**Key:**Editor/Reviewer Comments  
Author Response  
**Text that was already in the manuscript**  
Modified Text now included  
  
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**EDITOR COMMENTS**  
  
(1) There was a focus on fertilizer N recommendation based on mid-season NDVI readings that was not the objective of this paper. The conclusion that mid-season estimates should be used to formulate N fertilizer recommendations was not supported by data presented in this paper.

This suggestion continues to be relevant and has been made, but is no longer stated as a conclusion.

If it is understood that both yield level and N responsiveness impact N demand, and are independent from each other as has been demonstrated, it is suggested that using both will assist in formulating N fertilizer recommendations.

(2) The stated objective was to “further test the null hypothesis of Raun et al. (2011) that yield potential and N responsiveness are related with additional data sets from long term maize and wheat trials.” Thus there was an immediate expectation that there would be new ground broken in the form of new data, new data analyses and new conclusions. In fact, the final conclusion seemed very similar to that of Raun et al (2011). This seemed disappointing given the expectations raised in the Introduction section.   
  
Finding that yield potential and N response are independent challenges current approaches used in the Central Great Plains. Because, recent work (Sawyer et al. 2006) states that “the results provide no clear indication of a change in N rates over time.” (page 11), and that is being extended, the importance that rates actually do change becomes increasingly important. We recognize that some are challenged by work that shows how important yield is and how important N responsiveness is, and most importantly, how radically fertilizer N rates change from year to year. One group currently suggest that there is no change in rates, but then go on to use an average! Why do they even bother with an “average” if there were no changes?

(3) Much of the literature reviewed seemed to be dated and recent literature was often missed or overlooked. For example, the use of yield goals to make fertilizer recommendations is no longer used by many agronomists, especially at state institutions.

Yield goals are still used, and are still relevant for the majority of areas where corn and wheat are produced in the world. Reviewers provided the Editor with erroneous information.

11/07/2012: The official N rate recommendation for Kansas (provided by Dave Mengel) is   
1.6 x yld goal - 20 x %OM - nitrate in profile - crop factor - manure - N in irrigation water

11/16/2012: Currently used N rate recommendation for Ohio (provided by Robert Mullen) is  
Yield goal \* 1.05 (lb N/bu)) - soybean credit (typically 35 lb) (assuming the ratio of corn price and N cost is 10)

11/14/2012 University of Nebraska,  
Current rec from Nebraska (provided by Richard Ferguson), has been unchanged since 2008. Here is the basic equation:  
N (lb/acre) = [35  + (1.2 x YG) - (0.14  x YG  x  OM) –( 8 x NO3-N) - other credits] × fA × fR  
  
(4) There was poor justification, or description, of the statistics used to analyze the data.

The stated objective was “to further test the hypothesis that yield potential and N responsiveness are related with additional data sets from long term maize and wheat trials.” Linear relationships were thus targeted.   
  
(5) One reviewer felt that a major flaw was that the paper assumes that the year effect on both yield and N response is random, when it is well know that corn yields have doubled over the interval being considered and that N use efficiency has increased. Not accounting for this could lead to some inappropriate conclusions.

We stated on page 7, lines 9-13 that “**The linear relationship between grain yield and year did show that a significant positive slope existed in 2 of the 6 data sets (P<0.05, Table 3). Both of these incidences were recorded at Arlington WI (1958 to 1983 and 1984 – 2007) where 16 different improved maize hybrids have been planted since 1986. This was expected since genetic yield potentials have increased over time (Hammer et al., 2009).**  
In addition, all three reviewers pointed out quite a number of editorial weaknesses such as poorly constructed sentences that were often unclear, the use of English units instead of SI units, and there were places where the ASA Style Manual was not consulted when preparing the references and tables.   
  
SI units were used throughout. The only place where English units were used was on page 4 (lines 12-14) to better communicate common values that are understood in the discipline. This is consistent with the Style Manual, Chapter 7, “For certain paper or publications, traditional English counterparts may be used along with the SI units.” We have done this, being careful to put the English units in parenthesis.

It seems like a revised manuscript could be reconsidered for publication but it would need to be a substantially different one from what was submitted at this time.

