Research Review

Specification of Bernoullian Utility Function in Decision Analysis: Comment

By Steven T. Buccola*

In an article published in this journal, Lin and Chang (*5*) demonstrated the use of Box-Cox transformations in the estimation of Bernoullian utility functions. Where the utility and money wealth originally elicited are represented by U and M, respectively, Lin and Chang suggested the use of transformations $U^* = (U^\lambda - 1)/\lambda$, $M^* = (M^\lambda - 1)/\lambda$, and $M^{2*} = (M^{2\lambda} - 1)/\lambda$. The transformed data may be utilized in such alternative formulations as:

$$U^* = \beta_0 + \beta_1 M^* \qquad (\lambda \neq 0) \qquad (1)$$

$$U^* = \beta_0 + \beta_1 M^* + \beta_2 (M^{2*}) \qquad (\lambda \neq 0) \qquad (2)$$

One can estimate either equation by sequentially generating variables U^*, M^*, and M^{2*} corresponding to alternative λ's and by applying ordinary least squares (OLS) to each alternative set. The estimates of $(\lambda, \beta_0, \beta_1)$ or $(\lambda, \beta_0, \beta_1, \beta_2)$ which correspond to the greatest maximized likelihood, $L_{max}(\lambda)$, may then be identified (*3*, p. 215).

Lin and Chang note that as λ approaches zero, equation (1) approaches the doublelog form. When λ is 1, equations (1) and (2) are linear and quadratic, respectively. Thus, intermediate λ values represent functional forms intermediate to those commonly employed, permitting exceptionally close data fits. Lin and Chang illustrate this by applying (1) and (2) to estimate a utility function that Lin, Dean, and Moore (*6*) had represented as cubic in a previous study. Applying equation (2) resulted in selecting optimal farm plans closer to the plan actually adopted by the farmer than was the plan identified with the cubic utility function. Forms (1) and (2) also resulted in higher $\bar{R}^2$'s than did the cubic form.

I want to show that equations (1) and (2) above do not satisfy the properties of a valid Bernoullian utility function. They should not be applied, as Lin and Chang have applied them, to problems in which decisionmakers are assumed to maximize the expected utility of risky outcomes. The reason is that optimal decisions implied by (1) and (2) depend on the arbitrary origin employed in the utility measurements: an arbitrary shift in origin does not affect a Bernoullian utility function but it does affect the OLS estimates of $(\lambda, \beta_0, \beta_1)$ or $(\lambda, \beta_0, \beta_1, \beta_2)$ in the Box-Cox transformation.

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The Problem with Box-Cox as a Utility Function

The essential argument against equations (1) and (2) is that when either is normalized in U (expressed so that the original utility variable U is on the left side), a functional form results that lacks an intercept term. Because equation (2) is especially complex, I will illustrate this by reference to (1) only. One can then see that this reasoning applies just as well to (2). Normalizing equation (1) produces:

$$U = [(\beta_0\lambda - \beta_1 + 1) + \beta_1 M^\lambda]^{1/\lambda} \qquad (1')$$

In (1′), the addition of an arbitrary constant K to U cannot be accommodated by merely augmenting $(\beta_0\lambda - \beta_1 + 1)$ by K because the parenthesized term is, along with $\beta_1 M^\lambda$, raised to a power $1/\lambda$. Any attempt to make a compensatory adjustment in λ would affect the overall shape of the function. Thus, the only way a constant can be added to U without affecting the shape of (1′) is to augment the entire right side—a procedure that destroys the Box-Cox representation.

This feature of Lin and Chang's form (1) is inconsistent with Bernoullian decision theory. As Von Neumann and Morgenstern (*9*, pp. 24-25) have shown, utility should be preserved under any linear transformation, implying that the origin and scale of Bernoullian utility measurements are arbitrary. The addition of a constant to each utility observation produces a shift in origin; hence, such addition should result in a utility function containing the same essential decision-making information, the same "overall shape," as the original function.

One can make an equivalent argument by observing that the decisionmaking information of a utility function is, for small bets, uniquely contained in its absolute risk aversion (Pratt) function $r(M) = -U''(M)/U'(M)$ (*7*). If utility is preserved under linear transformations of U, then such transformations should also preserve $r(M)$ at any given wealth level M. The absolute risk aversion corresponding to Box-Cox form (1) or (1′), that is:

$$r(M) = -U''(M)/U'(M) = -(\lambda - 1)(M^{-1} - \beta_1 U^{-\lambda} M^{\lambda - 1}) \qquad (3)$$

is a function of λ. Because a shift of the utility origin by the amount K must affect λ, it must also affect $r(M)$ at any level of M. Hence equation (3) is not a permissible absolute risk aversion function.

Intercepts for Other Functional Forms

It should be clear from the above explanation that the presence of an intercept is required not only in conjunction with such highly nonlinear utility forms as (1′) but also with such commonly used forms as the quadratic, the semilogarithmic, and the exponential. For example, the intercept-free exponential function:

$$U = -\theta \exp(-\gamma M) \quad (4)$$

is just as incapable of absorbing utility origin shifts as is (1′). What is perhaps confusing is that utility theorists often express exponential functions as in equation (4), reasoning that the presence of an intercept is arbitrary. Intercepts can be subtracted from both sides without affecting the decision content of the function (*7*, p. 130). This reasoning is correct, but the obverse is not: once the utility function has been expressed as in equation (4), a constant cannot be added to U unless an intercept term is provided to absorb the addition, thus changing the functional form. Unfortunately, one cannot know in advance of estimation whether a particular sample configuration will require an intercept. Thus, the use of a form without an intercept term is unacceptable for purposes of utility estimation.

Note that the doublelog specification belongs to this class of intercept-free functions. In the form in which Lin and Chang express the doublelog, $\log U = a + b \log M$, one might think that term a acts effectively as an intercept. But this is deceiving, as is clear from observing the normalized form:

$$U = cM^b \quad (5)$$

where c = antilog (a). Equation (5) implies that U must equal zero when M = 0, which is inconsistent with the Bernoullian assumption that the utility of zero wealth can be fixed arbitrarily.

To illustrate the problem of using doublelog utility, I used an undergraduate's utility responses elicited over the $5 to $5,000 range by the Equally Likely Certainty Equivalent method (*1*, pp. 70-75). The seven original U, M response pairs were: 10, $5; 16.25, $40; 22.5, $100; 35, $300; 60, $2,000; 85, $3,500; and 110, $5,000. Using nonlinear least squares routine LSQ (*8*), I fit equation (5) to these data, then augmented all the utility observations by 500, and refit the equation. As the top of the table shows, utility parameter b shifts upward as the arbitrary utility origin is changed. Because the absolute risk aversion function corresponding to equation (5) depends only on b and M, the risk aversion coefficient at a given money level is also altered by the origin change. In contrast, if an intercept A is added to (5), no shift in b or in the risk aversion coefficient is effected when the utility observations are arbitrarily augmented (bottom of table). Here A absorbs the origin shift so that the estimate of b is invariant (except for rounding error) and unbiased. Because the use of A is inconsistent with a doublelog formulation, Lin and Chang should not have listed the doublelog as a legitimate utility function in their table 1.

Use of a normalized doublelog utility function with and without an intercept

Function	A	c	b	R^2
	Results of fitting $U = cM^b$ (doublelog)			
Original utility	—	3.065 (2.702)	0.4117 (8.937)	0.976
Augmented utility	—	473.578 (33.325)	.0254 (5.427)	.856
	Results of fitting $U = A + cM^b$			
Original utility	12.80 (2.84)	.407 (.946)	.6386 (5.157)	.987
Augmented utility	512.80 (113.79)	.407 (.944)	.6385 (5.168)	.987

— = Not applicable.

