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ABSTRACT

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Allocative decisions concerning public sector agricultural research appear to be driven by both supply and, politically mediated, demand forces. In-sample Granger and Modified Sim's tests, along with post-sample predictive tests, suggest that simultaneity issues should not be ignored when modelling the research expenditure-output relationship. The results also provide strong evidence that the impact of research expenditures on agricultural output persists for at least 30 years. These lags are substantially longer than those commonly used for agricultural research to date. The lagged effect of output on research appears much shorter, at something less than 10 years.

Keywords: technical change, supply, demand, causality, United States, research lag, public sector.

DYNAMICS OF THE AGRICULTURAL RESEARCH AND OUTPUT RELATIONSHIP

In this paper we analyze the empirical relationship between public sector agricultural research and agricultural output. Our interest in this question was spurred on two fronts. First there has been a shift in emphasis of the contemporary economic literature, from an analysis of the consequences of technical change to an explanation of the observed rate and direction of inventive activity. A substantial proportion of this literature has focused on appropriable technologies originating in the private sector and remained largely silent on related issues in the public sector.[1] Here we analyze the dynamics of public sector research, in particular publicly-sponsored agricultural research in the U.S.

The second motivating issue concerns the extensive use of estimated marginal internal rates of return to U.S. agricultural research expenditures in debates over agricultural science and technology policy. These rates of return have repeatedly been estimated under nontrivial assumptions as to the lag length between spending on research and its impact on agricultural output and productivity, as well as the structure and stability of this relationship over time.

Much of the prior empirical work has also been based on measures of research expenditure which were often incomplete and improperly deflated. We bring to bear a unique and carefully constructed time series of research expenditures, developed from research input factors and deflated with factor-specific price indices. We also abandon the use of an imposed lag structure and begin by questioning both the lag length and the nature of the relationship between research and output.

Our paper follows, in Section I, with a background survey of previous studies of the relationship between research and output in agriculture and other sectors. In Section II we describe the data on research expenditures and agricultural output and present new evidence on research lag lengths and the results of causality tests. Finally, in Section III we draw some conclusions about estimation of the returns to research.

I. BACKGROUND

Causality:

A stylized view of technical progress can identify two broad schools of thought concerning the relationship between research and output. The first is a supply-driven or science-based view. Economists have formalized this notion into traditional production function models, where an industry's final output is a function of conventional factors of production such as labor and capital along with non-conventional inputs such as research expenditures.[2] In its more rudimentary form this view took technological change as an exogenous variable, something that, as Rosenberg (1982) observed, had important economic consequences but no readily identifiable causes. Consequently, there is an implicit view that research expenditures are causally prior to output in the sense of Granger (1969).

A second school of thought is presented in various discussions of demand-driven or endogenous technical change (see Mowery and Rosenberg (1979), Rosenberg (1982), and Thirtle and Ruttan (1986)). Somewhat separate literatures concerning the role of these demand forces on technical change have emerged - one centered on private and the other on public demand. Significant empirical impetus was given to the private demand case by the early work of Schmookler (1966). He ran log-linear regressions of value added, an investment proxy, on successful patent applications for capital goods inventions file in the succeeding three years. The results indicated a rough proportionality between the two variables and was taken, inter alia, as strong evidence in support of the notion of demand-induced invention. In particular, these and related results led economists to the interpretation that potential gains from successful inventive activity, as indexed by expected market size for the output of R & D activities, drives inventive activity at the firm or industry level. According to Pakes and Schankerman (1984) additional determinants of research demand include the degree to which firms can appropriate the benefits from the industrial knowledge they produce, and the technological opportunity or cost of producing this knowledge. From this a direction of causality which runs from output to private research activity follows quite naturally.

