

Dr. Grayson Badgley
Black Rock Forest
65 Reservoir Road
Cornwall, NY 12518

August 25, 2020

Mr. Sami Osman
Climate Action Reserve
818 W. 7th Street, Suite 710
Los Angeles, CA 90017

Dear Sami:

With the closing of the public comment period, I want to voice my continued opposition to the adoption of the Soil Enrichment Protocol. While our conversations have yielded some improvements, I still have serious concerns about how the Protocol treats sampling depth.

I first raised issues about sampling depth in June 2020, both during and after the fourth working group meeting. I repeated those concerns through email and in a series of letters dated August 6, August 13, and August 17. At each stage, I found resistance to implementing my suggested feedback. Exasperatingly, it was only after the final letter with only days left in the public comment period that I was asked to comment on new, non-public Protocol text in response to sampling depth concerns I had been raising for months. Those revisions, specifically changes to section 6.5.1, would still allow overestimation of SOC accumulation under the Protocol.

During the public comment period, I have continued reading about sampling depth and have spoken with numerous experts in measuring soil organic carbon. From those conversations, it is clear to me that the Protocol has not been informed by the best available science. Simply put, the criteria adopted to potentially exclude some forms of tillage are insufficient to overcome the shortcomings of only sampling soil organic carbon to a fixed 30 cm depth. The science demands deeper sampling, as more fully detailed in a letter I have co-signed with fellow scientists who share my concerns.

I have included a courtesy copy of our complete correspondence since the fourth working group meeting for the public record.

Sincerely,

A handwritten signature in black ink that reads "Grayson M. Chen Badgley". The signature is written in a cursive, flowing style.

Grayson Badgley
SEP Working Group Member

Dr. Grayson Badgley
Black Rock Forest
65 Reservoir Road
Cornwall, NY 12518

August 17, 2020

Mr. Sami Osman
Climate Action Reserve
818 W. 7th Street, Suite 710
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Dear Sami:

Thank you for your reply. This is the sort of consideration I had hoped to receive previously.

I want to focus exclusively on what you refer to as our “disagreement” about soil sampling depth. Disagreements are often productive, especially when both sides point to a body of evidence or data to support their argument. My concern is grounded in well-cited, peer-reviewed literature. According to Google Scholar, the Luo et al., 2010 article has garnered an excess of 400 citations. The patterns they describe, whereby fixed sampling at 30 cm results in over-estimation of SOC, seems to be a well-established, empirical fact. What is the basis for your disagreement with these findings? As you suggested, having a call to discuss your disagreement could be useful. Prior to that call, would you send me the literature you are using to advocate for a fixed 30 cm sampling depth? I’m not challenging the fact that changing tillage practices increases SOC. I’m simply pointing out that the peer reviewed literature indicates that those increases can be significantly over-estimated if sampling does not account for historic plow depth. Accuracy of SOC accumulation is especially critical in the context of carbon offsets given that issued credits must represent a real reduction in atmospheric CO₂ to be of use for climate mitigation.

I would also like to note that I agree with the feedback from the “[other] lead author of both of those IPCC reports”, who states that simply banning moldboard tillage is likely excessive, in cases of “plowing depths of 20 cm or shallower.” The example exactly conforms with what I’m advocating: do away with limits based on tillage type (e.g., moldboard vs. chisel) and simply exclude any field with tillage depths exceeding 15 or 20 cm over a historic (e.g., 25 year) period.

Sincerely,

A handwritten signature in black ink that reads "Grayson M. Chen Badgley". The signature is written in a cursive, flowing style.

Grayson Badgley
SEP Working Group Member

Works Cited

Luo, Z., Wang, E. and Sun, O.J., 2010. Can no-tillage stimulate carbon sequestration in agricultural soils? A meta-analysis of paired experiments. *Agriculture, Ecosystems & Environment*, 139(1-2), pp.224-231.

Dr. Grayson Badgley
Black Rock Forest
65 Reservoir Road
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August 14, 2020

Mr. Sami Osman
Climate Action Reserve
818 W. 7th Street, Suite 710
Los Angeles, CA 90017

Dear Mr. Osman:

On 11 August, 2020, the Climate Action Reserve released a revised draft of the proposed Soil Enhancement Protocol (SEP) for a second round of public comment. Those revisions fail to address concerns I have raised, repeatedly, since the previous public comment period.