Examples of Using Box-Cox

The prohibition against doublelog utilities should not be surprising in view of their role as a limiting case of Box-Cox function (1′), which is itself an illegitimate specification. To demonstrate the difficulties one encounters when using a Box-Cox form as a utility function, I first fit equation (1) to the student's original utility data listed above. I employed 10 alternative λ values, ranging from 0.30 to 0.40. The highest maximized likelihood (−5.3269) and R^2 (0.9623) occurred where $\lambda = 0.35$. Associated OLS regression coefficients were $\beta_0 = 2.80566$ and $\beta_1 = 0.181371$ with t-values of 5.74 and 11.30, respectively. Next, I augmented each utility observation by 500 and refitted equation (1) using 10 alternative λ values ranging from 0.45 to 0.55. The highest maximized likelihood (−5.6146) and R^2 (0.9668) occurred where $\lambda = 0.51$. Associated regression coefficients were $\beta_0 = 44.8593$ and $\beta_1 = 0.033825$ with t-values of 209.45 and 12.06, respectively.

Finally, the absolute risk aversion function (3) corresponding to utility function (1) was solved, at a wealth level of $2,500, for each of the two sets of coefficients generated above. Use of the first set yielded a risk aversion value of 1.0166 E-4. In contrast, the utility measurements with an augmented origin yielded a risk aversion value of 1.8520 E-4, 82 percent higher than the original value. Thus, use of Box-Cox form (1) mistakenly implies that the student became more risk averse when a higher utility origin was chosen. Use of form (2) would involve a similar dilemma.

Sensitivity of the absolute risk aversion coefficient to changes in the utility origin possibly varies with sample configuration. The high R^2 of Lin and Chang's semilog fit

to Grower No. 5 (their table 3) suggests that the authors faced a nearly semilogarithmic sample. To approximately reproduce such a sample, I generated eight observations from their semilog equation, arbitrarily multiplied the utilities by 2, then randomly altered some of the utilities to produce a slight scatter. The U, M sample became: 200, $10; 350, $1,000; 368, $3,000; 390, $4,000; 394, $5,000; 404, $7,000; 408, $8,000; 416, $10,000. I fit Box-Cox form (1) first to this sample and then to an augmented sample in which 400 was added to each utility observation. The results for the first sample were: $\lambda = -0.13$ ($L_{max} = -2.5543$), $\beta_0 = 3.24116$, and $\beta_1 = 0.112676$, yielding a risk aversion value at M = $3,000 of 3.4703 E-4. Results for the augmented sample were: $\lambda = -0.06$ ($L_{max} = -4.9988$), $\beta_0 = 5.2228$, and $\beta_1 = 0.041775$, with an associated risk aversion value at M = $3,000 of 3.3972 E-4.

Hence, given the approximately semilogarithmic sample with which Lin and Chang dealt, variation of the utility intercept by 400 would result in a 2-percent shift in the risk aversion coefficient. This may have misled the authors into thinking that the risk aversion coefficient always shifts negligibly in response to utility origin changes. In fact, the shift was small only because, for approximately semilog and doublelog sample configurations, term $\beta_1 U^{-\lambda} M^{\lambda-1}$ in equation (3) is small and M^{-1} dominates $r(M)$. This would not be true for other sample configurations, as the student's utility data illustrate.

Conclusions

Functional forms such as equations (1) and (2) which employ Box-Cox transformations of utility observations may not legitimately be used to estimate Bernoullian utility functions. When the utility origin is arbitrarily adjusted, the absolute risk aversion coefficients associated with these forms also change, incorrectly implying that the decisionmaker's risk preferences have shifted.

Note that Box and Cox (*3*, pp. 213-214) recommend their dependent variable transformations for purely cardinal variables or for ordinal ("nonextensive") variables in which any monotonic transformation is valid. Bernoullian utility falls into neither category; only linear transformations preserve the decisionmaking content of a utility series, and this restriction is violated by a Box-Cox transformation on U. Lin and Chang have apparently identified a functional form which, in the particular case they address, predicts farmer behavior better than does cubic utility. But the prediction method does not conform to the principles of Bernoullian utility.

A possible solution to this problem is to fit (1′) by nonlinear least squares methods after adding to it an intercept term. But convergence to a stable set of parameters may be difficult to achieve when one attempts simultaneously to estimate four coefficients in such a complex functional form.[1] If a function permitting decreasing absolute risk aversion is desired, the form $U = B + \alpha_1 M - \alpha_2 \exp(-\alpha_3 M)$, $\alpha_i > 0$, is more manageable. Hildreth (*4*) reports using this form and I have found the Gauss-Newton search (*8*) to converge quickly when using it.

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[1] I attempted to fit (1′), with and without an intercept added, to each of the above set of utility responses by use of the LSQ option of TSP (*8*). No reasonably stable solution was found despite the use of about 20 sets of starting values. In a private communication, Clark Edwards reports a successful solution of (1′) itself (without the intercept) using the NLIN option of SAS (*2*).

Linear Programming, Duality, and Cost of Production

By Lloyd D. Teigen*

The total cost of production (COP) per unit of output cannot be less than the product price. Therefore, if one surveys COP for a sample of farmers, and if some are inefficient, and if resources are valued at opportunity costs, then it follows that the estimated production cost will be greater than the product price. One might expect the opposite, because we are accustomed to think that price must exceed cost to produce a profit. But this assumes that the operator's time and possibly some of the operator's resources as well are not accounted for in the cost computation. When we value these resources using the economist's idea of opportunity cost, the total COP per unit for efficient producers exactly equals the product price. The interpretation of the dual to a general linear programming model of a firm reaffirms this somewhat surprising result, which is independent of the selection of commodities or resources or of the farm organization.[1]

Since 1974, the Economic Research Service (ERS) has provided annual estimates of the COP for most major agricultural commodities as mandated by the 1973 Agriculture and Consumer Protection Act. The COP estimates include variable costs, machinery ownership, farm overhead, management, and land costs.

Under the 1977 Food and Agriculture Act, policymakers used a COP concept to determine changes in the target prices for wheat and feed grains. Replacement legislation proposed by the House of Representatives in 1981 (H.R. 3603) would have expanded the set of commodities for which production costs concepts determine changes in the target prices, and it would also have established a review board to oversee the estimation process.

The Senate version of the legislation (S. 884) provided for specific minimum levels of target prices for the commodities in each year 1982-85 and gave the Secretary of Agriculture discretionary authority to increase them as appropriate to reflect increases in the per acre production costs. The conference report reflected the Senate language.[2] With the exception of upland cotton, the essential difference between the 1977 and the 1981 language regarding the influence of production costs on target prices is that the 1977 act mandates the change, whereas the 1981 language permits, but does not require, the adjustment of target prices beyond minimum levels specified for each year. The 1977 act based the changes on per bushel (unit) costs, whereas the 1981 language considers per acre costs. The National Cost of Production Standards Review Board under the conference language would review the methodology of the COP estimates, but would not prepare independent COP estimates.

The COP concept used in the 1977 legislation to adjust target prices reflects variable costs associated with the specific commodity production enterprise, machinery ownership, and general farm overhead costs (for the 1978 crop, a return to land and to management was included). The COP concept estimated by ERS includes land and management and is the total COP per unit of output, valuing all resources at their current market prices. This cost is shown to be greater than the internal accounting cost (the true cost according to economic theory) which uses the shadow prices (or opportunity costs) rather than market prices of the resources. Furthermore, the COP at current market price for all farms is shown to exceed the market price of the commodity whose cost is estimated.

The total unit COP that is calculated by valuing resources at their opportunity costs for efficient profit-maximizing firms will equal the price of the product. Averaging the costs across both efficient and inefficient producers for a sample of farmers will result in an estimated total unit COP which is higher than the national average price of the commodity. The model shows how higher product prices affect the shadow prices for all resources owned by the farm (not just land), resulting in higher costs of production.

In recent years, the COP, which includes a return to land based on current land values, does indeed exceed the average crop prices, as predicted by this model. Thus, proposals have emerged within ERS to modify the accounting procedures so as to derive cost estimates more nearly equal to the true cost (opportunity cost) of production, by using the accounting costs (shadow prices) of the fixed resources for a representative farm rather than market prices.

In this article, I present the model and analysis on which COP conclusions are based and briefly describe the modifications proposed for COP accounting.

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[1] Analysts reached similar conclusions using cost curves as early as the thirties, but with much more cumbersome mathematical notation (for example, (*4*, footnote 12)). The same issue has been in and out of the agricultural economics literature ever since. Italicized numbers in parentheses refer to items in the References at the end of this article.