However, these models relate to situations in which the returns from research (or part thereof) are directly appropriated by the institution or firm undertaking the research investment. By contrast, public demand models of inventive activity have had limited attention. Here some distance is placed between those agents sponsoring the research and those who will ultimately benefit from it.[3] Thus it is the mechanisms by which demand is articulated, and in particular the conditions of appropriability, which largely distinguish private from publicly demanded research activity. Recent versions of this theme, which deal with public research activity in

U.S. agriculture (for example, Guttman (1978) and Rose-Ackerman and Evenson (1985)),[4] argue that rent seeking behavior by the ultimate beneficiaries of research operates through the political system to draw resources into public agricultural research. Such an argument would lead one to expect agricultural output or sales to lead publicly-committed research expenditures.[5]

Of course, there is always a third option: namely that there may be feedback between output and research. This issue was reviewed in general terms by Griliches (1979) for the case of private sector research activity, while Baumol and Wolff (1983) developed, but did not test, a formal model of this feedback phenomenon. For them, the scale of R & D activity affects the rate of productivity growth in manufacturing, which in turn may raise the relative price of R & D thereby reducing the quantity of R & D demanded. A recent empirical study by Mairesse and Siu (1984) did address this issue, and found that 'innovations' in both the stock market one period holding rate of return (taken as an indicator of changes in expectations about the firm's future profitability) and sales cause subsequent R & D and gross investment changes. However, they observed no further feedback from R & D and investment to either the stock market rate of return or sales.

Turning to U.S. public sector agricultural research, feedback would be consistent with an agricultural sector which benefits from technological innovations arising in, say, the State Agricultural Experiment Stations (SAES) and a system of SAES whose funding depends upon the performance of the agricultural sector. While some researchers have recognized this possibility (for example, Huffman and Miranowski (1981)) it seems that none have formally tested for causality. We fill this gap using both in-sample and out-of-sample test of causality suggested in the time series literature.

Lags:

We have already noted the heavy reliance on imposed rather than tested priors when estimating the (aggregate) lag relationship between public research expenditures and agricultural output. For example, Evenson (1968) experimented with De Leeuw and so-called rational distributed lag forms and rejected the declining weight structure implied by the rational lag estimates in favor of the more 'plausible' inverted-V results. Many subsequent studies have mimicked Evenson's lag forms or variants thereof, including geometric and polynomial distributed lags. Unfortunately the various combinations of exact, linear restrictions which are usually imposed on these models have often not been subject to systematic testing. This structure has generally been introduced to skirt multicollinearity and degrees of freedom problems and not on the basis of some theoretically derived priors. While summary measures such as the mean and variance of this lag relationship appear relatively insensitive to the presumed lag structure, unfortunately the implied internal rate of return to agricultural research is quite sensitive to the partial research production coefficients derived from these models (see Pardey (1986)).

We take advantage of our relatively long time series to address specifically the issue of lag length. Numerous studies have proceeded with finite lag structures involving lag lengths in the 15 year range, while many can also be found with lags as short as 5 to 10 years.[6] The empirical evidence to support these maintained hypotheses is rather thin. One of the few empirical studies to give closer consideration to this issue was Evenson (1980). His results suggest that somewhat longer lags may be more appropriate. However, the lag lengths were chosen only after substantial structure had been imposed on the form of the lag relationship in the context of a model which did not admit the possibility of feedback between output and research expenditures. Both these major restrictions are relaxed in this study.

II. THE EMPIRICAL ANALYSIS

Causality Tests:

The concept of causality being tested here must always be treated gingerly. The tests proposed by Granger (1969) and Sims (1972) hinge on the idea that random variable x is causally prior to random variable y if the past history of x improves significantly our ability to predict series y compared to our ability to predict y using all relevant information apart from x. Such a definition of causality lends itself naturally to testing with time series data, although there are delicate issues in the implementation of these tests and their interpretation.

