# Critical Comments on the 1998 AAEA Selected papers Session

John H. Sanders and Mohamed A. Ahmed
Professor and Research Associate, Department of Agricultural Economics
Purdue University

The American Agricultural Association Annual Meeting
Salt Lake City, Utah

August 2-5, 1998

AAEA Selected Paper Sessions

**Critical Comments on the 1998 AAEA Selected papers Session**

John H. Sanders and Mohamed A. Ahmed

The main reaction to these papers is that three of the four of them get so lost in the methodology that the basic message for policy makers and the justification for the topic is lost. Perhaps this is indicative of the type of thing that the association is looking for in the development field, i.e., a principal focus just on using some of the latest things in applied methodology without much concern if anything useful comes out of it.

The first paper by Houndedekon and De Groote on Health Costs and Externalities is an exception to this generalization. It follows Antle and Pingali, 1994 in estimating the determinants of disease especially the association with pesticides using a logit model. The fits are good. Pesticide use was often a factor. Surprisingly fertilizer use was also often significant in determining health problems. Other habits such as smoking and tea drinking created health problems (or were associated with other personal characteristics or habits that created health problems). Then a total health costs dependent variable was also estimated and significantly related to years of pesticide use, being overweight, and drinking tea. This included direct health costs plus the value of time lost (but no information was reported on how the value of time lost was calculated). More detail would be interesting on health conditions, morbidity, and productivity. The authors need to be commended for getting out into the field and tackling difficult measurement problems and presenting some interesting results This was useful quick reporting of surveying in a very difficult region for obtaining information.

The next two papers are very nice state of the art use of modeling and would be very useful if you can swallow their initial assumptions which are the same, i.e., that a weed (*Striga*) or weeds in general are the principal constraints to millet production in Mali and rice production in Cote d'Ivoire. Unfortunately, we can not accept this initial premise so that the results of the two papers are fine for demonstrating a methodology but seem trivial in practical importance.

Not surprisingly given the cast of characters in Mullen et. al., there is a very nice use of simple dynamic programming of various activities to control the parasitic weed, *Striga* There are several methods for controlling *Striga*, plant resistance and increasing soil fertility being two of the principal control methods. Rotations are also utilized but the seeds stay in the ground for a long time so this strategy just puts off the effects rather than controlling them.

The solution proposed is weeding with the new cultivar. This solution still does not attack the basic problem of not being able to grow plants without nutrients and the fact that *Striga* is more a symptom of low soil fertility than the principal problem on these low fertility soils. Focusing on *Striga* leads to impressive model development here but moves away from the essential problem that all cereals need to have sufficient plant nutrients to grow and without them there will be no production. At low soil nutrient levels *Striga* becomes a problem but even resolving it will not have very much effect on cereal yields unless additional soil nutrients are also provided. Nitrogen is the critical one and would be provided by the Urea especially if combined with improved water retention techniques but these additional benefits in plant nutrition were not part of the model.

Population dynamics of *Striga* is certainly interesting but arguing that this is the principal constraint to increasing millet yields does not make much sense. So the importance of the solution revolves around this assumption that *Striga* is the principal factor constraining millet yields and this is very hard to justify and is certainly not adequately justified by the sources in the first paragraph.  Otherwise, this is an interesting use of the model and controlling *Striga* is certainly a worthwhile objective. Nevertheless, the specified activities for further testing are not expected to have a large impact on millet yields and if soil fertility had been posed as the principal constraint or one of them and evaluation was also done for the benefits of soil fertility improvement, we would expect a much higher ranking for Urea (See Sanders, Shapiro and Ramaswamy, 1996).

Given this, the model dynamic programming formulation has some problems that are easier to deal with than the major conceptual problem discussed above.   One major problem here is the specification of the resource constraints for labor.  Although this was not clearly outlined in the paper, it appears that availability of labor for weeding is overestimated since in the model resource availability is constrained by "the resource constraints faced by a representative UP" and not by the amount of labor allocated to cereal production.  Clearly, the household cannot afford to allocate all its labor resources to millet production as this is only one activity.

The second problem, the yields of millet in the cowpea intercropping activity appear to be highly overestimated. Although millet is expected to benefit from intercropping with cowpea through nitrogen fixation and smothering but not to the extent described in this paper.  It is not convincing that intercropping will maintain the same yield level of the sole cropping.  Presumably, cowpea in intercropping will be allocated to half of the plot, this represents 100% increase in millet yield plus the yield of cowpea.  We suspect that the overestimated yield of this activity and availability of weeding labor are the principal factors determining the model results.  If these are corrected, the fertilizer activity is expected to be in the optimal solution on the basis of previous work on fertilization of cereals in the Sahel (see Sanders, et. al., 1996).

The Dalton et al. paper is harder to review because the first section of the paper makes the argument that land quality is becoming the principal constraint to increasing rice yields as population pressure reduces the fallow time and it is well known that without a long fallow period as ten years there is little soil fertility recovery. After making this case it is quickly ignored and the authors argue the conventional wisdom that labor scarcities and especially for weeding are the principal constraint to increasing rice production in Africa. They want a low management technology. You could argue for this on the basis of a long learning time or the lack of an effective extension service but to base the entire analysis on the supposed shortage of labor and abundance of land seems to just indicate a failure to interpret his own numbers in the first part of the paper and a failure to understand qualitative differences in land and the high public and private costs of continually extending the frontier even if this were possible. Both this paper and the previous one need to more systematically analyze methods to increase soil fertility and especially inorganic fertilizers.

