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Empirical Implications of Discontinuous Policy: The Case of Acreage Controls in Agriculture

Robert D. Weaver

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Agricultural Economics and Rural Sociology Department
College of Agriculture
The Pennsylvania State University
University Park, Pennsylvania 16802

A general problem involved with measuring economic relations over a time series is that the economic environment or regime in which decisions are made may change. When this occurs the determinants of decisions may change and/or the relationship between choices and particular determinants may change. In what has become a well-known work, Nerlove [1958] carefully chose the time series over which he measured acreage supply response to exclude any years during which acreage control programs were in effect. Understandably, he argued that the determinants of acreage decisions made during a free market period differ from the determinants of such decisions during years when government programs were operative. Despite this early recognition of the potential problems involved with estimation of acreage choice functions over a time series encompassing various economic regimes, subsequent literature has ignored many of them. Traditionally, studies which have estimated acreage response functions over time series have failed to investigate the hypothesis that several different regimes may have existed. Examples of such studies include national aggregate studies by Lidman and Bawden [1974], Garst and Miller [1975], Just [1973], and Houck and Ryan [1972]. In each case, despite variation of government policies and response to them, parameters of estimated acreage response functions were assumed to be stable over the entire time series involved.[1] The premise of this paper is that the existence of an effective acreage control policy implies the existence of an economic regime which is distinct from that which might exist in the absence of that policy.

A careful review of acreage policies suggests that another important characteristic of the policies has been overlooked. Although policy details have varied over the years the generalization may be made that they offered producers a choice between two sets of incentives depending on the level at which acreage of the controlled crop was set. During typical programs if the producer planted in excess of the allotment, he would face market prices, penalties and ineligibility for various program benefits. If instead, acreage was set equal to or less than the allotment a different set of government determined incentives were faced.[2] It will be argued that this discontinuity in incentives over the range of acreage implies that under such policies, decisions are not continuous functions of any one set of incentives.

The objectives of this paper are to investigate the implications of these two complications for empirical measurement of supply response and to assess their importance through empirical tests. The studies cited above have focussed on national aggregate acreage response models. As will be illustrated, the later problem re-emphasizes the need for sub-national aggregate models. For this reason, aggregate data for major wheat producing states will be employed to re-examine the relationship between acreage decisions and their determinants.

Section 2 will address the specification of wheat acreage response during various post-war policy regimes by presenting a theory of choice under discontinuous policy. From this theory hypotheses will be derived concerning the existence of different regimes during the time series as well as different sets of determinants of acreage response within particular regimes. In section 3 we will empirically investigate these hypotheses by presenting winter and spring wheat acreage response functions for major wheat producing states based on an annual time series of 1948-1974.

II. Specification of Wheat Acreage Response Under Discontinuous Policy

There exist three general regimes of wheat policies which may be distinguished during the post-war period. With the exception of 1950, the years 1948-1953 and 1974 may be characterized as free market years during which acreage to be planted was derived from a consideration of market information and was unconstrained by policy restrictions. During the years 1971-1973, the wheat set-aside programs required diversion of crop land for eligibility for price support. However, both the support mechanism and the role of allotments were changed to allow market prices to play a greater role in income determination and land allocation. A minimum set-aside of cropland was the only requirement for eligibility for support. Although price support was only available on acreage less than or equal to the allotment, cropland remaining after the diversion could be allocated without further restriction. Prices were supported at 100% parity by deficiency payments. Thus, one may conclude that as during free market periods, acreage was allocated during the set-aside programs after a consideration of relative prices receivable. We will label these years as Regime 1. From 1954 to 1963 the existence of quotas with penalties for planting in excess of the allotment suggests a second regime (Regime 2) may have existed. Although quotas were not in effect in 1950 and 1964, allotments were maintained as a basis of qualification for price support. Thus, the years 1950, 1954-1964 will be grouped by years during which acreage allocation may have been controlled by policies. We will label these years as Regime 2. Finally, from 1965-1970 government policies employed marketing certificates and allotments, however substitution and excess production provisions may have effectively relaxed the constraint placed on acreage decisions by acreage controls. We will distinguish these years as Regime 3. To proceed, we will characterize decisions during each of these three regimes.

Regime 1: Free Market Years (1948, 49, 51-53, 71-74)

We will maintain the hypothesis that firms allocate acreage between two types of crops: wheat (w) and its alternatives (A) to maximize expected profits subject to technological, total land and other fixed factors, and any government policy constraints which may be existent and binding. During a free-market period, the following reduced forms for allocation of acreage to crop i may be derived from the hypothesized choice problem:

1) $a_{ti} = g(E(P)_t, r_t, \theta_t)$ $\quad\quad i = 1, \ldots m, \; t \varepsilon \tau_1$

where $E(P)_t$ is an 1 x m vector of expected prices for output vector Q.

r_t is a 1 x n vector of factor prices for a variable input vector X.

θ_t is a 1 x s vector of factors of production which are fixed in the short run.

τ_1 is the set of years t in regime 1.

Regime 2: Quota Years (1950, 1954-64)

During the quota years 1954-1963 producers who did not exceed their wheat acreage allotments ($\bar{a}_w$) could place their production under non-recourse loans at a supported rate of R_w. Because the market price is uncertain, the expected support receivable $R_w^* = \max\,(R_w, E(P)_w)$. In addition, a penalty tax (t_w^1, a percentage of R_w) was imposed on any portion of the product of acreage planted in excess of the allotment which was marketed. Although this tax could be avoided by storing the excess, opportunity and storage costs were nevertheless incurred. To generalize, we may conclude that for each acre planted (harvested after 1955) in excess of the allotment the expected tax equalled the product of the farmer's normal yield (Y_w) and a per bushel tax rate t_w equal to the minimum of total storage cost per unit and t_w.[1] Thus, for all acreage in excess of the allotment, expected profit was reduced by a marginal tax on acreage. In 1950 and 1964 although the penalty tax was zero producers utilizing acreage in excess of their allotments, were ineligible for support and instead faced market prices.

