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Interactive comment on "A new synthesis for terrestrial nitrogen inputs" *by* B. Z. Houlton and S. L. Morford

Anonymous Referee #2

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This manuscript provides a timely discussion of recent advances in understanding nitrogen inputs to ecosystems. In particular, it points to the need to quantify how bedrock N inputs vary, how they compare with N deposition rates, and how the different sources of N may influence biological N fixation. As the title suggests, there is a fair bit of good synthesis of specific results, including previous work by the authors. Much of this synthesis is geared toward recommendations for a framework for future research. The manuscript is short but this is appropriate, given that it does not aspire to provide a comprehensive review of N inputs. Rather, the manuscript focuses on results that are most relevant to pressing the case about bedrock N inputs. In effect, this is "posing-the-question" paper about the relative roles of parent material and atmospheric deposited N in ecosystems, with highlights on how N systematics likely differ from other nutrients

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Interactive Discussion
Discussion Paper

in soils and ecosystems.

Overall, the case studies and other information presented here are highly relevant to the call to action at the end about including bedrock N in N cycle studies, but I do have some criticisms about the framing of everything within the state-factor approach, and thus with the core of the manuscript. This main concern is a philosophical one, and it would be difficult to fix without major revision. In addition, there are both specific and technical problems that will be much easier to fix. I present all the concerns that I had in the following three sections: general comments, specific comments, and technical comments.

General comments

Although I ultimately think that posing-the-question papers can be very effective and important (even game changing/trend setting), this one fell short in my view. To sum it up bluntly I found the manuscript to be not ambitious enough. Before I explain why I think this, I should make it clear up front that I think that this paper could be published with minor revisions, at the discretion of the other reviewers and the editor. However, with major changes in the framing of the paper, I think the main points could be made much more effectively. Thus it could make a much more significant contribution to the literature after substantial revision. So I am recommending major revisions.

In a discussion paper with a call to action about advancing a new vein of research, it makes sense to build on existing frameworks, to help give readers context for the new ideas. Here, in building on the state-factor approach of Jenny for soils, the authors have history on their side. Moreover, in focusing on the importance of the bedrock N inputs (a heretofore underappreciated concept) within the context of the state factors (a many-decades-old idea), the authors are taking the conservative approach of taking one step at a time in advancing knowledge.

This kind of conservative approach may often be a wise course, but I believe the authors have erred on the side of being too conservative. When I first read the title (i.e.,

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"A new synthesis. . ."), skimmed the abstract, and saw the authors' names, I expected something fresh and bold from the manuscript. Yet, my sense from reading it carefully was that the discussion presented there was entrenched in (maybe even forced into) old ideas (Jenny's state factors) that do not actually work very well as a framework for answering questions about N inputs. Moreover, the manuscript misses, in my view, an opportunity to build on some of the process-based advances that have been made since Jenny in thinking about soil evolution and inputs of other nutrients in soil systems. These advances – in particular in the papers by Porder – were was cited in the paper. So the missed opportunity was all the more surprising. The final message of the manuscript in review – to me at least – ultimately comes across as antithetical to the message that the authors seemed to want to send, which is that the state-factor approach can be usefully applied to frame N input problems.

Make no mistake, Jenny's state-factor approach has proven useful in soil science time and again. Its impact would be very difficult to measure in part because it has been so very big and far-reaching. At its core, however, the state-factor approach is really just a soil-specific framing of a popular way of doing experimental research in every field of science that I am aware of. That approach can be summarized as follows: hold all but one thing constant and vary the rest to explore the effects of the factor of interest on the parameter or variable in question. Thus, choosing a chronosequence (or a climosequence, or a you-name-it-sequence) for the study of soils, after Jenny, boils down to the business of designing an effective experiment in a natural setting where things like climate, organisms, topography, parent material, time and human activity (Jenny's state factors) all vary.

