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A CRITICAL APPRAISAL OF AGRICULTURAL ECONOMICS IN THE MID-SIXTIES

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Outstanding Developments

For purpose of today's discussion, I will divide the outstanding contemporary developments of agricultural economics into two broad classes--those that are substantive, that have to do with subject matter, with conceptual content and those that are procedural, that have to do with methods and techniques of analysis and measurement.

Procedural developments -- With increasing tempo since the mid-forties, the mathematico-computer revolution has been reforming our lives. Quite likely, no previous impact on the economic, technological matrix of our living had consequences as sudden and dramatic as this revolution portends. The industrial revolution, the reformation, the discovery of money, of numbers, of written language, of fire have been equally great watersheds of economic history. But though the final impact of each of these and of others has been as great as will be that of the mathematico-computer revolution, in keeping with the tempo of our contemporary culture, no one of these previous great changes occurred with such swiftness and with such dramatic impact. Its consequences are not confined to agricultural economics research nor even to research generally, but enters into every nook and cranny of our daily lives--recall the relation between automation and employment and the rapid increase in leisure time and the problems of fruitful use of that time; and recall the proliferating substitution of numbers for words--I am 516-42-2429 residing at 5-982-85721 whose phone is 602-327-1675. But you are all familiar with these developments.

But the mathematico-computer revolution has its really dramatic consequences in problem solving research clear across the full spectrum of the sciences, pure and applied, not excepting agricultural economics. To one who, as I did, worked out his master's thesis only 35 years ago, using the advanced techniques of that era--multiple curvilinear correlation (by the graphic method)--and a hand-cranked Monroe calculator for all calculation (you can see why that graphic method of correlation had such an appeal to me), the advent of the computer is fantastic and the proliferation of mathematical applications is bewildering.

The revolution's impact arises from the immensely increased powers it gives us in the handling of complex functional relations and the deductive working out of their consequences. The mathematical part of the duo vastly increases our powers of deduction from complex sets of complex premises;

the computer partner makes it possible for us to "operate" this vastly more complex deductive system quantitatively. Thus, we now are able to deduce quantitative assertions concerning complex economic phenomena about which only a few years back we could only speculate in grossly deficient qualitatively expressed verbal statements.

The powers of this tremendous analytical development are felt in both the ever-widening sweep of aggregative analyses of economic phenomena related to agriculture and in the ever widening array of phenomena being brought to bear on the micro-analysis of what Boulding 1/ calls the "individual economic organism" in or related to agriculture--the "firm" and the "consumer" in traditional language.

The other procedural development of note in agricultural economics is the reaching out for more meaningful, yet operational, decision functions to incorporate into these complex analytical systems. Traditionally, man in our models has been described by a decision theory of riskless choices--an "economic man." This economic man has three properties: (a) He is completely informed. (b) He is infinitely sensitive. (c) He is rational. 2/

Since the appearance of von Neumann's and Morganstern's Theory of Games and Economic Behavior twenty years ago and the subsequent developments by Wald, Hurwicz, Savage and others of statistical functions by which the theories of risky choices and of uncertainty adumbrated in that work could be made operational, agricultural economics has plugged away at substituting in its analytical models a somewhat more humanized man for the highly simplified mechanical man of an earlier era.

Though this procedural development in agricultural economics has paralleled in time the mathematico-computer revolution, it has, in fact, been in part facilitated, even stimulated, by the latter. The footprints of a more human man on the findings of our research are far less evident than are the imprints of the mathematico-computer revolution. Man as a warm blooded, emotional, as well as rational, being is still too complex even for the advances of the mathematico-computer age to handle with ease and, more importantly, with science satisfying exactitude. But the steps our discipline has taken toward the humanization of the actor in our models by incorporating statistical decision functions and models of risky choice and of uncertainty must be counted among its real advancements in the mid-sixties.

1/ Boulding, Kenneth E. A Reconstruction of Economics. John Wiley and Sons, New York, 1950. Chapters 2 and 3.

2/ Edwards, Ward. "The Theory of Decision Making," Psychological Bulletin, 51:4. July 1954. Also reprinted in Rubenstein, Albert. H., and Chadwick. J. Haberstroh, Some Theories of Organization. The Dorsey Press and Richard D. Irwin, Homewood, Illinois, 1960, in which it appears as Chapter 33.

Substantive developments -- Let us now turn to consideration of four developments in agricultural economics that have to do with subject matter inclusions, with conceptual content of the field--what I called above, substantive developments.

First, are those additions to the conceptual content of our discipline that have related agriculture to the total economy, that have explored the role of agriculture in economic growth and development, viz., that have been concerned with agriculture in "aggregative dynamics." Not that agricultural economics heretofore had its attention focused only internally on the agricultural sector. Far from it. Concern and consideration for these aggregative, relational aspects of agriculture have always characterized some branches of the field. In fact, agricultural economics probably has moved less far from the traditions of political economy than has its parent discipline, general economics. But with the impact of the agricultural depressions of the twenties and thirties and the agricultural prosperity of the forties and early fifties and the racking strains of agricultural adjustment to the ballooning technologies of the late fifties and sixties, together with the coincidental concern of our national society with economic growth of underdeveloped economies around the globe, the role and relations of agriculture in and to the aggregate society, its strategic role in the early stages of economic growth, its passive, even subservient, role in advanced economies, its relations to the economic fluctuations of advanced capitalistic cultures, its role in international economic diplomacy have altogether conspired to make this area one of the most deeply tilled of all of those newer areas into which our discipline might be divided.

