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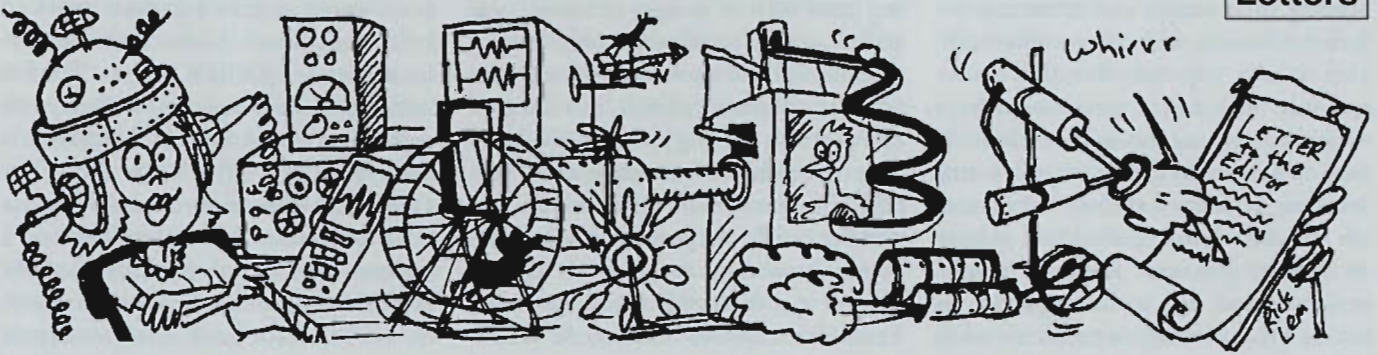
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## Grazing Policy

### Cattlemen's Assoc. Comment

■ I read with disappointment Whittlesey, Huffaker, and Butcher's 1993 Third Quarter edition discussion of grazing policy on public lands and their responses in subsequent issues of *CHOICES*. Their article raises a number of points which need to be addressed. I will comment on just two.

The authors state, "rangeland quality continues to deteriorate despite over a half century of federal effort to protect and rehabilitate public lands." Unfortunately, a statement such as this has been often repeated by many without knowledge of rangelands. The facts in this area do not support the authors. The January 1994 National Academy of Sciences report *Rangelands Health* stated, "...so little is known about the ecological condition of U.S. rangelands that it is extremely difficult to determine how they should be managed."

Whittlesey, Huffaker, and Butcher contend that grazing fees do not recover programmatic costs. Again, the rubber does not meet the road. The grazing program costs listed in the original article mislead the reader. The figures the authors list as "administrative-cost components" are for the BLM and Forest Service total range program (BLM and FS 1992 report to Congress: "Grazing Fee Review and Evaluations—Update of the 1986 Final Report"). The grazing portion of the entire range budget is just that, a portion. BLM and Forest Service's current account management costs for the grazing programs of these two agencies weighted by the number of AUM's each, as reported in the agencies' report to Congress: "Grazing Fee Review and Evaluations—

Update of the 1986 Final Report," demonstrate the cost to the government to be \$1.59/AUM. Too often, in haste, people use the \$3.21/AUM and \$3.24/AUM figures repeated by the authors without investigating them further. Using the same base year as the above report used, the federal grazing fee in 1990 was \$1.81/AUM. Using data from the above-mentioned report, we see the fee pays for all grazing management costs, plus one-third of the capital account range improvement costs which benefit multiple uses such as wildlife and livestock.

We believe if the authors' article has a sound foundation, it should stand on its own merit and not necessitate responses to all those who disagree. The issue itself deserves more.

Ken Davis, President  
Washington Cattlemen's  
Association

## Grazing Policy

### The Authors Respond

■ We appreciate the opportunity to respond to the concerns of the Washington Cattlemen's Association. Regarding the rangeland quality issue, we refer Mr. Davis to our earlier responses in *CHOICES* Fourth Quarter 1993, and First Quarter 1994.

