

Interactive comment on “A new synthesis for terrestrial nitrogen inputs” by B. Z. Houlton and S. L. Morford W.

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Review of Houlton and Morford A new synthesis for terrestrial nitrogen inputs Soil 2014- 29

Author response in bold.

The authors provide a nice summary of the sources of reactive nitrogen on the Earth's land surface, covering the well-known inputs by N-fixation and industrial fertilizer, and focusing on the nitrogen derived from the lesser-known pathway of rock weathering. The latter was recognized by these workers and their colleague, Randy Dahlgren, at UC Davis only within the past couple of decades.

The authors make a strong case for the importance of rock-derived nitrogen in a variety of regional settings. Surprisingly, they do not mention that in the preindustrial world, rock-derived N (20 TgN/yr) may have been in the same order of magnitude globally as nitrogen from biological fixation (60 TgN/yr; Vitousek et al. 2013), a major modification of a paradigm of biogeochemistry. Now, of course, human production of nitrogen fertilizer (~140 TgN/yr) and cultivation of leguminous crops (60 TgN/yr) dwarfs both natural sources.

We agree that the role of rock N inputs to the global N supply, both under modern and pre-industrial input scenarios, is an area that deserves more research. Not only are rock N inputs potentially of similar magnitude to N fixation, but because the factors that regulate these two N input processes at biome and landscape scales differ, our new synthesis suggests where rock N inputs may dominate in a given ecosystem site.

Of course, human actions have more than doubled the amount of fixed N inputs to the land system. However, the importance of rock N to the remainder of the terrestrial biosphere, where anthropogenic N inputs are more limited, is still uncertain, though likely meaningful (at least ~20 Tg N/yr).

We have added several sentences in the introduction to address the potential global N input magnitudes:

“Geochemical models have pointed to the importance of N weathering in regulating atmospheric N₂ over deep time (Berner, 2006). The burial of fixed N in marine environments (~25 Tg yr⁻¹, Gruber and Galloway, 2008) cannot be compensated by solely volcanic degassing (~ 0.4 Tg yr⁻¹, Busigny *et al.* 2011), suggesting that the majority of the N transferred to the crust must be recycled via rock uplift and weathering. This implies that global rock N inputs may be of

similar magnitude to lower-bound estimates of biological N fixation in natural terrestrial sites (58 Tg yr⁻¹, Vitousek et al. 2013)."

This is an interesting and readable paper that I could easily imagine as an assignment in a graduate class in biogeochemistry. Aside from atrocious misuse of the hyphen, the authors only give me a few points to quibble:

We have substantially edited the text to further improve the clarity of the manuscript. This includes removing many of the em-dashes. Thank you for the advice.

Line 159. With such a robust literature documenting ecotypic differentiation of enzyme temperature optima in plants and soil microbes, I am surprised that the same is not true of nitrogenase. Or at least, that no comment was made on this.

We have added the following text: "There is little evidence for acclimation of N fixation across tundra to tropical climates, perhaps owing to the complex nature of the nitrogenase enzyme (Houlton et al. 2008). Rather, the integrated data in Fig. 1 can be fitted to a single Arrhenius function with slope that falls between the steep temperature-dependence of the nitrogenase enzyme and the less pronounced temperature sensitivity of photosynthesis."

Line 214. Free-living (asymbiotic) N-fixation receives scant treatment in this paper, even though at rates of 1 to 5 kg/ha/yr, it may account for as much as 1/3 of the biological nitrogen-fixation on land. In particular, it would be nice to comment on the importance of free-living nitrogen fixation, especially in certain desert environments and their soil crusts.

We have addressed free-living rates globally and in desert crusts.

We have addressed this point in the following text:

"This is pronounced in desert ecosystems where patch-scale heterogeneity in soil-crust communities and seasonality in moisture and temperature alter spatial patterns of N fixation and nutrient cycling (Belnap, 2002)."

"Studies using foliar ¹⁵N/¹⁴N in arid sites have suggested similarly high rates of N fixation (9 to 22 kg N/ha/yr) in *Prosopis glandulosa* (mesquite) stands (Geising et al., 2000)."

