COMMENTS ON DATA AND DATA SOURCES FOR TABLE 1
in McDonald, Hobbs & Riha article on SRI, being published in *Field Crops Research*
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[Note: The numbers below refer to the data sets/points reported in Table 1 of the article. These constituted the data analyzed to reach the conclusions presented in the article. There were 40 data sets in all.]

1 & 2: Trials on farmers' fields in Madagascar, with 6 replications for calculating each result. Trials were done on 2.5x2.5 m plots laid out with Fischer block design, randomly assigning different combinations of treatments (SRI vs. non-SRI, except for water management which was varied within two major blocks to avoid seepage from saturated to unsaturated trial plots, and fertilization within sub-blocks, for the same reason, to avoid external effects of the treatments).

BMP results reported were from 20-day seedlings, 3 per hill, NPK fertilizer, continuous flooding. All trials, both SRI and BMP were with wide spacing. The spacings evaluated randomly on all trial plots were either 25x25 or 30x30 cm, both SRI spacings. There was only 80 kg/ha difference in yields between the two sets of plots at Anjomakely (N=120 each), so the results of the two spacings were combined, giving 6 replications for the other five factors being evaluated.

SRI results reported were averages from 6 plots with 8-day seedlings, 1 per hill, use of compost (no fertilizer), and recommended water management. Spacing was same as for BMP. The plots were respectively on better clay soil and on poorer loam soil. With such systematic comparison of SRI and BMP, SRI showed an average yield advantage of >3 times the BMP yield. These plus 3&4 were the data sets (4 out of 40) that most carefully tested the author's hypothesis, and yet they were excluded from the article's quantitative analysis on the ground that "maybe Madagascar conditions are different from elsewhere."

3 & 4. These trials at Morondava followed the same protocols as at Anjomakely, with all results reported being an average for **6 replications** of the combination of practices being evaluated. In this case, there were identical average yields for 25x25 and 30x30 cm spacing. These trials were NOT done on a conventional experiment station because the Centre de Baobab is a demonstration and testing center for **organic farming**. This means that the soil had been free from agrochemical and fertilizer applications for some number of years. Thus it can also be considered as, in effect, 'on farmers' fields.' The soils here are considered very poor, i.,e., course sandy. This site was chosen for the first set of trials because pest and disease problems are minimal in the dry season so there was least likelihood of such extraneous impacts on yield results. The SRI yield advantage on such soil was 150 to almost 200%.

5. These results were from replicated trials on the Beforona station in Madagascar that CIIFAD had taken over the management of, from the government agency FOFIFA, under a USAID project. The fields had not been used much in recent years, so probably there was little effect of agrochemicals, but this was not measured. These were proper SRI vs.

BMP trials, but the trials were all started about **1 month later** than the standard planting time for the area. So these trials were not regarded by the researchers themselves as an evaluation that could assess fully the potential of the alternative methods, particularly of SRI because these plants tillered profusely but there was not time for full grain filling and ripening.

6. This set of trials was a proper SRI vs. BMP head-to-head comparison, done at Comilla station by a BRRI researcher in 2000, Mian Hassan. However, these were just two small side-by-side comparison plots, not replicated. The 1 t/ha yield advantage, testing SRI methods against on-station recommended BMP, persuaded Hassan to propose that BRRI get more involved in SRI evaluation. BRRI trials at the main station in Ghazipur, however, did not show the same advantage for SRI methods. Farmer results in Bangladesh have usually showed a bigger yield differential than in the Comilla trials. The BRAC, SAFE, POSD and Syngenta studies, under an IRRI-funded evaluation with almost 2,000 on-farm comparision trials, showed an average SRI yield advantage of 1.45 t/ha.

7. These data from China, from the Anqing Research Institute of Agricultural Sciences and the Anqing Academy of Agricultural Sciences, were very systematic, with several varieties. The researchers concluded that there is, in fact, an SRI yield advantage even against the best practices available to them in China. This conclusion -- based on the kind of systematic comparison that the article's authors want to consider -- is ignored in the article. They just skimmed this piece of research for a data point -- one among many, most of the others being spurious – not taking account of the substance and findings of this solid work by Chinese scientists who deserve more respect.