[2] As of December 16, 1981, both houses of the Congress had passed the conference report.

The Model

Let x be the vector of commodity outputs produced by the farm, and let b be the vector of resources of the farm at the beginning of the planning period. Some resources, such as fuel and fertilizer, may be at zero level at the beginning and be acquired later. When the farmer sells or rents some of the owned land, labor, or capital resources to others, those activities are also part of the x vector. A is the nonnegative matrix of technical coefficients, and a_{ij} is the amount of the i-th resource required to produce one unit of commodity j. Let z be a vector of the same dimension as b which represents the acquisition of resources—whether land or gasoline—during the planning period. The price of each unit of resource is c, some of which may be prohibitively expensive. The price of the output of the farm is the vector p.[3]

The linear programming (LP) problem for the farm is given by:

$$\begin{aligned} &\text{Maximize} && p'x - c'z \\ &\text{Subject to} && Ax - Iz \leqslant b \\ & && x \geqslant 0 \\ & && z \geqslant 0 \end{aligned}$$

The farmer maximizes current net revenue subject to the resource constraint with the understanding that x and z are nonnegative. Ax are the resources used by the firm, and $b + z$ are the resources available to the firm; $p'x$ is the cash revenue, and $c'z$ is the cash expenditure for purchased resources. If the existing farm resources were acquired at today's prices, the amortized outlay would be $c'b$.

If the above primal LP problem has an optimal solution which maximizes farm profit, then a dual problem can be structured which conveys the same technological and economic information (*1*, p. 222). The primal problem directly determines commodity sales and input purchases and indirectly determines the shadow prices (the amount profit would increase if one more unit of the resource were added) for the fixed resources of the firm. The dual problem directly determines the shadow prices and indirectly determines the commodity sales and input purchases. y is the vector of shadow prices for the firm's resources. The objective function in the dual problem minimizes the imputed payments to the farm's owned resources (b). In the optimum solution, $b'y$ equals the maximum net revenue ($p'x - c'z$) derived in the primal problem. This dual problem is given by:

$$\begin{aligned} &\text{Minimize} && b'y \\ &\text{Subject to} && A'y \geqslant p \\ & && 0 \leqslant y \leqslant c \end{aligned}$$

where A' is the transpose of A. The first constraint corresponds to the x-vector in the primal, and the second constraint corresponds to the z-vector and the nonnegativity constraint.

Analysis

$A'y$ is the accounting cost of producing the set of outputs and the j-th row of $A'y$ is the cost of producing the j-th commodity. The first constraint of this dual problem states that any feasible set of accounting costs (even valuing the resources at less than market prices) for producing each commodity will never be less than the price of the commodity.

From the second constraint, the accounting value of the resources cannot exceed the acquisition (market) price of the resource in question. Under the 1977 act, ERS' COP estimates are based on market prices for the respective inputs.[4] Combining these two constraints gives the result:

$$\begin{array}{ccccc} A'c & \geqslant & A'y & \geqslant & p \\ \text{USDA COP} & \geqslant & \text{Firm's internal accounting cost} & \geqslant & \text{Market price} \end{array}$$

For the commodities which the farm produces, the internal accounting cost (true cost according to economic theory) of production will just equal product prices. The farm will acquire additional units of any resource whose accounting value is equal to its market price. The quantity of outputs produced or resources purchased are computed as the "shadow prices" of the binding constraints in the dual problem (a result of the complementary slackness theorem) (*1*, p. 239).

No commodities will be produced whose accounting costs exceed the product price and no resource will be acquired whose internal value is less than the market cost. In longrun

[3] In the text which follows, p and c will be called market prices of products and resources. This is to distinguish them from the shadow prices which apply to the resources. "Market price" is really a shorthand way of denoting that price which induces producer response in the planning and resource allocation process. This may be the amount of money which changes hands when the commodity or resource is bought or sold, but more likely the response-inducing prices reflect the effects of support programs, risk, tax laws, inflation, credit terms, and a host of other factors. For durable resources, the annual outlay required to acquire another unit of that resource is presumed to be the entry in the c vector.

[4] The charge for management is a fixed percentage (10 percent) of the variable costs, machinery ownership, and farm overhead costs per acre. Prior to 1978, the management charge was based on gross receipts per acre.

equilibrium, the COP has to equal the product price, as the exit of firms with higher costs of production will ultimately lower the industry mean cost to the product price. Even when prices are not known with certainty or contain stochastic elements, the same relationships pertain to the supply-inducing price and cost expectations. The COP will be at least as large as the expected price.

The second constraint of the dual problem is the reason total farm returns ($p'x - c'z = b'y$) are generally less than their current market values ($b'c$). Thus one should expect not to be able to pay for a farm at current prices solely from the net receipts from current production. However, many new entrants to farming will not believe this fact and complain bitterly when they learn its truth.

This simple model explains why commodity prices never seem high enough to farmers and why COP estimates defend the farmer's position. Even if a farmer is the most efficient profit maximizer in the State, that farmer's true COP (using shadow price of resources) will just equal the price received. Any other producer (not at optimum) will have internal costs higher than the product price. If resource shadow prices are used in the estimation process, the mean of the costs of efficient and inefficient producers will give an internal (true) COP in excess of the average price of the product, and, if market prices of resources are used, the difference will be accentuated.

Our model demonstrates how higher product prices (whether based on price supports or demand shifts) immediately translate into higher imputed costs to all farm resources—not just to higher land values. Thus, basing product price supports on current costs of production contributes to a never-ending cycle of cost inflation and increasing budgetary exposure.

A Modified COP

Given that production costs exceed the prices of most agricultural commodities, ERS has proposed, and to an extent implemented, a modification of the COP estimates that would bring the estimated COP closer to the "true" COP. This procedure treats part of the factor costs as a residual to be distributed among the fixed resources of the firm.[5]

[5] U.S. Department of Agriculture, Economic Research Service, *Report of the ERS Cost of Production Task Force*, forthcoming.

Specifically, returns to land, operator and family labor, financial capital, and management and risk would be separated into portions "paid to others" and the "returns to the operator." Contractual payments to others would enter at the transaction prices, but owned land and capital would receive a return comparable to the "real" interest rate (net of the inflation premium required by savers and lenders) with the operator's human resources receiving the residual. This would determine a "user cost" vector (c^*) with values of the operator-supplied factors that may differ from the market valuation (c) of those resources so that:

$$A'c^* = p$$

The c^* vector attempts to estimate the average of the accounting costs (y) for all firms producing the commodity.

Such a procedure would avoid the problem of publishing production costs which are higher than product prices. This procedure would also break the cycle in which next year's prices are set above this year's price as the basis of estimated production costs, leading to a cost-price spiral. The modified procedure that ERS has proposed would center the arguments for alternative ways of providing a "safety net" for agriculture on the rates of return to factors, rather than on the national average COP. As this article shows, the basic truth is that the mean of total costs per unit of production across all farms must exceed the product price.

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Changes in the Chicago Corn Basis, 1960-75

By Kandice H. Kahl*

Individuals who produce, process, or market a commodity face the risk of cash price changes. By hedging in the futures market, they can transform this risk into the risk of basis (that is, futures price minus cash price) changes. Basis risk is generally smaller than price risk. This does not imply that hedgers are risk avoiders; they may be viewed as risk selectors (*4*).[1] Deciding which alternative is more advantageous and then determining the optimal time and location to trade requires a knowledge of basis movements.

Futures prices and cash prices for most commodities generally move in the same direction, but not necessarily by the same magnitude. Seasonal variations have tended to be stable over time, so an average of the historical values of the basis at a given time of the year has generally provided reasonably accurate predictions. Many agricultural economists advocate using the seasonal average basis in developing hedging strategies.

However, evidence for recent years indicates that the Chicago corn basis has changed from its historical average in certain months. This article compares the theoretical pattern of basis movements with observed behavior, generates hypotheses to explain the observed basis changes, and presents an empirical model. The results of regression analysis are interpreted to offer some possible explanations for the observed basis changes.