Reviewer(s)' Comments to Author:   
  
**Reviewer: 1**   
Comments to the Author   
Overall   
The objective of the manuscript is to “further test” the results reported by Raun et all (2011). I think this can be considered a valid objective especially if the authors are presenting new information that can perhaps dispute or further examine the results by Raun et al. (2011). The use of new sites and including “mid N rate” would be an addition. But I would also expect new statistical analysis or more in-depth evaluation of the data in addition to that of Raun et al. (2011), that can provide further evidence or show new results that were not demonstrated already.

The data included in this manuscript was all new, and not reported in the Raun et al. (2011) paper. Testing the use of the mid-N rate was also new. It is an important concept, and one that needs to be validated with additional data sets, particularly from other regions. This is precisely why it was so important to find that this exact same relationship (independence of yield potential and N response) was found at Wisconsin and Nebraska. Clearly this is “validation” data, and analysis. But, considering the stakes (methodology for making fertilizer N rate recommendations), knowledge of this, and knowing that it wasn't just one site or two, but all of them, and now in many states.

“New statistical analysis”… This just does not make sense. They are either related or not. Simple linear relationships are well understood and communicated. Even if a quadratic model would have been better, linear has to be significant first. This paper is not about a using a complicated, poorly understood model to say anything. It very simply documents that yield potential and N responsiveness are independent (not related), and linear models easily communicate whether they were related or not. For this work it is just a yes or no answer and further analysis only complicates the issue. If they are independent (not related), then they have to be used separately.

I think the authors have a great opportunity to do a comprehensive and in-depth analysis using a dataset that seems larger than that of Raun et all (2011), especially if the objective of this paper is to complete a more thorough “test” of the result obtained by Raun et al (2011).   
Some parts of the introduction, as well as the final conclusion are essentially the same as Raun et al (2011). With the use of new sites (and more sites) can you expand your conclusions?

This manuscript was written completely independent of the 2011 paper. The concept, is obviously similar but there isn’t anything in the paper that is “essentially the same.” This paper verifies that the concepts presented in the 2011 manuscript were valid, but using robust long-term data sets from other states (Wisconsin and Nebraska). Was “**band versus broadcast**” and/or “**agrotain vs \_\_\_\_\_**” only conducted in one state, and included in one manuscript? If the 2011 paper was right, work from other states need to either confirm or refute the concept. This remains consistent with the stated objective **“… to further test the null hypothesis of Raun et al. (2011) that yield potential and N responsiveness are related with additional data sets from long term maize and wheat trials.”** and while admittedly mundane, it is a concept that is structurally violated in current N recommendations in many mid-western states.

One conclusion was that mid-season yield estimates are needed for better formulation of N recommendations; however, this was not evaluated in this paper with the data presented. Documented in the introduction and discussion sections. 1. Yield levels influence the demand for N. 2. The demand for N changes every year (Scharf et al. (2005). 3. Knowledge of both can help to make better N recommendations.

**“Many research articles reported here show that both N responsiveness and yield potential influence the final demand for fertilizer N. Results from the 5 long-term experiments reported in this paper document that N responsiveness and yield potential are independent of one another. Algorithms for accurate mid-season fertilizer N rates will thus require the inclusion of both potential yield and the response index as independent variables.”**

The materials and methods section need significant work including more description of the long term sites.

Each site was referenced and further delineated in Tables 1 and 2. **“**Long term trials included in this analysis were long term wheat plots at Stillwater OK (Magruder Plots) **(Girma et al., 2007), a long term irrigated maize study near Shelton, NE (Varvel et al., 2007), a long-term dryland maize trial near Arlington, WI (Bundy et al., 2011), and two long-term dryland winter wheat trials near Altus, OK (Experiment 406, Experiment 407) (Raun et al., 1998).”**

The statistical analysis was not described, except indicating that “SAS was used”. Some questions are:   
-What procedure(s) in SAS?

“The linear relationships between grain yield, RI (mid-N and 0-N), and year using a fixed effects model were evaluated using **PROC GLM** (SAS, 2008).