8. These data from the M. S. Swaminathan Research Foundation, from trials at its Ecovillage center in Pondicherry, India, have been confirmed by other MSSRF experience with SRI. Dr. M. S. Swaminathan is satisfied that SRI offers important new opportunities to Indian farmers, and he is accordingly encouraging wider use of SRI. Surely his evaluation that SRI methods offer a yield advantage should get some respect.

9, 27, 36 & 40. These data come from a table provided by Dr. Karl Goeppert, at the time head of IRRI's program in Laos, who encouraged a number of institutions in Laos to undertake evaluation of SRI after attending the international SRI conference in China in 2002. Six sets of trials were undertaken; only the four sets of results that were **the least favorable** to SRI were included in the article's data base, however. The other two sets that showed SRI results of 3 to 6 t/ha but they were disallowed from Table 1 analysis because no 'control' data were reported for comparison.

In fact, there was no evidence given in the report from which the data were extracted that the data in the other four sets of data included in the analysis were **either** really BMP or SRI. Indeed, the email from Goeppert that transmitted the table stated (May 23, 2003):

"Attached is the summary of the SRI trials that were conducted by the various organisations during the rainy season June-November 2002. We all came to the conclusion that (with one exception in Huay Khot), we could not adhere to all the SRI principles in this first year of our trials

as there were either problems with the trial design, incorrect measurements, different treatment by farmers (e.g. one plot weeded, the other one not), or that the plot was flooded and we could not adhere to the "drying out" period recommended, or other reasons). We would regard this first year as an observational year and continue our evaluation."

Thus, the organizations that generated these data (which constitute 10% of the total sample on which the article is based) did not have any confidence that these numbers really represented SRI performance. More important, when the table from IRRI/Laos with these six sets of data was emailed to one of the article authors (Hobbs), he responded (May 23, 2005):

"Laos SRI trial -- No description of control plot when used -- several didn't have check plots. Several of the check plots did better than SRI and the one plot that did better for SRI did weeding but the check didn't. Hardly data we can use for comparing SRI and BMP."

Thus, he was disavowing these data himself. For the reasons he stated, none of the Laos data should have been included in the article's analysis, because there was no information provided on what, if any, were the BMPs used in the control, only that there were some controls. The low BMP yields suggest that they were hardly BMP (the average for the controls was 3.34 t/ha). There was no information that SRI practices were fully or properly utilized; what information was contained in the transmitting email said that they had not been done properly. *So this 10% of the data base should have been discarded.*

10. These data were reported by Sri Lankan university researchers in a paper given at the 4th International Crop Science Conference in September 2004. I have no reservations about them, although the location (Hingurakgoda) should be spelled correctly.

11, 17 & 24. These data, from on-station trials on small parcels of land in China, are subject to the caveat expressed for several years now, based on data: one should not expect to see SRI methods giving results as positive in on-station trials as on farmers' fields (more on this below). With these three trials, there was no evidence of any active soil aeration on the SRI plots; 'hand-weeding' in China does not usually mean weeding with a rotary hoe, which is what is recommended with SRI to get best results. Moreover, although N fertilizers can be used productively with SRI, the best results (demonstrated in factorial trials with replications) come with fully organic applications.

In these three on-station trials, there was **heavy inorganic fertilization** (P, K, Zn plus 180-240 kg N/ha). The SRI results in Jiangsu were, despite not following an SRI protocol, superior to BMP in Jiangsu; and they were the same as BMP in Guangdong. In Hunan, SRI results were inferior to BMP. Note, however, that the article acknowledges (only in passing) that in the Hunan trials, **the SRI crop partially lodged**. We have almost never seen SRI crops, grown with organic inputs, lodging, so this in itself makes the comparison trial results suspect. If an SRI protocol had been followed, with organic fertilization (e.g., no 200 kg inorganic N/ha) and active soil aeration (with rotating hoe), it is quite probable that SRI would have given higher yields in all three trials.

In any case, these are the results of three small trials (5x8 m, therefore, just 40 m^2). They provide no adequate empirical basis for the broad claim made by Sheehy et al. (2004): "SRI has no major role in improving rice production generally."

