong acids--at any point over the next 1,000 years, then it's a loss) should be spelled out somewhere.	Several sources indicate that mineral carbonation is a highly permanent form of carbon sequestration. For example, the IPCC special report on carbon capture and storage (https://www.ipcc.ch/site/assets/uploads/2018/03/srccs_wholereport-1.pdf) mentions regarding mineral carbonation (p. 330) that "...the fraction of carbon dioxide stored through mineral carbonation that is retained after 1000 years is virtually certain to be 100%. As a consequence, the need for monitoring the disposal sites will be limited in the case of mineral carbonation." This and other references were added to introduction section 1.3 in response to this comment. Additionally, unexpected reversals of CO2 seqestration (e.g. due to extreme soil acidification following an industrial accident) are covered via the mechanism in the Puro Standard General Rules concerning CO2 reversals.	Minor change
39	p. 46	In addition to the proposed monitoring protocol, we suggest that practitioners carry out conservative mass balance calculations that account for PTE concentrations in soils, PTE concentrations in feedstocks, application rate, and soil bulk density to calculate the maximum PTE load if all feedstock is dissolved and no heavy metals leave the soil. These should be compared against local regulations. This framework ensures that even worst-case scenario deployments will not exceed regulatory thresholds.	Doing such a mass balance was implicitly required by rules asking to adhere to legislated PTE thresholds in soil after feedstock applications. We now explicitly require it in a new subrule 3.6.2e.	Minor change
40	4.5.1	Since SOC is a risk where developers are being asked to prove the absence of loss, suggest requiring an analysis of statistical power that shows the probability of type II error is lower than 0.2 for a 5% change in SOC. Otherwise developers can just rely on doing sparse sampling to show no significant change in SOC.	The requirement 4.5.1. was modified to include more details about the sampling and analysis of results (incl. statistical power).	Minor change
41	Table 6.1	Wondering if Puro is considering watershed-scale measurements or gas phase measurements as an MRV approach? Can't remember if this came up in the Working Groups but I know that those are two options that are popping up in discussions and the literature. The other one that has also been mentioned in conversations is agronomic inversion and while there isn't ERW-specific literature related to this, I'd be curious to know if Puro would consider this a validation method or a sub-method of the solid phase quantification methods.	At present, neither watershed-scale measurements, agronomic inversion, nor gas phase measurements are included as quantification / validation approaches. Gas phase measurements were considered as a validation option during methodology development, but this option was ultimately dropped, following conversations with subject matter experts, partly due to the associated operational challenges (e.g. elevated cost, uncertainty). While there are some recent research relating to watershed-scale measurements in the context of ERW (e.g. https://doi.org/10.70212/cdrxiv.2025394.v1), we do not yet consider it robust enough to be included in the methodology (and similarly for agronomic inversion). However, Puro Standard methodologies are regularly assessed and updated, and new MRV approaches may be added in future updates as more research and field trials become available.	No change

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42	6.1.3	Table 6.1 Suhrhoff et al. 2025 (https://cdrxiv.org/preprint/390) preprint contains updated guidance on mobile/immobile element mass balance. It has not been through peer review at present, but is perhaps worthwhile including as an alternative source of guidance on the calculations.	ERW quantification approaches are an area of rapidly evolving research, development, and scholarship. Faced with this evolving landscape, the standard strives to balance rigorous evaluation and the need for clear requirements with the desire to permit a variety of approaches and accommodate innovations and refinements of evaluation practices. This flexible approach is designed to allow and encourage practice refinements, such as the SOMBA models. We expect these practices to evolve as they are deployed and their performance is evaluated within commercial and research contexts. Future versions of the Puro ERW Methodology will be updated to incorporate these learnings and emerging best practices. We recognize that to incorporate such refinements, minor updates or revisions to the quantification equations detailed in section 11.3. might be necessary. To this effect, rule 11.3.1 b was added to allow such flexibility.	Minor change
43	6.3.1	This is just a note that won't change the methodology but there is continued conversation about if strong-acid weathering needs to be considered as a loss (or under what conditions it might need to be considered a loss). I don't think this should change the protocol for this go-round but something for the Puro team to keep an eye on	Thank you for the comment. In reply to other similar comments (#8 and #36), an explanatory note was added in the methodology to explain the nuances of losses due to strong acid weathering.	Minor change
44	6.3.1	For losses of secondary silicates in the NFZ, wouldn't this be integrated into all of the different measurement options? I think the issues for secondary silicate formation are where there is possibility for formation outside of the measurement zone	We agree and updated rule 6.3.8 accordingly. The public consultation draft originally considered some edge cases where, for some reason, the full extent of the NFZ might not have been sampled, but this has been removed for clarity as rule 11.2.9 requires sampling depth to equal the depth of the NFZ. Additionally, rule 6.3.8 was amended to include recommendation to avoid sites with a high propensity for secondary mineral formation in the FFZ.	Minor change
45	6.3.4	In a.iii. Would amend to "especially fertilizer application and sulfur deposition"	Thank you, we added sulfur deposition as another specific case that needs to be paid attention to when seeking an exception from anions anion measurements for non-carbonic acid loss quantification.	Minor change
46	6.3.8	In-line with the comment where this loss is first mentioned, would expect that the loss is not 0 at times but also that it would be integrated as part of NFZ measurements. Instead, I think this assumption of a 0 loss is important for lower soil/FFZ secondary silicate formation but there should be some guidelines around when a 0 loss could be assumed.	We agree that this loss factor would be implicitly included in the eligible quantification methods (see also comment #44). Additionally, rule 6.3.8 was changed to add a recommendation to avoid sites with high propensity for secondary silicate mineral precipitation, especially slow-draining soils.	Minor change
47	6.3.9	In c, I do worry that only 2 annual measurements may not be enough to capture the variability.	We require these annual measurements at opposing seasons (e.g., summer/winter, wet/dry). This is a minimum requirement that should still cover the extremes of biology and temperature (and thus carbonate system and CO2 solubility effects) that we are interested in. Note further that river sampling already represents a significant additional burden for project developers, and adding to these requirements must be carefully weighed against the expected gains.	No change

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48	6.3.9	In d, I'm not sure that 5% is appropriately conservative due to the locations where >15% of CO2 loss was sometimes modeled. Would be better to run a model in this case I would think for degassing.	We consider 5% to be a conservative value here based on the Zhang et al. (2025) paper because in Zhang et al.'s model of 100 river segments across the US, only 1 river segment showed a CO2 loss of approximately 15%, the remaining 99 showed a CO2 loss of less than 5%. We would argue that a conservative estimate need not always be based on the most extreme outlier in a large (model) data set.	No change
49	6.3.2	Typo: R is the set of loss terms in the FFZ, not NFZ.	Typo corrected, thank you for spotting it!	Minor change
50	6.3.1	Currently, estimates of losses in FFZ deep soils are not constrainable, but we suggest adding in a deep soil loss term for future flexibility in the methodology. The F[i] of this term could be specified to be 0% for current crediting purposes, but may change in the future.	Thank you for bringing up this important point. We will insert such a term, for example via a minor update, once the science and technology have caught up to a point where - as you say - assessing such losses will become feasible.	No change
51	6.3.4	Subrule (c): In our view, default value of soil pCO2 of 1000 ppm is too conservative, suggest using the lower bound of 4000 ppm suggested in the Lal citation in Dietzen and Rosing.	The relevant section in (Dietzen and Rosing, 2023) states: "Though it is often stated that soil gas pCO2 ranges between 10 and 100 times present atmospheric pCO2 (4000–40,000 µatm) (Lal, 2017), if pCO2 is not known, we recommend conservatively assuming a pCO2 of 1000 µatm." As subrule c) explicitly refers to the method of Dietzen and Rosing, we find it appropriate to follow the recommendation of the authors.	No change
52	6.3.4	While not perfect, a mass balance approach to strong acid weathering quantification is also possible, and is likely preferable for developers who are using solid-phase measurements as the primary quant method. Aqueous measurements are likely to be too sparse to reliablly estimate the strong acid discount. The mass balance would consist of acidifying fertiliers (N and P not charge-balanced by base cations) and estimates of wet deposition of SO4, minus plant uptake and assuming soil storage is at steady state.	We already have several options for estimating non-carbonic acid loss, the main ones being the Dietzen&Rosing model and the quantification of soil water anions. The former presents a very accessible options for suppliers focusing on soil samples (rather than soil water samples) and so we do not think it is necessary to offer this alternative, which - as you admit - is not ideal. It relies on estimates of wet deposition of sulfur (which are likely to be imprecise) and requires a new category of analyses (N and P In plant biomass). Therefore, we decided to not include your suggested approach, but thank you regardless!	No change
53	6.3.4	Subrule (a){ii) Two time points are insufficient for quantifying anion flux from a soil for determining a strong acid discount. Given the extremely strong seasonality of nitrate fluxes in agricultural systems is widely recognized (Ebeling et al. 2021 GCB among many others) only requiring two time measurements opens up this loss term to "gaming the system" in a big way by sampling in late summer/fall or after periods of very high precip.	We agree and added the requirement to sample at least every three months for these anion measurements. Given that porewater measurements need to be done anyways for validation or quantification approach and that there is a modeling alternative to determine the non-carbonic acid loss, this should not put too much strain on suppliers.	Minor change
54	6.3.9	Subrule (d)(3) In our view, the carbonate precipitation calculation should not assume that calcite precipitation brings the river to equilibrium conditions (SI=0), but instead it should calculate precipitation needed to bring SI down to 1 (consistent with (d)(2)) and then add the 2.5% from (d)(2).	Thanks for pointing out this inconsistency with (d)(s), this makes a lot of sense. We changed subrule (d)(3) in the way you suggested.	Minor change
55	Overall	This section is outside the domain of our expertise and we refrain from comment on it.	Thank you for the comment	No change
56	Overall	This section is outside the domain of our expertise and we refrain from comment on it.	Thank you for the comment	No change
57	p. 85	Ideally, site characterization will also include information that will inform the practitioners sampling strategy. For example, high immobile tracer concentrations in the soil may increase the sampling density (or application rate) needed to capture a signal or that another approach should be used. This is partially addressed in 9.4.4.d but could be clarified.	Thank you for bringing this up. We added this as a recommendation to Table 9.2.	Minor change