To be clear, I am not debating the depth of Jenny's contribution to the field. It was enormous, has reached beyond soil science (e.g., to the sister fields of geochemistry and geomorphology), and will continue to resonate for many years to come. Thus it is very important to recognize Jenny's work and influence whenever natural experiments are used to understand things like weathering, erosion, and soil development.

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Rather, I am debating whether it is instructive to place the discussion of N inputs into the confines of the state-factor approach. I do not think it is justifiable to make it an organizing principle for a synthesis of observations about N inputs. Part of my problem with the state-factor approach in this context is that it is too limiting. By focusing on the six things of the revised Jenny system – i.e., climate, organisms, topography, parent material, time, and humans, after Amundson and Jenny (1991) – this work overlooks the potential to set the problem of N inputs more squarely in the context of ecosystem processes. Instead, the processes are mentioned within each state factor. The state factors influence the processes of interest, but at a level of abstraction that limits gains in understanding. In fact, by dividing the discussion of the processes into each state factor subheading, I think the authors have actually reduced the cognition potential of their manuscript, relative to a process based framing.

To back some of these assertions up, I point to some of the specific limitations that I see in this approach. My list here is not exhaustive, but rather exemplary of the kinds of problems that the state-factor approach alone fails to fully address. For example, look at the discussion under “Climate”. It addresses temperature sensitivity of N fixation, temperature sensitivity of weathering, precipitation sensitivity of rock weathering, the coupled precip/temp dependence (through the influence of T on ET), and climate related variations in biological weathering. But weathering rates also depend on things like topography and time. Moreover, the N input from bedrock due to weathering depends on the concentration of N in the bedrock (a subfactor of parent material). It should also depend on how deeply weathered. The processes of interest are fixation and weathering and erosion, and they are influenced by a wide range of factors. But here, under the Climate subheader, just some of the factors are noted, and not all of them are soley coupled with climate alone. Organisms and topography should play a role. (For example, the influence of topography on the water balance should be mentioned here too, since water balance probably falls under climate?).

Indeed, the processes that drive the inputs are mentioned in each of the sections. For

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example, weathering and erosion appear in all of them both implicitly and explicitly. In effect, the authors have taken a discussion of the three avenues of N inputs (fixation, deposition, and rock weathering) and split it up into a discussion of Jenny's state factors. I am not sure I learned anything new here, except that splitting these processes up this way introduces a level of abstraction because it chooses the empiricisms implicit in the state factor approach over the mechanisms of the processes. Part of the problem is we are never actually given any razor sharp reasons for breaking it into the state factor approach at the beginning of the paper, and moreover, at the end, the authors never arrive at some razor sharp analysis of what we learned from the state factor framing. I searched and only found a couple of clear questions at the end of the state factor sections. For example, at the end of the organisms section, the authors write: "This suggests active uptake of rock N by plants in N-rich parent material, which is likely to be facilitated by root-associated ECM in Douglas fir forests. Examining this hypothesis in a range of sites by measuring mycorrhizal abundance, N concentrations and δ15N of various N pools is deserving of future work." It is true that this does deserve further work. But it's a hypothesis you arrive at not by using a state factor approach, but rather by considering the processes, specifically the evidence behind ECM weathering N-rich rock and its possible implications for N inputs. The state factor framing was not needed.

Rather than break the discussion out into a state factor mold (which is done here at least in part because others have done it successfully?), it seems this analysis of N inputs would be better served by some sort of synthesis that systematically discusses rock weathering and its role in driving N inputs. This could then be followed by a parallel section on N fixation, and then again by a parallel section on N deposition. Why not express each term mathematically? For example, consider the supply rate of N from bedrock. It should equal the concentration of N in bedrock times the rate of conversion of rock to soil. This puts the spotlight on two things that need to be measured to understand bedrock N inputs. The product of the two is the conversion rate of N in rock to N in soil. Maybe the authors could come up with some sort of expressions for N deposition and fixation. It seems like expressing all of the various input terms in a

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mathematical framework would open the door to some new questions. For example it could culminate in an expression for the mass balance of N in ecosystems. dN/dt = a series of terms that express the inputs and outputs. A discussion section could then focus on considering where the different terms dominate over the other sources, both in the past (relevant to forming soils we see today) and in the present (relevant to predicting the future)? This could culminate further perhaps in a series of questions and testable hypotheses about the relative importance of the different input pathways. A lot of the basis for a bold new synthesis like this is already here in the paper, but it is not organized in a way that strongly motivates the reader to take action on the observations that have been made thus far.

