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NPP constraints are crucial to realistically project soil organic carbon sequestration. Response to Minasny et al.

The rigorous scrutiny Minasny et al. (2022) devote to our paper (Janzen et al., 2022) is both gratifying and edifying. The issue we addressed – the prospects for soil C sequestration to mitigate climate change – is so complex, ecologically and technically, that its underlying science is never fully settled. Any paper, including our own, is therefore best seen as a further installment in an iterative conversation toward deeper understanding. Minasny et al. contribute valid observations toward that ongoing development, but have not dissuaded us from persisting with our underlying thesis: that amounts of photosynthetically-derived C constrain potential soil C sequestration and offer a way of roughly estimating its potential magnitude.

We emphasize again that the primary intent of our paper was not to provide yet another prediction of potential global soil C sequestration. Our aim, rather, was to explore a rudimentary approach – explicitly and deliberately framed as a Fermi-like approximation – for establishing an *upper boundary* for such opportunities. Our thesis, simply stated in our title, is that the amount of C trapped by photosynthesis on croplands globally constrains the additional amount of C that can be stored in soil. Since all soil C is derived from photosynthesis, the amount of net primary productivity (NPP) applied to soil must present the ultimate limit on additional C sequestration. In our view, beginning with the amount of biomass C available and working downward – from global to local – offers a crude but robust constraint on opportunities for C sequestration. At the very least it represents a complement to the inverse approach – beginning at a local level (Mg C ha^{-1} yr^{-1}) and building upward to a global level – which is not so easily bounded.

We make no pretense of original equations or a new model; our rudimentary mathematical construct derives from the long-established principle that soil C stocks reflect a net balance of inputs and losses (e.g. Albrecht, 1938; Russell, 1926). What may be slightly new in our paper is not the development of a new algorithm, but rather the straightforward notion that we can begin with an initial estimate of global NPP and, by estimating its fate, derive some constraints on net gains of soil C.

The first step toward our objective was to estimate the absolute upper bound: the amount of photosynthetically trapped C that can be stored as 'lingering C'. The assumptions inherent to this step, we contend, are reasonably secure: we know from extensive literature that soil C gain is directly related to amount of C entering soil as plant residues. We know, further, that most of this C is quickly returned to the atmosphere because of soil biota's relentless pursuit of energy and nutrients, leaving only a small remnant to 'linger'. (Our terms, 'ephemeral' and 'lingering' are not proposed as two 'conceptual pools,' as Minasny et al. imply, but merely denote that most plant C added to soil is quickly lost, and only a small fraction 'lingers' for more than few years (e.g. Basile-Doelsch et al., 2020; Berthelin et al., 2022)). Based on cited isotope-based studies (which measure the fate of C directly), this fraction is actually fairly consistent across a wide range of soils and climates. Although temperature affects how quickly the 'ephemeral C' disappears, the amount of 'lingering C' remaining after this early rapid flux averages around 15% (Ayanaba and Jenkinson, 1990; Gonzalez and Sauerbeck, 1982; Gregorich et al., 2017; Ladd et al., 1985). Thus, if we know the amount of NPP available annually, and correct for the large amount of C removed in harvest, we can estimate annual accrual of 'lingering C'. In our approximation, using data from Wolf et al. (2015), we derive an estimate of 0.44 Pg C yr^{-1} for the absolute upper bound for C sequestration. This value, as indicated in our paper, can be easily updated using any improved estimates, or even by exploring future possibilities, such as emerging agronomic practices or genetic enhancements of photosynthetic efficiency.

This absolute upper bound, however, is well beyond what is achievable, because the historically-stored C already present in soil is always decomposing. The rate of this ongoing decay, we readily acknowledge, is uncertain. Indeed, as explicitly emphasized in our paper, our crude approximation was offered merely to *"illustrate how the decomposition of this indigenous soil C further limits net SOC gain"*. Minasny et al. propose that our estimate of existing soil C was too high, based on an area inconsistent with that of our assumed NPP value, and proffer ~~a lower value of 73 Pg C (to 30 cm)~~. While we are happy to consider improved estimates of current C stocks, the turnover rate of 'lingering C' might be even more uncertain. Aware of this uncertainty, we deliberately chose assumptions that would underestimate turnover and yield optimistic upper boundaries for sequestration. In particular, we assumed:

a) that the overall mean residence time (MRT) of C in surface 30 cm of soil is 500 yrs, much higher than generally cited in the literature (e.g. Lal, 2004; Yan et al., 2017);
b) that all C below 30 cm, a substantial stock of C, is completely inert (MRT = infinity); and
c) that all existing C in soil is 'lingering' (when, in fact, some is 'ephemeral').

Choosing a more likely MRT and allowing for even slow turnover of SOC below 30 cm would reduce net soil C gain to values well below Minasny et al.'s calculated value of $\sim$ 0.3 Pg C yr^{-1}, even if their lower estimate of soil C stock is accurate. Particularly crucial in such approximations is choosing an MRT value consistent with the soil C stock; Minasny et al., using 73 Pg C as soil C stock and an input of 0.44 Pg C yr^{-1}, imply a current MRT of 166 yrs. Increasing that to 500 yrs on a global scale (as in their calculation) presents a rather formidable challenge. All of these machinations aside, they lead to differences of merely tenths of Pg C yr^{-1} – small amounts relative to the magnitude of

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anthropogenic C emissions (10 Pg C yr^{-1}).

