In an article published in Agronomy Journal, Kablan et al. (2017) presented results from 45 nitrogen (N) trials on corn (Zea Mays) conducted in Quebec between 2002 and 2010 (excepting 2005). The title is rather misleading, trials being conducted within a radius of 10 km from La Coop research farm located at St-Hyacinthe, Quebec. The objective was to determine optimum N dosage as a function of planting date (before or after May 15), soil texture (two groups), and rainfall accumulation and distribution during May and June (two groups). The authors concluded that “N rate guidelines may need to be increased for the optimum planting window, and should be based on soil texture and weather conditions”. However, there were flaws in the experimental design, the grouping methods and statistical analysis that led to major interpretation biases and misleading conclusions.

Methodological elements are missing.
1. The experiment had been established during the 2002-2010 period. The objective of this study was thus elaborated a posteriori to answer questions from “experts-conseils” (free translation: expert crop advisers) as asserted publically by the senior author (http://www.cooperateur.coop/fr/affaires-agricoles/des-travaux-la-coop-sur-lazote-encenses-par-les-scientifiques). Trials were conducted at some eleven locations. Among 45 trials, 22 were conducted at La Coop research farm, hence biasing the results toward the specificities of the farm site. Such systematic bias was not discussed in terms of local and regional (Eastern Canada) representability.

2. Except for N dosage, there was no treatment applied. As a result, all factors but N dosage were compounded into a global year effect where several factors were confounded. This should be stated explicitly.

3. Factors were year, planting date, soil texture, pH and organic matter content, method of fertilizer application, AWDR, hybrid, source of N fertilizer, moisture regime, previous crop, and N dosage. Several factors were thus assumed to have equivalent effects on maize yield. Were different previous crops, N sources, pre-plant and side-dress applications, hybrids (2300-2900 CHU), soil pH and organic matter content equivalent? Where did the authors test for such over-optimistic assumptions?

4. Early planting occurred in 2004, 2007, 2008, 2009, and 2010, and late planting in 2002, 2003, and 2006. Therefore, planting dates were tied to seasonal conditions, and were not arranged as a treatment. For planting date to be considered as a treatment, different seeding dates should have been set in the same year at the same site.

Planting dates and years were thus confounded despite different seasonal climates.
5. The N sources were UAN, urea, ESN, ammonitrate, and urea-Agrotain. Were fast- and slow-release fertilizers equivalent? The N sources were further confounded with the application methods (ESN solely applied as pre-plant fertilizer, UAN solely applied as side-dress fertilizer). How can the authors separate the effect of application method from that of the N source? This should be clarified.

6. There was no zero control even among the split application treatments. The starting point was 80 kg N ha\(^{-1}\). This should be justified in the context of the study because there was no significant response above 80 kg N ha\(^{-1}\) in eight trials.

**Methodology**

1. The paper lacks important metadata on seasonally cumulated corn heat units (temperature regimes) as well as on soil properties such as soil tests (nutrient availability) and soil series (hydrological class). The paper requires references on methodology for organic matter content (loss on ignition, Walkley-Black, Dumas combustion?) and pH (in water or salt solution?).

2. Two-way ANOVAs were run for each site. However, the authors presented no result of their two-way ANOVAs to support the effect of planting date \(\times\) N dosage on yield in Tables 4 and 5, as well as differences between “treatment” means at several interaction levels in Table 7. ANOVA results should be presented.

**Interpretation biases**

1. The authors referred to CRAAQ (2003) to criticize the recommended N rate guideline of 170 kg N ha\(^{-1}\). Such guideline has been updated in the range of 120 to 170 kg N ha\(^{-1}\).
1 in CRAAQ (2010) to avoid recommending a single N rate across myriad of yield-
impacting factors. Did authors miss that more recent provincial reference?

2. The authors wrote: «The increase in EONR [Economic Optimal Nitrogen Rate]

above the current provincial N recommendation [CRAAQ, 2003] also means that the
average yield potential for this group of site-years exceeds the provincial average for
which the current recommendations were developed by CRAAQ». False. The
CRAAQ recommendations were not based on any provincial average but on hundreds
of fertilizer trials.

3. The authors wrote: «The wider range of EONR obtained by these authors [Nyiraneza
et al., 2010] is probably due to the fact that this study was done over many regions (7)
with different climate conditions, with CHU from 2000 to 3000. In our study, all of
the sites had narrower CHU varying from 2873 to 3383.» False. The Nyiraneza et
al. (2010) study comprised 62 trials, 32 of them conducted in the same Montérégie
region during the same period, making both studies comparable.

