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REVIEW ARTICLE

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# 'Shifting gears ain't easy': Disciplinary resistances to perspective shifts in soil science and how to move forward

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## Abstract

Over the last decade, the fact that novel perspectives on various aspects of soils have remained strongly controversial long after they emerged, without any kind of consensus being reached about them, raises question about the underlying reasons for this phenomenon. The on-going debate on the usefulness of aggregates to describe the functions of soils illustrates some of the key aspects of that question. Similar debates on other soil-related issues also appear stalled, or have been for a long time and are only now moving forward. This might suggest a fundamental aversion to change, which when it gets overcome, only does so slowly. However, at the same time, somewhat surprisingly, researchers appear willing to quickly seize opportunities provided by new idea or novel perspectives on other topics. In that context, the objective of the present article is to analyse in detail what may cause such contrasting reactions to novelty. We consider, then ultimately dismiss, explanations based on how strongly or not novel perspectives have been actively promoted, on how access to suitable technology may impede or only slow down perspective shifts and on whether a recent theory of the 'slowed canonical progress in large fields of science' applies to the relatively small soil science community. Then, taking soil aggregates as a case in point, we come to realize that it is the extent to which a novel perspective mandates an interdisciplinary approach that determines whether or not it is adopted quickly. From that standpoint, we envisage a number of practical actions that could be taken to facilitate in the future the emergence in soil science of interdisciplinary research efforts, which we argue are absolutely essential to successfully tackle the enormous complexity of soils and to come up with satisfactory answers to the daunting environmental and food security problems we currently face in their management.

**KEYWORDS**

aggregation, biochars, breakthroughs, innovations, scientific debate, soil aggregates

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## 1 | INTRODUCTION

Over the last decade, an intense debate in the soil science community has raised questions about the ability of researchers to make their viewpoint evolve on critical aspects of the functioning of soils to keep apace with theoretical or experimental developments. This debate, still on-going, is concerned with the significance of aggregates for the description of the structure of soils and of the multitude of physical, (bio)chemical and biological processes taking place in them. Two schools of thought are involved in this debate. The first, and chronologically the oldest one, holds the view that the structure of soils is best approached via the dismantlement of soils in chunks of material of various sizes, and via the subsequent assessment of the stability of these 'aggregates'. The other approach, inspired by Kubiëna (1938) and outlined explicitly by Letey (1991), considers that the architecture of soils should be studied in its undisturbed state. For the last 33 years, these two opposing views have persisted and clearly appear likely to do so for years to come, judging from some of the recent articles on the topic. Given that state of affairs, it seems legitimate to wonder not only what has made a resolution of this debate so elusive for such a long period of time, but also, perhaps more fundamentally, whether this lack of a consensus reveals an aversion on the part of soil scientists to readily adopt new perspectives. If so, then, the question is what might cause the underlying reluctance to shift to new approaches, and what can be done to alleviate it.

In that general context, the objective of the present article is to address and try to answer some of these questions. The analysis we propose is organized as follows. The apparent standstill on the debate on soil aggregates, which serves to illustrate some of the points we raise, is discussed in the first section, immediately after this introduction. It is followed by an overview of other examples in soil science where perspective changes have historically been slow to unfold, or even have not taken place at all. The logical inference this might suggest, namely that soil scientists are not particularly keen to rapidly adopt new perspectives, is strongly contradicted by the observation, in the next section of this article, that they have been able at times in the past to quickly embrace new ideas without much struggle, let alone debate, at all. Trying, in the subsequent section, to resolve this apparent contradiction, we consider, then ultimately dismiss, an explanation based on how strongly or not some novel perspectives have been actively promoted. Another possible explanation that has been proposed to explain the 'slowed canonical progress in large fields of science' (Chu and Evans, 2021) is argued in the next section not to apply readily to paradigm shifts within the relatively small soil science community. Then, we come to the crux of our argument. Taking soil aggregates as a case in point, we argue that the extent to which a novel perspective requires an interdisciplinary approach determines whether or not it is adopted quickly, with the need for interdisciplinarity being in virtually all cases a severe impediment to the evolution of ideas. From that standpoint, in the last section of this article, we envisage various actions that could be taken to facilitate in the future the emergence in soil science of interdisciplinary research efforts, which we argue are absolutely crucial, given the enormous intrinsic complexity of soils and the daunting multifaceted challenges we currently face in their management.

**Highlights**

- Recent exchanges in the debate on soil aggregates suggest a reluctance of soil scientists to shift perspective.
- Other examples in the literature show similar occurrence in some cases, but not in others.
- We review in detail and eventually dismiss a number of possible explanations for this apparent reluctance.
- A resistance associated with the need for interdisciplinary research seems the more likely explanation, and we discuss what to do about that.

## 2 | THE 'AGGREGATE' DEBATE

For many decades, the sizes and shapes of aggregates obtained by dismantlement of the soil matrix have been used in the field as criteria to classify soils, to determine how suitable they would be to grow certain crops and to assess how stable their structure would be to raindrop impacts (e.g., Amézketa, 1999; Beare & Bruce, 1993; Díaz-Zorita et al., 2002; Horn et al., 1994). In sharp contrast with this traditional viewpoint on aggregates, Letey (1991) argued that they are entirely artefactual and of limited relevance to the description of the processes occurring in undisturbed soils. His perspective was that an approach of the 'architecture' of soils emphasizing, not aggregates, but instead, the geometrical organization of the solid phase and pore space is far more conducive to the description of the functioning of soils, and in particular of the environmentally important fluxes occurring through them. As recounted by Baveye et al. (2022), the technology available to soil scientists for most of the 1990s did not allow researchers to fully take advantage of his perspective.

The advent, first, of dedicated X-ray beamlines at various synchrotron facilities around the world, then, at the turn of the century, of commercially available table-top X-ray computed tomography (CT) scanners was a game changer in that respect. Over the last two decades, the technology has made tremendous advances and is now entirely capable to deliver on Letey's (1991) vision, as clearly demonstrated by the sizeable body of literature that has been produced on the topic (e.g., Ashman et al., 2003; Baveye, 2006, 2021; Baveye et al., 2018; Kravchenko et al., 2019; O'Donnell et al., 2007; Wander, 2004; Young et al., 2001; Young & Crawford, 2004).

In the 33 years since Letey (1991) spurred a discussion on the best way to look at soil architecture, one might have expected that a consensus view would have emerged at some point. However, evidence suggests that we are still very far from any kind of resolution. This is hardly surprising since over the last 30 years, literally thousands of articles and book chapters dealing with soil aggregates, their dynamics and their stability, have entirely avoided acknowledging that there is any kind of controversy surrounding the topic. The review article of Bronick and Lal (2005), which has been cited more than 5000 times so far, refers to aggregates 183 times, but does not cite Letey (1991) or any of the articles that, between 1991 and 2005, have been even mildly critical of the reliance on aggregates (Baveye, 2006). In recent years, many authors have continued to bypass publications that challenged the traditional view (e.g., Erktan et al., 2020; Felde et al., 2021; Ferreira et al., 2023; Hartmann & Six, 2023; Lavelle et al., 2020; Lv et al., 2023; Najera et al., 2020; Or et al., 2021; Pitumpe Arachchige et al., 2024; Rieke et al., 2022; Ritschel & Totsche, 2019; Rötzer et al., 2023; Totsche et al., 2018, 2024; van den Bergh et al., 2024; Yudina & Milanovskiy, 2017; Yudina et al., 2022).