In the definition of causality we are using, we must first decide upon what information will constitute the set of all information relevant to the prediction of a particularly random variable. Most empirical tests involve the analysis of a bivariate system such as equation set 1

1a. $y_t = a_o(L)y_t + b(L)x_t + e_{at}$

1b. $x_t = c(L)y_t + d(L)x_t + e_{bt}$

where variables x and y are covariance stationary processes with autoregressive representations. a(L) is a polynomial of degree v in the lag operator, i.e., $a(L)z_t = \sum_{k=1}^{v} a_k z_{t-k}$. Using the definition of causality described above, then x causes y if any coefficient b is non-zero; y causes x if any coefficient c is non-zero; and there is feedback between x and y if both b(L) and c(L) have non-zero coefficients.

We too will adopt a bivariate system without apology. The danger of drawing conclusions about causality from a bivariate model lies in the omission of relevant variables, particularly variables which may cause the two variables being considered. When measuring the effect of public sector agricultural research on agricultural output, the most obvious omissions are conventional agricultural inputs such as land, labor, fertilizer, feed, seed and livestock, plus private sector research which has applications to agriculture. To deal with the contribution of conventional inputs to agriculture we have used two different measures of agricultural output which remove explicitly or implicitly quantitative changes in inputs. A third proxy for output is also included in the bivariate tests which corresponds more closely to a measure of agriculture's importance to the whole economy.

The treatment of data to induce stationarity is also a crucial issue in the implementation of the tests. As Sims (1977) points out, prefiltering of the data makes causality tests asymptotically biased except in the case of no relationship between the two series. Consequently, we have kept prior adjustment of the time series to a minimum by simply removing means and trends from logged series.

Finally, causality tests are often criticized for the arbitrariness of the tests. Pierce and Haugh (1977) find that the results of causality tests often depend on the test chosen. Consequently, we follow the recommendations of Geweke, Meese and Dent (1983) in using Wald tests to evaluate the Granger causality test and a modified version of Sim's causality test. Using Monte Carlo methods, they have found that these test outperform others in detecting the presence or absence of causality.

To make the discussion of our findings more concrete, we now describe the Granger and Sims tests with reference to equation system 1. The Granger test of the hypothesis that x does not cause y is a test of the restriction b(L)=0 in equation 1a. Under the null hypothesis the distribution of the Wald statistic

$$T = n[\hat{\sigma}_c^2 - \hat{\sigma}_a^2] / \hat{\sigma}_a^2$$

converges uniformly to a Chi-square variate with p degrees of freedom as the sample size, n, increases. Here p is the degree of polynomial b(L), $\hat{\sigma}_a^2$ is the maximum likelihood estimate of var(e_{at}) in equation 1a, and $\hat{\sigma}_c^2$ is the maximum likelihood estimate of var(e_{ct}) in equation 1c.

1c. $y_t = a_1(L)y_t + e_{ct}$

The test proposed by Sims of the hypothesis that x does not cause y is a test of the restriction that leads of y have zero coefficients in a regression of x on lead, current and lagged values of y. Geweke, Meese and Dent (1983) suggest a modified version of the Sims test in which lagged values of x are used as regressors in addition to the leads and lags of variable y in order to avoid the need to use GLS estimators when performing the Sims test.

The modified Sims test of the hypothesis that x does not cause y, is then a test of the restriction g(L) = h(L) in equation system 2.

2a. $$x_t = f_o(L)x_t + g(L)y_t + u_{at}$$

2b. $$x_t = f_1(L)x_t + h(L)y_t + u_{bt}$$

Here, $g(L)\, y_t = \sum_{k=-q}^{r} g_k y_{t-k}$ and $h(L)y_t = \sum_{k=1}^{r} h_k y_{t-k}$.

Under the null hypothesis the distribution of the Wald statistic

$$T = n[\hat{\sigma}_b^2 - \hat{\sigma}_a^2]/\hat{\sigma}_a^2$$

converges uniformly to a Chi-square variate with q degrees of freedom as the sample size, n, increases. Here $\hat{\sigma}_a^2$ is the maximum likelihood estimate of $\text{var}(e_{at})$ in equation 2a, and $\hat{\sigma}_b^2$ is the maximum likelihood estimate of $\text{var}(e_{bt})$ in equation 2b.