Comment no.	Rule or section	Comment	Response	Action
58	p. 85	This subsection is not clear to me; consider revising	Unfortunately the comment lack detail as to how the subsection is unclear. Minor changes in wording have been done in section 9.4 and table 9.2 to improve clarity.	Minor change
59	9.4.4	Table 9.2: Soil taxonomy is likely to vary substantially, even over smaller fields. Recommend specifying at what spatial scale this needs to be measured.	We inserted a link to rule 11.1.2 where the exact requirements are specified.	Minor change
60	9.4.4	Given that variable charge is likely going to be a significant changing parameter in ERW applications, measuring buffer CEC is not optimal. Ideally, CEC and base saturation would be measured at the pH of the soil.	Thank you for pointing out this important detail. We changed the requirement (Table 9.2) to specify measurement of CEC at soil pH.	Minor change
61	p. 89	We note that this method for assessing agreement between MRV approaches is quite lenient, although this may be appropriate as MRV technologies mature. On one hand, having a validation threshold based on narrower SD band (1 or 2 SD) could incentivize under-measurement of primary quantification techniques so that error bars are large and validation approaches are very likely to show agreement. On the other hand, tighter requirements may encourage more sampling density/innovation for validation techniques.	The corresponding validation requirement was changed to test for equality of means at the 99% confidence level. This will better incorporate the uncertainty in both quantification and validation measurements and make the test slightly stricter compared to the 3 standard deviations limit. (Note that rule 9.5.2 was moved to 11.3.1)	Minor change
62	10.5.3	Formula 10.1 would only be valid if all terms under the root are uncorrrelated. That may be a valid assumption, but I just want to surface that that is an assumption being made.	Added footnote to rule 10.5.3 a to specifically point the approximate nature of the equation.	Minor change
63	10.5.10	Subrule (a)(ii) In cases where some measurements of parameters have been taken sparsely, the estimates of correlation between parameters will be poorly constrained, and this could substantially impact the propagation of errors, either more or less conservative.	We agree that it can in practice be very challenging to constraing covariance, and this is explicitly pointed out in the methodology (see e.g. footnotes to rules 10.5.10 a and 10.5.11 d). Furthermore, there is already a footnote in rule 10.5.10 pointing out that practically speaking, covariance terms should be included in the computation only if they have been estimated from sufficient data.	No change
64	10.5.11	Subrule (c) We see that you address correlation below (d), but we urge that you prominently state that Formulas 10.6 through 10.9 only apply in the case of uncorrelated variables.	This is already explicitly stated in footnote to subrule d, but we have added a shorter note to this effect more prominently to subrule a	Minor change
65	10.5.11	Subrule (d) Given the challenges you outline in this paragraph, we recommend not using analytical error propagation formulas in almost all cases, instead recommending the methods in 10.5.12 and 10.5.13	Propagation of error is very commonly utilized, and it is prominently included e.g. in the JCGM 100:2008 standard (and the corresponding ISO/IEC Guide 98 series) and 2006 and 2019 IPCC Guidelines for National Greenhouse Gas Inventories. Each of the methods outlined in the methodology possess distinct strengths and weaknesses, and while Monte Carlo and Bayesian inference can be very robust, they are in our view not automatically superior. Further, the issues outlined in subrule d relate also more generally to the method of collecting data and quantifying covariance rather than just the propagation method itself (i.e. if correlations can be accurately quantified, they can rather straightforwardly be included in a propagation formula as well, but if not, no method can coax out covariances that are not present in the data).	No change
66	10.5.12	The key principle for these Monte Carlo methods is that variables that were measured together (.e.g. from the same sample) are resampled together. This is how these models can implicitly account for corrleation in the variables. For example, base cation and immoblile tracer concentrations for solid phase samples should never be resampled independently from their respective distributions, but instead jointly.	We agree with the comment. Footnote to rule 10.5.12 discussing resampling was changed to explicitly point this out	Minor change

Comment no.	Rule or section	Comment	Response	Action
67	Table 11.1	It seems more appropriate to have control/evaluation area be a continuous function of stratum size to avoid big gaps in monitoring with small changes in project area (e.g. 4,999 ha would have ~2x the control/eval area of 5,001 ha as is). I would also advocate for multiple control plots per stratum, particularly when strata are large (>1000 ha) As for the comment regarding the amount of control plots, having more plots (e.g. two smaller plots vs. one larger plot) does not automatically equate to better control data. What is more important is that the plot(s) constituting the control site capture the variability present in the stratum. There are already requirements in section 11.1 to ensure this.	We have replaced the tier-based approach with a continuous logarithmically decreasing function of stratum size with constant cutoff values for very small (<10 ha) and very large (>10000 ha) strata. Adopting this function leads to minor changes in the percetages presented in table 11.1.	Minor change
68	11.1.1	Subrule (a) Allowances should be made for edge cases where a deployment straddles two Köppen-Geiger zones but the underlying climate across the stratum is substantially similar.	The categorical nature of Köppen-Geiger zones requires a line to be drawn somewhere. These zones are quite wide-ranging in most environments (i.e., outside steep alpine valleys for example) and allowing strata to spend two of these zones would be almost equivalent to not setting any constraints regarding climate at all. Note further that this requirement does not prevent a project spanning several climate zones, but merely requires stratification respecting the climate zones.	No change
69	11.1.1	These guidelines on stratification are highly prescriptive, and it's worthwhile considering why stratification guidelines need to exist. The reasons for project developers to want to stratify is to minimize variance and increase creditable CDR as a result. This doesn't really matter for Puro, though; if developers stratify poorly, they will simply have larger error bounds and be able to credit less, but it's not the job of a methodology to help developers maximize tons. The interest that Puro has in ensuring good stratification is to ensure that the control plots are truly representative of the stratum. To this end, if developers have multiple control plots that are randomly located within a stratum, it should not matter what variables they're stratifying on, from a standpoint of methodlogical rigor. If they stratify on the wrong variables, they simply won't be able to credit or credit at a low number. To this end, one tentative thought is that Puro shouldn't mandate what variables to stratify on, but instead just insist on multiple randomly-placed control plots within a stratum. Then, it is up to the project developer to weigh if the potential benefits of stratification outweigh the costs of more control plots needed to implement the stratification. As stated, the number of variables listed to stratify on is very long (esp. with the large number of ag practices to consider), and this is likely to result in the division of fields into an unworkable number of strata.	The main reason for requiring stratification is to ensure representativeness of control sites, not to maximise CDR output. While it is in theory true that enough randomly selected control sited would ensure representativeness even without stratification, it is in practice not possible to ensure that control sites have been truly randomly chosen. Further, it is likely that at least in some cases, practical operational realities can clash with random selection as the vast majority of ERW sites are agricultural plots, and not purely research plots. For example, access to some application plots might be prevented during spreading time for various reasons (e.g. closed gate, delays in feedstock supply , machine breakdown, etc.), but such sites might still be utilized as control plots later. Keeping more detailed stratification requirements in the methodology ensures that control sites are and remain representative of the application sites, and improves the robustness of the carbon accounting. However, we do recognize that some of the current requirements migt be difficult to operationalize, and result in overstratification. We have modified the requirements regarding the mandatory stratification parameters (e.g. soil type similarity) to include more flexibility and project-specific approaches while still keeping the overall stratification framework intact.	Major change
70	11.1.1	Subrule (a) Soils are often mapped in units that contain complexes and associations of different soils that can even span the highest levels of soil taxonomy. Additionally, soils are also insufficiently mapped to accurately predict taxonomy at the sub-field scale. While incorporating soil taxonomy into stratification seems important, the workability of this rule in practice is unclear.	This is a concern we share and we updated our rules around this in response to your comment. Specifically, the newly designed rule 11.1.2 now offers a fallback option to the less complex USDA soil texture classification.	Minor change
71	p. 119	"The minimum absolute amount of samples taken per evaluation area"; this is per hectare, as shown in Eq 11.1, correct? If so, update the language here.	No, this is not per hectare, this is in fact the absolute minimum sample number required per areal unit (evaluation area, control site) no matter how small your field site is.	No change

