

ICTA in Guatemala 1974-1979:

Establishing the Rural  
Socioeconomic Discipline and  
Creating the Basis for Farming  
Systems Research-Extension

Methodology

While a Field Staff Member of  
The Rockefeller Foundation

From the memoirs of  
Peter E. Hildebrand

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# ICTA in Guatemala 1974-1979:

## Establishing the Rural Socioeconomic Discipline and Creating the Basis for Farming Systems Research-Extension Methodology

Early in the presidential term (1970-1974) of General Carlos Manuel Arana action was taken to reorganize the agricultural sector of Guatemala. Coronel Miguel Angel Ponciano was minister of agriculture and Ing. Agr. Mario A. Martínez was vice minister. There were two principle goals: increase food production, mainly of the basic grains and stimulate rural development. The small and medium sized farmers were the primary target group. The first emphasis was placed on INDECA (marketing), BANDESA (agricultural credit) and DIGESA, which became the extension sector.

The first concrete move on the part of the Guatemalan government to reorganize agricultural research was a visit to CIMMYT in Mexico and the Ford and Rockefeller foundations in New York in October, 1970 by Ing. Mario Martínez, at that time vice minister of agriculture; Ing. Astolfo Fumagalli, the director of research; and Robert Culbertson, Head of USAID/Guatemala. A meeting was held in the office of the Ford Foundation with Dr. Lowell Hardin of the Ford Foundation and Drs. Sterling Wortman, president, and John Pino, director for agriculture, of The Rockefeller Foundation. The Rockefeller Foundation indicated interest in the project of a new research institute for Guatemala.

Following a series of workgroups beginning in January, 1971 in Guatemala and through March, 1972, The Rockefeller Foundation agreed to support the new institute, the *Instituto de Ciencia y Tecnología Agrícolas* (ICTA). On October 24, 1972 the Congress of the Republic

passed the law creating ICTA. ICTA was formally inaugurated on May 10, 1973 and officially opened its doors on June 1 of that year. Prior to the inauguration of ICTA, all research personnel of the old research organization were let go and only select persons from that organization were hired into the new institute. Ing. Agr. Astolfo Fumagalli became the first *Gerente* and Ing. Mario Martínez was the Minister of Agriculture at that time. Later when Ing. Martínez stepped down as Minister, he took over as Gerente of ICTA and Ing. Fumagalli became Sub-Gerente. The institute had technical and political skill at its head. Where there were no Guatemalans with appropriate training and skills to fill certain positions, a global search with the support of USAID and the Ford and Rockefeller Foundations was organized to fill the missing positions. All together when all positions were filled, there were about 10 expatriates working with the Guatemalans, all but one in line positions.<sup>1</sup>

In March, 1974, while I was still working in El Salvador, Guatemala began taking an interest in our *multicultivo* work. In early March I had a call from Kirby Davidson, Deputy Director, Social Sciences, in The Rockefeller Foundation, asking if I might be interested in being considered for a Field Staff position with the Foundation, posted to ICTA (the new Guatemalan Institute of Agricultural Science and Technology). I had just arrived at the office at CENTA in Santa Ana, having come from a disheartening meeting with the Director of USAID in San Salvador where we were discussing the potential new North Carolina State University contract funds that would have supported the work I was doing as acting director of agricultural economics in CENTA. It was obvious that the mission did not want us to get that contract and have funds that we could use independently of their control, even if they generally agreed with and were enthusiastic about what we were able to accomplish by having some outside funds. I told Kirby that he had called at an opportune time and that, yes, I would like to be considered for the position. My dilemma in El Salvador was that if we went ahead with the NCSU contract in order to have the funds to continue working at the level we were working, I would lose the support of the local (El Salvador) USAID mission. Without these additional funds, it was obvious that there would not

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<sup>1</sup> Excerpted from Robert K. Waugh's "Four Years of History."  
<http://ufdc.ufl.edu/UFO0066725/00001/2j>

be enough funding to carry on the program except in a much weakened state.

In April, Miguel Ramírez (an *Agrónomo* in the Agricultural Economics Department at CENTA) and I were invited to a cropping systems conference in Guatemala hosted by ICTA and IICA (the InterAmerican Institute of Agricultural Sciences from Costa Rica) where we gave two papers. The Director General of CENTA, Ricardo Cabezas, accompanied us. This was the time I first met Astolfo Fumagalli, then *Gerente General* (General Manager) of ICTA, and many of the other people who worked with and for ICTA in Guatemala. Astolfo says this was when he decided he wanted to hire me to head *Socioeconomía Rural* in ICTA. I did not know it at the time, but that was apparently one of the reasons they had invited us to come to Guatemala for the conference. I was invited by ICTA and CIAT (the International Tropical Agricultural Center in Cali, Colombia) to make another visit to Guatemala in June to look at some *multicultivo* plots they had established there.

One of the big differences in being with The Rockefeller Foundation rather than on an AID contract like I had been in Colombia (with the University of Nebraska) from 1968 to 1972 and in El Salvador (with the University of Florida) from 1972 to 1974 was that I was no longer looking at a temporary two-year tour of duty *possibly* to be renewed. The Foundation considered their Field Staff to be relatively permanent in a country. I asked them when we were discussing about my going to Guatemala for how long I might be there. They said they did not know. *As long as I felt I was being productive and as long as ICTA wanted me was the time frame.* A rough estimate might be from three to five years, but time was not the determining factor. This difference might not seem that critical, but in fact, it is exceedingly important. Even if

AID contract persons are in a country more than just two years, their horizon is always at the most two years away.<sup>2</sup> If they are new to the post, a relatively high percentage of that time is spent in getting settled, either looking for the next position or negotiating for an extension, and in packing up if they leave after two years. In my case, I knew that if I were successful in Guatemala I would eventually work myself out of a job, but I had an indefinite horizon in which to do that. It makes a tremendous difference in one's outlook. I could do things *well* instead of in a hurry.

ICTA was organized into three administrative divisions called *Direcciones*: Programming, Administrative, and Technical. The *Dirección Técnica* was the main operating unit. It was organized into "Programs" and "Disciplines." The Programs included: Corn, Beans, Rice, Sorghum, Wheat, and later Vegetables and Livestock. These had the usual and quite traditional plant breeders or animal scientists as Coordinators (instead of Department Heads, each program and discipline was headed by a Coordinator). Disciplines included Socioeconomía Rural (SER), Soils, and Publications (*Divulgación*).

When I arrived in the fall of 1974, ICTA, which had been created just over a year before, had already hired an anthropologist and an economist and there was a Spanish sociologist as well, all at the bachelor's degree level. So we had the beginnings of a Socioeconomía Rural (SER) discipline. *ICTA was convinced that it was necessary to have us but they had little idea what they wanted us to do or how we were supposed to do it.* During my first days in the institute, I asked various persons, especially the Gerente and Sub-Gerente what they thought we should be doing. One of the early responses was, "We want you to *sell* our technology to the small and medium farmers." They felt that one of our jobs was to learn about these farmers to help sell them the technology the institute was going to develop. I agreed but on the condition that our studies and what we learned about these farmers would help

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<sup>2</sup> Even though I was on an AID contract in Colombia for four years, the first two were in Bogotá where I headed the University of Nebraska agricultural economics team posted with ICA in Tibaitatá (Bogotá), Medellín and Palmira. The last two years I was posted to Palmira creating a regional agricultural economics extension program to train regional agricultural economists and initiate the rural program in that region.

*serve as the basis to develop the kind of technology that the farmers wanted and could use.* By this time, and based heavily on what we had accomplished in El Salvador with the *Multicultivo* project, I was confident that if we developed improved technology really appropriate to their conditions, then it would sell itself.

The other thing I often heard was that the institute was supposed to help make the country self sufficient in the production of basic grains, primarily maize, beans, rice and sorghum of which the first two were the most important. Facetiously, I argued that this would not be difficult. We could import a couple of big farmers from Texas with all their machinery, give them enough land, get rid of the small farmers on it, let the Texans import everything they needed, and they would easily produce lots of grain. Their response, of course, was that no, we had to do this with the small and medium farmers already on the land. This response then solidified my position that we had to produce *scale-specific technology*. That is, technology aimed specifically at the small-scale farmers so they could use it. This left open the argument of whether technology was scale neutral or scale specific. It was thought by some that so called “improved seed” was scale neutral because it could perform equally on large or small fields. I argued that it was not the size of the field, but what was done on that field and the quality of the soil to begin with (in other words, the environment in which the seed was planted) that was important. Small farmers most often had poorer soil to start with and they did not have the resources to modify that kind of an environment (with irrigation, fertilizers, pesticides) like the large farmers did. So the environment in which small farmers produced was more often than not, inferior to that of large farmers. For this reason, *most technology was scale specific, not scale neutral*. This was an argument I was going to have to repeat for many years.

One of the first things I had to do was set up a program and budget for 1975. It was for this reason that I had to find out as quickly as possible what it was the institute expected from us. It was difficult, but it also provided me with a very rapid introduction to the institute and especially to the other Coordinators. I was pleased with the interest of the other Coordinators in what Socioeconomía was planning. They were also interested in

cooperating with us. It was obvious that the use of the term “Coordinator” instead of Department Head was very meaningful and productive for ICTA. The Coordinators and the Technical Director had a meeting in the main offices in the capital every Monday morning before everyone went to the field, in different parts of the country, for the week. This was also a very important and productive activity in ICTA. As an example of this type of cooperation, in the first month we were in operation, the Sorghum program asked us for help in determining the response of farmers to three new varieties of sorghum that had been distributed in small bags of seed the previous year. With their help, we designed a questionnaire and pretested it. The survey was conducted in our second month. We found that the farmers were not very interested in the new varieties and were asked to find out why. At first it was difficult and we could not get answers that made much sense. Even our SER personnel (by then we had some *perito agrónomos* working for SER in the different regions) who were in the area and acquainted with many of the farmers were frustrated. They knew it had something to do with the way the women felt about sorghum, but couldn’t figure it out. Finally I sent my bilingual secretary, Inez Tujab, who had three years of sociology at San Carlos University to see what she could find out (all the other ICTA personnel in the area were men). It took only a short while for her to find out that an important use of sorghum of the small farmers in the area was for tortillas. This was a way to stretch maize, particularly in years when maize yields were down. Their local white sorghum, tall growing and photoperiod sensitive so it matured in November, made very good tortillas. Two of the new sorghums were not for human consumption but were designed for animal feed which was not important to these farmers. The third was a white sorghum like their local sorghum, and made very good tortillas as well. The problem was that it had purple glumes and could be detected in the tortillas. The women did not like to have to admit that they were using sorghum in tortillas so they did not like it. Also, because it had little tannin, the birds liked it very much, and because it matured much earlier than the other sorghum, birds were a major pest.

Joe Black, the Director of Social Sciences of The Rockefeller Foundation in New York and my superior in the

Foundation, visited us during the first month. He traveled with me to El Salvador to attend the closing of a week-long short course on *multicultivos* and we also had an opportunity to visit the basic research on the system at the school and one of the regional trials where we were accompanied by Tito French (the American Peace Corps volunteer who had worked with me and helped develop the *multicultivos*) and Mario Barahona who were in the Ag Econ Department at CENTA.

The Foundation had a regional representative stationed in Guatemala, Lewis (Jocko) Roberts, who was one of the old RF agriculture hands, and we also had Woody Harwood who had constructed experiment stations for the foundation in many parts of the world. His job was designing and constructing the ICTA stations. Bob Waugh and Eugenio Martinez were also paid with RF funds, but through CIAT (the international agricultural research center in Cali, Colombia). Bob Waugh was also one of the old RF hands who had been in Colombia and with ICA and later CIAT. Thus, RF had a sizeable representation in Guatemala and all except Jocko Roberts were working in ICTA. Bob was attached to the Gerencia as an advisor, Eugenio was the first Technical Director, and I was the Coordinator of Socioeconomía Rural. Other foreigners were also filling positions for which there were no qualified Guatemalans. Besides Americans, we had a Chilean, a Japanese, a Salvadoran and a Mexican (Eugenio). We were paid from different sources, but all considered that *we worked for ICTA*.

## 1975

In January, SER was augmented with a new appointment, Ing. Bruno Busto Brol who had been Director of Natural Resources in the Ministry of Agriculture. All ICTA project proposals for 1975 were due by the end of January and SER prepared five and submitted them for approval:

1. A detailed agrosocioeconomic study of an important stratum of small and medium farmers in the eastern part of the country (Jutiapa).
2. Farm trials in cropping systems in eastern Guatemala.
3. Three sub projects with pigs:

- Improvement in the productivity of non-confined pigs.
  - Technology transfer for pigs in confinement (with CIAT).
  - Production of feed for pigs in cropping systems.
4. An agro-socioeconomic study of one area in the altiplano (San Juan Comalapa).
  5. Economic farm trials on cropping systems and fertilizer use in La Máquina (on the southern coast).

These proposals indicate the nature of the SER program, which covered a wide range of activities and regions of the country. Also, in preparation for the project approval process I presented a seminar (in a series that ICTA created) on what at that time we thought was the role of SER in the institute.<sup>3</sup>

At this time, the budgeting process proceeded ahead of project approval. The programming unit of ICTA that had never been very friendly towards SER, had taken out of our budget the pickup we requested. This left us with only two pickups for 10 people working in three widely separated areas of the country.

Still early in 1975, well before the planting season (usually around mid May in the eastern part of the country) we in SER, along with the Bean Program (whose Coordinator was an American-Wayne Porter), decided not to utilize tractors in any of our trials on the new experiment station in Jutiapa. We planned to use bullocks and only the other hand tools used by the small farmers in the area. We also wanted a piece of the station on which they would not apply a blanket level of fertilizer (which the Regional Director who was also the Station Director) wanted to do. We suggested further that we be given the most sloping land for our trials because most small farmers were on sloping land. The Regional Director, at that time an American on a USAID contract through Texas A&M University, insisted that no bullocks were going to be used on “his” station and that because of low fertility he was going to apply phosphorus over all of it. “Otherwise, there would be no response to anything we do on the station.”

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3. Hildebrand, P.E. 1975 (Jan). El papel de socioeconomía en el Instituto de Ciencia y Tecnología Agrícolas. ICTA. Guatemala. <http://ufdc.ufl.edu/UFO0081587/00001>

We came to an impasse and asked the Technical Director (Eugenio Martinez) to resolve it. On the last day of March, he authorized SER and the Bean Program to rent land off the station on which to conduct our trials. By the third of April we had found 15 manzanas (a manzana in Guatemala is 0.7 hectares or about 1 3/4 acres) of mostly steep, rocky hillside to rent. About one manzana could be plowed with bullocks (it was not accessible by tractor) but the rest could only be worked by hand. We cleared one manzana of this rough land as well. We had pretty well satisfied ourselves that this was the most common situation for the small farmers of the area and that the station was on the type of land accessible only to the larger farmers. So in balance, *this was a very strategic move in the direction of research specifically oriented to the conditions of small and medium farmers.* The land was in the Aldea of La Barranca, Municipio of Santa Catarina Mita, Departamento of Jutiapa. It was all in brush at the time and we hired the son-in-law (Ricardo Guerra) of the owner and his older sons to help us clear it and to work on the trials. They also farmed several manzanas of their own right next to our area.



The brush we had to clear

We went right to work clearing the land and preparing it for planting while we began planning the trials. We were conducting a survey in the area (we had decided to limit the survey to farms from one to five manzanas and on hilly land) and were finding out

how farmers did things. By the end of April, we had interviewed 60 farmers. The most common system, followed by 2/3 of the farmers, was planting beans, corn and sorghum together. Average reported yields for this system in 1974 were: corn 8.3 qq/mz (about



Ricardo Guerra and his sons planting maize, beans and sorghum on their own land with their *macanas*. Our land (the part we did not clear) is right behind them.

5400 kg/ha), beans 6.4 qq/mz (4200 kg/ha) and sorghum 9.7 qq/mz (6300 kg/ha). We also had initiated farm record keeping with 25 farmers in the area. Ricardo was invaluable in providing information on farming in the area as well. We had decided to test a multiple cropping design using the same crops and cultivars (maize, beans and sorghum) the farmers used, but modifying the planting arrangement based on the kind of design that had been successful in El Salvador.

Shortly before the rains started we had everything set up and ready to plant and were staying in Jutiapa. Every morning we would go out to see if it had rained so we could start planting. One afternoon we were sitting on the patio of Ricardo and Doña Blanca's house when it started to rain (the first rain since September 27, 1974). After a while, Ricardo said, "Tomorrow we plant." He had decided that it had rained enough for germination.



Maize, beans and sorghum intercropped in the traditional pattern.