Data and Model Specification:

The data on research expenditures is a total of spending by the United States Department of Agriculture (USDA) and by the 48 coterminous State Agricultural Experiment Stations (SAES) annually from 1890 to 1983 in current dollars. The SAES component from 1890 to 1974 was compiled from Cooperative State Research Services (CSRS) reports along with annual reports of the SAES in all states except Alaska and Hawaii. Over this period, separate figures were derived for expenditures on land and buildings, plant and equipment, total funds available and the year-to-year carryover of unspent funds. We calculated non-capital expenditures by subtracting both carryout and capital expenditures from the current years funds available, inclusive of the previous year's carryover.

USDA appropriations for research were generally taken from issues of the U.S. Congress's House Appropriations documents.[7] Each year's total expenditures were divided among the three factor types, based on the average factor mix of total SAES expenditures over the previous ten years. The resulting three factor level expenditures were added to the SAES counterparts to get total public sector expenditures on 'labor', land and buildings, and plant and equipment used in agricultural research.[8]

Having divided expenditures according to factor type, we were able to use factor-specific deflators to express total research expenditures in 1967 dollars. Non-capital expenditures were deflated using an index of average university salaries, since this component was primarily salaries of research and support staff. Land and building expenditures were deflated using the Handy-Whitman index of public construction prices. The implicit price deflator for State and Local Government Purchases of Goods and Services was used to deflate plant and equipment expenditures. None of these deflator series was available as early as 1890, so they were extrapolated using related prices series taken from the Department of Commerce's Historical Statistics of the U.S. (1975) (see Pardey, Craig and Hallaway (1986)).

After 1975, expenditures of the SAES were no longer reported as separate factors. In fact, from 1976 to 1983 the USDA's Inventory of Agricultural Research - the only source available - reports total obligations of the SAES. For the years 1970 to 1974, in which direct comparisons can be made,[9] these figures initially understate but later marginally overstate CSRS expenditures. Given no alternative, this source was used to approximate SAES research expenditures for the years 1975 to 1983. Based on the average percentage of total funds available allocated to each factor type in the years 1960 to 1974, we allocated the Inventory of Agricultural Research figures to the three factors, added them to the USDA expenditures, and deflated as before.

In constructing a measure of total real research expenditures, two approaches were used. In the most straightforward measure, real expenditures in each of the three factor categories were simply added together. In the second measure, real service flows from the two capital categories were calculated and then added to real labor expenditures to yield a measure we call total real service flows from research expenditures. The land and buildings were assumed to have a 25 year service life while plant and equipment were assumed to have only a 10 year service life. Additionally, both service flow profiles were proxied by a

One-Hoss Shay assumption with zero salvage value. Both the expenditure and service flow measures were logged and detrended before the causality tests were performed.

The ability to split the expenditures and deflate them by factor categories, rather than deflate the total with a single price index or an arbitrarily constructed price index, significantly changed the measurement of total real research expenditures as compared with measures used previously (see Pardey, Craig and Hallaway (1986)). The measurement of research expenditures in service flow terms is quite appealing conceptually, but, as reported below, does not appear to measurably affect the causality tests.

Two of the measures of agricultural activity used in the causality tests were based on annual indexes of agricultural output, major agricultural inputs and productivity published by the USDA (1983 and 1986) for 1910 through 1984. We report below the results of causality tests using the USDA productivity index logged and detrended. The productivity index is a rather crude proxy for measuring, inter alia, the presumed technological impact of agricultural research on agricultural output. It was constructed, according to the USDA's documentation, by forming the ratio of the aggregate output index and the index of agricultural inputs. A more elegant total factor productivity measure using Tornqvist-Theil input and output indices, calculated by Ball (1985), was available only for 1948-1979. We opted, however, for the cruder measure because we need the longest possible time series and because Ball himself found that the USDA's productivity index is not significantly different from the measure he calculated.[10]

An alternative output measure was constructed using the residuals of a regression of the logged output index on logged indices of farm labor; farm real estate; mechanical power and machinery; feed, seed and livestock; agricultural chemicals; and miscellaneous inputs. This measure, which we refer to as residual output, is another crude proxy for that part of the growth in agricultural output we cannot account for with changes in conventional inputs. It is quite appealing to ask if research can help explain the residual variation in output, but we must keep in mind that we may weaken the measured impact of research on output by not accounting for research-driven quality changes in the conventional inputs.