Among the minority of defenders of the 'aggregate' perspective who have been willing to acknowledge that there are different perspectives on the structure or architecture of soils, it has not helped the debate move forward that some appear to have misunderstood, and hence end up misrepresenting, the exact differences between the opposing perspectives. For example, Amelung et al. (2024) claim that the perspective focused on the study of soil architecture in undisturbed soil samples fails to 'focus research on the domain that links the fluid-filled void space with the solids: The biogeochemical interfaces'. Even a cursory reading of the related literature, in particular the recent articles by Vogel et al. (2022) and Baveye et al. (2022), makes it clear that this assertion is not correct at all. However, such misrepresentations appear to be isolated occurrences. A more general trend recently has

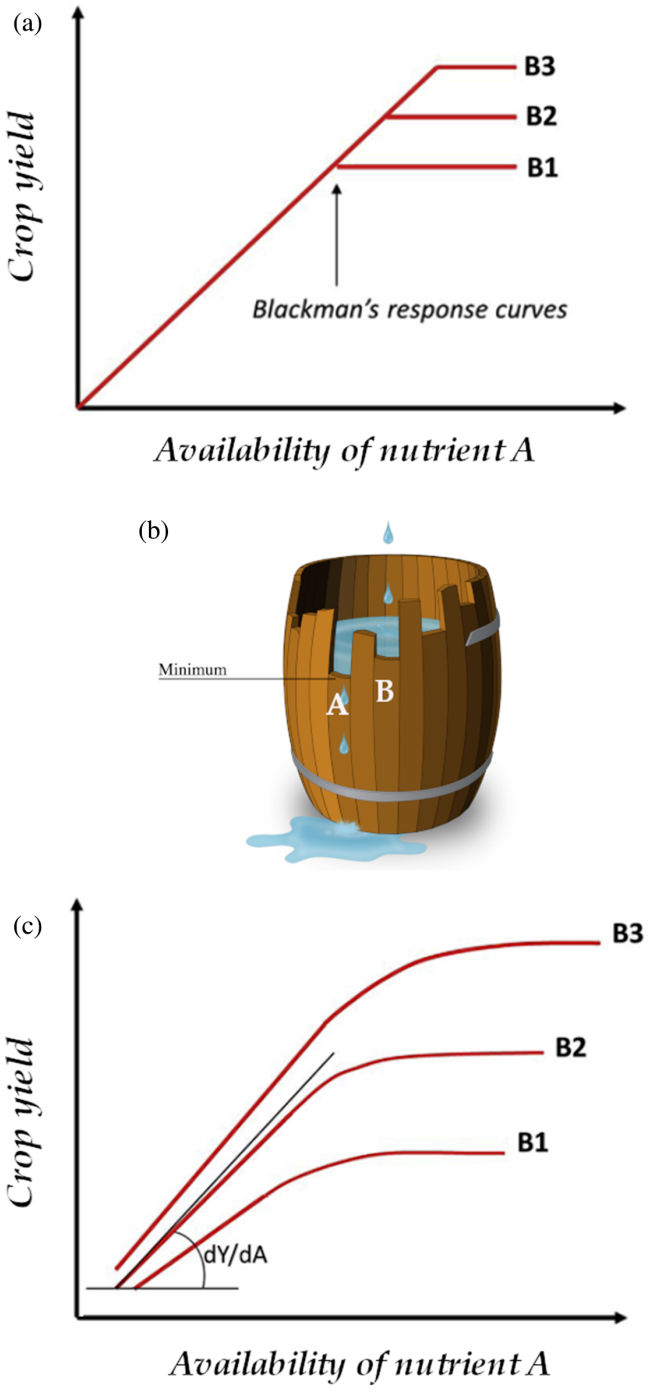


FIGURE 1 (a) Schematic representation of Blackman's curves illustrating the Sprengel-Liebig's Law of the Minimum. The response of the crop yield to the availability of nutrient A is eventually limited by the availability of nutrient B, but the rate of the response of the yield to A is not affected by B. (b) Traditional illustration of the Law of the Minimum by a 'pierced' barrel, with staves of unequal lengths. The maximum practical capacity of the barrel is limited by the length of the shortest slave. Similarly, a plant's growth is limited by the nutrient in shortest supply. (c) Schematic representation of Liebscher's Law of the Optimum, according to which the yield response to A (dY/dA) depends on the availability of nutrient B (modified from Lemaire et al., 2021).

been for authors to consider that the two perspectives on soil structure/architecture are complementary, not contradictory, and in effect correspond to 'two sides of the same coin' (e.g., Garland et al., 2023; Sullivan et al., 2022; Yudina & Kuzyakov, 2019, 2023). The recent survey by Roosch (2024) is very enlightening in this respect. When 251 German academic researchers were asked whether 'the pore and aggregate perspectives are conflicting', 229 respondents (i.e., a whopping 91.2%) said that they rather, moderately or strongly agreed with that statement (Fig. 1f in Roosch). However, when the same individuals were asked specifically whether 'the aggregate and pore perspectives are two sides of one coin', 172 respondents (i.e., 68.5%) were rather, moderately or strongly in agreement (Fig. 1e in Roosch et al., 2024). These numbers are puzzling, since they evince a definite lack of coherence. What they suggest, arguably, is that when respondents were given an opportunity to take a position that in effect alleviated the need for a debate, a sizeable majority of more than two thirds of the respondents opted for it.

Regardless of how one positions oneself relative to the on-going debate about the usefulness, or lack of it, of the concept of aggregates in soils, it seems difficult to avoid the conclusion that, in the last 33 years since the publication of Letey's (1991) article, the overwhelming reaction of soil scientists has been a combination of a fundamental resistance to a non-traditional perspective, and a manifest reluctance to engage in a constructive debate on the topic.

## 3 | OTHER EXAMPLES OF RESISTANCE TO PERSPECTIVE CHANGES

The usefulness of soil aggregates is not the only topic about which one can objectively conclude that a protracted divergence of views and a resistance to shift perspective have persisted over the years. Another example concerns the nature of humic substances in soils. After Waksman (1936) proposed a model of soil humus as a continuum of organic fragments of all sizes and with a heterogeneous composition, his viewpoint remained in the background of many discussions. Meanwhile, another, completely different perspective occupied centre stage. It advocated for the traditional practice of extracting soil organic matter with alkali, followed by the identification, based on water solubility, of three different categories of high-molecular-weight, inherently stable and chemically uniquely defined substances, referred to as fulvic acid, humic acid and humin (e.g., Chen et al., 1977, 1978; Schnitzer, 1967). In the last two decades, various authors (e.g., Kinyangi et al., 2006; Kleber & Johnson, 2010; Piccolo, 2001, 2002; Sutton & Sposito, 2005) have argued repeatedly that Waksman's (1936) ideas are more in line with available spectroscopic evidence. On that basis, Lehmann and Kleber (2015) advocated for a 'soil continuum model' that is virtually identical to the one Waksman proposed 80 years earlier (Baveye & Wander, 2019). Nevertheless, the debate is still not closed, and no clear consensus has emerged yet. Some authors, especially since 2015, continue to steadfastly focus on the 'fixed-chemical-structure' of humic substances and the use of alkali extracts (e.g., Mohinuzzaman et al., 2020; Olk et al., 2019; Ukalska-Jaruga et al., 2021; Zherebker et al., 2019).