Maybe the paper could still work in the state factor framework if the discussion and implications could somehow be made to justify it. Currently in the discussion there are three long paragraphs (after the brief into to the discussion section) on why it is important to understand the different N inputs but there is no demonstration of why the state factor approach is so insightful. Then there is a paragraph in which the authors ultimately admit that the traditional state factor approach is not enough. Then a paragraph about the oddity, relative to other nutrient systems, that is introduced by the fact that of N is made available by fixation. Then the last paragraph is about human-derived N inputs. None of this puts a razor sharp point about why we should be thinking with our state factor caps on.

This manuscript is really all about the framing of N inputs within the concept of the state factor framework. Thus a substantial change – e.g., from the state-factor framing to a process-based framework – would be a major burden, requiring a major revision here. My opinion is that it would be a much more valuable, thought-provoking paper. One alternative would be minor revisions to what is here and just go to press. Another option is to go back to the state factor sections and really put a point in a razor sharp way on why it is so useful: prove it to the reader that this is a useful framework for advancing N input research. Still another option is to seek middle ground, maybe

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adding a long process-based section that starts “although the state factor approach is potentially useful, there are other ways to frame the problem”. Then, in that section, provide a process-based framework with math et cetera.

A statement from the abstract: “We conclude that a state-factor framework for N complements our growing understanding multiple-source controls on phosphorus and cation availability in Earth’s soil. . .” So really whether to publish boils down to a matter of opinion about whether the state-factor approach is useful enough in the context of N inputs to win it the centerpiece of this posing the question paper. I was not convinced, and so recommend major revisions. However, since this is something of a matter of opinion, the views of the authors (who are some of the leading experts on bedrock N), the other reviewers, the public commenters, and the editors should factor in just as much as my own. After reading this paper, I personally was not convinced that the state factor approach does much to improve understanding of N inputs.

Specific comments (p refers to page number, l refers to line number)

p 500, l 4: “ecosystems progress interminably” seems a little strong. In the next paragraph you present evidence that ecosystems are renewed. The citation to Vitousek at the end of this sentence seems to misattribute this outdated idea in a recent publication to someone who helped over turn it by recognizing the role of erosion of depleted soil and weathering of fresh rock in rejuvenating nutrients.

p 500: Paragraph 3 of intro seems to build the case for thinking about a mass balance framework, not a state factor framework.

p 500, l 14 and p 508 (middle of page): These excellent insights on P inputs from Porder arose from a mathematical framing of the problem. Specifically, there is a conservation of mass, with consideration of various inputs. In a paper cited here in other places (Hilley and Porder, 2008), Porder helped take it a step farther and applied it across the globe and included dust inputs. Seems like this is a fruitful direction that the authors here could adopt in this paper on N inputs.

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p 500, l 21: It seems a bit disingenuous to suggest that a multi-input framework is lacking for N. People have been thinking about deposition and fixation a lot. That is more than one and thus multi

p 500, l 25: OK. So you are casting aside textbook paradigms for N inputs, but forcing your very good bedrock N idea into the state factor framework, also a textbook paradigm, just because it is there, even though it does not give you any razor sharp questions and directions moving forward on the research?

p 502, l 1: "devise" is the wrong word. Dokuchaev and later Jenny devised it. You are adopting something that has already been devised as a framework for synthesizing analyses of N inputs to ecosystems, with an eye towards understanding the relative importance of bedrock N inputs, which have generally been overlooked.