The final measure of agricultural activity used below is the share of agricultural income in the GNP of the U.S. from 1910 to 1984. The series used to measure agricultural's contribution to GNP was gross realized income less the imputed rental value of farm dwellings for 1910 to 1963. For the most recent 21 years, the change in inventories, imputed rents and other income were subtracted from gross farm income to yield a consistent measure.[11] Both farm income and GNP were measured in millions of current dollars. This measure was not logged - it enters regressions with mean and trend removed. This last output measure does not really correspond to our notion of that part of output which is driven by research. It was included because it is expected to capture the influence of relative sector performance on the allocation of public research funds.

As with any empirical test of causality a certain degree of arbitrariness must enter as to choice of lag lengths. While the time series at out disposal are substantially longer than those which have generally been used in the past to measure the internal rates of return to public sector research, they still impose important constraints for our estimation. Lag lengths of around 15 years have been imposed in many prior studies of the lag from research to output. As we observed earlier these cutoff points were arrived at under some rather severe maintained hypotheses, including questionable assumptions of symmetry and smoothness, so we have allowed a more generous lag of 30 years when regressing output on past research expenditures in the Granger tests. Both individually and jointly, these longer lags -- particularly in the 23 to 29 year range -- were found to be significant at confidence levels of 0.10 or less. In the Sims tests of causality from research to output, we had too few observations to allow more than 20 leads of output without severely restricting lags of both research and output variables. As reported below, the results of the test were quite sensitive to the lead length.

In regressions of research on its own past, lags of six and sixteen years were often found to be significant at 0.10 percent. Lags of 6 to 20 years were also found to be jointly significant. Consequently, 20 own lags were used in all of the regressions on which the Granger tests are based. However, we had too few observations to allow more than 10 lags of research in the Sims test of the hypothesis that research does not cause output.

There is even less information in the literature as to a reasonable number of lags to allow for the output series. In no regressions were lags beyond the tenth year significant either individually or jointly in predicting current output when 30 years of lagged research was included in the regression. And, although the political system may be relatively slow to respond to the economic performance of agriculture, we reasoned it may also have a short institutional memory, leading us to use relatively short lag lengths in regressions of research on output. This was borne out by the time series; lags beyond 10 years were not significant either jointly or individually. Significant coefficients appeared most often at two, four and six year lags.

Findings:

The test statistics for the Granger tests are reported in Table I, Parts A and B. The hypothesis that research does not cause output was rejected in all tests at a significance level of 0.01. Absence of causality from output to research was also rejected in all six regressions, giving us evidence of feedback between all pairs of variables.

The test statistics for the modified Sims tests are reported in Table II, Parts A and B. The hypothesis that output does not cause research was rejected by all tests. Two sets of test statistics for the test of the hypothesis that research does not cause output are reported in Table II, Part A. When only 10 leads of research were used, the hypothesis is rejected only when evaluating the relationship between productivity and real service flows from research expenditures. This rejection pattern was sensitive to the pattern of leads and lags allowed in the regression. Expanding the leads of research from 10 to 20 in the regression of output on itself and research led us to reject the absence of causality from research to output in all of the bivariate systems. So, with the Sims test we also find strong evidence of feedback between research and output but only when we narrowly restrict the number of lagged research and output variables entering the equation used in predicting research.