"Free-living rates of fixation in rocks and soil are lower than symbiotic ones, but the widespread distribution of cryptogams, and the capacity of such organisms to respond rapidly to change, means that this functional group is globally important, perhaps accounting for up to 50% of natural terrestrial N fixation (Elbert et al., 2012)."

Lines 272-273. The increase in rock weathering expected with plant growth at elevated concentrations of atmospheric CO₂ may be slight, inasmuch as plants and soil microbes normally maintain pCO₂ at high levels in the soil pore space, so the increment with rising atmospheric CO₂ is likely to be small, although nevertheless significant over geologic time (Andrews and Schlesinger 2001).

We agree that the direct changes in soil pCO₂ will likely be small under elevated CO₂, but the integrated effect of higher belowground C allocation contributes to enhanced root production, reactive mineral surfaces, and acidity that can significantly increase weathering rates at both short and long timescales. We've altered the text reflect this combined effect on mineral weathering.

Line 341. Presumably the authors mean “catenae” Line 354.

Fixed.

Line 354. The huge pool of nitrogen in the Earth's crust is not so easily interpreted to indicate that N-fixation has exceeded denitrification through geologic time. Some of this nitrogen may have never circulated in the biogeochemical cycles at the Earth's surface. Rather, it would be interesting to estimate the amount of nitrogen being subducted in sedimentary rocks passing into the Earth's mantle versus the amount that is being degassed as juvenile nitrogen by volcanoes. Recent estimates suggest that volcanic emissions (78 to 123 x 10⁹ gN/yr) are less than subduction (330 to 960 x 10⁹ gN/yr), suggesting a net entrainment of N in the Earth's mantle. See Schlesinger and Bernhardt (2013, p. 456) for references.

We acknowledge that there may be some confusion here because Mantle fixed-N reservoirs may be substantial, and do not contribute substantially to surface (biogeochemical) N cycling. However, excluding Mantle reservoirs, the 99% statement still holds.

Further, 75% of the continental crust N reservoir is found in sedimentary and meta-sedimentary rocks, and most of this N derived from burial of organic matter in Phanerozoic rocks (See Bebout et al, 2014). The N content of the early continental crust was likely on the the order of 10¹⁶ kg (assuming the N content of the upper mantle is good analogy for early continental crust; see Marty 1995 - Nature), while the modern N reservoir in continental crust is 10¹⁸ kg, an increase of two orders of magnitude. While a fraction of crustal N may have originated prior to the rise of biological N fixation (See Goldblatt et al. 2009 – Nature Geoscience), the majority of this N has accumulated as a result of organic matter burial in sedimentary rocks derived from terrestrial and oceanic N fixation.

With respect to N exchange with the mantle, the net transfer of N from earth surface + crust reservoirs to the mantle is estimated at 0.9 Tg yr⁻¹ (see Busigny et al,

2011), and has been hypothesized (controversially) to be substantially larger during early earth evolution (Goldblatt et al, 2009). However, with respect to a planetary N cycle, the primary imbalance appears arise from the 15 – 35 Tg N that is buried in marine environments annually. Given that combined volcanic emissions are $\sim 0.5 \text{ Tg yr}^{-1}$, more than 90% of the N flux into the crust reservoir must be recycled to the biosphere to maintain atmospheric N reservoirs over geologic time.

New text reads:

“Rocks contain ~99% of Earth’s fixed N (Schlesinger, 1997), even when excluding mantle reservoirs that interact sparingly with earth surface processes (Bebout et al., 2013). Approximately 75% of the fixed N reservoir within the continental crust is found in sedimentary and meta-sedimentary rocks (Goldblatt et al., 2009), primarily reflects higher rates of N fixation compared to denitrification to N_2 gas over Earth history. Nitrogen concentrations are much higher in sedimentary/meta-sedimentary than igneous parent materials, though either class can contain appreciable geological N (Holloway and Dahlgren, 2002). Further, reservoirs of geological N can occur as silicate-bound NH_4^+ , organic-N in sedimentary organic matter, or nitrate in evaporites. Variation in both the amount and form of rock N is controlled by local depositional environments, the degree of biological and thermal diagenesis in sedimentary basis, and the degree of N volatilization during metamorphism (Bebout and Fogel, 1992; Hedges and Keil, 1995; Hedges et al., 1999; Boudou et al., 2008). Rock-bound nitrate can be seen in desert/arid ecosystems where hydrological losses are minimal and nitrate accumulates at depth or in the surface of caliche deposits (Walvoord et al., 2003). On average, Holloway and Dahlgren (2002) found that the parent material factor is a strong driver of rock N contents, with trace amounts of N found in cratonic assemblages to $>20,000 \text{ ppm N}$ in sedimentary rocks such as coal. Generally, N enrichment is highest among fine-grained siliciclastic rocks (i.e. shales, mudstones), and their low-grade metamorphic counterparts (i.e. slate, phyllite, and mica-schist). These rocks comprise ~30% of earth’s continental surfaces (Durr et al., 2005) and have an average N concentration equal to $700 - 1000 \text{ mg N kg}^{-1}$ (Goldblatt et al., 2009; Morford, 2014).”

Line 419. Schlesinger et al. (1998) document phosphorus deficiencies in recent volcanic soils on Krakatau. These do not appear to be related to low P concentrations as much as to a stoichiometric deficiency of P relative to high concentration of N that was accumulated in these soils by cyanobacteria, which are reported to have colonized immediately after the 1883 eruption.

Thanks, this is an interesting paper that escaped our radar. The high rates of N accumulation in Krakatau seem to be consistent with free-living fixation, although Schlesinger et al.’s 1998 analysis is inferential rather than direct. We have modified the text as follows:

“In both newly formed volcanic (Vitousek, 2004) and de-glaciated sediments (Chapin et al., 1994) cyanolichens are among the earliest colonizers, with direct and indirect evidence for significant free-living N fixation rates in fresh parent material (Schlesinger et al., 1998; Crews et al., 2001)”

Line 486. These air-borne agricultural losses of N represent an input to ecosystems that are downwind, but globally they merely represent a recycling of N added to the land surface by the application of industrial fertilizer

Agreed. New sentence added to text:

“From a global budget perspective, agricultural emissions of NO_x and NH₃ comprise a large-scale recycling term, despite representing a new N input to downwind ecosystems.”

Reviewer 2

Authors' response in bold

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 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., “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.

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 solely 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 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 $\delta^{15}\text{N}$ 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 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 intro 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 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.

We appreciate the reviewer’s philosophy and suggestions. We realize that we did not adequately explain what we meant by the new synthesis. This, as the reviewer discovered, made it seem like the main point of the manuscript was to adopt the state-factor approach to N inputs. Although we believe that the state-factor framing is useful, as we detail in the subsequent paragraphs, this is not by itself the major advance or thesis of the manuscript. Let us be clear: The new synthesis refers to the explicit inclusion of both rock and atmospheric N inputs to terrestrial ecosystems. This is the most important point of our paper. We have made this point more clearly throughout the manuscript by substantially revising the text.

Prior to our paper, to our knowledge, neither textbooks, nor conceptual papers, nor process-level models have considered N inputs via rock sources in terrestrial ecosystem models. Moreover, a general state-factor framing of N inputs has been altogether lacking, regardless of the source. This stands in contrast to models for many rock-derived elements. Thus, the state-factor framing we provide is new with respect to N, though widely used in the past to understanding other element inputs.

We believe that the state-factor system is the best way to present the new synthesis for terrestrial N inputs for two main reasons. The first is historical. The state-factor system has arguably led to a richer understanding of soil nutrient inputs and cycling than any other single conceptual framing. It continues to shape our understanding of large-scale controls on element cycling, nutrient availability, and process based models. The essential feature of the state factor system is that, it necessarily argues for changes in the nutrient fertility of soils, which thus links it to the ecological system. Given that we interested in understanding the implications of N inputs, we believe that this state-factor system is the best framing for our new synthesis.