The same crops in which we arranged the maize and sorghum in twin rows leaving more room for the beans.

Early the next morning we were there and by the time we had climbed up to the field (it was nearly one kilometer up a steep hill) we could hear the *macanas* hitting rocks as all the neighbors were also planting. Ricardo had found us a few older boys from several miles away to help us because he and his boys had to plant their land before they would help us. No one from around close would commit to helping us because they all figured that once it rained on our land it would also rain on theirs and planting as soon as it rained was very important. *This was an introduction to labor scarcity on small farms at critical times of the year.*

Most of SER was involved in the work in the Oriente and the trials in La Barranca. I was convinced that to be able to appreciate the problems and points of view of the other professionals in ICTA and to hope to be able to evaluate their programs, it was imperative that the personnel of SER have first-hand experience not only with the farmers and farming, but also with the experimental process (which I had learned while working in Palmira in Colombia and in El Salvador). In addition to the time spent on measuring and staking plots, directing seeding, etc., we all spent a good deal of time visiting with farmers in the area, listening to opinions (about us and our work, about the weather, about methods of seeding, etc.) We also tried to fill in some of the holes in our survey and answer some questions that had arisen during tabulation and analysis of the data. We also spent a lot of time, particularly in the evenings, talking with and listening to the other technicians of ICTA who also lived or stayed in Jutiapa, trying to determine how they viewed our work, their work and that of the Institute, what problems they were having, etc. Many did not agree with what we (social scientists) were doing (planting trials), but at least, most of them were talking and arguing about it, so they were aware of the existence of *non-traditional approaches to helping the small farmer.*

At the same time we were doing this work in La Barranca, we were also designing fertilizer response trials in several regions of the country. We were looking for *minimum* amounts to recommend rather than the amount to maximize per hectare yields such as was traditional and had been done in the past. Maximizing yields (or profits) assumes unlimited fertilizer as well as unlimited capital, neither of which was a condition relevant to the small

farmers with whom we were working. *Even though previous fertilizer trials had indicated no response to phosphorus, DIGESA and BANDESA recommended it in all cases.* So we were designing trials to look into phosphorus because we felt we could save the farmers who used fertilizer a great deal of money if they did not purchase and apply it. This, of course, was in direct contradiction to the expressed opinion of the Regional Director.

In March, 1975, we sorted out responsibilities within ICTA regarding evaluation of ICTA and its programs. Armando Fletes, also an economist and Director of *Programación*, and his group were going to analyze progress of each program toward its stated goals (number of farmers interviewed, number of trials initiated, etc.) as well as budget expenditures and obtain information for the various reports required by the government. SER had responsibility for the technical evaluation of the programs. This included analysis of the orientation of each program, its contribution to the overall objectives of ICTA, and the efficiency of the projects in making progress toward raising incomes and production of the small and medium farmers. It was agreed that SER would work with each of the programs in helping to write their project proposals in such a way that the orientation toward the goals and objectives of ICTA was clear and the relationship between the project and the goals was evident. However, unfortunately, this never became operational.

In June, with the controversies over 1) whether SER should be doing field trials to begin with, and 2) whether we should be working on the steep, rocky hillsides at all, we were assigned the additional work of justifying ICTA's program particularly in the Oriente. To do this we requested and were authorized to undertake air photography of the region. Jaime Wyld of the Soils Program of ICTA was loaned to SER to coordinate this activity and Roberto Bosarreyes, my driver, also worked closely with it. We were convinced that most of the farmers of the Oriente worked on the steep hillsides and that the most common system was the one we were working with, but we had to prove it and air photography was agreed upon as the means. Carlos de Leon Prera was assigned to SER to do the ground truthing (we sent him to a 6-month course in Spain as part of this and he later became Director General of

DIGESA and Vice-Minister of Agriculture). Even though the project proved that we had been right, there was still concern in ICTA of working under the severe conditions of these farmers. **It was argued that *it was too hard to make big gains in yield under these small farmer conditions* (even though this was what we were supposed to be doing!).**

As the year progressed and people began to visit our work in La Barranca, many not in SER were becoming more convinced that it was *unwise to work under the conditions of the small farmers on the hillsides!* We were hard hit by drought, as were the farmers. It was a hot, one-kilometer walk up a steep hillside to get to our plots. It became popular to say that this trial looked like something social scientists were running! Of course, except for the size of the plots and the many stakes scattered about, it looked just like the fields of our neighbors except where we were trying something different. As a comparison, the Regional Director, who also ran the regional Technology Testing Program, *had his people plant their trials either on irrigated land or on bottomland with sub irrigation. He told them that any trial with a Coefficient of Variation (CV) over 20% would be thrown out. One way, of course, to keep the CVs low is to keep the average yields high!* He certainly had high yields on his trials, but they were several times the yields the farmers and we were able to get under the conditions of the farmers we were trying to help. He was able to show that *under good conditions and using technology not available to the small farmers*, it was possible to get about 20 qq/mz (about 20 bushels per acre) of the black beans Guatemalans eat. Our yields and those of the farmers were more on the order of 4 to 5 qq/mz. He thought we were wasting our time and ICTA's resources by getting yields that low (and at the same time were bringing shame on him and ICTA) and we thought he was wasting time and resources running trials under conditions that only a few elite farmers in the area could duplicate.

As a result of the trials that year in La Barranca, I was more convinced than ever that the only way to do something for farmers under those very poor conditions was to ***learn how to do it better in those same conditions.*** To do that, of course, we also had to learn how to farm the way they did in the first place! And the Regional Director complained bitterly that he couldn't drive his

pickup to our trials -- not even his motorcycle! He was the same person who first took me to the region. As we were driving over the hills and I was observing all the fields up on the rocky hillsides, he said, “We’re almost there. See that valley down there? That’s where the farmers are.” He (a Texan) did not even recognize that the people up on the hillsides were farming! Obviously, we were going to have a long, hard battle.



The trail up to our trials in Jutiapa. That’s me in front.

In October, (one year after I had arrived) The Rockefeller Foundation created a team to review ICTA. It was comprised of: Dale Harpstead, Michigan State University; Ken McDermott, USAID; Ralph Cummings, Jr., RF; Ed Wellhausen, CIMMYT; Fernando Fernández, CIAT; and John Pino, RF. Jocko Roberts coordinated the group. They hiked up to our trial in La Barranca and we provided them a typical lunch of tortillas and beans. Although our crops were not “pretty” because we were not using

fertilizer nor insecticides (because the farmers didn't), and also because of the drought, we felt the team was able to comprehend our approach to working on small farm problems.



Eating our lunch up in the area where we worked. In the center with the black blouse is Inez Tujab, my bilingual secretary who also came to the area from time to time. The other women are wives or friends of our workers.

Other organizations were beginning to take notice of what we were doing in Guatemala. CATIE asked that I attend a session in Turrialba, Costa Rica to help them with their “benchmark” survey questionnaire. Stillman Bradfield, Kalamazoo College; Ken McDermott, USAID; Don Fiester, ROCAP; and Howard Ray, Basic Village Education also attended, among others. After a great deal of discussion, they were convinced that a census-type benchmark survey (very much in the mode at the time) was not the most appropriate for their needs, and instead, they were going to follow a program very similar to what we were doing in ICTA. Mario Martínez, the Gerente of ICTA, requested that CATIE visit ICTA later in the year to get better acquainted with our Institute prior to opening discussion concerning ICTA’s participation in their project.

As a follow up, Jorge Soria and Rufo Bazan of CATIE visited ICTA in November to get better acquainted with ICTA and our methodology prior to making any proposals concerning our participation in the CATIE/ROCAP project. In those days, the

regional (CATIE) and international (CIAT and CIMMYT) centers treaded very heavily on the national institutions (ICTA) and tended to tell the national institutes what they wanted from them. By that time, ICTA was getting strong enough that we felt we could tell the regional and international centers what we *needed* from them, not the other way around. CATIE was very concerned about ICTA because ROCAP (a USAID regional project for Central America) would not let CATIE start their project unless and until all Central American countries had agreed to participate (the donors were also very heavy handed).

The itinerary for the CATIE visit included a two-hour briefing with Mario Martínez, Astolfo Fumagalli, Eugenio Martínez and Bob Waugh on the organization and philosophy of the Institute. That evening in Quezaltenango they received another very good briefing from Werner Schmook, the Regional Director there on his regional functions and the orientation toward the small farmers and their systems of production. The second day included a tour of the Labor Ovalle Production Center (ICTA called their experiment stations Production Centers) and some on-farm field trials in the region. That night at a reception back in the capital, Fumagalli gave a very clear explanation of how ICTA viewed any possible relationship with CATIE. The third day involved a trip to the Oriente and La Barranca where SER stressed the methodology we employed to learn about the farmers and work on cropping systems in and for their conditions. CATIE staff had accumulated a great deal of high quality data on agro-climatic multiple cropping interaction on the CATIE station, but they had never had any real involvement with farmers. That was a basic weakness of their program, which was supposed to help the countries develop better cropping system for small farmers. I felt *CATIE had not yet, at that time, grasped the importance of the small farmers' socio-economic and cultural influences on their cropping capabilities and needs.* This was a major problem in collaboration.

USAID in Bogotá asked for me for one week to evaluate two projects with small farmers they were initiating with ICA and the National Planning unit. Bogotá agreed to let me take Carlos Reiche (economist) and Sergio Ruano (anthropologist) along on the trip. Ken McDermott apparently had told the mission about our work in Guatemala. They wanted us to write up the ICTA

methodology in “AID-ese” as the basis for their project proposal. Besides leaving AID the work they requested, our participation provided us with two by-products. First, we were able to formulate a more theoretic base for our methodology, and second, we ended up with a draft explaining what we do. The latter had not been done before and we anticipated that it would be useful in ICTA. Carlos and Sergio managed to visit the Cáqueza project where Hubert Zandstra (later Director General of CIP) was working. It was probably the best-known project in Colombia, but the small farmers were not yet accepting the technology and AID and ICA were interested in testing other methodology. We also visited Tibaitatá, the ICA headquarters, where I presented a two-hour seminar to about 110 people on the ICTA methodology and the cropping systems we were working with in La Barranca. Mario Martínez asked me to report on the trip to the Board of Directors (*Junta Directiva*) of ICTA after we returned.

The University of Florida Multiple Cropping team advising CENTA in El Salvador asked to meet with us and see our work in La Barranca. They indicated that the Minister was talking about having 3,000 farm trials the next year, which was gratifying. However, the team had little orientation toward the needs of the small farmers and thought mostly in terms of agro-climatic conditions. I was concerned that this would reduce their effectiveness as they went into new conditions. They did seem to grasp the nature of what we were doing and why. It was interesting for me to note that in the new five-person Florida team, the soils advisor was the team administrator and did no technical advising in soils. That meant that 20% of their team effort was consumed with administration. Also, they were then serving strictly as advisors and not completely incorporated in CENTA, as I had been when I was there. I felt that the ICTA system of using foreign personnel was much better. We were incorporated directly in line positions in ICTA and had no identifiable chief of party.

## **1976**

But all was not rosy within the ICTA program. In March 1976, I wrote the following memorandum (in Spanish) to the Technical Director of ICTA, Eugenio Martínez:

In Region VI [Jutiapa] a dichotomy in the work is growing that is making the process of developing and generating technology difficult. This dichotomy has to do with the definition of the target group of farmers. The Technology Testing Team [those who conduct the trials for the commodity (maize, sorghum, etc.) programs] considers that their clients are the *extension personnel of DIGESA who work with the medium size and larger farmers who generally have more resources and better soils*. Many of them plow with tractors or bullocks, have spray equipment, and are accustomed to using fertilizers. Their yields are low, but they could be increased substantially. The Sorghum Program that is emphasizing work in the Mita Valley also is oriented to this type of farmer.

On the other hand, the Bean Program and SER are orienting their work toward the small and medium farmers with few resources and poor land *who are the majority of farmers in the region*. These farmers in general have not benefitted from modern technology because of their economic conditions and poor soils: they plant with *macanas*, without plowing, use their own seed and do not use fertilizer or insecticides. Nevertheless, *because of their numbers, these are the farmers who produce the majority of the basic grains in the region*, even though individually, their yields per manzana are extremely low.

The dichotomy is a problem because the large differences between the classes of farmers make it impossible that technology generated for one class works for the other. The small farmers who generally farm steep, rocky land almost exclusively use crop associations while the medium and larger farmers on better land and using animal traction or tractors are accustomed to sole cropping.

To orient their work to the most favored farmers, the Technology Testing Team chooses level land with good moisture, designs their trials so that there are no limiting factors except those being tested, and insists on high yields or else the trial is considered "lost." Very few of these trials are applicable to the small farmers of the region for the reasons mentioned. That is, they favor only those farmers who have credit from BANDESA and other economic advantages.

For the same reasons, the Technology Testing Team does not consider the technology generated (by SER and the Bean Program) for the small farmers to be acceptable for testing in their trials. For example, when the Bean Program requested that the Technology

Testing Team test two of their varieties over a wide range of conditions, the Regional Coordinator rejected their request saying he wanted *a complete package of technology* for each variety and they would be tested only under favorable conditions.

Another example was the elimination of the study proposed by the Bean Program of planting distances that would have been very useful to the small farmers who plant in association, but not to larger farmers (and the Technology Testing Team) who plant in sole cropping. A third example was the rejection of a suggestion by SER that the Technology Testing Team initiate studies of the sorghum variety from El Salvador, Centa S-1, because it was excellent in tortillas and because the small farmers were interested in it. The reason they gave was that S-1 was not as good a *yielder* as Guatecau (developed by the ICTA Sorghum Program) in sole cropping, which for the small farmers was not relevant.

Although few results of the Technology Testing Team will be of direct use to small farmers in the region with the existing infrastructure, they still are not completely irrelevant. It may be useful in the medium and long term if the necessary infrastructure is developed in the region. But in the short term, these farmers will only adopt a very much simpler technology (not a “complete package”). And this simpler technology will not be useful to the larger farmers whose technology is presently “more advanced” than that needed by *the majority of farmers* in the region.

In other regions where ICTA is working it has not been so necessary to carefully define for which client the technology is being generated. The most classic example is in La Máquina where there is a large degree of homogeneity among the farmers (because it is an agrarian reform project and all farms are the same size). In the Altiplano, everyone is in agreement that the small farmers are the clients because they *obviously* are the large majority.

But in the Oriente there is neither homogeneity nor agreement and the high degree of heterogeneity between large and small farmers makes it imperative that it be decided *who is the client*.

The February 4, 1976 *terremoto* (earthquake) that hit Guatemala (magnitude 7.6 on the Richter scale) had a profound effect on the country as well as on ICTA. It happened a few minutes after 3:00 in the morning and was an experience terrifying beyond words. Of course we had all felt earthquakes before, but nothing like this one. We heard it coming. It sounded in the middle of the night like a freight train plowing up the track as it

came toward us. In thinking it over afterwards, I think the bed moved up and down about eight inches and back and forth about a foot. There was nothing to do but hold on for the 20 or 30 seconds it lasted. After it stopped, it was pitch black and dead silent. We were stunned for a few moments. Then we heard our Salvadoran maid calling us. She had a flashlight so we were able to see well enough to get outdoors. By then, of course, the greatest danger was past, but we had no way to tell what kind of damage had been done to the house. Our neighbor, Jaime, came out of his house also, and we all sat on the curb of our joint driveway listening to the radio Jaime had brought out. Every few minutes during the hours before daylight we felt new tremors, but none anywhere near the big one.

Just after daylight I felt the need to go to the bathroom and decided to brave going into the house. I don't remember if we had lights at that time, but it was light enough to find my way. Just after finishing, the phone rang! I answered and it was Tito French calling from College Station, Texas. He had just heard on the news about the earthquake and decided to call to see if we were all right. I told him we were and that the house appeared to be OK, but it was a mess inside and asked him to call my Mother to let her know we were OK. He did and that was the last phone call that got through for many days. We felt very good knowing our families in the States knew we were OK.

We spent as little time as possible in the house for the next few days. It was really a mess. Things were broken all over. Bookcases had fallen down spilling their contents all over the floor. We had some very nice obsidian arrowheads and spear points and some pieces of Mayan pottery that were powdered from hitting the tile floor. We had a glass decanter on the mantel over the fireplace. It was about 16 inches tall with a stopper in it. The decanter was still standing on the mantel, but the stopper was on the floor several feet away! One of the carvings from Ecuador, carved from a tree trunk about six inches in diameter, was broken in two. Miraculously, the house suffered no damage. Picture windows were not even broken nor was the floor cracked. The reason we spent little time in the house was because every few minutes there would be another tremor (after shock).

