Editor/Reviewer Comments  
Author Response  
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Reviewer: 1

Comments to the Author

This manuscript addresses the relationship between crop yield and Nitrogen response.

The paper is well written, but it has significant deficiencies and many confounding factors in regard to different data sets involved in this study. The general observations I have about this paper are:

1. The abstract is not very informative and lack any rationale to approach this research question. Also, it lacks many details about the studies involved and the implication of this work.

The abstract has been modified to change the “Rationale.” You are right to critique the lack of direct “implications” of this work. We have diluted this down as best we can to conform with requests coming from other reviewers who felt we went over the top. Hopefully the final sentence of the abstract (conclusion) is now acceptable to all reviewers.

2. The literature review is disjointed and lacks any coherent articulation of the research question in addressing the proposed objective of this work. I do not see any creative synthesis of previous research that can be linked to the research presented in this paper. For example on page 2 (the manuscript pages are not numbered) line 9 to line 10 on page 3, I found this text lack any creative or coherent presentation. It is just merely list of different articles and what they did. How this support the rationale of this work. That is the question the authors need to articulate.

Structure of the literature review has been altered to better reflect how this addresses the rationale that has been put forth. The first section was restructured. Do not agree with the comment that this was disjointed since 3 sections were clearly labeled, and addressed, and all 3 directly tied back to the objective.

1. Importance of Yield Potential for making N recommendations
2. Importance of N Responsiveness for making N recommendations
3. Nutrient Management Theory

3. The Materials and methods need significant rewriting, where a lot of details about these experiments were not presented, such as, what kind of experiment design was used and replications, what tillage system was used, weed control, equipment used, pest and disease management, planting dates, plant populations, irrigation management and system for the irrigated site, time of N applications, weather data for each site, just to name few. These managements will affect crop response to N fertilization differently in different sites.

Tillage, experimental design, and reps are included in Table 2. Each of these trials included references that elucidate all management variables. Irrigation was only used at the Shelton, NE site and that was indicated.

4. Under materials and methods on page 6, line 18-24 the N rates that were listed for 5 sites, the Arlington site (1984-2007) is listed wrong for the RI value and not consistent with other sites.

Thank you for the correction. Rates were flipped for Arlington (1984-2007). Iowa sites have also been added.

5. Results: I found the results confounded by site and genetics, where different sites with different environment were used and I am sure they have different management practices. Within each site, the results of regression analysis presented in Table 4 show large CV for the majority of the data from all sites. This will raise questions about the confounding effect of different variables, such as different management, different environment, varieties and hybrids differences, N sources, N application methods, etc.

Management practices are better delineated in Table 1. The CV’s that ranged from 8 to 46 (generally higher for maize than wheat), for large field experiment work are within the ranges seen in most large scale field work. Within site changes in environment, hybrid and variety are obvious issues that we deal with in long-term trials.

This raises doubt about the validity of using N rate indicator rather using soil N supply as an indicator for N response or lack of it, where many soil factors are in play. The interpretation of N responsiveness based on Applied N rate only is misleading. The potential supply of N by certain soil through mineralization process play larger role in N supply than just using N rate as an indicator, where soil supply will be highly affected by soil moisture and soil temperature. It is not a surprise to see poor correlation between yield and N rate indicator (RI).

There was either a response to fertilizer N or there was not (adequately fertilized versus not fertilized, or fertilized at a lower rate). We do not agree with the reviewer that this is somehow misleading. We do agree with the reviewer that N supply via mineralization can play a large role, and that is influenced by the environment (moisture, temperature). That the reviewer recognizes that it is no surprise to see poor correlation between yield and N responsiveness is precisely the point. They are not related and this for the reasons you point out. And this is also why the demand for fertilizer N will change from one year to the next, even if yield levels were the same. Shouldn’t I estimate this if I can, using mid-season sensor readings? (if it were possible?) . Yield levels just like N responsiveness will change from one year to the next too (environment). And if I could estimate yield potential, mid-season (understanding that it changes a lot too, from year to year), using sensor measurements, wouldn’t I want to look into this as well? (if it were possible?). That YP0 and RI are not related is an important finding over many locations, and many years, because it clearly points out that they must be dealt with separately (if indeed it is possible).