Comment no.	Rule or section	Comment	Response	Action
72	p. 121	Refers to setting the NFZ by soil horizons, which is 11.2.12 and not 11.2.13	Thank you, we fixed this.	Minor change
73	p. 121	Sampling to the midpoint of horizon A will encourage practitioners to select an effectively shallower NFZ. This risks poor characterization of fluxes such as secondary silicate formation and cation sorption, which can be large NFZ loss terms. This approach may also lead to difficulty comparing results between treatment and control sites.	We strengthened the rules for sampling depth when sampling along soil horizons (rule 11.2.9 b).	Minor change
74	11.2	Strongly agree with the focus on the primacy of zero-tension lysimeters, and really appreciate in general the discussion surrounding what pools of water each lysimetry approach captures. In fine textured soils, though, flow divergence around ZTLs is a very substantial problem that should perhaps be noted.	Thank you for this input, we added a remark about flow-divergence as a problem for ZTLs.	Minor change
75	11.2.10	Aqueous sampling at the bottom of the NFZ will necessarily underestimate the Loss[plant] term because plants of course have roots below the NFZ. We feel comfortable with this approach as-stated, but want to draw out that point.	Thank you for pointing this out. Whie it is true that sampling above the bottom of the rooting zone might impact the determination of plant losses, it is our opinion that these impacts would be overall very minor (as plant losses are small to begin with). We further think that it would not be operationally feasible (nor balanced compared to the potential gains) to require plant biomass sampling below the NFZ.	No change
76	11.2.12	It is often very difficult to determine horizonation from isolated small diameter samples, and will field technicians be trained in soil science to a degree adequate to support this approach?	The sampling along soil horizons is entirely optional and we would therefore expect that suppliers only choose it if their field technicians are up to the task.	No change
77	p. 125	Would suggest "Approach 2 - CAT(Ti)" is renamed to be agnostic to the immobile tracer used. I would recommend "CAT(it)" for "immobile tracer".	Any nomenclature system has to balance precision and accuracy as well as clarity and recognition. CAT_it was considered as the name for this approach, as you suggest. Because TiCAT is already a recognizable term for soil quantification approaches using immobile tracers, we felt CAT_ti would more clearly identify the approach. The approach itself is agnostic to the trace elements that are used. It is not restricted, by text or intention, to the use of titanium as an immobile trace element.	No change
78	p. 128	"The loss pathways Lossadsorb, Losscarbonate, and Losssilicate are implicitly accounted for by this approach if soil sample integrate over the entire depth of the NFZ and are fully digested, but need to be corrected for if only part of the vertical extent of the NFZ is sampled." If this is accurate, then what is the meaning of the NFZ? How would this extrapolation occur? Any NFZ extrapolation scheme needs to be clearly laid out with evidence from the literature.	Thanks for highlighting this inconsistency. We removed this implicit exception for sampling only part of the NFZ.	Minor change
79	p. 132	It should be noted that care should be taken to avoid re-speciation of DIC after sample collection; temperature should be kept low and contact with headspace should be minimized.	A good point that was added to the method description for approach 4b.	Minor change
80	p. 133	In variable table: why does n not scale with speciation of DIC? It seems like n should scale from 0-2 based on relative proportions of carbonic acid, bicarbonate, and carbonate.	You are correct, the factor n representing average charge of the DIC should apply to both approaches 4a and 4b and it should scale continuously between 0 and 2 rather than in increments of dominant DIC species as wrongly suggested before. This was changed in the variable table and the associated equations.	Minor change

Comment no.	Rule or section	Comment	Response	Action
81	p. 136	I disagree with the basis behind Approach 6. Annual variations in precipitation could lead to yearly cycles of carbonate precipitation and dissolution as reactants are concentrated in evaporating fluids. Furthermore, it is very difficult to prove that pH will not substantially decrease on timescales of centuries (the timescale of durability noted for this protocol), particularly given changing climate and farming practices. These concerns are counterbalanced by the possibility of sequestering more carbon as SIC is dissolved to DIC, which in most cases will be the dominant pathway, but tracking the fate of this DIC would require one of the approaches listed above (e.g., porewater sampling).	Rule 11.3.8(b) sets a pH limit for this approach that makes it eligible at pH less than 6.5. This is a safeguard agains carbonate mineral dissolution by non-carbonic acids (which only becomes a problem at pH6.3 and below, Dietzen&Rosing, 2023)). This pH limit covers the concern about seasonals wet-dry cycles because any seasonal dissolution of carbonate minerals at pH>6.5 would most likely be by carbonic acid. It also covers the concern over long time scales to a reasonable extent. Soil carbonates are typically stable for millennia and more (see added references in section 1.3). A strong environmental change would be required for non-carbonic acid weathering of these new carbonate minerals to become a problem. Such unforeseen CO2 losses after CORC issuance are covered by Puro's standard rules as reversal.	No change
82	p.130	Constraining ET, and which portion of ET comes from the NFZ, is the major issue here. Suggest a fuller exploration of means of calculatingf the water balance, including the simple, conservative estimate that PET=AET, variably saturated flow models (e.g. HYDRUS 1D) that have root distributions represented, and box model approaches (e.g. as employed in SMEW). The treatment of the water balance will likely dramatically impact the creditable CDR.	The requirements for how to calculate the soil water flux (from precipitation, evaporation, and runoff) have now been specified in much more detail. Ways to estimate precipitation, evaporation, and runoff are specified in rule 11.3.6 as well as information box 1.	Major change
83	p.132	The quantification of organic alkalinity is very much not straightforward, and if you genuinely want people to address this issue, more guidance is likely required than included here.	More guidance is now provided in the introduction to approach 4a as well as rule 11.3.7c. The protocol by Kerr et al. (2023) is briefly described and explicitly required as guidance for organic alkalinity correction.	Minor change
84	p. 135	The kinetics of sorption on IEX resins are very fast, and the likelihood of non-quantitative recovery because of fast flow rates is probably very low, except in the very sandiest of soils.	We agree and deleted that comment. about an underestimation of resin-based signals at high soil water volumes.	Minor change
85	p. 135	The volume of influence of the resins (where ions from water not actually flowing through the resins accumulate on the resins via diffusion) should be constrained, either by requiring bag sizes to be large (to minimize SA:V) or (better) having impermeable sidewalls and a defined vertical capture area (i.e. a passive flux meter, like REDACTED is doing.)	Requirement to constrain the volume of influence of the resin by utilizing impermeable sidewalls was added to rule 11.3.7 a iii.	Minor change
86	p. 135	Resins should be emplaced below the zone of feedstock application, such that dissolution during extraction of the resins is not an issue. Further, I do not believe the citations provided here say that the resins themselves dissolve feedstock, as is stated, but rather that feedstock is dissolved when strong acids are used to extract the resins. So the functional groups of exchange should not impact mineral dissolution.	Thanks for adding this nuance. We agree and adjusted how this potential problem with resin sensors is discussed accordingly. We also added the requirement that resins be placed out of contact with the feedstock (at the bottom of the NFZ).	Minor change
87	p. 135	About the representativeness of the fluxes estimated from resins: The dryness or wetness of the soil should reflect the leaching flux, so it is difficult to understand how to dry soils would underestimate the cation flux because soils are too dry. Also, there are considerations surrounding textural discontinuities and breaks in capillarity with resins which might be expanded on when mentioning "the hydrology of the resins". Final point: The robustness of various resins to dessication is one of the most important things that needs to be trialed empirically. Various resins have different performance characteristics under dessication. Because of this, acceptance of "resins" should be on a case-by-case basis for a specific resin/SOP.	We adjusted the discussion of the effect of dryness on resins. We also mentioned "dryness" as one of the environmental parameters that need to be taken into consideration when selecting an appropriate resin type (rule 11.3.7a).	Minor change