Another reason is that the inclusion of rock N sources is transformative. No other paper to our knowledge has made the case for inclusion rock N. In fact, this N input pathway has been consistently dismissed in the review literature. We thus believe that our couching of rock N inputs is ambitious and sufficiently forward-looking. Presenting our ideas within a widely adopted and highly regarded conceptual system as Jenny’s state-factors allows for readers to quickly assimilate and access our argument. Questions such as, how should I think about rock N inputs and

atmospheric N inputs? for example, are answered efficiently with the state-factor framing that readers will most certainly be familiar with. The state-factors cover the general conditions of the pedosphere and thus allow for an appraisal of where or where not rock N inputs would be expected to be important.

Moreover, the question of relative important and interactions is operative in our new synthesis. A process-based focus would reduce this aspect of our analysis. Separating N inputs into N fixation, deposition and weathering processes has the side-effect of reducing the potential to find commonalities and interactions between N inputs. We cite cases and discuss how N inputs can affect one another, and believe that this is a significant advance. This interactive model would be less obvious by focusing on a given process (e.g., weathering, deposition, fixation) in isolation.

We recognize that conceptual papers or syntheses can be written in different ways. We have published papers using both state-factor approaches and process level understanding and modeling in the past. We do not view these approaches as cases of either/or. Rather, we believe that these approaches are complementary. We have written several lines of text and new paragraphs to make clear that we encourage process-level analysis of N inputs. This step, along with clarifying what we mean by the new synthesis, makes our paper better and we appreciate the reviewer's suggestion, which is best summarized in the new paragraph in the discussion:

“The state-factor approach we adopt points to testable predictions for patterns and magnitudes in N input pathways, but it by no means should be taken as the only approach to understanding atmosphere, biosphere and geosphere effects on the N cycle. We view the state-factor model as a powerful, integrative tool that offers useful sets of concepts to help guide experimental research in the Earth system sciences. It is historically important to soil and ecosystem science, and in the case of N, places this element in a similar lens as the classic rock derived elements. The weakness of the state-factor approach lies in the lack of quantitative predictions of N input kinetics and the absence of focus on individual processes. We suggest that process-based model development should go hand-in-hand with state factor approaches to understanding the new synthesis. This combined approach has proven quite effective for understanding weathering of the classic rock derived elements. For example, work along a set of chronosequences has been used to develop process based models and quantitative predictions of P inputs and limitation patterns globally. We envision parallel activities, in which state factor assessments are combined with an examination of reaction kinetics, particularly N fixation and N weathering kinetics in controlled settings, give rise to a more general understanding of N inputs and process-based modeling of the terrestrial N cycle. Experiments that evaluate the kinetics of chemical weathering vs. physical erosion will be vital to determining the availability of rock N sources to terrestrial biota in particular.”

In the revised introduction (several lines of text, including):

“Here, we argue for a new synthesis for terrestrial N inputs that explicitly considers both rock and atmospheric sources of N. We review evidence for atmospheric vs. rock N inputs within the ecosystem state factors model to address the diversity of N input patterns and magnitudes among Earth’s terrestrial environments . We use case studies, consilience and analogy to present a new era of soil N cycling research issues and opportunities. We make reference to elements other than N (i.e., P and cations) to infer likely patterns of rock N weathering inputs where research is less well-developed. We also discuss implications of the new synthesis for conceptual nutrient cycling models, terrestrial C storage, patterns of soil fertility, climate change feedbacks, and widespread changes to the global N cycle via human actions. “

In the section titled “State factor approach”:

“We adopt the classic “state-factor framework” to build toward a more comprehensive understanding of N inputs in terrestrial ecosystems. We emphasize regulation, pattern and interaction of N inputs with soil pattern and process, across local, landscape and global scales. Our approach takes advantage of Jenny’s system (1941), which has been applied widely to other nutrients (e.g., Vitousek, 2004), wherein five ecosystem state-factors are used to understand soil fertility and pedogenic patterns across the Earth system. The five factors include parent material, climate, organisms, topography (or relief) and time (Jenny 1941). In addition, given the importance of human actions on Earth’s biogeochemistry, we include an anthropogenic factor in our analysis here, consistent with previous calls for this sixth factor (Amundson and Jenny, 1991).

