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FARM DEBT

From: Harold F. Breimyer

University of Missouri-Columbia

Re: Hanson's "Beyond the Farm Debt Crisis"

My comment on Hanson's generally good piece is brief. In ascribing origins to the farm decapitalization debacle of the 1980s Hanson over-credits farmers, ranchers, and farm lenders' boom mentality of the 1970s, while scarcely mentioning the devastating turnabout in Federal Reserve monetary policy. No sector, and in fact no economy, should be subjected to an arbitrary monetary-policy reversal from a negative real interest rate to a positive one of 15 percent. Yet that was the U.S. record between the mid-1970s and early 1980s. The Fed's action far overshadows the effect of the federal budget deficit of the 1980s, cited by Hanson.

Secondly, why does no one mention an incredible default, during the 1970s, in accounting practices of both farmers and their lenders? It was a failure to show on balance sheets the cumulative capital gains tax obligation. Lenders ought never to have worked from farm owners' inflated asset values uncorrected for accrued tax.

Incidentally, I claim to be one of the few extension economists who published warnings, in the later 1970s, against playing the asset-appreciation game in buying farmland. I know of two farmers who listened. Only two.

From: Neil E. Harl

Iowa State University

Re: Hanson's "Beyond the Farm Debt Crisis"

The article by Greg Hanson is exceptionally well done, balanced, perceptive and, in my opinion, generally on target. Dr. Hanson was toiling for much of the 1980s in a sometimes unfriendly economic vineyard as a USDA economist. He managed to keep mostly in check the understandable urge to kick the intellectual shins of those who were critical of USDA (myself included) during the 1980s. I especially appreciate his "key management guidelines" for the 1990s and beyond. Now that the farm debt crisis of the 1980s is substantially behind us, there is an obligation to leave well-etched guideposts for whatever generation must deal with the next period of great economic trauma in agriculture.

Other than to commend Hanson for the article, my reason for taking pen in hand is to provide a brief comment on his statements about Washington's role in the crisis. For those wishing to engage in some heavyweight Washington bashing, I would suggest Chapter 6 of my recent book, *The Farm Debt Crisis of the 1980s* (Iowa State University Press, 1990). That chapter is entitled "Indifference in Washington" which, I believe sums up the situation at least in the early years of the farm debt crisis.

Unfortunately, Dr. Hanson leaves the reader with little guidance as to the anatomy of government's role in the crisis. Somewhat charitably, perhaps, he offers up civil servants in USDA as the "sacrificial lamb" to those who urged government to do more; at the same time, with his disarming candor, he blunts the wrath of critics as to the rightful targets.

As one who was fairly close to the heat during that era, I can say that, with a few exceptions, I did not hear critical comments about civil servants in USDA *causing the problem*. That does not mean to say that government did not contribute substantially to the problem. Indeed, as I explain in greater detail in my book, in my view the three principal factors contributing to the farm debt crisis of the 1980s all arose from fiscal and monetary policies. Several agencies of government contributed to those policies but foremost were the Congress, the Office of the President, the Office of Management and Budget, the Department of the Treasury and the Federal Reserve Board. Those agencies shaped policies leading to (1) high rates of inflation in the economy which had profound effects on decision-makers, (2) high real interest rates as the Federal Reserve moved abruptly to deal with associated economic woes in 1979, and (3) the tax cuts of 1981 which arguably contributed to the federal budget deficit and served to keep interest rates at a higher level than would have been the case otherwise.

A major response by government to the farm debt crisis of the 1980s was to pour federal funds into the sector in a manner calculated to raise all boats, not merely those in financial difficulty. Indeed, I made several trips to Washington in 1984 and 1985 before concluding, reluctantly, that it was politically unacceptable to provide benefits targeted on the basis of financial difficulty. The eventual response in requiring debt restructuring throughout the Farm Credit System and in FmHA was, in my view, more rational but less acceptable politically.

The area where USDA civil servants perhaps merited critical comment was in the slowness of the response to the problem and, in certain celebrated instances, in opposing federal intervention efforts. These aspects are discussed in some detail in my Chapters 6 and 9.