Another example where a shift of perspective has been very slow to occur is related to soil fertility. In the mid-19th century, Carl Sprengel then Justus von Liebig proposed a perspective on plant nutrition that has become known as the Sprengel-Liebig Law of the Minimum (van der Ploeg et al., 1999). According to it, plants grow only to the extent allowed by the single nutrient that is most limiting (A in Figure 1a) and is not affected by the availability to plants of other nutrients, until another nutrient becomes limiting (B in Figure 1a). This perspective, generally illustrated by a barrel with staves of unequal lengths (Figure 1b), still constitutes the basic theory in most soil fertility textbooks and on websites devoted to explaining the calculation of mineral fertilizer needs of crops. Except for the minor change introduced by Mitscherlich (1924), in which the linear response of the Blackman curves was replaced by a more realistic asymptotic one, not much has changed for over a century in this perception of plant nutrition, entirely controlled by how much the soil solution could supply of a number of essential and minor nutrients, with the plants having no active role in the process. Criticisms of the Law of the Minimum or of Mistcherlich's law of 'diminishing returns', when they emerged, most often went unnoticed. Liebscher (1895), for example, suggested that 'all nutrients are used most efficiently when the availability of the nutrient that is most limiting is increased near its optimum'. As illustrated in Figure 1c, this means that there are interactions between the way a plant takes up different nutrients, as well as other resources including water. Rabinowitch (1945) suggested already 80 years ago that plant growth is not a sequence of processes determined by only one factor but by two or more factors that can interact with each other. Nevertheless, for many decades, until very recently, this complexity and the active role of plants in their own nutrition were largely ignored, and considerable efforts were devoted, largely unsuccessfully, to devise the 'best' *soil* tests that could provide guidance for fertilizer applications (Fardeau et al., 1988; Morel et al., 1992; Zehetner et al., 2018). In recent years, in parallel with the accumulation of practical problems associated with this soil-centred approach, in particular the widespread pollution of aquifers and surface waters

resulting from frequent over-fertilization, increasing numbers of researchers have started to realize that 'nutrient availability for plants is an emergent property of the whole ecosystem and not simply a soil characteristic being manipulated by fertilization' (Lemaire et al., 2021). Research in this context is showing, among other things, that plants are able to use information about soil nutrient concentrations (e.g., of N, P, Fe, Zn) to adapt their absorption capacity within integrated starvation or satiety responses (Briat et al., 2015, 2020; Lemaire et al., 2019, 2021; O'Brien et al., 2016; Ohkubo et al., 2017). Very little of the information available in this context, however, appears in the numerous web sites designed to help farmers assess the fertilizer needs of their crops, which are still overwhelmingly computed using the Sprengel–Liebig Law.

The debate on soil health provides yet another example of a perspective shift that for a long time has failed to occur. On the basis of a long tradition of research connecting soils with animal and human health (e.g., Balfour, 1943; Rodale, 1945; Voisin, 1959), several authors introduced 29 years ago the concept of 'soil health' as an advantageous alternative to that, until then commonly used, of 'soil quality' (Acton & Gregorich, 1995; Warkentin, 1995). Doran et al. (1996) defined soil health as 'the continued capacity of soil to function as a vital living system, within ecosystem and land-use boundaries, to sustain biological productivity, maintain the quality of air and water environments, and promote plant, animal, and human health'. Soon afterwards, Oliver (1997) pointed out that, besides having positive effects on animal and human health via crop production, soils could also affect very negatively the health of animals and humans, because of their contamination by various toxic organic and inorganic compounds, because of minerals they contain (leading, e.g., to podoconiosis) or because of nematode eggs and larvae they host, leading to very widespread soil-transmitted helminth (STH) infections. Oliver's (1997) thorough review should have led to a balanced view of the relation between soil and human/animal health, leaving open the possibility that in various cases, soils that are considered healthy by a set of standards may nevertheless be deleterious to human health. Over the years, despite a handful of articles re-emphasizing Oliver's (1997) message (e.g., Oliver & Gregory, 2015; Pepper et al., 2009; Wall et al., 2015), the most common portrayal of soil health among soil scientists has tended to conform to the view of 'healthy soil as fundamental to human health' (Montgomery et al., 2024). Many articles on soil health over the years have not mentioned diseases like the STH infections, and when they did, it was as a passing remark, which did not alter very much the big picture (e.g., Blanco & Lal, 2023; Lehmann et al., 2020; Svoray et al., 2015). It has taken until very recently for the trend in this respect to start changing slowly, with soil scientists beginning to envisage soils from a more encompassing perspective (e.g., Montgomery et al., 2024; Singh et al., 2023).

These various examples of debates that did not result in a change of perspective and, in some cases, did not even take place at all, are not entirely surprising, given what we know now about the way scientific revolutions have taken place in the past. Kuhn's (1962) seminal book on the topic popularized the concept of perspective shift and showed through a number of historical examples that in many cases, shifts, when they take place, occur slowly, over a protracted period of time. In the huge body of literature that Kuhn's analysis has stimulated in the last 60 years, researchers have shown that hindrance to the evolution of ideas can be caused by many factors, for example, cognitive dissonance, inertia and habit, investment in the old perspective, lack of information or understanding, threat to researchers' identity, selective exposure, as well as power structures and interests.[1] One could argue that one or more of these factors has been at play in slowing down the acceptance among soil scientists of new perspectives about, among others, soil architecture, the chemical nature of humic substances in soils, the nutrition of crops or the negative impacts of soils on human health. This could straightforwardly translate into alternative perceptions of the soil science community overall as either very cautious or overly conservative. Yet, upon further scrutiny, such overall perceptions rapidly run into a significant obstacle when one notices that, at the same time, in quite a few instances, soil researchers have accepted novel approaches enthusiastically and have very quickly jumped on the bandwagon.

## 4 | SOME NOVEL IDEAS OR PERSPECTIVES SEEM TO BE ADOPTED OVERNIGHT

In the early 1980s, after Mandelbrot's (1982) foundational book on the topic was published, the theory of fractals attracted quite a bit of attention from researchers in many disciplines, including in soil science. It took a few years for a clear understanding to emerge of how fractals could be used to describe soils. In 1989, a number of soil physicists and physical-chemists, in rapid succession, published several articles relying on fractal geometry to describe the sorption of water vapour on soils, the

[1]The upshot of these different causes seems aptly summarized by the 'shifting gears ain't easy' quote in the title of this article. This was mentioned to one of us (PCB) years ago by a car salesman who wanted to make a sales pitch for a model with an automatic transmission, versus a less onerous one with a standard shift.

geometry of the pore space or the retention of soil moisture (Ahl & Niemeyer, 1989; Sokołowska, 1989; Tyler & Wheatcraft, 1989, 1990; Young & Crawford, 1991). These early articles suggested that a single number, the fractal dimension, could unlock a wide range of properties of soils, which clearly is very appealing, particularly as, in theory at least, it could be linked to microbial processes, water retention and diffusion (Young & Crawford, 2004). At the same time, a very ingenious set of images (Figure 2) presented widely at conferences, demonstrated vividly the applicability of fractals to soils. Almost immediately after that, the literature on the subject literally exploded. In just a few years, the number of articles mentioning the topic, according to Google Scholar, grew very rapidly, from 460 during 1991 to 1030 during 1995, and continued to expand thereafter, for a few years, at least.

At about the same time fractal geometry became very popular, soil microbiologists got interested in the idea, then being circulated, that a collection of genes extracted from some environment like soils could be sequenced in a way analogous to how it was done for a single genome. Initially, implementation of the idea faced serious practical obstacles. However, technological advances in the late 90s on the 'shotgun sequencing' of genes made possible the handling of the vast quantities of complex data involved in the process. As soon as the related technology, for which the term 'metagenomics' was coined, became widely available, soil microbiologists enthusiastically started to adopt it in their research. The number of articles published yearly on the topic grew slowly but steadily for a few years, until 2004, while increasing numbers of laboratories were getting equipped, and then, it took off, exponentially. During 2005, 480 articles mentioning metagenomics were published, according to Google Scholar. During 2009, that number was a whopping 1640.