As we have pointed out elsewhere nowhere in the world have cereal yields been substantially increased without increasing levels of inorganic fertilizers and we would not expect this to happen for the first time in Sub-Saharan Africa (Sanders, Shapiro and Ramaswamy, 1996). Basically a cultivar alone strategy does not do much except maintain poverty. To increase yields and incomes of farmers inputs will be necessary and especially with rice, which responds well to nitrogen and is generally grown where there is sufficient water.

With declining marginal productivity, production can be increased by increasing labor use up to a limit. As pointed to, elasticity of output with respect to labor inputs is already negative indicating super-optimal use of labor input. This does not seem to support the notion that labor is scarce but rather the low productivity of land. Note that the point elasticity of output with respect to area is less than unity implying production cannot be increased proportionally by expanding land area. Output per unit area will be lower. But due to population pressure, land is becoming increasing scarce as evident from the reduced fallow and more intensive cultivation. Therefore, upland rice productivity can only be increased by improving productivity of land by fertilizer application and, thus, shifting the production function. Looking for easy solutions with the existing technology without increasing use of purchased inputs is of little help.

Aside from this fundamental problem the modeling is fine and clever utilization of transcendental log production function following Dvorak (1992). Unfortunately, the results were not interpreted to identify the real problem of low productivity, i.e., low productivity from poor soil fertility. It is rather gratifying that our interpretation of the problem setting in Mullen et. al. and the results in Dalton et. al. confirm our belief that agricultural productivity in semiarid, Sub-Saharan Africa cannot be improved without increased utilization of purchased inputs, especially inorganic fertilizer, and water retention technologies.

The forth paper by Ekboir, Jarvis and Rey on asset depreciation also provides a nice application in the analysis of farm investment. The topic is also interesting because most of farm investment analysis is often applied to farm machinery while this paper extends this type of analysis to pasture. The issue is also relevant to obsolescence of agricultural technology, the replacement requirement and capital stock investment.

In an analysis of depreciation, a distinction needs to be made between physical and economic depreciation. According to Hulten and Wykoff (1981), economic depreciation refers to the asset’s loss in monetary value with age at a point in time. Physical depreciation refers to loss in productive capacity of a physical asset (mortality) due either to loss of in-use efficiency or to retirement. Only in the case of geometric depreciation are the two congruent by duality relations. While the paper made no distinction between the two types, measurement of the depreciation rate, from the panel data, in terms of the area of pasture that has lost its productive capacity implies physical depreciation of pasture. With the limited data available, the authors cannot determine the proportion of pasture that has lost its productive capacity due to natural retirement of the perennials due to age, biotic or climatic factors, (exogenous component), and that due to economic changes, (endogenous component). We find the distinction here very impressive and clever treatment of depreciation of pasture and similar capital assets.

As the authors pointed out, most analysis of asset depreciation has attempted to determine whether depreciation occurs at a constant (geometric) rate or at a variable rate. While the implications of the hypothesis for further empirical work are significant, we feel that the question in this paper is irrelevant for two reasons. First, endogenous determination of factors determining the depreciation rate becomes irrelevant if the depreciation is geometric. Secondly and most important, the data used in this study is not appropriate to test the geometric depreciation hypothesis.

Testing whether the depreciation rate is geometric or variable requires an age-stock (or price) profile of the asset (see Hulten and Wykoff (1981) and Nelson and Caputo (1997)). In contrast, the data used here appears to be pooled for a large number of pasture units at different “ages” in any given year. The linear time trend (1..10) and quadratic terms included in the estimation equation may capture the effect of technological change but by no means can they measure the age of the retired pasture. Hence, the parameter estimates cannot be interpreted in terms of changing depreciation rates over time. To allow this, the panel data needs to be further disaggregated by pasture age. It would be interesting if the authors analyzed the effect of technological change on depreciation of pasture and attempted to draw the policy and research implications of their results given any technological change in both types of pastures that has taken place.

Two additional observations are related to interpretation of the data and the results. First, in calculating the depreciation rates from the panel data, internal inconsistencies and outliers were encountered. However, no attempt was made to explain why such a problem would occur. One possibility is the reversibility of the decision to declare a pasture as deteriorating. Since the endogenous depreciation depends on current input and output prices, an extensive pasture may not be profitable for a set of input of output prices (in the current period) but may become profitable again under a favorable set of prices. Note that reversed pasture status does not mean it is a new investment. This explains internal inconsistencies such as the negative depreciation rates and outliers for some of the observations in the data set.

The second observation is related to the interpretation of the difference in the rates of depreciation of extensive and intensive pasture types. The rates for extensive pastures vary across *departamentos* from 0.13 to 0.28, and those for intensive pastures from 0.23 to 0.38. Contrary to the authors' conclusions, it appears, intuitively, that extensive pastures have a longer average productive life since their range of depreciation rates is lower. The relevant issue here is what happened to the depreciation rate over the ten year period considered here and how is that related to the desired level of depreciation from the society point of view? What are the policy or research implications?

**References:**

Antle, John M., and Prabhu L. Pingali, 1994. “Pesticides, Productivity, and Farmer Health: A Philippine Case Study.” *American Journal of Agricultural Economics* 76:418-430.

Dvorak, K., 1992. “Resource Management by West African Farmers and the Economics of Shifting Cultivation.” *American Journal of Agricultural Economics* 73:809:815.

Hulten, C. R. and F. C. Wykoff, (1981). “The Estimation of Economic Depreciation Using Vintage Asset Price.” *Journal of Econometrics* 15:367-396.

Nelson, R. A. and M. R. Caputo, (1997). “Price Change, Maintenance and the Rate of Depreciation,” *Review of Economics and Statistics*, Vol X: 422-430.

Sanders, John H., Barry I. Shapiro, and Sunder Ramaswamy, 1996. *The Economics of Agricultural Technology in Semiarid Sub-Saharan Africa*. Baltimore, JD: Johns Hopkins University Press.