As pointed out by Ashley, Granger and Schmalensee (1980), causality tests based entirely on the in-sample predictions of the competing univariate and bivariate models are not the only possible applications of Granger's notion of causality. They argue that it is more appropriate to compare the models' post-sample predictions of the non-prefiltered time series. If estimation of the bivariate system involving both x and y leads to a reduction in the mean square error of the forecast variable y as compared to the mean square error of the forecast based on a univariate system, then there is evidence of causality running from x to y.

We constructed a series of one-step-ahead forecasts of both logged research and logged output measures. Initially the bivariate and univariate models were each estimated using data through 1970 to produce forecasts for 1971. Next the models were re-estimated using data through 1971 to generate forecasts for 1972. This procedure was repeated to generate forecasts of output proxies through 1984 and forecasts of research spending through 1983. We report, in tables IIIa and IIIb, the means and variances of these forecast errors for the univariate and bivariate models.

Based on the post-sample performance of univariate and bivariate models, we do not find such uniform evidence of feedback between output and research. Instead, these tests highlight relationships between particular pairs of the variables used. We detect causality running from research - however measured - to productivity. The prediction of residual output and agriculture's share of GNP are not improved by inclusion of information contained in past research expenditures. The only evidence of causality from output to research is found to run from residual output to total real expenditures on research.

The causality evidence of post-sample tests does not contradict the Granger and Sims tests reported above, but it does cast some doubt over the strength of the evidence concerning feedback. The post-sample forecasts appear to be more discriminating as far as identifying the particular measure of output and research expenditures which are significantly correlated.

The in-sample tests provided support for causality running from research to agricultural output regardless of the measure used for output. In both the Granger and Sims tests, the absence of causality from research to output was rejected more strongly and consistently when productivity was the proxy for the performance of the agricultural sector. Causality running from research to productivity is the only research to output relationship to hold up in out-of-sample tests. As we discussed above, the productivity variable is, a priori, the most appealing measure of agricultural output if one is looking for support of the science-based view of technological change.

Evidence of causality running from output to research in the in and out-of-sample tests is more difficult to reconcile. Since total real expenditures are more highly correlated with appropriations of state and national research funds than is the measure of real service flows, we would expect this measure of research to be the most likely to be driven by demand for appropriations. The Granger and Sims tests reject the absence of causality from output to research regardless of the measure of research expenditures. Only in the Granger tests is the evidence for rejection uniformly stronger when using total real expenditures rather than real service flows. The out-of-sample tests are, again, more discriminating -

causality running from residual output to total real research expenditures is the only output to research relationship to hold up in out-of-sample tests.

The fact that the out-of-sample tests yield more sharply defined results may be explained by the fact that the relationship between public research expenditures and agricultural output is not stable over time. Possible instability might arise because the underlying structure has changed over the sample or because our imperfect measurement of output and research may mean the particular statistical relationships we are estimating are not stable over time. We do know that the sources of funding for public agricultural research have been changing quite markedly over the course of this sample. Averaging across all 48 states the state-level component of total agricultural research funds available was only 19.9 percent for the 1889-1900 period rising steadily to 54.9 percent for the 1974-1983 decade. Our use of aggregate output and research data may well serve to hide the response of local appropriations to local agricultural conditions. The use of exclusively public sector expenditures on research may also cause problems when dealing with such a long sample if the public and private mix in agricultural research has changed over the sample.

III. CONCLUSIONS

The results of causality tests reported here represent a preliminary step in a study of the returns to public sector research in agriculture. As such, they do not give us detailed information on the structure or stability of the relationship between research and output over time. The results do, however, have important implications for estimation of structural parameters.

Prior estimates of marginal internal rates of return from public sector research have not taken into account the possible simultaneous determination of output and research funding. Based on in-sample causality tests, we would conclude that those estimates may suffer from simultaneity bias. Evidence of feedback is not so strong in post-sample tests, but it still leads us to caution against treating research expenditures as exogenous in econometric models predicting output. These results are, we should point out, not likely to be a problem for empirical estimates of the rates of return to agricultural research alone. A large body of literature devoted to the productivity effects of research in other sectors may well be subject to the same empirical biases.

