Letter to the Editor

Dear Editor:


In reference to the above mentioned article, I would like to make some comments. I congratulate the authors for evaluating a large number of published records on intercrop systems to develop their new technique of evaluation of these systems.

However, I have serious doubts about the biological validity of their method. An intercrop system is normally practiced in semi-arid tropics where the crop growth period is not quite long enough for growing two crops in sequence. Taking the authors' example of corn and bean systems, corn produces one crop in 135 days whether as a pure crop or as an intercrop. Similarly, a bean crop, which needs a 90-day growth cycle, can also produce only one crop in a 135-day cycle, either as a pure crop or as an intercrop. The remaining 45 days of the crop growth period available if bean is grown as a sole crop have no biological value, unless one could use a bean variety that can complete two growth cycles within that time frame.

In conclusion, I would like to tell the authors that intercrop systems are commonly practiced where two successive crops cannot be grown in a crop season; therefore, introducing the time factor into the calculation doesn't make any biological sense even if it makes mathematical sense.

I am not a supporter of LER, but with acknowledged limitations, it is still a better indicator of land use efficiency under intercrop systems than is ATER.

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Reply to Chandra K. Reddy

We appreciate Dr. Reddy's comments on our paper and welcome the opportunity to respond to his "doubts about the biological validity" of our method.

First, however, we would like to register our disagreement about where in the world intercropping systems are "normal" or "common." We have seen farmer-managed intercrops throughout Central America, the Amazon basin, and parts of tropical Africa. Each region has its semi-arid part, but each also has areas with sufficient rainfall for year-round plant growth. The "crop growth period" in these humid tropical areas could perhaps best be described as infinite. While we are inclined to agree that intercrops have the greatest potential for increasing agricultural production in finite-season environments (e.g., the semi-arid tropics), our experience suggests that they are neither abnormal nor uncommon in other places. Francis (1986) concludes that "multiple cropping has evolved to fit a nearly infinite number of geographic and climatic niches." Intercropping is not synonymous with multicropping—an intercrop is but one form of a multicrop—but we believe Francis' statement applies equally well to intercrops.

We cannot tell whether Dr. Reddy disagrees with the logic of our argument(s) about ATER or whether his principal concern is its utility for farm-level decision making. If it is our logic he doubts, we reiterate relevant arguments, made in the paper, on which our logic is based: (i) Any cropping system requires an investment of land and time. (ii) Yield per unit of time (day, year, season), rather than yield per harvested crop, is the ultimate criterion for comparing cropping systems. (iii) LER makes no provision for the biologically inescapable fact that "cropping system A" may—and frequently does—require a larger investment of time than "system B." Finally (iv) a computational methodology that will quantify system A relative to system B on the basis of two possibly differing investments (land area and time) is needed.

We submit that ATER fulfills this need—not only mathematically but also biologically and, in large measure, agriculturally—and to date, Dr. Reddy's reservations notwithstanding, we have found no reason to doubt its validity.

If, on the other hand, Dr. Reddy doubts the "agronomic universality" of ATER for farm-level decision making, he gets no argument from us. It was not proposed as "the ultimate comparator"—not even for cropping-systems research, and especially not for produc-