p 502, l 7-9: At the risk of seeming heretical, I would suggest that one of the problems with these state factors in the context of ongoing research on N inputs and other areas is that they are too broad. Where does climate change fit in? When looking at a chronosequence of marine terraces across a zone with uniform climate today, the older terraces have experienced a different climate. . . maybe multiple glacial-interglacial swings. Likewise, the oldest soils have likely experienced considerable erosion. So where does erosion fit in? It cuts across many of these factors. Climate, lithology, organisms, topography. Ultimately many important factors in soil formation and nutrient cycling cut across the 6 state factors listed here. Passing all of the great N input research through the six-factor construct here is like putting blinders on when it comes to understanding the process. Or maybe its like taking your glasses off when looking at a fine painting.

p 502, l 15: "Rather, our aim is to examine how different state factors broadly influence the distribution and magnitude of atmospheric vs. rock N sources, with case studies presented throughout our synthesis." This is a fine aim, but to make such an examination effective, it should culminate in some sort of discussion of why the state-factor

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approach provides a good way to think about the N input problem moving forward.

p 503, l 13: Here the topic is rock weathering, a process which is influenced by many climatic factors and also topography, rock type, time, organisms. It seems more compelling to me to talk about process. Not break them into state factors.

p 504, l 10: Here, the organism connection is largely discussed in the context of climatic differences in vegetation. Not as originally proposed in the state factor approach (where variations in organisms within a single climate would control things).

p 507, l 24. What about topographic curvature? This is perhaps more important at the scale of a toposequence... over a range of climates and rock types that might be considered constant.

p. 508, l 17: Right, so some estimate of the relative amount of chemical to total erosion is needed. It seems like this paper would benefit from some very explicit prescriptive statements about what is needed to advance N input research. This would be one of those things. Maybe a bullet point list in a new section entitled “Conclusions”

p 509, l 1: Please define “stable”. Stable relative to what? Erosion? How slow does erosion have to be to constitute stable? Ridgetops can erode quickly, even when they are gentle, due to diffusive processes, as long as the slope is curved.

p 510, l 5: It seems a bit misleading to say that 99% of Earth’s fixed N is in rock. Most of that rock is buried and thus not plant available. What matters is the percentage of Earth’s surface that is underlain by N-rich bedrock. I feel like some attempts to estimate that would put the bedrock N problem into a more realistic context. It would be important in any such estimate, to exclude those areas where bedrock is so far from the surface that contributions from weathering do not contribute to the ecosystem. I am thinking of the broad floodplains that cover much of Earth’s surface.

p 514, l 3: “where parent material N contents are typically low” (cites) and where rock weathering rates are slow (cites)

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p 515, l 20: I never actually saw a new framework here. This is N inputs divided into the 6 state factors. That's not a new framework. It would be more correct to say: We suggest that N-inputs can be usefully cast within the state factor framework.

p 516, l 5-6 : Wait. The title of this paper is "new synthesis". Is the synthesis mentioned here on this line different? Used in future tense, so this is something coming up, not presented here in the paper?

p 516, l 13-14: Authors are basically saying here that you need to use existing knowledge of N inputs to design efficient studies. This is fine, but it does not justify the state factor framework.

p 533-540: table 1 and figures 4 and 7 are the only visual aids that seem closely linked to the state factor framework that is the centerpiece of this paper. The other figures are just aids to explain process. They are compelling, however, and it seems to drive home the point that the right way to frame this is in terms of process.

p 540: I do not understand this figure. Why are there differences? What is a stable landscape versus a denuding landscape? Where would you draw the cutoff between these things?

Technical comments

p 499, l 14: demonstrative is the wrong word

p 501, l 21: Awkward. "Our objective is to present a general approach to N fixation,. . .". Maybe you mean to say: "Our objective is to present a general approach to understanding N fixation,. . .". Key word missing.

p 508, l 25: You never defined "shoulder" or "backslope positions". I do not understand these statements.

p 517, l 6: The classical pedogenic models are never going to be anything but classical, so the "hitherto" adverb sounds wrong. (One would never say, Classical models will

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soon not neglect the rock N sources.

p 517, l 19: new section?

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