Our review is not necessarily deep into any given N input path; for in-depth reviews on N fixation see Vitousek et al. (2002) and Reed et al. (2011); N deposition see Lovett (1994) and Lamarque et al. (2005); and rock N chemistry see (Holloway and Dahlgren, 2002). Instead, 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. We further stress that there are other important approaches beyond those steeped in the tradition of Jenny’s framework, particularly the widespread development and application of process-based models in the biogeosciences. We thereby point out several cases in which process-based models have been developed to examine patterns of soil nutrient availability and how such models both build and advance the state-factor framing used here.”

And other parts of the manuscript, for example (Organisms section):

“Using process-based modeling and mass-balance approaches, Cleveland et al. (2013) proposed a positive correlation between the abundance of Fabaceae and ecosystem-level N inputs in primary forest sites in central Rondonia of the Amazon Basin.”

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 overturn it by recognizing the role of erosion of depleted soil and weathering of fresh rock in rejuvenating nutrients.

We understand this point and have changed the references and added text to reflect the role of disturbance.

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.

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

We have clarified the text on multiple inputs as being rock and atmospheric sources.

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?

See our reasoning for organization above.

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.

We have modified the text to highlight that we are applying the state-factor approach in our synthesis.

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.

See response above. We reference process level understanding throughout the state-factor system, and discuss cases where the state-factor approach is not adequate. Table 1 provides a set of testable alternative hypotheses with directions indicated. We think that this more than justifies the state-factor framing, as it give the community a way to address the questions presented, and advance the field.

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

We make several points about the usefulness of state-factor approach in the discussion, in the Table 1 summary, and in the final Figure. We have further modified the text to clarify these points, and in the general response-text above, also indicate that process is also important and necessary to advancing the new synthesis.

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.

We discuss many different controls and processes involved in weathering reactions throughout the manuscript and newly revised text. See response above regarding state factor framing.

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).

Our discussion of the organism factor focuses on the presence (and absence) of N fixing organisms, different plant functional types (e.g., conifers vs. broad-leaf trees), plant fungal associates, and the specific processes involved in all three N input paths. . We have clarified the text so that it is clear that climate cannot adequately explain any of these organism effects on N inputs.

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.

This a good point. At large scales, erosion is set by landscape relief, but soil production & erosion curvature dependent at local scales. We've modified the text to clarify these scale dependencies.

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"

This is addressed in the new text rather than a separate Conclusion statement:

"We envision parallel activities, in which state factor assessments are combined with an examination of reaction kinetics, particularly N fixation and N weathering kinetics in controlled settings, can give rise to a more general understanding of N inputs and individual process-based modeling of the terrestrial N cycle. Experiments designed to evaluate the kinetics of chemical weathering vs. physical erosion will be vital to determining the availability of rock N sources to terrestrial biota."

We note that we make reference to useful experiments throughout the text, especially in the Discussion, with a summary of Hypotheses in the Table, rather than doing this as a separate section or "Conclusion", thus sufficiently addressing new research opportunities for the community.

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.

We've adjusted the text to reflect that weathering and erosion rates are slope dependent at summit positions, and to better define flat, non-eroding, summit positions where weathering rates are low.

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.

We've added text to clarify that 75% of this N is found within sedimentary and meta-sedimentary rocks, which are the dominant lithology across the earth surface. Further, we discuss how N enrichment is highest among fine-grained siliciclastic rocks which comprise ~30% of parent materials across the continental surface.