We did take time to shower and dress that first morning figuring that we might well be without water for many days. Jaime had driven around a few streets enough to know that major damage had been caused and by then we were getting reports on the radio that helped a little bit to assess what had happened. However, the radio from Tegucigalpa, Honduras said they thought the epicenter must have been there because it was so strong that far away! We were supposed to be going with the Taiwanese delegation to see some work we were collaborating on in La Máquina. So at 8:00 AM, the time we were supposed to meet them at the ICTA offices on the Plazuela España, I went down to see what damage had been done to the ICTA offices, but with absolutely no intention of going to work. There, sitting in front of a shattered building that had been our offices, were two black diplomatic cars full of Chinese. No one else was around so I told them the trip was canceled and they just as well go home. I couldn't tell how badly the building had been damaged, but most of the windows were out and it looked to be in very bad shape.

I had had a team doing a survey in Tecpán, which as nearly as we could tell was right in the middle of the worst damage. We had no communication for a few days, but I later found out that for some reason they had decided to come back to the capital that night, so they were all with their families when it happened. Tecpán was virtually leveled that night and about one in ten persons died. We were fortunate they had decided to come back.

It took a few days before we were able to get communication going even within ICTA. The phones were mostly out as was electricity and water. We were fortunate in being among the first to get them back in our neighborhood. The ICTA offices were unusable. The administration moved to the livestock fairgrounds but there was no room for the technical unit. SER moved into our house on February 9. About that time we were finally given permission to go into the old offices to get materials and equipment that we needed in order to keep functioning. The building was really a wreck. Steel doors were bent from when the two floors came together. There were cracks in the floor, some as wide as nine inches across, so you could look down on the floor below. The window by my desk had been shattered and glass was all over my desk and everything else. What a nightmare it would

have been if the quake had happened while we were in there working.

For days afterward, an interesting phenomenon happened. People were all of a sudden very courteous driving. Almost no one used their horns and there was no rush to keep people from entering a street. It seemed to me that no one wanted to hear loud noises. Virtually no one shouted either. Perhaps part of the problem was that every aftershock was preceded by a rumble so we knew when it was coming. It was too fast, however, to be able to do anything about it. Sometimes when a jet would take off it would sound like a tremor coming and everyone would tense up waiting to see how strong it was going to be. Even 20 years later, some noises still did this to me. The first month after the initial shock, we experienced 1,000 aftershocks, some over 6.0 in magnitude. In the year afterwards there were 2,000! I hope I have had my quota of earthquakes.



Part of what was Tecpan following the earthquake.

It was two or three weeks before the roads were cleared enough for us to get back up to the Tecpán area. The sight in that town was almost indescribable. Virtually nothing was higher than about one meter. Everything was covered with about six inches of dust. It was said that the reason so many people lost their lives in

that town and others like it was because as the adobe was falling down and on top of people, the dust prevented them from finding their way out if they were capable, or of finding others who were not. I believe it. At the first place we stopped to sort of take it all in, a young girl, probably about 12 years old, asked us if we were looking for babies and children. Apparently people were working on getting the orphaned some care (although even at that time there may have been people really kidnapping children). We said, no, that we were looking for some land to rent. She gave us the name of another young girl whose family had all been killed figuring that she would not be able to care for the land for a while anyway. We did rent some land and spent quite a bit of time in that area that year. On every trip I carried food, water and a blanket so in case there were more landslides and I could not get home, I at least would be OK. Often times when we would be sitting on the ground eating lunch or resting, we would feel the ground move.



The roads often were closed from aftershocks.

I have a number of slides of the area following the earthquake. Some of the most graphic are two of Patzicia (near Tecpán). I had been there two months earlier with Chris and Mary Andrew in December when Chris was down on an official business. They wanted to walk around the market in the central

plaza. I took the opportunity to sit up on my pickup until people quit paying any attention to me and then began taking pictures. In two or three, I have the church in the background with an extremely colorful and busy market scene in the foreground. After the quake I returned to the same place and took pictures of what was left of the church. Everything was covered in dust and the church was a ruin. It was foggy as well, and on one picture, two women, one older and one young, dressed in black, are walking past the ruin. It is a very gripping comparison of before and after.

The response of the United States and other countries after the quake was incredible. Early in the morning following what I believe was the first night following the terremoto, we heard the first C5A cargo plane arrive from the US. It brought an entire field hospital that was set up outside Chimaltenango. The Guatemalan government requested the US to fly U2 spy planes over the country to assess the damage, because there were still many areas no one could get to except by helicopter and all available were busy evacuating people. Even private pilots were helping, landing on straight stretches of road when they could. The US Corps of Engineers sent a battalion to rebuild the entire road from the capital to the Atlantic coast. Most of it was out as were most of the bridges. I remember the Guatemalan President interviewing the Colonel commanding the battalion shortly after they arrived and asking how long the Colonel thought it would take to do the job. Based on the U2 photos and their analysis in the US, the Colonel said they planned to leave at the end of May. The President smiled and said that would be great, but he didn't expect miracles and if it took a lot longer, he would understand. Believe it or not, the battalion finished the road and left on May 31 as planned!

I was moved to write a short note on the relief effort as an appendix to my usual monthly report. Attached to my report for February 1976, was the following:

Much has appeared in the media about the inefficiencies, wastefulness, stockpiling and local laziness in the massive relief effort. First of all, it is obvious that the effort was rapid and massive -- it was overwhelming and everyone is grateful for it. What few, if any, who had not gone through the quake were aware of is that virtually everybody who had been here was in a state of shock, even if they appeared near normal. I am convinced that 99% of the rational decisions made in the first few days following

the first quake were made by persons from outside the affected areas.



Market in Patzicia six weeks before the earthquake



The same market square in Patzicia two weeks after the earthquake.

We have heard about locals idly watching as volunteers from outside unloaded supplies from helicopters. Imagine the situation. A huge machine descends on a scene of devastation and some people jump out. Other volunteers who have arrived from the outside and know what is happening join in to help unload the supplies. On the sidelines, the dazed local people gather to stare at the strange goings on. How could they help? They are not even sure what has happened or what is happening. The volunteers, unaffected by the tragedy, do know and they know what to do. (Of course, some of them are about as confused as the local people because of the general lack of organization in such a rapid, massive effort.)

Inefficiencies arise at such a time because no one knows for sure who needs what or in what priority. It is only known at first that a terrible tragedy has taken place over a large part of the country and all sorts of supplies are needed. As they arrive, these supplies, whatever they are, are sent by private plane, helicopter and truck to somewhere in the quake damaged zone. In fact, I think a tremendous effort was made to get priorities sorted out very early in the game and get organization into sorting the various supplies.

Stockpiling is an interesting phenomenon. So many things arrived from everywhere so rapidly that it was impossible to handle it all. There simply was not enough local manpower. At such a time, clothes that are sorted and sized must take priority over clothes thrown at random in a box -- there is no one available to sort them in the short run at the scene of the tragedy. To prevent complete loss, the loose clothing is put in storage to be sorted when possible.

But in all these occurrences, the media can find a story to sell. Personally, I feel the situation was handled extremely well here in the country, given the state of mind imposed by the magnitude of what happened in those 23 seconds.

When I wrote the above, I was obviously upset by what the media were saying about the relief effort. I continued my report with comments on the situation in the Altiplano:

Our continuing work in the Altiplano has provided some insights into the situation there. The heavy losses of life occurred in the larger towns because of the confusion caused as people fled their collapsing homes into narrow streets where walls were falling

on both sides. After the initial panic ended, there was too much confusion to be able to determine who had and who had not gotten out of the debris. Many, who were otherwise not badly hurt, smothered under the rubble.

In contrast, in the smaller towns and settlements, people were better able to determine who had not escaped and to get to them before it was too late. Loss of life was relatively light in the more isolated areas. The poorest, who live in thatched huts, fared better than those who lived in adobe or block houses.

Grain for consumption and seed (from the same crop but stored in a different manner) has been uncovered and restored. Cooking implements and dishes are very scarce, but food, now, is no great problem. The first preoccupation, after food, has been to construct temporary shelter, and most people now have something (some, quite elegant tents obviously brought in, and others only the most rudimentary shelters).

The short run preoccupation with food and shelter has delayed land preparation for crops that is normally taking place at this time of year. One now observes women undertaking heavy work normally reserved for men. Nevertheless, much land will not be adequately prepared before planting deadlines arrive. Hence, much land will be planted without adequate preparation. Some land will be left idle because entire families were lost although even where most members of a family were killed, the remaining members express the desire to plant their land. Perhaps a large supply of low-priced fertilizer could help alleviate what could otherwise be a very short corn harvest this year in the Altiplano.



Woman working in a field following the earthquake.

As if by some common accord, on Sunday, 29 February, people flocked to the streets to partake of the sunshine and whatever diversion was available. People were out right up to (and some even after) dark -- a phenomenon that had not been seen since the earthquake. The next day, March 1, there was a very notable increase in work productivity. Although the tremors continued, most people had either decided they could live with them or that at least, another big one was not very likely.

We in SER were able to rent about three manzanas of land outside Tecpán, find a few people willing to work with us on it, and get a trial planted based on the information we had collected prior to the earthquake. We also shared this area with the Bean Program. We were able to obtain a current (February 13) air photo of the rented land taken by the U2 spy plane as part of the US program to assess earthquake damages. It was an exceptionally clear and detailed photo.

One morning I noticed a small group of the workers gathered around in a circle and went over to see what was happening. They said there was a snake there and I asked if it was poisonous and they said yes. Then I asked if it had bitten any of them and they said yes! Don Pedro had been bitten on the foot. All of them were barefoot so it wasn't too surprising. His foot, of course, was very dirty and there were two very obvious fang marks on it. I asked him if it would be OK for me to cut into the fang marks and he said sure. So I took out my pocketknife and made some fairly deep cuts through the fang marks. We put his foot in a plastic bag that we had been carrying sandwiches in, loaded him up in the pickup, and took him to the makeshift health center on the highway near Tecpán. They used alcohol to wash off his foot, which according to his flinching hurt worse than either the snake bite or my knife, but they had no antivenin for the bony tailed cantil (a relative of the rattle snake) that bit him. After checking with the hospital in Chimaltenango who did have the antivenin, they loaded him up in an ambulance and took him away. Although he was really fine, we didn't see him again for several days.



On-farm trial with wheat planted between double corn/bean rows.



On-farm trial with potatoes planted between double corn/bean rows.  
Ralph Cummings, Jr. of The Rockefeller Foundation is in the center.

Besides the trials on the rented land in Tecpán, we designed some trials in the altiplano with the Corn Program utilizing the concept of double corn rows (from La Barranca and El Salvador) and interplanting with potatoes, cabbage and wheat. In another trial we looked at the distance between twin corn rows when interplanted with wheat.

We had our first casualty in April 1976, when Roberto Loranca, an SER Perito Agrónomo in Nueva Concepción on the Pacific Coast, was killed in a motorcycle accident. We would not have had time to find and hire a replacement in time to initiate the project we had planned there so instead, we hired another Perito Agrónomo, Teodoro Lopez Yos, a Kakchiquel, to work on the farm records in the Altiplano freeing Rolando Duarte to analyze the data taken in Tecpán before the earthquake.

Institutional inertia was being felt in the Oriente. The management of ICTA made the decision that the Bean Program would no longer work with SER in La Barranca. They indicated that *plant breeders are supposed to work either on the station or under highly controlled conditions on farms*. Of course, La Barranca did not classify as “highly controlled conditions.” This left SER with 3.5 manzanas rented in La Barranca.

It *appeared* that there was no opposition to SER working with small farmers on *ladera*. Eugenio Martínez, the Technical Director, said he was interested in the work. But somehow, it ended up that only SER considered those farmers as clients. The majority of the effort in the Oriente continued to be with the farmers in better conditions and who were clients of DIGESA and BANDESA. The Technical Director also asked us to begin keeping records with that type of farmer. However, we had no one free to do it so he had to provide us another position for the job.

In part, I think this problem came about because we were relatively new in the region and without ongoing projects when we arrived. When it was decided that we should work with the small farmers, we were able to do it -- 100%. All other programs had ongoing research on the station or in the flatlands and only the Bean Program made an effort to begin switching. The other programs stayed where they were and that left us as the ones who were “out of line.”

In May and June we were very busy *in several parts of the country* putting in trials on our own rented land and on farms, and in getting farmers started with record keeping. In La Barranca, it rained four inches the night of May 19 so on the 20th planting began. We put in five basic trials at that location: 1) cropping systems, 2) distance of planting beans, 3) distance of planting corn, 4) bean varieties, and 5) commercial plantings on each of three areas. The Soils Program also established a fertilizer trial on beans at our La Barranca site. Five farmers in La Barranca and six in other parts of Jutiapa planted on-farm trials of the most promising systems from 1975. We had 16 farmers keeping records on ladera crops and 15 in flatlands in Asunción Mita. Leonzo Godínez, a Perito Agrónomo, had been hired to do this.

In the Altiplano, we had 23 farmers keeping records and in Tecpán we had several trials including: 1) a large trial on systems of corn, beans, *haba* (fava) and wheat, 2) one with wheat and cabbage, 3) 15 varieties of soybeans (for Eugenio Martínez, the *director técnico!*), 4) 3 varieties of wheat, 5) a trial with *choreque*, a legume commonly grown with corn, and 6) a circular trial to study row orientation and distance between double corn rows. These were on the land we rented following the earthquake. On the station in Chimaltenango, also in the Altiplano, we had two large trials in collaboration with the Corn Program. One was on corn associated with bush beans, wheat, lettuce and potatoes. The other was on distances between pairs or corn rows in association with wheat.

In La Máquina, on the south coast, we had 53 farmers keeping records to give us a total of about 125 farm records being kept in several parts of the country.

In July and August we had two very interesting field days for farmers in the areas where we were working: La Barranca and Tecpán. The primary purposes were to study the reactions of the farmers to the cropping systems and help us determine the extent of the area of influence of these systems (what later we would be calling Recommendation Domains) and in this way test our basic means of locating clients -- based on their traditional cropping system. In La Barranca we invited farmers from Yupiltepeque whose conditions are similar but less rocky than those in Barranca. Also invited were people from La Barranca. Forty-seven farmers

(9 from Yupiltepeque) and 32 other people attended the initial talks including a slide presentation and 42 farmers visited the plots and had lunch. Joe Black, the director of Social Sciences at The Rockefeller Foundation was among those attending both sessions.



Some of the trials in Tecpán. The circular trial is in the center

The slides were presented in a bahareque (bamboo structure with mud in between the slats) structure close enough to a source of electricity that we could string about 100 feet of extension cord to get power. After the presentation, I showed some slides of the earthquake damage in the Altiplano. They couldn't believe what they were seeing.

In Tecpán, there were 32 farmers from 3 aldeas of San José Poaquil, 14 from Pueblo Viejo (Tecpán) and 23 other people. Three farmers who had come from the aldea Hacienda María, were obviously outside the "homogeneous" group. Joe Black and Susan Almy of The Rockefeller Foundation visited Tecpán, but were not at the field day.

On the coast (La Máquina) we initiated a series of studies that at that time we called "acceptance" of technology on the farms where we had farm trials the previous year. Later, we realized that this was really a test of the "acceptability" of the technology. It



Wives of our workers cooking lunch at the field day in Tecpán

was not a test of how many farmers “adopted” the technology (the usual criterion), but rather was a *test of a quality of the technology*. It took us a while to figure it out, but eventually we came up with an “index of acceptability.” This was calculated the year following the trial and after the farmers on whose land we had conducted the trial had an opportunity to use it on their own. The index was the percent of those farmers who used it multiplied by the percent of the area of that crop on which they used it, and divided by 100. With a little experience, we calculated that an index of 25 (providing that at least 50% of the farmers were trying it) indicated a high level of acceptability and, therefore, ultimate adoption.

At the end of July I left on a trip to Spain and Italy, the first of many international trips that were to come about because of the work we were doing in ICTA in Guatemala. I visited Carlos de Leon Prera in Sevilla attending a six month course on remote sensing so he would be equipped to do the photo interpretation for our work in Jutiapa. Then I went to a conference at the Rockefeller Foundation Conference Center in Bellagio, Italy, on Lake Como at the foot of the Alps. I wrote a journal on that trip and have taken the following from it:

Madrid, 31 July, 5:15 PM. The Pan Am flights were on time all the way, but with the unexpected (to me) stop in San Juan, P.R., we arrived in Madrid at 11:00 AM and I found that the Iberia flight to Sevilla had been canceled. They put me on one at 10:25 PM that night. By the time I had this all straightened out I had been going 24 hours with 3 hours sleep and 12 hours flying including 7 ½ from San Juan to Madrid.