Briefly, I noted that the SEP: i) violates best practices for SOC sampling, as laid out by the IPCC; ii) enshrines an easily subverted additionality standard; and, most importantly, iii) fails to address for the peer-reviewed literature demonstrating that sampling to 30 cm, especially with changes to tillage practices, can cause systematic over-estimation of accumulated SOC. I raised these issues, in greater depth, prior to the public comment period and feel that my concerns have gone unaddressed. Instead, I received an email informing me “that after carefully considering your recent feedback, we have determined that it’s best to release the protocol now for public comment.” While my past comments have yielded some changes in the protocol, many of those changes are accompanied by caveats and exceptions that undercut the substance of my concerns, which I see as firmly grounded in the established literature.

As written, the SEP invites the generation of non-real, non-additional carbon credits.

I am especially frustrated by what seems like a pattern of avoiding meaningful engagement on substantive issues affecting the efficacy of the SEP. During the initial drafting phase, I received my first copy of the protocol text roughly 72 hours before the first public comment period, which prevented me from providing meaningful comment. When I raised concerns through the public comment process, I was portrayed as having been unengaged. Since then, I have been quick to speak up, only to find my concerns frequently ignored. After what felt like a rushed fourth working group meeting (in which serious topics, like sampling depth, were hardly discussed), I reached out several times, citing literature by Angers et al., 2008, Luo et al., 2010, and Powlson et al., 2014, all of which point to the high likelihood of SOC overestimation if using a fixed sampling depth of 30 cm. Your initial response on 10 July 2020, which referenced no opposing literature, concluded that “we do not think these concerns justify mandating sampling below the 30cm depth...” Unsatisfied, I pushed again, pointing out that Luo et al., 2010 specifically recommends sampling to 40 cm. I even proposed a more flexible, per field standard, where sampling varies by historical plow depth. Instead, the revised protocol simply excludes a handful of historical plowing techniques and takes no stand

on eligibility based on actual, historical plow depth. After corresponding with both Dr. Angers and Dr. Wang, the corresponding author of the Luo study, I am unaware of any literature contradicting their findings or recommendations that might serve as the basis for your decision to maintain a fixed 30 cm sampling depth.

Throughout the process, representatives of the Climate Action Reserve have openly acknowledged the complexity of the task at hand and the need to have the process informed by the best science. As drafted, the SEP does not fully embrace the real, on-the-ground processes that govern SOC accumulation. These realities greatly complicate designing a robust protocol, but are too important to be ignored.

Sincerely,

A handwritten signature in black ink that reads "Grayson M. Chen Badgley". The signature is written in a cursive, flowing style.

Grayson Badgley
SEP Working Group Member

Works Cited

Angers, D.A. and Eriksen-Hamel, N.S., 2008. Full-inversion tillage and organic carbon distribution in soil profiles: A meta-analysis. *Soil Science Society of America Journal*, 72(5), pp.1370-1374.

Luo, Z., Wang, E. and Sun, O.J., 2010. Can no-tillage stimulate carbon sequestration in agricultural soils? A meta-analysis of paired experiments. *Agriculture, Ecosystems & Environment*, 139(1-2), pp.224-231.

Powlson, D.S., Stirling, C.M., Jat, M.L., Gerard, B.G., Palm, C.A., Sanchez, P.A. and Cassman, K.G., 2014. Limited potential of no-till agriculture for climate change mitigation. *Nature Climate Change*, 4(8), pp.678-683.

Grayson Badgley
65 Reservoir Road
Cornwall, NY 12518

August 6, 2020

Dear Sami:

I have reviewed the revised draft of the Climate Action Reserve's Soil Enrichment Protocol. In my expert opinion, fundamental flaws remain that prevent the protocol from realizing its stated goal of "[ensuring] the complete, consistent, transparent, accurate, and conservative quantification and verification of GHG emission reductions associated with a soil enrichment project."

My concerns center around additionality and soil sampling depth.

Additionality

I was encouraged to see the adoption of a more rigorous additionality standard that explicitly excludes widely adopted tillage and cropping practices. It is also nice to see Negative List will be updated through time.