Text now reads: "Approximately 75% of the fixed N reservoir within the continental crust is found in sedimentary and meta-sedimentary rocks (Goldblatt et

al., 2009), primarily reflects higher rates of N fixation compared to denitrification to N₂ gas over Earth history. Nitrogen concentrations are much higher in sedimentary/meta-sedimentary than igneous parent materials, though either class can contain appreciable geological N (Holloway and Dahlgren, 2002). Further, reservoirs of geological N can occur as silicate-bound NH₄⁺, organic-N in sedimentary organic matter, or nitrate in evaporites. Variation in both the amount and form of rock N is controlled by local depositional environments, the degree of biological and thermal diagenesis in sedimentary basins, and the degree of N volatilization during metamorphism (Bebout and Fogel, 1992; Hedges and Keil, 1995; Hedges et al., 1999; Boudou et al., 2008). Rock-bound nitrate can be seen in desert/arid ecosystems where hydrological losses are minimal and nitrate accumulates at depth or in the surface of caliche deposits (Walvoord et al., 2003). On average, Holloway and Dahlgren (2002) found that the parent material factor is a strong driver of rock N contents, with trace amounts of N found in cratonic assemblages to >20,000 ppm N in sedimentary rocks such as coal. Generally, N enrichment is highest among fine-grained siliciclastic rocks (i.e. shales, mudstones), and their low-grade metamorphic counterparts (i.e. slate, phyllite, and mica-schist). These rocks comprise ~30% of earth's continental surfaces (Durr et al., 2005) and have an average N concentration equal to 700 – 1000 mg N kg⁻¹ (Goldblatt et al., 2009; Morford, 2014).”

Among alluvial floodplains (~15% of continental surface), rock N inputs would be expected to be lower than moderate-to-high relief environments, owing to both 1) low rates of rock exhumation, and 2) as the reviewer suggests, deeper weathering profiles that may disconnect surficial ecosystems from zones of rock nutrient inputs.

We make this point in the new text: “Similarly, rock N inputs in low-relief landscapes are expected to be substantially lower than moderate-to-high relief environments owing primarily to low rates of rock exhumation and denudation. The development of thick zones of saprolite/regolith weathering in some low-relief landscapes may also result in rock N weathering at depth within the critical zone, beyond the reach of plant life.”

However, we also note that the depth over which plants, particularly trees, can acquire water and nutrients is poorly constrained, and may extend deeply into the critical zone, as recent work suggests (Oshun *et al.* 2014). Further, these alluvial environments also receive periodic inputs of sediments that may contain rock N. Thus, the rock N contribution to these ecosystems cannot be automatically discounted, but are clearly smaller than landscapes with higher denudation rates.

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

We have added citations about parent material.

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.

We have modified the text. We agree that the state-factor framing is a way to organize the concepts but not our 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?

Change made so that it is clear that we are presenting the new synthesis here.

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.

We agree that this by itself does not justify the state-factor framing of the new synthesis. See response above.

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.

See reasoning about state-factor framing and inclusion of process-level understanding above.

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?

We've update the figure labels and legend to emphasize the important distinction between eroding and non-eroding landscape end-members. In the updated legend, we address how long-term rock N inputs can be substantial among eroding landscapes with N-rich parent material, but are otherwise either important only among landscapes w/ inputs of fresh bedrock when a rock N reservoir exists.

Technical comments p 499, l 14: demonstrative is the wrong word

“demonstrative” deleted.

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.

Change made through deletion (see above).

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

We’ve altered to the text to distinguish between convex and concave topographic positions, corresponding to shoulder and footslope/toeslope positions, respectively.

“At local scales, erosion and soil production (conversion of rock to soil) is proportional to slope and is linearly correlated with the negative curvature of topography (Heimsath et al. 1997, Roering et al. 1999). Both weathering rates and erosion in hillslope sequences are generally highest at convex positions near ridgetops (i.e. shoulder positions), contributing to accumulation of weathering products in concave positions lower in the landscape (i.e. footslopes and toeslopes, (Milne, 1936; Gessler et al., 2000; Yoo et al., 2007). Consequently, the highest rates of weathering become spatially decoupled from where nutrient accumulation (and putatively plant nutrient availability) is greatest (Yoo et al., 2006; Yoo et al., 2007).”

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

“Classical” removed from sentence.

References:

Oshun, J., Dawson, T., Dietrich, W., Rempe, D., Fung, I., (2014), Critical zone co-evolution: evidence that weathering and consequent seasonal rock moisture storage leads to a mixed forest canopy of conifer and evergreen broadleaf trees. Abstract H53K-06. Presented at 2014 Fall Meeting, AGU, San Francisco, Calif., 15-19 Dec.