A particularly illustrative example of the swift adoption of a new perspective by the soil science community involves the use of pyrolyzed organic residues, generally known as 'agrichars' or 'biochars', in agriculture. More than a century ago, archaeologists doing field work in the Amazon basin noticed very dark soils, known as 'terra preta do índio' or 'terra mulata' (Nimuendajú Unkel, 1914). Much later, several soil scientists (Pabst, 1993; Sombroek, 1992) studied the characteristics of these soils and pointed out that, in addition to elevated charcoal contents, they also had increased levels of organic matter, nitrogen and phosphorus. Sombroek (1992), apparently downplaying the significance of these other chemical components of terra preta, suggested that the addition of charcoal to agricultural soils in other parts of the world could be very useful to sequester carbon. The idea did not take off in 1992, but when researchers picked up on it again, a decade later, the claim was quickly made that biochars, when added to soils, represented a uniquely beneficial solution to a number of soil-related problems, most notably to combat climate change (e.g., Bruges, 2009; Laird, 2008; Lean, 2008; Lehmann, 2007a, 2007b; Lehmann et al., 2006; Renner, 2007). Its alleged benefits also extended far beyond soils, since as advocated by Marris (2006), it could be viewed as 'the only way to make a fuel that is actually carbon negative'. Despite a fundamental lack of corroborating

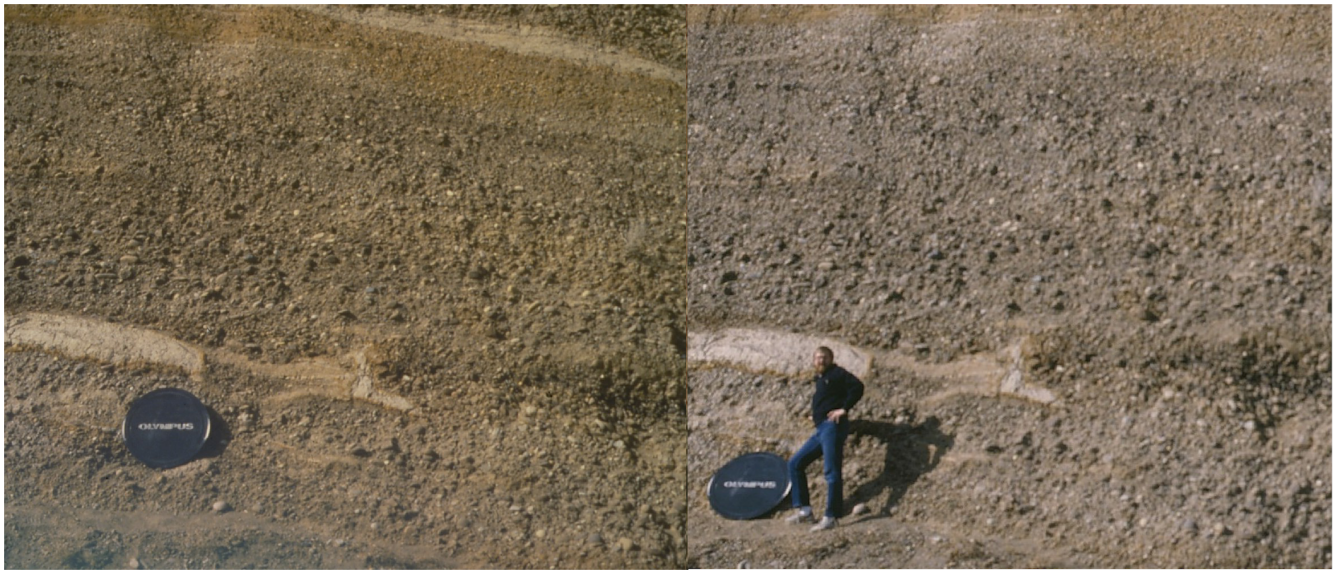


FIGURE 2 Ingenious visual demonstration of the intrinsic self-similarity of soils and therefore of the appeal of fractal geometry to describe them. Left: Traditional picture of a soil on which one has placed a lens cap, as is customary in soil science to give an idea of the spatial scale of the image. Right: Photograph of the same soil, showing that what one thought was a regular lens cap, was in fact a very large object (photo credit: Scott Tyler).

experimental evidence backing 'win-win' claims about biochars use in soils, let alone of a deep understanding of the mechanisms underlying most of them, demonstration experiments were very rapidly initiated all around the world, and the related literature started growing exponentially. According to Google Scholar, a whopping 663 articles and book chapters published in 2009, a mere 3 years after the biochar movement was launched, contained the words 'biochar' and 'soil'. The number of articles published yearly on the topic has continued to grow dramatically ever since and, through the documented existence of a sanctioned 'citation cartel' (e.g., Abduh, 2023), has literally exploded in recent years. At this juncture, the whole literature on the topic contains many tens, if not hundreds, of thousands of articles.

More recently, another perspective has also been adopted enthusiastically, and relatively rapidly, by soil scientists, without much debate on whether or not it was adapted to our discipline. In this case, the original idea is relatively old, since it goes back more than a century, to Pearson (1904), who showed how to collate the results of several studies to reach general conclusions. After further theoretical developments in subsequent decades (e.g., Yates & Cochran, 1938), the method, termed 'meta-analysis' by Glass (1976), became extremely popular in psychology, and later in most other disciplines, prompting some authors to describe the phenomenon as a 'meta-analytic big bang' (Shadish & Lecy, 2015). At the turn of the century, several ecologists (Johnson & Curtis, 2001; Rustad et al., 2001; Wan et al., 2001) applied the method to soil data, but it was only after the publication of an article by Angers and Eriksen-Hamel (2008) that interest in the method took off among soil scientists. By 2014, according to Google Scholar, 135 articles had been published that contained 'soil' and 'meta-analysis' in their title. By 2023, that number had grown to 1200, and virtually, every issue of the main soil science journal now routinely contains at least one meta-analysis. Nevertheless, the growth of the related literature may seem slow compared with some of the other examples described above. However, analysis of the citation patterns of the early articles by Johnson and Curtis (2001), Rustad et al. (2001) and Angers and Eriksen-Hamel (2008) shows that they had immediate impact on the discipline.

There are probably many other examples that one could mention of quick adoption of a novel idea or a new perspective. Machine learning and artificial intelligence techniques, as well as the security-centred perspective on soil issues, come to mind in that respect. Rather than cataloguing all possible cases, the question we want to address is why, in some situations, researchers are keen on jumping rapidly on the bandwagon, and in others not at all, even after many years

## 5 | IS LACK OR EXISTENCE OF CONVINCING EVIDENCE AN EXPLANATION?

A first possible explanation is related to the existence or absence of 'convincing' arguments or experimental evidence to settle a debate on a particular topic. What is convincing to a group of researchers may not be necessarily so for another group, however, so that some degree of subjectivity may be associated with this explanation. Still, if indisputable experimental evidence were obtained in support of one of the conflicting perspectives in a debate, one would expect everyone to eventually recognize the fact and move on. So, the fact that some debates in soil science linger for very long times might conceivably be due to the absence of solid evidence or arguments.

Yet, that does not seem to be what has happened. A case in point is the debate on the nature of humic substances in soils. After Solomon et al. (2005) and Kinyangi et al. (2006) provided clear evidence, via the use of NEXAF spectroscopy, that natural organic compounds in soils were spatially very heterogeneous and did not exhibit a uniform chemical structure, it seems that the theory of humic substances having unique, well-defined chemical structures should have been abandoned, superseded by Waksman's (1936) perspective. However, as pointed out earlier, some researchers are still debating the issue almost 20 years later.