The second substantive area of outstanding growth in agricultural economics is the proliferation, aided and abetted by the computer revolution, of "process" or "systems" analyses of the agricultural sector. Where we used to see studies of needed farming adjustments in dairy farming in eastern Connecticut, or in wheat farming in the triangle area of Montana, for example, we now find studies of the efficient locational pattern of cattle feeding, or of feed grain production, or of beef and pork production, etc., for the whole United States, or of desirable farming adjustments in the whole cotton-producing or wheat-growing economy, for instance, or in the entire milk-producing industry in the North Central Region, or in New England.

Strides have been taken toward working out the impacts on employment in agriculture of the efficient application of new technologies and of the impacts on area and regional economies that will stem from these changes. In this context, too, we find great changes in the studies of markets, marketing, and prices, wherein "markets" are studied as "structures" of functionally related systems and the analysis attempts to unravel the process and to quantify its parameters by which demand of consumers is reflected "backward" to producers and supplies of producers are reflected "forward" to consumers, with what degree of economic efficiency and at what economic cost.

This area is the domain of the aggregative linear programming device, the input-output model, market structure research and similar contemporary developments.

The third substantive development in contemporary agricultural economics is the expansion in the application of welfare criteria and welfare models to the appraisal of public action. This development has proceeded most rapidly and reached by far its highest peak of development in the economic appraisal of public investment in the development or protection of "renewable" or of "destructible" natural resources, more especially of water. This growth sector had its beginnings in the welfare criterion written into federal law pertaining to flood control in 1936 and was extended through a combination of Congressional and Executive demand and of Agency practice to all other recommended programs of investments in water resources. In no other area of economic analysis are the welfare criteria and the welfare models so highly operational and so widely operated--and yet even here their operability leaves much to be desired. Agricultural economists are in the vanguard of those who are working out and applying the tools of welfare analysis to federal investment plans in this problem area.

Though legislative demand for application of these operational criteria and analytical models to other forms of public expenditure do not yet exist, there is growing tendency of the federal executive and the Congress to question proposed expenditures in many other fields that for their answer require the application of the benefit-cost models of welfare criteria. Some applications of the benefit-cost model have been attempted by agricultural economists on the economic impacts of public actions that interfere with the private market generation of and response to price signals--welfare analyses of price supports, of input and/or of output controls, of various measures for demand enhancement for farm products, etc. So far, little or no attempt has been made to apply the welfare criteria and models to such qualitative and subjective problems as expansion of expenditures for education and research and welfare, although a few agricultural economists have been in the front ranks of those who have been exploring the possibilities of quantitatively assessing the economic significance of education, of research and development, and other such tenuous matters.

The fourth substantive development in agricultural economics at the mid-sixties is the serious effort to incorporate dynamic functions into its models.

> It is the essence of dynamics that economic variables at different points of time are functionally related; or, what is the same thing, that there are functional relationships between economic variables and their rates of change, their "velocities," "accelerations," or higher "derivatives of derivatives." It is important to note that each such dynamic system generates its own behavior over time....This feature of self-generating development over time is the crux of every dynamic process. 3/

3/ Samuelson, Paul A. "Dynamic Process Analysis" in A Survey of Contemporary Economics. Edited by Howard S. Ellis. The Blakiston Co., Philadelphia, 1948. Chapter 10, p. 354. (Italics in the original)

The close family relation between this substantive development and the first (agriculture in relation to economic growth) is evident. But, whereas the former is focused on aggregative relations between the aggregate agricultural sector and the aggregate of the economy within which it functions, this fourth or "dynamic function" development, though it may include the first, is much broader, involving dynamic functional processes of the individual economic organisms as well. Attempts have been made to include in agricultural economics research saving and investment functions by individual economic organisms, be they farm or marketing firms, or by classes of firms in aggregative settings; the studies that include functions of technological change, population, and income growth are examples of this development in contemporary agricultural economics.

But, surely, you all must be familiar with all these developments. I feel it would be an insult to your familiarity, as agricultural economists, with your own discipline were I to belabor, further, the description, analysis, or criticism of the details of these developments.

Have These Developments Helped Us?

Of much greater interest and concern to me and I hope to you is to explore whether or not these developments, great as they have been, have brought agricultural economics any closer to what I presume to be its goal--that of being a science and a handmaiden to policy. I intend, therefore, after these few pages in which the current outstanding developments in agricultural economics have been condensed to two procedural and to four substantive classes, to spend the rest of my time on more fundamental, more critical issues. Let us ask ourselves these questions--"Have these developments in agricultural economics, dramatic as they have been, endowed us agricultural economists with greater ability to predict outcomes and consequences in the agricultural economy of the real world either as a positivistic (i.e., explanatory) science, as a normative (i.e., ethical) philosophy, or as an analyst (i.e., an appraiser) of economic policy, public or private? In other words, are we better forecasters of agricultural economic phenomena than we were? Better prescribers of individual or group choices in the interest of maximizing whatever it is the decision-makers want to maximize? Better analysts and critics of private or of public economic policies? Have these developments made us better agricultural economists?"

If the economic question posed for an agricultural economist today is the following--"What behavior should (or ought) a rational actor choose, who possesses 'almost' perfect knowledge and omniscience, in order to maximize a specified 'economic' consequence over time?"--if the question by or addressed to the agricultural economist today resembles that one, he can give a much more complete and a more satisfying answer than he could a decade or two ago. Our discipline has experienced significant developments in the areas implied by this normative question.