Mr. Davis alleges that we are incorrect in asserting that the governmental cost of providing grazing is more than the fees collected. We stand behind the U.S. General Accounting Office's "Briefing Report to the Chairman, Environment, Energy, and Natural Resources Subcommittee's Committee on Government Operations, House of Representatives; Rangeland Management; Current Formula Keeps Grazing

Fees Low; June 1991" (GAO/RCED-91-185BR). The GAO report states that "[T]he existing grazing fee formula is not generating grazing fee revenues sufficient to cover the amount spent by federal agencies to conduct their current level of grazing program management... [The] livestock management-specific portion of the [BLM's] rangeland management program totaled about \$21 million in fiscal year 1990. Gross grazing fee receipts during the same year were about \$19 million."

Norman K. Whittlesey  
Ray G. Huffaker  
Walter R. Butcher

## Science & Social Advocacy Confront or Avoid the Value Problem?

■ Katherine Reichelderfer Smith's recent article (*CHOICES* First Quarter 1994) serves us well in bringing again to our attention the ages-old conundrum of objectivity versus advocacy in policy research. (It is, of course, only more obvious in overtly policy-oriented studies than in others.) Researchers who hide behind a veil of supposed scientific neutrality with respect to moral issues delude both themselves and others. There is simply no escape from responsibility for the consequences that may accrue as a result of the choices we make in research. These include the hypotheses, implicit or explicit, which we choose to investigate. Alan Randall pointed this out in an *AJAE* article in 1994.

Smith's article alludes to Dan Bromley's earlier complaint (*J. Environ. Stud. and Manage.*, 1990). In effect, he contends that to do policy research



bearing on a simple and arbitrarily selected criterion such as economic efficiency is a cop-out. For the consequences of a policy action are always multiple, not singular, some indirect as well as direct. To define what is worth knowing is surely a valuational process no less than deciding what god to pray to or what political ideology to subscribe to and just as inescapable. To decide what to study is to decide what information *not* to provide as well—which has its own consequences. And, as she points out, just because the research subject-matter fits some archaic definition of what economics is, is no escape at all. So far so good!

But Smith draws the advocacy versus objectivity issue far too sharply. We depend on the scientific community of scholars to evaluate the procedures through which we validate hypotheses, making it as unattractive as possible to 'fudge', and weeding out bad logic and questionable empirical evidence. And we *could* if we *would* install more rigorous critiques of the social merit of hypotheses which are accorded high priority in research agenda. For some hypotheses if researched and published without companion studies of other consequences may prove to be sterile or dangerously misleading to the affected publics.

The "justification" statement protocol in research funding requests was once an attempt to bring that element of critique to bear on the scientific process. But the predominance of methodological positivism in the past half century has virtually mandated that we maintain a sort of studied aloofness from social moral concerns. Assessments of technical merit and whether there would be excessive duplication of other studies suffices in evaluation of proposed research. Avoidance of value judgment biases in hypothesis testing legitimizes a "copout" with respect to decisions about what research is worth doing. Even the Land Grant experiment stations now seldom reject projects because they have no potential, near term or far, to contribute to the solution of important human problems.

What appears to matter to the fund-

ing agencies now is rigor of testing procedure or the spurious prestige which complexity and avoidance of any hint of value judgment affords, not the general problem-solving purpose which defines the public's interest in the scientific community and its activities. No wonder public support for scholarly research is waning, even in the historic bastion of problem-solving research, the Land Grant system. One could plausibly argue that in the methodological orthodoxy, the less relevant the better, because it maximizes the social distance between political and scientific communities. But climbing a research Mount Everest simply "because it is there" has an increasingly hollow ring as a rationale for what we do!

Eldon D. Smith  
University of Kentucky

### Science & Social Advocacy Farm Bureau Comment

■ Katherine Reichelderfer Smith's article, "Science and Social Advocacy: A Dilemma for Policy Analysts," in *CHOICES* First Quarter 1994, made numerous comments about "the accuracy, adequacy, and appropriateness of efforts to assess the economic consequences of wide-scale pesticide use restrictions," regarding the study entitled, "The Economic Impacts of Reduced Pesticide Use on Fruits and Vegetables." This study was conducted by Knutson and Associates (Knutson 2), a group that also conducted a previous study on major crops (Knutson 1). As a senior economist with the American Farm Bureau Federation (AFBF), I was involved in both studies and offer these observations.