In the debate on the usefulness, or lack thereof, of the concept of aggregate in soils, one could argue that an architectural argument suggested initially by Kubiëna (1938) close to 90 years ago, and reiterated numerous times since (e.g., Baveye, 2023a, 2023b; Baveye et al., 2022; Kravchenko et al., 2019; Letey, 1991; Vogel et al., 2022; Young et al., 2001), should have sufficed to bring some closure to the discussions. One version of this argument involves a real-world situation with which everyone can easily relate, and in which one is asked to try to improve the functioning of a given hospital. The way one would normally proceed with this task is to first obtain the plan of the hospital building and determine in detail where the furniture it contains is located. Then, one would try to get a good understanding of who works in the building, how many patients are in it on average and how all these people move from room to room during normal daily operation. Anyone even slightly computer savvy could attempt to model all the people and material fluxes (of food, medications) occurring through the hallways. Once such a model is developed, it could be used to see how slight modifications of the

features of the hospital, including even knocking down walls if necessary to change the configuration of some rooms, rearranging the furniture or having people circulate in the space in a different pattern or at different times, could affect the overall functioning and energy efficiency of the establishment. All this is what hospital management specialists do routinely. Nobody with a right mind would ever recommend an alternative approach, in which one would actually or virtually demolish the hospital building, reduce it to rubble and then either measure the size of the chunks that are left, or repack these chunks somehow, and try to figure out from the resulting pile anything about the original operation of the hospital. This approach might perhaps serve as an extremely crude way to obtain information on the strength of what binds together the materials (e.g., stones, bricks) of which the walls of the building are made, or to determine if the hospital building would be resilient to extreme events like a hurricane or an earthquake, although even these determinations would seem to be far more robust if stress tests were carried out on the intact building itself or on a computer model of it. However, clearly, demolishing the building would provide absolutely no useful information on the functioning of the hospital and would never be construed as 'complementary' to the approach that is normally adopted.

In addition to this architectural argument, which would seem hard to counter (and incidentally, perhaps for this very reason, is often found 'offensive' by proponents of the aggregate concept), another piece of evidence has appeared recently, which one could argue should also have settled the debate very effectively. Garland et al. (2023) recently conceded that once a soil sample is dismantled into aggregates, it is currently impossible (and it does not seem likely to be feasible in the foreseeable future) to determine where each individual aggregate was located in the original, undisturbed soil, a conclusion that is in line with the earlier experimental work of Koestel et al. (2021). In effect, this means that whatever one learns by looking at a given aggregate can never be used retroactively to explain the functioning of the original soil that the aggregate came from. It also means that one cannot monitor over time the fate of any individual soil aggregate, which would seem to be a prerequisite to study the kinetic of aggregate formation. Despite that, it appears from the open review process of the article of Garland et al. (2023) that the authors and at least one of the reviewers still consider that it remains important to consider the concept of soil aggregate when trying to predict the functions of soils (see review at https://soil.copernicus.org/articles/10/23/2024/soil-10-23-2024-discussion.html).

If the existence of solid experimental evidence or theoretical justification does not appear to settle debates in soil science, perhaps, conversely, it might explain the rapid adoption of novel perspectives and ideas. However, that too appears contradicted by factual observations. One could argue that the enthusiasm for the use of fractal geometry to describe soils in the early 1990s was not based on any evidence that this theory was useful to predict any of the processes we were interested in. In that respect, Kadanoff (1993) warned early on that fractals were strictly descriptive and not predictive. Nevertheless, that did not prevent many researchers in soil science from adopting fractal geometry in their work, at least for a few years, until Kadanoff's assessment became evident. To a large extent, one could argue that the same situation pertained to the research on the addition of pyrolyzed organic matter to soils. Although there was virtually no experimental evidence initially to suggest that this practice made sense, that did not prevent hords of researchers from jumping on the band wagon very rapidly.

## 6 | PROMPT ADOPTION OF NOVEL IDEAS OR PERSPECTIVES: TRIGGERED BY HYPE?

Another possible explanation for the observation that some ideas, like those briefly alluded to in the previous section, are adopted very rapidly by soil researchers, whereas others are not, is that it has to do with the fact that the progenitors or promoters of the quickly adopted perspectives published high-profile articles, meant to rapidly convince a wider public, including decision-makers, that the novel ideas held huge promises. In his landmark book, Mandelbrot (1982) described fractal geometry as 'the' fundamental geometry of nature and in many ways a 'theory of everything'. With such a launch of the theory, one should probably not be astounded that, soon after the publication of a series of pioneering articles in 1989, increasing numbers of soil physicists, at least for a time, focused their work on the application of fractals to soils. Likewise, given the way metagenomics was ballyhooed by Handelsman et al. (1998), who coined the term, it is not surprising that soil researchers adopted the technique with such manifest keenness. That was also definitely the case with biochars. Research on the topic took off like a rocket as soon as biochars were advertised as a providential, 'win-win' or even 'win-win-win' solution to a number of problems of great societal relevance, like global climate change and food insecurity (e.g., Bruges, 2009; Laird, 2008; Lean, 2008; Lehmann, 2007a, 2007b; Lehmann et al., 2006; Marris, 2006; Renner, 2007).

In the case of the rapid adoption of meta-analyses in soil science, it is less clear that it is the result of a

high-profile article. Evidence suggests instead that it is the widespread popularity of the topic in scientific research in general that has eventually convinced soil scientists to start using the method.

Even more clear as counter-evidence against the 'high-profile publication' hypothesis is the fact that, in many other cases, articles published in high impact journals by prominent researchers have not managed to entice soil scientists to relatively rapidly redirect their research. For example, even though Wall et al. (2015) published an interesting article in Nature, in which they suggested that soil scientists could have a significant role in elucidating the factors controlling the incidence of soil-transmitted helminths infections, which affect a very large proportion (almost 2 billions) of the world's population, in poor countries (Schlosser-Brandenburg et al., 2023), very little research has been carried out in soil science along those lines since 2015. Likewise, a little over a decade ago, several authors (Clothier, 2010; Kahlon & Lal, 2011; Sposito, 2013) tried to stimulate enthusiasm among soil scientists for the concept of 'green water', first introduced by Rockström (1999) and Falkenmark and Rockström (2004, 2006) and defined as the fraction of rainfall that infiltrates into the soil and is available to plants. However, the seed did not take, and the concept has been seldom mentioned in the soil science literature during the last 10 years. Another example, more recently, concerns claims that sound could be used to monitor the activity of a whole range of organisms in soils including, counter-intuitively (and without any experimental evidence), even bacteria. Rillig et al. (2019) tried to stimulate interest in this technique by arguing that sound 'could be useful in an "acoustics-based" precision agriculture as a means of assessing aspects of soil biodiversity, and the topic of sound pollution could move into focus for soil biota and processes'. Again, this did not give rise so far to any significant trend among soil scientists to put their ear to the ground, so to speak.

Thus, although it may be appealing at first to think that the quick espousal of some novel ideas is strongly fostered by the publication of high-profile articles or by significant hype, there are enough counter-examples to conclude that something else must be at play.

## 7 | LACK OF SUITABLE METHODS MAY SLOW, BUT NOT PREVENT, PERSPECTIVE CHANGES

As some of the examples mentioned earlier show, the lack of a suitable technology can in some instances slows down the adoption of a new perspective, and conversely, its availability may also sometimes accelerate a transition to a new paradigm.

