Toward a useful dialogue,
Stephan Reeve
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From: Minifarms
 Subject: poor soils/Bunch

Dear Mulchers,
If you want to understand how poor soils [low in most things] can have high yields, you need to read and/or download a document by Roland Bunch.
It can be found at: **http://ppathw3.cals.cornell.edu/mba_project/moist/Roland.pdf** .
Excellent.

Ken Hargesheimer
Minifarms

From: "Reid, Aileen"
Subject: Soil fertility without NPK?

I haven't had time to follow all the dialogue but one thread coming through makes a lot of sense according to what we are experiencing here in Perth,Western Australia. We have been working with compost, trying to build soil out of our gutless sands. That is one project. The other project I have been involved in is trying to grow organically in sand (y) soil. Neither is working.

The organic approach runs into MAJOR N problems. We can't get enough organic N - even with green manures under the constraints of the NASAA accreditation. And even the biodynamic people we have surveyed that have been doing it for years, still don't have appreciable levels of organic matter in their soil . And they are putting on in the order of 150t/ha/year of compost - way above what NASAA allows.

On the compost side of things we are running into problems with irrigation due to the non-wetting characteristics of the compost. Water is running through/slipping through the profile more than before. Our P goes up high quickly - don't tell me using compost - or organic production in sands prevents leaching of P!

We are coming to the conclusion - and in fact our most trials are incorporating clay to try and get a bit more structure into the soil.

Aileen Reid
[Perth, Western Austrailia]

From: Norman Uphoff
Subject: Soliciting comments: P Sustainability

I am interested in the lively debate on the MULCH-L list-serve that you told me about, over the extent to which one needs to "replace" P taken out of mulch/organic systems, or whether biological processes can "replenish" the supply, in whole or in large part. As you know, I am not an agronomist, but I have been looking into this question in order to understand how and why we continue to get some remarkable rice yields with the System of Rice Intensification (SRI) developed in Madagascar. I will assume that most MULCHers know about SRI already, or can easily learn about the System from our home page that is part of your/CIIFAD's outreach operation < **http://ciifad.cornell.edu/sri/** >

This issue of P limitation, and the effects of taking off large harvests where available P is low, was raised dramatically when we found that farmers around Ranomafana National Park, by using SRI methods, could raise their irrigated rice yields from 2 t/ha to 8 t/ha on average (some got as high as 12-16 t/ha) on soils that NC State PhD agronomy thesis research had concluded were some of the poorest NC State ever evaluated: pH 3.8-4.5; low to very low CEC in all horizons; Fe toxicity; Al toxicity. The most critical deficiency pointed out was an average (available) P of only 3-4 ppm, less than half the usually assumed threshold for getting an acceptable yield. The low yields usually obtained around Ranomafana were attributed to P deficiency, among other things.

The 1994 thesis by Bruce Johnson said that there were "no inherently fertile soils within tens of kilometers of the park" due to the nature of the parent rock from which the soils had been created. With HYVs and fertilizer, NC State staff helped farmers get average yield up from 2 t/ha to 3 t/ha, with a maximum of 5 t/ha. How could our NGO partner help farmers get 8 t/ha average and up to 16 t/ha without fertilizer or new vareities?

With such dismal soil chemistry, how could yields be quadrupled, not just one year but for five years in a row, with no sign of yield decline on fields where SRI was used, despite the high yields taken off? A few yields even increased 6-8 times, without adding chemical fertilizer to build up the soil fertility. Something strange was going on, though it was surely something explainable.

My proposition now, after several years of observation, talking with farmers, and reading in the literature, is that the soil, plant, water and nutrient management practices have been building up the soil in terms of abundance and diversity of microbial life, and they in turn have been improving the soil chemically and physically. Farmers say that their soil gets "better" year to year with SRI cultivation, without adding fertilizer, despite taking off high yields. Farmers who put compost on their fields usually put it on their inter-season vegetable crop (potatoes, beans or peas) rather than on the rice crop, and get better yields from both that way than by putting it onto the rice directly.

The main changes in soil and water management with SRI are keeping the soil moist during the vegetative growth period but never continuously saturated, so that it does not become anaerobic for more than a few days at a time. This is done through alternate periods (up to 5 days) of flooding and then draining and keeping the field dry; or applying small amounts of water daily in the afternoon or evening and draining off any standing excess in the morning, with the field being drained for 3-5 days at a time several times during the growth period.