The most striking omission in the Sheehy et al. article was its failure to recognize that rice scientists at leading research institutions in China have been evaluating SRI for several years, since 2000. This includes the China National Rice Research Institute, the China National Hybrid Rice Research and Development Center, Nanjing Agricultural University, and the Crop Research Institute of the Sichuan Academy of Agricultural Sciences, among others. They have reached a conclusion contrary to that of Sheehy et al., who ignore the research findings by dozens of experienced and respected scientists.

Prof. Yuan Longping, World Food Prize laureate for his contributions as 'the father of hybrid rice,' stated in the proceedings of the 2002 Sanya conference, which he hosted: "SRI is a promising way to increase rice yield and to realize the yield potential of any variety whether high-yielding variety (HYV) or local variety, but HYV can be expected to give higher absolute yield with SRI methods." This conclusion, for the record, was based on his own evaluations and what he knew of other Chinese evaluations. It is indefensible from a scientific perspective to take no account of such a conclusion -- by one of the most respected rice scientists in the world -- and to dismiss implicitly the work of dozens of Chinese researchers who have many more years of experience evaluating rice than do the article's authors. These three data sets, on-station trials with trivial area, constitute *another 7.5% of the data set that is not defensible, even though published.*

12. These Indonesian data were done in a proper comparative mode. Note: there were also data on the SRI home page from West Timor, reported by the agricultural advisor of ADRA, where 7 farmers had tried SRI methods in 2002, and got an average of 11.7 t/ha compared to 4.4 t/ha on the same farms with their conventional methods, which were in fact BMP because the average yield in that area of West Timor was 2 t/ha.

13. These data come from SRI vs. BMP trials at Hunan Agricultural University in China. Not all varieties responded equally well to SRI methods, as is to be expected. Yaza 1 produced as yield of 12.79 t/ha, more than the Pei'ai variety for which BMP data were given, 12 t/ha. These researchers concluded in their paper that, after testing six varieties, SRI methods give a definite yield advantage of BMP. However, their extensive and careful research was reduced to a single data point in the article's data base, and their findings, which completely contradict the article's conclusion, were given no attention.

14, 15, 19, 20 & 22. These trials conducted by Chinese researchers were properly done and were well documented as to methods and results. More important from a scientific perspective, the researchers' measurement and reporting documented significant phenotypical differences in rice plants of same variety when they were grown with SRI vs. BMP methods.

16, 23, 29 & 30. These data from Bangladesh are all 'personal communication' data of the sort that critics of SRI have objected to when giving favorable results for SRI. Here,

where unfavorable results are reported, they get included in the data set. Their source, John Duxbury, has several times explained to me that when doing their on-station trials of SRI, they were not been able to maintain proper water control, so that the soil is not continuously saturated (hypoxic). This was seen as a reason why SRI would not be practical and acceptable in this part of Nepal, though other subsequent data, from farmers' fields elsewhere in the terai has shown that young seedlings and water control are possible, with good results.

These 'personal communication' data contribute nothing to answering the question whether SRI methods, when and where used as recommended, will meet or surpass BMP. One should not mix pragmatic considerations with scientific ones. These four data sets, none of them defensible according to the criteria stated by the authors, constitute *another 10% of the article's data set*.

18. These data were generated on-station at Bhairawa in Nepal where there was no proper water control, and thus the data contribute nothing to an assessment of SRI vs. BMP performance. *These data represent another 2.5% of the set*, so we are up to 30% that do not meet the authors' own criteria of being demonstrably proper tests of SRI vs. BMP.