Comment no.	Rule or section	Comment	Response	Action
88	p. 143	Soil moisture dynamics appear both in required and recommended (should) parts of the requirements; I suggest that soil moisture dynamics are required due to their primary importance in determining weathering rates and transport of solutes.	Thank you, the redundant mention was removed from the list with recommended ("should") items.	Minor change
89	p. 145	What are the guard rails for extrapolation from measurements made in the evaluation area of a stratum? "shall be validated against data measured in-field" is not sufficiently specific -- what is the nature of this validation? Agreement within some uncertainty threshold? I would argue for capping Cstored derived from models at the amount of Cstored demonstrated by empirical data as a safeguard against model manipulation.	Requirements for extrapolation are given in rule 12.2.1 f. In addition to these requirements, the fact that the modeling uncertainty needs to be quantified and added to the uncertainty discount factor serves as a guardrail against model manipulation (i.e. when modeled results deviate a lot from the measured results, the uncertainty increases and amount of credits decreases). For the comment regarding validation, additional clarification was added to the rule regarding the type of data and the purpose of validation. The purpose of this validation is to quantify the mean differences between modelled and measured values and utilize them further in the quantification of total modelling uncertainty. For the comment regarding capping of Cstored, we do not agree that this approach would be very effective: If the proposal is to implement such capping for all plots modeled, it would defeat the purpose of modeling (as you'd need to measure the Cstored a priori, and the only possible outcome of modeling would be to lower this number, so why use models?). If the proposal is to impose this capping for the evaluation site (only), there is no guarantee that such limit would hold for extapolated sites (i.e. if one fits a model with data from evaluation sites x, y, and z such that C_stored, modeled (x) < C_stored, measured (x), etc., there is no guarantee that for an extrapolated site a, the bound C_stored, modeled (a) < C_stored, measured (a)) strictly holds. In fact, this capping might very well lead to worse results and increased uncertainty, as it is imposing an artificial constraint to the fitting.	Minor change
90	12.2.1	"supplementing empirical data with modeling results can reduce the required sampling density and/or frequency while achieving statistical significance". It is not at all clear what "supplementing" measurements with model results to achieve statistical significance would employ. Both forms of data are fundamentally different, and combining them in a single statistical test doesn't make sense on its face.	We agree that this sentence is somewhat vague. The intention is to allow modelling as a tool for extapolation from measurements. The premise is that while all suppliers must respect the minimum sampling requirements in the methodology, suppliers utilizing calibrated models might achive a desired level of accuracy (note that accuracy affects the number of credits issued) with fewer measurement than what would otherwise be necessary. The sentence cited in the comments has been removed for clarity and replaced with a footnote better expressing the intention stated above.	Minor change
91	12.2.1	Subrule (c)(x): Dynamic modeling of SOC stocks is well outside the scope of capabilities of all the models listed, and would require an ecosystem model and/or ecohydrologic model that would add very substantial complexity to modeling activities. It is not clear if this is possible or desirable.	Please note that modeling SOC is only recommended, not required. Given that the SMEW model has a simple component modeling SOC at least as a simple function of inputs (litter fall, soil amendments) and decomposition (biological activity), we decided to keep that recommendation, which might help progress ERW models with regards to SOC if applied by CO2 removal suppliers.	No change

Comment no.	Rule or section	Comment	Response	Action
92	12.2.1	Subrule (c)(i) and (c)(iv): It's hard to imagine how one could model dissolution and precipitation (i) without modeling fluid supersaturation (iv) so it's unclear why these are different list items.	Thank you, we fixed this implicit redundancy by removing "fluid supersaturation" from the list of recommendations.	Minor change
93	12.2.1	Similar to the comment on subrule (c)(x), explicitly modeling soil respiration is not a strength of these models, and it is likely that, for many of these models, imposition of a soil pCO2 value, where not required by these models, would be preferable to dynamic modeling of resipratory fluxes and subsequent diffusion.	Thank you, we added this alternative to modeling respiration to rule 12.2.1c.	Minor change
94	12.2.1	Subrule (e)(ii) I am reading this as saying that measurements in the evaluation area can be foregone in favor of modeling the evaluation area during different seasons. i.e. you can skip winter measurements if you do summer measurements. If this is the correct interpretation, I do not think it advisable to allow this sort of extrapolation. You're biasing your validation data by season.	This is not the intention of this rule, which states explicitly that seasonal extrapolation is only allowed for models that "have been calibrated and validated with empirical data reflecting those seasonal differences". We have added a clarifying remark in the requirement to better reflect the intended meaning.	Minor change
95	12.2.1	Subrule (f). This uncertainty quantification should be specified in stronger terms. There are going to be many model parameters that are tuned or imposed and not taken from field data. How should uncertainty in these parameters be quantified, and their net effect on Cstored be estimated? Additionally, while the protocol talks a lot about uncertainty, there's no discussion of how to deal with bias in the model, which is an altogether different problem. Additionally, how to deal with structural error (problems hard-coded into the model, incorrect form of process representation) is not considered in an actionable way here.	Firstly note that for the purposes of the methodology, bias (i.e. systematic errors) is a type of uncertainty as explained in section 10.1. Treatment of ERW model uncertainties is treated in more detail in the cross-referenced rule 10.5.6 b. This includes uncertainties assiciated both with the input parameters as well as with any incorrect / imperfect treatment of modeled phenomena. Both of these need to be quantified. As mentioned in the rule, uncertainty can be quantified e.g. via a Monte Carlo analysis and via comparison of modeled signals against measured signals. Note also that further relevant clarifying requirements regarding uncertainty quantification were added to subrule e (see also comment #89). Finally, we note that there exist also other approaches that have been utilized in the scientific literature to quantify uncertainty in similar models (e.g. biogeochemical reactive transport models), including e.g. a more sophisticated uncertainty analysis utilizing Bayesian inference (https://www.doi.org/10.1029/2018WR023589). Thus, while the quantification of uncertainty is already required, the precise framework can be chosen by the project developer (from the ones listed in rule 10.5.9 b), aligning with the treatment of other sources of uncertainty.	Minor change
96	12.2.1	Subrule (h): Model optimization is the identification of the parameter set(s) that minimize some objective function (e.g. the negative log likelihood), and is pretty different from a sensitivity analysis. The big question is, what are you proposing to do with the sensitivity analysis? Require more data collection on sensitive parameters? Have some sort of requirement to run models in all corners of the sensitive parameter hypervolume and credit at 1 sigma of that distribution?	In this case, the word optimization was used in the wider sense of "making something as effective as possible" instead of the narrower sense of mathematical optimization. However, to avoid clarity, we have changed this term to "model inspection" instead. The purpose of the model inspection stage is to establish that the model is internally robust and perfoming as intended. This intent was clarified in the rule text.	Minor change