Again, I commend Hanson for a timely article.

From: Greg Hanson

Penn State University

Re: The Author Responds

Breimyer and Harl's points are well taken. A specific mention of the Federal Reserve Board change in policy was warranted. Also, if the article seemed to over-credit the farmer and lender boom mentality, that was not my intent. The three causal factors Harl cites: high inflation rates, high real interest rates, and the deficit provoking 1981 tax cut affected all sectors and not just farmers. But farmers are much more export sensitive, and were harder hit by the combination of the three problems.

The first page of the article stressed the critical role of government policies in general, and that of high real interest rates in particular. Two quotes: "The prevalent economic model is that mammoth federal budget deficit raised real interest rates...and farm exports fell." "...forget the farm subsidies; they don't make up for Washington's structural damage to my ability to export corn and pay even higher real interest rates."

I did pinpoint the Federal budget deficit as the chief cause of high interest rates, underlying the interest rate policy reversal of the Federal Reserve Board. I viewed the shift in FRB interest rate policy as (can I say) quasi-endogenous. The Fed's hands were forced by larger and larger federal deficits and related effects such as inflation—and the deficits became far larger under Reagan.

Also, the shift in the Fed policy shot through the entire economy. But, most sectors of the economy survived this policy change quite well, as evidenced by the record-setting 8-year length of the recovery in the 1980s. Thus, the change in interest rate policy was too general and too early to “over-shadow” other causes of \$20 billion in farm loan losses during 1984-89.

We need to look at an array of causes for the agricultural crisis: high real interest rates, high exchange rates that choked off exports, the recognition that high Farm Bill supports were pricing us out of world markets, unusually good growing conditions for 1981-86 (with the exception of 1983) that led to excess stocks, a \$50 billion surge in farm expenses during 1977-81, and also, the residue of boom mentality farming and agricultural banking.

On Breimyer's last point, it seems to me that the accrued capital gains tax did not directly pertain to a period of declining land prices in the mid-1980s. The point is more valid now in the 1987-91 period and for the future.

When I mentally pigeon-hole the government role in the crisis, federal fiscal problems are immediately related to the 35 percent drop in total farm exports, lost overseas markets and plummeting commodity prices in the mid-1980s. The stark contrast between market loss in the mid-1980s, and 1989-1990 export toughness in USDA, aided and abetted by better exchange rates and a market-oriented farm bill, illustrate how “Washington fundamentals” reverberate through the farm sector.

As I suggested in my article, we also overlooked some key income/finance relationships that were fast slipping out of balance. I recall standing in a farmer's cornfield in 1975, thinking that \$3.00 per bushel corn made that ground a bargain. As a graduate student I knew enough about the income/capitalization equation to make me dangerous. Dangerous even though I was both rational and right (at least for a few years).

A federal program targeting money only to farmers financially on the ropes was not “the solution.” Two reasons. First, it would have encouraged and rewarded a “boomer philosophy” promoting high cost long-run production. Second, farm program payments “scattered-gunned” throughout the farm economy helped stabilize land values—thus, keeping the healthy in business, and minimizing or lessening, further land value losses among stressed farmers. In addition, targeting would not have been acceptable in farm communities, not just in Washington.

Let me direct another thought into this exchange. It seems to me we need to think about long-run commitment to farm and agribusiness ownership that is truly long run. That is, formulate decisions over a planning horizon that spans 20 or 30 years, or more. Then when the “winds shift” again, we will bend rather than crack. Many European entrepreneurs and family businesses have taken a super-long-run perspective, and have stayed in business by doing so. Of course this is the opposite of the classic boom-bust mentality.

A final comment. Harl is probably correct about Washington responding more slowly to the early farm crisis trauma that engulfed particularly Iowa, Minnesota, Missouri, and Nebraska. He, and others, including Robert Jolly (also at Iowa State) and Abner Womack (University of Missouri), are to be especially credited for their aggressiveness in pointing out the severity of the crisis to those of us in Washington. My analysis became sharper because of their influence. Open communication and the

healthy exchange of views are essential. If we can relearn this key lesson, hopefully there will be better and more widely shared economic intelligence, and less need for “Washington Bashing”, or at least USDA bashing, in the next farm crisis.