The direct implication of this policy was to introduce a discontinuity in the relation between expected profits and acreage planted to wheat. This discontinuity is immediately apparent when we write the expected profit definition. During the Regime 2 we have:

$$
\begin{aligned}
2)\quad E(\pi) &\equiv [K^*_w Q_w + E(P)'_A Q_A - r'X]\,[1 - \alpha(a_w)] \\
&\quad + [E(P)_w Q_w + E(P)'_A Q_A - T_w A_w - r'X]\,\alpha(a_w) \\
&\equiv \{R^*_w[1 - \alpha(a_w)] + E(P)_w \alpha(a_w)\}\,Q_w + E(P)'_A Q_A - \alpha(a_w)\,T_w A_w - r'X \\
&\equiv P^*_w\,Q_w + E(P)'_A Q_A - \alpha(a_w)\,T_w a_w - r'X
\end{aligned}
$$

where Q_w, Q_A are quantities of wheat and a vector of quantities of alternative crops, and $E(P)_A$ is a vector of expected market prices for Q_A.

$R^*_w = \max\,(R_w, E(P)_w)$

X, r are vectors of variable inputs and their prices,

$T_w = [(a_w - \bar{a}_w)/a_w]\,Y_w t_w$

$\alpha(a_w) = \begin{cases} 0 & \text{if } a_w \le \bar{a}_w \\ 1 & \text{if } a_w > \bar{a}_w \end{cases}$

P^*_w is the expected price receivable for w.

We hypothesize that acreage is allocated by maximizing 2) subject to a technological constraint:

$$F(Q_w,\ Q_A,\ X,\ \theta) = 0$$

and a land constraint which is contained in the vector θ:

$$a_w + a_A \leq a.$$

The discontinuity of expected profit implies that in place of a single set of necessary conditions for maximum expected profit there exist two sets of conditions depending on the level of a_w.

In the textbook case, the necessary conditions provide a structural equation system or set of choice rules which establish a continuous relationship between incentives, constraints and optimal choices. When discontinuity is introduced by control policy as described here, the best that may be hoped for is what might be labelled "locally continuous" relations between choices and the particular sets of incentives and constraints faced in various ranges of the controlled choice. That is, for $a_w \leq \bar{a}_w$ the necessary conditions relate acreage decisions to $(P^*_w, E(P)_A, r, \theta)$ and for $a_w > \bar{a}_w$ our model suggests decisions may be locally related to $(P^*_w, E(P)_A, r, t_w, \bar{a}_w, \theta)$. Although both sets of incentives play a rôle in the decision of whether to comply $(a_w \leq \bar{a}_w)$ or not $(a_w > \bar{a}_w)$, neither set of incentives is continuously related to the final choice of acreage utilization.[3] Thus, in the presence of the discontinuous policy we must acknowledge that any one of the following three reduced forms for choice of a_w might characterize a producer's choice of acreage in Regime $2(\tau_2)$:

3) $a_{wt} = f(P^*_{wt}, E(P)_{At}, r_t, \theta_t)$ if $a_{wt} < \bar{a}_{wt}$

4) $a_{wt} = \bar{a}_{wt}$ if $a_{wt} = \bar{a}_{wt}$

5) $a_{wt} = g(P^*_{wt}, E(P)_{At}, r_t, t_{wt}, \bar{a}_{wt}, \theta_t)$ if $a_{wt} > \bar{a}_{wt}$

Which of these cases is appropriate to characterize decisions during the quota period is an empirical question which past studies have not only failed to address, but also whose existence they have failed to recognize.

Regime 3: Certificate and Substitution Years (1965-1970)

Without going into equal detail, decisions made during the years 1965-1970 may be analyzed in an analogous fashion. In brief, we note that the fundamental difference between policies during these years and those of Regime 2 was the termination of the penalty tax and institution of a variable subsidy to be paid to those planting within their allotments. This subsidy amounted to the product of the difference between the parity price (P_{pw}) and the expected support receivable (R_w^*) and the normal yield (Y_w) on the farm. Thus, the amount of subsidy payment (S_w) varied with acreage planted up to the point at which allotment was exceeded. We may write this subsidy payment S_w as:

$$6)\quad S_w \equiv s_w a_w \equiv (P_{pw} - R_w^*)\, Y_w a_w \quad \text{for } a_w \leq \bar{a}_w$$

In addition, a substitution and an excess wheat provision were introduced which allowed for overplanting of wheat allotments. Under the substitution provision, wheat allotment acreage could be used for feed grains and vice versa. Alternatively, a producer who was relatively specialized in wheat could take advantage of the excess wheat option which allowed overplanting of the allotment by up to 50%. So long as the excess production was stored, eligibility for certificates on the allotment and loans on the entire production was preserved. Similarly, under the substitution provision, support was limited to permitted acres (all $a_w \leq \bar{a}_w$) while loans at R_w were available for the entire production.

The effect of these provisions for producers setting $a_w > \bar{a}_w$ was to shift vertically the expected profit by a lump-sum payment $\bar{S} = (P_{pw} - R_w^*)x$ $Y_w \bar{a}_w$ for all levels of acreage in excess of the allotment while preserving the discontinuity of the expected profit definition introduced by the certificate program. Because the shift of the objective function was vertical, the only decision affected was the choice of acreage if that choice would otherwise have been equal to the allotment. That is, if in the absence of these provisions it would have been optimal to set acreage equal to the allotment, it is possible that the vertical shift of the segment of the expected profit definition for acreage in excess of the allotment may have been sufficient to render overplanting optimal.

As during Regime 2, the discontinuities in the objective function introduced by the marketing certificate, substitution programs of Regime 3 imply that relations between choices and information may only be defined locally over various sets of information variables. When the optimal acreage to be planted to wheat fell short of the allotment a local relation of the following form may be defined for any year t in Regime 3 (τ_3).