I was less encouraged by section 3.4.1.2, "PST – Project-Specific Means to Demonstrate Additionality." This section largely invalidates the optimism I had about the new additionality standard. According to this section, "stacking" a second practice change would automatically render all practices, including those excluded on the Negative List, as additional. This raises serious concerns given the expansive definition of "project activities" (Section 2.2.1) adopted by the protocol. Let me walk through a simple scenario that outlines my concern.

Assume there is a management practice, called M^* , that results in a 2.5 t ha^{-1} increase in SOC. Due to various economic reasons, 51 percent of farms in a certain LRR have adopted M^* , independent of the effects of M^* on SOC. Now imagine there is a field, Field A, that currently employs M_0 (another totally imagined management practice) but is considering transitioning to M^* . According to the Negative List criteria, such a transition would not be additional and could therefore not be credited. Such a limitation, however, could be easily sidestepped by adopting a second practice, which we will call L^* . Now, L^* isn't widely practiced and, on its own, only results in a 0.1 t ha^{-1} increase in SOC. But, by combining M^* and L^* , Field A can effectively "wash" the non-additional M^* credits with the addition of a significantly smaller tranche of credits from L^* .

In the above example, 96 percent of the issued credits (2.5/2.6) would be non-additional and should not be credited. It seems fairly straightforward that the project baseline should incorporate M^* and the project should only be credited for activities that occur "in addition" to the common practice (L^*). The protocol already heavily relies on modeling. As such, we should use modeling to isolate the contribution of non-additional from additional practices, as opposed to ignoring the additionality standard if more than one project activity is adopted. From our previous working group discussions, I am under the impression that CAR is expecting "stacking" to be a common practice, which further amplifies my concern. If stacking is common, the new additionality standard would effectively be rendered useless.

I have further concerns about section 3.4.1.2, sub-section 2 (“Demonstrating new tillage practices are still rotated with conventional tillage”). This section states that if a project can point to **three** fields that still incorporate CT with new tillage practices, the new tillage practice is considered additional. Apart from three being an incredibly small number, the mere existence of Section 3.4.1.2, sub-section 2 raises concerns our ability to evaluate projects as additional. I am familiar with the literature this sub-section is referring to — it is well documented that many farmers, after adopting alternative or no-till, will occasionally revert back to conventional tillage. This pattern raises a simple but tricky question: “what fraction of farmers in region X practice NT agriculture?” As written, 3.4.1.2, sub-section 2, argues that as long as even three fields still alternate between CT and NT, NT should be considered additional. I would argue that if determining additionality is so difficult, do we really have any business issuing credits for these types of projects in the first place?

Sampling Depth

I have three concerns about sampling depth that I hope we can discuss.

First, the 30 cm sampling depth is too shallow and would result in over-crediting (Angers et al., 2007; Baker et al., 2007; Govaerts et al., 2009; Luo et al., 2010; Powlson et al., 2014). Though Appendix B raises some interesting points, the peer reviewed literature is unequivocal: deeper sampling is needed. Plowing causes SOC to accumulate just below plow depth. When tillage changes, that deeper SOC begins to dissipate, offset by the accumulation of SOC in the previously plowed region. Failure to sample both the decline in deeper SOC that occurs in tandem with SOC accumulation in the upper layers will lead to over-crediting. Luo et al., 2010 showed that sampling to 40 cm all but eliminated gains in SOC that were detected when sampling at shallower depths. In light of this literature, I had recommended either: a) sampling to 40 cm (as recommended by Luo et al., 2010, page 229) or b) having a variable sampling depth of 15 to 20 cm below historical plow depth. The protocol adopts neither approach.

Appendix B argues that modeling limitations prevent deeper sampling. If models truly cannot go below 30 cm, then the Protocol should only allow projects that have historically plowed to a depth of 15 cm (10 cm if being conservative). Such a standard would respect both the limits of models and the empirical evidence of how SOC is affected by changes in tillage.

Shallow sampling depths will likely result in money for project development flowing to fields that practiced forms of deep tillage that are not explicitly prohibited. This is because the reduction in SOC from changing tillage would be under sampled, yielding more CRTs and higher returns.