We readily acknowledge the uncertainty in the turnover rate of existing soil C, a level of uncertainty perhaps not adequately emphasized in our paper. But uncertainty goes both ways, not necessarily only towards more optimistic predictions for C storage potential. For example, in our approach we assumed that cropland soil C stocks are currently more-or-less at steady state, and then calculated additional gains from inputs of new photosynthetically-derived C above this existing stock. Minasny et al., however, offer evidence that global soil C stocks are in fact declining. If true, this limits any possible net gains of soil C *even more.* If cropland soils are currently *losing* C, we will need even *more* inputs of C to achieve net gains; that is, global photosynthetic inputs are even *more* constraining to soil C sequestration. And if climate change accelerates soil C turnover, that further increases the amount of plant C inputs needed to build soil C. This is perhaps a point worth emphasizing: if croplands are now losing C, and are vulnerable to even higher losses, then increasing soil C significantly poses a daunting challenge.

Our approximation, as indicated explicitly in the title of our paper and as highlighted by Minasny et al., refers only to croplands. These lands seem the logical starting point because they are most amenable to new management strategies. Other lands, notably extensive grasslands, as we discuss briefly, could also be repositories for soil C gain, but opportunities there may often be limited by constraints on C inputs. Lands devoted to agroforestry also could be C sinks, as Minasny et al. indicate, but again, the magnitude of those sinks depend on net retention of photosynthetically-derived C, relative to that under their existing uses.

Minasny et al. propose that researchers avoid getting *"fixated on the number"* and instead should *"discuss the way forward."* We agree that endless re-working of projected estimates is unproductive, but contend that establishing reasonable upper limits is still crucial. As policymakers search for 'net-zero' strategies, nature-based solutions are especially alluring as 'win-win' opportunities but, if these are unrealistic, might detract from the urgency of difficult reforms needed in energy and land-use strategies (Amundson and Biardeau, 2018; Baveye and White, 2020; Schlesinger and Amundson, 2019).

With the preceding as a qualifier, we whole-heartedly endorse Minasny et al.'s advocacy of forward-looking research into ways of maintaining or enhancing soil C stores. Although Minasny et al. seem to imply otherwise, our paper does address the question of "the way forward," devoting to it our entire 'Implications' section. Indeed, we contend our thesis offers a lens through which to contemplate future research by asking explicitly: *"If we aim to increase soil C stocks, where will the additional C come from?"* As we emphasized, *"More important than the numerical value of our estimate, perhaps, is the change in orientation: what matters in projecting soil C change is not past losses but future C inputs."* Our thesis, then, insists on an ecosystem-based approach to C sequestration including more deliberate measurement of NPP and its fate, notably in the rhizosphere. Rather than merely measuring changes in soil C over time (as we ourselves have too often done), we need to follow more studiously the flow of C through the entire ecosystem, beginning with photosynthesis and including all relentless streams of C through soil and beyond. More than that, we will need also to include emissions and removals associated from affiliated practices such as fertilization. In our view, this is perhaps the strongest argument for our thesis, with all its admitted shortcomings: it enforces an honest appraisal of soil C sequestration based on a full ecosystem perspective, following C from CO_2 to CO_2.

To cite one instance, such full-system accounting enforces clear distinction between C sequestration and C re-distribution. For example, amendment of soils with exogenous organic material is widely seen as an effective soil C sequestration strategy and has been included, sometimes prominently, in previous assessments of potential soil C gains (e.g. Lal, 2020; Minasny et al., 2017). But soil C gains from such amendments are roughly balanced by soil C losses in the area where the source plant material for these amendments was originally grown. Thus, exogenous soil C inputs withdraw CO_2 from the atmosphere only to the extent that they increase overall NPP and its return to soil. Following the flow from atmosphere to soil and out again may help avoid overoptimistic expectations of net CO_2 withdrawal from the atmosphere by soil C sequestration.

Minasny et al. suggest, correctly, that our estimates of C inputs are based on current productivity, and imply that future inputs will be appreciably higher, leading to greater sequestration. We agree that increasing cropland NPP may indeed be conceivable, even within the constraints posed by climate change, and fully endorse ongoing work toward this urgent aim. But what matters, we have said, is not only NPP but its eventual fate. Many agronomic efforts to elevate NPP are undertaken with deliberate motive of greater harvest (that is, plant C removal), in response to growing competition for plant biomass from competing demands – food, fodder, livestock bedding, fuel, among others. As our approach emphasizes, higher NPP alone may not offer much benefit to soil C input if a larger fraction is harvested. In contemplating future scenarios, our approach may help to quantify how much additional plant biomass would be required to augment soil C reserves beyond those now achievable. Other recent studies have adopted similar approaches at regional levels to assess whether C inputs are adequate to support sequestration targets (e.g. Bruni et al., 2021; Martin et al., 2021; Seitz et al.).

Minasny et al. evidently see little merit in our thesis, and seem inclined to dismiss it outright. We respect this considered view (knowing we have erred before), but are not persuaded by their arguments to jettison our approach. We harbor hope that we have made some progress towards setting reasonable limits for soil C sequestration. In effect, we propose that top-down approximations complement and help constrain bottom-up estimates. Minasny et al. seem to emphasize the latter, normalizing their illustrative estimate (0.3 Pg C yr^{-1}) to a per ha rate and even suggesting that it concurs with the aspirational 4 per mille goal.

We agree wholeheartedly with Minasny et al. that, as soil scientists, we should energetically pursue all avenues to understand better the dynamics of C in soil, and enhance the amount preserved wherever possible. Wisely managing soil C flows and stores is crucial to biospheric integrity and to human habitation therein, and ongoing debates about magnitudes of future climate change potentials should never obscure or distract from this critical imperative. We maintain, however, that efforts to scale up local soil C sequestration rates to global levels may benefit from sober evaluation using approximate upper boundaries. Our thesis, we contend, offers a way of doing that and we look forward to letting other readers, now and in the future, to adjudicate its fate.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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