21 & 26. These results are from one of six sets of SRI evaluations that were done 2002-2004 with support from IRRI's program in Bangladesh, using funds from the PETRRA project supported by DFID. The protocols for all six evaluations were drawn up by five organizations: the Bangladesh Rice Research Institute (BRRI), three NGOs (BRAC, SAFE and POSD) and Syngenta Bangladesh Ltd, a private company working with SRI. The few trials that are included in the article's analysis, managed by Dr. Latif, a BRRI researcher, *constitute <2% of the total number of trials undertaken in this large systematic study.* Moreover, the PI reported that many of the SRI practices followed in the trial were done incorrectly. So this was hardly a proper evaluation of SRI methods. *Drop another 5% from the data base.*

More significant, *the other 5 evaluations*, including one by another BRRI researcher, Dr. AbuBakr, came up with very positive assessments of SRI and its yield advantages. These were excluded from the article's data base, even though McDonald et al. cite the overall project report to IRRI which informed them about these other data sets. The authors apparently concluded that the other studies did not compare SRI with 'BMP.' However, the average 'control' yields for the other five studies were 5.52 t/ha, on a par with the BMP yields that Dr. Latif reported. Why the evaluation by Dr. Latif (who even before the evaluation was an outspoken skeptic about SRI) which includes *just <2% of the data base* from this IRRI-funded evaluation of SRI in Bangladesh should be included in the analysis while *the results from the other 98% of the evaluation are excluded* is hard to explain or justify.

25, 37 and 38: These trials done in 2001 at Chiangmai University in Thailand were done properly as far as I know. The SRI results were clearly inferior to the controls. But there is no evidence in the report from which the data were taken that the control practices constituted BMP. They are described only as 'conventional,' no more specification of

BMP than was used to exclude a number of other data sets that showed SRI results to be more favorable than the control. So thus this is another example of a "double standard" used when deciding which data would be screened in and which out. This 7.5% of the data base might be acceptable by the authors' criteria, but they have no evidence for this. Subsequent comparisons in Thailand have shown more advantage for SRI.

32. These data reported by John Duxbury from Nepal have the same limitation as the data he reported from Bangladesh. He has acknowledged that in the Khumaltar trials, there was not proper water control maintained for the SRI plots. These were on-station trials, so they they do not give any reasonable basis for drawing conclusions about how SRI methods will perform against BMP, or any other methods, *on farmers' fields*. This is another 2.5% of the data base that is not relevant to the question at hand.

33 & 39. These results are 'Exhibit A' for our conclusion that SRI methods perform more poorly on experiment stations than on farmers' fields. This is probably due to the effects of monocropping and heavy applications over many years of chemical fertilizers and agrochemical biocides, thereby affecting soil biota adversely. We have no systematic evidence on this, but it is a conjecture supported by plenty of published literature. SRI methods induce much larger root growth and with larger canopies there would be more exudation into the rhizosphere. Unless there is sufficient abundance and diversity of soil biota, we do not expect a one-time use of SRI methods to give the kind of phenotypical response seen normally on farmers' fields that have not been so heavily affected by use of chemicals or by unbalanced provision of nutrients.

IRRI's SRI trials at Los Baños have averaged 2.2 t/ha, whereas over 50 trials on farmers' fields (and even some experiment stations less 'intensive' than IRRI's), SRI yields have *averaged over 6 t/ha*, almost three times more. How is it that on the world's premier rice research station, SRI methods (admittedly on just a few small plots) have give only about one-third as much yield as the new methods do on-farm? This is an anomaly that should be attracting the curiosity and attention of scientists.

The highest yield that any management method obtained in these trials at Los Baños was 4 t/ha (Rickman 2004), a pretty paltry showing given all of the inputs provided. This should give scientists and others pause to wonder, what is going on? Unfortunately, IRRI cannot say anything about what is going on its soils because it no longer as any full-time soil microbiologist or soil microbial ecologist working on its research staff. Anyway, *here is another 5% of the data set that is dubious*.

34. These are the most puzzling results for SRI trials. The agricultural staff at Annapurna Farm were trying to use SRI methods properly when I visited them in December 2002. When I am asked, "Where don't SRI methods work?" I have pointed to this as one of the cases. The soils there have a pH of 8.0 and are dark 'cotton' clay soils. We have known all along – as some of the skeptics have argued – that for soil chemistry reasons, SRI management practices may be most beneficial on acid soils where Fe and Al concentrations impede crop production, and were a cycle of alternate wetting and drying as recommended with SRI can reduce these problems.

The authors nowhere acknowledge that proponents have not claimed that SRI methods will be beneficial everywhere. Because we are trying to capitalize on biological processes and recognize the great diversity of conditions where rice is grown, we have encourage farmers (and researchers) to try out the methods under their own conditions, to see if they can get the kind of benefit that other farmers have already started getting.