Comment no.	Rule or section	Comment	Response	Action
97	12.2.1	Subrule (i)(iii) and (i)(iv): All fine and good that differences in modeled and observed results should be reported, but what level of divergence is acceptable? Without criteria for model acceptance or rejection, these requirements are somewhat toothless. Somebody could build a model that is biased high toward CDR estimation and reports low uncertainty due to structural limitations of the model and then say "Oh well, yeah the modeled results come back 20% higher than measured but it is what it is."	The approach adopted in this methodology is that instead of explicit bounds for uncertainty, the quantified total uncertainty is used to discount the amount of CO2 removal credits issued. This serves as an incentive to reduce uncertainty as much as possible. The methodology already requires quantification and accounting for the modelling uncertainties (rule 12.2.1 g), which would include systematic biases such as the one mentioned by the commenter. Therefore, such a situation would lead to a comparable decrease in the amount of credits issued. We have clarified the treatment of uncertainties associated with modelling as a response to related comments #89 and #95 (see also response to comment #99).	Minor change
98	12.2.1	Subrule (j): "Should" would need to be replaced with "must be open-source" and "must be available in public repositories" or developers will certainly have 0% compliance with this rule.	The requirements for data transparency are outside the scope of the methodology. They are set in the Puro Standard General Rules (section 5.1 Public reports), and are the same for all methodologies. While we encourage full data transparency, there is a valid need for project developers to protect certain sensitive information and IP. Currently, suppliers may request to redact confidential or personal information subject to approval from Puro.earth. Note finally that there is also a requiremet for models to be based on peer-reviewed articles (subrule a. i.), which would be publicly available.	No change
99	general comment	There are essentially no actual criteria for rejecting a model due to non-performance / bias. There are many things specified that the model "should" do but no measure of how well. This is a major shortcoming of this section, and stands in stark contrast to the quantitative rigor of all the other sections. While I understand the need for flexibility in the protocol to evaluate a ton of models, this falls WELL too far on the side of too flexible. There need to be performance targets that the models meet relative to the data, or we will be in a world where anything goes with models. Anybody can write a model that is biased toward overprediction of CDR and underprediction of uncertainty. Model validation and criteria for rejection are particularly important because many of the models on the list in the table haven't even been validated academically against field data, but only batch reactor, lab column or mesocosm data.	The uncertainty quantification and discounting included in this methodology serves as an implicit limit to how well does a model need to perform. This treatment is fundamentally very similar compared to the treatment of the quantification and validation approaches, and ensures that appropriate safeguards are in place to prevent deliberately biasing models towards overprediction of CDR. Note the quantification of uncertainty needs to include also the accuracy component (i.e. from comparisons against measured data). Hence, attempts to purposefully bias a model would result in increased uncertainty and diminished number of credits. We have clarified the treatment of uncertainties associated with modelling as a response to related comments #89, #95 (see also response to comment #97).	Minor change
100	general comment	Who is actually going to be capable of evaluating model design choices and vouching for the soundness of modeling approaches? Neither verifiers nor registries have this sort of expertise.	CDR methodologies are becoming more and more scientificly complex and sophisticated, with ERW models only one of many examples. We are aware of this challenge to our operational effectiveness in evaluating crediting claims. Specifically for the evaluation of ERW models, we feel well-prepared with in-house expertise. Similarly, it is also necessary to carefully identify select qualified auditors with the relevant expertise, which we agree is somewhat challenging but quite possible.	No change
101	12.2.1	Subrule (c)(iii) Suggest removing "soil moisture dynamics and/or". The flux out of the NFZ is the key thing to model. It is unclear if soil moisture needs to be explicitly modeled... many of these models on the list do not explicitly model time-varying soil moisture, which in the case of RTMs would require a full implementation of Richards' equation. I note that this recommendation is opposite that of my colleague [redacted].	The purpose of this "and/or" choice between soil moisture and fluxes is to leave the choice open between reactive transport models (using fluxes) and ecohydrological-biogeochemical models like SMEW (using soil moisture). We therefore did not change this rule.	No change

Comment no.	Rule or section	Comment	Response	Action
102	general	What about sulfide minerals while assessing environmental safety?	The role of sulfide minerals in non-carbonic acid weathering, including requirements for screening and evaluation, has been added to the Loss_acid quantification rules, including revisions to rule 6.3.4. Sulfide minerals are not specifically listed in the environmental risk section because they are not included in soil and feedstock regulatory requirements (e.g., EU, USDA). However, any materials that pose health or ecosystem risks would need to be identified, monitored, and reported in accordance with the environmental and social risk assessment safeguards described in section 4.4.	Minor change
103	9.5.2	In point a) for iv) - Original text - Approach A6 (SICsoil) shall not be utilized as either the quantification approach or the validation approach. Rather, it is an optional approach that may be used independently of the other approaches to quantify additional Cstored in the form of secondary carbonates (see rule 11.3.7). Sugested text. - Approach A6 (SIC-Soil) may be used in addition to Approaches A1–A5 (Table 6.1) solely to quantify additional C_stored as secondary carbonates (see Rule 11.3.7). Where applied, net removals from A6 may be summed with removals quantified under A1–A5. Rationale - Currently it is not clear how the removals via A6 can be added to removals from either of A1-A5 mentioned in Table 6.1. Additionally it would be helpful if it can be clarified that uncertainty is calculated separately in A6 from any other approach A1-A5.	Thank you, we added a modified version of your suggested sentence "Where applied, net removals from A6 may be summed with removals quantified under A1–A5." to subrule a-vi of rule 11.3.1 (formerly 9.5.2). We note that this point is mainly made in subrule e of rule 11.3.7 but we added it also to rule 11.3.1 (formerly 9.5.2) for clarity.	Minor change

Comment no.	Rule or section	Comment	Response	Action
104	Sampling: Soil sampling tools	Original Text: Typical soil sampling tools include augers, tubes or cores and simple shovels. Augers are one of the most commonly used tools, available in various designs such as bucket, spiral, and Edelman augers, each suited for different soil textures. Soil cores provide intact soil columns that preserve natural stratification and bulk density. Piston corers can be useful for deeper profiles, as they minimize soil compression and disturbance during extraction. Tube samplers and split-tube samplers are often employed for undisturbed soil sampling, providing high-quality cores for laboratory analysis. Shovels and trowels, while convenient for quick soil sampling, are not precise enough for ERW projects, which require accurate control over sampling depth. In the context of this document, soil samples are expected to be cores that are later homogenized to integrate the soil between the soil surface and the bottom of the soil core sample. Such soil core samples can be used for several Cstored quantification approaches (section 11.2): CATTi , CATWM, SICsoil. Sampling Method Clarification: 1. Does the methodology explicitly prohibit the use of spade or trench sampling methods for ERW CDR quantification? 2. If a consistent depth can be maintained, can spade or trench slice sampling still be used as a representative method? 3. In soils with high coarse fragment content (stones/gravel), where core extraction is physically difficult, are there any recognized alternative sampling methods or protocols that can be applied?	Spade or trench sampling are not explicitly prohibited as the cited text is not a rule. Thus, as long as they are "appropriate and consistent" (rule 11.2.1) and requirements like stable sampling depth can be adhered to, there are no restrictions to alternative sampling tools like spades.	No change

Comment no.	Rule or section	Comment	Response	Action
105	11.3.7	In point a) Origial text- SICsoil shall only be eligible in soils with pH levels of at least pH 6.5 (Dietzen & Rosing, 2023) prior to the first feedstock application of a given Crediting Period (as determined by pre-application soil characterization, see rule 9.4.4. Suggested text: "This methodology allows crediting for secondary carbonate formation in soils where net SIC increases can be accurately measured and verified through approved analytical methods." Rationale: The current pH ≥6.5 threshold may compromise on scientifically valid carbon sequestration processes. Some researchers demonstrate that even acidic (pH 4.3–6.0) soil conditions significantly enhances carbonate recrystallisation under elevated CO2 conditions (Ferdush & Paul, 2021). In naturally acidic soils (pH <6), after basalt deployment the pH levels do not suddenly increase but evolve gradually through pedogenic processes. If SIC increases are observed in these conditions despite naturally acidic rainwater inputs (pH <5.6 due to atmospheric CO2), this represents genuine carbon sequestration through active biogeochemical processes that overcome natural acidifying inputs. Reference: Ferdush, J., & Paul, V. (2021). A review on the possible factors influencing soil inorganic carbon under elevated CO2. CATENA, 204, 105434. https://doi.org/10.1016/j.catena.2021.105434	When it comes to the pH 6.5 threshold for SIC_soil eligibility, our concern is less that it is not possible to form carbonates below pH 6.5 but more that the risk of reversal is significantly increased in acidic conditions, and small environmental changes (perhaps merely from a ceasing of the ERW activity) could cause non-carbonic acid weathering of the initially accumulated carbonate minerals down the line. Regarding the referenced review paper: it cites two studies supporting the claim that carbonates can form in low-pH soil but it is not clear to us that these studies in fact support the claim that Ferdush & Paul make. The first study (Cailleau et al. 2005) studies an anomalous region with a soil type that normally (!) has low pH but in the studied locale in fact has ~pH 8.0 due to a local anomaly caused by bacterial activity; so this does not support carbonate formation at low pH. The second study (Zhao et al. 2020) reports an experiment where desert soil is subjected to different CO2 atmospheric concentrations with the higher pCO2 favoring carbonate formation more than the lower pCO2. Their experiment was designed to answer the questions of whether microbial activity and pCO2 influence carbonate formation, not whether low pH is conducive to it.	No change
106	Table 3.1	Loss on ignition should be mandatory; Calcite can be a significant component in volcanic rocks. Yet, if only present in very low percentage, x-ray diffraction will not identify this phase. Since calcite dissolution rate is orders of magnitute above volcanic rock, the weathering signal even from minor calcite could dominate the overall weathering signal and lead to a significant overestimation of weathering rates, if wrongly attributed to silicate weathering. Especially if direct measuremnts of bicarbonate in solution are used, calcite content is important.	Thank you for the comment. Loss on ignition (LOI) indeed is not required but recommended to be measured in feedstock but that is only because carbon content of feedstock is required to be measured via dry combustion. We are aware of the importance of measuring carbon in feedstock referenced by the commenter but cover this issue by requiring a different measurement, which should be more accurate in determining feedstock carbon content given that LOI measures carbon content indirectly via weight loss, which can also stem from water or clay loss.	No change
107	Data transparency	Puro should require ERW Suppliers to accept higher levels of data transparency. The current level of disclosure required by Puro does not allow for ERW credits issued on its registry to be audited by members of the ERW / CDR community. This lack of transparency, specially t this critical stage for the Enhanced Weathering field, interferes with the community’s ability to ensure that Puro’s high standards are being uniformly enforced, which will only serve to undermine the public’s ability to trust ERW credits issued on Puro’s registry	The requirements for data transparency are outside the scope of the methodology. They are set in the Puro Standard General Rules (section 5.1 Public reports), and are the same for all methodologies. Puro utilizes independent and verified 3rd body auditors to evaluate compliance and quantify carbon removal. Audit statements, Audit reports, registrations, and project descriptions are already publicly available. The CO2 Removal Supplier may request to redact confidential or personal information subject to approval from Puro.earth.	No change