There is also evidence that quite long lags - at least 30 years - must be allowed if hope to capture all of the impact of research on agricultural output. These are significantly longer than the lags commonly used for agricultural research to date, and approximately three times longer than analogous lags in the private (non-agricultural) sector. We also observed lags in the responsiveness of research expenditures to changes in agricultural output, presumably the result of intertia in the political process, but this effect does not seem to persist for longer than 10 years.

It is difficult to speculate about the combined influence of these lag length and simultaneity findings on the implied internal rate of return to research. Any such inferences should be conditioned on the exact form and stability of the lagged research-output relationship - an issue plan to address in future work. Nevertheless, some of the structure within which this relationship should be estimated has been identified here.

The results of this study, based on aggregate U.S. data, also point to the desirability of performing similar empirical tests on state level data to arrive at a more definite assessment of causality patterns and their stability. Tests for feedback between research and output on a state-by-state basis might yield even sharper evidence as to the structure and dynamics of this relationship since it would provide us with an even larger pool of data for out-of-sample tests as well as an opportunity to replicate the in-sample tests.

FOOTNOTES

1. A widely cited exception is the induced innovation model which has sought to account for inter-country differences in the factor saving bias of (public sector) agricultural research. See Binswanger and Ruttan (1978) and Hayami and Ruttan (1985).

2. See, for example, early studies of the U.S. agricultural sector by Evenson (1968) the chemical industry by Minasian (1969), and a more recent study of U.S. manufacturing firms by Griliches (1986).

3. Of course some publicly demanded and sponsored research is carried out by private firms. For instance, Levy and Terleckyj (1983) present evidence which is consistent with the notion that government contracted (within industry) R & D stimulates additional private R & D in the order of $0.27 per dollar of government contracts. These results relate to the Department of Defense and NASA contracts so whether-or-not they hold in the case of public sector agricultural research is open to question.

4. See also McLean (1982) for an interesting variant of the demand-driven model in the Australian context.

5. Peterson (1969) estimated an empirical model for U.S. agriculture under this maintained hypothesis.

6. Pakes and Schankerman (1984) present evidence which suggests that new product and process 'life-spans' (defined as the period after which the product of R & D was 'virtually obsolete') in the order of 8 to 11 years are the norm.

7. No USDA research component was recorded in the pre-1930 House Appropriations documents. The research expenditure figures for these earlier years were taken from Latimer (1964) for the 1915 to 1929 period and derived as a pro-rated component (0.196) of total USDA appropriations for the remaining years. The total appropriations figures were obtained from annual issues of the U.S. Treasury's Combined Statement documents.

8. Research 'labor' when used in this study refers more specifically to non-capital research expenditures.

9. Direct comparison can be made back to 1966 but the Inventory of Agricultural Research figures for the 1960's are of questionable reliability.

10. Ball does note that the similarities in the productivity indices masks a fairly substantial downward bias in the growth rate of individual inputs (particularly labor and capital) implied by the USDA series. This however is of no consequence for our study.

11. The GNP figures through 1946 were from the Department of Commerce's Historical Statistics (series F1-5) while the 1947-1984 figures were from the "Total" column of Table 3, Gross National Product by Industry, in the Department of Commerce's Survey of Current Business (1986). The 1910-63 Farm Income statistics were the Historical Statistics "Realized Gross Farm Income" (series K264) figures minus the "Gross Rental Value of Farm Dwellings" (series K270) figures. The 1964-69 figures were from the 1979 Agricultural Statistics, Table 654 while the 1970-84 figures were from the 1985 Agricultural Statistics, Table 584.