Most of the developments in today's agricultural economics described in the first section of this paper, viz., the mathematico-computer

revolution and the inclusion in agricultural economics of process or systems analysis, of agriculture in relation to economic growth, of applications of the welfare criteria to public investment decision-making, and the incorporation of dynamic functions into our models has greatly increased our power to answer the "rational," "economic man", "normative projection" kind of question stated above. This still does not constitute power to predict future states of the real world because the economic actor included in the projective model is not yet a warm blooded man but a lightening calculator of the functional relations operating between himself and his world throughout all space and time in order that he may make the rational maximizing choice relative to a very simple criterion. He is still a far cry from a fully human figure.

However, this is no small accomplishment. We live in an age of rationalism and science. Ability to answer questions with a show (and feeling) of relative certainty and in positive terms receives the accolade in this era. Many of the outstanding developments of agricultural economics over the recent past have greatly increased its projective power albeit still in the context of a highly artificial situation.

But what if the question posed for the agricultural economist today were to be couched in much more positivistic language, do the modern developments help us much? Unfortunately, they do not. Suppose the questions asked of agricultural economists were "What will the real world man do and with what economic consequences in the setting of circumstances that actually obtain or are imminent or that might be created? What economic goals and values are held by real world men, the attainment of which economic analysis can help them reach? By what prescription of economic-socio-political institutional actions can these goals be reached, these values realized and at what cost (or sacrifice) and with what degree of risk and uncertainty?" Or the question might be "Which prescribed program of action from among several alternatives promises the highest net gain in value attainment with due allowances for what uncertainties?"

If these are the questions asked of us today, than I fear all the outstanding developments of agricultural economics at the mid-sixties do not help us very much. We're but little better at answering questions of this sort today than we were a generation or even a century ago. I fear that most of the remarkable current developments in agricultural economics equip us only to be more exact, more positive, more quantitative, more complex in our rationalistic analysis of hypothetical, simplified, imaginary systems from which man as a partly irrational, unpredictable, emotional animal is banished to be replaced by the lightening calculator in human form.

Let us turn then to an exploration of <u>why</u> this blindspot in our discipline persists and, <u>if</u> we can do anything about it, <u>how</u> we can do it.

"Rational" Projections Versus Real World Predictions

The crux of this issue can be expressed in the contrast between "rational" projections on the one hand and real world predictions on the other. Agricultural economics becomes an ever more powerful "projector" of economic outcomes given the determinative premises of the economic man. It becomes little if any better as a predictor of real world outcomes of economic processes. Why does this condition persist? What can agricultural economists do about it?

Rational projections of maxima -- This is an age of rationality and science is its handmaiden. Science is a quest for certainty. As a social phenomenon, science is an attempt by mankind to escape from the terrors of the unknown and the uncontrolled and to substitute the contentment that would accompany his dominance of a subservient nature. Man finds some sectors of the world about him easier to dominate than others; in some sectors the functional relations are sufficiently simple, so dominated by a few relations and so regular and mechanical in their responses that his abilities to predict and to control have developed astoundingly. We only need recall the amazing developments of technology to appreciate the consequences of man's ability to predict or to control in many parts of nature's garden. But, also, we need only to recall the many parts of that same garden in which man has so far not found the bases for prediction and control and, hence, where he lives in an environment of uncertainty and of uncontrol of the unknown. Generally speaking, these still baffling sectors are those wherein the functional relations are so many, so complex in form and interrelation, so tenuous, so difficult to observe and measure as to baffle man's finite and still childlike abilities to analyze, to synthesize, to measure. In this sector we find, unfortunately, our discipline of agricultural economics, but it is not by any means alone in this frustrating predicament. Fellow bedfellows are meteorology, ecological systems such as wildlife, fishery and range management and the control of insect and disease outbreaks, geophysical changes (earthquakes and avalanches, e.g.), not to mention, of course, all the social sciences including our own parent discipline, economics.

But this is not a mystical age; this is an age of rationality. The accolades go to those who can predict and control the world around us. The pressures are on us to be scientists.

> All sciences are...to a certain extent unrealistic...It is not that sciences abstract from certain aspects of reality that is psychologically significant but from what they abstract, what they exclude from their considerations...In the social sciences abstraction serves a two-fold purpose. First it simplifies reality to make it understandable, comprehensible, predictable. Isolating abstraction satisfies the desire for synthesis and avoids the anxiety which stems from the realization that social processes are difficult to comprehend and to predict. Second, by abstracting from disturbing and conflict-creating elements, the social institutions assume the character of a harmonious, rational mechanism...

> Every science has to choose out of the infinity of real phenomena some basic concepts, has to build abstract models by which it measures reality. Scientific thought means to select certain aspects of reality, and this in turn means abstraction and model-building.
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> (However), the question remains unanswered why models are built which contain elements not to be found in reality instead of choosing certain aspects out of the infinite realm of real phenomena...
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> Models of society constructed by economists for scientific purposes...tend to alleviate tensions and to remove sources of anxiety by intellectual imagery. If our economy would conform to the model of the "perfect state," if we had perfect knowledge, and if equilibrium could be attained, many of the uncertainties of our economic and social life would be eliminated. 4/

Endowing the actor in our models with omniscience and with a rationality directed at the maximization of a simple and uncomplicated value; permitting the social environment to enter our models only as endogenous variables often even than held invariant--these are the ways by which we agricultural economists play at being scientists and at being policy advisors. For these simplifying and unreal abstractions clothe us with the mantle of predictive power (though we carefully call it always, "projection") and give us a platform from which we can speak positively, assuredly, grandiloquently.