Comment no.	Rule or section	Comment	Response	Action
108	Overall	The language could be more precise: For example, the description of reversal risk vs. carbon losses should be more precisely differentiated (p41). If a reversal risk is known a priori, then it seems like it would be a potential carbon loss by the definitions in the text. The ifference is that you can know a carbon loss will occur and calculate it in your predictions, but reversals may or may not occur. This is not clearly explained. Secondly, the use of concentrations is often used instead of stocks (e.g. p45-46).	Thank you for your comment. We have tried to be as clear as possible throughout the text, actively incorporating suggesions from different stakeholders. For reversal risks, section 4.1 states "Reversals are therefore considered as unaccounted-for events ", which matches your definitional aspect of "may or may not occur". Section 4.3 further defines reversals as " reversals refer to previously unknown or unanticipated re-emissions that occur after the issuance of CORCs, and are addressed via the procedures described in the Puro Standard General Rules [section 6.7]". On the second point, we acknowledge the imprecise language you pointed out and clarified in various places where SOC stocks rather than SOC concentrations are being referred to.	Minor change
109	p. 45	“Use SOC concentration analysis values to ensure that SOC concentrations in soils do not decrease drastically” — “dramatically” is very vague, and it’s so difficult to measure meaningful changes in SOC concentrations. This should also be clearly stated to be relative to a BAU plot. Further, should this be stocks, rather than concentrations? Bulk density may change due to rock application and tillage.	This is now clearly stated with concrete statistical language and requiring comparison to control (= BAU) plots. We also changed this rule from talking about SOC concentrations to SOC stocks.	Minor change
110	9.4.3 p. 85	9.4.3 is confusingly worded, and a reminder of the definitions of t0 would facilitate understanding (e.g. what is the difference between pre-application and t0 sampling). A timeline of acceptable and possibly also unacceptable timelines would be helpful.	We decided that this rule was not needed since soil characterization should happen well in advance of monitoring and we do not want to actively encourage re-use of possibly too-old data for t0 measurements. There is no rule actively prohibiting the re-use of such data if they are taken sufficiently recently and, starting at the second monitoring period, rule 9.4.2 still allows the use of t1-measurements from the previous monitoring period as soil characterization data.	Minor change
111	p. 87	Clarity and consistency regarding requirements and recommendations - e.g. Are the methods in this table recommendations or requirements (e.g. EPA 3050B for metals)? Plant cations are recommended, while SIC is required.	Note that the table heading states that these are "required and recommended measurements with recommended analytical methods for soil characterization".	
112	p. 89	Note that iii implies that more heterogeneous regions are more likely to pass verification.	Thank you for your comment. Rule 9.5.2 has been moved to 11.3.1, and the content in subrule b.iii about the confidence level has been modified.	Minor change
113	p. 89	“The estimation shall be based on measurement of the major cation concentration in the weathering feedstock, e.g., by full acid digestion followed by either XRF or ICP-OES analysis (table 3.1)” — XRF measures the solids, so you don’t do a full acid digestion before it. ICP- OES or ICP-MS should be acceptable.	This section has been revised and clarified. The CDR_max description (in 1.4) has been updated indicating the acid digestion is for ICP-OES and not XRF.	Minor change

Comment no.	Rule or section	Comment	Response	Action
114	6.2.1-6	Please clarify the language regarding treatment of emissions in the baseline scenario. We agree that, to prevent crediting avoided emissions, the baseline sequestration amount can never be negative. That said, completely excluding baseline scenario emissions is overly conservative. Assume for instance that the baseline scenario involves lime application. In the absence of the project, lime would be quarried, transported, and applied to the field. Any resulting removals of CO2 from the atmosphere would not be able to occur without emissions associated with the lime production/transport also occurring. Therefore, the project shouldn't be penalized by having to deduct the potential gross removals from liming without adjusting for the corresponding emissions. Baseline scenario emissions should be allowed to reduce the magnitude of the baseline sequestration amount. However, if the counterfactual emissions exceed counterfactual removals, then the baseline sequestration amount should be set to zero to prevent any crediting of avoided emissions. What this means in practice: Quantification of net CDR should involve comparison of the net removals in the project scenario (NFZ removals - NFZ losses - FFZ losses - project emissions) and the net removals in the baseline scenario.	Thank you for requesting clarification on this point. We have added subrule 7.3.2a to capture the impact of ceased emissions from liming operations replaced by ERW application in the determination of the in-field baseline component.	Minor change
115	7.1.5	While we understand the rationale for excluding monitoring-related emissions to avoid discouraging robust MRV, those emissions are real and directly attributable to the project. In the absence of project activities, neither trips to the field for sampling nor lab processing would occur. Excluding these emissions sets a problematic precedent: it suggests that emissions can be ignored if they serve a “good” purpose, which undercuts the goal of transparent and comprehensive carbon accounting. As long as minimum robustness standards are clearly defined in the protocol, including emissions from monitoring won't disincentivize suppliers from meeting those requirements. If a project is cutting corners on monitoring to improve its carbon balance, that's a signal of poor project quality—not an issue of fairness in emissions accounting. Following similar logic, however, we agree that emissions from corporate activities, such as travel for R&D purposes, should not be included in project emissions quantification since the activities are not directly resulting from the project.	Thank you for your comment. We include monitoring activities as part of the LCI scope.	Minor change
116	7.1.5 Note 1	Unclear what sources are explicitly required to be monitored (section 3 does not appear to contain this information).	Thank you for your comment. We have eliminated this note.	Minor change

Comment no.	Rule or section	Comment	Response	Action
117	7.1.9	This requirement allows little flexibility to use conservative estimates when data is unavailable, despite the reality that third-party suppliers may lack the capability or willingness to provide certain information. In particular, many mining and logistics suppliers have no commercial reasons (outside of the CDR project requirements) to track data required for emissions quantification, making these requirements burdensome. Strictly requiring 100% data collection from suppliers can significantly limit the pool of eligible suppliers and strain critical relationships, especially in the near-term when most potential partners are unfamiliar with CDR projects and registries and when project developers are operating at smaller scales and have less purchasing power. For this reason, in cases when primary data is not available, we urge you to consider accepting transparent, conservative estimates paired with higher uncertainty deductions and audit scrutiny.	Thank you for your comment. We will incorporate your suggestion.	Minor change
118	Table 7.1	What are "waste treatment" and "stack emissions" under transport?	Thank you for your comment. We have eliminated these items that are not relevant to the transport stage.	Minor change
119	8.1.1	Do the market reports need to be created by a third party, or are supplier-generated analyses sufficient?	Thank you for requesting clarification on this point. We will include the requirement for 3rd party market report to mitigate potential conflict of interests associated with the development of the report.	Minor change
120	8.2.1	What are the requirements for analyses to be considered "local"?	Thank you for requesting clarification on this point. We have specified local as "national" level.	Minor change
121	8.3.1(a)	Do the tiers represent registry preference?	Thank you for requesting clarification on this point. The preference is for using the highest tier available when using data sources (tier 3 over tier 2 or tier 1) as presented in the IPCC guidelines. We have made a new proposal for leakage calculation. So, the clarification language is included in sub-rule c)	Minor change
122	8.3.1(b)	Where does the 5000 t/ha/year come from? How geographically- and/or process-specific is that value?	Thank you for requesting clarification on this point. We are proposing another approach to quantifying/estimating leakage associated with land use change which avoids such an absolute number. The approach estimates the attributable share of LUC impacts to the project based on quantity of alkaline material applied to the project, and the total permitted alkaline material and mine area. This avoids	Major change
123	6.3.4	Could you provide clarification on why quantifying the primary sources of strong acids to the system (i.e. ammonium-based fertilizers and reduced sulfur species) is not an accepted approach for constraining the non-carbonic acid weathering loss term?	We already have several options for estimating non-carbonic acid loss, the main ones being the Dietzen&Rosing model and the quantification of soil water anions. The suggested approach would be based on somewhat unreliable data like fertilizer application rates and wet deposition of sulfur. Also, it is unclear how this approach would account for natural acidity already present in the soil. We therefore decided to not add this kind of approach for the non-carbonic acid loss quantification.	No change