Smith suggests that, "The question around which the newest study is framed appears exaggerated if not downright unrealistic," referring to the assumption that pesticide availability would be reduced either 100 or 50 percent. Apparently Smith is not aware of the impact of FIFRA reregistration on pesticide availability or the potential for widespread pesticide cancellation as a result of the Delaney Clause. She also fails to acknowledge that there have

been repeated calls for these types of reductions. The National Resource Defense Council (NRDC) says, "Studies indicate that a 50 percent reduction in pesticide use in American agriculture is feasible..." Public Voice said, "The Clinton administration should exercise bold new leadership by establishing a national goal for substantial reductions in reliance on pesticides and fertilizers to curb health and environmental risk..." Readers may also recall the "Big Green" initiative in California which called for the virtual elimination of pesticides.

Smith goes on to say, "There is ... little or no basis in reality for believing, as assumed by Knutson et al., that biological pesticides, like *Bacillus Thuringiensis*... will be subject to regulation or policy-induced removal from the marketplace." However, at a mid April joint USDA/EPA meeting, several biological pesticide registrants expressed their frustrations about the difficulties biological registrants were having in getting products through the EPA registration process. At that meeting, the EPA argued that all toxins, including biological pesticides, need to be examined on a case-by-case basis. Just because a pesticide is biological does not mean that it is automatically exempt from health and safety tests as implied by Smith.

Smith talked about the study's choice of unlikely policy scenarios that may, as before, generate unwarranted hysteria about the issue of pesticide regulation. "Technical coefficients may again be challenged (as is the case of the 100 percent apple yield loss employed by the study under a 'no pesticide' scenario when, in reality, organic apple markets flourish in states evaluated for apple production impacts)." Smith may not have paid close attention to the study which states, "If no pesticides were used to protect apples in the humid climate of Michigan and the eastern United States, 100 percent of the fruit would likely have substantial damage from disease and insects and be of unacceptable quality to be saleable in the commercially fresh and processing apple markets."



Smith's assertions about organic production aside, the Knutson 2 steering committee was thoroughly convinced by the scientists from Michigan that there would be no marketable production available through normal commercial channels as outlined in the report. Granted that a few apples might be sold in roadside stands, but apples could not travel far without serious quality deterioration. Ironically, "...a 1991 USDA study of fungicide benefits concluded that apple production in the eastern states would not be commercially viable without fungicides because several uncontrolled diseases could each cause yield losses approaching 90 percent" (Kucher and Ralston, *Agricultural Outlook*, 1993).

There was a virtual firestorm centering around the funding of Knutson 1 as reflected in a 1990 *CHOICES* article by Ayer and Conklin which stated, "...the project is flawed by the appearance of a conflict of interest because agriculture and chemical interests provided funds for the project." Smith also stated, "Like the earlier study, major funding came from the AFBF..." I leave it up to readers to judge whether or not the source of funding is, in and of itself, sufficient criteria to invalidate research.

As a result of the concerns expressed about Knutson 1, there were significant changes in Knutson 2. First, the funding was handled via the American Farm Bureau Research Foundation, not the AFBF. Most of the money, 46 percent, came from state and county Farm Bureaus representing donations from producers. Donations from other organizations, 29 percent, were very widespread including individuals, cooperatives, marketing associations, processors, distributors and wholesalers. Funding for Knutson 2 was widely diverse, both in terms of groups and organizations represented (over 100) and geographic distribution. The remaining 25 percent came from the American Farm Bureau Research Foundation. No donations were accepted from chemical manufacturers.

Smith talks rather extensively about the political motivation of the two stud-

ies. "Knutson et al. may also have had a modicum of political impact. The newer study's sponsor must have felt it had political potential as they hosted a congressional briefing on the findings and featured them prominently on page one of the *Farm Bureau News*." This statement is incorrect in that the AFBF hosted the congressional briefing and publishes *Farm Bureau News*, not the American Farm Bureau Research Foundation. I might add that congressional briefings are a routine activity for the AFBF.