For the slowing-down effect, the advent of commercial X-ray CT is a very clear case in point. After 1991, when Letey (1991) introduced a new perspective on soil architecture, it took a little over 10 years to be able to do anything about it, until the first table-top X-ray scanners made it possible to routinely describe quantitatively the geometry and connectivity of the pore space in soils. When the first such machines became commercially available, a few laboratories jumped on the opportunity and demonstrated their potential, which progressively encouraged other groups to follow suit. It is only when there was a sufficient number of machines dedicated to soil research that the movement really picked up momentum. Superficially, it seems that one could say the same about the application of metagenomics to soils, which took off in earnest only after a suitable technology, shotgun sequencing, became routinely available in a sufficient number of laboratories. However, one could argue that these two situations are different. In the latter case, the transition from single-species genomics to metagenomics seems to have been relatively rapidly accepted at a conceptual level by soil microbiologists, who saw in it an ideal way to make their discipline evolve toward more quantitative analysis than had been possible before. As a result, many researchers were ready to implement the new approach as soon as the technology made it practical to do so. In the case of the use of X-ray CT, it seems fair to say that most soil scientists were not convinced that there was anything wrong with the traditional aggregate perspective when CT scanners became available. Some individuals are still not convinced of it at this stage. So, even though clearly the non-availability of a suitable technology can slow down perspective shifts, it is not obvious that it has much to do with preventing them in the long run.

Conversely, the existence of a given technology may speed up considerably the adoption of a new idea or the shift to a new perspective. Certainly, the fact that portable equipment for the pyrolysis of organic residues was readily available 20 years ago made it possible for biochars to be considered widely as a soil amendment around the world as soon as they were portrayed as a win-win solution. It is not clear, however, that technology is the cause of the initial perspective shift. A counter-example is provided by the proposed use of acoustics to monitor the behaviour of microorganisms in soils. The technology to do so is readily available and could be implemented easily in practice, and yet, nobody seems to have jumped on the idea since it was suggested.

## 8 | VOLUMINOUS LITERATURE SWAMPING DISSENTING VIEWPOINTS?

Yet another possible explanation is related to an interesting theoretical argument put forth recently by Chu and Evans (2021) in support of the claim that 'too many papers published each year in a field can lead to stagnation rather than advance'. According to these authors, 'the deluge of new papers may deprive reviewers and readers the cognitive slack required to fully recognize and understand novel ideas'. Examining 1.8 billion citations among 90 million articles across 241 subjects, they find that 'when the number of papers published per year in a scientific field grows large, citations flow disproportionately to already well-cited papers; the list of most-cited papers ossifies; new papers are unlikely to ever become highly cited, and when they do, it is not through a gradual, cumulative process of attention gathering; and newly published papers become unlikely to disrupt existing work'. These observations are intriguing, but one might argue that when a field grows very large, what happens is not that revolutionary ideas get lost in the mass of publications. Instead, what might happen is that Highly Cited Researchers (HCR) ride such a huge wave of citations year after year, bringing them fame and financial rewards, that they have no interest in paying much attention to ideas that may challenge their privileges.

Be that as it may, it is not clear to what extent Chu and Evans' (2021) ideas and observations apply to the soil science literature, or to the subset of this literature that concerns each of the topics, mentioned above, about which perspective shifts have been slow to occur. Compared with many other fields, the discipline of soil science is small, numerically, but many other disciplines publish about soil issues these days. According to Google Scholar, 174,000 articles published in 2023 contain the word 'soil', with 58,700 containing it in their title. These numbers would put the overall literature on soils within the range encompassed in Chu and Evans' (2021) analysis. Sub-disciplines in soil science might also fit in this range. For example, since 1450 articles, again according to Google Scholar, were published in 2023 with both 'soil' and 'biochar' in their title, one might see a size effect on the citation pattern, with articles that present a dissenting point of view on the usefulness of biochars in agriculture, for example, being largely ignored. That is indeed what one observes in practice.

However, if we concentrate on the specific soil-related topics, mentioned above, about which perspective shifts have been slow or have not taken place at all, the numbers of relevant articles are far lower. In particular, again in 2023, only 275 articles contained both the words 'soil' and 'aggregate' in their title. With such a low number of articles, it is hard to imagine that any new idea might get swamped by the sheer volume of the literature. As a case in point, the review article by Rabot et al. (2018) has been cited abundantly since it was published, even though it explicitly mentioned the existence of two different approaches toward the structure of soils. With only a few hundred articles being published on the topic per year, it seems likely that something else than the volume of the literature explains why some ideas are not taken on board rapidly, whereas others are.

## 9 | DIFFICULTIES OF ENGAGING IN INTERDISCIPLINARY RESEARCH

That 'something else', we argue, appears to be intimately related to interdisciplinarity. Perhaps the easiest way to grasp this is to analyse a little more deeply what a transition from soil aggregates to a largely aggregate-less perspective of soil architecture entails practically in terms of several disciplines collaborating together.

The traditional perspective of soils being constituted of more-or-less stable aggregates of different dimensions can be adopted by specialists of any of the various sub-disciplines of soil science without necessarily requiring assistance from researchers from other disciplines. In other words, it is compatible with a mono-disciplinary approach to soils. Indeed, all that is required, from this perspective, to characterize the structure of soils, is to dismantle soil samples in various chunks and to use a sieve (which is attractively easy), after which one can proceed to various classical measurements. For example, researchers interested in the question of how soil structure affects the distributions of organic matter or microorganisms can simply measure these parameters in different classes of aggregate sizes (e.g., Puget et al., 2000; Six et al., 2000).

If, on the contrary, one adopts the perspective that soil architecture should be studied in undisturbed soil samples, and that what matters in the description of soil properties and functions is the geometry, continuity and connectivity of the pore space in soils, as well as the physico-chemical properties of the pore surfaces, then a strictly mono-disciplinary approach in soil chemistry or soil microbiology is no longer sufficient. At the very least, a cross-disciplinary approach is needed, in which concepts and tools are borrowed from other disciplines to reach specific objectives (see illustration in Figure 3). In particular, computed tomography and image analysis techniques typically developed by soil physicists to characterize the matrix of soils need to be used. If one wants to go beyond merely characterizing the static properties