After arranging the new flight, I caught a taxi to the city, and naturally got taken on the price of the cab. He gave me good information, but charged me ten prices. I checked out a city tour, but it didn't leave until 3:30 PM so I just walked the streets. I had noticed a nice looking sidewalk cafe on the way in from the airport and walked back there for a beer. I was getting sleepier all the time, and after my beer walked back to the airport bus terminal and returned to the airport hoping for some lunch and, maybe, sleep.

The terminal was discouraging, so with a seven-hour wait, I invested more money and returned to the city in a bus, and returned to the original site of the beer. When I had been there before, I had smelled good fish and ordered sole and white wine. It seemed to me to be the best I had ever had. It also included a lettuce and tomato salad with fresh and delicious olive oil and vinegar.

I thought Madrid was really a pretty city. It amused me that because of my Spanish, people had absolutely no idea of where I was from, but guessed "gringo" because of my "looks." Madrid was pretty much what I had expected a European city to be with monuments, plazas, some wide streets and some narrow. Mostly traditional architecture, but some modern. The sad thing is that the city is surrounded by satellite high rises, in groups, that from the air look like outcroppings of toad stools! The city had 4 million people and it looked to me like 4 million cars. With the exception of IBERIA, that reminded me of AVIANCA, the people all seemed very friendly and easy going. To me it was very interesting to see the similarities and differences from people of Latin America. In general I thought they seemed very much like the upper class Centro Americanos such as Enrique Alvarez and Mario Martinez (both of whom were Ministers of Agriculture -- Enrique in El Salvador and Mario in Guatemala). I had a feeling that one should know Spain, after being in Central America, to really know Central America.

I observed that in Spain the miniskirt was dead. Pants and Levis or jeans were in. Also there were long skirts with high slits. Klogs and related shoes were in. So were Renaults, Fiats and Seats (the latter from Spain).

I returned to the airport, which I rated at the bottom of world airports that I had been in, ranking just above Bangkok when I was there more than 12 years before. They were, however, constructing new facilities. I had been sitting in front of the British Airways “slot” (it didn’t merit any other name, I observed) and thought you could tell the British by the “angle of the nasal.” At about that time I finally discovered why I was having such a miserable time and why everything was so fouled up. I had arrived in Spain on July 31, the day most of Europe goes on vacation. The airport was jammed. I finally found a bar. That is, I saw where it was, but I couldn’t get in. Finally about 9:00 PM I made it into the bar and found a place to sit down. Just after I sat down I watched an Aeroflot (Russian) plane land. It was a jet that looked like a British VC-10. I remembered that there had been a Cuban plane there when we landed. The Russian flight was from Moscow. A Marriott food truck pulled up to the Russian plane to service it. That was in the middle of the Cold War! At the same time, there were SwissAir and South African Airways planes on the ground. It could have been an interesting airport on other occasions.

We finally started boarding the plane for Sevilla at 11:30 PM then sat on it for another half hour! We at last arrived in Sevilla at 12:45 AM on August 1 (I had left Guatemala on July 30) and still hadn’t had any sleep. Carlos de Leon was still waiting for me at the airport with a friend, the friend’s wife and a little girl. They had been there since 8PM! The airport was an hour from town. Carlos had arranged for 18 fellow students to meet at the hotel at 10PM thinking we could all go out on the town. They had all waited nearly four hours and finally left about 1:30 AM, just 20 minutes before we got there. I never did get to see them because they all left later that morning to return home. The course closed just in time for August vacations, something I had not known ahead of time.

I convinced Carlos I needed sleep more than to see the night life of Sevilla and finally got to bed about 2:30 AM. I slept until about 9:30, which for me was very late. Carlos came over for what passed as a breakfast (Continental, of course). The hotel, the Doña Maria, was right by the Cathedral that Carlos said is third in the world for size, riches and importance. It really was worth seeing. We spent Sunday, August 1, walking all over the place and every little bit stopping for a beer or wine and fish. I discovered that squid, fried in fresh olive oil, are exquisite. Most of what we ate was cooked in olive oil. The wine was great and the beer, including San Miguel (the brand from the Philippines we had in Pakistan), good, too. The beer I liked best was Aguila.

Because in August it is just getting dark at 10:00 PM and it is still very hot until about 8:00, people just start to show up on the streets at 8:00. Then things begin. After swimming in the hotel pool until about 9:30 PM, we went out to “top off” with some more squid, fish and beer, and then went to a flamenco show. The place Carlos really wanted to take me was closed for vacation!

The next morning, Carlos checked on our flights back to Madrid (he was also going to Madrid and from there was going on a tour). He had arranged for us to have lunch with the Guatemalan Consul, Dr. José Antonio Cañadas Villalta, an M.D., investor and businessman from Andalucía, the area around Sevilla. He and Carlos became very good friends because 1) Carlos was his first “client,” and 2) the Guatemalan earthquake. Sevilla donated huge quantities because of their joint efforts -- including radio broadcasts. Carlos also made many other friends as a result of that activity. His “novia” was another Guatemalan, Roxana Simón, of the Salvadoran Simón family. Her family lived in La Cañada in Guatemala City.

Dr. Cañadas was going to visit Guatemala in December, probably with his family. He was interested in fomenting trade between Guatemala and Spain and had just established a Guatemalan Chamber of Commerce in Sevilla. He was thinking of things from Guatemala that can be canned or put in jars. Spain would trade goods for goods. He mentioned small tractors, but I discouraged him on that. We stopped at his house and I saw his collection of knives and swords. I told him he should get a “*cuma*” when he is in Guatemala. That evening, Carlos and I went to dinner with the U.S. Vice Consul, Norman Alexander from Louisiana. He had also been a good friend to Carlos.

The next day things seemed to be working again relatively well so far as travel was concerned. Carlos and I were to be picked up by an airport bus at a certain point in the city he knew. When we arrived, a small cafe was open and we stopped in for coffee (it was very early in the morning and still dark). Most people had a shot of gin with their coffee, but we didn't! We had clear flying to Madrid and I did from there to Milano. I had an excellent view of the Alps between Madrid and Milano. I was met at the airport by a driver and a limo that took me to Bellagio. The drive up to Bellagio was wild -- pure Italian, I thought -- but it was beautiful once we got out of the city. As I had observed on landing, the city is pushing out into the cornfields surrounding it. Lake Como was clear and beautiful and only slightly polluted even though it was surrounded by many, many small (and some large) villages. There was snow still visible up in the Alps north of the lake.

At the **Villa Serbelloni** (the RF conference facility) my room was down on the lake, about a three minute drive or 10 minute walk from the main Villa which is quite a way up the hill that parts the lake into two legs. They would actually drive down to get us if we called the villa, but the walk was much too nice so I always walked. This particular location was for men only because even though the rooms were very nice, the bathroom facilities were shared. The view from my room was spectacular. I wondered if it was one of the best rooms at the Villa. The Rockefeller Foundation “hosts” at the Villa were Bill and Betsy Olson and were from Denver. They had quite a bit of news on the Big Thompson flood that had recently occurred and about which I had only sketchy details from the Spanish newspapers.

The Olsons host the sit down dinners with butlers, etc. It is quite an affair. But I thought there were too many rules and regulations such as that after dinner, the bar was open only from 9:00 to 9:30. Period. Also, meal hours were punctual. But the food was excellent. I thought the conference was very good but we did work long hours. One night I went to the town of Bellagio (actually right down the hill) with Joe Black and Gertrude, his wife. She had to stay in a hotel in town because there were no facilities for wives to stay at the villa. I really enjoyed the town. Before returning to the villa I shared a bottle of wine with three others from the conference, from Colombia, Costa Rica and Sri Lanka.

Besides the presentation I made to the conference itself, they asked me to make a presentation to anyone who wanted to attend after dinner, including the resident scholars (who did have spouses with them). These were mostly pictures of La Barranca and the “corn on the rocks.” In particular I was pleased by one of the scholars, a photographer, who commented on the quality of my slides.

I thought my paper about ICTA was also very well received. It contained the “transistor radio” diagram that we had put together with the *Gerencia* to explain our methodology. It had the approval of the Institute and was an official unveiling of a process that was very new and that remained essentially unchanged for many years. <http://ufdc.ufl.edu/UF00081824/00001> The group was very knowledgeable and they impressed me as well. Sue Almy of the Foundation and who continues to be a friend was one of the coordinators of the conference. Another participant was Uma Lele who impressed me greatly. (Many years later she was to become a Graduate Research Professor in Food and Resource Economics at the University of Florida.)

Because I left the villa at 5:30 in the morning to catch my flight home, they had made a continental breakfast and a thermos of coffee for me the night before. After taking off from Milano, I had a regular breakfast over the Alps, lunch over England and Iceland, and dinner over Canada and Maine, landing in the Lufthansa 747 in Philadelphia. I continued on with a visit to Jim and Evvie Trexler and other cousins in Pennsylvania before returning to Guatemala on Pan Am from New York. Edited to here

On the way to vacation in September, I stopped in Houston where I met Tito French (and Pat, his wife and Chris, their son) so that Tito and I could put the finishing touches on the paper on *Multicultivos* from the El Salvador work. <http://ufdc.ufl.edu/UFO0055291/00001> On the way back after vacation, I stopped in Gainesville, at the University of Florida, to give a seminar on the work in Guatemala and for general consulting. While I was gone, Carlos de Leon returned to Guatemala and finalized the work on the air photo reconnaissance work. Our preliminary conclusions had been strongly confirmed. Of the total area cultivated, 64% was in ladera, the land with more than 12% slope and only 36% was on land with less than 12% slope. Only 9% of the maize was being grown as a single crop (not associated with another crop). Of all the area cultivated, 71% was in associated crops. Nearly five times as much land was producing beans in association with other crops (maize and sorghum or just with maize) as was in beans alone. *So Socioeconomía was working with by far the most important group of farms and farmers both in number and total area, and the rest of the institute was working with the less important number and area.*

By the end of October we had all our farm record data tabulated and preliminary analyses completed and were waiting for the farmers to finishing harvesting to complete the analyses. In October, the Gerencia called for a meeting with *Socioeconomía* to discuss our philosophy and how we thought we should fit in with the other programs after having completed two years of experience. We translated the article I had written for the Bellagio conference and were planning to use this as well in the meeting with the Gerencia. <http://ufdc.ufl.edu/UFO0055293/00001>

In reviewing the transcription of the tapes of the Bellagio conference, sent to me by The Rockefeller Foundation, I decided to

add a section answering some concerns expressed about *location specificity* with respect to our ICTA methodology for generating technology for traditional agriculture, and that I had not adequately discussed. This was a very important concern regarding working with limited resource farmers.

I would like to discuss the “problem” of location specificity. It is my feeling that in working with traditional agriculture, this is a necessary condition if one is to be effective. Agriculture is still traditional in these areas *because* it is location specific, and hence, it has not been receptive to the kind of technology that requires the *homogenized conditions* created by mechanization. Location specificity is a condition peculiar to traditional agriculture, which must be recognized both by agricultural workers and by government decision makers.

But I also believe that the methodology we are developing for working with traditional farmers is not a high cost method for arriving at only small groups of isolated farmers. By choosing farmers on the basis of their cropping system, rather than on the basis of geographic location, we have been able to reach a large number of them with a relatively small investment. Granted, this may ultimately involve having two or more specific groups of clients within one geographic area, but still the cost can be very reasonable. With respect to extension, I can see definite advantages of having agents specializing in commercial agriculture and others specializing in traditional agriculture. After all, in most places, the ag agent does not handle problems of the housewife; there are home demonstration agents for that. Why not go one step further if we are serious about reaching the traditional farmer? *Monthly report, September-October, 1976.*

The concerns I expressed above in 1976, predated the creation of the term “Recommendation Domain” which later gained wide acceptance and use. But it was quite evident to me at that time, that the kind of “widely adaptable” technology that was being spread in the United States, and which many traditional scientists were trying to spread in the Third World, was not appropriate for the traditional farmers we were supposed to have as clients of ICTA. *Broadly adaptable technology (often seeds) requires that the farmer be able to modify his or her environment to satisfy the **needs of the technology**.* This, the well capitalized

North American farmer could do, but the traditional farmer with whom we were working could not. Therefore, *the challenge was for the scientists to create technology that fit within the environmental and economic conditions of the farmers who were their clients*. This, of course, was very much more difficult and a challenge that many scientists were not willing to accept. It is easier to prescribe the environment (fertilizer, pesticides, irrigation, etc.), that is, the complete technological packages, required for the technology (seed).

Another innovation that we in SER had worked on was the evaluation of the *acceptability* of technology. We considered this a *quality of the technology* rather than a measure of the level of adoption. We felt at the time that it was a means of discriminating among the different parts (components) of the technological “packages” with respect to farmer acceptance. We also thought it would help differentiate among regions in the general level of acceptance of the technology being generated by ICTA.

In December 1976, the long anticipated meeting between Socioeconomía and the Gerencia finally took place. Bob Waugh and Astolfo Fumagalli (as Sub Gerente) and Eugenio Martínez (Technical Director) attended, but the Gerente General, Mario Martínez, was on vacation. On the first day, which was only a half day, we presented a description of how we had been working to develop a methodology for integrating Socioeconomía into the technology development process of the Institute and then presented, in detail, the methodology that had been published by ICTA in the form of the paper I gave in Bellagio. The second day was spent in and around Tecpán where we discussed the specific case of our work there. We had carried out a Sondeo, a crop systems trial, and farm records, which included three of the four components of the methodology (the fourth was evaluation). During the day we visited three of the farmers who were record keepers (one of them was also a worker in our crop trial).

*As a result of these meetings, ICTA decided to some modification of the approach to regionalization. The regional teams including the regional director, members from the relevant commodity programs and two or more from Socioeconomía would work as a **single team** to conduct the Sondeos, design and conduct the cropping trials based on it, and initiate the farm record*

*keeping project. Heavy emphasis in the first year in both the sondeos and the crop trials was to be on getting to know the farmers and the region. In the second year, the original team was to be augmented by additional personnel and the emphasis would shift more to the biological or agronomic but with continuing participation from SER. By the third year, the primary work of SER was to be farm records and evaluation.*



Left to right, me, Bob Waugh and Astolfo Fumagalli talking with some of the farmers with whom we were working in Tecpán.

## **1977**

January and February of 1977 were extremely busy months even though it was in between cropping seasons for most of the country. Because it was the off season, all the programs in ICTA used the time to analyze their data. I had written calculator programs for the three *hand-held, programmable calculators* and with them and the non-programmable hand held calculators available to the technicians, we had enough capacity to analyze around 85% of the computational work of the Institute. I had written programs to do 1) analysis of variance on split plots with

no limit on number of treatments or replications, 2) randomized complete blocks with up to 6 replications, 3) multiple linear regression with 3 variables, 4) quadratic and 5) Cobb-Douglas regression and quite a few others. On all of the programs, the calculator gave all the statistical information including the complete table on the analysis of variances as well as an “F” test for the regression programs. Some very useful programs were those for converting plot data to *quintales* per *manzana* or kilos per hectare. I also wrote a program to convert all the tabulated farm record data into the form for the tables we use. In fact, *the calculator programs were so efficient that within two days after we obtained the last data from the farmers, we could have the draft report written with all the basic tables included.*

It was quite an undertaking, but we did manage to get all our SER reports written in at least first draft form before the deadline for making presentations at the sub-regional meetings. All together we had 14 reports to complete. Two were on sondeos and I was thinking that perhaps in the future, this type would not be needed. Their main purpose was to provide information for the Regional Teams to use in orienting and planning their work, and we had learned that this was being accomplished shortly after the survey was completed because members of the regional teams had been incorporated into the survey teams. Also, the data we were getting from the farm records that we initiated at the same time we did the survey, provided more accurate information on the economic aspects of farming. The main benefit of the sondeos was that it provided information from the year before we started work, which served as a reasonably good benchmark.

Three of the reports we did were on evaluation of technology. The one from the Oriente was not going to be published because the team had not really conducted “*Parcelas de Prueba*,” now called farmer managed plots, but rather farm trials managed by the researchers. However, the reports had already served a very useful purpose. Based on what we found in completing them, we were much better able to understand the function of the *Parcelas de Prueba* and we modified the definition of them accordingly. The most important conceptual change was that in these, it is *the farmer who becomes the prime evaluator rather than the Institute or the technician.* We could evaluate the

farmers' acceptability of the technology the following year, but we could also obtain useful information from the Pruebas *if the process does not interfere with the farmers' capability to evaluate them.*

Based on the new definition of the Parcelas de Prueba, as well as the selectivity being shown by the farmers in choosing *parts* of a complete technological package rather than the whole, it also became evident that the technology we generated needed to be simpler and designed specifically for the farmers with whom it was being tested. It did not appear to make sense to test on a large farm, the technology developed for a small farmer, nor vice versa, because we could anticipate beforehand that it would be rejected as not being appropriate. The whole concept of orientation to a specific type of farmer came into a clearer focus and there were signs that others in the Institute were beginning to modify their planning accordingly. There was a definite trend toward simplifying the technology being generated as well as that being tested with the farmers, and of specifying the conditions on the farms where each class of technology should be tested. In the Oriente, for example, the Bean Program began recommending one type of test on farms where no fertilizer was used, another where they used fertilizer but not insecticide, and yet a third where both were used.

