Dear Prof. Dharmawardena,

I indeed have missed out on your reply. It seems that you have not fully appreciated my views on the fertility levels in tropical soils. The scientist, researchers, farmers, and consumers were unaware of the harm done in applying inorganic fertilizers and other agrochemicals to ferruginous soils. As a result, many field trials of organic as well as inorganic fertilizer experiments were carried out without knowing the base conditions, like carbon depletion. Because of this reason, the yield responses to inorganic fertilizer over the years were like BFBFs applications, very erratic.

Our studies with 30% inorganic fertilizer and 70% biochar gave similar yields to inorganic fertilizers in the first year, but higher yields than 100% inorganic in the second year (Gamage et al., 2015). Dr. Gamage was able to highlight the role of biochar in the mobility of the phosphorous cycle as an adsorption receptor. Pot experiments, as well as field trials, were done on statistically designed experimentations that were evaluated and published in reputed journals. We have published much research on biochar. If you wish, I can circulate them so that people become familiar with sustainable agriculture productions in the tropics. In fact, TRI research shows similar or better yields with 100% biochar vs inorganic fertilizer. I am still waiting to conduct official comparative trials in tea, which will only take six weeks to show superiority of BFBFs/quality compost with biochar over inorganic fertilizer because we have excellent results in many plantations applied with biochar biofertilizer. We can do the same in CKDu affected areas to monitor As and Cd uptake rates leftover from inorganic fertilizers, but it will take at least one season. However, there are locally produced commercially available biochar biofertilizers as well as productions by farmer cooperatives.

As I mentioned in the previous email, a more detailed response can be given if we reexamine the available data with NIFS and the Department of Agriculture (inorganic fertilizer trials). We will certainly examine and learn from Dr. Nimal Chandrasena's suggested readings.

You have quite correctly pointed out that one could leave the fields fallow for a period to rejuvenate the soils to conduct the field trials. But then, if you do not remove the plants grown during the fallow at the correct time (after the highest uptake rate like in constructed wetlands to prevent precipitation of these heavy metals (HMs) from wastewater), the HM concentrations in the soils cannot be removed, thus toxicity remains. It is then a burden because we need to consider soil remediation and restorations.

Therefore, as I stated before, it is timely to undertake a comprehensive review of all the publications and recommend remedial measures to apply organic fertilizers to overcome toxicities and inhibitive conditions facing the farmers and the consumers.

This review should be based on two important aspects, namely: HM in soils and in food. I purposely did not give my evaluation of the paper "Arsenic and Cadmium and Associated Risk in Farm Soils of the Dry Zone Sri Lanka where Chronic Kidney Disease of Unknown Etiology (CKDu) is Endemic" Gunadasa et al., 2021., as opposed to "Chapter One - Managing cadmium in agricultural systems". I left it to the capabilities of each one of you to comprehend scientific misconceptions.

Unfortunately, your reasoning is the same as most soil scientists' and some geologists' points of view so far on HM concentrations in the soils. In other words, safe HM levels are permitted in soils. Apparently, the paper in reference and others you refer to, compared heavy metal concentrations in CKDu locations to higher levels elsewhere. Hambanthota soils, as published, accumulate more heavy metals than Giradurukotte. It stands to reason that the applied heavy metals by means of fertilizer directly and indirectly through irrigation water have been uptaken by the plants in Girandurukotte than Hambanthota. We must interpret in terms of uptake rates rather than based on simple conclusions made in these reputed journal papers. We must also compare with uncultivated lands as Bandara et al., 2010 did. They reported that the mean Cd content in cultivated vs. uncultivated soils in Anuradhapura district was 0.02 ± 0.01 vs. 0.11 ± 0.19 mg/kg while in Polonnaruwa district, it was 0.005 ± 0.004 vs. 0.016 ± 0.005 mg/kg. Also, other publications of Prof. J.M.R.S Bandara give different ways of heavy metal entering the food chain. Such marvelous works were disregarded by subsequent publications.

Therefore, the type of heavy metal entry into food should be considered because direct uptake from inorganic fertilizer is taking place in these soils rather than accumulating to form mineralized HMs. As you have mentioned in your paper Dharma-wardena, 2018, there are no Cd accumulations in soils, thus washed off or much held in the grain if you consider the entire watershed. Inorganic fertilizers or organic fertilizers formulated with cadmium sources promote chelation of Cd and N. Organometallic uptake is always preferred as reported by (Haydee and Delma, 2017: http://dx.doi.org/10.5772/67755).