The divorce of economics from political economy in order that it might aspire to scientism permitted economics to make generalized abstract predictions with an aura of certainty about them (although the certainty was a sham, for these "predictions" were but hypotheses, untested or, if tested, found to be unwarranted or "untrue"). But the multiplicity of functions involved and the complexities of their multitudinous interrelations made even these projections difficult if not impossible in the before-computer (B.C.) era. Mathematical models better to fit these complexities and computers to handle them have permitted us to adduce complex hypothetical projections where not long ago we would have found it impossible. The powerful tools of static and dynamic programming at micro and macro levels, the varieties of systems analyses such as input-output, the use of statistical models to estimate production functions, demand and supply functions, cost and revenue functions, marginal revenue productivities, marginal costs, etc., are all in use but, with rare exceptions, come up with rational projections, not predictions, and usually with the error term in the function so large as to seriously reduce the warrantability of the projection hypothesized.

The weakness in all these tools of analysis, greatly increased in power as they are, is that they rest on the primitive presuppositions of

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4/ Weisskopf, W. A. "Psychological Aspects of Economic Thought" Journal of Political Economy, 57:4,304. August 1949.

perfect knowledge and of rationality, the latter defined as the maximization of an uncomplex value; or they attempt inductive generalizations of real world data so diverse and so variable as to endow the error term in the equations with magnitudes so large as to neutralize the warrantability of the derived predictive equations. In consequence, these tools do not lead to warrantable predictions of real world outcomes deduced from generalizations induced from contemporary or past events; they do not lead to prescriptions for individual or social policy that will ensure real world goal attainment. Nevertheless, we all use these analytical tools. When pressed to justify doing so, we usually fall back on the apologetic that the primitive presuppositions of omniscience and rationality represent the "central tendency" - the "natural norm" - toward which all individual economic decision-makers grope their way being prevented from reaching them only by "obstacles" and "error" and anyway, we go on to say, were decision-makers not to strive toward these idealized goals they would bring bankruptcy and long-run elimination on themselves if they are firms or, if they are consumers, they will reach lower indifference curves than would be possible and any (we would say, rational, meaning intelligent or "normal") human being would want more rather than less, caeteris paribus, when he can get it. But this is reasoning in a circle--justifying the rationality and omniscience premises by asserting that rational and omniscient persons must surely be rational and omniscient as we have defined these terms. That men do reason with something less than omniscience we can assert from self-knowledge; that they do strive to maximize something by their choosing is a truism. They are not "prevented" by "obstacles" from possessing omniscience; as well say men are prevented from flying by the twin obstacles of gravity and the absence of wings. This is just the way men are and the way they will continue to be until the "second coming" and we all become omniscient. Nor is their rationality applied to the maximization of such a simple and uncomplex and as "annual net profit" or even of "long-run profit." The value premises on which men choose include much more than the comparison of marginal rates of substitution and price ratios.

Boulding 5/ criticizes firm theory on two grounds: (1) the theory is unrealistic because it does not contain enough variables and those it does contain are not the variables that are significant to the firm and (2) the principle of maximization does not correspond to the actual principles that motivate behavior. He argues that in place of the maximization principle, simply conceived, must be a "theory of organization" that specifies a mechanism for stabilizing the variables that impinge on the firm within limits that are tolerable to the firm. This would be a "dynamic", a "volitional," a "functional" theory of behavior, not of the firm but of the firm's "peak coordinator." Not "maximization" but homeostasis--or maintenance of the state of the organism--should be, according to Boulding, 6/ the central

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5/ Boulding, K. E. "Implications for General Economics of More Realistic Theories of the Firm" American Economic Review, 42:2, 35. May 1952.

6/ Boulding, K. E. A Reconstruction of Economics. John Wiley and Sons, Inc., New York, 1950. Chapter 2.

concept of the short-run theory of the individual economic organism.

> The simplest theory of the firm is to assume a "homeostasis of the balance sheet"--that there is some desired quantity of all the various items in the balance sheet, and that any disturbance of this structure immediately sets in motion forces which will restore the status quo....
>
> A theory of the course of an organism through time must be a theory of disequilibrium rather than of equilibrium. Indeed, true equilibrium is unknown in the world of nature, for all things are subject to the irreversible processes of entropy...It may well be that the great bulk of human behavior does not follow the patterns of sober, reflexive maximization of advantage, but rather follows first the principle of inertia (nobody does anything unless he has to!) and secondly the principle of least resistance (if you have to do anything, you do the thing that is easiest to do!). There is nothing which says that the line of least resistance is the same as the line of greatest advantage except the long, slow retribution of natural selection.

Boulding also says that:

> ...there is a great need to integrate the general theory of organization into the body of economic analysis, and that such integration will immensely improve economics as a technique for analyzing the actual behavior of firms. Economists may even become useful to business!! 7/

Rational projections of maxima derive from the all-too-human drive for intellectual tidiness, for certainty, for analytical elegance. These human traits plus developments in mathematical models of economic behavior plus computers taken all together may pose a threat instead of a boon to agricultural economists. Listen to Dr. A. C. Harberger 8/ reviewing a book by G. Stuvel on an econometric analysis of the foreign exchange market.

7/ Boulding, K. E. op. cit. "Implications for General Economics of More Realistic Theories of the Firm," p. 44.