Nevertheless, in the Institute in general, there was still a continual tendency to resist, or at least be uncomfortable in working with small farmers. This resistance surfaced again during the presentations we made in Tecpán, and specifically when we were describing our survey sample and the way we had divided it into three sub-groups, with approximately one-third of the farmers in each. The smaller, or poorer, farmers were those who could not achieve self sufficiency in corn and usually had less than one manzana (1 3/4 acres) of land. The middle group was right on the border of self sufficiency, buying in some years and selling in others, and tended to experiment with wheat as a cash crop. They farmed generally from one to two manzanas. The group of larger farmers, with more than two manzanas in most cases, was getting into vegetables and potatoes as well as wheat and had a little risk capital to invest. We felt that each group must be treated separately with respect to the kind of technology being developed for them,

and we had generated practices for each group in our own trials. As we were explaining this, the question again came up of why we were working for the small farmer. After a lively discussion, we finally made them understand that given the selection of the area, there was hardly any choice as to what farmers we were going to work with because nearly all were small farmers. The area was, in fact, recognized as a typical *minifundio* area. Just the fact that it came up at that time and place, though, underlined *one of the most difficult problems of working with small farmers -- the lack of commitment of the government (or the Institute and its agricultural scientists) to really doing it.*

Visitors to ICTA and SER during these two months included Larry Stifel of The Rockefeller Foundation (and later to become Director General of IITA, one of the CGIAR centers, in Ibadan, Nigeria); Ken McDermott of USAID; and William F. Whyte of the School of Industrial and Labor relations of Cornell University. Bill Whyte happened to be visiting IICA in Guatemala and they suggested he might want to visit us. He was very interested in what we were doing and also visited Tecpán. In a meeting at IICA before leaving, he mentioned that Cornell had formed a multidisciplinary group to study how to generate technology for small farmers but that *we were at least two years ahead of anything they had been able to come up with.* John Nickel (Director General of CIAT in Cali Colombia) and Alex Grobman (I believe Deputy DG at CIAT) also visited during this period. They were interested in learning more about our approach, but also were quite interested in the programs I had written for the HP programmable calculators.

By the time planting season rolled around in 1977 (about May 15), we had record keeping projects in La Blanca, La Máquina and Nueva Concepción on the Pacific coast; Quezaltenango, Totonicapán and Tecpán in the Highlands; Jutiapa in the Oriente; and Cristina and Panzos in Izabal near the Atlantic coast. We also were initiating projects in Montúfar and Chiquimulilla on the eastern Pacific coast. That gave us about 100 records in the Pacific coast, 100 in the Highlands, 110 in the Oriente and 50 in Izabal, for a total of about 360 farm records.

We were still conducting surveys, but at the time were considering two parts to the process. First we did a reconnaissance

survey or *Sondeo*. Then later we returned to do the complete survey sometime during the course of the year. We were doing both with the technicians who would be assigned to the area. During the second quarter of 1977 we completed a survey in La Blanca on the coast, and did the *Sondeo* of another in Montúfar, on the southeastern coast.

In March I presented the Socioeconomics program to ICTA's Board of Directors. The preparation for this presentation was useful in preparing the paper I presented to the twelfth West Indian Agricultural Economics Conference in Antigua in April. This was an updated version of the paper I presented at the Bellagio Conference in 1976 and was an attempt to describe the complete ICTA methodology in detail. I anticipated that the paper would be published by ICTA, but the Caribbean Agro-economics Society also published it. <http://ufdc.ufl.edu/UFO0055921/00001/1j>

In May, accompanied by Astolfo Fumagalli and the Minister of Agriculture, I attended the XVI annual meeting of the Board of Directors of IICA (the InterAmerican Institute for Agricultural Science) in Santo Domingo, Dominican Republic, to participate in a round table on Agricultural Production Systems. This was an important event because the scientific community was finally beginning to realize that most of the farmers in the Americas had *complex systems* including intercropping. Actually the round table focused on intercropping rather than farming systems in the broader context.

I was also scheduled to go to Panama to the PCCMCA meetings in May to chair a round table on the role of Socioeconomics in agricultural research institutes. I organized the round table, but Astolfo Fumagalli kindly chaired it for me when I was unable to attend due to the death of my Mother.

While I was in Colorado following my Mother's memorial service and clearing up her affairs, the research unit of the U.S. Wildlife Service in Denver asked for a meeting with me. I had worked with some of them while stationed in Palmira in Colombia. They were initiating a project to measure the economic impact of vampire bat control using Nicaragua as a base and wanted my advice on how such a program could or should be undertaken. They asked if I might be able to advise them from time to time

when they came through Guatemala. I agreed because it was to be only on an informal basis.

The fact that ICTA and the Socioeconomics unit were receiving a lot of international attention can be attested to by the list of visitors just during the second quarter of 1977: Hugh Roberts, L.I.F.E.; Hugo Manzano, IADS/Nepal; Richard Harwood, Loma Linda University, California; Santiago Friedman, Ford Foundation/Mexico; Chris Andrew, University of Florida; Bruce Johnston, Food Research Institute, Stanford University; Jim Converse, Cornell University; Doug Horton, CIP/Peru; Kosuke Yamashita, Japan; Lehman Fletcher, Iowa State University; Reed Hertford, Ford Foundation/New York; Arnoldus Van Huis, FAO/Nicaragua; Tom Walker, Stanford University; Ken Laurent, USDA/Washington, D.C.; and Warren Barham, Horticulture Department, Texas A&M University. A little bit later, Hubert Zandstra from IIRI in the Philippines also visited us. Also related to the international exposure of ICTA, in late June I attended the IADS meeting at Airlie House outside Washington, D.C. For me it was a very useful meeting both because it gave me a much better understanding of IADS, and because of the opportunity to meet many fellow Rockefeller Foundation Field Staff members as well as others working in agricultural development with whom I had not yet had any contact.

By August we were busy on a number of fronts. Sergio Ruano finished the report on sorghum quality for tortillas that had been requested by the sorghum program. He found that the use of sorghum for tortillas was very widespread in the Oriente and the quantity used in proportion to corn increases inversely with income. One negative factor associated with sorghum for tortillas is the presence of the glume in criollo varieties requiring the use of more water for washing it. <http://ufdc.ufl.edu/UF00055940/00001/2j> Also, the tortillas are a bit darker in color and they tend to produce hyperacidity when eaten.

Three people from SER conducted a Sondeo in the Izabal area (Region VII) at the request of the Regional Director and the report was completed by the end of the month. At the request of the Regional Director in Quezaltenango (Region I) we initiated a complete agro-socioeconomic study in the general area between

Momostenango, Totonicapán and Quezaltenango. We began the sondeo the last week of the month and planned the survey for the last two weeks of September with the help of the full Technology Testing team from that area. Neither of these sondeos had been planned nor budgeted and were putting stress on the resources of SER. However, they appeared to be urgent and certainly the requests for them were showing an increasing interest in our work, so we worked hard to carry them out. We were also requested to carry out an evaluation of the *acceptability* of and the potential demand for seed in 1978 of the new corn variety, ICTA B-1. It was scheduled for October after the harvest had begun.

Visitors during August included Joe Black, RF; Dick Tenney, Academy for Educational Development, Inc.; Roy Bronson, Purdue University; Emil Haney, University of Wisconsin/Green Bay; Ken McDermott, AID/Washington, and Ray Meyer, CIP/ Perú. Damon Boynton from CATIE visited in October.

I was in Honduras working for IADS for about 10 days in November. There I worked with some of the group of students the Rockefeller Foundation had sponsored for their PhD degree program at Cornell and who worked as a team at CIMMYT to do their research (Dan Galt, Josh Posner and Mario Contreras). Dan and Josh were working as consultants for the Ministry of Agriculture for whom Mario then worked under a contract with IADS and were creating a program that would look much like what ICTA was doing.

I was acting Technical Director of ICTA for about six weeks in November and December 1977, while Carlos Crisóstomo (from Chile) was on vacation. Mario Martínez (the Gerente General) and Bob Waugh (assistant to the Gerencia) were also on vacation during this period leaving Astolfo Fumagalli (as acting Gerente General) and me alone during a very busy time. During this period we had the presentation of results of the year's work and finalization of the plans for the next year in two regions (IV in La Máquina and VI in the Oriente). We followed the methodology of the institute that had been published in NOTICTA and distributed widely (it was heavily based on the Andrew and Hildebrand methodology book that was translated into Spanish while I was in El Salvador <http://ufdc.ufl.edu/UF00080893/00001>

and published by ICTA in 1977). This was the methodology I had reported in the Bellagio and Antigua meetings. I felt the technicians at the regional meetings finally really began to understand what it was all about. Visitors in December included a delegation from CATIE and Jack Traywick and Damaris Chea (the Director General) of IDIAP in Panamá. Jack and Damaris asked Sergio Ruano and me to go to Panamá for two weeks following *Semana Santa* to help them conduct a Sondeo.

I had been searching for a female sociologist for several months. During the period I was acting Technical Director we finally hired one for SER. She was Maria Chinchilla, a Guatemalan sociologist (B.A.) from Brockport State University in New York. I had the authority to hire *peritos* and *ingenieros* on my own, but the Gerencia insisted on interviewing any sociologists before I could hire them. They (Astolfo Fumagalli) approved and she started on November 16.

## 1978

January 1978 visitors included Dan Galt of the PNIA team working with IADS in Honduras and Tom Walker from the University of Florida team still working with CENTA in El Salvador. Chris Andrew overlapped with them in early February. Other visitors included Congressman Clarence Long from Maryland; Josh Posner, also of the PNIA team in Honduras; John Hildebrand, a cousin of mine and an agricultural economist (PhD, University of Chicago); and Bill Whyte (from Cornell) who came to Zacapa while we were there in a regional meeting.

In February 1978, I made a trip to West Africa to participate in a symposium on integrated farm production systems for the Sahel region sponsored by the Club du Sahel and the Interstate Committee to Combat Drought in the Sahel (CILSS). The meeting was held in Mali, and on the way I went to Nigeria to visit IITA and the University of Ibadan. At IITA I had good conversations with Christine Okali, a sociologist, and Fred Winch, an agricultural economist. I quote from my trip report:

Fred Winch is on the Farming Systems team and showed me the trials they have on the station. Their emphasis is on the use

of low lying land along the rivers and streams that they think is one of the greatest under-utilized resources in West Africa. They are trying out a series of crop and technology combinations using local farmers, who as I understand it, were laborers on the station who showed particular promise and capability. The different treatments are interesting and show imagination in the use of the resources. Good records are being kept and a great deal of excellent information should result. But one must raise the question of the area of applicability of such work, and to what extent one of the international centers should be involved in it. Coming from a national institute, I could not help but make comparisons between the orientation of an international center and a national program.

From what I have been able to determine, most technicians who work in national institutes have accepted the fact that small and medium, and particularly traditional farmers, are all subject to very site specific agro-socioeconomic conditions which complicate the generation of technology for them. *Because of their very nature, international centers must work on technology that can be generalized over large regions and in different countries.* This requirement does not coincide with the realities of the traditional farmer. Hence, it seems to me that the centers are investing funds in an impossible task to the extent they are undertaking applied work such as that which I saw at IITA, oriented to traditional farmers.

In Nigeria there is not necessarily an overlap or competition with a national institute such as there is in some of the other countries that have international centers (CIAT and ICA in Colombia, for example). However, it seems to me that serious consideration should be given to strengthening the national institutes as an alternative to the continuation of technology generation for traditional farmers via the international centers. I feel that because of site specificity, it necessarily must be the national institutes that directly benefit the traditional farmers, and for that reason, competition from the international centers is counterproductive.

What then should the international centers be doing? I see three highly productive areas of work, all of which would be complementary, rather than competitive with the national institutes. One would be the mass production of national institute publications using the money now invested in competitive research. Virtually none of the national institutes in developing countries have sufficient budgets to publish the amount of material and the number

of copies required to put their findings into the hands of others around the world who are working on similar problems.

I am not thinking of scientific or journal-quality documents. Rather, what needs to get into the hands of fellow workers are all the research reports put out on a periodic basis in all national institutes. Experts from the international centers could, through training, help improve the quality and format of such reports, but this would not necessarily be the primary thrust of the project. The principal object would be to accumulate all possible information of value, publish as many copies as appropriate and send them to as many other national institutes and technicians as would have need for or interest in the information.

A second area of work for the international centers would be to provide the basic, or backstop research which is generally so expensive that it is a luxury few developing countries can afford. In ICTA, for example, we have to accept that certain phenomena happen without being able to look into why they happen. We simply do not have enough resources to be able to investigate them. The international centers have not only the resources, but also the personnel and laboratories to be able to do it. And they can do it on a regional and international basis without being competitive with the national centers.

A third area in which the international centers can provide valuable help to the national centers is in training. But again, the type of training must be such that it is complementary to and not competitive with the different national institutes and universities. One example has already been mentioned -- training in writing technical articles. This could be extended to training in the reproduction of the same articles as well as all other types of publications that are done at the national level. I am aware that CIAT is doing some work in this field at the present time.

As part of my visit to the University of Ibadan where I talked with Milt Snodgrass and Tyler Biggs in the Department of Agricultural Economics, I visited two villages, with which the Extension Department is working in a project headed by Johnson Ekpere, whom I had met at the Bellagio Conference in 1976. One of the things that most caught my attention in Nigeria was the apparent bad state of the economy, the great inflow of petroleum money notwithstanding, and more likely the cause. Prices were skyrocketing, but many essential items were very scarce. Salt was just coming back to the market but flour was not available. Cars

were streaming into the country leading to an abundance of traffic jams and in an unbelievable number of wrecks, but there were long lines to buy gasoline. It appeared to me that the country was simply not equipped to handle the tremendous amount of money that was coming in from the petroleum.

Mali, on the other hand, I found to be a quiet, almost quaint country, in which the relatively small amount of development that was taking place was quite orderly. Besides attending the conference I was able to get to a livestock experiment station and visit a village where both crops and livestock were produced, thanks to the kind offer of Frits Penning de Vries of the PPS Project, and in company of Bob McDowell of Cornell University. Later I gave a seminar for the Mali II Project group at the request of Dr. Almouzar Mohaly Maiga who headed the group and was a former student of Bob McDowell. The organization of the Mali II group made me appreciate how ICTA is set up. They had a relatively weak staff of Malians and about 12 expatriates, who were supposed to be working as a single, integrated group under Malian leadership. But the expatriate group acted and operated as a separate body under their own chief of party (they were recruited through about three separate "body shops"). Even though Dr. Maiga seemed to be strong, patient and capable, he had a very difficult task on his hands trying to merge all his people into one body. I had to contrast this with ICTA where even though the expatriates came from a number of countries and were financed through several different arrangements, all of us *worked for ICTA* in well defined and established line positions and were not just advisors. The difference between being an advisor and holding a line position can be quite subtle, but it is critical. I was very proud to be a representative of ICTA and to be able to talk about what the institute had accomplished. The methodology which had been created within ICTA for generating and promoting technology for traditional farmers was, on the one hand, both simple and logical so that people could immediately understand it and on the other hand, effective and feasible within limited national budget constraints. Wherever I talked about it, it always attracted a great deal of attention and comment, and it did at this conference as well.

One of the highlights of this trip was finally getting to meet David Norman who was then, and still is at the Department of Agricultural Economics at Kansas State University. He impressed me a lot both personally and as an important resource in development work. Independently, during his 11 years in Northern Nigeria, he developed a methodology very similar to ICTA's, but was unable to get it incorporated into an action program, at least by the time of the conference. However, the Malians were interested in it and were going to try to adapt it to their conditions. That was one of the reasons they were so interested in what we were doing at ICTA.

Obviously, SER was under a lot of pressure during all these years. We had activities throughout the country and were working with ever more limited resources. Nevertheless, there was increasing pressure for us to undertake an "impact evaluation" of the work of the institute. As ICTA was approaching five years as an institute, and with hopes of outside funding particularly from the InterAmerican Development Bank and USAID, many people said we needed such an evaluation. I tried hard to make the ICTA administration understand the complexities of an impact evaluation and further, argued that the only way we could undertake one would be either with additional resources or cut out much of the ongoing work. I was very reluctant to do the latter. Facetiously I argued that if it looked good, take the credit, and if it looked bad, blame it on the weather! That's usually what happens anyway.