7) $a_{wt} = f(s_{wt}, P_{wt}^*, E(P)_{At}, r_t, \theta_t)$ if $a_{wt} < \bar{a}_{wt}$

Where a corner solution was optimal, we have

8) $a_{wt} = \bar{a}_{wt}$

Where substitution or overplanting was optimal, the choice of acreage was independent of the lump-sum payment, and so may be written for a local range of information variables as:

9) $a_{wt} = h(P_{wt}^*, E(P)_{At}, r_t, \theta_t)$ if $a_{wt} > \bar{a}_{wt}$

Additional Complications

During the years 1956-1958 and 1965-1970, three different diversion programs were offered to encourage farmers to reduce their allotments. When diversion was mandatory (1962-1963) the requirement resulted in a lump-sum subsidy or tax depending upon whether any payment made exceeded the opportunity cost of such a diversion. In either case, the requirement did not affect the marginal conditions for optimal acreage allocation by a participant. Instead the only impact on decisions resulted from the reduction of the actual allotment to an effective level equal to the difference of the actual allotment and the required diversion. The effect of this was to shift the point of discontinuity in the objective function.

Similarly, voluntary diversion programs (1956-58, 1962-66, 1969, 1970) reduced allotments to some effective level. However, these programs introduced the additional complication of appending an inequality constraint to the choice problem. Although the acreage which could be diverted was constrained between a minimum and a maximum, the producer could be expected to chose any amount between these points. That is, because the voluntary diversion payment rate (d) became the opportunity cost of allocating land to crops, maximization of expected profit required allocating land to crops until the marginal value product of that land fell to equal the diversion payment rate. At that point, all additional land would be allocated to diversion so long as this additional land did not exceed the maximum diversion allowable (MA_d). When the optimal diversion exceeded the maximum, the constraint became binding and thus, MA_d became a determinant of acreage allocation by participants as represented by 3), 4), 7), 8) or 9). Alternatively, where the maximum allowable diversion failed to place a binding constraint on land allocation, it can be shown that the diversion payment rate would play a role in the land allocation decision of an expected profit maximizing participating producer. We conclude that the discontinuous nature of diversion opportunities introduced discontinuity to the relation between land allocation decisions and their determinants. Thus, the impact of the inequality constraint on diversion was to introduce additional discontinuity in the objective function. As in the cases already discussed, this discontinuity implies that the reduced form relating choices to information is also discontinuous. Estimation over a time series is further complicated by the fact that the nature of the inequality constraints, and the diversion payment rates changed over time. To proceed, we will recognize that when the diversion constraint was binding the maximum diversion established by policy determined the diversion, otherwise the diversion payment rate together with other determinants of acreage allocation determined diversion.

An additional complication remains. The above reduced forms are appropriate for characterization of the choices of a single producer or a homogeneous group of decision makers. However, although we might safely assume all producers face similar technology, differences in scale and other factors which are fixed in the short-run would imply that we may expect that different farmers may find under-planting, over-planting or corner solutions ($a_w = \bar{a}_w$) to be optimal. If choices could be continuously related to a single set of information variables, and producers could be assumed to face a common technology, the inclusion of a vector of fixed factors would control for this effect. However, where different levels of fixed factors imply that decision-makers find themselves in different ranges of a discontinuous objective function, different locally continuous choice functions would describe their decisions. In such a case, aggregation might render parameters describing the responsiveness of choices to a particular determinant unidentifiable. We will label aggregate acreage response (A_{wt}) in this case of heterogeneity as "mixed." As a simple example of the identification problem involved, if we ignore diversion complications during the quota years, then each of equations 3) - 5) might have characterized decisions of a subset of producers. Where the number of producers whose choices are characterized by 3), 4) or 5) in year t are respectively n_t, m_t p_t we may write the aggregate acreage as:

$$10)\quad A_{wt} = n_t\{g(P^*_{wt}, E(P)_{At}, r_t, \theta_t, MA_{dt}, d_t, t_{wt}, \bar{a}_{wt})\}$$

$$+ m_t\{f(P^*_{wt}, E(P)_{At}, r_t, \theta_t)\}$$

$$+ p_t\{\bar{a}_{wt}\} \qquad\qquad t \in \tau_2$$

where MA_{dt} and d_t have been introduced to 3) as suggested by the discussion above. Clearly, if the number of producers (n_t, m_t, p_t) in each case changed each year, as determinants in different cases changed, we would be unable to identify the response of aggregate acreage to changes in various determinants by estimating 10). Similar, though more complicated, "mixed" cases may be defined for aggregate acreage response during the marketing certificate, substitution years as well as when diversion programs were operative. To summarize this section, although past studies have presumed parameters are stable over time series composed of several different policy regimes, we have found theoretical basis for the hypothesis that 1) different policies imply different choice functions, and so even in a simple world their parameters may be expected to vary across these policy regimes, and 2) alternative sets of determinants may in a locally continuous sense be related to choices within any particular regime when policy is discontinuous.

III. Measurement of Acreage Response under Discontinuous Policy

The third part of this paper will illustrate the impact of the above complications on estimation of aggregate acreage response. In order to proceed, we will focus solely on expected prices receivable, allotments and technology as determinants of aggregate acreage response. Although the above theoretical analysis suggests input prices (r) and levels of fixed factors (θ) are important determinants, we shall exclude them from the present analysis in order to retain focus on the implications of the existence of multiple policy regimes over time and discontinuity within each of these regimes. To the extent that these excluded determinants are uncorrelated with those included, their exclusion will not bias estimated parameters.

We will demonstrate the importance of our theoretical results by establishing a general unrestricted model and expressing the alternative characteristics as sets of parameter restrictions on this general model. This procedure will allow use of F-tests to test the validity of the alternative hypotheses. Specifically, we will pose the "mixed" case in each regime as the general unrestricted form. That is, during each of regimes 2 and 3 we might find that both market and policy incentives and constraints are determinants of aggregate acreage response due to the aggregation over heterogeneous response to both acreage and diversion contraints. These hypotheses may be written as a linear representation of 10) during Regime 2:

$$11) \quad A_{wt} = \alpha_2 + \beta_2 P^*_{wt} + \gamma_2 \bar{A}_{wt} + \delta_2 D_t + \lambda_2 MA_d + \mu_2 d_t + \varepsilon_t \quad t \varepsilon \tau_2$$

if we assume $n_t = n$, $m_t = m$, $p_t = p$.

where linear homogeneity of prices has been imposed leading to appropriate

re-definition of P^*_{wt} as a relative price scaled by $E(P)_{At}$.