A footnote: in February 2005, Dr. A. Satyanarayana (until recently Director of Extension for the state of Andhra Pradesh, and main proponent of SRI in India based on his and hundreds of farmers' experience) told me that his cousin, who has some position in the Auroville ashram organization, got permission to do his own trials at Annapurna Farm with SRI methods. The cousin got a very good SRI yield response, so maybe the situation is not as negative as reported here. I cannot give the details on this as it was reported to me orally, not in writing, and thus qualifies only as 'hearsay.' But it means I may have lost one of the few cases that I could point to where SRI methods 'don't work.'

35. These results from Thailand are probably valid, and they may reflect the problem of parasitic nematodes in Thai soils discussed above. As stated there and many other times, the yield advantages reported for SRI are not claimed to be universal, since any biological innovation is subject to many different influences, positive and negative. We have been encouraging not the *adoption* of SRI but its *evaluation*. There is a difference between these which we are very conscious of, but critics gloss over it. Evaluation should be done fairly, of course. We suggest that it be done initially on a small scale, to see how suited the methods are to that particular set of soil, climatic and other conditions, and for the farmer to gain skill and confidence in the methods. We have nowhere proposed large-scale adoption or promotion of SRI, instead urging farmer trials and modifications.

Discussion

This data set does not even come close to representing 'the empirical record,' as it claims. The data reported from Laos, Bangladesh, Nepal and Philippines, constituting half of the total sample, are demonstrably unrepresentative of the empirical record with SRI from those countries. Moreover, the results of expert evaluations of SRI conducted in 2004 by major research institutions in India and China, the two leading rice countries in the world, *flatly contradict the conclusion of the article*. These recent studies, based on head-to-head evaluations of SRI vs. BMP, found SRI having *a 1.5 to 3 t/ha yield advantage*, across hundreds of on-farm comparisons, and with reduced water and purchased inputs. Each of those studies was larger and more carefully controlled than any of the older studies included in Table 1. Such results are thus more conclusive than any that can be drawn from the article's cobbled-together and flawed data set, where at least half should be eliminated by the author's own data set and another quarter are contestible.

It may be coincidental that in selecting data from the proceedings of the 2002 Sanya conference on SRI, the article accepted data *only from the three countries where SRI had not performed well*. Note: in all three countries SRI has subsequently performed more successfully. For unstated reasons, the author disqualified most of the results reported from all of the other 12 countries reporting positive results at the Sanya conference

(except China). As noted above, the conclusions of Chinese researchers based on systematic research concluding that there is indeed a yield advantage for SRI methods compared with BMP, were unreported by the authors.

If the selection of data to be analyzed from the Sanya proceedings had been done according to some explicit criteria consistently used, this improbable selection excluding *all* of the yield reports from countries where SRI was 'working' might be justified. But in fact, the screening in and out of which data sets would be considered was so inconsistent that it can most charitably be characterized as opportunistic. That the authors chose to compile two-thirds of their data set from on-station trials -- when we know from experience and have often reported that with SRI methods, on-station results are usually inferior to on-farm results – shows another bias that the authors intentionally or unwittingly introduced.

This critique of the data set may be attacked as 'subjective' and 'defensive.' But the statements of fact above are just that: statements of fact. No objective reviewer of the data used and the criteria employed and applied would conclude that Table 1 represents 'the empirical record.' Calling this 'a synopsis' does not relieve the authors of the responsibility to ensure that they are analyzing and reporting a representative sub-set of the available data. This is a very unrepresentative sample of 'the empirical record,' resulting in a conclusion that is contradicted by more extensive and proper evaluations done in China and India.

A much sounder article could have been written by analyzing the data from the agricultural universities and extension services of Andhra Pradesh and Tamil Nadu states of India and from the Sichuan Academy of Agricultural Science and China National Rice Research Institute in China. These comparison evaluations meet the criteria for inclusion specified by the authors in ways that the author's own data set does not. They show an SRI yield advantage of 1.56-3.31 t/ha against genuine 'BMP' when a comparison is made under controlled conditions on farmers' fields.