8/ Harberger, A. C. "Pitfalls in Mathematical Model Building," American Economic Review, 42, 1952, p. 855. A review of Stuvel, G., The Exchange Stability Problem. Leiden: H. E. Stenfert Kroese's Vitgevers--Maatschappij N.V., 1950.

> What a delight it is, when you're faced with a task like Stuvel's, to know that you can call to your aid the powerful tool of differential calculus! Set down your assumptions in explicit mathematical form, assure yourself that your model is mathematically complete, differentiate the model with respect to the exchange rate, and read off the answer. In Stuvel's case, however, the answer isn't very easy to read: the expression for the change in the trade balance consequent on a change in the exchange rate is an impressive jumble of coefficients--a quotient with both numerator and denominator stretching all the way across page 162, with three orders of brackets and numerous sub-quotients in each. It is the kind of expression which may be useful in that econometrician's paradise where the numerical values of all coefficients are known --but to earth-bound economists is not of much help. We can't use it "practically," since our estimates of coefficients are much too shaky to stand that much compounding. And we can't use it "theoretically," either, for by itself it contributes very little to our intuitive understanding of how the economy works only the assurance that "it's very complicated."

Harberger continues that approaches of this kind represent a kind of carry-over into mathematical models of the old adage that "figures don't lie" but this does not guarantee the validity or appropriateness of their assumptions. There are two main ways in which they may be and often are inappropriate. He calls them (1) the fallacy of completeness and (2) the problem of translation.

The fallacy of completeness arises from the baffling and frustrating complexity of economic functions which cannot be comprehended in their totality forcing us to separate our models into mathematical sub-systems assuming for purposes of a particular investigation that each such sub-system is complete and self-contained - but they are not. The pressures on us for positivism, for "insight", for profound and authoritative statements tempt us to forget the limited validity of our conclusions and to assert them with more assurance than they warrant with a consequent warped or distorted impression on our readers. But we also may even delude ourselves because of the problem of translation in that the elegance and symmetry of our models may lead "the unsuspecting economist to accept without murmur answers which he does not fully understand." 9/

Dr. Henry Grayson puts it this way: 10/

> There is a growing tendency underway, probably unconscious, to warp the economic theory to suit the exigencies of the mathematical theory. Mathematical economists should be reminded that...mathematics

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9/ Harberger, A. C. -- op. cit., p. 861.
10/ Grayson, Henry. "The Econometric Approach: A Critical Analysis," Journal of Political Economy, 56:3,253. June 1948.

> is a tool and must be shaped to fit the task for which it is designed. The economic considerations are paramount.

He then lists several "unclarified assumptions" that underlie most econometricians' approaches among which the following are most pertinent to our concern here with "rational projections":

(1) The assumption that in determining which variables will be pertinent in the systems from which the projections will be deduced, only those variables will be accepted for inclusion which fluctuate frequently or recur at short intervals because only variables of this kind will be found to be statistically significant. The rest will be discarded. Therefore, occasionally significant factors which do not fit the econometric scheme will be omitted. This assumption arises from the statistical inference that factors which show high correlation must necessarily be closely related and vice versa. Econometricians insist on satisfactory theoretical explanations for "high" correlations before they jump at conclusions. But such careful study appears to be lacking in the case of low correlations...

(2) The assumption that because certain influences are uncontrollable they must be random -- an assumption which is without foundation.

(3) The assumption that the behavior patterns of the actors will remain constant over the time span of the analysis. Such "constant" behavior patterns are really only a sort of average of the behaviors that actually occur, hence variability within them may be wide and disturbing --and may vitiate the "projections"; in fact, it is this inconstancy of the fundamental behavior parameters of the economic models that leads F. S. C. Northrop 11/ to assert the inability of economics to be a predictive science for want of what he terms a "conservation law" among its fundamental postulates.

The elegance, the tidiness, the aura of certitude that surrounds our mathematical models may, in fact, only give us the ability to assert answers that "ain't so" with more polish, a greater show of erudition, more positively, may lead us, in fact, only to being wrong in a more elegant manner--if you will pardon a metaphor, to being a confidence man in white tie and tails instead of a street corner betting tout.

The natural sciences proceed methodologically by erecting possible or plausible hypotheses, deducing their consequences, and testing reality for the latter; if the consequences predicted are found there, the hypotheses continue to be accepted at least provisionally; if the consequences expected aren't there, the hypotheses are rejected and reformed until consequences are deduced that are observable. But a funny thing about economics is that it erects hypotheses from which deduced consequences "aren't there" or are so "faintly" there as to raise question as to their warrantibility and goes on using these hypotheses year after year, decade

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11/ Northrop. F. S. C. "The Method and Limited Predictive Power of Classical Economic Science," Chapter XIII in The Logic of the Sciences and the Humanities. Macmillan Co., New York, 1947. Reprinted from Quarterly Journal of Economics for November 1941 where it bore the title "The Impossibility of a Theoretical Science of Economic Dynamics."

after decade. And yet we want to be scientists!

"Rational" projections deduced from simplifying, artificializing, mechanizing postulates may be "fun"; they make us feel like we're unrolling the future; they may satisfy our egos and justify our existence in this scientific era, but they may not do much else. If we really want to be scientists, if we really want to be policy advisers, then we must move in a direction that will allow us to make real world predictions that will lead to real world goal attainment (either private or public).