-What type of liner model was used? (fixed, mixed?) the authors discuss the large uncertainty associated with year and the effect on yield potential, should “year” be used as random variable in the analysis across years?

The PROC GLM statement is based on a fixed-effects model. Year cannot be used as a random variable.

The use of linear mixed models (Proc mixed, glimmix) can be very useful.   
- Some years were not used due to the use of “improved genetics” how would this affect your results?

That entire sentence was “At Stillwater, OK, only years from 1958-present were included due to changes in yield potential as a function of improved genetics” and was in reference to the Magruder Plots that were identified. Those plots are unique in that they span years from 1892 to present. Semidwarf biotypes introduced in the late 50’s dramatically altered yields. Wheat produced prior to this introduction, had problems associated with lodging, and disease. Data from this time period was thus not considered to be relevant to modern day agriculture. Exclusion of this data did alter results as the reviewer implies (lower yield levels in a subset of data that clearly was different). However, this would not have altered interpretation (independence of yield potential and N responsiveness).

-What is the overall significance level used? 5%  
- Is regression analysis the best (and only) option to evaluate the relationship between these variables?   
- With basically not description of statistical analysis methods, is not clear how some statistics were calculated (tables 3 and 4). Statistics reported in Table 3 and 4 were taken from the SAS output.  
  
Some data interpretation (and discussion) seems inaccurate, for example some apparent significant increase in grain yield over time was ignored and discussion was focused on the lag of contribution of genetic improvements over time (P8 L1-9) and the uncertainty in yield potential. This uncertainty is real and expected, but trends in yield gains over time (maybe contributed by genetics) can only be observed over many years (despite year-to –year uncertainty).

Trends in yield gains over time were not ignored. We stated on page 7, lines 9-13 that “**The linear relationship between grain yield and year did show that a significant positive slope existed in 2 of the 6 data sets (P<0.05, Table 3). Both of these incidences were recorded at Arlington WI (1958 to 1983 and 1984 – 2007) where 16 different improved maize hybrids have been planted since 1986. This was expected since genetic yield potentials have increased over time (Hammer et al., 2009).**

We agree that there is clear uncertainty in yield potential, and that this uncertainty is real and should be expected. Trends in yield gains over time were thus evaluated and their importance properly reflected in the text.

Specific comments   
P1, L5. What theory? May want to consider changing this line, what is the objective of the paper? Introduction section of the paper delineates relevant theory (cited manuscripts) concerning the use of “yield potential” and “N responsiveness in making fertilizer N recommendations. Components of the abstract (rationale, objectives, methods, results, conclusions, Publications Handbook Style Manual, page 13) implicitly follow this order.

Evaluate the yield-RI relation or evaluate fertilizer recommendation theory? That somehow we cannot reflect on how the lack of a relationship between yield and N responsiveness impacts N fertilization approaches would be comparable to saying that phosphorus sufficiency indexes cannot be used to make P fertilizer recommendations. Enough.

P1, L7. Throughout the manuscript be consistent in the use of “maize” or “corn” Correction made

P1, L15-16. Is the data presented providing support for this conclusion? Is mid-season yield estimation better? I would assume yes, but need to present some evidence here.

Statement altered accordingly.

P2, L3-7. This paragraph seems out of place. This was included in the introduction of Raun et all (2011) but presented in a context that provided support for the paper, again here seems disconnected.

Sentence altered as per your suggestion.

P3, L6. “predict final grain yield”: this is probably referring to “estimating yield potential”

Sentence included accurately reflects the Teal et al. (2006) paper.

P3, L11-13 this is not clear need to be reworded.

Sentence reworded

P4, L4. “optimum N fertilizer rates” is this referring to economic optimum?

“economic” added to the sentence.

P4, L13. Delete “acre”

This was a direct quote, and changing would be inappropriate.

P4, L16, URL should follow proper guidelines for reference (see ASA “Publications Handbook and Style Manual). Same for P5, L5.

Change made

P5, L20-21. Delete “Furthermore, estimates of both would be needed on a by-field basis”

Deleted

P6. Materials and methods, need significant improvement, some comments in the “overall comment”

Changes made. When using data from other sources, to avoid duplication and avoid omissions, reliance on their papers is needed.