At the time I wrote my observation, it was a hypothesis, but I hate to think it was speculative. I totally agree with you regarding the determination of heavy metal compounds in urea using plasma mass spectroscopy or other methods. In fact, only a few studies were done to determine the HM quantities in inorganic fertilizers (Benson, 2014: Trace Metals Levels in Inorganic Fertilizers Commercially Available in Nigeria) but not the structural forms. It will be wonderful to find out the type of binding to additional carbon in urea fertilizer. It is also surprising such studies have not been undertaken. It is understandable since the contaminate quantities would vary depending on the industrial process and the age of the equipment used. In India, most of the naphtha plants have been converted to gas, but high-temperature steels in the piping and reactors contaminate (Engwa et al., 2018) (https://www.intechopen.com/chapters/64762) in the urea production processes. It will be very difficult for anyone to take responsibility for negating this fact stemming from industrial processes because it is legally binding. It is also the case for all agrochemicals since any patient or relative of the affected party can take legal action because there are proven cases in many parts of the world.

It is a statistical nightmare to compare highly variable contaminants in urea compared to a few research studies indicating low concentrations of heavy metals in grains. For example, the values of As and Cd in grain varied from <18.17-575.94 and <6.00 - 261.786 μg/kg, w/w respectively found by (Edirisinghe and Jinadasa, 2019). In such situations, the **precautionary principle** should be applied. We must also consider heavy metals in TSP that the farmers have applied for many years. Unfortunately, we are unable to find the quality of applied fertilizer over the years since it is not in the public domain. Perhaps the regulatory bodies did not statistically test the mandatory samples.

Now that we have ironed out the issue of soil, let's examine the water. We all agree that the first runoff or the release of the first irrigation water after a dry spell will contain the highest concentrations of heavy metals. We also need to consider rainfall pattern, storage, sediment load movement in the watersheds, and streamflow rates to undertake a detailed study on sediment and contaminant transport. Let us revisit those papers you have referred to evaluate if they have complied with the correct scientific methodology and request the authors to accept errata inclusions in those papers not meeting the outlined criteria. It should also include soil-related publications. It will be for the benefit of the authors because likely litigations to follow with the young educated classes in the affected rural communities. It is a kind request to make those publications worthwhile reading for the younger generations and make the scientific contributions outstanding.

Such drastic requisitions should be made also to the studies on heavy metal concentrations in grains because the EU has recently halved Maximum Permissible Limit (MPL) for cadmium. Certainly, the WHO will follow EU standards. https://eur-lex.europa.eu/eli/reg/2021/1323/oj#ntc*2-L_2021288EN.01001501-E0002. Please visit also https://www.foodsafetynews.com/2021/08/eu-lowers-lead-and-cadmium-limits-for-food-products/. Also please compare the EU standard with (Edirisinghe and Jinadasa, 2019). The MPL for As has also been reduced.
https://efsa.onlinelibrary.wiley.com/doi/full/10.2903/j.efsa.2021.6380

There is also the issue of iron deficiency in rice, which we need to explore. In the absence of adequate Fe levels, Cd, Hg, and other HMs dominate enzyme reactions in humans, either to increase growth coefficient or retardation coefficient in catalysts of transition metal forming complexes. Inevitably, we must reexamine the high-risk-soils (McLaughlin et al., 2021) and make new recommendations, which should include soil remediation efforts with desired microbial selections.

I do agree with you that microbial activities are complex and could mobilize leftover heavy metals, particularly when EM solutions are promoted to break down carbon. Indeed, fast substrate decomposing active microbes will lower the pH to mobilize heavy metals. On the contrary, the combination of biofertilizers/BFBFs, thermophilic compost, and biochar will be the best and wholesome fertilizer for replenishing the nutrients supplied to the consumers in the cities.

The city dwellers profited (only in monetary terms) from the harvests because all the governments promoted inorganic fertilizer subsidies to control the market prices. It also allowed the entrepreneurs (between takers in French) and the creditors at high interest to prosper. Such conditions create sluggish money flow in the agriculture sector, thus no investments trickled down to develop the farms. Farmers as well as the successive governments were always financially in debt by applying inorganic fertilizer. We would have sustained over the years many developed counties by importing inorganic fertilizers. Now it is the chance for the Farmers to take charge of the means of production and gradually improve to take over once again paddy processing, production of quality products, and get rewarding market prices and live healthy lives.

Please give them this chance!!!!

Also please help us to write a long-awaited review, which should be an official undertaking to uphold the policy on Organic Agriculture for improving the health of the populations and creating meaningful employment.