Comment no.	Rule or section	Comment	Response	Action
124	6.3.7	For quantification approaches where the Loss_carbonate correction is required (i.e. weathering is constrained by solid phase mass balance approaches, but not over the entire NFZ), are SIC measurements to directly quantify potential stock changes always required, or can project proponents provide justification for a negligible probability of persistent soil carbonate formation in the deployment environment (e.g. given net infiltration rates, weathering rates, and/or carbonate mineral saturation states in soil porewater)?	Using carbonate mineral saturation state (SI_calcite) could be accepted as evidence for no carbonate loss seems not very practical because if one does find SI_calcite > 0, one has to then measure SIC anyways (or do some modeling) because SI_calcite can only give you are yes/no answer about carbonate precipitation, not a quantitative one. We also note that carbonate losses are rarely applicable and only should be applicable to soil based quantificaiton approaches (see table 6.1). For suppliers using soil-based methods, it should be much easier to measure SIC compared to measuring DIC and/or pH from soil water, which would be necessary.	No change
125	11,1,3	States that "Both control and the evaluation area (a more densely monitored subset of the application site) shall be monitored for Cstored using the same quantification and validation approaches." It is notoriously difficult to quantify Cstored using solid-phase (or even aqueous-phase) quantification approaches for determining removals from liming.	Properly quantifying the CO2 removals in both application and control sites is critical for any ERW project developer. We have revised the quantification approaches to include a factor (f) to account for feedstock carbon concentrations. This allows suppliers to account for the carbon content in lime applied to control fields (and avoid overestimating control field removals) while using the same quantification approach in their application site.	Minor change
126	11.2.7c	Is this meant to require that an even number of samples be taken across control and deployment areas in a 2-plot approach (+ comparable for 3-plot), or simply that at least 'minimum sample size/2' samples must be taken in the control area? If the former, why are equal sample numbers required? This assumes that the control area has the same variance as the entire rest of the homogeneous stratum.	Rule 11.2.7 and Table 11.2 have been revised for clarity. The indicated minimum number of samples applies to the full homogeneous strata. At least 1/3rd of that number must be performed in the control area. The remainder is the minimum number of samples required in the application area under a 2-plot approach.	Minor change
127	11.2.12	Why is the minimum NFZ depth shallowed to ~10cm in the case that NFZ depth is indexed on soil horizons?	We strengthened the rules for sampling depth even when sampling along soil horizons (rule 11.2.9 c).	Minor change
128	11.3	For approach A1 (Cat WM) potential CDR is calculated through changes in major cation stocks alone. This quantification approach only holds if we can reasonably justify that a) we have a well-constrained application rate and b) feedstock has not been physically lost from the monitored soil depth interval through erosion or migration into deeper soil layers. I don't see any requirements around constraining or justifying negligible feedstock loss through physical transport processes - is none required? What if loss due to physical erosion is a non-negligible concern in the deployment context? Or potential vertical mixing of material? Also, what are acceptable approaches to quantifying the application rate? Must application rate be constrained through post-application samples, or can it be determined from as-applied spreading maps? What validation of spreading maps is required? In general, more guidance and requirements around properly sampling in systems where bulk density can vary temporally (e.g. tilled systems) and implications for quantification would be appreciated. Including appropriate application of equivalent soil mass methods, etc.	Failing to quantify the loss of unwethered material due to runoff or other (non-dissolution) removal is indeed a potential weakness of Approach 1 (CAT_wm) relative to Approach 2 (CAT_ti) (however, this would require significant export of material from the application sites in a relatively short amount of time, which is unlikely to occur undetected). Approach 2 is the more robust approach, especially for removal suppliers who are able to perform post-application soil sampling (as required by CAT_wm). However, we also recognize that some feedstocks may not contain a consistent, reliable immobile trace element, and so allow the CAT_wm approach. All quantification approaches (including CAT_wm) require a supplementary validation approach. If substantial feedstock losses are occurring, for reasons other than weathering, the expectation is that the discrepancy will be discovered in the results of the validation approach. We further specified the type of evidence that will be required to quantify feedstock application rate.	Minor change

Comment no.	Rule or section	Comment	Response	Action
129	6.3.9	In general, we would like more clarification on this section to better understand what exactly is necessary at both site and project scale to align with Puro crediting standards. The way this methodology is currently written is contradictory in nature, on the one hand suggesting that publicly-available river chemistry datasets may be sufficient, and on the other hand suggesting a minimum additional effort to characterize stream chemistry through bi-annual sampling campaigns immediately downstream of all deployments. We encourage Puro to explicitly define the minimum stream order at which chemistry samples should be taken. Furthermore, we submit that sampling campaigns of “River water immediately (or as close as possible) downstream of the field site” at all sites is unnecessary to derisk the loss in the FFZ if basin and catchment lithology is well-understood (e.g. does not include areas dominated by carbonate lithology), robust regional stream chemistry datasets already exist, and projects are already conservatively estimating downstream loss.	Thank you for reviewing this complex rule. Datasets vs measurements: we changed our wording in subrule a that may have previously suggested falsely that something needs to be measured even in the presence of valid datasets (from "appropriate measurements" to "empirical data"). Subrule b then defines what this data can be (measurements or historical data) Stream order to be sampled: subrule a describes unambiguously where samples need to be taken, leaving room (in point subrule a-2) for local idiosyncracies (e.g., fields draining into different streams), which is why we do not simply prescribe sampling the first-order river. Carbonate lithology: the comment made us aware of an inconsistency in this requirement. However, we changed the rule into the opposite direction, removing the permission to not sample highest-order rivers if carbonate lithology is absent. This is for two reasons: 1) proving the absence of carbonate lithology anywhere (!) upstream of a highest-order river (think Mississippi draining half of North America) is very hard to do and practically impossible to audit, 2) absence of carbonate lithology still leaves the risk of CO_2 evasion from rivers due to low pH.	Minor change
130	6.3.9	Additionally, the stipulation that river water quality data must be less than or equal to 10 years old to conduct stream chemistry assessments significantly shrinks the spatial coverage of stream chemistry modeling without meaningfully improving the accuracy of hydrochemical values, especially in regions where data is sparse to begin with. For instance, if this language is maintained, the GloRiCh dataset, which globally contains over one million stream chemistry data points sampled between 1952 & 2010, will be fully excluded from assessments. We suggest that station-specific river chemistry should be computed by flow-weighted averaging of samples, and any samples taken post-1980, where robust daily streamflow data are globally available, should be eligible for inclusion in this averaging.	River chemistry is susceptible to (anthropogenic) changes on short time scales (e.g., new sewage treatment/graywater outflows, reservoir construction, changes in agricultural practices such as fertilizers, water use, land use changes). We therefore do not consider it appropriate for data older than 10 years to be used to assess SI_calcite and pH even if this allows for the use of comprehensive datasets like the one referenced. As far as flow-weighted sampling goes, the measurements required for pH and SI_calcite do not depend on discharge and so measurements based on simple water samples (or sensor measurements) are sufficient to make the required assessments.	No change
131	6.3.9	Section d) 2) states that a) “If pH >6.5 and SI calcite <1.0 consistently at all downstream measurement points and seasons, the supplier may either use a 2.5% estimate for the value of Lossrivers (Zhang et al., 2025) or apply an appropriate model to estimate CO_2 evasion due to carbonate system equilibration in downstream river systems (e.g., Zhang et al. 2025) and use this estimate to determine the value of Lossrivers.” b) “Appropriate modeling”—in the style of Zhang et al. 2025– suggests that, even if all downstream measurements are in line with the above stipulated values, loss may be higher than 2.5%. We recommend that Puro either a) raise the “no-model” default loss to a more conservative floor, or b) require (and reword) site-specific explicit modeling with clear data and QA/QC standards.	There seems to be a slight misunderstanding about the intention behind this rule: we consider this scenario (at least moderate pH, at least moderately low SI_calcite) to be a low-risk scenario, where a smaller conservative estimate of 2.5% is appropriate. We still permit the use of an appropriate model to further reduce this if the local conditions are such.	No change