A Profile

From: Harold F. Breimyer

University of Missouri-Columbia

Re: Kish's "Profile of Rensis Likert"

Leslie Kish's tribute to Rensis Likert and his commentary on Likert's contribution, during his USDA years, to the process of making farm policy is superb. Commendation is due not only Dr. Kish but *CHOICES* also for inviting and publishing it. Similar profiles could well grace the pages of *CHOICES*.

I want only to elaborate on the beginning of USDA's field surveys of farmers' opinions. Likert was not their originator. Nor did Secretary Henry A. Wallace initiate them, although he was strongly supportive. The project sprang primarily from the fertile mind of Milburn L. Wilson, Under Secretary of Agriculture. It was put in the hands of Chester Ellickson, a rural sociologist who was a protege of “M.L.” The project was modeled after the suddenly popular opinion surveying of George Gallup, who related his techniques and experiences before a Department-wide audience in USDA's Jefferson Auditorium.

Ellickson was not trained in opinion polling and Rensis Likert was invited to take it over. He did so with the skills and insights with which Dr. Kish correctly credits him.

In my memoir I review the pioneer opinion-survey project briefly and recount the incident wherein a Colorado farmer dispatched, with a shot gun, a youthful Harvard-accented interviewer who thereupon returned straightaway to Washington and a new job. Experienced interviewers met little resistance. And dear Gladys Baker, petite new Ph.D. and later to become a distinguished agricultural historian, was mothered so indulgently by her housewife interviewees that she found it impossible to meet her daily quota of interviews.

From: Leslie Kish

University of Michigan

Re: The Author Responds

In my profile of Rensis Likert I restrained from describing the many talented, devoted, and colorful men and women who make Program Surveys possible. But Wilson was often credited by Likert for his vital role in originating the project and also for supporting us in our adversities. We needed his support often in the USDA because Program Surveys was far from an orthodox agricultural and farmers' institution.

We should not argue the historical question about the relative importance of the originator of the idea, Wilson, and the head, the Secretary. That Henry Wallace was an innovator, scientist,

and statistician was both rare and fortunate, probably crucial. Though I omit many names, let me add two (in addition to Angus Campbell and myself): George Katona and Charles Canel, who were key players on the Likert teams in the USDA and also later at the University of Michigan. Much more information can be found in the excellent *Survey Research in the U.S.* by Jean M. Converse, University of California Press, 1987.

...a Japanese Barn

From: Bryant H. Wadsworth

*Minister-Counselor for Agricultural Affairs
American Embassy, Ottawa*

Re: Houck's "Reflections In A Japanese Barn"

I appreciated Jim Houck's Commentary, "Reflections In A Japanese Barn" in the Fourth Quarter 1990 issue of *CHOICES*. Having spent nearly 20 years working on agricultural trade issues with Japan (living in Japan for eight of those years) I have to say that Dr. Houck has done his usual excellent job of making an important point.

What he has illuminated are some facts that we Japanophiles understood quite well (for non-Japanese) and were able thereby to dissuade U.S. policymakers from attacking the rice question head-on for a number of years. But times have changed and it seems to me there is another set of facts that must be given at least equal time.

First of all, throughout its 2,000 year history, Japan has hardly ever been self-sufficient in rice. Famine was almost a way of life until Imperial Japan was able to draw on additional supplies of rice from Taiwan and Korea after the turn of the century and before WW II disruptions brought an end to that system.

In those days the land in Japan was owned by a few elite. Today, after the very effective postwar land reform program, it's owned by several million small farmers. Virtually all of these farmers are members of ZENNO, the all-Japan farmer's cooperative federation. ZENNO is a huge multinational business organization. It owns one of the largest banks in the world and the largest insurance company in the world. A major source of its income comes from the marketing of rice in Japan. By their own published data one can derive that ZENNO's annual gross revenue from marketing rice in Japan adds up to about \$U.S. 17 billion.