Real world predictions -- So let us turn briefly to consideration of what real world predictions in economics will demand of us as scientists and practitioners. And, at the same time, let us consider whether such a goal, an end, a "value" for us as agricultural economists is even possible--and if not what then?

The requirement that we make real world predictions means that our factual and value premises pertaining to the situation under study must conform to the reality of that world--or conform as closely as knowledge and the demand for warrantability in that knowledge will allow. I suppose there would be no disagreement among us that the factual premises, to be realistic, must be warrantable as existing in the situation rather than only in the mind of and imposed on the situation by the economist. But let us be clear about the "value premises." In exactly similar manner to the "factual premises," the value premises in reality must be those warrantable as existing in the situation also, rather than having been imposed on it from the mind of the economist. These value premises are those of the actors--are actually "factual premises" for purposes of analyses of the kind here being considered.

The economist is a human being studying human beings. As such, he has value premises of his own underlying his behavior as an analyst. His value premises as an analyst, however, are those we associate with "science," and "objectivity," and "honesty," viz., that to understand reality and to hope to control it is possible and good and that intellectual honesty in attaining it is right. But the value premises in the situation are those held by the actors under observation--what do they think they know? What ends in view do they have? What do they consider rational action to be? It is premises describing these that the economic investigator should not project into the situation if he aspires to understand it scientifically. (Did you ever pause to ponder how far we would have gotten in physics or biology, scientifically, if we had endowed electrons with rationality defined as minimization of the "pain" involved in collisions with one another "because any rational entity obviously wishes to minimize bumps and bruises," or if we had endowed boll weevils with the rationality to maximize their welfare by group action to thwart man's ability to destroy them? These would be our value premises imposed on the situation. Is it any different when we endow the human actor in our models with a rationality defined as maximization of annual net profit?)

Real world predictability on our part as economists will demand the following four broadly conceived components in our models--(1) humanized actors, (2) institutionalized social environment functions, (3) interconnectedness of the relevant economic theory with pertinent parts of

political science, law, geography, sociology, and social psychology on the "human" side of the involvment, and (4) inter-connectedness with the relevant biological and physical theories on the "nature" side. (It never ceases to amaze me how conscious we are of and how hard we work at the natural scientist's business of "production functions," how much time we spend exploring, explaining, and quantifying them while blithely skipping over the behavioral functions of the human actor. How far would we have gotten in our production economics if we had been content to stop with the simplification of the biological element in our models by assuming that marginal output in all situations always declines and at a constant absolute rate--i.e., is a negatively sloped straight line?)

Let us consider, here, only one of these four components, viz., what is involved in humanizing the actor?

We begin by asking--what is rationality?

> Historically, modern economic analysis began with the assumption that human beings do behave "rationally"... and proceeded to deduce and analyze the consequences of this assumed universal mode of action. The theories of the firm and of the consumer...were regarded as descriptive of actual behavior...and were used primarily as instruments in the study of the more complicated social phenomena. Economists...spent comparatively little thought on the problem of analyzing just what "rationality" means; they merely defined it as the maximization of a fully known utility function or profit function, and proceeded on to the more "interesting" questions...12/

Suppose we were to set forth a rational method for choosing an action. Among many important characteristics of the world in which we live and act that must be taken into consideration are these four:

> (1) The effects of an action radiate out in many directions and infringe upon an enormous number of value-effected and other aspects of the world...it may be said that an action may have an infinite number of consequences. (2) The results of an action not only involve a huge number of variables but affect these variables over a period of time. Strictly speaking, the effects of an action stretch from the movement of action to infinity...(3) The value criteria of a given actor tend to change over time...(4) (Therefore)

12/ Schoeffler, Sidney. The Failures of Economics: A Diagnostic Study. Harvard University Press, 1955. Quotations are from Appendix A, "Toward a General Definition of Rational Action," which was originally published under the same title in Kyklos VII (1954), No. 3. In this connection, also see Giriacy-Wantrup, S. V. "Concepts Used As Economic Criteria For A System of Water Rights," Land Economics 32:4, 295. November, 1956. Especially Section VI, "Economic Criteria and the Public Interest," pp. 309-312, is pertinent. Also, Lindblom, Chas. E., "Handling of Norms in Policy Analysis," The Allocation of Economic Resources. Essays in Honor of Bernard F. Haley,Abramovitz, Mases et al. Stanford University Press, Stanford, California, 1959.

> the process of judging the outcome of a given action may be infinitely long. We must consider a possibly infinite number of variables, over a possibly infinite stretch of time, in the light of a possibly infinite diversity of value criteria...
>
> Under these circumstances, the procedure of rational action (does not permit us to) do our computations and judging first, decide upon a course of action, and then proceed to carry it out. Now, the calculation and judging and the acting must proceed concurrently. Furthermore, our decision to adopt an action will always be based on an incomplete calculation, and will usually turn out to be "mistaken" as the calculation proceeds...

Rationality is an ideal to be striven for; the best that can ever be done about an infinitely distant goal is to proceed in its direction--it can never be attained--so it is with rationality. But if man in our models can't be rational and be real, what then? Neither can he be rigged out with actions that are wholly ignorant, illogical, blind, or rash. Not only would this extreme not be real either, but this would condemn man's behavior forever to completely random actions, hence to total unpredictability. We know he isn't like that! In other words he is very human! Neither saint nor sinner, neither all wise nor a complete dullard--but somewhere in between.