Theoretical and Empirical Patterns

The theory of storage, generally attributed to Working (*10*), but also discussed by others (for example, (*1*, *2*, *3*, *7*, *8*, *9*)), offers a theoretical explanation of basis movements. In this view, the basis for a storable commodity is a market-determined price that offers incentives or deterrents for storage. The price of storage, defined broadly, incorporates not only a warehousing charge, but may also include: (1) the interest rate (which represents the opportunity cost of the capital investment), (2) the cost of insurance, (3) the cost of related processing or transformation services, (4) a representation of risk aversion to unanticipated price fluctuations, (5) anticipated losses from deterioration (in the case of semi-perishables), and (6) convenience yield (that is, benefits from holding an inventory).[2]

The theory of storage implies that the seasonal basis should be widest at harvest. A gradual decrease in the basis is expected in the months following harvest, primarily because of the decreased costs incurred in the shortened period until contract maturity. As stocks are gradually depleted, convenience yield plays an increasingly important role, causing the basis to narrow and ultimately to become negative. Before harvest, however, the basis begins to rise to attain its maximum at harvest.[3] Because the cash commodity and the futures contract are relatively good substitutes at the delivery point in the delivery month, the basis at that location and time tends to approach zero.

To analyze basis behavior, I calculated the daily basis for No. 2 yellow corn in Chicago from the daily closing futures price, quoted by the Chicago Board of Trade, and the daily Chicago cash price, published by the U.S. Department of Agriculture. Although corn futures contracts are traded for delivery more than 1 year forward, only the prices for 12 months prior to contract maturity were used. The monthly average basis was computed from the calculated daily basis for 1960 through 1975. I deflated the data by the consumer price index (CPI) to remove the influence of changes in the general price level.

To describe the change in the seasonal pattern of the basis, I partitioned the data series at 1970 because of the increased uncertainty in the seventies.[4] The figure presents the deflated monthly means of the basis for the March contract for the sixties and seventies. In general, the theoretical seasonal pattern prevails. The basis is widest at harvest and gradually decreases, rising again prior to the next harvest.

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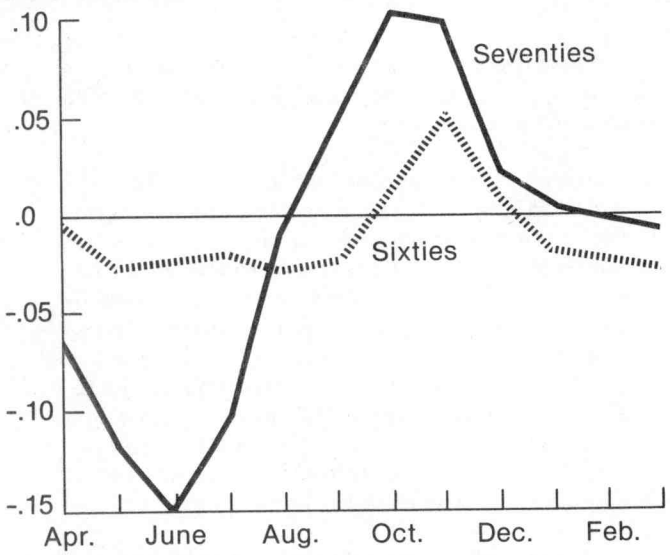
[1] Italicized numbers in parentheses refer to items in the References at the end of this article.

[2] Transportation costs, included in the basis for locations other than the delivery point, are ignored in this study.

[3] If expected harvest is very small relative to expected demand, the basis may always be positive prior to harvest, providing incentives for more carryover stocks (*3*).

[4] Uncertainties were greater than the sixties for a number of reasons. (1) The passage of the Agricultural Act of 1970 signified the beginning of the transitional period from prices which were largely Government-controlled to market-determined prices. The risk previously borne by the Government was gradually transferred to the private sector. (2) U.S. crop production in the seventies was marked by both record-breaking and very small harvests. (3) Devaluation of the U.S. dollar in 1971 appears to have increased the demand for American commodities abroad. Export demand was quite volatile, adding uncertainty to commodity prices. (4) Interest rates and inflation rates were more volatile than in the sixities.

Real March Basis for Grouped Years, 1960-75 March Basis△



△Monthly average of the daily price of March corn futures minus the daily cash price, at Chicago, deflated by the Consumer Price Index.

The extreme values of the basis seem to have changed. The basis increased for the months around harvest (September, October, November) and decreased for the summer months (May through August) during the seventies relative to the sixties. The results of two-tailed *t*-tests indicate a statistically significant difference in the basis between the sixties and seventies for harvest and summer months (table 1).

Hypotheses

Several hypotheses may explain the observed basis changes from the sixties to the seventies.[5] First, a change in price expectations from one decade to the next may have caused the basis to change. To explain the significant basis changes which occurred during the harvest and summer months, however, changes in expectations would have had to occur only during those months.

Second, the supply of storage space available for corn may have been decreased by the increased production and inventories of other grains and soybeans. Any observed change in the basis caused by a shift in the supply of storage space should be limited to the period around harvest, because storage space at other times of the year would probably be ample.

Third, the increased corn production, and thus corn inventories, and the earlier harvest time (primarily because of the improved drying techniques) may have caused monthly corn inventories to increase earlier and thus have contributed to the increased basis at harvest. The reduction in corn carryover stocks in the seventies, caused primarily by a reduction in Government stocks and large export sales, may have caused convenience yield to increase, and thereby the basis to decrease, in the summer months.

Fourth, shifts in the consumption function may have caused the basis to change. Although consumption shifted from the sixties to the seventies, the shift would have had to occur predominantly during harvest and summer months to explain the observed change in the basis only during those months.

Regression Results

The model used in estimation is:

$$B_t = a_0 + a_1 LAGB_t + a_2 PBS_t + a_3 CGC_t + e_t \quad (1)$$

where:

B_t = the basis for futures contract i at month t,

$LAGB_t$ = the basis for futures contract i at month $t-1$,

PBS_t = the percentage of current bin space capacity available for corn at month t,[6] and

CGC_t = private corn inventory minus corn consumption multiplied by 1 minus the price elasticity of demand, at month t.[7]

The three explanatory variables are included to test the four hypotheses given in the previous section. The variable $LAGB_t$ is used to measure the effects of changes in price expectations (utilizing a rational expectations approach). The variable PBS_t is used to measure the effects of shifts in the supply of

[5] These hypotheses can be generated from the reduced form equation for the basis, solved from a structural model of the cash, futures, and storage markets (see (*5*) and (*6*)).

[6] The variable PBS_t is calculated as the difference between U.S. bin space capacity off-farm and soybean stocks (a commodity competing for storage space with corn), divided by U.S. bin space capacity off-farm. Data on U.S. bin space capacity off-farm are available annually only. The reported figures (as of January 1) are assumed to apply throughout the previous calendar year. One can compute monthly data on soybean stocks by adjusting quarterly stock data by production, quantity consumed, quantity exported, and percentage of the crop harvested.

[7] Quarterly corn stock data are adjusted to compute monthly data following the same procedure used to compute monthly soybean stocks, as discussed above.

Table 1—Differences between the real corn basis in Chicago for the seventies and sixties, by month and contract[1]

Month	Contract				
	December	March	May	July	September
October	0.0968** (7.767)	0.0967** (6.969)	0.0912** (6.238)	0.0799** (5.128)	0.0588* (2.369)
November	.0458* (2.889)	.0438* (2.186)	.0370 (1.749)	.0276 (1.296)	.0169 (.752)
December	.0080 (.629)	.0151 (.984)	.0030 (.163)	-.0109 (-.547)	-.0273 (-.973)
January	-.0750 (-1.618)	.0212 (1.302)	-.0022 (-.103)	-.0217 (-.817)	-.0438 (-1.302)
February	-.0710 (-1.603)	.0216 (1.563)	.0029 (.157)	-.0188 (-.745)	-.0413 (-1.252)
March	-.0613 (-1.436)	.0199 (1.440)	.0025 (.199)	-.0487 (-1.612)	-.0349 (-1.285)
April	-.0495 (-1.123)	-.0577 (-1.281)	-.0031 (-.398)	-.0182 (-1.295)	-.0327 (-1.236)
May	-.0808 (-1.905)	-.0906 (-2.062)	.0031 (.444)	-.0272 (-1.935)	-.0563* (-2.198)
June	-.1117 (-2.140)	-.1245* (-2.276)	-.1378* (-2.419)	-.0146 (-1.169)	-.0729* (-2.333)
July	-.0670 (-1.709)	-.0813 (-1.944)	-.0948* (-2.147)	.0293* (2.607)	-.0344 (-1.694)
August	.0312 (.983)	.0172 (.498)	.0008 (.021)	-.0156 (-.388)	.0434* (2.552)
September	.0663* (2.254)	.0622 (2.019)	.0553 (1.716)	.0435 (1.305)	.0437** (2.998)

[1] The figures in parentheses are the calculated t-values. A two-tailed t-test was executed to determine significance at the 95-percent confidence level (denoted by *) and the 99-percent confidence level (denoted by **). Data from October 1970 to December 1975 are compared with data from January 1960 to September 1970, deflated by the Consumer Price Index (1967 = 100).

storage space. The variable CGC_t is used to measure the effects of changes in private corn inventory[8] and shifts in the consumption function.