The farmers who produce that rice, on average, are earning about 15 percent of their income by farming. The rest is coming from non-farm sources.

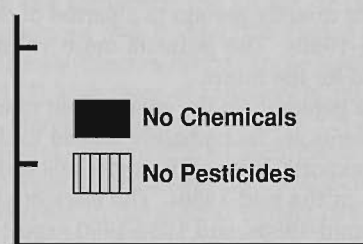
ZENNO, largely through its political lobbying arm, ZENCHU, would have us believe that all Japanese people insist that for "food security" and even cultural or religious reasons Japan must be self-sufficient in rice, and that to reduce the protective and supportive measures to bring that about would devastate the agricultural sector of Japan. Both of these arguments are far from the truth. Most well-informed Japanese people will recognize historic fact that the only times Japan has suffered hunger have been when they were relying solely on their own productive capacity to feed themselves. And most will also acknowledge that a rationalization of their agricultural sector is long overdue,

that far from being left behind, Japanese farmers are much better off today than the average city dweller.

As in most developed countries, Japanese farmers are best off when off-farm employment is readily available at good wages. In the case of Japan, that means world trade needs to be thriving.

The choices then become more clear, viz., continued religious protection of rice for ZENNO's sake, with the heightened risk that brings to the world trading system, or gradual relaxation of barriers and accelerated rationalization of Japan's agricultural sector.

Why, it might be asked, should Japan liberalize rice imports for the benefit of a few California rice farmers? It shouldn't. It should liberalize rice imports for the benefit of Japan as a world economic leader, and for the benefit of the world trading system. Such action will have relatively little economic impact on either potential supplying countries or Japan, but its geo-political benefits to the world community could be of major significance by helping avert solidification of the already well-recognized trends in the world toward renewed protectionism and isolationism.



From: Harry Ayer and Neilson Conklin

University of Arizona

Re: Knutson, Taylor, Penson, and Smith's "Ag Chemicals"

Knutson, Taylor, Penson, and Smith's (KTPS) response to our critique of their study on the Economic Impacts of Reduced Chemical Use (Fourth Quarter 1990 *CHOICES*) falls even shorter of the mark than their original study. After a careful review of their rebuttal, original consulting firm report, survey questionnaire for crop scientists and working paper documenting the Ag-Gem model, we stand by our conclusion that the study's flawed methodology renders it of little value to the policy debate. Although we erred in our presumption that the KTPS study was not reviewed, it is significant that the previously unmentioned reviewers shared our primary concern—the use of the "zero chemical" scenarios. We continue to believe that the study's results are not germane particularly because they are based on unrealistic scenarios of total chemical bans. KTPS contend that the only way to establish the uselessness and irrelevance of a study is to determine "...that the results (the numbers themselves) are somehow in error..." and that this proof "...requires more than an attack on...clearly stated assumptions..." In other words KTPS, schooled in positive economics, subscribe to the theory that the proof of the pudding is in the eating, i.e., if the model predicts observed outcomes, all is well. Unfortunately, in simulating a world without ag chemicals, they have baked what Just, Hueth, and Schmitz refer to as a "welfare cake" which "...is so difficult to taste that its ingredients [the assumptions] must be sampled before the baking..." KTPS seem to have little defense for their study beyond sophistry.

Rather than deal with our genuine concerns about the "...appearance of a conflict of interest..." KTPS have set up a straw man to misrepresent our position as one that "...private

capital corrupts a study..." We never implied "that because private interests are involved, the numbers must be cooked!" What we did say was that "Private funds need not taint research, but time and extreme care are necessary to overcome the appearance of a conflict of interest." It is the lack of time-consuming care, which we detailed, and not necessarily the cooking of numbers which leads to the appearance of a conflict of interest.

We stand by our conclusion that the KTPS study, even though timely, is too flawed to be a useful guide in the debate about the regulation of agricultural chemicals.

Cited: Richard E. Just, Darrell L. Hueth and Andrew Schmitz, *Applied Welfare Economics and Public Policy*. Prentice-Hall, Inc., Englewood Cliffs, 1982, p. 4.