P6. L2-8. Please include the crops in the description table. Seems like all these data was published (reference provided for each long term study). Please indicate if the data was collected from published manuscript. If so how were the calculations made, using reported mean values? Using actual raw data from the field studies?

Crops included in Table 1. References for each study were provided.

P6, L10. Change “actual” for “number of”.   
P6, L15. How you define “improved genetics” what this cut off year? Some specific change in genetics?   
Please describe.

(introduction of semidwarf varieties) has been included

P6. L18-21. This is not clear, please reword. L19-20 may need to move to results section.

Corrected

P7, L4-5. Delete “(not applying any N is not something farmers will do)”

Modified as per comment above

P8, L1-9. This seems to contradict some of the discussion in the paper. Year selection for analysis was modified by the authors due to “use of improved genetics” however there seems to be no increase (or change) in yield due to improvements in genetics over time.

Using only data from 1958 on (“use of improved genetics”) referred only to the Magruder Plots and that was delineated. Addition that this was from the use of semidwarfs clarifies this sentence.

if yields are expected to remain constant despite “expected increase in yield” due to genetic improvements, would that help to “predict’ yield a little easier in the future? At least seems like a yield increase should not be expected?

Clarified sentence

Some data (table 3) seems to indicate a significant increase in yield over time (particularly Arlington).

This is correct. Bundy et al. (2011) paper referenced this too. 16 different hybrids used since 1986.

P8, L10-16. This is not clear, the number of positive slopes and statistically significant values are different in table 3. Please revise.

revised

P9, L12-15. Is not clear why this is surprising, would the soil N pool continue to supply some N even with no fertilizer N? Please expand your discussion and provide some reference.

P9, L16, Change “varied” by variable. changed

P9, L21-23. Is not clear what the intention is by posting these questions, please revise and make clear for the reader what is your interpretation.

Modified accordingly to explain the importance of temporally dependent N demand.

P10, L13. Is not clear what are you referring to with “fertility practice” (any fertilizer application?) or “nitrogen fertility” please be more specific, is this referring to N fertilizer applications?

Changed to “fertilizer” not fertility.

P10, L19-20. “This is because farmers would never have a true 0-N reference plot as they will always apply some N unless in a legume-cereal rotation” This was already mentioned before, please delete.

Modified to note this comment

P11 L2-4. Please delete, this was mentioned in the materials and methods.

Deleted  
P11, L6-7. Is this conclusion from your findings?

Modified to reflect your comment

P11. L7-10. This conclusion is basically the same as Raun et all (2011). Any additional findings that would support or complement Raun et all (2011)?

It was important to find the same relationship in other long-term trials and other states. This is better communicated  
  
Tables   
All tables: Please follow ASA “Publications Handbook and Style Manual – Tables and Figures” to prepare tables (one example is the use of correct footnote). Table 1. the “Trial” column seems to provide little information to the reader who is not familiar with these designations (LTM, 106, 407), please delete this column (use Altus 1 and Altus 2 for repeated locations).

Added clarification, sources included

For Table 3. Missing rows for the Magruder site, need to describe somewhere in the table why. I believe authors can easily present more data: can include the entire regression equation.

Modifications made

Table 4. “Mean” and “average” be consistent. Are these statistics calculated only for the control “mid-rate” and “high-rate”? only for controls? For all rates?   
Be consistent with decimal places.

The mean values were calculated for the variable delineated in the second column. RI0-N, RI-mid N, and Yield were the variables for which the range, mean std. dev., and CV are reported.  
  
  
Figures:   
Why only one site? This was discussed in the text (pages 8 and 9). See below.  
  
“Considering that no N had been applied for 37 years, it was somewhat surprising to find a yield level almost 60% of the maximum yield (9.5 Mg/ha) (Figure 1). This same result where near maximum yields were randomly achieved in check plots having received no fertilizer N for many years was observed at all sites (data not shown).”