6. The development of RI is a good tool, but variability in field conditions and water availability makes difficult to articulate an overarching conclusion of the research. Also, what is the take home message where results are confounded by differences in environment, management and N source? It is well know that N use efficiency for most crops does not exceed 50%. Therefore, developing N response is more complicated than just correlating yield with RI.

Excellent point and we concur. We have not stated anything that would say otherwise. Yield level and RI and both a function of the environment. We use them as tools, but they have to be tools that are independent of one another. That N sources and other variables influence both yield level and RI is not disputed. Using data collected by many institutions, we further understand that even within a specific location, there will be issues that “confound” our ability to estimate RI and yield for that matter. Nonetheless, the overarching question, (are YPO and RI related) remains useful, and the comprehensive amount of data presented can be used to answer this question.

7. It will be worth the effort if the authors include data on N mineralization to better understand the potential N supply of soil at each site and how that affects N responsiveness.

This is a nice suggestion, but not possible. First we don’t have N mineralization data, and second this isn’t an N mineralization study. Estimates of mid-season nitrogen responsiveness are presently possible using sensor-based estimates. But they rely on documented benchmarks, like long-term studies where NDVI readings can be taken (or N Rich Strips that have been widely adopted). We have published many papers on estimating N responsiveness using NDVI readings (mid-season), and several of these are referenced (Mullen et al., 2003, Ortiz-Monasterio and Raun, 2007, Raun et al., 2002).

Reviewer: 2

Comments to the Author

I have reviewed AJ paper "Relationship between Grain Crop Yield Potential and Nitrogen Response". An uploaded pdf file has specific comments using "sticky notes" and I highlighted in yellow text relevant to the comments or suggestions. Therefore, here I make general comments. I wrote the comments on the paper as I was reading it, so there may be some duplication of questions or comments. But this should help the authors understand questions that could arise as the paper is read by readers.

I think that this is a great and hot topic and a potentially great paper, which deserves publication in AJ after the authors consider some of my most relevant comments at least, because I am convinced this should improve the paper and its interpretation by readers. My main observations, relate to three or four issues.

1. The authors talk about "accepted theory" from the start, but never really say or explain what this theory is, and provide little or no proof, just general statements about previous authors' findings. In the first sentence of the abstract, and with some different words somewhere else, they say "Cereal grain fertilizer nitrogen (N) recommendations should conform to accepted theory". This is a philosophical issue, but I disagree with this statement. A theory by definition is a theory, not a proved "law". Therefore an "accepted theory" by some in the scientific community may be wrong or incomplete.

This is an excellent point. Text addressing “accepted theory” has been deleted. Instead have focused on what we found and that is relevant to the topic.

1. Moreover, as Carl Sagan used to say, new evidence may contradict and proved a theory wrong, and in the agronomy or nutrient management area if this evidence has strong support it can be used to guide N fertilizer recommendations, until the "accepted theory" is modified or a new theory is put in place. This is a problem with some hot N issues: Some scientists put their feelings or theories ahead of experimental evidence. Obviously this is not the case for these authors and paper, so they should use better verbose to make their good points more clear and acceptable by readers.

Reference to accepted theory deleted as per your comments

A major problem out there; at the level of producers, crop consultants, extension agronomists, and researchers; is confusion about terminology concerning yield level. Many use interchangeably terms that have or should have very different interpretations. I will not get into explaining this, because I am sure the authors do have the concepts clear, but they confuse matters further by sometimes imprecise use of different terms. I made comments for several sentences of the paper. Given this paper aim and characteristics, it is important that when authors refer to published research, they explain well what "yield" those studies referred to, if to potential yield, yield goal, or actual yield levels. Then, of course, they should precise about what "yield" they refer to for their own past or present research. Usually they identified well what they are talking about, but sometimes it is confusing, at least for me.

This is an excellent comment. We have tried to clarify specific terminology when “yield” was used in this paper and that references grain.

A main problem with concepts about N management, which is at the core of this paper, is confusion among scientists (yes!), extension agronomists, and crop consultants about impacts of "yield level" (potential, goal, or actual) on crop N demand on one hand and on optimum N rates or responsiveness to N on the other hand. These are two VERY different things, and the authors have this clear. There is universal agreement in the the "yield level" influences crop N demand. This has been proved by so many experiments, although the exact relationship is affected by genotype, site, and climate variables. Study of the factors affecting these relationships is of interest, because it may provide clues about differences in N uptake, removal, N use efficiency and, PERHAPS about optimum N rates. The latter is the hot issue. Many recent studies have shown that the "yield level" is not well related to crop responsiveness to N application (as the authors proved) or to observed optimum N rates across sites and years (which the authors could have included in the paper). The problem I have with the way authors wrote some sections of the paper is that they do have strong support for a lack of relationship between yield level and responsiveness, but they "accept" as a fact that yield level is useful to determine N rate.