From: Otto Doering

Purdue University

Re: Knutson, Taylor, Penson, and Smith's "Ag Chemicals"

Today, it is critical to recognize that the agricultural community, environmentalists, and the general public have a breathing space. The defeat of Big Green, a recession, and the Mid East crisis have diverted attention from the chemical debate. This gives economists a brief opening to clean up our analytical act and do some public policy education. We must not miss this opportunity. The issue is still there. The fat lady has not yet sung!

With respect to the quantitative modeling issue: people who express concerns about modeling work are not automatically anti-modeling. I tried to make this clear by listing my concerns under the captions "Inadequate Data" and "Inadequate Models". In most of the chemical reduction work I believe the models far outstrip the depth and accuracy of the data. Subjective estimation of extreme events is dicey at best. Some of the models are not as appropriate or adequate as I would like for the task. I support an effort using a GCE model to assess some of the broad economy-wide impacts. I frankly think this is preferable to using models designed for other purposes, like macro/financial analysis. Some of my best friends are modelers. What more can I say, except that these issues urgently need to be widely discussed so that we can do a better job.

The sponsorship issue is real. When a public policy debate is highly polarized, the stakes high, and both sides are running major media campaigns the public has certain understandable preconceptions about the information it receives. Work that is undertaken at the height of the debate is commissioned for the debate by major players from one side, and focuses on maximum economic impact is going to have a perceived credibility problem no matter how arm's length or technically correct it is. It is naive not to recognize this fact of life. A stand-alone consulting activity might have been a more appropriate institutional approach.

In terms of ethical issues, I am most disturbed by the NRDC's and state legislators' reported attempts to stem information from the University of California. I am appalled insofar as this resulted in the University not playing an active public policy education role in the Big Green debate. The University had a responsibility to inform the public about different alternatives and their consequences without taking sides in the debate. The Graduate School of Public Affairs did just that. The NRDC targeted report from Berkeley was largely a compilation of work done earlier on economic aspects of pesticide use. The tragedy is that the extremes in the Big Green debate ground out the middle. The middle now has a brief opening!

From: Knutson, Taylor, Penson, and Smith

Texas A&M University and Auburn University

Re: The Authors Respond

The responses by Ayer-Conklin (AC) and Doering to our study *Economic Impacts of Reduced Chemical Use* (Fourth Quarter 1990 *CHOICES*) contribute precious little to the debate. Ayer-Conklin raised three issues: (1) the model and our reliance on subjective estimates of yield and cost changes, (2) the design of the scenarios studied, and (3) the appearance of conflict. These were the same issues raised in their initial expression of concerns to which we responded. No new information was added in their contemporary response. There has been no response to our request for specifics concerning the model or the estimates of yields and costs associated with the banning of seven different chemical groupings in our study.

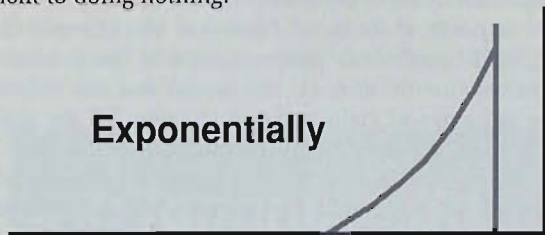
Specifically, AC provides no indication how they would have done things differently. What model would they have specified and employed? What assumptions would they have made in designing their scenarios? If not a ban, what specific percentage cutback would they have chosen. Would they reduce application rates by that percentage, or reduce the number of applications by that percentage? Would they impose a reduction uniformly across all regions, or target the restriction by completely banning chemicals in some regions but allow restricted use in other regions? Would they effect the reduction by adopting regulations or by economic incentives/disincentives? How would they have obtained yield and cost estimates for the scenarios they identified? And what different general conclusions would they have expected to reach than those presented in our writings on this subject? With no new issues having been raised by AC nor positive suggestions for improving the study, we choose to stand by our initial reply.