Reviewer: 2   
Comments to the Author   
GENERAL COMMENTS:   
  
This paper reviews results from five long-term winter wheat and maize field experiments located in Oklahoma and Wisconsin. The relationship between grain crop yield and nitrogen response was evaluated. Nitrogen responsiveness was determined by dividing the grain yield from high N rate plots by either a 0-N check plot or a medium N rate plot. Relationships were then determined between maximum yield, response indices and year. The findings were summarized by stating that yield and N responsiveness were not related. The conclusion was that mid-season estimates of both yield level and N responsiveness should be used to formulate N fertilizer recommendations.   
  
The first part of this paper, i.e. that yield and N responsiveness were not related, was supported by field data. However, the conclusion that mid-season estimates of both yield level and N responsiveness should be used to formulate N fertilizer recommendations were bit supported by data. It is an approach that may lead to improved N fertilizer recommendations, but research is needed to confirm or dispute this approach.

This is now stated as being a suggestion, as per your comment (use of RI and YP0 independently for the formulation of better fertilizer recommendations).

There are several editorial issues that need to be addressed in this paper. For example, English units need to be replaced throughout with SI units. There was a long sentence on P7, L8-11 that was extremely confusing and difficult to read and comprehend. Several long paragraphs could probably better be divided into small paragraphs to break up the large blocks of text and make the paper easier to read. Not all of the references are written properly according to the style of Agronomy Journal.   
  
I suggest deleting Figure 1. The reason to suggest deleting Figure one is that it merely shows that the high N rate plots at Arlington, WI outyielded the check plots. But without quantitative information included, that is about all that can be said and this type of information is not very informative or new because we probably could all predict that this would be the case. Also, the contrast in the check and high N rate bars could have been made much greater.

We agree with the reviewer that this is a bit cumbersome. Nonetheless, reporting this data in a Figure from Arlington, WI show how N responsiveness varies considerably over time (and unpredictable). Yield level being unrelated with N response can also be visualized.   
  
  
SPECIFIC COMMENTS:   
  
1. I suggest removing the subheadings from the Introduction section. Most journal articles do not include subheadings in the Introduction and they are not needed.

This concept is complicated. Addressing the components using subheadings was considered prudent.   
  
2. 9, L10-15. Is a 5.6 Mg/ha yield really near the maximum yield that was reported as being 9.5 Mg/ha? How do you define “near maximum yields” noted in L14?

Good point. Highest yield observed substituted for maximum yield.  
  
3. Tables 1. Could you add a column indicating the crop grown at each site?   
Crop added

4. Tables 3 and 4. These tables would be easier to read and more informative if the crop grown at each site was somehow noted directly in the table.

Crop added within the heading of each table.

5. Units are missing for some values in Table 4.   
  
6. For all tables, I suggest putting the columns in the same order they are listed in the Table captions.

Modified  
  
  
Reviewer: 3   
Comments to the Author   
Review of AJ-12-0220-A   
  
This focused paper reviews the relationship between crop yield potential (for both wheat and corn) and the degree of response to nitrogen (N) based on long-term experiments conducted between 11 and 53 years in 3 different states. The writing itself of this brief paper is sufficiently clear to understand the goals and inferences of this overview type of analysis of previous data sets. The general subject matter of the variation in grain yield responsiveness to N is pertinent to the Agronomy Journal. Science efforts to integrate data from several N fertility studies are normally of extra value to readers of the journal.   
  
For reasons that are not clear, the paper tries to link relationships about yield response to N and yield levels from these long-term trials involving mostly a single N application and pre-plant timing to the specific question of criteria to use in arriving at “mid-season” N rate recommendations. The latter is an important matter, but one that is unique and not at all addressed in these long-term trials. Therefore, the attempt to integrate discussions about the validity of mid-season NDVI readings (which are a valid and perfectly scientifically acceptable route to fine-tuning late-season supplemental N applications) does not fit the theme of the paper. It is also not clear why only linear regression models were tested.

Text altered to reflect that implications are suggested coming from the different sources.  
  
Unfortunately, the paper rationale is based on the premise that the majority of agronomists still base their N rate recommendations on yield potential (which is no longer true for public-institution corn agronomists in the majority of the CornBelt). The old Nutrient Management Theory (as referenced to papers like Rehm and Schmidt in 1989 on page 4), are no longer accepted theory. See the Iowa State Website N Calculator as an example for the ongoing development of N rate recommendations based on ongoing N trial results in each state with specific region/soil types versus yield goals driving the current N recommendations.