I also find Smith's position in this area intriguing in view of the fact that she is currently employed by the Henry A. Wallace Institute for Alternative Agriculture. Consider what was stated in their 1992 annual report: "The Wallace Institute's Achievements in 1992 consist of... It provides leadership and helps coordinate the policy research and analysis necessary to influence agriculture policy in the nation's capitol. The program has been praised for its commitment to objectivity, and its dedication to a collaborative and empowering relationship with scientists, farmers and other sustainable agriculture advocates and analysts throughout the United States."

The central problem with Smith's criticism of Knutson 2 is that it leaves agriculture powerless to promote or defend policy positions. Smith's standard places agriculture and objective researchers into a no-win situation. If a purely scientific and objective research project somehow agrees with a position taken by the sponsor(s), is it automatically tainted or corrupted? Does this standard apply to studies sponsored by the Wallace Institute?

Smith also spends considerable time talking about "the evolution of science as separate from the soul." Her assertion that "environmental and health benefits and costs, income distribution or social justice—(were considered) inconsequential" by our group is totally off base. Simply stated, such issues were beyond the scope and expertise of the group. Her statement seems to infer that micro-issues cannot be examined without a total, all encompassing,

macro-analysis. I believe that economists who work at the micro-level may take a different view.

The criticism directed to the two Knutson studies can be summed up in just a few words: the studies are not considered politically correct in some circles, particularly those traveled by Smith. However, keep in mind that no one had addressed this issue prior to the two Knutson studies. While the AFBF encourages research in all aspects of the pesticide issue, someone had to take the first step and accept both the kudos and the criticism. Now it is time for the critics to step forward, make their assumptions, and devote the time, money, and energy necessary to explore their views. I look forward to reviewing their results.

Terry Francl

American Farm Bureau Federation

## Science & Social Advocacy

### The author responds

■ I am gratified by the discourse my article appears to have stimulated. Bromley (see this issue, pages 31-32) expands upon the limited utility of limited-scope policy analysis in his usual compelling fashion. I couldn't agree more with his observation that analysts who choose to emphasize mainly or only the losses associated with change serve as defenders of the status quo rather than as architects of creative response to changing circumstances. But, we all have different roles to play in the policy analytic arena. And analytically-based defense of the agricultural status quo is a role that is entirely appropriate to those who have or who serve the interests of, for instance, the AFBF.

My response to Francl is that I believe it is entirely possible, and, in fact, can be desirable, to conduct research which both meets rigorous scientific critique and furthers an advocacy position. The problem with the study by Knutson et al. is that it fails to meet either of these criteria very well. Despite the objective rigor of the modeling procedures used to derive results, the technical assumptions used as in-



put to the model have not been subjected to objective peer review, and I continue to be persuaded by my colleagues in agronomy and horticulture that the study's technical relationships could not survive such a process. The study's failure to effectively further the defense of a policy position relates to the unrealistic nature of the questions that framed the analysis. Since only those individuals and groups that buy into the exaggerated and inappropriate interpretation of statements made by environmental and consumer interest groups can accept the premise of the study, then the study's results merely "preach to the choir." The "choir's" position can only be advanced if the underlying analysis is also convincing to individuals and groups who had not already decided that pesticide reduction was a bad idea. It is doubtful that the Knutson et al. studies did that to any great extent, if at all.