Doering is more specific and responsive. He now states that he would have preferred a GCE model (we think he means CGE model) to the econometric-based macroeconomic model used in this study. While we agree some CGE models provide greater sectoral detail for the nonfarm business sectors, the AG+GEM model is a general equilibrium model in the true sense of the word in that it does concern itself not only with the real economy but the monetary economy as well; certainly a major linkage between agriculture and the general economy in the 1980s and likely to be again in the 1990s. Furthermore, most CGE models are static rather than dynamic like the AG+GEM model, and we know from Doering's art work in his article appearing in the Third Quarter 1990 *CHOICES* that he prefers dynamic evaluations. Finally, most CGE models are based on subjectively estimated demand and supply elasticities and other parameters. Yet Doering criticizes the KTPS study for using subjectively estimated yield and cost changes. This suggests that subjective estimates made by agricultural economists are somehow better than subjective estimates made by soil and crop scientists. What different general conclusions would Doering have expected to reach than those presented in our writings on this subject?

We share Doering's concern about the actions taken to prevent researchers focusing on the Big Green referendum (which called for a complete ban of specific chemicals under certain conditions) from publishing their results on the effects of chemical use restrictions. They and we, to use Doering's words, have "a responsibility to inform the public about different alternatives and their consequences without taking sides in the debate." That is precisely what we attempted to do. Perhaps what is "correct" depends on what side of the fence you "appear" to be on. We again urge a *careful reading* of the publications listed in our original *CHOICES* article.

Finally, we recognized up front that some would question the objectivity of our study because private funding was involved. We chose to live with it because it made the project possible, we were given a free hand, and to proceed was preferable in our judgment to doing *nothing*.

Exponentially



From: John K. Hosemann

American Farm Bureau Federation

Re: Ayer and Conklin's "Economics of Ag Chemicals"

The recent article by Ayer and Conklin criticizing the KTPS report presents a troubling puzzle for agricultural interests in the private sector. On the one hand there is continuous need for timely unbiased research on emerging policy issues that have potentially dramatic effects on the economics of agriculture in the United States. By their own admission, "The role of the land grant university in public policy debate is to provide the public with the best and most objective assessment of policy alternatives possible." However, when producer groups and concerned agribusiness interests turn to the land grant university to perform that role we are subjected to innuendo regarding the credibility and objectivity of the assessment provided.

On the other hand, Ayer and Conklin recognize an environment of shrinking public research funds. Assuming that there is a need to maintain funding for land grant university research, the puzzle is simply where will the money come from? Little incentive exists for private funding for important policy research by land grant university scientists if it cannot be done without creating the "appearance of a conflict of interest." The very reason for using the land grant system in this project was to let the scientists have a free hand and let the numbers fall where they may. It would have been much easier, believe me, to have contracted with a Beltway bandit to provide preconceived results.

This dispute also raises another issue. Is research done by land grant scientists and funded by government money unbiased? Do researchers that get a LISA grant shade the outcomes to ensure they will get another LISA grant? Reports from California indicate that research outcomes that do not please powerful legislators can lead to threats of reduced funding in future years.

Finally, those of us who sponsored this research are left to wonder that if the findings had verified the conventional wisdom that key farm inputs can be arbitrarily reduced—without scientific footing—without major economic implications, would the critics have complained about the source of the funding and the objectivity of the researchers?

The truth of the matter is that this research raised pertinent questions for those climbing aboard the low-input-sustainable bandwagon. This outcome in itself was well worth all the criticisms encountered. Maybe, just maybe, we can now get about finding low-cost, competitive and *real* solutions at the farm level for environmental problems and give up on looking for grand environmental plans for agriculture.

We continue to believe that the integrity of the scientists involved in this study, and most of those practicing in the nation's land grant university system, is beyond question. It is unfortunate that among their colleagues there are some who do not share this respect.

From: Harry Ayer and Neilson Conklin

University of Arizona

Re: The Authors Respond

In our critique of the KTPS study we noted that "...the conflict of interest issue promises to be increasingly troublesome for agricultural economists..." It is obvious that we struck a raw nerve. We raised the issue not as John Hosemann suggests to question the integrity of the researchers—we did not, nor to subject agribusiness and producer organizations to innuendo—we made a concerted effort to be straightforward, nor because we are on a LISA bandwagon—we are not. We did raise the issue because the appearance of conflict of interest in this case was too obvious to ignore and we believe the issue of increasing concern.