MA_d represents the maximum diversion constraint

d represents the diversion payment rate/bushel

D represents a time trend

ε_t represents a stochastic aggregate measurement error.[5]

A similar aggregation of 7) - 9) for Regime 3 would allow us to write:

$$12)\quad A_{wt} = \alpha_3 + \beta_3 P^*_{wt} + \gamma_3 \bar{A}_{wt} + \delta_3 D_t + \mu_3 d_t + \varepsilon_t \quad t\varepsilon\tau_3$$

while for Regime 1 we may unequivocally write the traditional free market acreage supply function

$$13)\quad A_{wt} = \alpha_1 + \beta_1 P^*_{wt} + \delta_1 D_t + \varepsilon_t \quad t\varepsilon\tau_1$$

In addition to these models (which we will label jointly as model 0), if we augment equations 3) - 5) and 7) - 9) to include appropriate diversion variables, then linear representations of the resulting equations constitute alternative hypotheses of acreage response during Regimes 2 and 3. For example, during Regime 2 producers may have found it optimal to participate, but allotments may not have imposed a binding constraint on acreage allocation. If such were the case, a linear aggregate representation of 3) could be hypothesized and tested as a parameter restriction ($\alpha_2 = 0$) on 11). Alternatively, allotments may have been binding during Regime 2, a hypotheses which can be represented by the restriction of $\alpha_2 = \beta_2 = \delta_2 = 0$. Although such an approach is quite simplistic it serves as an adequate basis for illustrating the point that the discontinuity and changing nature of control policy during the post-war period implies that parameter variation over time may be expected and that several locally continuous relations may characterize choices made under any particular policy. We could, of course, go a step further and investigate the specific role of diversion variables by recognizing that for each time period where acreage is less than or equal to allotment the diversion constraint may have been binding, not binding or in the aggregate may have been binding only for some farmers. To proceed, we will not investigate this issue and instead maintain the hypothesis that the diversion programs had a mixed effect. Specifically, Table 1 presents parameter restrictions of the Model 0 which are consistent with each of the alternatives defined by 3) - 5) and 7) - 9). Testing these hypotheses is facilitated if we note that in several cases they are nested. That is, various models may be represented as further restrictions of other models in the Table 1. In such cases we shall proceed by testing the least restrictive model against the unrestricted model 0 and proceed to test the next more restricted model conditional upon the validity of the less restrictive model. The nesting of hypotheses in Table 1 is indicated in Chart 1.

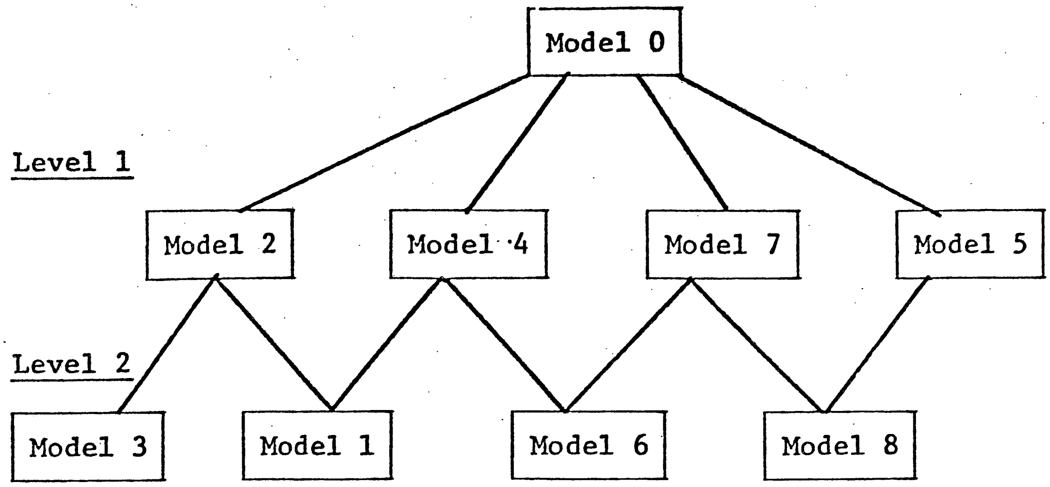
Where hypotheses are independent (not nested) our testing becomes more complicated. For example, if we are unable to reject two independent sets of restrictions on the unrestricted model, then we are left with two alternative hypotheses which can not be forced into direct competition and tested against each other using parametric tests. Unfortunately, when hypotheses are independent their differences can not be summarized as a set of point restrictions on parameters whose distributions are known. This renders typical statistical tests inappropriate. However, the comparison of alternative independent composite

Table 1. Alternative Models and Parameter Restrictions* for Acreage Response During Regimes 2 and 3.

Regime 3 (1965-1970)	Regime 2 (1950, 1954-1964)		
	Underplanting	Mixed	Allotment Binding
Underplanting	$\alpha_1 = \alpha_2 = \alpha_3$ $\beta_1 = \beta_2 = \beta_3$ $\delta_1 = \delta_2 = \delta_3$ $\gamma_2 = \gamma_3 = 0$	$\alpha_1 = \alpha_3$ $\beta_1 = \beta_3$ $\delta_1 = \delta_3$ $\gamma_3 = 0$	$\alpha_1 = \alpha_3$ $\beta_1 = \beta_3$ $\delta_1 = \delta_3$ $\gamma_3 = 0$ $\alpha_2 = \beta_2 = \delta_2 = 0$
Mixed	$\alpha_1 = \alpha_2$ $\beta_1 = \beta_2$ $\delta_1 = \delta_2$ $\gamma_2 = 0$	No Restrictions	$\alpha_2 = \beta_2 = \delta_2 = 0$
Allotment Binding	$\alpha_1 = \alpha_2$ $\beta_1 = \beta_2$ $\delta_1 = \delta_2$ $\beta_3 = \gamma_2 = 0$	$\alpha_3 = \beta_3$ $= \delta_3 = 0$	$\alpha_2 = \beta_2 = \delta_2 = 0$ $\alpha_3 = \beta_3 = \delta_3 = 0$

*Parameter restrictions are those which are sufficient to restrict model 0 to consistent with each respective alternative model.

Chart 1. Nesting Alternative Models of Post-War Acreage Response.



hypotheses amounts to what Thiel [1957] has labelled specification analysis. Recognizing this we employ Thiel's residual variance criterion to discriminate between alternative, though independent hypotheses. The validity of this approach is conditional upon the set of alternatives compared being an exhaustive set.