One of the points I tried to make in the article is that the Knutson et al. studies could have better furthered an advocate's stance that pesticide reduction was costly, and also withstood the rigors of an actual peer review process, had they been framed and interpreted more creatively. Accurate agronomic and horticultural information might have lowered the price effects generated by the model, but would still have resulted in some commodity price increases which the analysts could have interpreted as large or small, depending upon how they chose to couch them. And Bromley's valid observation (this issue) that the study presumed, unrealistically, no technological response to regulation, could have been avoided by stating that results were applicable only to the short-run, before adjustments take place. Finally, relying on the Natural Resource Defense Council's or Public Voice for Food and Health Policy's actual translation of policy statements into pesticide reduction strategies would have increased rather than diminished the seriousness with which other groups (including policy decision makers) would have considered the study's results. If the Knutson et al. studies' critics have not,

as per Francl's expectations, "step(ped) forward, (made) their assumptions, and devote(d) the time, money, and energy necessary to explore (other) views," then perhaps it is because they do not judge the political or scientific impact of the Knutson et al. studies to be a large enough stimulus for resource expenditure.

Incidentally, I apologize for not having noticed that the second Knutson et al. study was funded by the American Farm Bureau Research Foundation rather than by the AFBF. But I must admit that the distinction does not seem overwhelmingly significant to me.

In a final point of response to Francl, I object to the suggestion that I use political correctness as a basis for judging analytical quality or contribution. If, as Eldon Smith's letter suggests, I went "too far" in drawing some points about the separation of social merit from scientific merit, it may be precisely because I see political correctness as a dangerous criterion for setting research agendas.

I actually agree with everything that Smith articulates in his letter. I will go even further by suggesting that one of the greatest current vulnerabilities of the U.S. public agricultural research system is that it is difficult to see how, if at all, its research agenda-setting process corresponds to social priorities. I do not, however, see this as a problem in the context of the Knutson et al. studies. First, those studies were not conducted (at least not strictly) in the public research sector. Second, they do address an issue, the role of pesticides in the food and fiber system, that is of great social concern. I feel comfortable in assailing those studies' technical assumptions and questioning their policy contributions, but I would be the last to suggest that we institute a research selection process based on the "merit" (judged by whom?) of proposals' positions on social issues as judged from their statements of hypotheses. This would overstep the line between research conducted in the social interest, and the social engineering of research perspectives, a boundary I believe must definitely be protected.

Katherine Reichelderfer Smith  
Henry A. Wallace Institute

## Ag Subsidies U.S. Sugar Producers' Point of View

■ I read with interest the article in the First Quarter 1994 *CHOICES*, "Agricultural Subsidies in Canada, Mexico, and the United States, 1982-91." Unfortunately, because of assumptions made, the article misleads, rather than informs the reader, as it reinforces pernicious myths about farmer subsidies.

Let me address my concerns from the point of view of U.S. sugar producers. Your chart suggests that for the decade covered, some 60 percent of their average income was derived from subsidy. As there is *no* government subsidy—indeed there is an assessment on the industry to help finance federal debt reduction—one must assume the authors refer to a consumer subsidy.

I find much fault with the authors' analysis. The PSE concept assumes that there exists a standard free market price, which, absent government interference, is the price consumers would pay for the product. They use the #11 FOB Caribbean ports price for raw sugar. This is by no means the price consumers pay.

The authors overlook the key fact that the #11 market is a residual market, one into which world producers dump their surplus production, and not a market which determines either producer income or consumer prices around the world. If the relationship between the internal U.S. raw sugar price and the #11 market price is a true measure of the producer's subsidy, how does one explain that the U.S. consumer pays substantially *less* than the world average retail price for sugar, according to USDA surveys?

For the Canadian consumer who has free access to world price sugar, prices are, on average, somewhat lower than U.S. prices. In contrast to U.S. policy, Canadian producers receive direct subsidy payments from their governments for the 10 percent of Canada's consumption needs they produce. How



ever, they sell almost one-third of that sugar to the U.S. market, for which Canadians receive the U.S. price.

No mention is made of Mexican sugar subsidies. Why? Mexico is a major producer; it surpasses the United States in cane sugar production. In USDA's March 1991 *Sugar and Sweetener Situation and Outlook Report*, Peter Buzzanell reports the 1990 Mexican government mandated price at 18.7 cents per pound, FOB mills, for raw sugar. This is slightly higher than U.S. producer support prices, suggesting Mexico's sugar PSE is higher than the United States'.