Thanking you, Kind regards,

Ben Basnayake

From Prof. Vijaya Kumara.
Dear Ben,
I am a little intrigued by your penultimate paragraph as I cannot see how a change from subsidized inorganic fertilizer to subsidized biofertilizer can make a difference in the relative prosperity of the entrepreneur/creditor elite vis-a-vis the farmer. The domination of elites controlling finance is a fact of life in capitalist societies and you would be naive to think a change in fertilizer will have a large impact.
All the best with your efforts at introducing a new fertilizer strategy. It is obvious that the initial effect of the transformation to "organic" has been a disaster for the farmers.
Regards
Vijaya

Dear Vijaya,

As you pointed out, most of us would regard the capitalist system to remain the same although the inputs are different. Unless otherwise, it is a paradigm shift. Such a shift can be expected from biochar because the farmers can produce it themselves. Biochar applications give consistent yields, unlike inorganic fertilizer. We are now promoting the farmers to produce biochar and compost. So that subsidized BFBFs or other biofertilizers will increase yields much above the maximum inorganic fertilizer productions in these ferruginous soils. They will be able to build up capital if they form cooperatives like in Europe. I believe several of them are sprouting.
Hope that well-trained Monsnato agronomists/sociologists will not kill them. The farmers also must demand guaranteed prices like in India. Etc etc

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From Chandre Dharmawardana

I will upload Dr. Ben Basnayake's comments to the BBF webpage

Together with some of the replies, somewhat abbreviated where suitable.

However, I will not make extended comments for the moment, especially on biochar etc. Instead, let me add the following for you all to mull over.

Ben B says that "Biochar applications give consistent yields, unlike inorganic fertilizer".

This is in my view the very opposite of what one would rationally expect.

Biochars are not substance with a fixed composition, but contain many substances including a gamut of polycyclic aromatics. So different biochars are different, and their mass-spectra or IR spectra provide unique finger prints that may even differ significantly from sample to sample.

Meanwhile the chemical composition of an inorganic fertilizer can be specified accurately to about one part in a trillion and we can make perfectly reproducible samples.

So leave aside consistent output results, even the input is not easy to specify accurately with biochars and organic manure unless one works very very hard.

The second issue is related to health effects that are being ignored by many people.

Polycyclic aromatic hydrocarbons in biochars may range from 12-400,000 micrograms/kg of biochar.

So, in effect, the upper-end biochars fall into class I carcinogens in terms of the IARC classifications of carcinogens (where glyphosate is only a class-II carcinogen).

When biochar applications become more widespread, these issues also will become more acute.

With inorganic fertilizers, we have had almost a century of experience and we know their short-comings, and how to guard against them.

In the case of microbial fertilizers, inoculation of the soil with a microbe to a normal soil is somewhat analogous to causing a medium to have "a microbial infection" where one monoculture of microbes becomes over-whelming.

The long-range consequences of such intoxication of the soil, and how the microbes evolve and mutate (even in the course of a few years) can be quite remarkable, as some simple calculations can show. Also, they show that a BFBF that works today will fail to work (or rarely, if you are lucky, might work better) after a few years.

So, all those effects have to be sorted out with these emerging technologies. Instead, people are ready to throw out the well tested and take the plunge into the unknown. So we have a lot of people who support "organic agriculture", but by that they mean all sorts of things.

The Sri Lankan government took that plunge last April, largely because of the simple but incorrect message of the toxin-free claim that has mass appeal, and the favourable claims of fuzzy minded scientists as well as "militants" who are ready to attack Monsanto and become "progressives".

The CKDu article by Valhose et al in the water journal (2021) is also another example of the dazzling attraction of an agrochemical explanation of the Sri Lankan CKDu that researchers have been hoping to see from day one as it fits in with their pre- conceptions, in spite of over-whelming evidence to the contrary.

In reality, In my view, Valhose et al have unwittingly provided some more key data to prove that CKDu has little to do with agrochemicals, as I will show in due course.

Gamini Seneviratne <gamini.se@nifs.ac.lk>
To:Bodhi Dhanapala
Cc:Vijaya Kumar,B.F.A. Basnayake,Chandre dharma-wardana,Nimal Chandrasena,Chandra Dissanayake

Sat., Dec. 11 at 1:23 a.m.
Dear All,

BFBF application is not inoculating a single or a mere mixed culture of microbes, but a beneficial microbial community. During BFBF manufacturing, the developing biofilms secrete a wider range of environmentally important biochemicals than when the microbes are in monoculture mode. Those biochemicals are the active ingredients of BFBF rather than its microbial cells. Their numerous actions in the soil increase ecosystem functioning, which leads to sustainability. Thus, the BFBF action is different from the action of conventional biofertilizers. That is why they show better beneficial effects than conventional biofertilizers.