Soil fertility specialists at Land-Grant Universities recognized about 10 years ago that yield-based N recommendations were invalid.

This is not a valid statement. Please see below.

From KSU faculty member, 11/07/2012,  
The official N rate recommendation for Kansas is   
1.6 x yld goal - 20 x %OM - nitrate in profile - crop factor - manure - N in irrigation water  
  
Currently used N rate recommendation for Ohio is (11/16/2012)  
Yield goal \* 1.05 (lb N/bu)) - soybean credit (typically 35 lb)     (assuming the ratio of corn price and N cost is 10)

University of Nebraska, 11/14/2012 current rec from Nebraska, as it has been unchanged since 2008. Here is the basic equation:

* N (lb/acre) = [35  + (1.2 x YG) - (0.14  x YG  x  OM) –( 8 x NO3-N) - other credits] × fA × fR

The paper repeats well-known facts about the large influence of weather on crop response to N rates.   
  
This manuscript is also very confusing to read because it switches back and forth between wheat and corn, often without clarity within a sentence about which grain crop is being referred to. From p.2, line 19, to p. 4, line 9, sentences go back and forth from one crop species to another. I don’t regard these 2 crops as being similar in their pattern of N response (timing, rate, form, etc.), so it would be very helpful to divide this paper into 2 separate manuscripts with one on wheat and a separate one on corn.

Discussing both corn and wheat in the same sentence is appropriate considering the nature of cereal crops and their similarities in grain crop production.

Much of the recent literature is missing in this manuscript. For instance, there are some recent reviews on NUE changes with corn genetics over time that need to be discussed in any serious treatment of this topic.

This manuscript is neither about corn genetics, genetic gains over time, or about NUE.

But an even bigger fault is that the paper assumes that the year effect on both yield and N response is random, when it is well known that corn yields have doubled over the interval being considered and that various N efficiency parameters have also improved with genetic improvement. A conclusion being reached about the lack of relationship between yield level and N responsiveness over 53 years of corn improvement (genetic as well as management) is much less convincing that a similar conclusion reached with hybrids from a similar era grown with similar management.

Nothing was assumed. In fact the opposite is true since the relationships between year and yield, year and N response are documented and reported based on these findings (linear relationships) in highly reputable long-term trials.  
  
The references, and data citations from the references, were not carefully presented in this new manuscript. For example, Table 2 suggests that AA (anhydrous ammonia) in the WI trial was broadcast and pre-plant applied, which is impossible. The AA form is always band-injected into soil. An actual check with the Bundy et al. paper from 2011 shows that the N form was AA from 1963-1984, and again from 1993 to 2007, but that urea was used from 1984 to 1992. The Bundy et al. study never mentions whether the N was pre or side-dress applied.

The mistake in Table 2 is inexcusable. We apologize for this error. A table covering 5 long-term experiments using different sources, over different years, different rates, and methods was a challenge.

This new manuscript also never clarifies whether data was used from the limed plots, or from the split which was not limed in the Bundy et al. study.

As per the Bundy et al. (2011) paper, yield data was from the lime treatment since 1985 (page 1348).

Then the Discussion section (p. 9, lines 12-15 goes on to state that the zero N yields were 60% of the 9.5 Mg/ha maximum when the actual high N yield in Table 5 of the Bundy et al. paper from 2011 was reported at 14.5 Mg/ha.

60% of the 9.5 Mg/ha yield in 1995 was what was intended and that is now clarified.  
  
The ultimate weakness of this paper, therefore, is that it does not present sufficiently novel or useful information that would help improve the science of N rate recommendations in either corn or wheat. Perhaps it can be re-written. Specific comments and suggestions are also noted in the attachment.

This would not be sufficiently novel or useful information for anyone who emphatically states that “Yield Based N Recommendations are Invalid,” (interesting news to Nebraska, Kansas, Ohio, and others) but then determines the Maximum return to N (MRTN) using “averages across all sites in the dataset” (Page 17 PM2015).