The sample employed is one of state level aggregate data for each of the major winter and spring wheat growing states for the annual time series of 1948-1974. Although error terms might be correlated between states and, thereby, call for a Zellner [1962] efficient estimation procedure, software for ZEF estimation of the set of equations for all 13 states was unavailable. While such an approach might allow an increase in the efficiency of estimates, ordinary least squares estimators are unbiased and was chosen as the estimation method. However, in order to allow for parameter restrictions across regimes the equations hypothesized for each of regimes 1, 2, and 3 were stacked before estimation.

The results of the tests of nested hypotheses for each state are reported in Table 2. The F-statistics represent the weighted percentage change in the sum of squared error of the model postulated under the alternative hypothesis vs. that held as the null hypothesis. Thus, the F-statistics present a basis for testing the parameter restrictions noted in Table 1. That is,

$$F_{q,n-K_u} = \frac{e'e_R - e'e_U}{e'e_U} \cdot \frac{K_U - K_R}{n-K_U}$$

where $e'e_R$ is the sum of squared errors for the restricted model (null)

$e'e_U$ is the sum of squared errors for the unrestricted model (alternative)

K_U, K_R are the number of parameters estimated in the unrestricted and restricted models, respectively

n is the number of observations.

Table 2. F-Statistics for Nested Hypothesis Tests

Model Maintained as		Branch 2			Branch 4			Branch 7			Branch 5	
Null Hypothesis:		2	3	1	4	1	6	7	6	8	5	8
Alternative Hypothesis:		0	2	2	0	4	4	0	7	7	0	5
Degrees of Freedom: $(n-K_U,\ K_U-K_R)$		(14,4)	(18,3)	(18,4)	(14,4)	(18,4)	(18,3)	(14,3)	(17,4)	(17,3)	(14,3)	(14,3)
State	Wheat Type*											
North Dakota	SW	87.586			16.436			1.475	16.887	7.468	9.673	
South Dakota	SW	52.387			3.413	33.936	5.068	11.241			3.714	6.418
Montana	SW	16.249			11.332	2.199	2.152	7.552	6.583	2.422	5.363	
Colorado	WW	4.183	1.654	0.361	0.440	4.980	7.765	9.239			1.984	
Illinois	WW	0.555	4.227	3.327	1.233	2.205	2.883	4.094	0.808	3.373	5.400	
Indiana	WW	2.156	1.257	0.834	1.071	2.101	2.379	3.356	0.668	1.676	2.841	2.188
Kansas	WW	3.641	1.528	0.357	0.831	3.507	1.280	1.610	0.782	1.631	2.211	1.097
Montana	WW	1.536	3.328	6.593	3.445	3.544	5.819	15.961			5.774	
Nebraska	WW	2.797	2.613	1.143	0.900	3.576	1.730	2.182	0.798	3.074	4.310	1.269
Ohio	WW	3.577	0.921	0.298	0.878	3.257	0.646	0.713	1.013	2.301	2.671	0.502
Oklahoma	WW	7.534			0.987	8.407	1.626	2.096	0.883	2.477	3.549	1.238
Texas	WW	4.673	1.223	0.780	1.233	4.618	1.673	2.750	0.790	1.697	2.997	3.530
Washington	WW	9.028	0.978	0.927	1.026	8.763	3.481	5.331			3.590	

*SW symbolizes spring wheat; WW, winter wheat.

As an example of the testing procedure, suppose we begin by testing the null hypothesis that model 2 characterizes the aggregate relation against the alternative hypothesis that model 0 is appropriate. If we reject model 2, we have evidence supporting the validity of model 0; however, model 4, 5 or 7 may not be able to be rejected by the current sample. Thus, we proceed to test model 4 as the null versus model 0. Should we fail to be able to reject model 4 we may then pose it as the alternative to the further restricted models nested under it in Chart 1, e.g., model 1 or model 6. However, despite our inability to reject model 4, we must also consider models 5 and 7 which are restrictions of model 0, but independent of model 4. Following this procedure Table 2 only reports F-statistics for those models on a branch which are nested under hypothesized models which could not be rejected.

Although each model could be tested directly against model 0, the nested structure of the hypotheses allows us to place these alternative in direct competition. In order to control for the overall level of significance of the nested tests, we will establish an overall critical level of significance at .02. Noting that the probability of type 1 error for a test at the second level of nesting (e.g., model 3 vs. model 2) is conditional upon the probability of type 1 error at the first level (e.g., model 2 vs. model 0), we will attempt to minimize the overall probability of type 1 error by allocating the overall level of significance between the two stages of tests as follows: level 1: .015; level 2: .005. Critical values for the F distribution employed are reported in Table 3. For each state (except North Dakota) we are left with several models which cannot be rejected as restrictions of models under which they are nested.

Table 3. Critical Values of $F(n-K_U, K_U-K_R)$.

$(n-k_U, K_U-K_R)$	.05	.02	.015	.005
14,3	3.34	4.68	5.12	6.68
14,4	3.11	4.27	4.65	6.00
17,3	3.20	4.40	4.79	6.16
17,4	2.96	4.00	4.34	5.50
18,3	3.16	4.33	4.71	6.03
18,4	2.93	3.93	4.25	5.37

Although this does allow us to eliminate a large number of models as inconsistent with the current sample, the remaining models are independent of each other. To proceed, we employ Thiel's [1971] residual-variance criterion.[6] That is, if one of the remaining models is the model which underlies the population from which the sample was drawn (i.e., the "true" model), then of the set of models remaining it will have the minimum sum of squared residuals after adjusting for degrees of freedom (n-K). Specifically, where $S^2 = e'e/n-K$

we have

$$S_T^2 = \min\ (S_T^2,\ S_F^2)$$

where T indicates the true model

F indicates a misspecified model.

The residual-variances for models which could not be rejected through nested tests are reported in Table 4. The model for each state which satisfies the residual-variance criterion is reported in Table 5 along with model 0.