By planting time, April 1978, the institute still had not firmed up on the general methodology even though it had been published in a special issue of NOTICTA and in the 1976 Report of the Gerente <http://ufdc.ufl.edu/UF00053941/00010?search=NOTICTA>. The Gerencia argued that the Regional Directors needed flexibility to maintain initiative on their part. I argued that we were not capitalizing on the result of four years of work that had gone into the formulation of the methodology. I felt that with each region doing its own thing, we were drifting into a state of confusion, if not chaos. This was particularly true for SER that had activities in each of the regions. Part of the problem was that the Technical Director, Carlos Crisóstomo, a very able agronomist from Chile, and who had been a Regional Director prior to his current position,

either did not agree with or did not understand the methodology as presented in the Gerente's 1976 report.

There were two major points of disagreement. One concerned the use and nature of the "Farmers' Tests" (Parcelas de Prueba), and the other was with respect to what was a "good" technology. In regard to the latter, *this involved whether the agronomists were the final evaluators or whether farmers evaluated the acceptability of the technology*. Crisóstomo, like most agronomists of that time, tended to think of a "good" technology as one that *increased yield per hectare whether or not it could be put into use by the clients*. If it required a credit program or a source of water that was not available to the farmers, that was not the fault of the agronomists who had completed their responsibilities by delivering a physical or biological *technology package* by which yield "could be" increased. For this reason, he objected to the use of the concept of "acceptability to the farmers" as a guide to what is a good technology. *He insisted that it was the biological result of the Farmers' Tests (how high the yield was) that was the key point of the tests*. He did not want to wait until the following year so that we could evaluate (actively) the farmers' evaluations of acceptability by seeing how many of them could use the technology on their own. He felt that a low "index of acceptability" implied that the *research* had been "bad." I tried to argue that the indices of acceptability would be higher if we did research, from the beginning, more oriented toward what the farmers who were our clients (presumably the small and medium farmers) would be *able to use*. And I understood the mission of SER from the beginning to be to help the institute understand the needs, capabilities and resources of these farmers.

This difference in the concept of the nature of Farmers' Tests was very important. One approach, that of SER, was the idea that after the agronomists and the economists had done all the evaluating they could do and were satisfied that a technology was "good" for the target farmers from all they had been able to learn from and about them and their conditions, it was still the farmers who must judge the technology for themselves. The Farmers' Tests therefore should be *run by the farmers* so they would have the first-hand knowledge about what the technology was, what it could do, and *what it required* from them, in order to be able to

evaluate it. It also followed that there should be a large number of these tests in the area to make their evaluation of the technology and our evaluation of the acceptability valid and useful. But if the agronomists ran the tests using the farmers only as laborers, then they would not be able to manage more than just a few.

Strangely enough, I was able to understand the nature of this conflict in points of view much better after attending the meeting of the Society for Applied Anthropology in Mérida, Mexico that April. I felt that anthropologists were trained to be descriptive, and for this reason are keen observers. But they do not normally identify with a particular commodity or product. Furthermore, they thrive on diversity. Agronomists on the other hand are trained to produce specific products, and traditionally, in an artificial surrounding (experiment station) with a very limited environmental variability. They strive for homogeneity rather than diversity and are interested in statistically significant biological differences resulting from their experimental treatments.

My training as an agricultural economist was oriented approximately in the middle of these two fields. I was product oriented, but based product development on more aspects of the biophysical and socioeconomic environment than the agronomists, though not as much as the anthropologists. As an economist, I tended to work with those aspects of the environment that I could *quantify*. Many times, this exasperated the anthropologists, but because this meant more variability, it also tended to alienate the agronomists. Yet because we were closer in orientation to agronomists than were anthropologists, agricultural economists tended to be accepted into agronomic research organizations easier than anthropologists. It was a difficult task to create a multidisciplinary team in which all three types of orientation could work in harmony. This challenge was not necessary if the development of agricultural technology is directed primarily to commercial farmers. But it was obvious to me that it is necessary if development of technology for small and medium farmers with limited resources is to be successful.

Another problem that I sensed in the institute was that many agronomists still did not want to, or felt we did not need to develop technology specifically for small farmers. Their feeling was that small farmers could get what they could from the larger

farmers if they were *really interested* in new technology. This, of course, was also the feeling in the United States at the time (trickle down; get big or get out), but that is another story.

Interestingly, the agronomists were complaining that the yields we were reporting from the farm records were too low. In part, this was because the “check plots” in *their* trials and Farmers’ Tests, that were *supposed to reflect what the farmers were doing in their fields*, were much higher than our reported yields. In reality, this reflected that fact that the agronomists managed the trials and used “best practices” even on the check plots, and these required more resources than the farmers were able to devote to the crop on their fields. The reasons for these differences seemed very transparent to me, but spawned many “yield gap” studies over the years even by international organizations.

To help us assess our technologies, I was pushing for two different types of “check plots” to be incorporated in the farm trials. One was a copy of what *each individual farmer* was doing on the land where we had the trial in order to estimate the effect on yield of the experiment, itself. In comparing this with the farm record yield estimated by the farmer, this would help respond to the concern that the farm record yields were too low. The other was a uniform or standard plot that represented the “traditional practices” of the area and would be the same in each trial, year after year. The purpose was to be able to judge how good or bad each year was in each region, to have a basis upon which to judge that year’s results, and compare them with results from other years.

In April, 1978, Sergio Ruano, one of the anthropologists in SER, and I spent two weeks in Panamá as consultants for IADS working with IDIAP, the equivalent of ICTA in Panamá. The purpose was fourfold: 1) to discuss the ICTA methodology with the IDIAP staff, 2) to train the majority of their staff in interview and survey techniques as it was used in the ICTA methodology, 3) to participate in a Sondeo of the four priority areas in Panamá to help them judge which were the most important, and 4) assist in the preparation of their USAID project paper. We spent more than 2/3 of the time in the field. One of those days we spent giving a seminar for the entire IDIAP staff and the Administrative Director, plus some people from other entities. The Director

General, Carmen Damaris Chea, was with us most of the time we were in the field, and also met with us in Panamá City to review the report before the final typing.

By mid year 1978 I began noticing more danger signals regarding the integration of the social sciences into the institute, and, in fact, the future of ICTA as a champion for the small and medium farmers. The eleven *Perito Agronomos* (high school trained) working for SER in the five active regions were transferred to the regional technology testing teams and out of my control as the Coordinator of SER. We had requested five new, university level positions to replace them and to have one in each region, but these positions were not filled. Instead, I had to assign each of the university level persons I had at the national level to be in charge of one or more regions. Part of the problem was that SER was originally budgeted in Programming rather than the Technical unit, so there was never a budget for SER in the regions, where Programming did not work. For us to get a budget in a region was considered a *new activity* which was very difficult to obtain under the ever more scarce funding situation. Even worse, Programming, the unit in charge of the budget, never budgeted anything even for the Peritos in the regions in 1977, so the regions had to share their budget with the SER people. This was almost guaranteed to alienate them from the others in the regions and to dampen integration. In the absence of integration, I could see the field work tending to revert to the traditional approach in which the orientation toward specific farm problems and conditions is lost. This, in turn, lowers the efficiency and usefulness of the work of the regional teams. I was afraid that if we were not able to maintain the level of integration we had in the earlier years, the socioeconomic unit would lose its usefulness and not warrant the international interest and support it had been getting.

An example was evident in the Tecpán area. The nature of the work had been modified and complicated to make it amenable to contribute to a Soil Science M.S. thesis and lost its direct relationship to what the farmers in the area were doing. The types of treatments and *the complexity and number of trials caused many farmers to become disenchanted with ICTA and some even refused to continue cooperating with farm records.* One of our Peritos, Teodoro López Yos, was permitted to continue working directly on

the project as we had originally conceived it. He had Farmers' Tests and Farm Trials utilizing the simplified technology and design we had used the year before. In 1978, most of the farmers who conducted the Farmers' Tests in 1977 had adopted the two row technology (both the system with wheat interplanted and the compact two row system to increase the production of corn). They were also continuing to experiment with other crops to interplant with the corn and were having very good results with potatoes. Further, we had no complaints from the farmers on whose land the Farm Trials were located. Unfortunately, because of budget limitations, we had only five farmers with Farmers' Tests and only three with Farm Trials, compared with 28 Farm Trials for the thesis!



Teodoro López Yos with one of his farmers and her farmer test.

To add to the woes, there was a rumor that we were going to get a new Gerente and Sub-Gerente in 1979. The benefits to the Institute of the political, professional and personal strengths of Mario Martínez and Astolfo Fumagalli, the Gerente and Sub-Gerente during most of the first five years were immeasurable. Without the kind of experience, foresight and stability they provided, it is hard to imagine that ICTA would have had the same

kind of local and global impact on agricultural technology generation and development that it had.

On the positive side, I did not detect any inherent antagonism toward SER from the other technicians in the field other than that generated by competition for scarce funds. Antagonism was not the cause of the problems we were having. I felt that the competition for scarce funds in the regions was the main element.

In May I spent four days in Honduras attending a meeting on “appropriate technology” called by USAID to discuss a project proposed following a recent visit by Congressman Clarence Long, Chair of the Appropriations Committee, who suggested Honduras as a site for such a project. As one might suspect, most emphasis centered on “gadget” technology rather than on considering “appropriateness” from the point of view of the farmers’ needs and limitations. An example was a mechanical planting “stick” that was on display. Its main advantage as near as I could see was that it placed the seed in the hole made by the stick. This, of course, was probably a great advantage for the person who invented it and who probably had trouble hitting the hole with seed dropped by hand. But it would not help farmers who virtually never miss the hole even without bending over. For the farmers who have to use steel tipped planting sticks all day long, the great increase in weight of the “improved” stick would be a distinct *disadvantage*.

At the meetings I met John Balis, Appropriate Technology Officer, Latin American Bureau of USAID/Washington. He impressed me and was very knowledgeable in the field. He was the first person I had met who could put together a reasonably good argument for introducing small tractors to farmers just as the world was facing an energy crisis with its attendant rising prices and scarcities. He argued that large number of small tractors would reduce the risk of small farmers having to depend on one large tractor. He also proposed developing engines that could run on agriculturally produced alcohol to reduce the problem of petroleum dependence. But his argument did not cover how the farmers were supposed to obtain and maintain the tractors.

In June 1978, SER was reinforced substantially with the return of two people with Master’s degrees. Carlos Reiche earned his in Food and Resource Economics at the University of Florida

and Rolando Duarte in Anthropology at Wayne State University in Detroit. I had supervised Rolando's thesis. On the other hand, in July we were going to lose Sergio Ruano who was going to New Mexico State University for an M.S. degree. Visitors to ICTA and SER during June included: 10 students and 2 professors from Purdue, 2 people from Cornell, Dr. Lohani from the IADS project in Nepal, 4 people from the program on the control of damage from vertebrates of the U.S. Dept. Of the Interior (the one I had visited earlier in Denver), Dick Harwood (formerly with IRRI) and Foster Cady who were both AID consultants to CATIE, Joe Saunders and Myron Shank of CATIE, Ralph Cummings, Jr. of IADS, and four others from CATIE including Pedro Oñoro, Joe Saunders (again), Ed Locatelli and Benjamín Quipondría.

The rumored change in Gerente and Sub-Gerente of ICTA came about at midyear shortly after the change in government. Ing. Carlos Ramírez, who was head of Development at DIGESA, was named Gerente. At DIGESA he had the extension service under his direction. The new Sub-Gerente was Ing. Jorge Pineda, who had headed the Seed Program at ICTA. An early change was new emphasis on the "Faja Transversal," a newly developing region in the Petén in the northern part of the country which was of special interest of the new president.

Partly because of the uncertainty prior to the change of government, partly because of the new management in ICTA, and partly because of other factors, communication between the central offices and the regions, and among the regions was diminishing during this period. This had a more pronounced effect on the technical unit because we were spread out over the whole country. Our coordination of activities was being adversely affected. It also affected SER more than the production or commodity programs. They could go on with their field trials, but SER depended on an interaction with all the programs and on farmers in order to provide a service. And when we were not working actively in the field with the agricultural technicians, we necessarily reverted back to traditional social science type work where we ended up evaluating work already done, rather than contributing directly to the work, itself. For the same reason, the work in the field also reverted back to the more traditional approach as well.

An example of what the lack of communication can mean, but also a big surprise occurred when Carlos Reiche and I attended two field days in Region VI (where the trials SER had at La Barranca were located). On the experiment station there were trials of the corn-beans-sorghum crop association like those we had at La Barranca. It was interesting to reflect that it was for wanting to work with these crop associations that we were thrown off the station in 1975 in the first place! However, during the presentation, they said that the trials were based on interesting work done at CIAT! *This meant that not only were they not recognizing the work we did, but also that CIAT was given (or taking) credit for something SER at ICTA had done.*

In July and August we began having a series of discussions with Christina Gladwin, a Rockefeller Foundation Postdoc (Rockydoc) who had been working in Guatemala for about a year. The methodology she was using (decision trees) appeared to be useful for us in our surveys and we were interested in trying it out in some areas. Partly because of this we began negotiating an agreement with the International Fertilizer Development Center (IFDC) where she was employed so she could be incorporated in ICTA as part of Socioeconomía. She had already been working with us (mostly Carlos Reiche and María Chinchilla) trying to help us understand better the relationship between livestock and crops in Nueva Concepción. We had completed the survey thinking mostly about livestock and did not give enough attention to the crops and to the very important interaction between the two. We also wanted Christina to work with us on the survey we were about to initiate in the Zacapa area.

In August I also traveled to Columbia, Missouri (University of Missouri) to participate in a meeting on the Title XII, Sorghum and Millet Collaborative Research Support Program (CRSP). I was a "Third World" representative on the so-called Panel of Experts that had to pare down the proposed program from about \$5 million to about \$3 million. It was an interesting experience for me but I was not convinced that the program (the CRSP) was going to leave something concrete in developing countries as hoped. I felt it would channel more funds to the participating U.S. universities and provide travel money for more professors, but would not be of much help to countries like Guatemala.

The rather bleak outlook we had during the change of government and management in ICTA began to improve in September and October. All of the national SER staff (those stationed in Guatemala City) traveled to Zacapa to conduct a Sondeo of that area. This is a near desert area where irrigation is required for almost any crop and where the Horticultural Program of ICTA had been working several years trying to develop technology and infrastructure for export vegetables and fruit (cantaloupe, onions and peppers, mainly) with some work on other crops for local processing and consumption such as tomatoes and watermelons. Tobacco was very important in the area and competed strongly with vegetable production. Tobacco also influenced livestock production because the land could not be used for grazing for up to 7 months when it was in tobacco. And in this area, the farmers were known for their livestock (and the cheese they make) and needed to give special attention to crop rotations to provide a constant source of feed for the animals.

Christina Gladwin accompanied us during part of the Sondeo and we became familiar with her decision tree methodology. I felt that there was no question the logic of the methodology helped us with the Sondeo. It seemed to particularly help us in trying to understand the relationships between competing alternatives. I could see it becoming a permanent part of our Sondeo routine. The major problem I saw was in the length and complexity of the questionnaire required.

Later on, Christina, María and I were going over Christina's decision tree questionnaire for Quezaltenango and discussing it with Ramiro Ortíz, the Regional Director there. Besides all the questions she needed, she also was including many questions apparently requested by IFDC for a possible 'production function' analysis sometime in the future. Since IFDC was paying her \$11,000 per year for her study, it was hard to cut out some of their questions. But they were creating problems for us because ICTA was collaborating in her project. Because of the length and complexity of her questionnaire, some of our SER farmer collaborators were complaining, and our technicians did not want to lose the confidence they had built up with them over time. She finally agreed to cut out about one fourth of the questions and we agreed to help with the survey. María, Carlos, Rolando and

Guillermo were all going to help in order to familiarize them with this type of survey.

In Jutiapa, Carlos and I had an opportunity to analyze data from the farm records that had been kept. All the bean data were complete by September. We found that the area planted in 1978 was down drastically compared with the areas of the same farmers in 1977. The area planted as a percent of area the previous year was: rice 66.7%; corn 93.2%; sorghum 87.3% and beans 66.8%. With respect to beans, the reason for the sharp drop was that the farmers simply had run out of bean *seed* because of the string of four bad years. Also, they did not want to risk the purchase of bean seed. However, the rains were very favorable and the yield of beans increased 316% over 1977 (11.7 vs. 3.7 qq/mz) resulting in an increase of overall production for these farmers of 212% over 1977 (that is, more than double). The fact that corn, in particular, and sorghum acreage had not decreased by much reflected the great importance of these crops as food for farmers in the area.