There is evidence that mixing aerobic and anaerobic horizons increases biological nitrogen fixation (BNF), which could explain where the N comes from for these high yields. Surely having both aerobic and anaerobic phases or horizons means that there is more opportunity for BNF. This could explain that part of the high yield with SRI. But where does the P come from?

We think that there is also increased P solubilization promoted by the plant, soil, water and nutrient management practices.There is an article that appeared in NATURE in May 2001, by British environmental scientists (who don't like to have P in the soil runoff) which I can forward to those who are interested (contact lhf2@cornell.edu as attachments are discouraged on mulch-L). This shows that when soil is wetted and dried alternately, the soluble P in soil water increases tremendously (the range report from studies in the UK were 185-1,900%, admittedly from a low base but huge relative increases).

The mechanism is for aerobic bacteria to acquire P from the "unavailable" pool in the soil, for their own purposes. When soil is saturated, these aerobes lyse (burst) under the osmotic pressure and release their P (and other nutrients) into the soil solution. When the soil dries again, the aerobes go back to work "mining" P from parts of the soil that the plant cannot normally access. Thus a process of wetting and drying soil can accomplish "microbiological weathering" that complements or competes with "thermogeochemical weathering," which is the process usually referred to to explain soil buildup. I think, though we don't have evidence on this, that MB weathering can be much faster, and more abundant, than TGC weathering.

I have read estimates that about 90% of the P in soil is "unavailable," meaning not accessible to the plant, for a variety of reasons, pH levels, physical location, sequestering in soil structure, etc. The process of continually renewing the "available" pool has gone on for eons, only disrupted by our agricultural practices in recent decades of centuries. The point is that there are huge reserves of P, and the question is, can this be accessed efficiently, sufficiently?

I remind myself when thinking about this question that plants have grown on the earth's surface for more than 300 million years. Surely there has been a lot of natural recycling, but it seems likely that even with 99.9% recycling, there has had to be massive transfer of unavailable P into available P to sustain obviously robust plant-based ecosystems. This is done surely not just by TGC processes.

Are we in danger of running out of P? This is certainly a possibility, but I think it is exaggerated because we do not pay enough attention to, or do enough to support, soil microbiological capabilities and processes. We note, with respect to SRI, that under flooded (anaerobic) conditions the rice plants will be deprived of the nutrient-accessing services of mycorrhizal fungi, which support the nutrient uptake of about 90% of plants. They can expand the volume of soil accessed by a root system, extended by mycorrhizal "infection," by 10-100 times.

Mycorrhizal fungi are especially important for accessing and uptaking P. So plant/soil/water/nutrient management practices that support mycorrhizal associations could be compensating for superficially reduced supplies of P. The mycorrhizal hyphae can get into soil pores smaller than roots can access. This is "mining" the soil, in a way, but there are no opportunity costs since this P could hardly be taken up otherwise.

I would also call our attention to what we all learned in Biology 101, about the plant stem being a two-way street (remember the xylem and the phloem? I always wondered why there would be any vascular tissue carrying nutrients down from the canopy into the roots). Plants send about 30-60% of their photosynthate into the roots, where some is exuded into the rhizosphere (to "feed" the bacteria, fungi, protozoa, etc.) while other material is lost through rhizodeposition (with the same effect). How much time has anyone in the MULCH network spent thinking about exudates in the past year? I think we should be paying a lot more attention to them.

Evolutionarily speaking, it is pretty clear that if plants did not get back more benefit from their exudation and root cell losses than their biological cost of creating this material (sugars, amino acids, vitamins, hormones, etc.), they could not have evolved as they have over hundreds of millions of years. In fact, we should never look at a plant as a separate species; its survival depends on its intimate association with millions and millions of microorganisms, just as ours does as mammals. [This is a view obviously influenced by the work of Lynn Margulis; anyone who hasn't read her book MICROCOSMOS has a treat in store. Her more recent books are even better, but broader.]

So I think there is reason for questioning a lot of the "closed system, chemically-focused" thinking that has gone into the conceptualization and measurement of soil/plant/nutrient relationships reported in current agronomic science. This is a bold statement that I can make more safely from