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TABLE I

Granger Test Results
(Chi-square Statistic)

	Research Variables	
Output Variables	Real Service Flows from Research Expenditures	Total Real Research Expenditures
A. Null Hypothesis - Research does not cause output*		
Residual Series from Output Index	79.8	83.2
Residual Series from Productivity Index	105.5	146.0
Residual Series on Agriculture's Share in GNP	105.0	99.1
B: Null Hypothesis - Output does not cause research+		
Residual Series from Output Index	71.1	97.9
Residual Series from Productivity Index	81.4	88.4
Residual Series on Agriculture's Share in GNP	48.7	59.2

* Output measures were regressed on 10 own lags and 30 lags of each research variable. Testing the null hypothesis amounts to restricting the 30 lags of research to have zero coefficients, so the test statistic is distributed Chi-square with 30 degrees of freedom.

\+ Research measures were regressed on 20 own lags and 15 lags of each output variable. Testing the null hypothesis amounts to restricting the 15 lags of output to have zero coefficients, so the test statistic is distributed Chi-square with 15 degrees of freedom.

TABLE II

Modified Sims Test Results
(Chi-square Statistic)

Output Variables	Research Variables: Real Service Flows from Research Expenditures	Research Variables: Total Real Research Expenditures
A. Null Hypothesis - Research does not cause output*		
Residual Series from Output Index	6.5/ 87.3	10.3/ 89.8
Residual Series from Productivity Index	30.3/142.0	28.9/145.2
Residual Series on Agriculture's Share in GNP	11.2/113.2	15.6/107.0
B: Null Hypothesis - Output does not cause research+		
Residual Series from Output Index	82.1	72.6
Residual Series from Productivity Index	143.4	83.9
Residual Series on Agriculture's Share in GNP	153.0	159.2

* Research measures were regressed on own lags as well as leads, current values and lags of the output variable. Two test statistics are reported here. The first statistic is distributed Chi-square with 10 degrees of freedom and is calculated by restricting 10 leads of output to have zero coefficients when both variables are lagged 10 years. The second test statistic is distributed Chi-square with 20 degrees of freedom. It is calculated by restricting 20 leads of output to have zero coefficients when both variables are lagged only 6 years.

\+ Output measures were regressed on 10 own lags as well as 10 leads, current and 20 lags of the research variable. Testing the null hypothesis amounts to restricting the 10 leads of research to have zero coefficients, so the test statistic is distributed Chi-square with 10 degrees of freedom.

TABLE IIIa

Means and Variances of One-Step-Ahead Forecast Errors of Output Measures: 1971-1984

Forecast based on:	10 own lags	10 own lags and 30 lags of real service flows of research expenditures	10 own lags and 30 lags of total real research expenditures
Forecast of			
logged output index	-.0300+ (.0028)	-.0539 (.0040)	-.0409 (.0039)
logged productivity index	-.0146 (.0031)	-.0043* (.0028)	-.0017* (.0028)
agriculture's share of GNP	-.0033 (.00002)	-.0028 (.00006)	-.0002 (.00007)

\+ Mean is unbracketed; variance is bracketed.

* Using the test described by Ashley, Granger and Schmalensee (1980), we conclude that the bivariate model outperforms the univariate model in out-of-sample forecasting of the output measure because the variance is not significantly different in the bivariate model while its mean error is smaller at the 5 percent significance level.

TABLE IIIb

Means and Variance of One-Step-Ahead Forecast Errors of Research Measures: 1971-1983

Forecast of:	Logged real service flows from research expenditures	Logged total real research expenditures
Forecast based on		
20 own lags	.0168+ (.0013)	.0297 (.0018)
20 own lags and 15 lags of logged total output index	.0025 (.0018)	.0039* (.0022)
20 own lags and 15 lags of logged productivity index	.0026 (.0016)	.0322 (.0020)
20 own lags and 15 lags of agriculture's share of GNP	-.0125 (.0031)	.1406 (.0221)

\+ Mean is unbracketed; variance is bracketed.

* Using the test described by Ashley, Granger and Schmalensee (1980), we conclude that the bivariate model outperforms the univariate model in out-of-sample forecasting of the output measure because the variance is not significantly different in the bivariate model while its mean error is smaller at the 5 percent significance level.