I had assigned María Chinchilla to work mainly in the Quezaltenango area as the representative of SER (each of the technicians from the office in Guatemala City had regional as well as national responsibilities). One day I accompanied her and some of the Peritos working with on-farm trials in the area to see how they were coming along. On one farm, we saw a farmer on the porch in a hammock and most of us went to talk to him. María, on the other hand, had noticed a woman working in a field near the house and went there to talk with the woman. After greeting each other, the woman said she couldn't figure out why the ICTA technicians always talked with her husband. She said he wasn't around that often and was usually drunk when he was at home. She did most of the farming and made most of the decisions, particularly when he was working on the coast. María berated us when we all got back in the vehicle, and she and Christina began a concerted effort to work with the women farmers in the area. *By September 1978, ICTA had entered the realm of Women in Development.*

On October 3, ICTA had the first gender-specific field day ever held (and it was probably the first anywhere in the world, for that matter). María and Christina had requested the technicians from that region to help them organize it. They had been working with a number of women farmers in San Carlos Sija and wanted them to see what was going on at the Labor Ovalle Experiment Station that was the ICTA headquarters in Quezaltenango. A total of 55 people came and 35 of them were women! The ICTA methodology was explained and the women were shown the corn, wheat and potato work on the station. Then all participated in a potato storage project in a “rustic silo.” It was reportedly the largest field day the station had ever had and there was much active participation by both the men and the women.



Field day for women farmers at Labor Ovalle

An interesting aspect of the “blending of the disciplines” came to light at the field day. People were invited from the areas of three of the ICTA technicians. From one of these areas no one came, from another, one couple showed up, and 53 people came from the area of the third technician. We had been

working in all the areas and considered that the best technician was the one from which the 53 people came. The Regional Director, though, rated them in the reverse order. He felt his best technician was the one from whose area none came and that the one who had 53 people show up was not a good technician because, “he spends too much time making friends with the people and not enough tending the trials.”

Carlos Reiche and I made a three-day trip to El Salvador at the request of USAID/El Salvador. I had understood that they wanted some help to decide what crops they should focus on in the upcoming “mini-irrigation” projects, but when we arrived, they had in mind someone to help them decide on *crop policy* for the whole country. That, obviously, would not have been possible in the time we had to devote to it. It also appeared that the Ministry of Agriculture was not very interested in the activity because AID was not able to obtain a single appointment for us to talk with them! So after spending a few hours with various people in CENTA, we found that a number of them were interested in the possibility that our entire group from SER in ICTA go to El Salvador for a week to work with them on a Sondeo of one of the areas in which they were beginning to work on a multi-disciplinary basis.

On October 15 I left Guatemala for a second conference in Bellagio on “Integrated Crop and Animal Production to Optimize Resource Utilization on Small Farms in Developing Countries.” It was one of the first formal attempts to integrate animal and crop production in the work we were doing. The topic that was given me for my paper was, “Motivating Small Farmers to Accept Change.” I did not like this title and in the published version in 1980, I changed it to: “Motivating Small Farmers, Scientists and Technicians to Accept Change”!<sup>4</sup> Among the many interesting people I met at the conference was Robert Chambers. He invited me to attend a seminar they were having the following week in Sussex (England) on “Quick and Dirty Survey Methods” as a result of my discussion of our Sondeo techniques. However, because of prior commitments, I was unable to go.

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4.(Agricultural Administration 8:373-383)  
<http://ufdc.ufl.edu/UF00055922/00001/2j>

Because it summarizes succinctly how I felt at the time about technology generation for limited resource farmers, I quote from the Bellagio paper here:

This discussion commences from the premise originally proposed by Schultz, and is widely, though not universally accepted: small farmers are efficient in the utilization and allocation of available resources among known technologies if they have been farming under stable conditions for some time. As we are, by design and purpose in this conference, concerned with farmers who are not changing their production methods, this premise should include most of those farmers. Schultz's position implies that small farmers will, and do accept change when the available resource base changes or new and *appropriate* technology becomes known. Otherwise, they could not be efficiently adjusted to alternatives they now have. But it is important to understand that this efficient adjustment is in terms of the *farmers' own understanding and interpretation* of their situations, and it is not necessarily efficient according to the perceptions of well meaning, but incompletely informed third persons. Since it is not third persons, in a free society, who make choice of technology and resource allocation decisions, it is evident that farmers' actions need not reflect third person solutions, *unless they are based on a near perfect conception of the farmers' situations.*

A second characteristic of small farmers, gradually being recognized, is the high degree of *location specificity* of their agro-socioeconomic conditions. In commercial agriculture, the tractor and a strong capital base are effective homogenizers of what is otherwise a complex milieu. To persons who are trained or accustomed to being able to produce widely acceptable, tractor based technologies, this characteristic represents a strong barrier that hinders their effectiveness in producing usable and acceptable results for small farmers. But it is also a characteristic that must be considered explicitly in any technology developing system if it is to produce technologies that small farmers will be motivated and able to accept.

If small farmers are not changing their production methods because they are not being offered appropriate technology when so many people are working to produce it for them, what is the problem? If it is agreed that small farmers are efficient in the allocation of their resources to known and

appropriate traditional technologies, it means they have been motivated in the past to accept change. Hence, the problem is not one of motivation, as such. Rather it is one of offering "changes" that are not appropriate as perceived by the farmers, themselves. *It makes no difference to a farmer how a third person views any specific technology. If he or she, himself or herself, does not feel it to be appropriate, he or she is not going to be motivated to accept it.*

In turn, the problem stems from having most top level technology "generators," who are agriculturally trained and "product" oriented, working on experiment stations or in other highly controlled conditions where they consider only a limited number of variables; most of the "transfer mechanism" generators, who are trained in the social sciences and are "cause," but not product oriented, struggling with the vast quantity of variables that condition acceptance or rejection of technology at the farm level; and "goal" oriented agricultural economists in the middle, complaining that the agricultural scientists do not consider enough of the variables in their work, but ignoring the pleas of the social scientists-that including just the quantifiable variables is not sufficient either. This picture is complicated further because agronomists work primarily with soils and plants, which they are convinced are the most important components of agricultural production; sociologists and anthropologists work with farmers, whom for them are obviously the most important component; and economists work with desks and computers studying means of achieving specified (and frequently unrealistic) goals. It is little wonder that the poor extension or "change" agent has little to offer small farmers even though he may be supported by an elaborate experiment station and extension network manned by high level technicians. It is even less amazing that small farmers are not motivated to accept many changes that come out of such a system.

In form of summary, it should be repeated that the resistance of small farmers to accepting change is not one of motivation but rather one of not having technology available that is appropriate from these farmers' own points of view. Because of the location specificity of the agrosocioeconomic conditions of small farmers, and because they are not subject to the homogenizing influence of tractors and capital, *it is a much greater challenge to develop technology that these small farmers will be motivated to accept than it is to develop*

*technology for commercial farmers.* The most efficient way is by means of strong multidisciplinary teams who live and work in each area and who orient the technology development work undertaken for the small farmers in their zone. This implies a drastic change in the traditional role of many scientists now working on technology development and probably will meet with no small amount of resistance on their part. It may well be that in another, future conference on small farm technology, one of the papers will be titled, "Motivating Scientists and Technicians to Accept Change."

Following the conference in Bellagio, I went to Cornell University in Ithaca, New York where I presented one formal seminar, participated in a weekly luncheon with the interdisciplinary group that is organized largely through international programs at Cornell, and had many less formal conferences with staff and graduate students. I was very pleased to finally get acquainted with Randy Barker, and I also met Bud Stanton, then President of the American Association of Agricultural Economists; Loy Crowder and Dave Thurston. Dave asked me to participate in the symposium he was organizing for the IX International Plant Protection Congress in Washington, D.C. in August. Following my visit at Cornell, I returned to Guatemala to meet with Kirby Davidson, Associate Director of the Social Science Division) and John Pino, Director of the Agricultural Division, of the Rockefeller Foundation on October 30 and 31. Kirby was unable to make the trip because of a Pan Am ground crew strike in New York, and John could only spend one full day. Nevertheless, I felt the trip was very worthwhile because John was able to have lunch with the group from SER and I was able to spend more time with him than all together over the four years I had been working for the Foundation. On November 1, I left Guatemala again for vacation in Colorado. During my vacation, Bill Shaner and Bill Schmehl spent a half day with me at the cabin on the Poudre River discussing farming systems research. Bill Shaner was the Coordinator for the Consortium for International Development (CID) Farming Systems Research Title XII Planning effort and stationed at Utah State University in Logan. Bill Schmehl was

a professor of agronomy at Colorado State University. They planned to come to Guatemala in early 1979.

Following vacation, on December 4, I spent a day at the Department of Anthropology at Wayne State University in Detroit with Jan Hill, Rolando Duarte's former major professor and Carol Browner, who had been to Guatemala earlier in the year. Besides conferences with staff and students, I presented a seminar to discuss the ways in which anthropologists are integrated in the work of ICTA. On December 5-7, I was at the University of Florida where I presented two seminars and participated in a workshop on Farming Systems Research. Much of the discussion at Florida centered on aspects of multidisciplinary work, and the workshop had people from a large number of fields. *Florida was seriously working on the multidisciplinary approach and was trying to create sufficient flexibility in their programs to allow students to pursue broader careers.* One good example, who made a very favorable impression on me, was Susan Poats, a PhD candidate in Anthropology, who had 48 credits in agriculture, and had the Certificate in Tropical Agriculture. However, it would be nearly 25 years before this really came about with the Interdisciplinary Ecology graduate program in the School of Natural Resources and Environment!

Upon reflecting on my visit to the Florida program and what we were doing in Guatemala, it seemed to me that it is critical for social scientists to have a strong working knowledge in the field in which they anticipate practicing their profession (such as agriculture) if they are to become involved in *ex ante* activities rather than just *ex post* evaluation. Another aspect that could be very critical is that they must fight the tendency to be too broad as well, and try to maintain a focus in their work. I think this was one of the reasons that the social scientists in ICTA were successfully being incorporated in the ongoing program. They had acquired a very acceptable knowledge of agriculture and I kept them working on aspects that would contribute to improving the generation of technology. Whenever they started to stray too far off the path, I got them back on. On the other hand, their ability to generalize and see the broad picture was also one of the very valuable traits they

brought to the multidisciplinary team. But it was effective only when their discussion of the situation and contribution to the team effort stayed within appropriate bounds so as not to lose the interest of the other team members.

In December there was a second field day in Quezaltenango organized by María Chinchilla, Christina Gladwin and some of the engineers and peritos. One of the major attractions was uncovering a silo in which potatoes had been stored on the previous field day. Again, a large number of women participated. As a result, some of them made a request to ICTA that María be allowed to work with them on some trials in 1979. Because she had not yet had the opportunity to do field plot work, we were going to try to work it out so she could work with one of the engineers or peritos on such a trial with the women in San Carlos Sija.



María learning the trade

## **1979**

During the week of January 8, Carlos Reiche, María Chinchilla, Esaú Samayoa and I undertook another Sondeo in the area of Santiago Sacatepéquez and Sumpango. It was the

first really complete Sondeo based on the new methodology we were developing. We were working with several people from the ICTA regional team and one from DIGESA. In just one full week (5 days) we completed the survey and the report was in draft form. Included in the Sondeo was the delineation of the area and we ended up with about 4,150 hectares in which one technician from ICTA was going to be working. It was an area of corn and vegetables and presented some very interesting problems. It was at this point that *I was convinced that the Sondeo methodology was working very well* and I intended to write it up in the near future. We no longer considered the Sondeo as a “preliminary” survey. We made the decision to use only Sondeos in the future and had “formalized” the methodology for these “informal” surveys.

One of the most important activities of the first quarter of 1979 was the presentation of 1978 research results in each of the regions, and, based on these results, the preparation of the regional work plans for 1979 that would begin in April. One full week was spent in each region where we had a team working (5 regions at that time). For the first time, technicians from DIGESA (Extension), BANDESA (credit), and INDECA (marketing) attended all the meetings. I felt the result was very favorable in all the regions, although some of the ICTA technicians felt that the presence of “outsiders” inhibited a fuller discussion of our own problems. Some also felt that our rather frank arguments and discussions should not have been held in open meetings. I felt that after about the first half day, the visitors were convinced we were very serious about our work and were really trying to come to grips with the farmers’ problems and our means of contributing to their solution. The other possible reaction, that we did not know what we were doing because we argued about methodology, evaluation and interpretation, I do not think they retained. In balance, I thought it was good to have them in the sessions, and in most regions they participated actively in the deliberations.

I felt that the meetings, in general, were much more productive than in previous years. The discussions centered on trying to *interpret* results and not just their presentation in the

form of a “show.” Most of the technicians that had been with the Institute for at least one year and had participated in other sessions understood the methodology pretty well and the reasons for these meetings.

Ramiro Ortíz, the new Technical Director and former Regional Director in Quezaltenango, attended nearly all the meetings and I was very pleased with his participation. He had an excellent grasp of both technical and logistical problems; he analyzed situations very well, and encouraged the participation of all the technicians in the discussions. He also was a strong backer of SER, which helped immeasurably in the integration process.

For the discussions, SER presented written reports on all the farm record data, except for those few crops where harvest was not completed, and for those we had partial reports showing inputs used and labor for the majority of the practices. For the first time, therefore, the technicians began using the farm record data in the review and planning process. This helped a great deal when it came time to design work for 1979. In most regions there was little or no resistance to having each technician (not just those associated with SER) work with up to 10 farmers on farm records in 1979. In turn, each of the technicians of SER was assigned some work in farm trials and/or farmers’ tests. *We were hoping to have from 480 to 500 farmers keeping records nationally in 1979* and involving from 1000 to 1500 different crops and/or crop associations.

In the presentations, in almost all cases where it was appropriate, the technicians were beginning to use economic analyses as well as agronomic analyses. In a few cases there were some errors, but they were the kind that would be easy to clear up. Also, in most regions the technicians of the Technology Testing Teams were using a risk analysis developed in SER that had received wide acceptance among them. It was a simple technique that could be calculated in the field using the farm trial and/or farmers’ test data as well as farm record data.

Another interesting and satisfying, for me, development was that in most regions and cases where the farmers used crop associations, these were being incorporated in the trials. *Even*

*the plant breeding programs were beginning to use associations* in some of their most advanced trials and not waiting until the materials had reached the technology testing teams to do it.

In February we did a new Sondeo in Region VI for a new area they were thinking of opening up during the year. Only Carlos Reiche from SER was able to participate for the full week, but several of the technicians who participated had worked in two other Sondeos the year before. This proved to be a good opportunity to test the *transferability of the methodology* because the Sondeo was very well done.

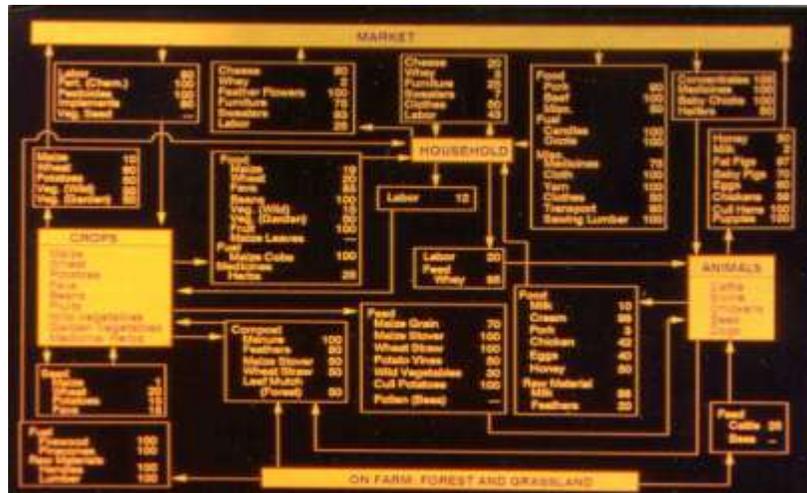
The report on the Sondeo in Santiago Sacatepéquez that had been conducted in January was revised and Maria presented it at the PCCMCA meetings in Tegucigalpa. There was a great deal of interest in the methodology as it is very fast and efficient. <http://ufdc.ufl.edu/IR00000270/00001/1j> Leonel Ortíz, of SER in Jutiapa (Region VI) also went to the meetings and presented a paper on the Farmers' Tests he conducted with rice using farm records as additional information. Participation of ICTA technicians at the PCCMCA meetings was based on both the quality of the papers written by the potential presenters and the quality of their presentations in the regional planning meetings.