The latter variable can be used to test two hypotheses, because the coefficients of private corn inventory and the exogenous shifters of the consumption function differ only in sign in the reduced form equation for the basis (see (6)). One can obtain an estimate of the shifters of the consumption function (a_o in the equation $C_t = a_o + a_1 P_t$) by substituting $\eta\ C_t/P_t$ for a_1 (obtained from the definition of price elasticity of consumption (η)) and solving for a_o. The result $a_o = C_t\ (1 - \eta)$ is incorporated as a shifter in the variable CGC_t.[9]

[8] Private corn inventory includes total (private plus Government) inventory during the preceding month minus Government stocks during the current month.

[9] Most estimates of the coarse grain price elasticity of demand for feed from previous research studies have been close to -0.4. Consequently, this value was used.

Practical problems arise in testing the hypotheses given above. First, although the basis lagged 1 month is included to measure price expectations, it would be improper to attribute all explanatory power of the measured variable to price expectations. Second, problems may arise in separating the individual explanatory power of the variables PBS_t and CGC_t because of the possibility of multicollinearity.[10] Third, some of the effect of consumer demand in CGC_t because of the possibility of multicollinear-($LAGB_t$).

[10] I did not measure the degree of multicollinearity by obtaining the R^2 from regressing each explanatory variable on the remaining explanatory variables. However, relatively high correlations between the explanatory variables indicate the possibility of a high degree of multicollinearity. For example, correlations between PBS_t and CGC_t are -0.6 and -0.7 during October and November, respectively. Correlations between PBS_t and $LAGB_t$ are -0.7 in November, and correlations between CGC_t and $LAGB_t$ are 0.7 in June and 0.6 in July and November. All these correlations are statistically significant at the 95-percent confidence level.

The regression analysis utilizes monthly data for the 16-year period, 1960-75. Hence there were 16 observations for each regression. Because the largest changes in the monthly average basis between the sixties and seventies occurred in the summer and harvest months, the analysis reported here focuses on separate regressions for the months June through November.[11] Graphical analysis of the dependent variable versus each independent variable gave no indication of nonlinear relationships. No evidence of autocorrelation of the residuals was found. Regressions for the March basis are repeated here. Conclusions from regression for other bases are similar. As evident from the results given in table 2, the model explains variation in the March basis far better in June, July, September, and October than in August and November.[12]

The lagged basis term, included to capture price expectations, offers much of the total explanatory power of the model. Despite the significance of the lagged basis term, it is not a perfect predictor of the current basis.[13]

The increase in the harvest basis during the seventies can be partially attributed to a shift in the supply of storage space (PBS_t). Because there is much excess storage capacity prior to harvest, PBS_t was expected to provide little explanatory power during the summer months. This expectation is confirmed. The coefficients are not significant during June, July, or August. In contrast, the explanatory power of PBS_t was expected to increase during the harvest season as excess storage capacity decreased. This expectation is weakly confirmed. The coefficients are significant at the 80-percent confidence level for September and October as excess capacity is reduced and the basis widens. Perhaps precise measures of excess storage capacity would have improved the result, but such data are not available.

The variable CGC_t should capture the effects of corn stocks and flows. A positive sign for the coefficient would be consistent with this variable measuring the demand for storage, as stocks increase (or as consumption decreases), the demand for storage space increases and convenience yield decreases, causing the basis to rise. However, the sign of CGC_t was negative in all months except October. Apparently, the variable is not measuring the effects of convenience yield. Perhaps the anticipated part of convenience yield is incorporated in the lagged basis term. If so, CGC_t may measure the effects of unanticipated convenience yield. The negative sign for the coefficient of CGC_t may suggest that expectations reflected in the lagged basis term tend to overestimate the effects of convenience yield.

Conclusions

The seasonal pattern of the corn basis was different during the seventies than during the sixties. The empirical results seem to support the hypothesis that the increase in the harvest basis in the seventies was at least partially caused by increased demand for available storage space. The results seem to refute the hypothesis that the decreased basis in the summer months of the seventies can be attributed to increased convenience yield, because of lower stock levels. However, the results do not necessarily refute that hypothesis because of the difficulty of interpreting the explanatory power of the lagged basis term. The latter term was

[11] Pooling the data over contracts to obtain more observations is not reasonable, because the values of the basis calculated for different contracts at the same time do not represent independent experiments.

[12] The relative lack of variability in the basis during November may explain the disappointing results in that month. The disappointing results in August are more difficult to explain.

[13] The coefficients of the lagged basis term are significantly different from 1.0 only in July, August, and October.

Table 2—Linear estimation of the real March basis, June-November, 1960-75[1]

Month	Intercept	$LAGB_t$	PBS_t	CGC_t	R^2	F-ratio
June	0.48 (.72)	1.38 (6.91)	−0.35 (−.53)	−0.10 (−1.44)	0.86	25.05
July	.44 (1.14)	.78 (10.88)	−.38 (−.94)	−.07 (−1.94)	.92	48.35
August	.20 (.23)	.58 (3.33)	−.09 (−.11)	−.14 (−1.80)	.48	3.72
September	.84 (1.52)	.88 (6.45)	− .81 (−1.44)	−.07 (−1.31)	.78	14.27
October	.54 (1.47)	.52 (3.90)	− .59 (−1.52)	.04 (1.73)	.75	11.97
November	−.08 (−.20)	.57 (2.12)	.16 (.37)	−.01 (−.22)	.34	2.08

[1] Numbers in parentheses are the calculated *t*-values.

significant and seems to support the importance of price expectations during a decade of increasing uncertainty in explaining the month-to-month changes in the basis.

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A Survey of Agricultural Economics Literature

Vol. III: Economics of Welfare, Rural Development, and Natural Resources in Agriculture, 1940s to 1970s. Lee Martin, ed. Minneapolis: University of Minnesota Press, 1981, 653 pp., $35.00.

Reviewed by Paul W. Barkley and Ron Mittelhammer*

Over 13 years ago, C. E. Bishop, then president of the American Agricultural Economics Association, suggested that the vast proliferation of information in that profession be surveyed so that agricultural economists could be made more aware of the literature that they as a group had generated and so that duplication in research and publication might be reduced. The task gained approval of the Association's Executive Board, and committees were named to generate topics, assign chapters, and monitor the work.

The original charge suggested a post-World War II literature review. At the time, this involved a period of approximately 25 years. The organizing, writing, and publishing of the surveys were time-consuming.

By the time Volume III—the subject of this review—was ready for publication, the time frame was not 1945 to 1970, but 1945 to 1980. This added decade, while important to the progress of agricultural economics and to the literature of the profession, must have imposed untoward hardship on some of the 17 authors. The more they surveyed and wrote, the more it seemed necessary to survey and write! The product of this quandary is evident in the individual chapters of Volume III. They are characterized by abrupt endings rather than by being rounded off to complete, balanced statements regarding the condition of the profession's literary heritage in the early eighties. But this is not the fault of the authors. It is, rather, the fault of the framers of the project who expected the keenest, most contemporary minds in the field to stop, look back, and reflect on the work they themselves had produced and were continuing to produce even while the review was in progress.