In the very simplest terms, what we did call for was *care, great care*, to avoid even the appearance of conflict of interest when using private funds to research public policy issues in which the donors have a vested interest. We suggested three specific ways to exercise care: the use of *appropriate methods*, especially assumptions; the use of *balanced external* oversight for controversial projects; and the use of *external peer review* before publication. We put the onus for this care squarely on the researchers.

We would also like to suggest that Land Grant university administrators need to be increasingly aware of conflict of interest situations, especially in the current funding environment in which John Hosemann's puzzle "...where will the money come from?" must be answered constantly. Land Grant administrators must also be prepared to wrestle with tricky issues such as the joint research involvement of a university and faculty-owned private consulting firms. And, Land Grant administrators must share the responsibility with researchers for maintaining the balance between the need for timely policy research and the maintenance of research standards.

Undoubtedly Land Grant researchers and administrators will face other conflict of interest issues. And these issues will not involve the private sector alone—we never said or implied that public sector funding was immune from these problems. Our hope is that researchers, administrators, and funding agencies (public as well as private) will remember the traps that the KTPS study could have avoided with some time and care.

EXTENSION'S ROLE

From: L. Tim Wallace

University of California, Berkeley

Re: Hood, Schutjer, Evans, and Boyle's "Cooperative Extension" (Second and Third Quarter 1990 CHOICES)

Over the last decade, I have become increasingly concerned about Cooperative Extension's (CE) anticipated future lifespan. The Hood, Schutjer, Evans, and Boyle articles heightened my fears, and the Nichols and Otte "Letters" relieved me a little. My fears about CE are: (1) there is too much reliance on administrative organization/reorganization; (2) there is too little trust in capable people; (3) relevant program seems to have ceased being the main driving force giving way to institutional concerns for survival; and (4) today's educational community and a complacent society may reject the Land Grant concept before they real-

ize what they're throwing out.

One of my disagreements with the authors is that they appear to rely too heavily on institutional organization to win people's minds when it is really individual people and their teaching styles which count for more in the education process. Too many attempts to cope with University budget restraint seem to have called on reactive reorganization strategies to perceived pressures rather than on strong people to evolve effective programs. Given an opportunity, people working on relevant issues backed by a spirited University faculty might even draw new sources of funding!

The right person given even minimal resources is critical for education effectiveness in almost any program regardless of the number of "opportunity lines" drawn on an organization chart. This is particularly true if CE people have not played a major role in planning the program. It is so easy to plan well for others. In addition, a sensitive CE person will use appropriate education approaches with his/her voluntary constituency, a fact that teachers of mandatory student classes perhaps discount. Finding teachable moments that present problems rather than trying to cure them ex post, developing effective commitment and follow-through within lay groups demands talents which may well not be touched by organization charts.

Another important point, trite yet true, is that many administrations have still not found ways to reward people equally who have equal training and perform equally well but who have different constituencies and different educational responsibilities, i.e., working with on-campus students and CE off-campus constituencies. Changing perspectives on research, outreach importance/relevance/effectiveness, disciplinary influence and "excellence" have created both chasms and turfs. It behooves all of us to overcome these created differences.

Characteristics of successful CE programs across the nation include: a person or team of people with solid content knowledge, an ability to use dynamic group skills effectively, the exercise of educational leadership, and evident administrative backing. The most educationally important components are the people involved and the perceived relevance of the issue, not how they are institutionally organized. The importance of organization is to help people do their jobs well. However, the reality of many organizational shuffles has been to kill leadership, imagination, and creativity by creating uncertainty and frustration by such things as unlimited committee reviews of other committee reviews and a lack of clarity of who is responsible for making

day to day program affecting decisions.

History shows that since CE's inception any effective CE program has had to have a solid research base for relevant outreach. Further, the issue attacked had to obtain audience "buy-in," and that if accumulated research were applied to actual farms and communities in the area, its educational impact would be increased many fold. By simply supporting CE's involvement in applied research for educational programs, most of today's rhetoric about research or issue based programming would be rendered obsolete and irrelevant. Has a new wheel really been discovered?