While comparisons of the relative subsidy levels for agriculture may be of interest, the reader should be alert to the limits of any such analysis. The sugar PSE would have been a lot lower if, instead of the last decade, the period selected were the last 2 decades, which would have included years when world raw sugar prices exceeded U.S. prices, or just the most recent 5 years. Not only the period chosen, but also currency exchange rates have a major impact on the relative subsidy level, if any exists.

Andrew Schmitz, chairman of the Department of Agriculture and Resource Economics at Berkeley, noted in a 1990 study that "it is quite possible that the current internal U.S. sugar prices may prevail under a free trade situation." By Schmitz' measurement, or by a comparison of consumer prices, there is *no* producer subsidy for U.S. sugar.

Eiler C. Ravnholt  
Hawaiian Sugar Planters'  
Association

## GRP—"Sounds Like Socialism"

■ Comments are offered on two articles from *CHOICES* First Quarter 1994.

"The Flood of 1993": Too bad there was not mention of the \$4 billion of crop insurance protection and about \$1.5 billion of indemnities paid in these states. The farmers who chose to in-

sure against such catastrophic events had their protection before the rains started, knew the minimum crop income they would receive and received indemnity checks months before the government handouts. Maybe more farm businesses should consider making crop insurance protection a part of their annual business plans.

"Group Risk Plan": Traditionally, insurance covers individual losses and only pays indemnities when an individual loss occurs. GRP pays only when county yields vary and when they do it pays everyone regardless of whether they suffer loss. Furthermore, individual losses are not covered unless the county goes down. That sounds a lot like socialism to me.

GRP is an idea that has much more appeal inside the "DC Beltway" than in farm country. Insured Kansas farmers responded to a KSU survey some time ago that if GRP was the only crop protection available, only one in five existing crop insurance users would buy it. I have yet to hear of an ag lender who will accept GRP for production loan collateral as they have been doing with MPCl for years. I'm also amazed at how much effort and taxpayer dollars the government commits to try to promote use of flawed ideas.

I wonder how much improved might the MPCl program be and how many more growers would be protected today if all of the efforts and money for GRP had been put behind this individual protection plan that has saved tens of thousands of farmers from financial ruin in the past decade. Come on academia, focus on the real insurance that over 700,000 farmers are currently using in their annual business plans!

Eugene Gantz  
Scenic Valley Drive  
West Des Moines, IA

## Group Risk Plan Alternative The Authors Respond

■ We welcome the opportunity to respond to Mr. Gantz. In our article we pointed to the shortcomings of GRP as well as the strengths. Many futures and options markets have similar char-

acteristics as GRP—users of both must accept basis risk. The crop insurance company where Mr. Gantz serves as a vice-president has many sales activities that are predicated on the complementarity between crop insurance and futures markets. Do they also believe that futures markets are socialist? *CHOICES* readers should also be reminded that the original idea for GRP was developed by Professor Harold Halcrow in his PhD dissertation at the University of Chicago. Milton Friedman, T.W. Schultz, and D. Gale Johnson served on his committee. They liked the concept because it reduces the serious problems associated with individual crop insurance—moral hazard and adverse selection.

GRP represents a choice, not a replacement. We illustrated two very different conditions under which one farmer would likely choose the individual insurance product (APH) and the other would likely choose GRP.

There have been several studies using farm-level data that have proven that GRP is a good product for a large number of farmers. In each of these studies, over half of the farmers would have had lower relative risk with GRP than with APH. In western Kentucky around 5 percent of the soybean acres were insured with individual insurance in 1993—15 percent were insured under GRP where it was available. Some lenders have also recognized that it protects against major losses within a region—that helps their portfolio.

Nearly two-thirds of the U.S. crop acres remain uninsured. Many of these uninsured farmers may find GRP attractive. This seems like an opportunity for expansion of the crop insurance industry. If private insurance companies are concerned about individual producer coverage, they could write a supplemental policy that would pay for losses when GRP does not. This would seem to be particularly attractive since GRP addresses the major problem with private insurance on crops—pervasive and correlated losses.

Alan E. Baquet  
Jerry Skees  
Montana State University