A reader will judge the value of this book on the basis of its technical content. And, it is on this basis that our review is written. The book has no single thread that ties it together so it must be reviewed piece by piece. It separates into four major sections.

Part I: The Economics of Rural Poverty

Three eminent, mid-career agricultural economists—Bryant, Bawden, and Saupe—agreed to examine the literature of rural poverty. Their 75-page narrative begins with an interesting sentence: "In an ultimate sense all of economics can be viewed as the economics of poverty." There is an undeniable element of truth in this statement, and, to the extent that the authors believe it, they make their own task more or less impossible. Even though they take five pages to delimit and narrow the subject, they are still left with a literature having indistinct boundaries in time, theory, and geography. They describe their approach to the review as eclectic, and the reader has no reason to quibble. They admit their inability to synthesize the literature; again, the reader must agree.

Poverty has an important role in the literature of agricultural economics. Indeed, had it not been for rural poverty, G. F. Warren might not have started his comparisons of farms in upstate New York and H. C. Taylor might not have begun his investigations of land tenure and land classification in Wisconsin. Had these two men not devoted years of thought and research to the problem of poverty (or income differentials), agricultural economics might not have become the best known and most productive of the applied branches of economics. Thus, isolating poverty as a problem and as a logical subset of the literature of agricultural economics ought not to be as intractable as the authors make it sound.

The authors are frustrated with the overlap between micro and macroeconomics; they worry about the definition of rurality; and they are concerned that the economics of poverty cannot be separated from the economics of growth. They define a maze-like framework and try to fit 1,429 references into that maze. The reader is left with a sense of futility over the possible status of the literature in 1945 and how it might have changed since.

The narrative on rural poverty is divided into 13 sections covering topics such as "Definition and Measurement of Poverty," "Cash Transfers," and "Human Capital." Cause and cure are given equal billing. Each major section opens with a paragraph or so that defines the subject. Summary statements appear throughout the sections and provide useful insights into the subject, but most often do not deal with the literature. The literature is described intermittently; it appears almost as filler.

Altogether, the writing on rural poverty is disappointing, and one senses that the disappointment stems from the authors' attempts to do too much: 1,429 items cannot be reviewed adequately in 75 pages. The narrative includes some good lessons, but they are hard to find. The 75 pages of references at the end of the narrative offer a vast store of information for any researcher or teacher who wants a quick list of the more prominent writings on the subject.

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Part II: Rural People, Communities, and Regions

After a brief and honest introduction by George S. Tolley, this part is further divided into three substantive chapters which must be considered invidually. Clark Edwards takes on the literature related to regional growth. He uses 90 pages to comment on 865 items. Many agricultural economists have participated in producing the literature of regional analyses and, for a time at least, regional analysis was almost a required part of an agricultural economist's training. Edwards recognizes the linkages and overlaps between agricultural and "other" economics and, correctly, includes the broad range of literature in his survey.

The survey begins with a framework, or classification scheme. Edwards chooses seven major categories of work, which he divides into three groups. The first group he calls "Descriptive Statements." This section is treated rather casually because descriptive studies are not held to explain behavior or to explain how things got the way they are. Considerable reference is made to pre-World War II writing, but one does not find the names of many main-line agricultural economists in this group.

The second group includes what Edwards calls the five bases for growth: increasing resource availabilities, advancing technology, expanding markets, conquering space, and building institutions. These provide the backbone of the chapter. Each could be the subject of an entire book. Edwards does well to mention the historical roots of each base and to bring the survey up to date with descriptions of both the most prominent and the less highly praised contributions to the literature.

His treatment of the five bases is unequal in depth and intensity. The sections on resource availabilities, advancing technologies, and conquering geographic space are quite good, but the ones on expanding markets and institution building are less than convincing. In the section on markets, one looks for a consistent and logical discussion of the modern literature, but finds instead an exhausting list of input-output and economic base studies. Neither does Edwards build a convincing case with respect to institutions. The conceptualization of an institution as merely group behavior is somewhat stifling and the overall conclusion that institutions either enhance growth or impede growth seems narrow. These limitations, though, are a small matter. Edwards has produced a thorough and insightful survey—much more than just a workmanlike job.

A group of four scholars—three agricultural economists and one rural sociologist—at The Pennsylvania State University undertook to survey the literature related to rural development. By the time they finished, Jansma, Gamble, Madden, and Warland had consumed 52 pages of narrative in commenting on 389 articles, many of which had been written by individuals outside the mainstream of agricultural economics. The Penn State group starts tediously. After suggesting that rural development refers to an increase in the well-being of rural people, they struggle to find a way to work with the definition. They settle on a two-way split with conceptual matters forming one approach and applied studies forming the second. This split is reasonable in that it provides places to put most items in the literature. However, the ensuing discussion is unbalanced because the concepts are subjected to interpretation and elaboration whereas the applications often seem like little more than an enumeration.

One can argue that the Penn State authors spend too much time on themes related to industrialization and regionalization (although these sections are very well done) and too little time on themes related to the delivery of specific services. However these are matters of choice about which no two professionals agree. The only serious omission is the limited reference to the Rural Development Act of 1972. This act was filled with promise and gave many researchers an incentive to continue pressing to learn more about rural development.

The Penn State group does its best work after their 48-page formal review. The authors then spend one page in perfunctory summarization before going on to their "Conclusions," and, finally to a perceptive postscript apparently written after the major article had passed through all but the final stages of editing and printing.

In their conclusions, the authors muse that the postwar literature related to rural development divides into three broad categories: (a) academic-rhetorical, (b) synthesizing-retrospective, and (c) prescriptive. One might suppose that the growth of any technical literature goes through stages roughly comparable to these three. Ideas are formed, programs or policies are synthesized, and, finally, prescriptions or recommendations are made. The four reviewers of the rural development literature indicate that ". . .the literature reviewed is highly weighted toward the first. . . and the second type. . ., but the third type seems to be an arena with much unfinished business" (p. 334). The unfinished nature of the literature comes from fragmentation of effort, noncomparability of research goals, and the nonadditive nature of much rural development work. The postscript alludes to the premise that rural development research has been coopted by rural sociologists since the major review was written. Although rural sociologists have indeed increased the tempo of their activity in this area, it does not necessarily follow that the work of agricultural economists has slowed.

These 100 pages on rural development leave the scientist with an empty feeling. Where has the profession gone in this field? Not very far. Where will it go? We cannot tell

until the process of abstraction and theorizing produces the seminal threads that can be woven into a fabric combining the many problems of rural development in a useful way. The authors seem to have had this same conclusion in mind.

The third chapter is a short essay by Philip Graves and Marion Clawson on rural-to-urban migration. Even though the article is supported by a 263-item bibliography, only a handful of references are explicitly referred to in the narrative. This is deceiving, for Graves' and Clawson's work does indeed tell readers what others have done, how the migration and population literature has progressed, and where it might go.

The essay does more. The authors incorporate economic theory into an analysis of migration and of the literature of migration. They devote 6 of 17 pages to describing a framework useful in understanding the literature. This framework—called "the two migration triads"—is apparently the authors' own design. One triad is based on perfect information about space and mobility and meets the equilibrium requirements imposed by a finely-tuned economic system. It is personal and rational and helps to describe why a farm family moves to town or why white collar workers rush to the large lots in the suburbs. The second triad is based on imperfect knowledge, immobility, and disequilibrium. It might be used to explain the movement of blacks from the agricultural South to the industrial North in the fifties and sixties or the mad rush to the Klondike in response to the discovery of gold in the nineties.

Graves and Clawson's work has an appeal that grows. It gets better with each reading! They saw a vast literature that impinged upon the work of agricultural economists, but recognized that agricultural economists themselves had contributed only minimally to its development. (Only 25 of the 263 articles referenced were authored by a "recognizable" agricultural economist or published in the *American Journal of Agricultural Economics.*) Instead of providing an annotated list or a classification of articles, the authors offer a useful framework and tell their readers to analyze the literature on their own. It is an interesting approach and must itself qualify as a net addition to the literature on human migration.