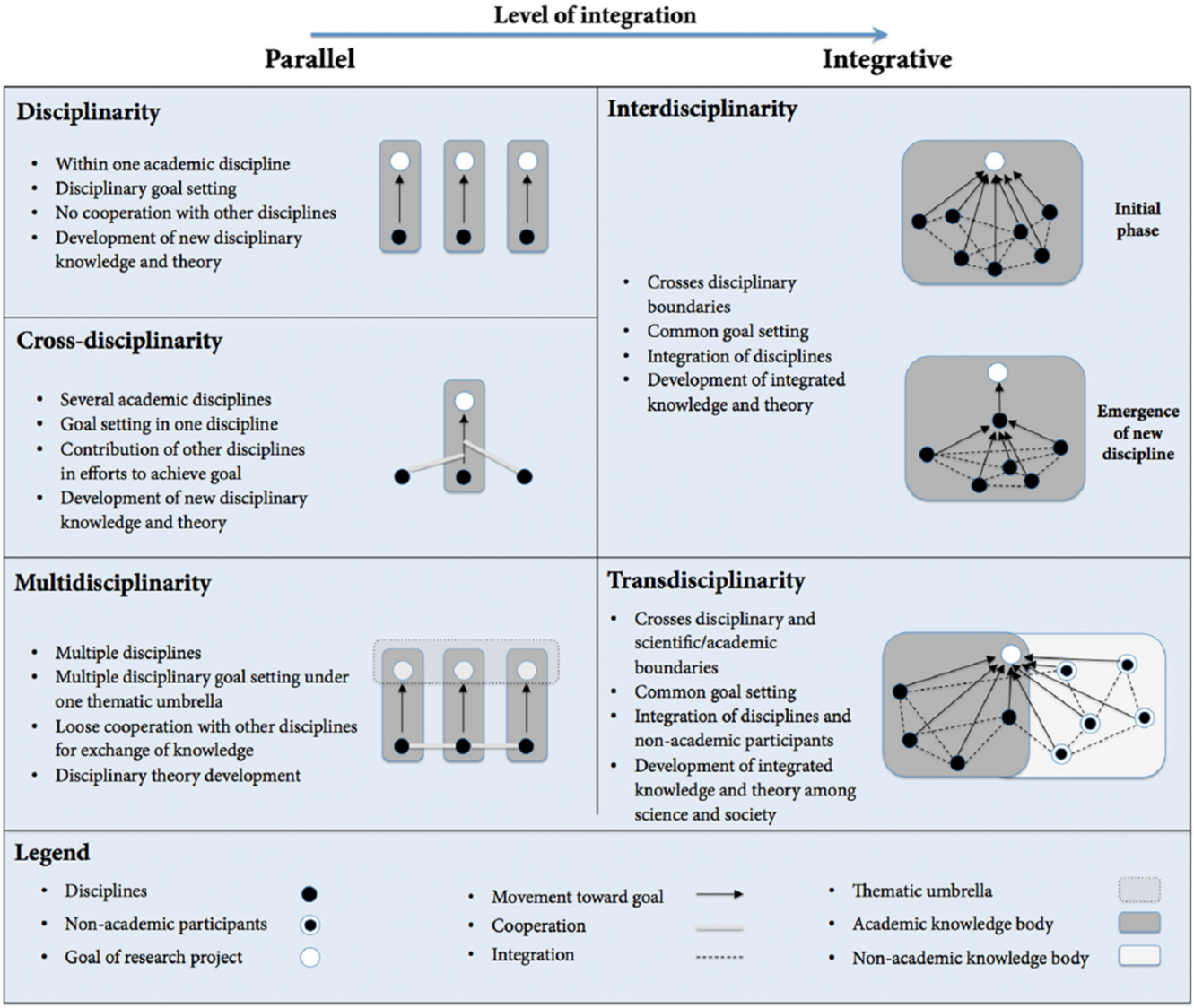


FIGURE 3 On the nature of different types of research efforts involving one or more discipline(s) (reprinted from Baveye et al., 2014, with permission).

of soils, but one is interested as well in describing their dynamics, for example, through computer models, experience has shown over the last decade that the close collaboration of researchers from different disciplines is absolutely required. At the onset, this collaboration may be multidisciplinary, where mono-disciplinary efforts run in parallel (Figure 3). However, as the work proceeds, and the contributions of the different disciplines become increasingly intermingled, it is clear that what emerges are full-fledged interdisciplinary research endeavours, where the complementary contributions of researchers from different disciplines become so closely entangled that they are no longer distinguishable (see, e.g., Baveye et al., 2018, 2022; Pot et al., 2022a, 2022b; Vogel et al., 2022).

As Baveye and Wander (2019) have discussed in detail, one can argue that in the case of soil humic substances as well, a shift of perspective to Waksman's (1936) viewpoint requires an interdisciplinary approach, involving soil physicists, (bio)chemists and microbiologists working in close collaboration. Likewise, to consider not just the soils-crops-human nutrition nexus but also soil-transmitted diseases, soil scientists belonging to several sub-disciplines would have to collaborate closely with, for example, epidemiologists, biologists (e.g., specialists of nematodes) and physicians.

The observation that some aspects of soils mandate interdisciplinary research is not new. Over the last few decades, there have been frequent and insistent calls for more interdisciplinary research efforts in that context (e.g., Baveye et al., 2014; Bouma & Hoosbeek, 1995; Brevik et al., 2015; Brown et al., 2021; Dobrovol'skii, 2006; Keller et al., 2013; Levy, 2012; Pohl et al., 2022; Portell et al., 2021;

Teuber et al., 2022; Wagenet et al., 1992). Many of the same authors who have made these repeated calls have also pointed out the very daunting obstacles that such endeavours face in soil science, because of funding, institutional and educational causes.

In terms of the financial support for research, even though many granting agencies openly advertise an interest in supporting interdisciplinary research, anyone who has served on proposal review panels at any level (regional, national or supra-national) quickly realizes that in practice, interdisciplinary proposals suffer from a sizeable handicap. In most cases, because of their educational background, both reviewers and panel members tend to use a mono-disciplinary lens to scrutinize proposals, and have a hard time relating to objectives that, in interdisciplinary efforts, fall outside the confines of any single discipline. When, by chance, truly interdisciplinary proposals manage to get funded, mechanisms are rarely in place to ensure that the ensuing research efforts do not get retrograded to multi-disciplinary ones, where several disciplines pursue mono-disciplinary objectives in parallel, but fail to work on elaborating a joint goal (Figure 3).

At the institutional level as well, several different traditions and structures concur to discourage researchers from engaging in interdisciplinary work. The division of most universities and research centres into distinct departments, or the mono-disciplinary focus of most appointments within these institutions, hinder interdisciplinarity in practice. To take an example close to home (for PCB), INRAe soil scientists of different subspecialties are located in different parts of France: soil microbiology in Dijon, soil (bio)chemistry in Ile de France (near Paris), soil physics in Orléans and Avignon. Even within one sub-discipline, soil (bio)chemistry, specialists of organic carbon are near Paris, whereas nitrogen experts are in Dijon and Orléans. That does not render interdisciplinary research impossible, but it definitely does not facilitate it!

In many cases, even the buildings in which we carry out our work do not foster interactions among researchers. In most academic institutions and research centres around the world, researchers are housed in individual offices located along austere, hospital-like hallways (Figure 3, left). Contacts among office neighbours can sometimes be very minimal in such spatial arrangements, and unless institutions provide space for spatial interactions, for example, for coffee breaks or meals, the type of casual exchange that promotes interdisciplinarity may never take place at all. This got further exacerbated by the increasing number of academics that have taken up working from home in the aftermath of COVID, resorting to organized meetings on-line and with newly appointed academics only occasionally meeting their colleagues face-to-face.

Another institutional aspect that is not particularly favourable to the conduct of interdisciplinary research is related to the 'publish or perish' trend started half a century ago. More and more since then, pressure has been exerted on researchers to publish abundantly. A sizeable publication record has become a *sine qua non* condition for employment or for advancement in one's career. In this context, especially for early career researchers, emphasis has to be put on research efforts that guarantee a steady stream of publications in a timely fashion. Interdisciplinary projects rarely satisfy this condition, requiring as they do the co-evolution of ideas and the progressive co-construction of methods among several disciplines. Tress et al. (2005) point out that one of the greatest challenges of integrative research is to bring together different epistemologies, which requires 'researchers to become immersed in one another's knowledge cultures, to understand the fundamental differences in their basic theories and axioms'. This commonly requires a sizeable investment of time from all parties involved to understand each other's perspectives and develop a common language to communicate effectively. Under present conditions in academe, emphasizing quantity over quality, there is very little incentive to invest the time needed to carry out meaningful interdisciplinary efforts.

As many authors have noted, aside from all the funding and institutional hurdles, one of the major impediments to interdisciplinary research efforts is the education that many of us have received, and that we continue steadfastly to dispense to generation after generation of students of soil science. The tradition has been, and remains, for the different facets of soils to be taught in separate courses, like soil physics, soil organic matter, soil mineralogy, soil microbiology, soil ecology and so on. Pedology could conceivably have served as a way to integrate all these mono-disciplinary perspectives into a coherent theory; however, one could argue that its progressive emphasis on classification and soil mapping in the 1960s and beyond got it substantially off track in that respect. At present, in many institutions, it is possible for soil science graduates to never have had an exposure to attempts to put together all the pieces of the very difficult puzzles that soils constitute.

Given these different obstacles to interdisciplinary research efforts that researchers have to face routinely, it is not surprising that perspectives that inherently require such an approach would take a long time to get support and may not even be as keenly debated as one would expect. Conversely, new ideas or perspectives that do not require interdisciplinary efforts in any way can be easily adopted. The use of fractal geometry in

soil physics or of metagenomics in soil microbiology, for example, both required some learning from researchers at the beginning, but neither required a profound transformation of the dominant paradigms of the two disciplines, respectively. In both cases, researchers could continue doing research like they had done for years, in a mono-disciplinary fashion, but with a new tool at their disposal.