The foregoing fears lead to my biggest concern which is that there could be so much misunderstanding and misconception about CE's mission and performance, and the need for credible and productive research from the Experiment Station, that the entire Land Grant concept could be passed over. This charge applies directly to all of us in the system: (1) To presidents of universities who, in their constant and understandable concern about funding, have chosen to support campus faculty who want to emulate other institutions of "higher learning" and thereby have inadvertently put CE in a terminal position through such tactics as FTE pooling and drastic travel support reduction; (2) To Cooperative Extension Directors who have been co-opted both by "campus only" needs and program priorities determined primarily by non-CE people with a minimum of CE involvement; And (3) To campus based specialists and field agents who have seen this blanket of benign neglect drop around us all and not called out. We could all collectively be responsible for perhaps the largest educational loss this nation might suffer: figuring the Land Grant concept was not applicable or useful to our contemporary rural, urban and foreign publics.

Reorganization, administrative memos and directives, and directed integration are not the best ways to pursue the Land Grant concept. People are the vital link to its success. All the other things mentioned seem only trying to make a new footprint in the snow before it melts. If CE is to be here 20 years from now playing an effective leadership role in the education of our general public, it will take individual backbone, commitment of and by perceived leaders, and the understanding and action of those responsible for funding allocations. Without that collective coalition of people, I believe public support for CE will turn elsewhere. I would like to be convinced otherwise and proven wrong before it is too late.

Continued on page 44

NEED TO SEARCH THE WORLD OVER ?



Continued from page 43

From: Patrick G. Boyle

Chancellor, University of Wisconsin-Extension

Re: An Author Responds

I completely agree with Mr. Wallace's comments that capable and creative people and relevant programs are the most important components of any Extension structure.

However, I strongly disagree with his comments that institutional structures and concerns for survival have replaced these two primary components. In fact, I believe that the entire Extension system has, in the past several years, placed a much stronger emphasis on relevant programs to address the highest priority issues and concerns facing people and society.

Our article tried to point out how the changes in Wisconsin's Extension structure were working to address the very points Mr. Wallace brought up in his letter:

- Closer linkages and collaboration among creative extension faculty and their colleagues in research and in all the disciplines that can contribute to important program initiatives.
- Full integration of the extension function into the mission of *all* University disciplines so that the greatest variety and mix of talents and ideas can be brought to bear on increasingly complex societal needs.
- A much stronger focus on issues affecting people and communities to drive the program, rather than the traditional discipline base.

As one example, the new structure has allowed us to bring together faculty in engineering, agricultural engineering, law, chemistry, and economics to begin a strong statewide initiative in solid waste management and recycling. This program effort unites faculty with expertise in both theoretical and applied

research with those who have a wealth of experience in working with communities and businesses to implement a program of great importance to Wisconsin people—both urban and rural.

In addition, when it approved the integrated extension structure in Wisconsin, the University of Wisconsin System Board of Regents issued a strong policy statement that the extension function is equal in importance to resident instruction and research. One of the major successful achievements of this structure is the encouragement of equal rewards for those who fulfill this most important mission of the Land Grant university.

**From: Lamartine Hood, Wayne Schutjer
and Donald Evans**

Penn State Cooperative Extension

Re: The Authors Respond

Few would disagree with the points raised by Tim Wallace that people and program matter more than bureaucratic structures, individual reward for performance is critical, and research by extension faculty and staff offers an effective method for enhancing the linkages between extension educators and a relevant analytical base. None of this, however, argues against attempts to create organizational structures and administrative procedures that facilitate scholarship, leadership and the delivery of relevant educational programs by extension faculty and staff. Clearly, the long-term future of cooperative extension as part of the land-grant system will depend upon our ability to "deliver effective educational programs." Our view is that institutional revision will not guarantee effective and efficient educational programming, but we feel certain that institutional paralysis and administrative inflexibility will assure failure. Our future depends upon teamwork and creative internal and external networks. **C**

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