Part III: Natural Resource Economics, 1946-75

Castle, Kelso, Stevens, and Stoevener have mastered the art of literature reviewing. They delimit, abstract, provide contexts, build fences, apologize for what has been left out, and then use 79 pages to provide a superb review of 518 referenced items.

Much of the strength of this contribution can be found in its first 20 pages, which provide a magnificent overview of the theoretical and philosophical development of what is now known as natural resource economics. The description of the lineage culminates in a flow diagram that puts each segment of historical and contemporary work in its proper place and shows mainly how resource economics became what it is today, how complex lines of thought converged, and how the progress in understanding resource problems was enhanced by progress in some related fields of applied economics. The authors then describe the political economy in which the post-World War II literature developed. This is a valuable tool for both neophyte and expert. It tells how occurrences in the macroeconomy and in the political sphere influence the behavior of applied scientists as they seek to advance knowledge.

The cataloging section is thoughtful and each topic is reasonably complete. Although a reviewer can object to the weighting of the themes (why is so much time spent on project evaluation and so little time on tenure arrangements?), this emphasis depends on personal choice. Nonetheless, reading the review of actual contributions will give an interested reader a grand overview of the important progress in this branch of the agricultural economics literature and one can identify a major part as being the work of agricultural economists.

The authors close as handsomely as they begin. The final 12 pages of the narrative are devoted to a statement about where natural resource economics must go. This section carries the reader through existing paradigms, institutional changes, and the emergence of public choice theory. The imaginative and thorough survey of the natural resource literature, 1946-75, provided by Castle and his coauthors will be informative reading to teachers and researchers alike.

Part IV: Organization and Performance of Agricultural Markets

Three professors of agricultural economics from the University of Wisconsin—Helmberger, Campbell, and Dobson—share the difficult task of surveying the literature on the organization and performance of agricultural markets. Each has made notable contributions to the agricultural economics literature dealing with marketing, policy, and the organization and structure of agricultural industries. The narrative fills 123 pages of text—the most of any of the surveys in Volume III—and discusses 553 items. The discussion initially appears long on structure, conduct, and Government policy with respect to agricultural industries and short on performance. It is not until the reader recovers from this initial disappointment and has had an opportunity to reflect on the survey that the value of the authors' approach to the performance question can be fully appreciated.

The professors from Wisconsin embrace the view that it is not useful for economists to research the performance question to judge whether a particular agricultural market is performing well. They believe such judgments inevitably involve the use of performance norms, that the value of the particular norm is almost always subject to debate, and that ultimately it is policymakers who must judge whether market performance is acceptable. Thus, Helmberger and the others focus their discussion on the factors affecting the variables commonly accepted as relevant to discussions of market performance. They do so by thoroughly examining the linkages among the elements of structure, conduct, and the performance variables. But, they do more than this. They also note the importance of the internal organization of business enterprise and the legal environment.

The first 15 pages of the narrative summarize the framework of industrial organization, examine approaches to the market performance question, and define the direction and scope of the remainder of the survey. The remaining 108 pages are devoted to surveying a body of literature originating predominately from within the agricultural economics profession. The authors examine the relationships between performance variables and market characteristics including competition in agricultural markets, the cooperative movement, the level of market information, systems of grades and standards, spatial and temporal market separation, and the move toward vertical coordination of market levels. In their conclusions, Helmberger, Campbell, and Dobson reflect on the body of literature and pronounce it impressive. However, they are quick to point out that much still needs to be done in the area of performance. They suggest that future research should emphasize a more rigorous quantification of relationships explaining outcomes of performance variables and that basic research with the promise of advancing the theory, research methods, and quality of data relevant to the performance question should be encouraged.

Overall, the survey achieves its purpose of providing the reader with a broad view of the various approaches used by agricultural economists in examining the organization and performance of agricultural markets. Helmberger, Campbell, and Dobson must be congratulated on producing a survey valuable for all who are interested in agricultural marketing.

Reviewing a review or summarizing a summary or distilling a distillate can be hard work. We found this review agonizingly difficult. At the outset, the chore seemed tractable—not unlike any other book review. Halfway through the reading, the dimensions of the task began to overwhelm us. What can be said about a book whose 17 authors use 422 pages of narrative to comment on 3,810 different items? What can be said about a book that deals with several related, but distinct, topics? What can be said about a project that has been nearly 15 years in the making and has changed demonstrably since its inception? We believe some general statements can be made; perhaps they will be useful:

1. The book is by and for professional agricultural economists. It will not generally be read for its intellectual content, but rather for its classifcation and listing of over 3,800 items in the literature. Some will use it like an annotated bibliography; others will find the interpretations more useful.

2. The sections of the book are uneven in quality. The articles on natural resource economics and on agricultural markets are excellent. The article on poverty is disappointing. The others fall somewhere between. The article on migration is more of a statement than a review.

3. The book will help researchers and teachers as they struggle to find what has been accomplished in the various subfields of agricultural economics. Meticulous researchers will probably not find this survey a substitute for a literature search because they will not know what has been left out. Teachers will find the book useful as a place to find information and, perhaps more important, as a place to send students for material for term papers.

4. The people who read this book (or parts of it) are conditioned to reading textbooks, polemical pieces, or technical pieces filled with facts, data, and hypotheses. The present volume neither fits these categories nor presents these menus, so it may be misunderstood by its readers. It is a survey, a review, an inspection, an appraisal. It deals with what has been written about a handful of special subjects during a time in the life of this profession. There is no lesson, no statistical proof, and no policy prescription. There is only a panorama of what has happened.

5. The American Agricultural Economics Association has produced a useful book. In our estimation, the Association did its members and the profession a service when it commissioned the articles included in Volume III. The practice should be continued.

The Methodology of Economics (or How Economists Explain)

Mark Blaug. New York: Cambridge University Press, 1980, 296 pp., $29.50 (cloth), $9.95 (paper).

Reviewed by Roger Conway*

Those of us involved in economic research should care about methodology because it is the floor we walk on. If economics is to claim a scientific stature, then the profession must formally define a program for rejecting or accepting a proposed economic theory and, on the basis of accepted theory, produce accurate and pertinent predictions that, in principle, can be empirically tested.

Economists' concern with methodology has been heightened by a decade in which the explanatory power and predictive accuracy of economic theory was questioned. The purpose of this book is to reexamine the basic principles of reasoning in economics and see whether there are flaws in those principles that have damaged the theoretical core of economics. Professor Blaug teaches at the University of London and the London School of Economics and is best known as the author of the justly celebrated *Economic Theory in Retrospect.* His basic conclusion is that economists have a satisfactory methodological program but do "not practice what they preach."

This book is a joy to read. Blaug is that anomaly in economics, a literary stylist. His prose is imbued by what is known in operatic critical circles as "face"—that is, the ability to transmit personality through a communications medium. Readers of Blaug's earlier works will be reaffirmed in their belief in his erudite scholarship and be reacquainted with his sometimes pungent wit.

Blaug's intended audience is the first-year graduate student or honors undergraduate. This book will prove a useful supplementary text for courses on research methodology or history of thought. However, economists desiring an introduction to economic methodology will also find it a valuable overview.

Blaug begins with a brief survey of recent developments in the philosophy of science. He then discusses the literature on economic methodology and "the troublesome question on the logical status of welfare economics." An appraisal of the neoclassical research program is given next in the context of the prior concepts. Finally, Blaug makes his own personal statement on the health of economics as a science and notes where improvements can be made.

The most important section of the book is the opening chapter surveying the philosophy of science because it is here that Blaug develops his methodological criteria. His discussion focuses on the work of Popper, Kuhn, and Lakatos. Economists with no background in this area will be surprised at the pervasive influence Sir Karl Popper has had on their discipline. He developed the falsification criterion, which was designed to subject theories to severely critical testing. The idea of falsification is: "whenever we try to propose a solution to a problem, we ought to try as hard as we can to overthrow our solution rather than defend it."[1] Popper advocates an almost heroic commitment to self-criticism that is beyond the grasp of most mortals. Realizing this, he places faith in others to "supply the criticism for us if we fail to supply it ourselves."[2] Popper also calls for a ruthless abandonment of theories that have failed to survive tests to refute them. Scientific theories, according to Popper, should be placed in loose-leaf notebooks rather than chisled in stone. Popper's statements make clear that the principle of falsification was a necessary (perhaps even sufficient!) condition for Friedman's "Essay on the Methodology of Positive Economics," especially in the context of Friedman's dictum that unrealistic assumptions are acceptable if the result is a hypothesis that leads to falsifiable predictions.