Here we are brought face-to-face with--have reasoned ourselves around to--the central conundrum in all social theory (not excepting economics, though it often appears that economists would like to have people forget the rest of their--i.e., the economists'--social science family as though economists were kind of ashamed of their social science relations' somewhat dowdy and boorish appearance when compared to the elegance of dress and manner of the scientist crowd they--the economists--run around with). That central conundrum of all social theory including economics is whether man's behavior is in part intrinsically, inherently unpredictable or whether its appearance of unpredictability is a result, only, of still inadequate knowledge of the functional structure of his actions. F. H. Knight 13/ has this to say:

> Life and society are orderly, up to a point which itself cannot be accurately determined. To have a mind means to change it occasionally; hence, to act unpredictably--but not too often, too erratically, or too far, or it would cease to be mind. As intelligent beings, we live somewhere between causation and chaos.

Rutledge Vining, 14/ in describing Knight's thoughts in this regard, describes Knight's two categories of action in man's economistic

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13/ Knight, Frank H. "Institutionalism and Empiricism in Economics." American Economic Review 42:2,45 May 1952.

14/ Vining, Rutledge. "Methodological Issues in Quantitative Economics: Variations Upon a Theme by F. H. Knight." American Economic Review 40:3, 267 June 1950.

behavior--one category being a truly creative activity; the other being a passive, mechanical and positivistic activity. The truly creative component is not subject to "scientific" analysis, to the derivation of "laws" that "explain" it and from which "predicitions" can be made: i.e., this component in man's behavior is the very activity of man remaking himself, changing the very stuff of his social relations and the categories of social behavior. It is these--the existing social relations and categories of social behavior --that are passive, mechanical, positivistic; hence, scientific, predictive, subject to discoverable laws and amenable to social control. But the creative part of man's behavior, the part that changes the relations and categories of his behavior, is not. This is Knight's way of formulating the idea of free will in economic philosophy.

> In addition to being actively economistic, man is a problem-solving entity at a higher level of critical deliberation about ends or values themselves: he is interested in changing not only the conditions under which he lives but also his own living self... Knight's classification (of economic behavior) implies that no law can exist that would reduce economistic behavior to predictable order in that this behavior is "active" and in that motives and preferences are developmental (i.e., subject to continuous change in the very act of "behaving" economistically, affecting their motives and preferences to modity them for the succeeding action).

Here again, while threading this intellectual maze, we face Northrop's contention that a predictive theory of economics is impossible because its fundamental entities are not subject to a conservation law--are not *immutable in space and time.*

We economists have taken for granted as positively recognizable and measurable so many of the entities of the world for so long a time that we must occasionally be jolted into self-analysis to remind ourselves how *ephemeral, indistinct, even imaginary, many of these entities really are!* Listen to G. F. Thirlby's 15/ description of the "organizational behavior" *of "policymakers" and of "administrators" in business firms. The busi-*nessman's decision is to conduct a series of planned operations over the future.

> (A) particular plan is simply the one chosen from possibly many contemplated alternative particular *arrangements or distributions of the same resources* ...(But) "the same resources" are largely idea-*tional in character and would be irritatingly non-*measurable to an external observer who felt he ought to be able to measure them. They are very much in the way of being a "function" of the man's mind. Their limits are much to do with the limits of the man's *mind...*
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> If...we desired a concise expression to describe the man's optimum distribution of his resources, and to indicate what he meant by cost, we should be

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15/ Thirlby, G. F. "The Economist's Description of Business Behavior." Economics N.S. 19, #74, May 1952, p. 148. (Italics in original.)

> driven to say that he adopts a course of action which maximizes his value and that the cost of that distribution of his resources is the value from an alternative distribution of "the same resources," that he would expect to achieve if...he adopted that course which would yield the second highest value.

The "resources" in question are a function of the man's mind; the "values" maximized are likewise. The very stuff of the economist's world fades into intangible mystery!

A. G. Papandreou 16/ argues that the economists' attempt to build a science of universal validity and to avoid all psychological and sociological commitments results in operational meaninglessness.

> The only way out of this impasse, the only way for arriving at an empirically relevant science is to make these commitments...
>
> ...the ultimate ends, the value systems, of the society in which behavior is studied, must find their place in the action framework employed in the analysis. *It is not sufficient to postulate the rational norm. We* must make commitments to value systems which are "ideally typical" in the culture under analysis...This will enable us to increase significantly the number of operationally *meaningful hypotheses in economics, albeit it will reduce* the universality of our science...In general, we should *extricate ourselves from the shackles of economic* universalism and experiment with less general but often more useful constructions.

Economics: Science? Art? or Both?

It's time we called this to a halt and see what we can make of it in the way of a message or a lesson--or something. As a result of my study of this problem, I am most impressed with these observations: (1) Agricultural economics is not now and will have a great deal of difficulty ever becoming a science. The method of science is to derive inductive generalizations from factual experience from which it is possible to deduce observable consequences such that the human agent--the scientist--can by application of the decision *rules of science assert whether such deduced consequences actually do occur* in reality and with a sufficient degree of probability to warrant accepting the validity of the hypothesized relationship. Science is a process for simplifying reality under stringent rules of conduct such that the artificialization resulting is not empirically meaningless, and yet reduces reality to comprehensibility by finite human minds. In those areas of reality that are nearly "sealed off" from the rest of reality or where the functional

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16/ Papandreou, A. G. "Economics and the Social System." Economic Journal 60, #240. December 1950, p. 715.

interactions with the rest of reality are tenuous or dubiously relevant to the problem at hand, it is possible to act scientifically as if the problem under study is a closed system and scientific generalizations and warrantable predictions about it are possible. We call such knowledge "science."