When BFBF is applied with compost and biochar, whatever the toxic effects (e.g. Polycyclic aromatic hydrocarbons etc.) of the compost and biochar are reduced, and nutrients contained in them are released for plant uptake.

Please see the paper below. There are so many examples for this, if you search Google scholar.

Biofilm-mediated bioremediation of polycyclic aromatic hydrocarbons: current status and future perspectives - ScienceDirect

This is the importance of having microbial and organic/chemical integration in agriculture.

Regards,
Gamini Seneviratne, NIFS

Bodhi Dhanapala <bodhi_dhana@yahoo.com>
To:Vijaya Kumar,B.F.A. Basnayake,Chandre dharma-wardana
Cc:Nimal Chandrasena,Chandra Dissanayake,Buddhi Marambe,Madduma Bandara,Udith Jayasinghe
Fri., Dec. 10 at 11:10 p.m.
I have a few concerns that I wish to express.

1. The article supplied by Prof. Kulasooriya on:
Post Coivd-19 agriculture: The way forward
is interesting but I am saddened to say I find that (a) this is hardly a peer reviewed article (b) it expresses NO competing interests, where as it should have stated that Dr. Kulasooriya and Dr. Ganmini Seneviratne have a commercial interest in the product being discussed.
Very limited data sets are given.

2. It is important to publish results done INDEPENDENTLY of those who have a commercial interest. That is, the TRI scientists must test these products and publish the results in the TRI journal, and similarly, the DOA and HORDI scientists must publish independent trials in the HORDI journal. Or, the results should be in the Director General's annual report.

3. I think there was a strong (unjustified) attack on Dr. Vijaya Kumara or some one like that in Jayasumana's book "Wakugadu Satana" claiming that the person X did not reveal some connections to a commercialization attempt.
I think Jayasumana's attack was baseless, but in the case of the BFBF case there are patents and commercialization since 2010 even if Prof. AK and DR. GS may not profit from them directly, let us say., they and also NIFS should always declare their commercial interest in the product.

3. I find the CKDu article with the US collaboration between Connecticut and Kandy very disappointing.
Its choice of references shows the lack of critical ability, and there are so many other weaknesses in the article. But when a multi-author article is sent in with institutional weight, backed by an international grant, referees let it go.
But we as readers must distinguish rubbish, mediocre and good work. This paper is in my view mediocre or below average.
Even its choice of references is intriguing.
It gives a reference to
International Journal of Agriculture and Environmental Research (IJAER), a journal which is itemized in Beal's list of predatory journals.
https://predatoryjournals.com/journals/#I
That is, in many academic institutions, **if some one publishes in such a journal, de-merit points are given against them unless they can explain that, say, they were trapped into it because some foolish conference organizer had used a predatory journal!**
These journals provide no peer reviews, and will publish anything you send as long as you pay a small page charge. Some people use them to booste their CV!
I did not check all the references against Beal's list, but by the look of them, this may not be the only predatory reference.

The authors had not even taken the trouble to check the quality of their citations. The paper also has typos and such errors, showing slip-shod work.
Remember that this Water journal is a money making journal for Nature, and is not the highly reputed Nature issue, but spin offs from that, put out by Springer to make money.
The text is full of unproved presumptions.

Now let me come to Dr. Basnayake's write up.

4. There are many things in Dr. Basnayake's write up that I find difficult to come to terms with.
For instance, lets take one. Dr. Basnayake writes:
. **It stands to reason** that the applied heavy metals by means of fertilizer directly and indirectly through irrigation water have been uptaken by the plants in Girandurukotte than Hambanthota. We must interpret in terms of uptake rates rather than based on simple conclusions made in these reputed journal papers.

NO, It doesn't stand to reason.

If you look at some of the recent work on cadmium in soils, e.g., even the paper by Prof. Dharmawardana, or other papers from the Dutch and Belgian research, we can consider the amount of cadmium put into the soil from 50 kg of phosphate fertilizer of the sort used by Europeans (imported from Morocco, 30-40 mg of Cd per kg).
Please calculate the concentration resulting from the addition of 30 mg of Cd from 50 kg per hectare, into the volume of soil contained in one hectare of land ploughed with a blade of 20 cm thickness. In Europe the ambient cadmium concentration can be taken as the value used by Smolders (Belgian study), and see what the percentage increment is. Instead, you can do the calculation for Hambantotata or Girandukotte ambient cadmium values. You can use Bandara's pre 2014 values or what ever.

When the plant uptakes Cd, it uptakes what is bioavailable in the soil.

You will be surprised to find that you are wrong. Simple arithmetic cannot be ignored.