4. The authors wrote: «Only high yield potential fields were used (from 8.5 to 14.7 Mg
ha\(^{-1}\), vs. average provincial ranging from 7.5 to 9.3 Mg ha\(^{-1}\) for the 2002 to 2010
period». This is a fallacious comparison. Provincial averages are estimated using a
different (survey) methodology, Authors should compare their results for hybrids
2300-2900 CHU with the research results of Nyiraneza et al. (2010) for hybrid ranges
of 2500-2950 CHU and yield ranges of 8.5-13.1 Mg ha\(^{-1}\) during the same period
(2007-2008). Including non-responsive trials, median EONR values were found to be
194 kg N ha\(^{-1}\) in Kablan et al. (2017) compared to 166 kg N ha\(^{-1}\) in Nyiraneza et al.
(2010) where most sites were seeded before May 15. The EONR varied between 80
and 237 kg N ha\(^{-1}\) in Kablan et al. (2017) and between 0 and 241 kg N ha\(^{-1}\) in Nyiraneza et al. (2010).

5. At several occasions in the discussion, the authors inferred based on a questionable analysis that higher maize yields call for higher N rates. In contrast, a fairly large body of literature showed a lack of relationship between expected or measured optimum yield and EONR (Blackmer, 1997; Kachanoski et al., 1996; Lory and Scharf, 2003; Tremblay and Seydoux, 2016). Even the very study the authors refer to brings additional evidence, for the same set of environmental and soil conditions: “Economically optimum yields were not correlated to EONR as also reported by studies conducted in many years and locations in the northern states of the United States and in the province of Ontario, Canada.” (P. 1487 in Nyiraneza et al., 2010).

This important issue should be discussed by the authors.

6. The quadratic pattern and curve flattening provided non-linear trends from a limited number of N doses. Were linear and quadratic trends first tested as polynomial contrasts in the ANOVAs? Judgement is needed here, not just \(r^2\) values. Could Kablan et al. (2017) provide confidence intervals about EONR values? EONR cannot be a unique value where there is experimental error on yield.

7. The authors did not provide any result on grain moisture and density. Drying costs could be included in the economic analysis. Why was drying cost, grain density and fertilizer application and extra handling costs not considered in the economic analysis?

8. The large difference in median EONR values and comparable yield values between the Kablan et al. (2017) and Nyiraneza et al. (2010) studies indicated relatively low
crop nitrogen recovery from fertilizer N in Kablan et al. (2017). Using yields at intercepts and EONR values presented in both studies, uptake rates of 11.5 kg N Mg\(^{-1}\) grain and 13.1 kg N Mg\(^{-1}\) grain at 15.5% moisture content for yield in control (intercept) and at EONR, respectively (Gilles Tremblay, MAPAQ, personal communication), and a benchmark nitrogen recovery rate of 37% in USA (Cassman et al., 2002), we found that 56% of the trials in the Kablan et al. (2017) study and 38% of the trials in the Nyiraneza et al. (2010) study performed less than 37% N recovery rate. This indicated that the Kablan et al. (2017) trials were biased toward sites where N recovery was low. The eight non-responsive sites warned that factors other than nitrogen limited yield. In a Quebec study, Nyiraneza et al. (2009) showed that 88% of the variation in maize yield could be attributed to N supply by the cropping system and to soil physical properties. Kablan et al. (2017) did not address such an important issue.

We conclude that the experimental design in Kablan et al. (2017) was inappropriate to test hypotheses on planting date; that factor effects were confounded; that too few site-years prevented any credible statement about treatment effects on maize yield across Eastern Canada; that methodologies were missing, limiting reproducibility; and that trials returned high EONR because nitrogen recovery rate by the crop was lower than in other studies. Crossing reliably a large number of factor combinations instead of confounding them requires data sets much larger than the one used by Kablan et al. (2017). Finally, good reasons for low nitrogen recovery at most sites as well as remedial methods and environmental impacts should be reported and discussed before concluding too early that maize N rates should be increased. In the light of its numerous weaknesses, using this
study to support any proposal to crank up the Quebec N fertilizer guidelines would be
detrimental to the sustainability of maize production systems in Quebec.

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