Two general conclusions may be immediately drawn from Table 5. First, only in the case of Illinois did the current sample fail to reject models in which parameters are stable across the three policy regimes. Secondly, within regime 2 and 3 different characterizations of the choice of acreage were found appropriate in different states. This result lends support to the theoretical results of section 2 that within any regime there exist three different choice relations (e.g., 3)-5) or 7)-9)) which may characterize choice. In more specific terms, we find evidence that supports the conclusion that during regime 2 ('50, '54-'64) farmers underplanted their allotments of winter wheat in Illinois and Montana while in Nebraska,

Table 4. Residual-Variance

State	Model	Degrees of Freedom	S^2
South Dakota(SW)	M6	21	.00252
	M8	20	.00251
Montana (SW)	M6	21	.01496
	M8	20	.0088
Colorado(WW)	M1	22	.0214
	M3	21	.02652
	M5	17	.01554
	M6	21	.02442
Illinois (WW)	M1	22	.02718
	M3	21	.02791
	M6	21	.02830
Indiana (WW)	M1	22	.02062
	M3	21	.02204
	M6	21	.02057
	M8	20	.02418
Kansas (WW)	M1	22	.02435
	M3	21	.02965
	M6	21	.01740
	M8	20	.01686
Montana(WW)	M1	22	.02168
	M3	21	.01452
	M6	21	.02503
Nebraska (WW)	M1	22	.01293
	M6	21	.00859
Ohio (WW)	M1	22	.01663
	M3	21	.01884
	M6	21	.01119
	M8	20	.01335
Oklahoma (WW)	M1	22	.03085
	M3	21	.03315
	M6	21	.01432

Table 4. Continued

State	Model	Degrees of Freedom	S^2
Texas (WW)	M1	22	.03121
	M3	21	.03356
	M6	21	.02064
	M8	20	.02376
Washington (WW)	M1	22	.02794
	M3	21	.02823
	M6	21	.01510

Table 5. Summary of Estimated Models.

State:	North Dakota		South Dakota		Montana (SW)		Colorado	
Model:	M8	M0	M8	M0	M4	M0	M4	M0
Coefficient								
α1	7.307 (3.250)	7.892 (4.952)	4.74 (5.683)	5.080 (8.982)	6.704 (2.383)	11.870 (6.906)	4.680 (6.478)	4.630 (5.487)
α2		3.277 (0.507)		1.800 (0.630)	16.140 (1.879)	8.953 (1.797)	14.572 (4.266)	14.813 (4.032)
α3		-1.958 (-0.330)		5.852 (1.810)	$\alpha_3 = \alpha_1$	6.752 (0.768)	$\alpha_3 = \alpha_1$	6.121 (0.998)
β1	3.998 (2.923)	3.343 (3.384)	1.956 (3.727)	1.792 (5.153)	1.509 (1.121)	-2.128 (-2.359)	0.529 (1.620)	0.622 (1.378)
β2		0.550 (0.569)		0.409 (0.652)	-2.066 (-0.816)	-1.539 (-1.099)	-1.777 (-3.110)	-1.771 (-2.893)
β3		-2.453 (-1.103)		0.280 (0.241)	$\beta_3 = \beta_1$	1.358 (0.242)	$\beta_3 = \beta_1$	1.310 (0.960)
δ1	-0.335E-01 (-2.060)	-0.291E-01 (-2.525)	-0.737E-01 (-10.085)	-0.718E-01 (-14.929)	-0.888E-01 (-3.625)	-0.959E-01 (-7.057)	-0.357E-01 (-4.569)	-0.387E-01 (-4.158)
δ2		-0.517E-01 (-0.807)		-0.456E-01 (-1.776)	-0.184 (-2.124)	-0.119 (-2.421)	-0.134 (-3.758)	-0.134 (-3.507)
δ3		0.126 (1.401)		-0.688E-01 (-1.822)	$\delta_3 = \delta_1$	-0.534E-01 (-0.637)	$\delta_3 = \delta_1$	-0.827E-01 (-1.081)
γ2	0.951 (42.469)	0.819 (2.951)	0.762 (26.256)	0.835 (2.798)	-0.114 (-0.130)	0.566 (1.118)	-0.543 (-1.005)	-0.652 (-1.106)
γ3	1.050 (36.299)	0.475 (2.716)	0.621 (16.508)	0.992 (0.499)		-0.594 (-1.814)		0.353 (0.850)

Table 5. Continued

State:	North Dakota		South Dakota		Montana (SW)		Colorado	
Model:	M8	M0	M8	M0	M4	M0	M4	M0
Coefficient								
λ	0.252	0.512	0.967E-01	0.418	0.194	1.472	-0.250	-0.636
	(1.492)	(3.146)	(0.428)	(2.143)	(0.764)	(4.577)	(-1.799)	(-1.667)
μ	-0.465	-0.926	-0.172	-0.323	-0.289E-01	-1.181	-0.973E-01	0.232
	(-1.800)	(-3.843)	(-1.362)	(-2.888)	(-0.172)	(-4.416)	(-1.250)	(1.059)
R^2	.9439	.9806	.9498	.9855	.7688	.9454	.7764	.8014
DUR-WAT	1.742	2.588	1.829	1.903	(1.250)	1.486	1.488	1.455
Price Elasticity								
Regime 1	.472	.3948	.825	.756	.606	-.740	.199	.247
Regime 2	0	.100	0	.268	-1.008	-.751	-.839	-.836
Regime 3	0	-.307	0	.164	.606	.759	.199	.446