I don't think that is a fact, and should not be taken as a fact as they did. It is true that yield level relates to N demand, but extending this to say it is useful to determine N rate is a completely different thing. This is where the major discrepancies out there are, and the hot issue. Let me be more specific:

This is another excellent point. We have modified the text to indicate that N demand can vary even at the same level of yield at the same site, but in different years (Arnall et al., 2009)

- When referring to previous research, they paraphrase the author's conclusions but do not explain or dig in detail what was done and how their methods and results support their conclusions. I know this is hard, but have to do this; they have no choice if they want their own study to be taken seriously. For example, when they write: "Lory and Scharf (2003) concluded that fertilizer recommendations that ignore yield entirely are likely limited to explaining less than 50% of the variation in the economic optimum N rate". Well, how they came up to that conclusion, what "yield" were they talking about, how was "yield" considered, what does "likely" mean (that they did not have strong support for that statement?)?

We are not in total agreement with the reviewer on this comment. We stated precisely what these authors reported. In order to give the authors of this paper the “benefit of the doubt,” we added “likely” that you have flagged. This has now been deleted, and makes their exact statement as emphatic as they originally reported.

Another example: "They [Spiertz and De Vos (1983)] further reported that an accurate assessment of the potential yield for different growing conditions would improve N fertilizer recommendations." Oh yes? But what was they did, what "yield" they used, what crop, in what conditions? I am not convinced this is the case in all conditions.

That entire paragraph (2 sentences) came directly from this work. The crop was stated in the first sentence (winter wheat). We have added that this was done in the Netherlands.

We have a large amount of data in our state that proves this IS NOT the case for corn at least in our state. If the authors want their own results and interpretations to perhaps establish a new theory or match new evidence with existing theory or assumptions, they need to explain better what others did and how they came-up to those conclusions. No, I am not saying they should re-interpret their conclusions, because would be "politically incorrect" and would create problems, but they have to explain things better and they could qualify the conclusions if needed.

We have added data from two long-term studies in Iowa, and this thanks to Dr. Antonio Mallarino. Their data supports what was included in the original manuscript.

- The authors complicate things further, and I can't understand why, by repeating several times (I approximately quote) that since research showed that yield potential influences the crop demand for N and current study demonstrates that yield level is independent from N responsiveness, then both should be used to determine N fertilizer rates. Their study DOES NOT show that using yield level, which of course relates to N demand, really helps determining optimum or maximum N rates. Their analysis of previous research concerning yield level influence value for N recommendations is superficial, so why do they accept that as a fact? They DO NOT HAVE to accept that "yield level" is useful to determine N rates to make their study worthy of publishing. The very important supported conclusion that yield level is not related to N responsiveness would be sufficient. So why they have to say "both yield levels and N responsiveness" need to be considered? They had a beautiful opportunity to make a great point by focusing on their own study, and the they found no relationship between yield level and responsiveness to N, and I bet they would have found no relationship to optimum or maximum N rate either. This takes me to the next point.

We have made reference to the need for both yield potential and N responsiveness for fertilizer recommendations, but have done so, clearly pointing out that this is our view. With 261 site years of data, we believe that a summary statement and/or some kind of distillation of the findings is in order.

- All experiments had several rates, so why didn't the authors estimate the optimum N rate or the rate that gave the maximum yield, for each year? Even a simple comparison of yield for the rates used, not even an ideal modeling for each site and year, could have been enough as a general approximation. The authors could have greatly improved their study with just three graphs, one for each site showing the relationship between yield level and the optimum or maximum N rate across years. Including this may need some extra weekend work, BUT WILL BE SO MUCH WORTHY, and at the same time would really make a difference from the previous paper by the authors.

The reviewer is astute wanting to see graphs. We have obviously looked at all of them. But, 36 graphs are not appropriate. We all know what linear functions look like and we all know how to interpret their significance. Because of the sheer volume, we chose to report what you have requested in one, rather lengthy table. We believe that the latter is more meaningful and that condenses the findings.