But where the reality surrounding the problem is a complex interrelated web of functional relations such that it cannot be comprehended as it stands nor neither can it be arbitrarily broken up into comprehensible segments from which dependable predictions can be derived, then we have an "open system" and prediction and control approach the impossible. This is where agricultural economics stands together with all other social and many physical and biological "sciences" (e.g., meteorology, range and wild life management, insect and disease, rodent and predator control).

Whenever reality is too complex and too elusive for "scientific" comprehension and control by human agents, human behavior for dealing with it is an art which leads us to my second observation, viz., (2) that agricultural economics is an art more than it is a science. It only weakly, if at all, erects inductive generalizations from which it can deduce explanations, predictions, and control. Its generalization premises are supplied to it by other disciplines that are consciously seeking such inductive behavioral and production functions such as its human behavior from psychology, its institutional structure and process premises from sociology, social psychology, and political science and its biological and physical function premises from these disciplines. The dictionary definition of art is (among others): "The practical application of knowledge or natural ability, skill, dexterity, facility, power. A system of rules devised for procuring some scientific, esthetic, or practical result." The art of economics lies in the assistance that it can render to private and public decision-making. As such it is practical, applied. If and when it is not predicting or cannot predict real world events with "scientific" warrantability, it is an art if it is anything more than a pure exercise in description like history--or geography.

To illustrate: when I studied feeds and feeding as an undergraduate, I used, as similar students do today, Morrison's famous text (only then it was called Henry and Morrison). But, no matter, then, and to this day, it carried on its flyleaf the quotation--"The eye of the master fattens his cattle". Nutrition is the science but feeding is an art; it is the art of the application of all knowledge, including science, available to the practitioner, plus experience, plus judgment, plus intuition, plus luck that "fattens his real world feeders in his real world feedlot."

So it is, too, with agricultural economics as a hand-maiden to "economic decision-making," private and public.

My third observation is that this science and art of agricultural economics divides into two separate branches: (1) general decision theory and (2) economic policy. Insofar as agricultural economics can aspire to joining the ranks of science, it will do so only through those contributions it can and does make to broadening and deepening the content of the theory of decision-making. Decision theory is like a branch of mathematics in that it specifies rules for the "rational" deduction of sound actions to guide goal-realizing decisions by individuals or by groups, both private and public. Decision theory can grow through the slow accretion of inductive

empirical generalizations of reality from which deduced consequences can be drawn, and tested for warrantability on the anvil of reality. This is the hall-mark of science. Agricultural economics as one of the family of theories of decision-making can add its bit to this accreting knowledge but as and only as a science of human behavior focused on the empirical study of decision-making in "economic," "agricultural economic," contexts. Economic policy on the other hand is concerned with devising appropriate prescriptions of action for the solution of what are customarily called "economic problems," governmental or private. As such it is and will remain an art. For these prescriptions must square with reality to the limits of man's poor abilities--like the cattle feeder, it is not science alone that fattens, but experience, judgment, intuition, luck--the prescriptions of real world policy are more than science, even much more than science.

I close with these quotations from Sidney Schoeffler in this thought provoking little book The Failures of Economics, A Diagnostic Study: 17/

> The history of economic thought...constitutes a gigantic blind alley, against the end of which economists have been bashing their heads for decades. Economics is not an autonomous self-sufficient discipline except by sheer custom, and it certainly is not and never can be an empirical nomothetic science.
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> The road to progress in economics seems to be blocked ...by a great deficiency in necessary methodological analysis. Before economists are able to accomplish very much they need to be clear in what they can do and what they cannot do and then concentrate their attention... on the solvable problems...
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> a. Those economists interested in general decision theory should come to regard their field as an independent area of specialization and combine their efforts with similarly interested people in statistics, philosophy, medicine, engineering, and other disciplines concerned with rationality of action.
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> b. Economists whose primary interests lie in the field of economic policy-making should self-consciously adopt the attitude of doctors and begin to learn the things they should know to do their job well...
>
> c. Economists interested in institutional analysis, national income accounting, labor and the various other ideographic fields (which, I add, includes agricultural economics) should frankly acknowledge themselves to be historians, join ranks with the avowed economic historians in the interest of a more complete and thorough-going study of the passing economic scene and also give

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17/ Op. cit. Chapter 7, pp. 162-3.

up the idea of being engaged in a search for universal principles.

d. A very much larger number of economists than at present should concentrate on the methodology of economics. They should become familiar with the philosophy of science, with epistemology, with the methodologies of other disciplines, with axiology, with mathematics, and above all with modern logic and its related fields...They should set themselves to a ruthless cleaning out of the Augean stables of current practices of economic analysis and then begin to fill the void by designing and constructing new and effective conceptual frameworks and analytic procedures.

And now my final thought is paraphrase of Dr. Boulding: 18/ I have been gradually coming under the conviction, disturbing to a practicing, professional agricultural economist, that there is no such thing as agricultural economics--there is only economics applied to the problems of agriculture. Indeed there is no such thing as economics--there is only social science applied to economic problems. More brutally, in fact, I fear there may not even be such a thing as social science--there may only be science applied to the economic problems of society, and moreover, the application of science alone is not enough--judgment, intuition, common sense, "the touch and eye of the master," all derived from experience--art, as well as science, must also be applied to the economic problems of society.

18/ Boulding, Kenneth E. A Reconstruction of Economics, op. cit., p. vii.