Comment no.	Rule or section	Comment	Response	Action
132	6.3.10	The suggestions that project developers using silicate feedstocks shall either calculate ocean loss by using the Renforth & Henderson, 2017 uptake efficiency equation, run through CO2sys using relevant ocean chemistry data, or simply assume a conservative loss of 10% based Kanzaki et al., 2023 “if [the] supplier cannot assess the carbon uptake efficiency equation based on local oceanographic data” are incongruous. The Renforth & Henderson equation yields losses in the 15-20% range based on observed ocean chemistry values through datasets like OCEANSODA, a deduction already 5-10% greater than the proposed Kanzaki deduction. This substantial discrepancy will incentivize suppliers to default to the more favorable deduction, while plausibly denying the feasibility of using reliable ocean chemistry data–even though such data exist. In general, the community should collaborate to align on a standard ocean loss deduction methodology.	Thank you for this comment which made us rethink this issue again. After some more discussions with internal and external experts, we decided to increase the conservative estimate that can be used for this loss term to 20% for the reasons given.	Minor change
133	Overall	How uncertainty is treated in the document, including statistical testing between groups and final calculations of should be harmonized. Currently, 6.2.13. specifies that null hypothesis significance testing should be used to demonstrate that project removals are greater than counterfactual removals. Later, in 10.5.3., the document specifies that project developers should propagate uncertainty across terms into a total uncertainty value, which is then used to generate a factor of conservativeness, called an “uncertainty deduction” in other protocols. These approaches are inconsistent.	We do not agree that the requirement for testing for statistical significance between control and application sites is in conflict with the CORC uncertainty quantification and discount. The former is a sense-check testing for viability of the results whereas the latter quantifies the uncertainty around the CORCs and its components.	No change
134	4.5	Effect of ERW on soil organic carbon (SOC): ERW may have a significant effect on SOC. It is at this point unclear whether a long-term stabilization and increase of SOC is occurring with ERW across different climatic conditions and crucially, whether there are significant short-term losses of SOC as a direct response to the pH shift associated to ERW. As such, we would recommend to elevate the last sentence of the paragraph “Soils with very high SOC concentrations (e.g., >5 wt% SOC, (Mills et al., 2024)) should be deprioritized for ERW project development in favor of less organic-rich soils to decrease the risk of SOC loss offsetting CDR.” to a binding requirement/rule (4.5.x) and phrase it in a more binding fashion (e.g. Soils with SOC concentrations >5 wt% are not eligible for certification under this methodology.)	We inserted a rule that excludes soils with >5 wt% SOC prior to the first crediting period (rule 3.9.1 c).	Major change
135	4.5.6	Boundary Ambiguity: The rule does not specify whether the mean concentration is to be calculated at the level of a single project site/field, at a stratified sampling unit (e.g., soil type, management zone), or across the entire project area or even a wider jurisdiction. This matters because the spatial aggregation affects both the interpretation and risk of overlooking localized contamination.	Rule 4.5.6 has been revised and clarified. It does apply to the full field site. The rules has been adjusted to account for outliers and to set exceedance limits.	Minor change
136	4.5.6	Risk of Masking Outliers: Using only the mean may hide the presence of significant outliers (e.g., one field heavily contaminated while others are clean). From a safety and environmental integrity standpoint, such localized contamination should not be "averaged away."	Rule 4.5.6 has been revised and clarified to address the risk of PTE contamination being averaged away as part of the overall mean.	Minor change

Comment no.	Rule or section	Comment	Response	Action
137	4.5.6	Suggested wording: "The CO_2 Removal Supplier shall demonstrate with appropriate statistical tests at >95% confidence level that the concentration of each relevant heavy metal is below its applicable limit value. The statistical assessment shall be applied at the field-level (or approved stratified sampling units, if applicable), and shall include safeguards against localized exceedances. Specifically, the mean concentration must be below the limit value with 95% confidence, and no single sampling unit may exceed the limit value by more than [X% or defined tolerance]."	Thank you for the suggestion. Rule 4.5.6 has been revised along these lines to address the risk of PTE contamination being averaged away as part of the overall mean and to refine the definition of acceptable tolerance levels.	Minor change
138	Table 4.3	Table 4.3: We would recommend to add an additional source to the table description. We believe that the EU regulation 2019/1009 on liming contaminants (page 51 of attached document) is closer to the use case of ERW as sewage sludge application in agriculture. The limit values outlined in Table 4.3 all seem to be within this EU regulation, except for Nickel, which is 60% higher. Any particular reason as to why?	EU regulation 2019/1009 on liming contaminants were consulted. However, the limits listed in Table 4.3 are for post-application soils rather than for feedstocks. These limits are consistent with EU regulations, with the exception of nickel, where we do allow for a concentration that exceeds the EU soil limit. The risks of higher nickel concentrations are a matter of dispute and ongoing research. Jurisdictions outside the EU typically allow for these higher nickel concentrations. We felt it appropriate to allow for the higher value given the importance of nickel-rich olivine feedstocks in ERW and the lack of strong evidence of human health and environmental risks. Suppliers must always comply with local regulations on feedstock and soil PTE concentration levels.	No change
139		Chromium limit values: While an overall limit on Chromium concentration is important, the key concern from a safety standpoint is Cr(VI) version of chromium. Analysis for Cr(VI) can be expensive, so putting a hard limit on Cr(VI) alone can be difficult. However, adding a softer statement to measure Cr(VI) in some select cases is important to further understand if Cr(VI) contamination should be a major concern.	Rule 4.5.3 has been added requiring suppliers using slags, fly ash, or similar feedstocks to test their feedstocks for hexavalent chromium.	Minor change
140	Table 6.1	Table 6.1 quantification methods: The table 6.1 for approach A6 appears to allow this method for overall quantification of C_stored, when rule 9.5.2.a.iv clarifies that this is an optional approach. A clarification in the final column of table 6.1 would be helpful here.	Rule 11.3.1 (as well as rule 11.3.9) clarifies that approach A6 is to be used optionally and in isolation from the other approaches. It quantifies soil carbonates rather than (bi)carbonate ions exported into the FFZ. We added a footnote to table 6.1 to avoid confusion.	Minor change
141	Table 6.2	in row R, you reference the NFZ rather than FFZ.	Thank you, this was fixed	Minor change
142	general	Overall: Enhanced rock weathering is still a relatively nascent CDR technology and many questions are unanswered. Looking into the scientific literature, there are a lot of question marks around the exact timing and magnitude of the CDR signal. As such it is of great importance to ensure end-to-end conservative accounting within this methodology. We appreciate your push to include a conservativeness factor (Section 10.5.3). This is an important step in the right direction. Thank you for this scientific leadership.	Thank you very much for your positive feedback.	No change

Comment no.	Rule or section	Comment	Response	Action
143	6.3.10	You state 10% ocean loss as a conservative estimate. In our research a value closer to 20% seems to be the more accurate conservative estimate. I have discussed this topic at length with the researchers involved in the 10% Kanzaki study and they were unable to provide a reasonable explanation as to this low number. There is a much more consistent agreement in the scientific community to the 20% calculated by REDACTED and REDACTED. I have discussed this statement also with the OAE community and have not heard anyone else claiming 10%. Both REDACTED and REDACTED are also our scientific advisors, and they do not agree with 10% conservative deduction. Thus, at a minimum, this citation should be shown separately with the comment of ~20% ocean losses. If Puro is indeed committed to conservative accounting, then a number much closer to 20% is essential. Here, I would clearly follow the recommendation of the Cascade foundations document (Section 9.4.2, attached) that “Importantly, there was consensus amongst the Working Group that using a single generalized loss estimate–for example, derived from early studies utilizing Earth System models to interrogate the stability of ocean carbon storage following large-scale ERW deployment (Kanzaki et al., 2023b)–is not appropriate at this stage of the field.”	Thank you for this comment which made us rethink this issue again. After some more discussions with internal and external experts, we decided to increase the conservative estimate that can be used for this loss term to 20% for the reasons given.	Minor change
144	7.2.2b	Transportation emissions: We believe that without any counterfactual, the return-trip of the transportation truck has to be included in the LCA. For clarity, this should be included in the statement.	The rule was clarified in this respect (see also similar comment #10)	Minor change