In contrast to Popper's dialectical view of the history of science where hypotheses are continually undergoing tests of validity is Thomas Kuhn's theory of scientific revolutions. Kuhn believes progress in the natural sciences was more a result of "theological" conversion than of intellectual advances. Development in the natural sciences has been a process of substitution by completing paradigms not rationally comparable. By paradigm, Kuhn means a *Weltanschaung*—"the entire constellation of beliefs, values, and techniques" shared by all the members of a given scientific community.

Kuhn has a catastrophe theory of scientific progress. Science is marked by gradual evolution punctuated by abrupt, unpredictable, and discontinuous jumps from one paradigm to another. For Kuhn, the history of science is a nondifferentiable function. Later criticisms that this view of history is not substantiated by revolutions such as Copernicus' and Newton's forced Kuhn to modify his views. Kuhn now holds that mutual incomprehension between scientific groups is only a question of degree and that paradigms do not instantaneously replace each other, but emerge preeminent through competition over time. These second

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[1] Karl Popper, *The Logic of Scientific Discovery*, New York: Harper Torchbooks, 1965. p. 7.
[2] Popper, p. 7.

thoughts, as Blaug notes, remove much of the originality in Kuhn's first statement.

It is from the late Imre Lakatos that Blaug draws his methodological insight. Lakatos, a former pupil of Popper, synthesizes concepts from Popper and Kuhn. Lakatos sees theories not as singular entities, but as connected to an integrated network of ideas which he calls a "scientific research program (SRP)." An SRP can be divided into rigid and flexible components called "a hard core" and the "protective belt," respectively. A hard core is the irrefutable and essential center deemed so by the methodological decision of the program's defenders, and the protective belt is the refutable and replaceable variant of the research program. An SRP may be progressive or degenerating. An SRP is theoretically progressive when every genesis of the program "predicts some novel, hitherto unexpected fact." If these predictions can be corroborated, then the SRP is empirically progressive. However, a program is degenerating when it accommodates new information with ad hoc adjustments to explain away any discrepancies. Lakatos is like Popper in that he views acceptance or rejection of a research program as determined through objective competition with a rival research program. Thus, he demonstrates why theories are replaced, an area where Kuhn is weak. Yet Lakatos is also like Kuhn in his building upon the paradigm idea to create an enlarged multidimensional concept in the SRP.

Blaug uses the Lakatos methodology to evaluate the neoclassical research program—which, as Blaug sees it, is predominantly defended by University of Chicago economists. Neoclassical theory, in general, is considered by Blaug as a progressive SRP because of its integrity in lending itself to testable predictions. However, neoclassical theory is also a progressive program because in many instances there is no competing SRP for comparison. Some, myself included, will find Blaug's assessment that monetarism is a degenerating research program premature. I suspect Blaug's Keynesian sympathies have filtered through his critical standards.

Blaug is, I think, right when he asserts the need for the profession to develop theories that yield unambiguously verifiable results and to be willing to test them. It is here that we do not "practice what we preach." The causal relation between hypothesis and testing is too often reversed. Surprisingly, Blaug places considerable faith in econometrics to test competing theories. In practice, unfortunately, cheap computer time and resultant data mining have played a large part in reversing the causal arrow.

Blaug's book is useful because it demonstrates the value of appraising the history of economic theory as a progression of choices between competing social programs. Lakatos' contribution, in my view, is to fill in many of the gaps in Kuhn's explanation of scientific revolutions. However, despite Blaug's advocacy, I see some problems in Lakatos' program as a touchstone for researchers. First, by embracing elements of Kuhn's work, Lakatos does not completely escape the concomitant charges against Kuhn of vagueness and tautology. Lakatos' hard core is as uncomfortably amorphous and mystical as is Kuhn's paradigm. Second, the evolutionary path through which a core "hardens" remains undefined. Third, he has some difficulty defining what constitutes a SRP. For example, are monetarist and Keynesian SRP's really separate, or are they part of a larger SRP? Finally, there is a danger that the greater flexibility of Lakatos' principles compared with Popper's might allow them to be diluted beyond value.

Lakatos' methodology may provide the best support for the economic historian where a macro-assessment of a body of thought over time is desired, but Popper's floor appears to me best for the researcher solving a specific problem or testing a specific theory. Blaug has shown us the tools for appraisal of economic theory; it is our responsibility to use them.

Resources and Development: Natural Resource Policies and Economic Development in an Interdependent World

Peter Dorner and Mahmoud El-Shafie, eds. Madison: The University of Wisconsin Press, 1980, 500 pp., $20.00

Reviewed by Gary C. Taylor*

A series of energy and food supply disruptions put natural resource issues on the agendas of the United States and of the international community during the seventies. But, when concerned citizens rushed to their libraries, they found that authoritative writings on natural resources scarcely mentioned international aspects and that those items on foreign affairs and economic development seldom mentioned natural resources. This disappointment was quickly perceived by authors in the Doom and Gloom and the Cornucopian schools, who rushed into print and thereby contributed to the paralysis in public policy formation.

This book is the capstone of a surprisingly successful international effort to help remedy this situation. In 1977, the University of Wisconsin joined with three sponsoring organizations in the Arab World to carry out a year-long series of seminars on resource and development issues. Scholars, practitioners, and students debated the issues, explored the disciplinary states of the art, clarified the positions of the developed and developing nations in nonstrident semantics, and pointed out the sobering challenges for international cooperation.

The proceedings of these seminars filled three volumes. Using this base, the authors developed the chapters of this book and subjected them to critical review by the seminar participants. The manuscript was then rigorously edited. The result is an extremely valuable reference for professionals, policymakers, and students.

The book consists of 15 chapters written by 12 authors—often in collaboration. These are organized in four sections dealing with natural resources, economic development, international cooperation, and analytical and policy redirections. Three are discipline-oriented chapters on topics including resource economics, market imperfection, and international law. Three are multidisciplinary, more general chapters dealing with topics including the availability of mineral resources, the North-South confrontation, and the developing countries' concern with multinational corporations (MNC), for example, the short time horizons of these MNCs and their exclusive control of technology and managerial know-how. All chapters are well written and well documented.

In the foreword, Ali Attiga poses the problem facing economies dependent on nonrenewable resources: "The main challenge for the future of these countries is managing their depletable resources in a way that will allow their societies to develop and join in the passage over the bridge to the world of renewable energy sources." This is, of course, a challenge facing all countries.

The "bridge" is not to be discovered; it is to be constructed. The necessary transformations of behavior patterns, institutions, and technologies are exceedingly complex, particularly in more traditional societies. Commenting on the outlook for accomplishing these transformations, Peter Dorner finds a middle ground between unrelenting pessimism and unfounded optimism. He is guardedly optimistic.

This reviewer is somewhat less sanguine, based on his observation of our political process. Often our genius has been to turn qualitative or doctrinaire issues into economic debates where tradeoffs can be discussed. However, with natural resource issues, which are usually essentially economic, we often exhibit an uncanny instinct to do the reverse. Obviously, the aspiring politician prefers to avoid doctrinaire issues requiring longrun solutions wherever possible. The confrontations over natural resources in international arenas suggest that this situation is not a uniquely American phenomenon.

I must admit to disappointment with the editors' concluding chapter. It is long on philosophy and short on prescription. I hasten to add, this was a naive reaction on my part bolstered by the competence of the proceeding papers. Obtaining the required international cooperation will be a difficult and continuing struggle.

Abdel-Rahman, in his chapter, suggests that international cooperation can be fostered at three levels, or "systems," of interaction: intergovernmental interaction, multinational corporations, and nongovernmental organizations and groups. This book would be useful to the actors at all three levels. I recommend it for individuals who recognize that the future is not what it was perceived to be 10 years ago and who want to reorient their perceptions.

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