Table 5. Continued

State:	Illinois		Indiana		Kansas		Montana (WW)	
Model:	M1	M0	M6	M0	M8	M0	M3	M0
Coefficient								
α1	1.427 (2.012)	0.658 (0.693)	0.661 (0.730)	0.394 (0.446)	19.208 (5.407)	18.507 (5.475)	-0.996 (-0.948)	-2.689 (-2.629)
α2	$\alpha_2 = \alpha_1$	1.679 (0.616)		-1.161 (-0.386)		-1.534 (-0.816E-01)	$\alpha_2 = \alpha_1$	-7.743 (-2.290)
α3	$\alpha_3 = \alpha_1$	1.477 (0.121)	$\alpha_3 = \alpha_1$	2.411 (0.233)		-0.176 (-0.506E-01)		-5.832 (-0.834)
β1	0.538 (1.645)	0.989 (1.864)	1.278 (2.744)	1.245 (2.510)	2.550 (1.327)	2.757 (1.526)	0.473 (0.876)	1.335 (2.328)
β2	$\beta_2 = \beta_1$	0.126 (-0.269)		-0.555E-01 (-0.126)		-2.326 (-1.019)	$\beta_2 = \beta_1$	-0.765 (-0.774)
β3	$\beta_3 = \beta_1$	5.177 (0.932)	$\beta_3 = \beta_1$	2.759 (0.613)		12.849 (1.537)		5.780 (2.793)
δ1	-0.607E-02 (-0.832)	-0.339E-03 (-0.429E-01)	-0.114E-01 (-1.455)	-0.588E-02 (-0.799)	-0.158 (-3.494)	-0.149 (-3.460)	0.434E-01 (4.941)	0.521E-01 (6.424)
δ2	$\delta_2 = \delta_1$	0.224E-02 (-0.770E-01)		0.271E-01 (0.737)		0.111 (0.522)	$\delta_2 = \delta_1$	0.120 (3.478)
δ3	$\delta_3 = \delta_1$	-0.416E-01 (-0.318)	$\delta_3 = \delta_1$	-0.412E-01 (-0.368)		-0.917E-01 (-0.224)		0.142E-01 (0.189)
γ2		-0.188E-01 (0.266E-01)	1.069 (16.099)	0.812 (1.048)	1.019 (20.687)	0.869 (1.267)		0.949 (2.576)
γ3		-1.067 (-1.087)		-0.818 (-0.793)	1.005 (15.608)	0.442 (0.942)	0.643 (20.991)	0.433 (1.936)

Table 5. Continued

State:	Illinois		Indiana		Kansas		Montana (WW)	
Model:	M1	M0	M6	M0	M8	M0	M3	M0
Coefficient								
λ	0.507	0.684	0.440	0.942	-0.622	-0.567	0.171E-01	0.341
	(1.184)	(1.317)	(1.878)	(1.668)	(-1.659)	(-1.346)	(0.216)	(1.478)
μ	-0.135	-0.172	-0.573E-01	-0.210	1.563	0.993	-0.160	-0.245
	(-0.926)	(-1.116)	(-0.872)	(-1.497)	(1.707)	(0.998)	(-2.172)	(-1.299)
R^2	.4020	.7032	.5680	.7631	.6027	.7567	.6992	.8655
DUR-WAT	1.239	2.304	1.322	1.6949	1.7312	1.894	2.466	2.536
Price Elasticity								
Regime 1	2.20	.555	1.075	.992	.237	.256	.28129	.796
Regime 2	2.20	1.233	0	-.051	0	-.275	.28129	-.453
Regime 3	2.20	5.848	1.075	2.59	0	1.13	0	2.421

Table 5. Continued

State:	Nebraska		Ohio		Oklahoma		Texas	
Model:	M6	M0	M6	M0	M6	M0	M6	M0
Coefficient								
α1	6.510 (7.450)	6.822 (7.666)	1.743 (1.816)	1.631 (1.489)	3.294 (2.310)	3.504 (2.488)	6.089 (3.858)	5.257 (3.429)
α2		5.631 (1.505)		2.981 (0.644)		4.196 (0.582)		-3.971 (-0.391)
α3	$\alpha_3 = \alpha_1$	-2.811 (-0.289)	$\alpha_3 = \alpha_1$	1.694 (0.174)	$\alpha_3 = \alpha_1$	-4.946 (-0.351)	$\alpha_3 = \alpha_1$	6.899 (0.419)
β1	1.054 (2.221)	0.695 (1.252)	1.597 (3.323)	1.487 (2.444)	2.102 (3.993)	1.745 (2.950)	1.893 (2.563)	1.525 (1.893)
β2		-0.728 (-1.131)		-0.182 (-0.325)		-1.056 (-1.101)		-2.515 (-1.708)
β3	$\beta_3 = \beta_1$	5.528 (1.821)	$\beta_3 = \beta_1$	3.538 (0.888)	$\beta_3 = \beta_1$	5.466 (1.949)	$\beta_3 = \beta_1$	6.242 (1.675)
δ1	-0.663E-01 (-7.702)	-0.645E-01 (-6.991)	-0.295E-01 (-3.465)	-0.245E-01 (-2.649)	0.523E-02 (0.316)	0.105E-01 (0.588)	-0.479E-01 (-2.385)	-0.256E-01 (-1.123)
δ2		-0.433E-01 (-1.019)		-0.265E-01 (-0.486)		-0.988E-02 (-0.127)		0.103 (0.838)
δ3	$\delta_3 = \delta_1$	0.187E-01 (0.165)	$\delta_3 = \delta_1$	-0.333 (-0.313)	$\delta_3 = \delta_1$	0.485E-01 (0.287)	$\delta_3 = \delta_1$	-0.124 (-0.620)
γ2	1.055 (30.153)	0.389 (0.962)	0.961 (16.926)	0.202 (0.201)	1.013 (22.924)	0.590 (0.968)	0.978 (15.522)	1.255 (1.762)
γ3		-0.182 (-0.470)		-0.881 (-1.142)		0.420 (0.985)		-0.430E-01 (-0.670E-01)

Table 5. Continued

State:	Nebraska		Ohio		Oklahoma		Texas	
Model:	M6	M0	M6	M0	M6	M0	M6	M0
Coefficient								
λ	0.133	-0.839E-02	0.137	0.702	0.474E-01	-0.496E-01	-0.823E-02	0.316
	(1.110)	(-0.273E-01)	(0.695)	(1.291)	(0.312)	(-0.123)	(-0.366E-01)	(0.521)
μ	-0.196E-01	-0.186E-02	-0.607E-02	-0.277	-0.324E-01	-0.701E-01	-0.197	-0.491
	(-0.205)	(-0.827E-02)	(-0.818E-01)	(-1.522)	(-0.189)	(-0.164)	(-0.825)	(-0.804)
R^2	.8196	.8886	.7649	.8303	.6992	.8154	.5665	.7493
DUR-WAT	2.033	1.066	1.698	1.756	1.991	2.947	1.422	1.598
Price Elasticity								
Regime 1	.325	.213	1.079	.936	.410	.362	.413	.335
Regime 2	0	-.261	0	-.146	0	-.288	0	-.729
Regime 3	.325	1.711	1.079	2.761	.410	.948	.413	1.344

