

# Review of: "Subsoil-potassium depletion accounts for the nutrient budget in high-potassium agricultural soils"

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Potassium (K) together with nitrogen (N) and phosphorus (P) are considered the three most important macronutrients in plant nutrition. However, studies focusing on K dynamics at the soil-plant interface have received much less scientific attention (from agronomy to soil and environmental sciences) than the other two in the trio (N and P), at least since the 1980s. The reasons for the lower interest in the “forgotten nutrient”, as Öborn et al. (2005) have nicknamed K, are multiple and remain unclear (from generous global and native soils reserves to a - possibly - less determinant role in plant physiology and crop yield). Nevertheless, the cycle of K’s marginality seems to be ending based on recent relevant contributions such as the present study. Another example is the book “Improving Potassium Recommendations for Agricultural Crops” (Murrell et al., eds.), also published in 2021, which marks 35 years after “the last major scientific book on potassium (K) in agriculture” (p. vii).

This renewed attention appears to stem from persistent negative K budgets reported in many agricultural systems around the world, suggesting widespread K depletion in soils under long-standing cultivation (Öborn et al. 2005; Rengel et al. 2008; Wang et al. 2008; Bell et al. 2021). As well specified by Rengel et al. (2008: 625), “although global K reserves are likely to be sufficient for hundreds (...) of years of agriculture, the profitability of agricultural production on marginal soils will increasingly rely on the efficient use of K”.

The reviewed paper comes from an Argentine and US team and adds to the literature a timely 9-year trial focusing on soil K-depletion in the Argentine pampas (developed on four farms from the southern Santa Fe region), which have mostly been cultivated without potassium fertilization. Particular attention was given to the little studied vertical development of K in soil. In short, the article argues (i) for the need to rethink K budgets in vast cultivated areas in Argentina that face long-term depletion of rich native reserves, (ii) for the inclusion of the subsoil for a correct assessment of K availability, and, more fundamentally, (iii) to address the dynamics of the chemical fractions, or pools, of K (already complex and still to be satisfactorily clarified) along vertical transits to a depth of 100 cm of soil.

In general, I think that the overall understanding of the article and its results would have benefited from a slightly longer description of the study design. I also felt that the writing is at several points rushed and

could have been clearer. Below are some brief critical notes on the contents of the paper.

#### *Soil history.*

Traditionally, an in situ fertilization trial does not require the presentation of the cultivation history of the soil under study, including at most some background notes on the agricultural transformation of the region. Recently, however, it has become clearer that agricultural systems are strongly conditioned by the long-term soil history (20, 50, or 100 years?), about which objective and systematic data are almost always lacking. The study by Correndo et al. (2021) seeks to circumvent this problem by introducing the concept of “cropping history” (simply, the number of successive years under cultivation prior to the start of the trial), which allows for a richer discussion of hypotheses and results. Nevertheless, I think that it would be useful to compile other data, even qualitative, on the “cropping history” and “management history” of the experimental sites in order to deepen conclusions such as the following: “The sites with longest agricultural history at the beginning of the experiment (Balducchi and La Hansa), presented significant  $\text{NH}_4\text{-OAc-K}$  reductions from 2000 to 2009”. When it is said that the soil has a cropping history of 8 years does it mean that there has been no cultivation until that time? How was this information obtained? Furthermore, the concept of pristine soil, which is important for the trial design, should be unambiguously defined. To what time span does the expression “no antecedents of agriculture” refer?

I am convinced that approaches with longer time frames, bringing together quantitative trials but also qualitative collection on the farming history of a given soil, foster the construction of richer agronomic hypotheses while building bridges with other disciplines, such as agricultural history and environmental sciences (e.g. Carmo et al. 2017; Cunfer 2021). Within this framework, the statement that “fertilizer K applications have been negligible during the Argentinean agricultural era, which started around 100 years ago” perhaps deserves some development and the inclusion of references.

#### *Recovery time of exchangeable-K ( $\text{NH}_4\text{-OAc-K}$ ).*

How long does the exchangeable-K pool take to recover? The discussion of the results (and even the study design) would benefit if the recovery time (or rate) of  $\text{NH}_4\text{-OAc-K}$  was approximately known, either by literature review or by including this determination in the trial itself (e.g. monthly measurements after harvest until stabilisation without any intervention in soil). If the recovery cycle is short (as some authors suggest: Öborn et al. 2005), in the order of 1-3 years, then we cannot conclude much from the series of measurements represented in Figure 1 (last row of graphs). In this sense, the first hypothesis put forward in the paper might need to be reviewed: perhaps it is not the  $\text{NH}_4\text{-OAc}$  test that is not very sensitive but the spacing between measurements that is too wide.

#### *Root K uplift.*

The results suggest, with some surprise, a greater depletion of both exchangeable-K and slowly-

exchangeable-K fractions (the latter more intensely than the former) in the subsoil layers (20-100 cm) compared to the topsoil (0-20 cm), which highlights the importance of root activity in the deeper layers and therefore the need to consider them in the assessment of K availability to plants. Further, this vertical differential also seems to suggest, as identified by the authors, a possible upward transport of soluble K<sup>+</sup> ions through the root systems. This passive transport, not measured in the study, is generally processed at night (Nadezhdina et al. 2010) and would be responsible for maintaining K concentrations in the topsoil above critical NH<sub>4</sub>-OAc-K levels. These interesting hypotheses certainly deserve further work in the future.

#### *K budget definition.*

The article does not define the K budget, nor what K fluxes are included in it. If I understand correctly, the K budget corresponds only to the flux of K removed from the soil in the grain, since no potassium fertilization is done, nor are other “agricultural” fluxes of K mentioned. Therefore, the sentence “differences in K removal are largely explained by final yields” does not need the adverb “largely”.

This study does not consider the numerous “natural” flows of K, such as leaching, runoff, wet inputs (rainfall and irrigation) and mineral releases, because it would be difficult to accurately estimate different rates for the different soils examined. To consider that their balance is equal in the different trials still remains a good approximation. However, it is important to note that these “natural” fluxes are far from negligible (e.g. Öborn et al. 2005; Carmo et al. 2017), as stated in the paper.

#### List of references:

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