In summary: This paper could become much more relevant and readers would interpret it much better by organizing better the text flow, define some things better, and avoid referring to things that their study did not address or support, such as that using "yield level" is useful to establish N rate recommendations. I don't think the study of the relationships between actual yield level and optimum or maximum N rate that I suggested is essential. But the authors should agree with me in that a week or ten days of additional work on that would make this paper MUCH stronger.

We agree with the reviewer, and thank him for his time and effort devoted to this paper. We have taken his comments to heart, evidenced in having sought additional data, and qualified authors to review and critique this work.

Reviewer: 3

Comments to the Author

Overall

Authors made very little changes base on reviewer’s recommendation. I think this paper can benefit from the very good comments provided by reviewers. However, authors focused on providing answer to each comment in the response letters and often without making the necessary changes in the manuscript. For example, one reviewer ask for the significance level, and authors provided a response in the response letter, however didn’t include that information in the materials and methods (readers won’t have this information). In the document sometimes authors use a p-value of 0.05 (P7, L8), and sometimes p-values of 0.01 or 0.10 (P7, L22). I think the reader would expect some consistency.

Failure to include the significance level in the paper was clearly our mistake. Consistency of probability levels has been addressed. Linear models with a slope significance of pr>|t| less than 0.05 were considered to be significant.

In other cases the authors indicate in the response letter that changes were made base on reviewers’ comments, when they didn’t change anything. For example P6, L15.

From the previous review we included

(introduction of semidwarf varieties)

I think this is good information that would be of interest for readers of AJ, the comments provided by reviewers would contribute to improve the paper. I don’t think authors need to try to convince readers that the yield goal approach is not the best for N recommendation (I think everyone knows already). Some states may still use this approach and may not be necessarily because they believe is the most accurate approach (so far may still be more practical and better than not using anything). So in my opinion authors shouldn’t worry about N fertilization approaches and focus on the objective of this paper. However authors focus their repose on this issue, and often ignoring the suggestions by reviews that can help to improve the paper.

This review and others were excellent. Failure to address requested changes has hopefully been taken care of.

The main conclusion is that: “algorithms for accurate mid-season fertilizer N rates will thus require the inclusion of both potential yield and the response index as independent variables”. How is this different from current algorithms used to estimate N in-season N rates?. Most (if not all) algorithms are already using estimated yield potential and RI using N-rich strips. As indicated in the previous review the paper need to focus on novel information; or relate this information on how are current algorithms working and what need to change (maybe nothing) base on these results.

The reviewer makes a good point. But we are now caught as one reviewer says that we cannot say anything about using both yield potential and RI to estimate N rates, and another says this is not new or novel. Hopefully this modified version is acceptable, understanding that we do agree with what you have pointed out.

Specific comments

P1, L4. The first line in the abstract refer to fertilizer N recommendation “theory”. Is this really what you are evaluating in the paper? Evaluation of recommendation theories? Or the next sentence? “evaluate the relationship between yield potential (yield level) and N responsiveness”. There is a disconnect between the introduction sentence of your abstract and there objective of the paper. Please make changes accordingly.

This has been changed and issues associated with theory have been deleted.

P3, L 5-6. This sentence regarding SPAD meter is contrary to P2, L17-18. Please describe these findings about SPAD in one paragraph.

Added text delineating this work now included.

P4, L3. Please change this to “nitrogen fertilization theory” change made

P5, L9. Please delete ”The results of these experiments were unexpected and controversial”.

deleted

P5, L10. Please state a clear objective, the objective in the abstract is probably more appropriate to what you are doing, since this is an independent paper from Raun et al (2011), with new data.

Objective added

P6, L23-24. Authors provided much detailed information in the response letter regarding statistical analysis. This information is very important for readers, why is not included in the manuscript? Please provide detailed information for the reader here.

Now included

P7, L11. Delete “was observed”

P7, L, 22, what is the significance level you are using to consider “significant” here you mention 0.01, and 0.10, earlier was 0.05. Please be consistent.

Modified as per your comments

P7, L23-24. Analysis across sites was not presented in the tables (only by site). “Overall sites, no consistent relationship between N responsiveness and year was found” please change this statement.