My second concern is that the Protocol establishes limits on tillage technologies, as opposed to tillage depth. Such a standard is insufficient. As Table B.2 makes clear, moldboard, ridge till, and chisel tillage all are practiced at depths ranging between 2 to 10 inches (5 to 25 cm) and potentially deeper. Rather than using broad regional averages for specific technologies, the protocol should use specific, field level information. Table 6.1 specifies that projects need to provide information about historical tillage depth. That information could be used to exclude any project that plows below a certain depth (e.g., 15 cm in the case where only 30 cm of soil is required to be sampled).

Let me use an example from forestry that demonstrates my hesitancy about qualitative ‘technological standards’, as opposed to quantitative, physical (e.g., depth) standards. Let’s say we wanted

to ensure that a certain fraction of trees remained in a forest. If we specified a technological solution, we might think about adopting a ban on clearcutting. However, clearcutting is a specific 'technology', whereby 100 percent of trees are removed in a single cut. With clearcutting, basal area instantly drops to 0. Another "technology" is shelterwood cutting. In shelterwood treatments, 65 to 80 percent of basal area is removed, with the remaining stems left standing for a short period to reseed the plot. Several years later, the remaining large trees are harvested. While the technologies are different, the end result is the same: basal area is dramatically reduced. Instead, we'd be much better off adopting a quantitative threshold (e.g., no more than X percent of basal area can be cut in a 100 year period).

While this example might be somewhat roundabout, let me state my concern more clearly: simply limiting moldboard or deep ripper technologies would still allow other deep plowing practices to be (over)credited. Exclusions need to be based off actual tillage depth, not one-time snapshots of regional averages.

I also believe the last sentence of section 6.5.1 needs revision. It reads: "Fields historically employing deep ripper or moldboard tillage practices may become eligible to be credited for SOC gains if/when they subsequently adopt any tillage practice other than no-till in subsequent reporting periods." SOC takes on the order of tens (20+) of years to equilibrate (Powlson et al., 2014 and references therein). However, this sentence would allow any project that stopped deep tillage prior to the baseline (e.g., 3-5 years) to be project eligible. This would result in over-crediting, as SOC accumulation would be at least partially due to the cessation of a practice specifically prohibited under the protocol.

My third and final concern with sampling is that the fixed 30 cm standard goes against best practices in quantifying National Greenhouse Gas Inventories, as outlined by the IPCC. As we discussed previously, these standards dictate that depths of greater than 30 cm are needed "if it is clear that land-use change and management have a significant impact over the proposed depth increment." Ample evidence exists that changes in tillage practices has a significant impact on SOC at depths that exceed 30 cm, meaning best practices require deeper sampling or exclusion of fields based on historical plow depth.

Sampling and additionality constitute my major concerns with the protocol, as currently drafted. While other issues exist (e.g., the uncertainty allowance in section 5.2 exceeds contributions to the buffer pool), I am unsure of the utility of discussing these important but peripheral issues before we address more fundamental concerns surrounding additionality and sampling. I look forward to future conversations to ensure real, quantifiable increases in SOC from soil enrichment projects.

Sincerely,

A handwritten signature in black ink that reads "Grayson M. Allen Badgley". The signature is written in a cursive, flowing style.

Grayson Badgley

Sampling Depth

As written, the protocol requires a fixed sampling depth of 30 centimeters for evaluating SOC (Section 6.4.1, Table 6.1). However, there is substantial evidence that change of tillage, such as reduced- or no-till, commonly change the vertical distribution of SOC. This manifests in a commonly documented pattern of SOC loss just below the previous plow depth, offset by SOC accumulation above the previous plow depth (Figure 1).