In February 1979, I returned to Cornell to work with Bob McDowell on the report of the Bellagio conference from the previous October. We had either been selected or volunteered (I forget which) to put the report together. During the conference, we had been toying with the idea of using flow charts to help explain the complexity of different crop/livestock systems and show the differences among them. During the week in Cornell, we were able to design one that was fairly standardized and we presented it to a Cornell group that was working on a book with Bill Whyte. They thought it was very useful and Bill thought he would reorganize the book and present some of our example charts to illustrate the complexity of interactions on mixed farms.<sup>5</sup> It was obvious to me during

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5. McDowell R.E. and P.E. Hildebrand. 1980. Integrated crop and animal production: Making the most of resources available to small farms in developing countries. A Bellagio Conference. The Rockefeller Foundation. New York.

the week that the reason early work in farming systems began with just crops was that this is relatively simple as compared to working with livestock. For example, we found in our work in Guatemala that it is possible to study and work with crops independently of animals, but as soon as one begins to be concerned with the animal contingent, it is necessary to consider the whole farm. Bob McDowell told me sometime later that the publication we wrote that week was one of the most popular he had. It is interesting that the same, or very similar figures are still being used in a number of publications.



An example of the flow chart for farms in the Guatemalan Altiplano near Quezaltenango

In March 1979, I went to a meeting at CIAT, in Cali, Colombia, on “pre-release” technology testing. Based on a point I raised, there was confusion at first regarding whether we were talking about the release of technology by the international centers to the national institutes, or by national institutes to farmers. I think this reflected the lack of differentiation of the two processes by many scientists at the international centers. After some interesting discussion, it was decided that we should concentrate on the release to farmers by national institutes. Although the process that we agreed upon was similar to that we used in ICTA, there was much more emphasis placed on macroeconomic considerations than is done by a national

institute. Also, *there was no agreement that farmers should have an active role in evaluation by the use of the “index of acceptability” like we did in ICTA. Productivity (per hectare yield) and profitability (as determined by their economists) were **the only criteria** retained for evaluation.* Thus, here again, it was the researchers who would decide what was a “good” technology not the farmers. Those of us who were concerned with this omission were asked to write letters to Howard Stepler who I believe was the Chair of the Board of CIAT at the time.

On my return to Guatemala, I spent a day in Panamá where AID and IDIAP asked me to spend two weeks in April to help them put together a new AID project paper for IDIAP. I spent time with Jack Traywick of IADS and Harlan Davis, the new Rural Development Officer for AID, as well as several IDIAP people.

INCAP requested Rolando Duarte’s services for up to three months to conduct a survey for them in Honduras. They were contemplating setting up a plant to produce *Maisoya*, a Honduran version of *Incaparina*, through some farm cooperatives and needed an anthropologist to evaluate the feasibility and interest in such a project. They said that ICTA was the only organization in the region that they could turn to for such help!

Bill Shaner (Utah State), Bill Schmehl (Colorado State) and Perry Philipp (University of Hawaii), representing the Consortium for International Development of several western universities, visited ICTA in February and spent several hours discussing our program with me. This was the third contact I had had with them. Bill Whyte (from Cornell) and his wife Kathleen also visited ICTA in February. We arranged a trip for them to San Martín Jilotepeque.

From April 15 to 24 I was in Panamá working on the IDIAP/USAID project paper from which it was proposed that AID would loan and grant \$6 or \$7 million to Panamá for IDIAP. Also working on the paper were David Weisenborn who had been with AID in El Salvador when I was there, and Milt Morris from the University of Florida. The nature of the methodology that IDIAP proposed to use was similar to that

being used at ICTA with the necessary modifications to adapt it to the conditions and institutions in Panamá. I also spent one day in New York at an IADS conference on “Defined Area Projects” that elaborated ideas for a Bellagio conference to be held in October.

We conducted three more sondeos during the second quarter. The first was in a potato producing area near Quezaltenango, the second was the completion of the work initiated the year before in the Zacapa irrigation area, and the third was in an apple producing region at Chichicastenango. *By this time the concept of the Sondeo (with no plans for a longer survey to follow) was becoming very popular in ICTA and there was a great demand for its use.*

Rolando Duarte completed three months working in Honduras for INCAP during which time he earned a salary of \$1000 per month. After returning he resigned from ICTA to take a permanent position at INCAP at a salary of \$1200 per month. We had been paying him \$450 at the government rate because they had not yet recognized his M.A. degree (it was not an M.S.) from Wayne State University!

During the second quarter of 1979 it was clear that I would be leaving ICTA, probably before September 1. The Rockefeller Foundation had told me in the beginning that I could plan on being in Guatemala until I no longer felt I was doing any good or until ICTA indicated they no longer needed or wanted my services. It was quite clear by this time that the new Gerente was uncomfortable with expatriates in the institute and he started proceedings to terminate our services as soon as he began working. My visa expired August 31 and he indicated he did not intend to renew it. I did not view this as a major disaster because part of my task was to work myself out of a job and I had been doing this all along. Carlos Reiche had been the acting Coordinator of SER during my absences and vacations (except once when Maria Chinchilla was) and was in line to become the Coordinator upon my departure. The methodologies were pretty well firmed up and operating. The budget was functioning. The big danger I saw was that there would be less (and less continuous) pressure on the Institute to

maintain the integration of the social scientists. It would be more difficult for a Guatemalan social scientist (with only a Master's degree) to maintain pressure on the administration, all of whom were agronomists, than for an expatriate PhD.

The following statement was written by Ing. Astolfo Fumagalli, former Gerente and Sub-Gerente of ICTA and presented at the celebration of the 25<sup>th</sup> anniversary of the Institute in May, 1998, in Guatemala. Obviously I am very proud of the achievements mentioned by Ing. Fumagalli, but he, the former Gerente, Ing. Mario Martínez and all the other Guatemalans and expatriates who worked for ICTA during the years 1974-1979 also contributed as a team to those achievements.

#### **STATEMENT OF ACHIEVEMENT**

Al organizarse el Instituto de Ciencia y Tecnología Agrícolas -ICTA- en los años 1974-1975, se hizo bajo la filosofía de trabajar en función de las necesidades, circunstancias y posibilidades de los pequeños y medianos agricultores de Guatemala, los principales productores de alimentos del país. Con el concurso de científicos nacionales y extranjeros se logró desarrollar un instituto nacional de investigación agrícola que fue el primero que integró a las ciencias sociales con las agrobiológicas, para generar, validar y transferir tecnologías bajo las condiciones de los productores, en donde estos de ser meros espectadores, pasaron a ser participantes en el diseño, conducción y evaluación del proceso de investigación-transferencia. Como consecuencia, el ICTA también fue el primero en desarrollar y aplicar, de manera institucionalizada, el enfoque de sistemas de producción y de investigación en finca. Es decir, este enfoque no era un proyecto entre varios, todo el instituto y todos los investigadores del mismo se integraron a trabajar bajo dicho enfoque.

En esos años iniciales se decidió por donde se quería caminar, pero no se sabía como hacerlo, puesto que el enfoque tradicional de investigación, el cual se había caracterizado por ser prescriptivo, centralizador, tecnocrático y estructurado con una visión de oferta de servicios, no aportaba experiencias para trabajar dentro de un nuevo paradigma a desarrollar en función de la demanda y la realidad socioeconómica y productiva de los campesinos. Se intuía que en todo

esto era esencial la contribución de las ciencias sociales y de científicos de este campo, pero no se podía precisar en que y en donde. Se sabía también que la mayoría de las tecnologías producidas bajo el enfoque tradicional no eran adoptadas ni adaptadas por los productores de alimentos, pero no se sabía por qué.

Con el transcurrir del tiempo se fueron encontrando las respuestas, cada vez mas claras y se fue consolidando un sistema tecnológico coherente y bien estructurado, el cual tenía como punto de partida y punto final el productor y su familia. Para lograr esto se necesitó del concurso y aporte de científicos destacados, uno de ellos y el de mayor mérito en la parte conceptual y metodológica del nuevo enfoque fue el Dr. Peter Hildebrand.

La primera enseñanza concreta del Dr. Hildebrand, fue que las tecnologías producidas bajo el enfoque tradicional no habían sido diseñadas para las circunstancias de los pequeños y medianos productores, por lo tanto la no adopción o rechazo a las mismas no era falla del sistema de extensión, es decir no era problema de venta, simplemente no se adaptaban a las condiciones de los productores. Ante este hecho, la decisión inequívoca fue la de diseñar tecnologías que si fueran adaptadas a esas condiciones. Esto permitió diseñar los pasos metodológicos que se debían seguir para lograr tal propósito, así nació el sistema FSR/E del ICTA, más conocido como "radio de transistores", adoptado y adaptado posteriormente en sus principios, por muchos otros programas nacionales y proyectos de investigación-transferencia.

El primer paso del FSR/E es el diagnóstico, el cual tiene que basarse tanto en las variables de producción, como las socioeconómicas del productor clientela. En otras palabras, en primera instancia se debe conocer que se produce, como lo hace el productor, que resultados obtiene, que problemas y restricciones enfrenta y lo que es más importante, el porqué de cada uno de esos aspectos. Obviamente, para realizar este tipo de diagnóstico se necesita del concurso de científicos tanto del área socioeconómica, como de las ciencias agropecuarias. Por una serie de razones, el método de encuesta tradicional no permite hacer esta integración y para poder conformar un equipo multidisciplinario se tuvo que inventar una nueva forma de encuesta, allí nació el "SONDEO". Según las evidencias documentales, este fue el primer método de diagnóstico de lo que hoy día se conoce y utiliza mundialmente como "rapid rural appraisal field techniques" creado, desarrollado y difundido por el Dr. Peter Hildebrand y su equipo de trabajo.

El Dr. Hildebrand logró integrar un grupo de socioeconomía, el cual dentro de un programa nacional de investigación agrícola, por primera vez realizaba tareas distintas a lo que sus colegas habían hecho tradicionalmente. Este equipo, aparte de liderar el diagnóstico, lo complementaba con otro método de recopilación de información, "LOS REGISTROS ECONOMICOS DE FINCA", que es la parte dinámica del diagnóstico y que permite dar seguimiento a familias y fincas representativas de cada "dominio de recomendación", nombre acuñado por el CIMMYT, pero concepto desarrollado por el Dr. Hildebrand y su equipo, mismo que adaptó los registros económicos de finca a las necesidades del enfoque de trabajo del ICTA. Otra característica de estos registros, es que fueron diseñados de tal manera que permite que sean conducidos por los propios productores colaboradores.

Con el procedimiento desarrollado, no solo se logra un diagnóstico de la situación socioeconómica y productiva, se logra algo aún igualmente importante, obtener buena parte del conocimiento local y de la tecnología que para el caso de Guatemala, por muchos siglos han estado desarrollando los campesinos Mayas. Sin embargo, contar con toda la información necesaria para diseñar la tecnología mejorada apropiada a las condiciones de los productores no es suficiente, los diseños estadísticos para realizar investigación en estación experimental o en laboratorios, no se adaptaban para las condiciones heterogéneas de las fincas de productores de recursos limitados, hubo que modificar dichos diseños, esta fue otra contribución concreta del Dr. Hildebrand, al igual que el desarrollo de programas sencillos de análisis, adaptados a pequeñas calculadoras programables de bolsillo, a manera de que dichos diseños los pudieran hacer los mismos investigadores en el campo.

A lo largo de la aplicación del proceso metodológico desarrollado por varios científicos e integrado por el Dr. Hildebrand, otras contribuciones concretas de este científico fueron: El análisis de estabilidad modificado, el diseño del método de validación de la tecnología para ser realizada por los propios productores; la metodología para documentar la evaluación de la validación realizada por los productores; el índice de aceptabilidad para medir el nivel o grado de aceptación o adopción de la tecnología por parte de los productores que la validaron; el concepto de dominio espacial de difusión; sistemas mejorados de cultivos múltiples; y conceptos claves para sistematizar e impulsar el sistema de transferencia de tecnología de campesino a campesino. Previo al empleo de la metodología formulada por estas técnicas la parcela demostrativa servía para enseñar al agricultor nuevas innovaciones tecnológicas, esta parcela era diseñada y

sembrada en el campo por el técnico de investigación o extensión, quien como dijera otro científico del ICTA de esa época: “la parcela demostrativa es una parcela diseñada para no fallar ante los ojos del agricultor”, fue entonces cuando el concepto de parcela de prueba se incorporó al sistema FSR/E por el Dr. Hildebrand. La parcela de Prueba es sembrada, manejada y experimentada con la nueva tecnología por el mismo agricultor, quien al final acepta o rechaza la tecnología propuesta. Esta innovación ha sido incorporada a muchos programas de investigación agrícolas como una herramienta útil para aceptar o rechazar nuevas tecnologías.

Sin estas contribuciones aportadas por el Dr. Peter Hildebrand, no hubiese sido posible para el ICTA desarrollar su sistema tecnológico de la manera que se logró, y sin este sistema no hubiese sido posible diseñar, generar, adaptar, validar y transferir tecnología apropiada a las condiciones de los pequeños y medianos productores. Como un resultado positivo las condiciones de productividad y producción de los principales alimentos básicos cambiaron significativamente a nivel nacional, como un efecto de la aplicación del FSR/E.

Como un ejemplo: el principal alimento de los guatemaltecos es el maíz, con un consumo *per cápita* anual cercano a las 200 libras, aunque en el área rural esta cantidad por lo menos se duplica. Según datos oficiales, en 1967 el rendimiento promedio de maíz a nivel nacional era de 1.1 toneladas métricas por hectárea, 30 años después, en 1997 dicho rendimiento promedio nacional se había elevado de manera consistente, a 2.0 toneladas métricas por hectárea, es decir, un incremento de 82% debido al cambio tecnológico.

De acuerdo a un estudio reciente del ICTA, la rentabilidad marginal derivada del incremento de productividad solamente en maíz, ha generado un valor mayor al monto gastado por este instituto en todos sus programas de investigación durante sus 25 años de vida. En otras palabras, solamente la rentabilidad obtenida en ese grano básico como consecuencia de la aplicación de tecnología mejorada, paga toda la inversión que el Estado de Guatemala ha realizado desde 1974 a la fecha.

### **STATEMENT OF IMPACT**

El impacto que el FR/E ha tenido en los sistemas de investigación agrícola en el mundo y como resultado en la disponibilidad de alimentos, es muy difícil de medir cuantitativamente. La contribución del Dr. Peter Hildebrand ha sido en aspectos conceptuales y metodológicos, mismos que han permitido a los científicos agrícolas

ampliar su visión respecto de su trabajo, en el cual el productor y su familia son ahora partícipes activos del proceso de innovación o adaptación tecnológica, aspecto central que ha permitido que la nueva tecnología sea apropiada y por ende adoptada y utilizada por dichos productores. Como efecto de esto, la productividad se ha incrementado y como resultado existe una mayor disponibilidad de alimentos, en los casos en donde el enfoque FSR/E ha sido aplicado de manera institucionalizada.

Para el caso de Guatemala, los productores de alimentos básicos constituyen alrededor del 60% de las familias del país (aproximadamente 7,200,000 personas). El ICTA como la entidad nacional de investigación agrícola trabaja a nivel nacional aplicando el enfoque FSR/E, por lo tanto el beneficio de su trabajo, con el apoyo de muchos otros organismos no gubernamentales (ONGs)- y la industria semillera, alcanza prácticamente a la gran mayoría de ese porcentaje de productores.

La filosofía participativa, aspecto central del enfoque FSR/E, ha trascendido la frontera de la investigación/extensión agrícola. Hoy día muchos de los programas y proyectos de desarrollo a nivel del mundo han incorporado este principio filosófico como un aspecto esencial y aún más allá, las políticas nacionales de muchos países impulsan la descentralización de servicios, de recursos y de toma de decisiones, bajo el mismo esquema. Algo que en la actualidad se busca con mucho empeño como base para el desarrollo es el llamado "EMPOWERMENT", mismo que tiene también sus raíces en el FSR/E.

En la última reunión de The Global Association for Farming Systems Research (AFSRE), efectuada recientemente en Pretoria, South Africa, a la que asistieron más de 550 delegados de todo el mundo, se reafirmó la utilidad científica y pragmática del sistema de FSR/E, del cual el Dr. Hildebrand ha sido un importante propulsor. En esta reunión se ha sugerido que la próxima reunión, a realizarse en el año 2000, sea en Guatemala, como reconocimiento al país en donde en buena medida se originó el enfoque y en donde el trabajo del Dr. Hildebrand fue un factor de crucial importancia y decisivo.

Statement presented at the 25th anniversary celebration of ICTA in Guatemala in May, 1998.