One could argue that the use of biochars in soils should have imperatively given rise to interdisciplinary research. Indeed, biochars are microporous materials, containing various chemical compounds (including nutrients and PAHs) resulting from the pyrolysis process. These features should logically have been fully characterized, prior to any attempt to understand the effects biochars might have on soils processes, which means practically that the soil organic matter specialists promoting biochars should in principle have collaborated with soil physicists to assess the microporous properties of different biochars (e.g., Schnee et al., 2016), and with soil (bio)chemists to identify all the chemicals, especially toxic ones, contained in the biochars initially and eventually diffusing out in the soils. This interdisciplinary effort did not take place. Biochars, from the start, were treated as a reagent-like material, a representation made more explicit by the use of the singular, 'biochar', as if it were a uniform product, like any other chemical reagent used in research. One could argue that the lack of an interdisciplinary perspective explains why the research on biochars use in agriculture took off as rapidly as it did.

Likewise, adoption of meta-analytic methods, like that of any other statistical technique, probably seemed straightforward enough initially that soil science researchers did not see a need to engage in interdisciplinary collaboration with professional statisticians to implement the method in their work. From that viewpoint, all that was needed, according to the model of cross-disciplinary research exhibited in Figure 3, was to borrow the meta-analytic procedure and incorporate it within mono-disciplinary efforts. In retrospect, it turns out that this approach did not produce very sound results, as shown in detail in the recent article of Fohrafellner et al. (2023), who identified a number of serious deficiencies in many of the meta-analyses on soil organic carbon published in the soil science literature. Again, one could argue that if, from the onset, the application of meta-analyses in soil science had been viewed as an interdisciplinary endeavour, adoption of this approach would have been significantly slower, in part because statisticians would have pointed out the fact that since many primary studies lacked proper statistics (e.g., variance) and researchers rarely reported on negative outcomes in the open literature, available data were not sufficient in many cases to carry out sound meta-analyses.

## 10 | TAKE-HOME MESSAGE AND THE ROAD AHEAD

A key take-home message of the above analysis is that difficulties associated with the pursuit of interdisciplinary research efforts in soil science appear to explain reasonably well why some innovative perspectives or ideas are not taken on board rapidly, nor even discussed very much at all, while at the same time novel perspectives that can easily be accommodated within a mono-disciplinary approach tend to be adopted quickly.

Given the nature of soils, and in particular the fact that most processes occurring in them are the result of interactions, at minute spatial scales, between very diverse micro-, meso- and macro-organisms, minerals, chemical compounds of many different kinds, water and several gases ($CH_4$, $N_2O$, $CO_2$, ...), it would seem to be a no-brainer that a good deal of the research carried out on soils should intrinsically occur at the confluence of many different disciplines. That has not been the case historically, and to some extent, from a reductionist perspective of science, this may have been more-or-less understandable 40 or 50 years ago. However, after decades of largely mono-disciplinary avenues of research, one could contend that the time to make them coalesce into an interdisciplinary framework has come, or came, several years ago already. In that context, one could consider worrisome that, still in 2024, the apparent reluctance of some to engage in interdisciplinary research appears to be the most plausible explanation for the fact that the reflexion on certain aspects of soils does not seem to make very much progress. Indeed, one could argue strongly that in order to address the many critical problems facing humanity at the moment, from food security to environmental contamination to global climate change, disciplinary blinders are likely going to constitute very serious roadblocks. To take just one example, agronomists are currently suggesting that a partial solution to some of these problems would be to increase through breeding the capacity of plants to obtain nutrients more efficiently from soil microsites where microbial mineralization-immobilization (MIT) operates actively (e.g., Briat et al., 2020; Ciampitti et al., 2022; Lemaire et al., 2021; Lemaire & Gastal, 2018). Soil scientists are, and will increasingly be, actively solicited to join in this effort along with plant breeders, agronomists, microbiologists and many other disciplines.

From the standpoint that interdisciplinary endeavours are critically needed in soil science, and that as a corollary, the reflexion on many aspects of soils needs to unfold unencumbered by disciplinary binders, then the question is what can be done practically to remove these blinders. At the funding level, one might consider that very little can be done, and we just have to accept the decisions made by authorities. However, one could argue

**FIGURE 4** (a) Illustration of a typical hallway in a university or research institution, with individual offices whose doors often have to remain closed not to offset the air circulation in the building, and (b) open-floor office space at the Simbios Center in Dundee (Scotland) in which a group of about 15 researchers belonging to different specialties collaborated from 2005 to 2015, including the three authors of this article. For meetings, sound-proof 'break-out' rooms were available, in order not to disturb the group (photo credits: Philippe C. Baveye).

that soil scientists regularly are members of proposal review panels and, in that capacity, could communicate to the decision-makers that the current approach, favouring mono-disciplinary proposals, is not optimal for the research on soils. We can continue to write articles conveying the same message in the hope that eventually, the coin will drop...

At the institutional level, some of the same communication effort will have to be carried out, not only to let academic or research leaders know that the criteria used to evaluate the performance of soil researchers should be adapted in the future to the interdisciplinary nature of the topic we address. From experience, all three of us know firsthand that some university leaders can be convinced by cogent arguments in that sense, to the extent that they are even willing to provide a physical space where the interactions of researchers from different backgrounds is strongly stimulated (Figure 4, right panel).

Perhaps the most critical change that should take place in priority is in terms of the education and training of soil scientists. In the context of promoting interdisciplinary research on soils, it is clear that the traditional mono-disciplinary focus of courses at both undergraduate and graduate levels (or graduate and post-graduate levels in the UK) is inappropriate. From the start, soil science needs to be presented to students as an integrated discipline, with many intimately interacting components that cannot be dissociated in practice. That seems like a tall order, but there is very fortunately a precedent for this kind of sea change. Traditionally, until the late 60s, all medical programs in the world required students to take mono-disciplinary courses (e.g., in anatomy, biochemistry, virology, etc.) for several years before they would be allowed eventually to start dealing with patient cases. In 1968, the newly created medical school at McMaster University (in Hamilton, Canada) launched a revolutionary new curriculum, where from the start, teams of students were confronted to the situation of patients having specific pathologies, about which the students were supposed to gather information on their own (or with the assistance of faculty members) in order to determine a suitable treatment. Along the way, after reviewing the files of a number of patients, the students would eventually learn all the bases of anatomy, biochemistry, etc., required for medical practice. Since 1968, many medical schools around the world have adopted this approach, known as 'problem-based' learning (PBL), with great success. Over the years, various soil scientists have suggested that PBL, or variants of it, should be adopted in soil science education as well, in particular to develop in students the aptitude to not only be self-directed in their learning, but also to be able to participate easily in interdisciplinary efforts (Figure 4) (e.g., Amador, 2019; Amador & Görres, 2004; Bastos et al., 2022, 2023; Baveye, 2000; Brown et al., 2021).

## AUTHOR CONTRIBUTIONS

**Philippe C. Baveye:** Conceptualization; formal analysis; writing – original draft; writing – review and editing. **Wilfred Otten:** Conceptualization; formal analysis; writing – review and editing. **Iain Young:** Conceptualization; formal analysis; writing – review and editing.

## CONFLICT OF INTEREST STATEMENT

The authors declare that the research was conducted in the absence of any commercial or financial relationships that could be construed as potential conflict of interest.

## DATA AVAILABILITY STATEMENT

Data sharing not applicable to this article as no datasets were generated or analysed during the current study.

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