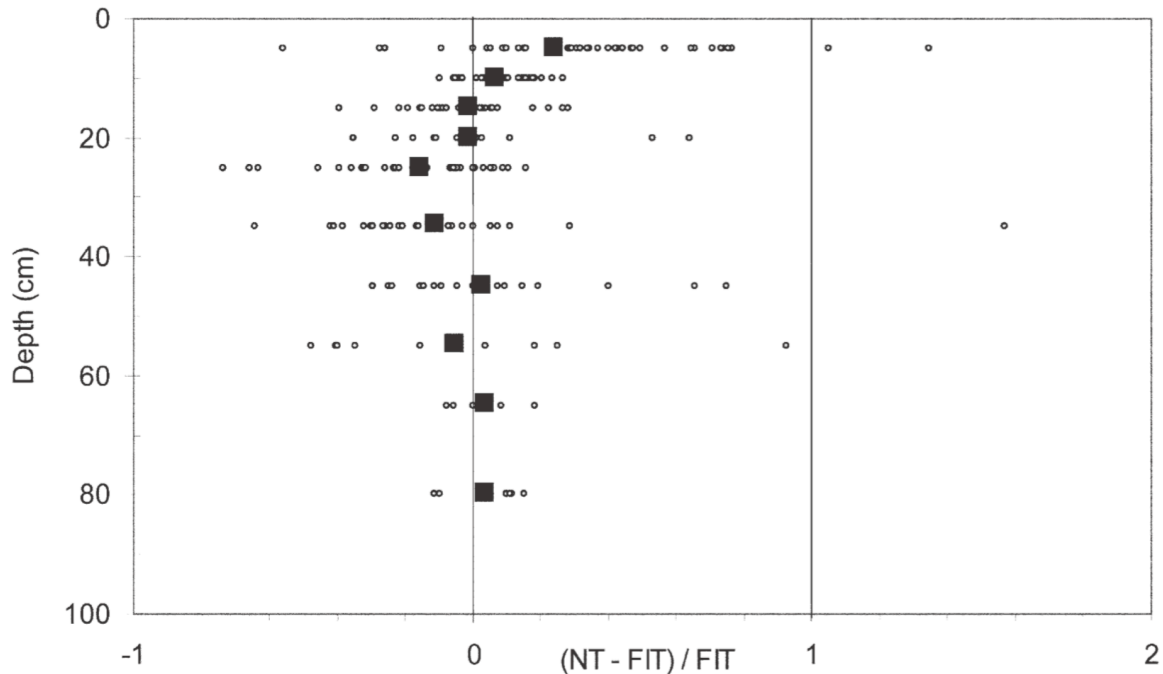


Figure 1: The percent change of SOC with depth, when comparing no-till (NT) to full-inversion till (FIT). Clipped from Angers and Eriksen-Hamel (2008).

Failure to account for this pattern would result in over-crediting. The SEP should adopt one of two options:

- Maintain a fixed sampling depth, but change that depth to something like 1 meter.
- Require sampling depth extend a fixed depth below historical plow depth. Eyeballing Figure 1, something like 30 cm below plow depth might be sufficient for distinguishing redistribution of SOC from the true accumulation of SOC.

Additional relevant citations include:

- D. Angers and N. Eriksen-Hamel (2008). "Full-Inversion Tillage and Organic Carbon Distribution in Soil Profiles: A Meta-Analysis". *Soil Science Society of America Journal - SSSAJ* 72
- J. M. Baker et al. (2007). "Tillage and soil carbon sequestration—What do we really know?" *Agriculture, Ecosystems & Environment* 118.1-4, pp. 1-5

- D. Powlson et al. (2014). “Limited potential of no-till agriculture for climate change mitigation”. *Nature Climate Change* 4, pp. 678–683

Ton-year Accounting Method

It would be helpful to specify the method used for calculating the schedule the weights in Table 3.2. Many flavors of ton-year accounting exist. I suggest adoption of some variant of the Lashof method that takes into account the i) dynamic and ii) reversible nature of SOC sequestration projects (e.g., Levasseur et al., 2012). Several national and international organizations have developed ton-year accounting methods that seem to coalesce around Lashof, though they rely on a different set of underlying assumptions (García and Freire, 2014; Brandão et al., 2013). The SEP would benefit from the explicit use of an existing method, which will garner further confidence in the ton-year approach.

- M. Brandão et al. (2013). “Key issues and options in accounting for carbon sequestration and temporary storage in life cycle assessment and carbon footprinting”. *The International Journal of Life Cycle Assessment* 18.1, pp. 230–240
- R. García and F. Freire (2014). “Carbon footprint of particleboard: a comparison between ISO/TS 14067, GHG Protocol, PAS 2050 and Climate Declaration”. *Journal of Cleaner Production* 66, pp. 199–209
- A. Levasseur et al. (2012). “Assessing temporary carbon sequestration and storage projects through land use, land-use change and forestry: Comparison of dynamic life cycle assessment with ton-year approaches”. *Climatic Change* 115