Table 5. Continued

State:	Washington	
Model:	M6	M0
Coefficient		
α1	0.906 (1.261)	1.121 (1.841)
α2		3.693 (1.079)
α3	$\alpha_3 = \alpha_1$	-4.492 (-0.750)
β1	0.417 (1.441)	0.228 (0.816)
β2		-1.454 (-2.481)
β3	$\beta_3 = \beta_1$	2.210 (1.872)
δ1	0.131 (2.389)	0.183E-01 (2.798)
δ2		-0.205E-01 (-0.537)
δ3	$\delta_3 = \delta_1$	0.577E-01 (0.296)
γ2	0.936 (20.865)	0.535 (0.904)
γ3		0.505 (1.383)

Table 5. Continued

State:	Washington	
Model:	M6	M0
<u>Coefficient</u>		
λ	0.145	-0.986E-01
	(0.934)	(-0.286)
μ	-0.438E-01	-0.473E-01
	(-0.632)	(-0.354)
R^2	.6704	.8576
DUR-WAT	1.367	1.613
<u>Price Elasticity</u>		
Regime 1	.191	.115
Regime 2	0	-.989
Regime 3	.191	.843

Indiana, Oklahoma, Texas, Washington, Ohio, North Dakota, Montana (spring wheat), Kansas, and South Dakota allotments appear to have imposed binding contraints on acreage allocation. Only for Colorado do we find evidence of heterogeneity among producers during regime 2 which supports the mixed model (model 0).

Similar results are found for regime 3 (1965-1970); however, the results support what is intuitive even at a theoretical level, namely, the characterization of choice in the two regimes is independent. That is, the finding that allotments were binding in regime 2 does not present a basis for inference concerning the appropriate model for regime 3. We find that in Illinois, Colorado, Nebraska, Indiana, Oklahoma, Texas, Washington, and Ohio expected prices receivable appear to have solely determined acreage decisions, acreage controls established by policy appear to have been ineffective. Alternatively, for Montana (winter and spring wheat), North Dakota, Kansas, and South Dakota acreage controls appear to have determined acreage allocation to wheat.

Table 5 reports both the mixed model (MO) and the restricted model identified through nested hypothesis tests. The essence of the inference procedure is to determine the statistical significance of the reduction of explanatory power resultant from the imposition of restrictions. Thus, in all cases the R^2 for the restricted models is lower than that for model 0; however, in all cases where nested tests were employed that reduction was found statistically insignificant.[7]

In nearly all cases, the signs of explanatory variables are consistent with the predictions of the comparative-statics of choice under government control programs. Also, in general the statistical significance of estimated parameters is quite high. Finally, perhaps the most important conclusion that can be drawn from these results is that the use of historical observations of choice for econometric measurement of choice relations may fail to present models which are satisfactory for use in the design of policies. For example, although the elasticity of acreage choice with respect to the acreage allotment is of great interest to policy designers, it is only identifiable from historical observations of choice when allotments were binding. Although both market factors and government policy variables lie behind the determination of whether acreage will be set less than, equal to or greater than the allotment, as section 2 points out only subsets of these factors are continuously related to acreage choice within any local range of that choice. Thus, the evidence presented here emphasizes the potential usefulness of econometric procedures (e.g., Weaver [1978]) which explicitly recognizes the discontinuity of government control policies.

Footnotes

[1] Although Just and Houck et al. include dummy variables to shift intercepts and Houck et al. have introduced price variables which change in definition over the time series, these represent untested, a priori specifications of how the supply function has changed over the sample time period.

[2] Often these incentives were dependent upon producers choice of acreage to be allocated to several different crops. Such cross-compliance requirements will be discussed below.

[3] See Weaver [1978] for a further explanation of these points.

[4] Weaver [1979] has found input prices and fixed factors to be statistically significant determinants of input and output choices. Our exclusions of them in the present paper is done to keep the present exposition as uncluttered as possible.

[5] We have excluded t_w here due to lack of data.

[6] If model X is rejected vs. model Y, then if model Z is a restriction of model X, it also will be rejected vs. model Y. Therefore, we proceed along a branch of nested hypotheses conditionally upon the validity of the prior hypotheses. For a test of model 1 vs. model Z when model 1 characterizes the population relation, Kleok [1970] points out that while $\text{plim}_{n\to\infty} S_1^2 = \sigma^2$, $\text{plim}_{n\to\infty} S_2^2 > \sigma^2$. Therefore, we may conclude that S_2^2 does not converge and that as $n\to\infty$, Thiel's criterion will lead to the identification of the correct model with high probability. Alternatively, this probability is reduced as $n\to 0$ and as the number of alternative models increases. To place the criterion in sharper perspective, Schmidt [1973] has shown that the probability of selecting the correct model according to the criterion is equivalent to the probability that $S_2^2/S_1^2 > 1$. Where $X_2 \epsilon X_1$ this probability is given by the non-central F-distribution. Finally, Atkinson [1970] has proposed a procedure which involves embedding the alternative models statement into a composite model. The probability density function of this model presents the basis for classical hypothesis tests which discriminate between the composed models. However, Quandt [1972] has noted that in the linear case Atkinson's approach leads to the same results obtainable by nested classical tests or by the Thiel criterion. Both Atkinson and Quandt's contributions may be viewed as applications of the work of Cox [1961, 1962] which presents a generalized log-likelihood ratio test amounting to the long-likelihood ratio of the composite model minus the expected value of difference between that of two alternative maintained models. However, the proposed test fails to lead to an unambiguous probability statement concerning which model

should be given preference. S. J. Press [1972] has demonstrated that this procedure is equivalent to an a posteriori Bayesian discrimination between unnested hypotheses where prior information (as represented by the prior distribution of the parameters of each model) is diffuse.

[7] Thus, if our sole objective were to predict aggregate acreage in the absence of an ability to employ estimated parameters as impact multipliers we would focus on model 0.

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