Modified

P8, L13-21. Please show the data for all sites, you have the data, no reason to show only one site that seems out of place here.

All sites have been included, added Figures 2-8.

P9, L3-6. Please change the questions to clear sentences indicating what is your interpretation/statement.

Changed accordingly

P9, L15-17. Please provide a reference.

included

Tables.

Table 1. Please use the appropriate symbols for table footnotes (no numbers for AJ). Same for all tables. Other tables will need these footnote symbols.

Page 12, Handbook and Style Manual, Footnotes “… also acceptable is placement at the bottom of the manuscript page where the footnote is called out. Use a superscript numeral 1 for citation (not asterisks or letters).”

For table 1, you can add another column with the reference. Please delete the column “Trial” (same for other tables).

Trial column deleted

I assume the number in parenthesis (years included) are the number of years, please use footnotes here.

This is now delineated in the table

Please combine tables 1 and 2, no need for two tables

Other reviewers asked for additions to Table 2 and that have been included. At this point with this paper, we can’t seem to please all reviewers (first and second review).

Table 3. there is no data for “Magruder” please delete that row or indicate in the table whey there is no data.

This was reported at the bottom of the Table

RI mid-N determined using a low or moderate N rate treatment as the denominator. No mid rate available for Magruder.

Table 4. please include the units you are using (Mg ha-1) ? included

In the footnote you describe “RI lowN” however I don’t see that in the table.

lowN was used in an earlier version. It is now RImid-N, and is reported at the bottom.

Why you have an empty row for “Magruder”? same as table 3.

Response above on Table 3 applies here.

Figures

Figure 1: please also include data for the other sites, one site alone seems out of place here.

All sites now included. Added Figures 2-8. Also 2 new figures (9 and 10).

Associate Editor: 1

Comments to the Author:

EDITORS COMMENTS:

I solicited three reviewers to look over this revised manuscript. One was a reviewer I had used in the previous round of reviews. One was a new reviewer chosen by me, and a third reviewer was one of the people recommended by this paper's authors. I received three excellent reviews back and based upon these reviews and my own reading of the paper, I am recommending a major revision. After receiving the revision, I will look carefully at the revisions to determine what steps to take next. Please note, that reviews should not always be viewed as adversarial, but are often written to provide suggestions to improve a manuscript. Both the repeat reviewer and myself noted that the resubmitted manuscript was changed little from what was originally submitted. That suggests to me that there was a feeling the reviews had little to offer in improving the manuscript. I do not think that is the case. I hope you can take these reviews and spend some quality time in considering how best to utilizer the review comments to provide an improved manuscript for publication consideration.

We have made a serious effort to make all changes requested. Justification is provided where we disagree.

The reviews were to recommend major revision and I believe that is an appropriate recommendation. One reviewer actually indicated that unless there is better focus on what is new or novel in this paper, it should not be accepted. Please respond in detail to each of the comments of the reviewers to help me understand why or why not these comments should be accepted and revisions made. I also suggest you number manuscript pages in addition to using line numbers to make it easier to refer to specific lines or sections of the manuscript.

Completed

There seemed to be general consensus that the topic of this paper is very important and would be of interest to readers of the Agronomy Journal.

There are several excellent suggestions by the reviewers, but here are some of mine.

1. Could you expand Table 1 to include more management data as suggested by one of the reviewers? These data are important as they provide a context of the responses measured.

Soil management (spring or fall) has been added for all sites

2. Abstract. This was confusing to me also. I could not understand how one sentence could almost immediately be contradicted by the next. I actually think it was just a matter of not communicating articulating your main hypotheses and findings.

Modified

3. With such a nice set of data, it seems to beg for combined cross-site statistical analyses and not merely presenting analyses site by site. The nice data set included in this paper is really a strength and is not taken advantage of like it could have been. In fact, the main points argued in this paper, if valid, should apply across all sites. Maybe statements of how results from individual sites differed from the combined data set would be interesting and actually add to the overall strength of this paper.

Added sites from Iowa have been included and that show the same lack of a relationship between YP0 and RI. Also included an over crop analysis (all wheat, and all corn)

I am not sure if this can be included in this paper, but I agree with the one review comment that suggests including some sort of soil N mineralization into the N recommendations would be a nice addition. This seems appropriate because often more than 50-70% of the N taken up by the crop is from the soil.

This is probably a good suggestion, but this data is just not available.